THE
Philosophical Works

Of the HONOURABLE

ROBERT BOYLE Esq;

Abridged, methodized, and disposed under the GENERAL HEADS

OF

PHYSICS, STATICS, PNEUMATICS, NATURAL-HISTORY, CHYMISTRY, and MEDICINE.

The whole illustrated with NOTES, containing the *Improvements* made in the several *Parts* of natural and experimental *Knowledge*, since his time.

By *PETER SHAW*, M. D.

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M. DCC. XXV.

TO

The RIGHT HONOURABLE

The Earl of *ORRERY*.

MY LORD,

YOUR known parts and learning juſtly entitle your Lordſhip patron to any deſerving production of genius or ſtudy: But the volume here inſcribed to your Lordſhip, has a farther right to your countenance and favour; as not only treating of ſubjects, for a thorough knowledge whereof your Lordſhip is become eminent; but being part of a work originally proceeding from one, whoſe immortal name adds to the dignity of your illuſtrious family.

Mr. *Boyle* ſurvives in his works; but if we would ſee him expreſs'd by real life, we muſt turn our eyes upon your Lordſhip, and your noble family, in whom reign, to perfection, the fine taſte, and the comprehenſive genius; the candid, generous, and communicative temper, ſo eminently predominant in Mr. *Boyle*.

I am,

May it pleaſe your Lordſhip,

Your Lordſhip's moſt humble,

moſt obedient,

and moſt devoted Servant,

Peter Shaw.

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E X-

EXPERIMENTS AND OBSERVATIONS UPON COLOURS.

SECT. I.

'TIS often thought that a diverſity of colours conſtantly argues an equal diverſity in the nature of the bodies wherein they reſide: but I cannot wholly give into this opinion. For not to mention changeable taffaties, the blue and golden feathers on the necks of pigeons, and ſeveral water-fowl, natural and artificial rainbows, &c. the colours whereof philoſophers call not real, but apparent; we ſee that the contiguous feathers in various birds are ſome of them red, others white, blue, yellow, &c. and that in ſeveral parts of the ſame feather there is frequently the greateſt diſparity of colours: ſo in tulips, july-flowers, and other vegetables, even ſeveral parts of the very ſame leaf are frequently found of different dyes; tho' no difference is obſerved in their other properties. And ſuch a variety we have much more remarkably in the marvel of *Peru*; for of the great profuſion of fine flowers which that gaudy plant affords, I have ſcarce obſerved any two dyed perfectly alike. But tho' ſuch particulars keep me from affirming, that a diverſity of colours always denotes ſome great difference in bodies, yet that it often ſignifies conſiderable alterations in the diſpoſition of their parts, appears from the extraction of tinctures, wherein the change of colour is the chief, and ſometimes the only thing by which the artiſt regulates his procedure in their preparation. Inſtances of this are alſo obvious in ſeveral ſorts of fruit, wherein according as the vegetable ſap is ripened, by paſſing from one degree of maturation to another, the external part of the *Diverſity of colours, what it ſignifies.*

PHYSICS. the fruit changes from one colour to another. A leſs obvious inſtance of this kind is afforded us by the method of tempering ſteel for gravers, drills, ſprings, &c. which is this. Firſt, the ſteel to be tempered is hardened, by heating it in glowing coals, and not quenched as ſoon as taken from the fire, but held over a baſon of water till it deſcend from a white heat to a red one; when 'tis immediately quenched in cold water. The ſteel thus hardened will, if it be good, look whitiſh; and being brighten'd at the end, and held in the flame of a candle, that the bright end may lie about half an inch diſtant from the flame, it will ſwiftly paſs from one colour to another, as from a bright yellow to a deeper and reddiſh yellow, from that to a fainter firſt, and then to a deeper blue; each of which ſucceeding colours argues ſuch a change made in the texture of the ſteel, that if it be taken from the flame, and immediately quenched in tallow, whilſt it is yellow, it will be of ſuch a hardneſs as fits it for drills; &c. but if kept for a few minutes longer in the flame, till it turns blue, it becomes much ſofter and proper to make ſprings for watches; which are therefore commonly of that colour; laſtly, if you keep the ſteel in the flame after the deep blue has appeared, it will grow too ſoft even for penknives. Any perſon may eaſily ſatisfy himſelf of the different hardneſs of ſteel of different colours, either by the file, or by breaking ſome ſlender wires thus tempered, and obſerving how they vary in point of brittleneſs.

Colour, what? But before we deſcend to a more particular conſideration of our ſubject, 'tis proper to obſerve, that colours may be regarded either as a quality reſiding in bodies to modify light after a particular manner, or elſe as light itſelf ſo modified as to ſtrike upon the organ of ſight, and cauſe the ſenſation we call colour; and that this latter is the more proper acceptation of the word colour, will appear hereafter. And indeed it is the light itſelf, which after a certain manner, either mixed with ſhades, or otherwiſe, ſtrikes our eyes, and immediately produces that motion in the organ which gives us the colour of an object. * Yet becauſe there is in the

* The words of Sir *Iſaac Newton* excellently clear and illuſtrate this matter. "The homogeneal light and rays (ſays "that great philoſopher) which appear "red, or rather make objects appear ſo, "I call rubrific, or red-making; thoſe "which make objects appear yellow, "green, blue, and violet, I call yellow-"making, green-making, blue-making, "violet-making, &c. And if at any time "I ſpeak of light and rays as coloured, or "endued with colours, I would be under-"ſtood to ſpeak not philoſophically and "properly, but groſly, and according to "ſuch conceptions as vulgar people would "be apt to frame. For the rays, to ſpeak "properly, are not coloured. In them "there is nothing elſe than a certain "power and diſpoſition to ſtir up a ſen-"ſation of this or that colour; for as "ſound in a bell, or muſical ſtring, or "other ſounding body, is nothing but a "trembling motion, and in the air no-"thing but that motion propagated from "the object, and in the ſenſorium 'tis a "ſenſe of that motion under the form of "a ſound; ſo colours in the object are "nothing but a diſpoſition to reflect this "or that ſort of rays more copiouſly than "the reſt: in the rays they are nothing "but their diſpoſitions to propagate this "or that motion into the ſenſorium, and "in the ſenſorium they are ſenſations of "thoſe motions under the forms of co-"lours. *Newton. Optic. p.* 108, 109.

coloured

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coloured body a certain diſpoſition of the ſuperficial particles to ſend the light reflected or refracted to our eyes, colour may alſo, in ſome ſenſe, be ſaid to depend upon the viſible body; and therefore I ſhall not reject the popular form of ſpeaking of colours, provided we may have recourſe occaſionally to the diſtinction laid down. But ſtill colour is ſo far from being inherent in objects, that light itſelf produces the ſenſation only as it cauſes a determinate kind of local motion in ſome part of the brain; for if the like motion happen from any other cauſe, wherein the light is unconcerned, a man ſhall think he ſees the ſame colour *. Thus it is uſual in dreams to fancy we behold coloured images; and the ſtrange imaginations of diſtracted perſons, with the flaſhings of light, and other appearances, upon ſome diſorders of the brain, farther confirm this particular.

The like effect may alſo be produced from internal cauſes which affect the optic nerve; for I remember in myſelf that, upon coughing with vehemence, ſudden flaſhes of vivid flame have appeared, as it were, before my eyes †. And a lady, to whom I am related, aſſured me, that all the objects of her ſight, once, of a ſudden, appeared of various unuſual colours, ſurprizingly bright and vivid; which ſymptom was, the next day, followed by a fit of the hyſteric diſeaſe. The like ſymptom a phyſician alſo informed me he had obſerved to be a certain prognoſtic of the plague, in a ſeaſon when that diſtemper raged; but that it generally went off after the exhibition of an emetic. As an appearance of colour may, therefore, be produced by internal motions, without the aſſiſtance of an external object; ſo the colour, which would otherwiſe be produced by an outward object, may, poſſibly, be ſometimes changed by a motion or new texture in the organ, as long as that motion or new diſpoſition continues.

Thus I have often obſerved, upon looking at the ſun thro' a teleſcope, darkened ſo as to make the ſplendor of that luminary ſupportable, the impreſſion upon the retina, would neverthele ſs be ſo vivid and permanent, that if afterwards I turned the eye, therein made uſe of, towards a flame, it would appear of a colour very different from its natural one.

* 'Tis certain, that whenever the ſame eye is affected twice alike, viſion will be the ſame in both caſes; but different when the eye is differently affected. And this is a juſt, not an imaginary, foundation for diſtinguiſhing red light from blue, yellow from green, *&c.* for that is properly a red light which excites in us the idea of red, a blue which excites in us the idea of blue, *&c.* Theſe ſeveral colours, therefore differently affect the eye, and conſequently have different diſpoſitions.

† To this purpoſe Sir *Iſaac Newton* ſpeaks thus: " When a man in the dark, " ſays he, preſſes either corner of his eye " with his finger, and turns his eye away " from his finger, he will ſee a circle of " colours like thoſe in the feather of a " peacock's tail. If the eye and the finger " remain quiet, theſe colours vaniſh " in a ſecond minute of time; but if the " finger be moved with a quavering motion, " they appear again. Do not theſe " colours ariſe from ſuch motions excited " in the bottom of the eye by the " preſſure and motion of the finger, as at " other times are excited there by light " for cauſing viſion? And do not the motions, " once excited, continue about a " ſecond of time before they ceaſe? And " when a man by a ſtroke upon his eye " ſees a flaſh of light, are not the like " motions excited in the retina by the " ſtroke?" *Newton. Opt. p.* 321, 322.

PHYSICS. And if I ſeveral times ſucceſſively ſhut and opened the ſame eye, this new colour ſeem'd changed or impaired by degrees; till at length the flame appeared to me of the ſame colour it did to other ſpectators. The like effect I have alſo found by looking upon the moon, when near the full, thro' a teleſcope, without employing a coloured glaſs to defend the eye. And here 'tis remarkable, that tho' my right eye, with which I looked thro' the teleſcope, were thus affected by the light, yet if I ſhut that eye, and looked upon the ſame object with the other, it appeared of its uſual colour; but if I again opened and made uſe of the dazzled eye, the vivid adventitious colour would again return. 'Tis further obſervable, that a vehement blow upon the organ of viſion, eſpecially if it be naturally weak, may for a long time vitiate the action of vivid objects. I know a lady of unqueſtionable veracity, who having, by a deſperate fall, received ſeveral hurts, and particularly a conſiderable one near her eye, had her ſight ſo diſordered, that the next morning, when one of her ſervants came to her bed-ſide, his clothes appeared adorned with ſuch a variety of dazzling colours, as preſently obliged her to command him to withdraw; and even the images in her hangings did, for many days after, appear to her, if the room were not greatly darkened, embelliſhed with ſeveral offenſively vivid colours, which nobody elſe could ſee in them. She ſaid alſo, that ſhe ſometimes thought ſhe ſaw colours ſo new and glorious, that they were of a peculiar kind, and ſuch as ſhe could not deſcribe, by their likeneſs, to any ſhe had ſeen either before or ſince; and that white objects greatly diſordered her ſight; that for ſeveral days after her fall, if ſhe looked upon the inſide of a book, ſhe fancied, ſhe there ſaw colours like thoſe of a rainbow. And even when ſhe ſeemed pretty well recovered, and left her chamber, happening to come into a place where the walls were white, they appeared to her of ſuch dazzling colours as much offended her ſight. She added, that this diſorder of her eyes laſted for five or ſix weeks; tho' ſince that time ſhe hath been able to read and write conſiderably, without finding the leaſt inconvenience. A man of great learning coming to adviſe with me about a diſtemper in his eyes, told me, that having once looked too attentively upon the ſun thro' a teleſcope, without a dark glaſs, the exceſs of light ſo ſtrongly affected his eye, that ever ſince, when he turns it towards a window, or any white object, he fancies he ſees a globe of light about the ſame bigneſs the ſun then appeared to him; tho' it were now ten years ſince he firſt obſerved it. 'Twere eaſy from ſome remarkable ſymptoms, obſerved by *Epiphanius Ferdinandus*, in perſons bitten by the tarantula, to ſhew, that without any change in the object, an alteration in the inſtrument of viſion may, for a great while, make ſome colours appear delightful, and others diſagreeable, and both to a high degree; tho' they had no ſuch effects before. But theſe already mentioned may ſuffice for our preſent purpoſe.

We before obſerv'd, that colour may, notwithſtanding all this, be conſider'd as a quality reſiding in the body ſaid to be colour'd; and indeed moſt of the following experiments refer to it principally under that notion.

For

For there is in colour'd bodies, and chiefly in their ſuperficial parts, a certain diſpoſition whereby they diſturb * the light that comes from them to our eye, ſo as to make that diſtinct impreſſion, upon whoſe account we ſay the viſible body is either white or black, red or yellow. But becauſe we ſhall hereafter more fully ſhew, that the changes, and conſequently the production and the appearance of colours often depend upon the continuation or alteration of the texture of the object, we ſhall here previouſly intimate two or three particulars relating to this matter. And firſt, it is not without reaſon, that I aſcribe colour chiefly to the ſuperficial parts of bodies; for, not to examine how plentifully opake corpuſcles may abound even in thoſe bodies we call diaphanous, it is plain, that we ſee little elſe than the ſuperficies of dark objects; for if we found the rays of light reflected from the object, pierc'd deep into the body, we ſhould not judge it opake, but either pellucid or ſemi-diaphanous †.

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Whether colours depend upon the ſurfaces of bodies.

The ſchools ſeem to teach, that colour is a penetrating quality, reaching to the innermoſt parts of the object; as if a piece of ſealing-wax be broken into ever ſo many pieces, the internal fragments will be as red as the external ſurface. This is indeed a particular example; but it will not overthrow the foregoing doctrine; eſpecially ſince other examples of a contrary nature may be alledged. And two or three negative inſtances are ſufficient to overthrow the generality of a poſitive rule; at leaſt when that is built but upon one, or a few examples. Not then to mention cherries, plums, *&c.* wherein the skin is of one colour, and the inſide of another; I ſhall offer an inſtance or two of the colours of durable bodies, that are thought tolerably homogeneous, and whoſe parts are neither organical, nor of a nature approaching thereto. And firſt, I need only repeat, that blue, red, and yellow may be produced upon a piece of temper'd ſteel: for theſe colours, tho' very vivid, yet if you break the metal they adorn, will appear to be ſuperficial; not only the innermoſt parts of the ſteel, but thoſe within a hair's breadth of its ſuperficies, having none of theſe colours, but retaining that of the metal itſelf. Secondly, we melted a large quantity of pure lead with a ſtrong fire; then immediately pouring it into a clean iron veſſel, and carefully and nimbly

* It follows from Sir *Iſaac Newton*'s diſcoveries, relating to light and colours, that all the productions and appearances of them in the world are derived not from any phyſical change cauſed in light by refraction or reflection, but only from the various mixtures or ſeparations of rays, by virtue of their different refrangibility or reflexibility. And in this reſpect, ſays that great author, " the ſcience of colours becomes a ſpeculation as truly " mathematical as any part of optics, ſo " far as they depend on the nature of " light, and are not produced or altered " by the power of imagination, or by " ſtriking or preſſing the eye." *Newton. Optic. p.* 219.

† Perhaps all opake bodies, when reduced ſufficiently thin or fine, will appear tranſparent, if viewed againſt the light. This evidently happens, when metals are diſſolved in proper menſtrua; when gold is beat thin; when ink aſcends in ſlender glaſs tubes; when an opake ſtone, or other object, is viewed thro' a hole made in the window-ſhutter of a dark room, *&c.*

taking

PHYSICS. taking off the ſcum, we perceiv'd the ſmooth gloſſy ſurface of the melted matter to be adorn'd with a very delightful colour, which almoſt immediately gave place to another vivid one, that was as immediately ſucceeded by a third, and this by a fourth. Thus theſe wonderfully vivid colours ſucceſſively appear'd and vaniſh'd, till the metal cooling, put a ſtop to this pleaſant ſight; tho' the colours which chanced to adorn the ſurface, when the lead began to cool, remain'd upon it, but were ſo ſuperficial, that how little ſoever we ſcraped off from the ſurface of the lead, all the colour came away, and diſcovered only that which is natural to the metal. But unleſs lead be brought to a very high degree of fuſion or fluidity, the phenomena will ſcarce appear. However, the ſame colours did neither always, nor regularly, ſucceed one another, as is uſual in ſteel; but in the following diverſify'd order, which paſſed ſo ſuddenly, that I was ſcarce able to commit it to paper; blue, yellow, purple, blue, green, purple, blue, yellow, red, purple blue, yellow and blue, yellow, blue, purple, green mix'd, yellow, red, blue, green, yellow, red, purple, green. To the ſame purpoſe I might add, that viewing a ſphere of rock cryſtal, which was ſawn aſunder by a lapidary, and expoſing the flat ſurfaces to the ſun beams, the little particles that, notwithſtanding their ſeeming ſmoothneſs in the ſhade, render'd their ſurfaces rough, ſo refracted and reflected the light, as to exceed the vivid colours of the rainbow; but in an interrupted order, ſometimes on one part of the ſurface, ſometimes on another, as it happen'd to be ſituated with regard to the ſun. And having cauſed a fine-grain'd touch-ſtone to be ſawn aſunder by the ſame artificer, I obſerved upon the new ſurfaces, tho' to the touch they ſeem'd ſmooth and poliſh'd, the vivid colours, as above-mentioned, according as the ſurfaces were put into various poſitions, in reſpect of the ſun and the eye; ſo that, notwithſtanding the great tranſparency of the cryſtal, and the great opacity of the touch-ſtone, their ſuperficial corpuſcles were found fit to exhibit the vivid colours we admire in the rain-bow.

The atomiſts of old, and ſome learned men of late, have attempted to explain the variety of colours in opake bodies from the various figures of their ſuperficial parts. The attempt is indeed ingenious, and the doctrine ſeems partly true; but I confeſs other things appear to me neceſſary to be taken in, as contributing to thoſe different forms of aſperity whereon the colours of opake bodies depend. But in order to prove it, we muſt aſſume, that the ſurfaces of all ſuch bodies, how ſmooth ſoever they may appear to our ſight and touch, are only ſo in a popular, or a phyſical ſenſe. The truth hereof is evident from the uſe of microſcopes, which ſhew us in many bodies, that ſeem ſmooth to our naked eyes, little protuberancies riſing above that which may be conceiv'd to be the plain of the ſurface; as alſo numerous depreſſions beneath that level. And of this ſort of cavities we have, by the help of an excellent magnifier, on the ſurface of a thin piece of cork, that appeared ſmooth to the eye, obſerv'd ſixty in a row, within the compaſs of that glaſs, tho' not above $\frac{1}{32}$ of an inch; and theſe too, which made that little piece of cork look almoſt like an empty

empty honey-comb, were not only very diſtinct, and of ſimilar figures, but conſiderably large, and prodigiouſly deep; ſo that their diſtinct ſhadows and ſides were very conſpicuous, and eaſily to be reckoned, and might have been well diſtinguiſhed, had they been ten times leſs than they were: whence we may make ſome eſtimate, what a ſtrange inequality, and what a multitude of little ſhades there may really be in a ſcarce ſenſible part of a phyſical ſuperficies, tho' they remain inviſible to the naked eye. There are alſo ſeveral experiments which confirm the ſame thing in other like ſubſtances: ſo that every ſenſible part of an opake body, may be conceiv'd to conſiſt of a multitude of corpuſcles ſingly inſenſible. But in giving theſe ſurfaces a diſpoſition to alter the light reflected thence to the eye, after the manner requiſite to make the object appear colour'd, the figures of theſe particles have a great, tho' not the only ſhare. 'Tis true indeed, that the protuberant particles may be of very various figures, ſpherical, elliptical, conical, *&c.* according to the nature whereof, and the ſituation of the lucid body, the light muſt be variouſly affected; as, after one manner from ſurfaces conſiſting of ſpherical, after another from thoſe made up of conical or cylindrical particles; ſome being fitted to reflect more of the incident rays * of light, others leſs; and ſome towards one part, others towards another. But beſides this difference of figure, many other things may greatly concur to vary the forms of aſperity, whereon colours have ſo great a dependance: for allowing the figure of the particles, the ſuperficial ones may be bigger in one body, and leſs in another; and conſequently fitted to allay the light falling on them with greater ſhades. The protuberant particles may alſo be ſet at a greater or a ſmaller diſtance from one another in different parts; and how theſe qualities may ſerve to produce colour, we may gueſs from what happens in the agitation of water; for if the bubbles that are thereby made, be large and few, the water will ſcarce acquire a ſenſible colour; but if it be reduced to a froth, which conſiſts of bubbles very minute, and contiguous to each other, it exhibits a very manifeſt whiteneſs. Beſides, it is not neceſſary that the ſuperficial particles which exhibit one colour, ſhould be all of the ſame ſhape; but different figures may be mixed upon the ſurface of the opake body: as when the corpuſcles that make a blue colour, and thoſe that make a yellow, come to be accurately and skilfully united, they afford a green; which tho' it ſeem one ſimple colour, yet in this caſe appears to be made by the union of corpuſcles of very different kinds. Moreover, the figure and magnitude of the little depreſſions, cavities, furrows, or pores, intercepted betwixt theſe protuberant corpuſcles, are as well to be conſider'd, as the ſizes and ſhapes of the corpuſcles themſelves.

* Sir *Iſaac Newton* proves, that the cauſe of reflexion is not the impinging of light on the ſolid or impervious parts of bodies, as is commonly ſuppoſed; and makes it probable, that the reflexion of a ray is effected not by a ſingle point of the reflecting body, but by ſome power of the body which is evenly diffuſed all over its ſurface, and by which it acts upon the ray, without immediate contact. *Newton Optic.* p. 237—241.

PHYSICS. ſelves. For we may conceive the phyſical ſuperficies of a body to be horizontally cut by a mathematical plain; and then, as ſome parts of that phyſical ſuperficies will be protuberant, or ſwell above the plain, ſo others may be depreſſed beneath it. Thus in many places of the earth's ſurface, there are hills, trees, *&c.* rais'd above the horizontal level of the valley; and rivers, wells, pits, and other cavities, depreſſed beneath it. And that ſuch protuberant and concave parts of a ſurface may remit the lights ſo differently as greatly to vary a colour, we ſhall ſee hereafter. But at preſent it may ſuffice to ſay, that the different degrees or kinds of aſperity in the two flat ſides of the ſame piece of red marble, the one whereof is poliſhed, and the other left rough, will ſo diverſify the light reflected from the ſeveral different plains to the eye, that a painter would employ two different colours to repreſent them. The ſituation alſo of the ſuperficial particles is conſiderable. This I diſtinguiſh into the poſture of the ſingle corpuſcles, in reſpect of the light, and of the eye; and the order of them in regard to one another: for a body may reflect the light otherwiſe, when its ſuperficial particles are more erect upon the plain conceiv'd to paſs along their baſis; and when the extremities of ſuch particles are obverted to the eye, than when thoſe particles lie ſo inclined, that their ſides are in great meaſure diſcernible. Thus the colour of pluſh or velvet will appear various, if you ſtroak part of it one way, and part another; the poſture of the particular threads in regard to the light, or the eye, being thereby varied. And 'tis obſervable, that in a field of ripe corn, blown upon by the wind, there will appear waves of a colour different from that of the reſt of the corn; becauſe the wind, by depreſſing ſome of the ears more than others, cauſes one to reflect more light from the lateral and ſtrawy parts than another. Thus when dogs are ſo enraged, as to erect the hairs upon their necks, and other parts of their bodies, thoſe parts ſeem to acquire a colour different from that of the ſame hairs, when, in their uſual poſture, they lay more inclined. And that the order wherein the ſuperficial corpuſcles are ranged is conſiderable, we may gueſs by the turning of water into froth, the beating of glaſs, and the ſcraping of horn; in which caſes the corpuſcles that were before ſo marſhall'd, as to be tranſparent, become, by a diſturbance of that order, diſpoſed to ſtop and reflect more light, and thereby to appear whitiſh.

There are alſo other ways wherein the order of the protuberant parts, in regard to the eye, may greatly contribute to the appearance of a particular colour: for I have often obſerved, that when peaſe have been planted or ſet in parallel lines, and are ſhot up about half a foot above the ſurface of the ground, by looking on the field where they grew along the lines they ſtood in, much the greater part of the ground appeared of its own dirty colour; but if I view'd the field tranſverſly, it would appear very green; the upper parts of the peaſe concealing the intercepted parts of the ground from the eye. And perhaps even the motion of the ſmall parts of a viſible object may in ſome caſes contribute, tho' it be not eaſy to ſay how, to the production or variation of a colour: for I have ſeveral times made

made a liquor, which, when well ſettled in a cloſe vial, is tranſparent and colourleſs; but as ſoon as the glaſs is unſtopped, begins to fly away very plentifully in a white, opake fume: and there are other bodies, whoſe fumes, when they fill a receiver, would make one ſuſpect it contain'd milk; and yet theſe fumes become a liquor that is not white, but tranſparent. And ſuch white fumes I have ſeen afforded by unſtopping a liquor, which is itſelf diaphanous and red; nor are theſe the only inſtances of this kind that our experiments can ſupply us with. And if the ſuperficial corpuſcles be of the groſſer ſort, and ſo framed, that their differing ſides may exhibit differing colours, then the motion or reſt of thoſe corpuſcles may be conſiderable, as to the colour of the ſuperficies they compoſe; becauſe ſometimes more, ſometimes fewer of the ſides diſpoſed to exhibit ſuch a colour, may by this means become or continue more obverted to the eye than the reſt, and compoſe a phyſical ſurface more or leſs ſenſibly interrupted. Thus I remember, that in ſome ſorts of plants, whoſe leaves were thick ſet by one another, and the two ſides of each leaf of a ſomewhat different colour, there would appear a notable difference in their colour, if look'd upon, firſt when the leaves being at reſt, had their true upper ſides obverted to the eye; and again, when a breath of wind paſſing thorough them, made great numbers of the ſides of the leaves, that are uſually hidden, become conſpicuous. And tho' the little bodies above-mentioned, may ſingly and a-part ſeem almoſt colourleſs; yet when many of them are placed by one another ſo near, that the eye does not diſcern an interruption in a ſenſible ſpace, they may exhibit a colour: as we ſee, that tho' a ſlender thread of dy'd ſcarlet ſilk, whilſt look'd on ſingly, ſeems almoſt quite devoid of redneſs; yet when numbers of theſe threads are brought together into a skein, that colour then becomes glaring. And in changeable taffaties, where we ſee differing colours ariſe and vaniſh upon ruffling the ſame piece of ſilk; I have often, with pleaſure, obſerved, by means of a convenient microſcope, their component threads to paſs under and over each other, in almoſt innumerable points: and if I thus look'd upon any conſiderable portion of the ſtuff that, for example, appeared red to the naked eye, I could plainly ſee, that in ſuch a poſition the red threads were conſpicuous, and reflected a vivid light: and tho' I could alſo perceive that there were green ones, yet, by reaſon of their diſadvantageous poſition in the phyſical ſurface of the ſilk, they were in part hid by the more protuberant threads of the other colour; and, for the ſame reaſon, the reflection from as much of the green as was diſcover'd, appear'd but dim and faint. And if, on the contrary, I look'd thro' the microſcope upon any part that appear'd green, I could plainly ſee the red threads were leſs fully expoſed to the eye, and obſcured by the green ones, which, therefore, made the predominant colour. And by obſerving the texture of the ſilk, I could eaſily ſo expoſe the threads either of the one colour or the other to my eye, as at pleaſure to exhibit the appearance of red or green, or make thoſe colours ſucceed each other: ſo that, obſerving their ſucceſſion, I could mark how the predominant colour ſtarted up, when the threads that exhibited it,

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A blind man, who diſtinguiſhed colours by the touch.

* Sir *Iſaac Newton* ſeems to be of opinion, that microſcopes may be ſo far improved, if they are not, in ſome degree, already arrived at that perfection, as to diſcover all the particles of bodies on which their colours depend, except thoſe of black ſubſtances, which he ſhews to be the moſt minute. *Newton. Optic.* p. 236.

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age, who, when he was two years old, had the ſmall pox, which render'd him abſolutely blind, tho' he is at preſent an organiſt in a public choir. The doctor diſcourſing with him over night, the blind man affirmed, that he could diſtinguiſh colours by feeling, but not unleſs he were faſting; for that any quantity of drink deprived him of that exquiſite touch which is requiſite to ſo nice a ſenſation. Upon this, the doctor provided againſt the next morning ſeven pieces of ribbon of theſe ſeven colours, black, white, red, blue, green, yellow, and grey; but as for mixed colours, this *Vermaaſen* would not undertake to diſcern them; tho, if offer'd, he could tell that they were mixed. To diſcern the colour of the ribbon, he places it betwixt his thumb and his fore-finger, but his moſt exquiſite perception is in his thumb, and much better in the right than in the left. After the man had four or five times told the doctor the ſeveral colours, whilſt a napkin was tied over his eyes, the doctor obſerved he twice miſtook, for he called the white black, and the red blue; but ſtill before his error, he would lay them by in pairs, ſaying, that tho' he could eaſily diſtinguiſh them from all others, yet thoſe two pair were not eaſily diſtinguiſhable from one another. Then the doctor deſired to know what kind of difference he found in colours by his touch. To which the blind man reply'd, that all the difference he obſerved, was a greater or leſs degree of aſperity †; for, ſays he, black feels like the points of needles, or ſome harſh ſand, whilſt red feels very ſmooth; that black and white are the moſt rough, or unequal, of all colours, and ſo like, that 'tis very hard to diſtinguiſh them; but black the rougheſt of the two: that green is next in aſperity; grey next to green; yellow the fifth in degree of aſperity; red and blue ſo alike, that 'tis as hard to diſtinguiſh between them as between black and white, tho' red be ſomewhat more rough than blue; ſo that red has the ſixth place, and blue the ſeventh in aſperity. To this information the doctor was pleaſed to add the preſent of three of thoſe very pieces of ribbon, whoſe colours, in his preſence, *Vermaaſen* had diſtinguiſhed; pronouncing the one grey, the other red, and the third green; which I keep by me as rarities. Before I ſaw the notes from whence this account is taken, I confeſs I ſuſpected this blind man might have thus diſtinguiſhed colours rather by the ſmell than by the touch; for ſome

† The different magnitudes of the parts of coloured bodies, whereon their different colours, according to Sir *Iſaac Newton*'s theory, depend, appear to be too minute for the ſenſe of feeling to take notice of. This ſenſe, indeed, may, by great degrees, be more exquiſite in one man than in another; and, if it could be ſuppoſed equal, or nearly equal to that of ſight, which perhaps is only a finer degree of touch, this different aſperity might, ſuppoſing the truth of the relation, become ſenſible to one, poſſeſſed of an exquiſite ſenſe of feeling. Sir *Iſ. Newton* well obſerves, that there is a harmony and diſcord of colours, which he ſeems to think ariſes from the proportions of the vibrations propagated thro' the fibres of the optic nerve into the brain, as the harmony and diſcord of ſounds ariſe from the proportions of the vibrations of the air. For ſome colours, if they be viewed together, are agreeable to one another, as thoſe of gold and indigo, and others diſagree. See *Newton. Optic.* p. 320.

PHYSICS. of the ingredients employ'd by dyers, have different and ſtrong ſcents, which a very nice noſe might diſtinguiſh; and this I the rather ſuſpected, becauſe he required that the ribbons he was to judge of, ſhould be offer'd him in the morning faſting; for I have obſerved in ſetting-dogs, that the feeding of them greatly impairs their ſcent. And, indeed, I could wiſh for the opportunity of examining this man myſelf, about many particulars which I do not find to have been yet thought upon. And tho' it be not incredible, that ſince the liquors uſed by dyers are qualified to give a colour, by multitudes of little corpuſcles of the pigment diſſolved and extracted by the fluid wherein they float, thoſe corpuſcles of colour inſinuating themſelves into, and filling up the pores of the body to be dyed, may give a greater or leſs roughneſs to its ſuperficies, according to the bigneſs and texture of the particles of the pigment; yet I can ſcarce believe that this blind man could diſtinguiſh all the colours he did, merely by the ribbons having more or leſs of aſperity: but the forms of it might alſo aſſiſt him herein, tho' theſe would perhaps have been very difficult for him to deſcribe; becauſe thoſe minute differences having not been taken notice of by men for want of a touch ſufficiently exquiſite, are things he could not have expreſſed intelligibly. Thus under the name of ſharp, ſweet, and ſour, there are abundance of peculiar intermediate reliſhes, or taſtes, in different ſorts of wine; which tho' critical and experienc'd palates eaſily diſcern, yet the judges cannot have their idea's convey'd to others; ſuch minute differences not having hitherto any diſtinct names aſſign'd them. And it ſeems that there was ſomething here in the forms of aſperity requiſite to the diſtinction of colours, beſides the degree of it; ſince this man found it ſo difficult to diſtinguiſh black and white from one another, tho' not from other colours. For I might urge, that he ſeems to contradict himſelf about the red, which in one place he repreſents as ſomewhat more rough than the blue, and in another very ſmooth; but becauſe he ſpeaks of this ſmoothneſs in that place where he mentions the roughneſs of black, we preſume that he meant but a comparative ſmoothneſs: however, he found it difficult not only to diſtinguiſh red and blue, but black and white from one another, tho' not from other colours. And indeed, tho' in the ribbons offer'd him, they might be almoſt equally rough, yet in ſuch minute corpuſcles as thoſe of colour, there may eaſily be conceiv'd not only a greater cloſeneſs of parts, a want of protuberant corpuſcles, and little extant particles otherwiſe figured and ranged in the white than in the black; but the cavities alſo may be much deeper in one than in the other. And perhaps, where the particles are ſo exceeding ſlender, we might allow the parts expoſed to the ſight and touch to be a little convex in compariſon of the erect particles of black bodies; as if they were wires many times ſlenderer than a hair, for whether you ſuppoſe them to be figured like needles, or cylindrically like the hairs of a bruſh, with hemiſpherical tops, they will be ſo very ſlender, and conſequently the points both of the one ſort and the other, ſo very ſharp, that an exquiſite touch will be able to obſerve no greater difference between

them,

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them, than that which our blind man allowed, when, comparing black and white bodies, he ſaid, that the latter were the leſs rough of the two. Nor is every kind of roughneſs, tho' ſenſible, inconſiſtent with whiteneſs, for the phyſical ſuperficies of a body is ſometimes made, by the ſame operation, both rough and white; as when the level ſurface of clear water being agitated and made rough, with a multitude of unequal bubbles, thereby acquires a whiteneſs; and as a ſmooth piece of glaſs by being ſcratched with a diamond, diſcloſes the ſame colour. This minuteneſs and figure of the extant particles being ſuppoſed, the depth of the little cavities intercepted between the extant particles, without being ſo much greater in black bodies than in white ones, as to be very perceptible to the groſs organs of touch, may be vaſtly greater with regard to their diſpoſition of reflecting the ſubtile rays of light. For in black bodies thoſe ſmall intercepted cavities, and other depreſſions, may be ſo figured, ſo narrow and ſo deep, that the incident rays of light, which the more extant parts of the phyſical ſuperficies are diſpoſed to reflect inwards, may be detained there, and prove unable to emerge; whilſt in a white body the ſlender particles may not only, by their figure, be fitted to reflect the light copiouſly outwards, but the intercepted cavities being ſhallow, and perhaps ſomewhat wide, the bottoms of them may be ſo conſtituted, as to be fit to reflect outwards much of the light that falls upon them.

There is no neceſſity of concluding, from the blind man's relation, that tho', according to his touch, black was the rougheſt, as it is the darkeſt of colours, that therefore white, which is the lighteſt, ſhould alſo be the ſmootheſt; for he makes yellow to be two degrees rougher than blue, and as much ſmoother than green; tho' yellow not only appears to the eye a lighter colour than blue, but we ſhall hereafter ſhew, that yellow reflects much more light than blue, and manifeſtly more than green; which ſeems to ſtrengthen our conjecture, that there was ſomething in the kinds of aſperity, as well as in the degrees of it, that enabled *Vermaaſen* to diſtinguiſh colours; and, at leaſt ſhews, that we cannot, in all caſes, from the bare difference in the degrees of aſperity in colours, ſafely conclude that the rougher reflects the leaſt light. Thus much, however, we gain from the teſtimony of this blind man, that ſince many colours may be felt with the circumſtances above related, the ſurfaces of ſuch coloured bodies muſt certainly have different degrees, and in all probability different forms, or kinds of aſperity belonging to them; which is all the uſe I here deſigned to make of the hiſtory of this blind man: for hence it ſufficiently appears, that colour greatly depends upon the diſpoſition of the ſuperficial parts of bodies, and, probably, wherein ſuch a diſpoſition principally conſiſts *.

But

* The permanent colours of natural bodies ariſe from hence, that ſome ſuch bodies reflect ſome ſorts of rays, and others other ſorts more copiouſly than the reſt. " Minium, ſays Sir *Iſaac Newton*, reflects the leaſt refrangible, or " red-making rays moſt copiouſly, and " thence appears red. Violets reflect the " moſt refrangible, moſt copiouſly, and " thence have their colour, and ſo of " other

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How changes are produced in colours, by liquors.

But to return; the causes of the several forms of asperity, that may diversify the surfaces of coloured bodies, will, perhaps, afford us some general conjectures as to several of the ways whereby 'tis possible to produce the sudden changes of colours, we shall hereafter find in our experiments; for most of those phenomena being produced by means of liquors, which generally abound with very minute, active, and variously figured saline corpuscles, they may well very suddenly alter the texture of the body they are employed to work on; and so change their form of asperity, and thereby make them reflect to the eye the light that falls on them, after another manner than they did before, and by that means vary the colour so far as it depends upon the texture or disposition of

" other bodies. Every body reflects the " rays of its own colour more copiously " than the rest, and from their excess " and predominance in the reflected light " has its colour.——Every body appears " most splendid and luminous when view- " ed in the light of its own colour. Cin- " nabar, in the homogeneal, red, prisma- " tic light is most resplendent, in the " green light it is manifestly less resplen- " dent, and in the blue light still less. " Indigo in the violet blue light, is most " resplendent, and its splendor is gra- " dually diminished, as it is removed " thence by degrees thro' the green and " yellow light to the red. By a leek " the green light, and next that the blue " and yellow, which compound green, " are more strongly reflected than the " other colours, red and violet, *&c.* But " to make these experiments the more " manifest, such bodies ought to be cho- " sen as have the fullest and most vivid " colours, and two of those Bodies are " to be compared together. Thus, for " instance, if cinnabar and ultramarine " blue, or some other full blue be held " together in the red homogeneal light, " they will both appear red, but the " cinnabar will appear of a strongly lu- " minous and resplendent red, and the " ultramarine blue, of a faint, obscure, " and dark red; and if they be held to- " gether in the blue homogeneal light, " they will both appear blue, but the ul- " tramarine will appear of a strongly lu- " minous and resplendent blue, and the " cinnabar of a faint and dark blue; " which puts it past dispute, that the " cinnabar reflects the red light much " more copiously than the ultramarine " doth, and that the ultramarine reflects " the blue light much more copiously " than the cinnabar doth. The same expe- " riment may be made successfully with " red lead and indigo, or with any other " two coloured bodies, if due allowance " be made for the different strength or " weakness of their colour and light. " And since these reflected lights, which " differ in colour, differ also in degree " of refrangibility, it's certain that some " bodies reflect the more refrangible, " and others the less refrangible rays " more copiously; and that this is the " only reason of these colours, appears " from hence, that the colour of homo- " geneal light cannot be changed by the " reflexion of natural bodies. For if " bodies, by reflexion, cannot in the " least change the colour of any sort of " rays, they cannot appear coloured by " any other means, than by reflecting " those which are either of their own " colour, or which, by mixture, must " produce it. But in making experi- " ments of this kind, care must be had " that the light be sufficiently homoge- " neal." *Newton. Optic.* p. 156——158.

" In transparently coloured liquors, " 'tis observable, that their colour uses " to vary with their thickness. Thus a " red liquor, in a conical glass, held be- " tween the light and the eye, looks of " a pale and dilute yellow at the bot- " tom where 'tis thin, and a little higher " where 'tis thicker, grows orange; and " where 'tis still thicker, becomes red; " and where 'tis thickest, the red is " deepest and darkest. For it is to be " conceived, that such a liquor stops the " indigo-making and violet-making rays " most of the visible parts of the object; for I do not absolutely exclude all other ways of modifying the rays of light, between their parting from the lucid body, and their reception into the common organ of vision. Now there seem to be several ways whereby liquors may suddenly alter the colour of one another, but I shall only touch upon a few. And, first, the minute corpuscles that compose a liquor may easily insinuate themselves into those pores of bodies whereto their size and figure adapt them; and these pores they may either exactly or inadequately fill; in which latter case they will, for the most part, alter the number and figure, but always the

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" most easily, the blue-making rays more " difficultly, and the red-making most " difficultly; and that if the thickness " of the liquor be only so much as suffi- " ces to stop a competent number of the " violet-making and indigo-making rays, " without diminishing much the num- " ber of the rest, the rest must compound " a pale yellow. But if the liquor be " so much thicker, as to stop also a great " number of the blue-making rays, and " some of the green-making, the rest must " compound an orange; and where it is " so thick as to stop also a great number " of the green-making, and a considera- " ble number of the yellow-making, the " rest must begin to compound a red, " and this red must grow deeper and " darker, as the yellow-making and " orange-making rays are more and more " stopped by the increasing thickness of " the liquor; so that few rays besides " the red-making can get through.

" Of this kind is an experiment lately " related to me by Dr. *Halley*, who, div- " ing deep into the sea in a diving ves- " sel, found, in a clear sunshine day, " that when he was sunk many fathoms " deep into the water, the upper part of " his hand, on which the sun shone di- " rectly thro' the water, and thro' a " small glass-window in the vessel, like " that of a damask rose, and the water " below and the under part of his hand " illuminated by light reflected from the " water below, looked green. From " thence it may be gathered, that the " sea-water reflects back the violet and " blue-making rays most easily, and lets " the red-making pass most freely and " copiously to great depths; for there- " by the sun's direct light, at all great " depths, by reason of the predominating " red-making rays must appear red, and " the greater the depth is, the fuller " and intenser must that red be. And at " such depths as the violet-making rays " can scarce penetrate to, the blue-mak- " ing, green-making and yellow-mak- " ing rays being reflected from below " more copiously than the red-making " ones, must compound a green. Now " if there be two liquors of full co- " lours, suppose a red and a blue, and " both of them so thick as suffices to " make their colours sufficiently full, " tho' either liquor be sufficiently trans- " parent a-part, yet will you not be able " to see thro' both together; for if only " the red-making rays pass thro' one li- " quor, and only the blue-making thro' " the other, no rays can pass thro' both. " This Dr. *Hook* tried casually with glass " wedges, filled with red and blue li- " quors, and was surprized at the event.

" Now whilst bodies become coloured, " by reflecting or transmitting this or " that sort of rays more copiously than " the rest, it is to be conceived that " they stop and stifle in themselves the " rays which they do not reflect or trans- " mit. For if gold be foliated, and held " between the eye and the light, the " light looks of a greenish blue; and " therefore massy gold lets into its body " the blue-making rays, to be reflected " to and fro within it, till they be stop- " ped and stifled, whilst it reflects the " yellow-making outwards, and thereby " looks yellow. And much after the " same manner that leaf-gold is yellow " by reflected, and blue by transmitted " light, and massy gold is yellow in all " positions of the eye; there are some " liquors, as the tincture of *Lignum Ne- " phriticum*, and some sorts of glass, " which

PHYSICS. the magnitude of the former pores. And in what capacity ſoever theſe corpuſcles of a liquor come to be lodged or harboured in ſuch pores, the ſurface of the body will commonly have its aſperity altered, and the incident light, that meets with a groſſer liquor in the little cavities that before contained nothing but air, or ſome more ſubtile fluid, will have its rays either refracted, abſorbed, or reflected more or leſs irregularly than if the body had been dry. Thus we ſee that even fair water falling on white paper, linen, *&c.* will, for ſome ſuch reaſon as thoſe aſſigned, immediately alter the colour of them, and for the moſt part render it deeper than that of the unmoiſten'd parts of the ſame bodies. In like manner, when, during the ſummer, the high-ways are dry and duſty, if there fall much rain, they quickly appear of a darker colour than before. Thus alſo if a drop of oil be let fall upon white paper, that part of the paper which by imbibing the liquor acquires a greater continuity, and ſome tranſparency, will appear much darker than the reſt; many of the incident rays of light being now tranſmitted, that would, otherwiſe, be reflected to the eye.

Secondly, a liquor may alter the colour of a body, by freeing it from thoſe things that prevent its appearing of its genuine colour; and tho' this may be ſaid to be rather a reſtoration of a body, or a reſtitution of

" which tranſmit one ſort of light moſt " copiouſly, and reflect another ſort, " and thereby look of ſeveral colours, " according to the poſition of the eye to " the light. But if theſe liquors or " glaſſes were ſo thick and maſſy, that " no light could get thro them, I queſtion not but they would, like all other " opake bodies, appear of one and the " ſame colour in all poſitions of the eye. " For all coloured bodies, ſo far as my " obſervation reaches, may be ſeen thro, " if made ſufficiently thin, and therefore " are in ſome meaſure tranſparent, and differ only in degree of tranſparency from " tinged and tranſparent liquors; theſe liquors, as well as thoſe bodies, by a ſufficient thickneſs becoming opake. A tranſparent body, that looks of any colour " by tranſmitted light, may alſo look of " the ſame colour by reflected light, the " light of that colour being reflected by " the farther ſurface of the body, or by " the air beyond it. And then the reflected colour will be diminiſhed, and " perhaps ceaſe, by making the body very thick, and pitching it on the backſide, to diminiſh the reflexion of its " farther ſurface, ſo that the light reflected from the tinging particles may " predominate. In ſuch caſes the colour " of the reflected light will be apt to vary from that of the light tranſmitted." *Newton. Optic. p.* 159—163.

This doctrine is farther confirmed, by mixing coloured light, ſo as to compound a beam of light of the ſame colour and nature with a beam of the ſun's direct light, and making experiments therein. Thus Sir *Iſaac Newton* found, that ſuch light is endued with all the properties of a beam of the ſun's light; natural bodies appearing in it of the ſame colours they have in day-light. Thus cinnabar appeared of the ſame red colour in it as it does by day, and if the green-making and blue-making rays were ſtopped, its redneſs became more full and lively; but if the red-making rays were intercepted, it became yellow or green, or of ſome other colour, according to the ſorts of rays which were not intercepted. So gold in this light appears of the ſame yellow colour as in day-light; but by ſtopping a due quantity of the yellow-making rays, it appears white like ſilver; which ſhews, that its yellowneſs ariſes from the exceſs of the intercepted rays tinging that whiteneſs with their colours when they are let paſs. See *Newton. Optic. p.* 163—167.

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its native colour, than a change; yet there here happens a change of the colour which the body afforded before this operation. And ſuch a change a liquor may work, either by diſſolving or corroding, or by ſome ſuch means carrying off the matter, which either veiled or diſguized the colour that afterwards appears. Thus we reſtore old pieces of dirty gold to a clear ſhining yellow, by putting them into the fire, and into *Aqua fortis*, which takes off the acquired filth from the metal; and there is alſo an eaſy way to reſtore ſilver coins to their due luſtre, by fetching off that which diſcoloured them. I have likewiſe a chymical liquor, which I employ to reſtore pieces of cloth ſpotted with greaſe, to their proper colour, by imbuing the ſpotted part therewith; which incorporating with the greaſe, and yet being of a very volatile nature, eaſily carries it away with itſelf. And I have ſometimes try'd, that by rubbing upon a good touchſtone a certain metalline mixture ſo compounded, that the impreſſion it left upon the ſtone appeared of a very different colour from that of gold; yet a little *Aqua fortis* would immediately make the golden colour diſcloſe itſelf, by diſſolving only the other metalline corpuſcles that concealed thoſe of the gold.

Thirdly, a liquor may alter the colour of a body, by comminuting its parts; and principally after two ways: Firſt, by disjoining and diſſipating thoſe cluſters of particles which ſtick more looſely together, only by means of ſome cement that is eaſily diſſoluble; and this ſeems to be the caſe in ſome of the following experiments, where the colour of many corpuſcles brought to cohere, by being precipitated together, is deſtroyed by the affuſion of very ſharp and piercing liquors. Secondly, By dividing the groſſer and more ſolid particles into minute ones, which will be, for the moſt part, otherwiſe ſhaped than the entire corpuſcles ſo divided; as happens in a piece of wood reduced to ſplinters or chips, or when a piece of cryſtal heated red-hot, and quenched in cold water, cracks into a multitude of little fragments, which, tho' they do not fall aſunder, alter the diſpoſition of the body of it, as to its manner of reflecting the light.

There is a fourth way oppoſite to the third, whereby a liquor may change the colour of another body, eſpecially a fluid, by procuring the coalition of ſeveral particles that before lay ſcattered, and too remote from each other to exhibit the colour that afterwards appears. Thus, when a ſolution of gold has been ſo diluted, that I queſtioned whether the liquor really imbibed any of the metal, I have been quickly ſatisfied it did, by pouring a little mercury into it; for the gold therein would ſoon clothe the ſurface of the quickſilver with a thin film of its own livery. And by this way of bringing the minute parts of bodies together, in ſuch numbers as to make them a fit object for the eye, many of thoſe colours ſeem to be generated which are produced by precipitation; eſpecially if made with fair water; as when reſinous gums, diſſolved in ſpirit of wine, are let fall again, if the ſpirit be greatly diluted with that weakening liquor. And thus out of the rectified and tranſparent butter of antimony, there will, by the bare mixture of fair water, be plentifully precipitated

 that

PHYSICS. that milk-white ſubſtance, which, by having its looſer ſalts well waſhed off, is turned into *Mercurius vitæ*.

A fifth way wherein a liquor may change the colour of a body, is by diſlocating its parts, and changing their order; at the ſame time, perhaps, alſo altering the poſition of the ſingle corpuſcles, as well as their ſituation in reſpect of one another. What ſhare particular commotions or diſlocations of the parts of a body may have in changing its colour, is evident from the mutations of that quality obſervable in quickſilver, and ſome other concretes, long kept by chymiſts in a convenient heat, tho' in cloſe veſſels; and alſo in the obvious degenerations thereof in bruiſed cherries, and other fruit: and that ſuch liquors as we have been ſpeaking of, may greatly diſcompoſe the textures of many bodies, and thereby alter the diſpoſition of their ſuperficial parts, the great commotion made in metals and other ſubſtances by *Aqua fortis*, oil of vitriol, *&c.* eaſily perſuade us. And what ſuch a varied ſituation of parts may do towards diverſifying the manner wherein they reflect the light, may be gueſſed, in ſome meaſure, by the pulverization of tranſparent glaſs, which thereby becomes a white powder; but ſtill better, by the experiments we ſhall hereafter ſet down, with relation to the production and deſtruction of colours by ſubtile, ſaline liquors. And in ſome chymical oils, as particularly in that of lemon-peel, the tranſpoſition of the parts, conſequent upon bare concuſſion, will repreſent on the ſurfaces of the bubbles thereby occaſioned, exceeding lively colours, which, when the bubbles relapſe into the reſt of the oil, immediately vaniſh. I know not whether I ſhould mention, as a diſtinct method, that power whereby a liquor may alter the colour of another body, by putting the parts of it into motion; for tho', poſſibly, the motion ſo produced, ſeldom, of a ſudden, changes the colour of the body whoſe parts are agitated; yet this ſeems to be one of the moſt general, tho' not immediate cauſes of the quick change of colours in bodies; for the parts being put into motion by the adventitious liquor, many of them, that were before united, may be thereby disjoined; and when that motion ceaſes or decreaſes, others of them may ſtick together in a new order: by which means the motion may ſometimes produce permanent changes of colours; as in the experiment we ſhall hereafter mention, where a ſnow-white body is immediately turned into a yellow, by the bare affuſion of fair water. And tho when you rub good blue vitriol upon the clean blade of a knife, it will not impart its latent colour thereto; yet if you moiſten that ſubſtance with common water, the particles of the liquor disjoining thoſe of the ſtone, and thereby giving them the various agitation requiſite to fluid bodies, the metalline corpuſcles of the vitriol thus diſſolved will lodge themſelves in throngs in the ſmall pores of the iron, and give the ſurface of it the genuine colour of copper.

Laſtly, The moſt important way, by which a liquor may alter the colour of another body, is by aſſociating the ſaline, or other more rigid corpuſcles of the liquor, with the particles of the body whereon it is employ'd to work. For theſe adventitious corpuſcles uniting with the protuberant

tuberant particles of the ſurface of a coloured ſubſtance, muſt neceſſarily alter their bigneſs, and moſt commonly their ſhape. And that the colours of bodies greatly depend upon the bulk and figure of their ſuperficial particles, ſeems probable, ſince many ancient and modern philoſophers have thought, that all colours might, in the general, be accounted for from them alone. The diverſification hereof will, in our caſe, be attended with theſe two circumſtances; the one, that the protuberant particles being increaſed in bulk, muſt often be varied as to the cloſeneſs or laxity of their order; fewer of them being contained within the ſame ſenſible ſpace than before; or elſe, by approaching one another, they muſt ſtreighten the pores; and perhaps too by the manner of their aſſociating themſelves with the protuberant particles, intercept new pores. And thus, alſo, theſe adventitious corpuſcles may produce a great change, as well in the little cavities, as in the protuberances of a coloured body; for they may likewiſe, by lodging themſelves in thoſe little cavities, fill them up; and it may eaſily happen, that they ſhall not only fill the pores they inſinuate themſelves into, but have their upper parts extant above them: and partly by ſuch new protuberances, partly by increaſing the bulk of the former, theſe extraneous corpuſcles may much alter the number and bigneſs of the ſuperficial pores, changing the old ones, and intercepting new; and very probably the order of the little extancies, and conſequently that of the little depreſſions, in point of ſituation will be altered likewiſe. Thus if you diſſolve quickſilver in ſome kinds of *Aqua fortis*, the ſaline particles of the menſtruum aſſociating themſelves with the mercurial globules, will make a green ſolution, which afterwards eaſily degenerates; and red lead, diſſolved in ſpirit of vinegar, yields a clear ſolution, the redneſs of the lead being deſtroyed by the liquor. But a better inſtance is afforded from copper; for I have try'd, that if upon a copper plate you let ſome drops of weak *Aqua fortis* reſt for a while, the corpuſcles of the menſtruum joining with thoſe of the metal, will produce a very ſenſible aſperity upon the ſurface of the plate, and thence coagulate into very minute grains of a pale blue vitriol; tho', if upon another part of the ſame plate you ſuffer a little ſtrong ſpirit of urine to remain for a competent time, the rough ſurface will be adorned with a deeper and richer blue. And the ſame *Aqua fortis*, that ſuddenly changes the redneſs of minium into a darker colour, will, being put upon crude lead, produce a whitiſh ſubſtance, as with copper it did a bluiſh, and as with iron it gives a reddiſh, and on white quills a yellowiſh colour: ſo much may the coalition of the parts of the ſame liquor, with the differently figured particles of ſolid bodies ſeveral ways turn the differingly diſpoſed ſurfaces rough, and diverſify the colour of thoſe bodies. And 'twill be eaſily believed, that in many changes of colour, which happen upon the diſſolutions of metals, and precipitations made with oil of tartar, *&c.* a coalition of ſaline corpuſcles may be made with the particles of the body diſſolved or precipitated; if we conſider how much the vitriol of a metal may be heavier than the pure metalline part thereof, upon account of the

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But I am not aiming to eſtabliſh a particular theory of colours, only deſign to offer ſome experiments and obſervations on the ſubject of them. I ſhall, therefore, add but two particulars more, with relation to the forms of ſuperficial aſperity in coloured bodies. And firſt, I ſay, that there are many other means whereby true and permanent colours may be produced in bodies, beſides thoſe practicable by the help of liquors; for proof whereof ſeveral examples might be alledged; but I need only refer to what we have already obſerved of the change of colours ſuddenly made on tempered ſteel and lead, by the operation of heat, without the interpoſition of a liquor. Secondly, I obſerve, what is of more importance to our preſent ſubject, that tho' nature and art may, in ſome caſes, ſo change the aſperity of the ſuperficial parts of a body, as to alter its colour by any of the mentioned ways a-part, yet 'tis generally by two or three, or perhaps by more of them together, that the effect is produced; and if it be conſidered how variouſly thoſe ſeveral ways, with others of the like nature, may be compounded and applied, it will not appear ſurprizing, that ſuch fruitful principles ſhould be fitted to change or generate many differing colours.

Whether all bodies are tranſparent?

Hitherto we have conſidered the little protuberances, or other ſuperficial particles which conſtitute the roughneſs of bodies, as if we took it for granted that they muſt be perfectly opake, and impenetrable to the rays of light, and ſo contribute to the variety of colours, as they ſtop more or leſs thereof, and reflect them to the eye particularly mixed with more or leſs of ſhade. I have often thought it worth a ſerious inquiry, whether or no particles of matter, each of them ſingly inſenſible, may not yet ſeparately conſiſt of many minuter particles, betwixt which we may conceive little junctures, where they adhere to one another, in ſome degree pervious to the prodigiouſly ſubtile corpuſcles of the rays of light; whence conſequently thoſe particles would, in ſuch a degree, be diaphanous. For as perfectly opake bodies can only reflect the incident rays of light, thoſe that are diaphanous refract * them too; and that refraction has a great hand

* Refrangibility of the rays of light is their diſpoſition to be turned out of their way, in paſſing out of one tranſparent body into another; reflexibility, their diſpoſition to be turned back into the ſame medium from the ſurface of any other. Sir *Iſaac Newton* firſt ſhewed the preciſe difference between them, in the *Philoſophical Tranſactions*, N°. 80. *A.D.* 1671. and in ſeveral ſubſequent pieces which occur in the ſame *Tranſactions*, ſo far vindicated his diſcovery from ſeveral objections made againſt it, that his opponents, and particularly F. *Pardies*, ingenuouſly acknowledged themſelves ſatisfied with it. At length Sir *Iſaac* publiſhed his doctrine more fully, in his excellent treatiſe of the *reflections, refractions, inflections, and colours of light*; wherein he confirms it by numerous experiments, made with the utmoſt ſimplicity and plainneſs. Herein he ſhews that the colours of light produced by the priſm, and alſo thoſe reflected from opake bodies,

hand in the production of colours, appears from light paſſing thro' drops of water, which exhibits a rain-bow; thro' priſmatic glaſſes, and many other tranſparent bodies. And I find, that in a darkned room, where the light is permitted to enter but at one hole, the little floating particles of duſt, commonly called motes, being viewed by an eye placed on one ſide of the beam of light, appear in certain poſitions adorned with very vivid colours, like thoſe of the rain-bow, or rather like thoſe of very minute ſparkling fragments of diamonds; and that as ſoon as their motion has brought them to an inconvenient poſition in regard to the light and the eye, they are only viſible, without affording any lively colours: which ſeems to argue, that this duſt, or minute fragments of ſeveral ſorts of bodies reputed opake, and only crumbled into duſt, do not barely reflect the rays that fall upon them, but reflect them to the eye refracted.

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We may alſo obſerve, that ſeveral bodies, which paſs for opake, appear in great meaſure tranſparent, when reduced into thin parts, and held againſt a ſtrong light. This I have not only taken notice of in pieces of ivory reduced to thickiſh leaves, in many conſiderable thick ſhells of fiſh, and in ſhavings of wood; but have alſo found, that a piece of very thick deal purpoſely interpos'd betwixt my eye and the clear day-light, appear'd quite thorough of a lovely red. And in the darkned room above-mentioned, bodies held againſt the hole at which the light enter'd, appear'd far leſs opake than they would elſewhere have done: ſo that I could eaſily and plainly ſee, thro' the whole thickneſs of my hand, the motions of a body placed at a very ſmall diſtance beyond it. And even in minerals, the opacity is not always ſo exceeding great, if the body be made thin; for white marble, tho' of a pretty thickneſs, being within a due diſtance placed betwixt the eye and a convenient light, will ſuffer the motions of one's finger to be well diſcern'd through it; and ſo will thick pieces of many common flints. But, above all, that inſtance afforded us by *Muſcovy* glaſs is remarkable; for tho' plates of this mineral of a moderate thickneſs often appear opake, yet if one of theſe be dexterouſly ſplit into the thinneſt leaves 'tis made up of, it will yield ſuch a number of them, as ſcarce any thing but experience could have induced me to believe. Theſe leaves afford the moſt tranſparent ſort of ſolid bodies that, for ought I have obſerved, are yet known: and a ſingle plate of it will be ſo

bodies, have different degrees both of reflexibility and refrangibility; and that the mixture of all the coloured rays, into which the ſun's light is thrown by the priſm, makes white light; and therefore, that all homogeneal light, that is, light whoſe rays are all alike refrangible, have a peculiar colour anſwering to their degree of refrangibility; that this is unalterable by any farther reflections or refractions whatever; that the ſun's light is compoſed of all the primary colours together; and that compound colours proceed from a mixture of the primary. Hence it follows, that no colours can ariſe from any new modifications of light. But Sir *Iſaac* is of opinion, that the permanent colours of bodies are owing to the diſpoſition of them to reflect ſome one ſort of rays, as red, blue, &c. more plentifully than others. Hence red bodies appear moſt beautiful in red light, blue bodies in blue light, &c.

far

PHYSICS. far from being opake, that 'tis ſcarce ſo much as viſible. And multitudes of bodies there are, whoſe fragments ſeem opake to the naked eye, which yet, when included in good microſcopes, appear tranſparent. On the other hand, there may, perhaps, be ſome bodies, whoſe minute particles, even in an excellent microſcope, will not appear diaphanous. For upon viewing, by the help of a good glaſs, mercury precipitated *per ſe*, the little granules that made up the powder, appeared like little fragments of coral beheld at a diſtance, with the naked eye. Filings of ſteel likewiſe, and copper, tho' viewed in an excellent microſcope, on a fair day, ſhow'd like pretty large fragments of thoſe metals, and appear'd conſiderably bright on ſome of their ſurfaces; yet I was not ſatisfy'd that I perceived reflections from the inner parts of any of them. Nay, looking thro' my beſt microſcope upon the red calx of lead, neither I, nor any to whom I ſhew'd it, could diſcern it to be other than opake, tho' the day was clear, and the object ſtrongly enlighten'd. And even calcined vitriol, tho' deeply red, appeared in the ſame microſcope like coarſe brick-duſt. Nor would I be forward to determine how far, or in what caſes the tranſparency or ſemi-diaphaneity of the ſuperficial corpuſcles of larger bodies may have an intereſt in the production of their colours; eſpecially ſince even in ſeveral white ſubſtances, as beaten glaſs, ſnow, and froth, where it ſeems manifeſt that the ſuperficial parts are ſingly diaphanous, we ſee no ſuch variety of colours as is uſual from the refraction of light in thoſe bodies, when by their bigneſs, ſhape, &c. they are properly qualified to exhibit the colours of the rain-bow and priſmatic glaſs †.

By

† For a clear account of opacity and tranſparency, let us hear the excellent Sir *Iſ. Newton*. The opacity of bodies proceeds (according to that great Philoſopher) from the multitude of reflections cauſed in their internal parts, by a diſcontinuity of them; for " between the parts of opake " and coloured bodies are many ſpaces, " either empty, or repleniſhed with me- " diums of other denſities.—And that this " diſcontinuity of parts is the principal " cauſe of the opacity of bodies, appears " from conſidering, that opake ſubſtances " become tranſparent by filling their pores " with any ſubſtance of nearly equal den- " ſity with their parts. Thus paper dipt in " water or oil, the *Oculus mundi* ſtone " ſteeped in water, linen cloth oil'd or " varniſh'd, and many other ſubſtances " ſoaked in ſuch liquors as will inti- " mately pervade their little pores, be- " come, by that means, more tranſparent " than otherwiſe: ſo, on the contrary, " the moſt tranſparent ſubſtances may, " by evacuating their pores, or ſeparating " their parts, be render'd ſufficiently o- " pake; as Salts, or wet paper, or the " *Oculus mundi* ſtone, by being dried; " horn, by being ſcraped; glaſs, by being " reduced to powder, or otherwiſe, " flaw'd; turpentine, by being ſtirred " about with water, till they mix imper- " fectly; and water, by being form'd " into many ſmall bubbles, either alone, " in the form of froth, or by ſhaking it " together with oil of turpentine, or oil " olive, or with ſome other convenient " liquor, wherewith it will not perfectly " incorporate. And to the increaſe of " the opacity of theſe bodies, it condu- " ces ſomething, that the reflexions of " very thin, tranſparent ſubſtances are " conſiderably ſtronger than thoſe made " by the ſame ſubſtances of a greater " thickneſs." *Newton. Optic.* p. 223, 224.

Sir *Iſaac Newton* farther obſerves, that the parts of bodies, and their interſtices, muſt not be leſs than of ſome definite big-

Whether objects are coloured in the dark?

By what has hitherto been delivered we may be aſſiſted to judge of that famous controverſy anciently held, betwixt the atomiſts on one ſide, and moſt other philoſophers on the other ſide; the former denying bodies to be coloured in the dark, and the latter maintaining colour to be an inherent quality, as well as figure, hardneſs, weight, &c. For, if colour be only light modify'd, how can it ſubſiſt in the dark? But if colour be conſidered as a certain conſtant diſpoſition of the ſuperficial parts of the object, to diſorder the light they reflect after a determinate manner; as this modifying diſpoſition perſeveres in the object, whether it be enlighten'd or not, there ſeems no juſt reaſon to deny, that, in this ſenſe, bodies retain their colour as well in the night as in the day. Or it may, upon this ſuppoſition, be otherwiſe ſaid, that bodies are potentially coloured in the dark, and actually coloured in the light.

Whether emphatical colours be real or imaginary?

But 'tis of greater moment in the inquiry into the nature of colours, to decide that controverſy, whether thoſe of the rain-bow; thoſe often ſeen in clouds before the riſing, or after the ſetting of the ſun; and, in a word, whether thoſe other colours, uſually called emphatical, ought to be accounted true: for it being generally granted, and may eaſily be proved, that emphatical colours are light modify'd chiefly by refractions, with

bigneſs to render them opake and coloured; "for the opakeſt bodies, if their "parts be ſubtily divided, become perfectly tranſparent. Thus water, ſalt, "glaſs, ſtones, and ſuch like ſubſtances, "are tranſparent; for, upon divers conſiderations, they ſeem to be as full of "pores or interſtices between their parts "as other bodies are; but yet their parts "and interſtices ſeem to be too ſmall to "cauſe reflexions in their common ſurfaces." *Newton. Optic.* p. 225.

"The tranſparent part of bodies, ſays "Sir *Iſaac Newton*, according to their ſeveral ſizes, reflect rays of one colour, and "tranſmit thoſe of another, on the ſame "grounds that thin plates or bubbles do "reflect or tranſmit thoſe rays;" and this Sir *Iſaac* takes to be the ground of all their colours: "for if, ſays he, a thinned "or plated body, which being of an even "thickneſs, appears all over of one uniform colour, ſhould be ſlit into threads, "or broken into fragments, I ſee no reaſon why every thread or fragment "ſhould not keep its colour; and, by conſequence, why a heap of thoſe threads "or fragments, ſhould not conſtitute a "maſs or powder of the ſame colour "which the plate exhibited before it was "broken. And the parts of all natural "bodies being like ſo many fragments of "a plate, muſt, on the ſame grounds, exhibit the ſame colour. Now that they "do ſo, will appear by the affinity of "their properties. The finely coloured "feathers of ſome birds, and particularly "thoſe of peacocks tails, do, in the very "ſame part of the feather, appear of ſeveral colours in ſeveral poſitions of the "eye, after the very ſame manner that "thin plates do. And therefore their colours ariſe from the thinneſs of the "tranſparent parts of the feathers; that "is, from the ſlenderneſs of the very fine "hairs, or *Capillamenta*, which grow out "of the ſides of the groſſer lateral branches "or fibres of thoſe feathers. And to the "ſame purpoſe it is, that the webs of "ſome ſpiders, by being ſpun very fine, "have appeared coloured; and that the "coloured fibres of ſome ſilks, by varying "the poſition of the eye, do vary their "colour. Alſo the colours of ſilks, cloths, "and other ſubſtances which water or "oil can intimately penetrate, become "more faint and obſcure, by being immerſed in thoſe liquors, and recover "their vigor again, by being dried after "the manner of thin bodies. Leaf-gold, ſome

PHYSICS. with the concurrence ſometimes of reflections, and perhaps other accidents depending on them both; if theſe emphatical colours be reſolved genuine, it will ſeem to follow, that colours, or at leaſt many of them, are but diverſify'd light, and not ſuch real and inherent qualities as men commonly ſuppoſe.

Now ſince we allow echoes, and other ſounds of bodies, to be true ſounds, all their odours to be true odours, and in ſhort, other ſenſible qualities to be true, becauſe they are the proper objects of ſome of our ſenſes; I ſee not why emphatical colours, being the proper and peculiar objects of the organ of ſight, and capable to affect it as truly and as powerfully as other colours, ſhould be reputed imaginary. And if we have proved, that colour, when taken in its more proper ſenſe, is only modify'd light, there will be little reaſon to deny, that theſe are true colours, which more manifeſtly than others ſhew themſelves to be produced by the diverſifications of light. There is indeed a difference taken notice of betwixt theſe apparent colours, and thoſe that are uſually eſteemed genuine, as to their duration; which has induced ſome learned men to call the former rather evanid than fantaſtical. But, as *Gaſſendus* obſerves, if this way of arguing were true, the greenneſs of a leaf ought to paſs for apparent, becauſe ſoon fading into a yellow, it cannot

" ſome ſorts of painted glaſs, the infuſion " of *Lignum Nephriticum*, and ſome other " ſubſtances, reflect one colour, and tranſ- " mit another, like thin bodies alſo. And " ſome of thoſe coloured powders which " painters uſe, may have their colours a " little changed, by being very elabo- " rately and finely ground. Where I ſee " not what can be juſtly pretended for " thoſe changes, beſides the breaking " of their parts into leſs parts, by that " contrition, after the ſame manner that " the colour of a thin plate is changed, " by varying its thickneſs: for which " reaſon alſo it is, that the coloured flow- " ers of plants and vegetables, by being " bruiſed, uſually become more tranſpa- " rent than before; or at leaſt in ſome " degree or other change their colours. " And thus, by mixing various liquors, " very odd and remarkable productions " and changes of colours may be effected, " of which no cauſe can be more obvious " and rational, than that the ſaline cor- " puſcles of one liquor do variouſly act " upon, or unite with, the tinging cor- " puſcles of another, ſo as to make them " ſwell or ſhrink, (whereby not only " their bulk, but their denſity alſo may " be changed) or to divide them into " ſmaller corpuſcles, (whereby a coloured " liquor may become tranſparent) or to " make many of them aſſociate into one " cluſter, whereby two tranſparent li- " quors may compoſe a coloured one. " For we ſee how apt thoſe ſaline men- " ſtrua are to penetrate and diſſolve ſub- " ſtances to which they are applied; and " ſome of them to precipitate what others " diſſolve. In like manner, if we conſi- " der the various phenomena of the at- " moſphere, we may obſerve, that when " vapours are firſt raiſed, they hinder not " the tranſparency of the air; being di- " vided into parts too ſmall to cauſe any " reflexion in their ſuperficies. But " when, in order to compoſe drops of " rain, they begin to coaleſce, and conſti- " tute globules of all intermediate ſizes, " thoſe globules, when they become of a " convenient ſize to reflect ſome colours, " and tranſmit others, may conſtitute " clouds of various colours, according to " their ſizes. And I ſee not what can be " rationally conceived in ſo tranſparent " a ſubſtance as water, for the production " of theſe colours, beſides the various ſizes " of its fluid and globular parcels." *Newton. Optic.* p. 226—228.

be compared, in point of duration, with the greenneſs of an emerald. PHYSICS.
But if the ſun-beams paſs in a convenient manner thro' a glaſs priſm, ſo as to be thrown upon ſome well ſhaded object, within a room; the rainbow thereby painted on the ſurface of a body that terminates the rays, may often laſt longer than ſome colours I have produced in certain bodies: which colours would juſtly, and without ſcruple, be accounted genuine, tho' they ſuddenly degenerate, and loſe their nature.

A greater diſparity betwixt emphatical colours, and others, may, perhaps, be urged; becauſe genuine colours ſeem to be produced in opake bodies, by reflection; but apparent ones, in diaphanous bodies, principally, by refraction: for, in ſome caſes, reflection, alſo, may concur. But ſtill this ſeems not to prove, that theſe latter colours are falſe ones; nor muſt what we lately ſaid of the differences of true and apparent colours, be underſtood in too unlimited a ſenſe: for if water be agitated into froth, it exhibits a white colour, which it ſoon after loſes upon the reſolution of the bubbles into air and water; in which caſe, the whiteneſs of the froth is either a true colour, or not. If it be true, then true colours, ſuppoſing the water pure, may prove as ſhort-lived as thoſe of the rainbow; and the matter wherein the whiteneſs reſided, may, in a few moments, be perfectly deprived of it. Beſides, even diaphanous bodies may be capable of exhibiting true colours, by reflection; for that whiteneſs is ſo produced, will ſhortly appear. But if it be ſaid, that the whiteneſs of froth is an emphatical colour; then it muſt no longer be ſaid, that fantaſtical colours require a certain poſition of the illuminating body and the eye; and muſt be varied or deſtroyed by the change thereof: for froth appears white, whether the ſun be riſing or ſetting; in the meridian, or any where between that and the horizon; and in what part ſoever the ſpectator's eye is placed. And ſince, by making a liquor tenacious, without deſtroying its tranſparency, or ſtaining it with any colour, we can give the little films whereof the bubbles conſiſt, ſuch a texture as will make the froth laſt for many hours, or days, together; it ſeems improper to aſſign duration for the diſtinguiſhing character of genuine from fantaſtic colours: for ſuch froth may long out-laſt the true colours of ſome natural productions. Thus in that gaudy plant, the marvel of *Peru*, the flowers often fade on the very ſame day they are blown. And I have often ſeen a *Virginia* flower, which uſually withers within the compaſs of a day. I am alſo credibly inform'd, that a curious neighbouring botaniſt has a plant whoſe flowers periſh in about an hour's time.

But if the whiteneſs of water, turn'd into froth, muſt therefore be reputed emphatical, becauſe it appears not that the nature of the body, but only that the diſpoſition of its parts, with regard to the incident light, is changed; why may not that whiteneſs be accounted emphatical too, which I ſhall ſhortly ſhew to be producible, barely by ſuch another change in black horn? And yet this whiteneſs, which is ſo eaſily acquir'd, ſeems to be as truly its colour as blackneſs was before; and is, at leaſt, more permanent than the greenneſs of leaves, the redneſs of roſes, or the ge-

PHYSICS. nuine colours of moſt natural productions. It may, indeed, be further objected, that, acccording as the ſun or other luminous body changes place, theſe emphatical colours alter, or vaniſh. If a piece of cloth, in a draper's ſhop, where the light is ſeldom primary, be variouſly folded, it will appear of different colours, as the parts happen to be more illuminated or ſhaded ; and if you ſtretch it flat, it will commonly exhibit ſome one uniform colour; yet theſe are not reputed emphatical : ſo that the difference ſeems to be chiefly this, that in the caſe of the rain-bow, and the like, the poſition of the illuminating body varies the colour ; and, in cloth, the poſition of the object does it. Yet I am not forward to allow, that in all caſes the apparition of emphatical colours requires a determinate poſition of the eye; for if men will have the whiteneſs of froth emphatical, we have already ſeen what may be inferred from thence. Beſides, the ſun-beams tranſmitted thro' a triangular priſm of glaſs, after the manner lately mentioned, will, upon the body that terminates them, paint a rain-bow, viſible to the eye, whether placed on the right hand of it, or on the left ; above it, or beneath it ; before it, or behind it : and tho' there may appear ſome little variation in the colours of the rain-bow, beheld from different parts of the room ; yet the like may be obſerved by an attentive eye, in real colours, view'd under the like circumſtances. Nor will it follow, that, becauſe there remain no footſteps of the colour upon the object, when the priſm is removed, that therefore the colour was not real ; ſince the light was truly modify'd by the refraction * and reflection it ſuffered in paſſing thro' the priſm : and the object, in our caſe, ſerved as a ſpeculum to reflect that colour to the eye. For a rough and coloured object may ſerve for a ſpeculum to reflect the artificial rain-bow we ſpeak of ; ſince, in darkned rooms, a wall, conveniently ſituated within, will ſo reflect the colours of bodies external to the room, that they may very clearly be diſcern'd and diſtinguiſhed : and yet it is taken for granted, that the colours ſeen in a darkned room, tho' they leave no traces upon the wall or body that receives them, are the true colours of the external objects ; together with which the colours of the images are moved, or reſt. And the error lies not in the eye, whoſe office is only to perceive the appearance of things, and truly does ſo ; but in the judgment that, by miſtake, concludes the colour belongs to the wall, which really belongs to the object ; becauſe the wall is that from whence the rays of light which carry the viſible ſpecies, come in ſtrait lines directly to the eye. And thus, for the ſame reaſon, we ſuppoſe at a certain diſtance from concave ſpherical glaſſes,

* We muſt again repeat, that Sir *Iſaac Newton* demonſtrates, all the colours in the univerſe, which are made of light, and depend not on the power of imagination, are either the colours of homogeneal lights, or compounded of them in a certain proportion. " For the changes " of colour, made by refraction, do not " ariſe from any new modification of the " rays impreſſed by thoſe refractions ; " and, by the various terminations of " light and ſhadow ; as has been the " conſtant and general opinion of phi- " loſophers." *Newton. Optic.* p. 99. & p. 138.

that

that we see the image ſtand out to meet us, and hang in the air betwixt the glaſs and us; becauſe the reflected rays, that compoſe the image, croſs one another in that place where the image appears to be; and thence in direct lines take their courſe to the eye. I might here add what will more fully appear hereafter, that colours, called emphatical, becauſe not inherent in the bodies where they appear, may be compounded with one another, as thoſe that are confeſſedly genuine.

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SECT. II.

The nature of whiteneſs.

I Come now to inquire into the nature of whiteneſs and blackneſs. Whiteneſs, conſidered as a quality in the object, ſeems, in the general, chiefly to depend upon the roughneſs of the ſurface of the body, call'd white; which gives it innumerable ſmall ſuperficies, that acting like ſo many little ſpecula in various poſitions, they reflect the rays of light that fall on them, not towards one another, but externally towards the ſpectator's eye. The ſun, and other very lucid bodies, not only offend or dazle our eyes; but if any colour is aſcribed to them, it ſhould be whiteneſs: * for the ſun at noon-day, and in clear weather, when his face is ſerene,

* White, in ſtrictneſs, we have already obſerved to be no ſimple colour; but a compound of all colours in a due proportion. "When the ſeveral ſorts of rays "are mixed, ſays Sir *Iſaac Newton*, and, "in croſſing, paſs thro' the ſame ſpace, "they act not upon one another, ſo as "to change their colour-making quali-"ties; but, by mixing their actions in "the ſenſorium, beget a ſenſation, differ-"ing from what either would do a-part: "that is, a ſenſation of a mean colour "between their proper colours. And "particularly, when, by the concourſe "and mixtures of all ſorts of rays, a "white colour is produced; the white "is a mixture of the colours which the "rays would have a-part: the rays in "that mixture do not alter their ſeveral "colour-making qualities; but, by all "their various kinds of actions, mixed "in the ſenſorium, beget a ſenſation of a "middling colour, between all their co-"lours, which is whiteneſs. For white-"neſs is a mean between all colours; "having itſelf indifferently to them all; "ſo as with equal facility to be tinged "with any of them. A red powder "mixed with a little blue, or a blue "with a little red, doth not preſently "loſe its colour; but a white powder, "mixed with any colour, is preſently "tinged with that colour, and is equally "capable of being tinged with any co-"lour whatever." See *Newton. Optic.* p. 117, 118: 138, 139.

We farther learn from Sir *Iſaac Newton*, that as a mixture of all the priſmatic colours makes perfect white light; ſo if ſoapy water be agitated into a froth, various colours will appear therein, when viewed near; and, at a diſtance, when thoſe colours can no longer be diſtinguiſhed, the froth will ſeem perfectly white. But, by mixing coloured powders, he tells us, we are not to expect a ſtrong and full white, but ſome dusky obſcure one; becauſe they ſuppreſs and ſtop in them a very conſiderable part of the light by which they are illuminated. Thus he produced a dark white, or a dun colour, by mixing one part of red lead with five parts of verdigreaſe; for theſe two colours were ſeverally ſo compounded of others, that, in both together, there was a mixture of all colours.

Again, one part of red lead, and four parts of blue biſe, compoſed a dun colour, varying a little to purple; and, by adding to it a certain mixture of orpiment

PHYSICS. ſerene, and his rays paſs thro' a much leſs part of the atmoſphere to our eyes, appears of a colour more approaching to white than when he is nearer the horizon; in which caſe the interpoſition of certain fumes and vapours make him oftentimes appear either red or yellow. And when the ſun ſhines upon ſmooth water, that part of it which appears moſt illumined, ſeems far whiter than the reſt. And I have ſometimes found, that when the ſun was veil'd with a thin white cloud, tho' ſtill too bright to be look'd upon directly; that by caſting my eyes upon ſtill water, his body, being not far from the meridian, appeared to me exceedingly white. And tho' we vulgarly ſay, in *Engliſh*, a thing is red-hot, to expreſs a ſuperlative degree of heat; yet, at the forges and furnaces of artificers, by a white heat they underſtand a further degree of ignition, than by a red one.

2. Common experience informs us, that as too much light overpowers the eye; ſo when the ground is covered with ſnow, thoſe who have a weak ſight, complain that this proſpect is offenſive to them. And even thoſe who have good eyes, are from hence generally ſenſible of an extraordinary light in the air; and, if obliged to look very long upon the ſnow, find their ſight injured by it. Thus *Xenophon* relates, that *Cyrus* marching his army, for many days, over mountains covered with ſnow, the dazling ſplendour of its whiteneſs prejudiced the ſight of a great number of his ſoldiers, and blinded ſome of them; and other accounts of the ſame nature may be met with in writers of good note. The like has alſo been affirmed to me by credible perſons of my own acquaintance, and eſpecially by one, who, during

ment and verdigreaſe, in due proportion, it loſt its purple tincture, and became perfectly dun. But the experiment ſucceeded beſt without red lead, thus: "To orpiment, ſays Sir *Iſaac*, I "added, by little and little, a certain "full bright purple, which painters "uſe, till the orpiment ceaſed to be "yellow, and became of a pale red: "then I diluted that red, by adding a "little verdigreaſe and a little more blue "biſe than verdigreaſe, till it became "of ſuch a grey or pale white, as va- "ried to no one of the colours more than "to another. For thus it became of a "colour equal in whiteneſs to that of "aſhes, or of wood newly cut, or of a "man's skin. The orpiment reflected "more light than did any other of the "powders; and therefore conduced more "to the whiteneſs of the compounded "colour than they. To aſſign the pro- "portions accurately, may be difficult, "by reaſon of the different goodneſs of "powders of the ſame kind. As the co- "lour of any powder is more or leſs full "and luminous, it ought to be uſed in a "leſs or greater proportion. Now con- "ſidering that theſe grey and dun co- "lours may be alſo produced, by mixing "whites and blacks; and, by conſe- "quence, differ from perfect whites, not "in ſpecies of colours, but only in de- "gree of luminouſneſs; it is manifeſt "that there is nothing more requiſite to "make them perfectly white, than to "increaſe their light ſufficiently; and, "on the contrary, if, by increaſing their "light, they can be brought to perfect "whiteneſs; it will thence alſo follow, "that they are of the ſame ſpecies of "colour with the beſt whites; and dif- "fer from them only in the quantity "of light." And accordingly, by placing ſome powder, compoſed of orpiment, purple, biſe, and verdigreaſe, in the ſun's rays; and, viewing it at a diſtance, it appeared intenſely white. *Newton. Optic.* p. 129—134.

his

his ſtay in *Muſcovy*, found his eyes much impaired, by being frequently obliged to travel in the ſnow: and this weakneſs of ſight did not leave him when he left that country, but ſtill continues, tho' he be a young man. I myſelf alſo, as well as others, have obſerved, that upon travelling by night, when the ground was all covered with ſnow, tho' it would otherwiſe have been dark, yet we could very well ſee to chuſe our way. But much more to my preſent purpoſe is that account given us by *Olaus Magnus* of a way of travelling in the *Northern* regions during the winter, where the days of that ſeaſon are very ſhort. "In the day-time, "ſays he, they travel twelve *Italian* miles, but twice or thrice as far in "the night, and that with eaſe; for the light of the moon, reflected by the "ſnow, renders both hills and vales conſpicuous; ſo that then they can "ſee not only precipices afar off, but the wild beaſts they would avoid."

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This teſtimony I the leſs ſcruple to alledge, becauſe it agrees very well with what has been affirmed to me by a phyſician of *Moſcow*; who informed me, that he could ſee things at a far greater diſtance, and with more clearneſs, when he travelled by night on the ſnow there, tho' without the aſſiſtance of the moon-ſhine, than we, in theſe parts, would eaſily believe. 'Tis true, indeed, the intenſeneſs of the cold might contribute ſomething to the conſiderableneſs of the effect, by clearing the air of dark ſteams, which, in theſe more temperate climates, are uſually thick in ſnowy weather: for this phyſician, and the ingenious navigator Captain *James* agree, that in dark froſty nights, in frozen climates, they could diſcover more ſtars, and have a clearer proſpect of the heavens, than we in *England*. I know, indeed, many learned men ſuppoſe ſnow thus ſtrongly affects our eyes, not by a borrowed, but a native light. I venture, however, to give it as a proof, that white bodies reflect more light than others, becauſe having once placed a parcel of ſnow in a room, carefully darkened, that no celeſtial light might fall upon it, neither I, nor an ingenious perſon skilled in optics, could find it had any light beſides what it received; and 'tis uſual, among ſuch as travel in dark nights, to make their guides wear ſomething of white to be diſcern'd by; for there is ſcarce any night ſo dark, but that, in the free air, ſome light remains, tho' broken and debilitated, perhaps, by a thouſand reflections from the opake corpuſcles, that ſwim in the atmoſphere, and ſend it to one another before it arrives at the eye.

3. And the better to ſhew that white bodies reflect much more light than others, I held in the darkened room, formerly mentioned, not far from the hole at which the light entered, a ſheet of white paper; from whence caſting the ſun-beams upon a white wall, it manifeſtly appeared both to me, and to a perſon I took to be witneſs of the experiment, to reflect a far greater light than any of the other colours; for the wall itſelf was not only thus notably enlightened, but alſo a conſiderable part of the room. And, further, to ſhew that white bodies reflect the rays outwards, let me add, that ordinary burning-glaſſes will not, in a great while, burn or diſcolour white paper; ſo that when I was a boy, and delighted to make

PHYSICS. make trials with ſuch glaſſes, I could not but wonder at this odd phenomenon, which ſet me very early upon gueſſing at the nature of whiteneſs; and the more, becauſe I took notice that the image of the ſun, upon white paper, was not ſo well defined as upon black; and becauſe that, upon inking over the paper, the moiſture would be quickly dried up, and the paper, that I could not burn before, would now preſently take fire. I have alſo try'd, that by expoſing my hand, with a thin black glove on it, to the warm ſun, it would thereby very ſuddenly be more conſiderably heated, than if I took off the glove, and held my naked hand to its rays, or put on another glove of thin white leather.

4. And to ſhew that white bodies are apt, like ſpecula, to reflect the light that falls on them, we have found, in a darken'd room, that the ſun-beams being caſt from a coloured body upon a white wall, the determinate colour of the body was from the wall reflected to the eye; whilſt we could, in many caſes, manifeſtly alter the colour arriving at the eye, by ſubſtituting, at a convenient diſtance, a colour'd gloſſy body inſtead of the white wall; thus by throwing the rays from a yellow body upon a blue, there would be exhibited a kind of green. I might alſo here take notice, that when looking upon the calm and ſmooth ſurface of a river, lying betwixt my eye and the ſun, it appeared to be a natural ſpeculum, wherein that part, which reflected to my eye the entire and defined image of the ſun and rays adjacent thereto, appeared of a great whitiſh brightneſs, but the reſt comparatively dark; when, if afterwards the ſuperficies chanced to be a little ruffled by a gentle breath of wind, and thereby reduced into a multitude of ſmall, ſmooth ſpecula, the ſurface of the river would, at a diſtance, appear almoſt white; tho' it loſt that appearance upon a return of the ſurface to a calm and uniform level. And I have ſometimes, as an experiment, brought in a lenticular glaſs, the image of a river illumined by the ſun, into a darkened room, at about the diſtance of a quarter of a mile; by which means the numerous declining ſurfaces of the water were ſo contracted, that, upon the body which received the image, the whole river appeared a very white object at two or three paces diſtance. But if we drew near it, this whiteneſs ſeemed to proceed from innumerable lucid reflections, made by the ſuperficies of the water gently waved, which looked, when cloſely viewed, like a multitude of very little ſhining ſcales of fiſh; many whereof every moment diſappeared, while as many were by the ſun, wind, and river, generated anew. But tho' this obſervation ſeemed ſufficiently to diſcover how the apparent whiteneſs, in that caſe, was produced; yet in ſome other caſes, water may have the ſame, tho' not ſo vivid a colour, upon other accounts; for it often happens that the ſmooth ſurface of the water appears bright or whitiſh, by reaſon of the reflection, not immediately of the image of the ſun, but of the brightneſs of the ſky; and in ſuch caſes a convenient wind may, where it paſſes along, make the ſurface look black, by cauſing many ſuch furrows and cavities, as make the inflected ſuperficies of the water reflect the brightneſs of the sky, rather internally

ly than externally. And again, if the wind increaſe into a ſtorm, the water may appear white, eſpecially near the ſhore, becauſe the rude agitation breaks it into foam or froth; ſo much do whiteneſs and blackneſs depend upon the diſpoſition of the ſuperficial parts of a body, to reflect the rays of light inward or outward. But that as white bodies reflect the moſt light of any; ſo their ſuperficial particles are of a ſpecular nature, I ſhall further endeavour to ſhew, by making ſpecular bodies white, and a white body ſpecular.

5. Upon diſtilling quickſilver in a cucurbit, fitted with a capacious glaſs-head, I have obſerved, that when the operation was performed by proper degrees of fire, there would ſtick to the inſide of the alembic a multitude of little round drops of mercury. And as mercury is a ſpecular body, each of theſe little drops was a ſmall ſpherical looking-glaſs; and a number of them lying near one another, made the glaſs they were faſtened to, manifeſtly appear a white body. And as many parts of the ſky, eſpecially the milky way, appear white to the naked eye; yet the galaxy, viewed thro' a teleſcope, does not appear white, but to be made up of a vaſt multitude of little ſtars; ſo, many lucid bodies, if too ſmall to be ſingly diſcerned by the eye, and ſet ſufficiently thick by one another, may, by their united rays, appear to the eye as one white body: and why may not the like happen, when a multitude of bright, little corpuſcles, crowded together, are made jointly to reflect vivid rays to the eye, tho' they ſhine by a borrowed light?

But to return to our experiments; we may take notice, that the white of an egg, which tho' in part tranſparent, yet, by its power of reflecting ſome incident rays of light, is, in ſome meaſure, a natural ſpeculum, being long agitated with a whisk or ſpoon loſes its tranſparency, and becomes a very white froth; that is, an aggregate of numerous ſmall bubbles, whoſe convex ſuperficies fit them to reflect the light every way outwards. And 'tis worth obſerving that when water, for inſtance, is agitated into froth, if the bubbles be great and few, the whiteneſs will be but faint, becauſe the number of ſpecula within a narrow compaſs is but ſmall; and they are not thick enough ſet to reflect ſo many little images or rays of the lucid body, as go to produce a vigorous ſenſation of whiteneſs. And leſt it ſhould be ſaid, that the whiteneſs of ſuch globular particles proceeds from the air included in the froth, (tho' who can prove that the air itſelf is white?) and at the ſame time to illuſtrate our doctrine of whiteneſs, I ſhall add this experiment. I put to ſome fair water, contained in a glaſs vial, a convenient quantity of ſpirit of turpentine, which will not incorporate with water, yet is almoſt as clear and colourleſs as that; and theſe being well ſhaken together, I found the agitation broke the oil into a multitude of little globes, which each of them reflecting outwards a lucid image, made the imperfect mixture of the two liquors, appear whitiſh; but if by vehemently ſhaking the glaſs for a competent time, a further comminution of the oil be made into far more numerous and ſmaller globules, whilſt it is alſo thereby more thoroughly

confounded

PHYSICS. confounded with the water, the mixture will appear of a much greater whiteneſs, and almoſt like milk; tho' if the glaſs be let alone awhile, the colour will gradually fade as the oily globules grow fewer and larger, and at length quite vaniſh; leaving both the liquors diſtinct and diaphanous as before. And ſuch a trial hath ſucceeded, when inſtead of oil, or ſpirit of turpentine, I took a yellow mixture, made with a large proportion of crude turpentine, diſſolved in that liquor; and it alſo ſucceeded better than one would expect, when I employ'd an oil brought to a deep green, by infuſing copper filings therein. Thus aromatics, diſtilled with water, often yield a white liquor, which may long continue of that colour; becauſe if the fire be made too ſtrong, the ſubtile oil is thereby much agitated, broken, and blended with the water, in ſuch numerous and minute globules, as cannot eaſily, in a ſhort time, emerge to the top of the water, and, whilſt they remain therein, make it look whitiſh. And hence, perhaps, it is, that we find hot water uſually more opake and whitiſh than the ſame when cold; the agitation turning the more volatile particles of the water into vapours, and thereby producing, in the body of the liquor, a multitude of ſmall bubbles, which interrupt the free paſſage of the rays of light, and from the innermoſt parts of the water, reflect many of them outwards. Theſe, and the like examples, have induced me to ſuſpect, that the ſuperficial particles of white bodies, may, for the moſt part, be as well convex as ſmooth; tho' it ſeems not eaſy to prove, that when diaphanous bodies are reduced into white powders, each corpuſcle muſt needs be of a convex ſuperficies; and, perhaps, it may here ſuffice that the ſpecular ſurfaces look ſeveral ways. We have ſeen that when a diaphanous body is reduced to very minute parts, it thereby acquires a multitude of little ſurfaces within a narrow compaſs; and tho' each of thoſe ſhould not be of a convenient figure to reflect a round image of the ſun, they may reflect ſome phyſical line of light, wherein ſome refraction of that which falls upon the body, whereon it depends, may often contribute to its whiteneſs. Thus if a ſlender wire, or ſolid cylinder of glaſs, be expoſed to luminous rays, you ſhall ſee, in ſome part thereof, a vivid line of light; and if we were able to draw out and lay together a multitude of theſe little wires or threads of glaſs, ſo ſlender, that the eye could not diſcern a diſtance between the luminous lines, there is no doubt, as far as I can gueſs by a trial of this kind purpoſely made, that the whole phyſical ſuperficies, compoſed of them, would appear white to the eye; and if ſo, it is not always neceſſary that the figure of thoſe corpuſcles, that make a body appear white, ſhould be ſpherical: and ſnow itſelf commonly appears both to the naked eye, and when viewed thro' a microſcope, to conſiſt principally of little ſlender icicles of ſeveral ſhapes, which afford ſuch numerous lines of light as we ſpeak of.

6. If you take a diaphanous body, as, for inſtance, a piece of glaſs, and reduce it to powder, the ſame body which, when entire, freely tranſmitted the rays of light, acquiring, by contuſion, a multitude of minute ſurfaces, each of which is, as it were, a little ſpeculum, becomes thereby by qualified to reflect, in a confuſed manner, ſo many rays, or little and ſingly unobſervable images of the lucid body, that from a diaphanous, it degenerates into a white ſubſtance. And heating lumps of cryſtal red-hot, in a crucible, I have found, that, upon quenching 'em in fair water, even thoſe which remained ſeemingly entire, exchanged their tranſparency for whiteneſs; the ignition and extinction having cracked each lump into a multitude of minute bodies, and thereby given it a multitude of new ſurfaces. And even with coloured diaphanous bodies, there may, by this way, be a great degree of whiteneſs produced. I have, by contuſion, obtained whitiſh powders from granats, glaſs of antimony, and emeralds; but the experiment is more eaſily made, by comparing deep blue pulverized vitriol of copper, with ſome of the entire cryſtals of the ſame, for this will, comparatively, exhibit a conſiderable degree of whiteneſs.

7. And as by a change of poſition in the parts of differently coloured bodies, they may be rendered white; ſo by a ſlight change, in the texture of its ſurface, a white body may be deprived of that property. A piece of ſilver newly boiled, with ſalt and tartar, after the goldſmith's faſhion, is of a lovely white; but if, with a piece of ſmooth ſteel, a part of it be burniſh'd, that part preſently loſes its whiteneſs, and turns to a ſpeculum, almoſt every where dark, like other mirrors; which adds a great confirmation to our doctrine. For hence we ſee what it chiefly is that made the body white before; ſince all that was done to deprive it of that whiteneſs, was only to depreſs the little irregular protuberancies, that ſtood out on the ſurface of the ſilver, into one continued regular plain.

8. What we have ſaid of whiteneſs may aſſiſt us to form a notion of blackneſs; thoſe two qualities being ſufficiently oppoſite to illuſtrate each other. And as that which makes a body white, is chiefly ſuch a diſpoſition of its parts, as diſpoſes it to reflect more of the light that falls on it, than bodies of different colours; ſo that which renders a body black, is principally a peculiar kind of texture of its ſuperficial particles; whereby it damps the light that falls on it, ſo that very little is reflected to the eye. *The nature of blackneſs.*

9. This texture is explicable two ſeveral ways; and firſt, by ſuppoſing, in the ſuperficies of the black body, a particular kind of aſperity; whence the ſuperficial particles reflect few of the incident rays outwards, and the reſt inwards, upon the body itſelf: as if, for inſtance, the ſurface of a black body ſhould riſe up in numberleſs little cylinders, pyramids, cones, *&c.* which, by being thick ſet and erect, throw the rays of light from one to another inwards, ſo often, that, at length, they are loſt before they can come out again to the eye. The other way ſuppoſes the textures of black bodies either to yield to the rays of light, or, upon ſome other account, to ſtifle and keep them from being reflected in any number, or with any conſiderable vigour outwards. According to this notion it may be ſaid, that the corpuſcles, which compoſe the rays of light, thruſting one another from the lucid body, and falling on black ſubſtances, meet with ſuch a texture, that they receive into themſelves, and retain almoſt all the

motion communicated to them by thoſe corpuſcles, and conſequently reflect but few of them, or thoſe but languidly, towards the eye; as when a ball, thrown againſt a floor, rebounds a great way upwards, but very little or not at all when thrown againſt mud or water; becauſe the parts yield, and receive into themſelves the motion which ſhould reflect the ball back. But this laſt manner of accounting for blackneſs I barely propoſe, without either adopting, or abſolutely rejecting it; for the hardneſs of touchſtones, black marble, and of other bodies that are black, and ſolid, ſeems to render it ſomewhat improbable that they ſhould be of ſo yielding a nature; unleſs we ſay that ſome bodies may be more diſpoſed to yield to the impulſes of the corpuſcles of light, by reaſon of a peculiar texture, than others, which, by particular trials, appear to be ſofter than they. Both the ſolutions, however, agree in this, that black bodies reflect but little of the light which falls on them. And it is not impoſſible that, in ſome caſes, both the diſpoſition of the ſuperficial particles as to figure and poſition, and the yielding of the body, or ſome of its parts, may jointly contribute to render a body black. The conſiderations which induced me to propoſe this notion of blackneſs, are principally theſe.

(1.) Whiteneſs and blackneſs being generally reputed contrary qualities, whiteneſs depending, as I ſaid, upon the diſpoſition of the parts of the body, to reflect light plentifully, it ſeem'd probable, that blackneſs might depend upon a contrary diſpoſition of ſurface; but upon this I ſhall not inſiſt. However, if a body, of an uniform colour, be placed, part in the ſun-beams, and part in the ſhade, that part, which is not illumined, will appear nearer allied to blackneſs than the other, from which more light is reflected to the eye; dark colours alſo ſeem the blacker, the leſs is the light they are viewed in: and all things ſeem black in the dark, when they yield no rays to make impreſſions upon our organs of ſight; ſo that ſhadow and darkneſs are near a-kin: and ſhadow, we know, is but a privation of light; blackneſs, accordingly, ſeems to proceed from the want of rays reflected from the black body to the eye; tho' the bodies we call black, as marble, jet, *&c.* are not perfectly ſo, for if they were, we ſhould not ſee them at all. But notwithſtanding the rays which fall on the ſides of thoſe erect particles we mentioned, do few of them return outwards; yet ſuch as fall upon the points of thoſe cylinders, cones, or pyramids, may be thence reflected to the eye, tho' they make but a faint impreſſion there; becauſe they are mixed with a great proportion of little ſhades. Thus, having procured a large piece of black marble to be well poliſhed, and brought to the form of a large ſpherical concave ſpeculum, the inſide thereof was a kind of dark looking-glaſs, wherein I could plainly ſee a little image of the ſun, when it ſhone thereon. But this image was very far from offending or dazling my eyes, as it would have done from another ſpeculum; and tho' this were large, I could not, in a long time, ſet a piece of wood on fire with it; tho' a far leſs ſpeculum of the ſame form, and of a more reflecting material, would preſently have made it flame. And having expoſed to the ſun a pretty large mortar

of

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of white marble, poliſh'd on the inſide, we found that it reflected a great quantity of glaring light, but ſo ſcatter'd, that we could not make the reflected rays meet in any ſuch conſpicuous focus as that we obſerv'd in the black marble; tho' by holding a candle, in the night-time, at a convenient diſtance, we were able to procure a concourſe of a few reflected rays at about two inches diſtance from the bottom of the mortar. But we found the heat of the ſun-beams ſo diperſedly reflected, to be very languid, even compared to the focus of the black marble: and the little picture of the ſun that appear'd upon the white marble as a ſpeculum, was very faint, and exceedingly ill defined.

(2.) Taking two pieces, the one of black, and the other of white marble, whoſe ſurfaces were plain and poliſh'd; and caſting on them ſucceſſively the rays of the ſame candle, in ſuch a manner that the adjacent ſuperficies being ſhaded by an opake and perforated body, the incident rays paſs'd thro' a round hole of about half an inch in diameter, the circle of light that appear'd on the white marble was, in compariſon, very bright, but very ill defined; whilſt that on the black marble was far leſs luminous, but much better defined.

(3.) When we look upon a piece of linen that has ſmall holes in it, they appear very black; ſo that men are often deceiv'd in taking holes for ſpots of ink: and painters, to repreſent holes, make uſe of black; the reaſon whereof ſeems to be, that the rays which fall on thoſe holes, penetrate ſo deep, that none are reflected back to the eye. And in a narrow well, part of the mouth ſeems black, becauſe the incident rays are reflected downwards, from one ſide to another, till they can no longer rebound to the eye. We may conſider too, that if different parts of the ſame piece of black velvet be ſtroak'd oppoſite ways, there will appear two diſtinct kinds of blackneſs, the one far darker than the other; probably becauſe in the leſs obſcure part of the velvet, the little ſilken piles, whereof 'tis compos'd, being inclined, there is a greater part of each of them turn'd to the eye; whilſt in the other part the piles of ſilk being more erect, there are by far fewer rays ſent outwards from the lateral parts of each pile: ſo that moſt of thoſe reflected to the eye, come from the tops of the piles, which make but a ſmall part of the whole ſuperficies of the velvet. This explanation I propoſe, not that I think the blackneſs of the velvet proceeds from the cauſe aſſign'd; ſince each ſingle pile of ſilk is black by reaſon of its texture, in what poſition ſoever it be view'd; but becauſe the greater blackneſs of a ſingle tuft ſeems to proceed from the greater defect of rays reflected thence, and from the want of thoſe parts of a ſurface that reflect rays, and the multitude of thoſe ſhaded parts that reflect none. And I have often obſerv'd, that the poſition of particular bodies, far greater than piles of ſilk, may, notwithſtanding each of them hath a colour of its own, make one part of their aggregate appear far darker than another. Thus a heap of carrots appear of a much darker colour when view'd with their points, than with their ſides obverted to the eye.

PHYSICS. (4.) I have obſerv'd in a darken'd room, that if the ſun-beams which came in at the hole were receiv'd upon white, or any other colour, and directed to a convenient part of the room, they would manifeſtly increaſe the light of that part; but if we ſubſtituted either a piece of black cloth or black velvet, it would ſo damp the incident rays, that the ſaid place would be leſs illumin'd than before, when it receiv'd its light only from the weak and oblique reflections of the floor and walls of a pretty large room; over which the beams that came in at the hole, were confuſedly and in a broken manner diſpers'd.

(5.) And to ſhew that the rays which fall on black bodies, as they do not rebound outwards to the eye, ſo they are reflected towards the body itſelf, as the nature of thoſe erect particles to which we have imputed blackneſs requires; we ſhall add an experiment, that will at the ſame time confirm our doctrine of whiteneſs. We took, then, a broad and large tile, and having whited over one half of its ſuperficies, and black'd the other, we expos'd it to the ſummer ſun. And having let it lie there a convenient time, we found that whilſt the whited part of the tile remain'd cool, the black'd part of it was grown very hot. And for further ſatisfaction, we have ſometimes left upon the ſurface of the tile a part that retain'd its native red; and expoſing all to the ſun, we obſerv'd the latter to have contracted a heat in compariſon of the white part, but inferior to that of the black. 'Tis alſo remarkable, that rooms hung with black are not only darker than they would be otherwiſe, but warmer too. I have known a great lady, of a tender conſtitution, complain that ſhe commonly took cold upon going into the air, after having made any long viſit to perſons whoſe rooms were hung with black. And this is not the only lady I have heard complain of the warmth of ſuch rooms; which, tho' perhaps it may partly be imputed to the effluvia of thoſe materials wherewith the hangings were dyed, yet probably the warmth in this caſe depends chiefly upon the ſame cauſe with darkneſs; for upon expoſing two pieces of ſilk, the one white, the other black, in the ſame window to the ſun, I have often found the former conſiderably heated, when the latter has remain'd cool.

(6.) A virtuoſo of unſuſpected credit acquainted me, that in a hot climate he had, by carefully blacking the ſhells of eggs, and expoſing them to the ſun, ſeen them thereby well roaſted, in no long time. But in *England*, the ſun's rays ſeem not to be ſufficiently ſtrong to produce ſuch an effect; for having expos'd eggs in the ſummer ſeaſon thereto, they acquired indeed a conſiderable degree of heat, but not enough to roaſt them.

(7.) Laſtly, our conjectures about the nature of blackneſs, may be ſomewhat confirmed by the obſervation of the blind man, formerly mention'd, who diſcerns colours with his fingers; for he ſays, that he feels a greater roughneſs upon the ſurfaces of black bodies, than upon thoſe of red, yellow, or green. And *Bartholine* tells us, that a blind earl of *Mansfield* could diſtinguiſh white from black only by the touch; which might ſufficiently argue a great difference in the aſperities, or ſuperficial textures

of

of the bodies of thoſe two colours; if the learned relator had affirm'd the matter upon his own knowledge. Let us next take in the aſſiſtance of our experiments, purpoſely made to bring us farther acquainted with the nature of white and black.

The nature of whiteneſs and blackneſs ſhewn by experiments.

1. Take any quantity of fair water, heat it, and add thereto as much good common ſublimate as it will diſſolve, or till ſome of it lie untouched at the bottom of the liquor; then filter this ſolution thro' cap-caper, and, to a ſpoonful or two of the clear, add four or five drops of good limpid ſpirit of urine: ſhake them together, and immediately the whole mixture will appear white, like milk. After this, if you preſently add a convenient proportion of rectify'd *Aqua fortis*, the whiteneſs will immediately diſappear, and the whole mixture become tranſparent; which you may, if you pleaſe, again reduce to a conſiderable degree of whiteneſs, by pouring thereinto more freſh ſpirit of urine. It is not neceſſary to employ either *Aqua fortis*, or ſpirit of urine, about this experiment; for we have made it with other liquors.

2. Make a ſtrong infuſion of bruis'd galls in fair water; and having filtered it into a clean vial, add more of the ſame fluid to it, till you have made it ſomewhat tranſparent, and ſufficiently diluted the colour for the credit of the experiment. In this infuſion, ſhake a convenient quantity of a clear, but very ſtrong ſolution of vitriol; and you ſhall immediately ſee the mixture turn black, almoſt like ink: and if, preſently after, you drop into this mixture a ſmall quantity of good oil of vitriol, and, by ſhaking the vial, ſuddenly diſperſe it thro' the two other liquors; you will ſee the dark colour of the whole preſently begin to diſſipate, grow clear, tranſparent, and loſe its inky blackneſs; which may be again reſtored by the affuſion of a ſmall quantity of a ſtrong ſolution of ſalt of tartar. And tho' both theſe atramentous liquors will ſeem very pale, if you write with a clean pen dipt in them; yet that is common to them, with ſome ſorts of ink, which prove very good when dry; as I have found, that when theſe were carefully made, what I wrote with either, eſpecially with the former, would, after a while, turn ſufficiently black. This experiment of deſtroying and reſtoring blackneſs, we have likewiſe try'd in common ink; tho' with this it ſucceeds not ſo well, and but very ſlowly; becauſe the gum uſually employ'd in making it, oppoſes the operations of the ſaline liquors. And tho' it be taken for granted, that bodies will not precipitate with alkalizate ſalts, which have not been firſt diſſolved in ſome acid menſtruum; yet I have found, upon trial, that many vegetables, barely infuſed, or but ſlightly boiled in common water, afford, upon the bare affuſion of a ſtrong and clear lixivium of pot-aſhes, a large quantity of coagulated matter; ſuch as I have had in the precipitations of vegetable ſubſtances, by means of acids; and that this matter was eaſily ſeparable from the reſt of the liquor; being left behind by it in the filtre. And, from the firſt ink mentioned in this experiment, I could, by filtration, ſeparate a conſiderable quantity of a very black pulverable ſubſtance. And when the ink was made clear again, by the oil of vitriol, the affuſion

PHYSICS. of diſſolved ſalt of tartar ſeem'd but to precipitate, and thereby unite, and render conſpicuous, the corpuſcles of the black mixture, that had been before diſperſed, into very minute and ſingly inviſible particles, by the reſolving power of the highly corroſive oil of vitriol. And that galls are not abſolutely neceſſary to make atramentous liquors, appears from the following experiment. We boil'd dry'd roſe-leaves for a while in fair water, and into two or three ſpoons-ful of the decoction, ſhook a few drops of a ſtrong and well filtred ſolution of vitriol; whereupon the mixture immediately turn'd black: and preſently ſhaking herein a juſt proportion of *Aqua fortis*, we changed it from a black to a deep red ink; which, by the affuſion of a little ſpirit of urine, may be reduced immediately to an opake blackiſh colour.

3. In theſe experiments, the infuſion of galls, the decoction of roſes, and the ſolution of vitriol, have each their own colour; but we may ſuddenly produce a blackneſs, by mixing an infuſion of orpiment, and a ſolution of minium, both whereof ſhall be limpid and colourleſs. And with theſe liquors may be exhibited a curious and ſurprizing phenomenon, if made and applied in the following manner: 1. Take of the ſtrongeſt unſlaked lime about two parts, of yellow orpiment one part, of fair water fifteen or ſixteen parts; beat the lime groſly, and powder the orpiment, with care to avoid the noxious duſt: and having put theſe two ingredients into the water, let them remain there for two or three hours; ſtirring the mixture from time to time. Thus you'll obtain a fetid liquor; the clear part whereof muſt be poured off from the reſt, or gain'd by the filtre. 2. In the mean time burn a piece of cork, and quench it, whilſt fired, for ſeveral times ſucceſſively in fair water; and having, by this means, reduced it to a coal, you may eaſily, by grinding it with a ſolution of gum-arabic in water, bring it to the colour and conſiſtence of a good black ink. 3. Take any quantity of red lead, and two or three times its weight of vinegar, or rather the weak ſpirit of it; and, putting the powder and that into a glaſs vial, let them infuſe in ſome conſiderably warm place for two or three hours, till the liquor has acquired a ſweet taſte. Matters being thus prepared, write what you pleaſe with a clean pen dipt in the ſolution of the red lead; which, if filtred, will prove ſo clear, as to be inviſible upon the paper. Over what is thus written, you may draw any characters or letters you pleaſe, with a pen dipt in the black ink made with cork. And, laſtly, to ſhew the experiment, dip a linen rag in the fetid ſolution of the lime and orpiment, which is alſo limpid, and draw it over the written paper; and this will at once both wipe out the ſtrokes of the black ink, and render all that was wrote with the inviſible ink conſpicuouſly black.

4. If pieces of white hart's-horn be, with a moderate degree of fire, diſtilled in a glaſs retort, they will, after the ſeparation of the phlegm, ſpirit, volatile ſalt, and the looſer and lighter parts of the oleaginous ſubſtance, remain one behind of a coal-black colour. And even ivory itſelf, when ſkilfully burnt, affords painters one of the beſt and deepeſt blacks

they

they have. Yet, in the instance of distill'd hart's-horn, the operation being made in glass vessels, carefully closed, no extraneous black substance insinuates itself into the white horn; but the whiteness is destroyed, and the blackness generated only by a change of texture made in the burnt body by the recess of some parts, and the transposition of others. And tho' I remember not to have ever found the *Caput mortuum* of distilled hart's-horn to pass from a black to a true whiteness, whilst it continued in close vessels; yet, having taken out the coal-black fragments, and calcined them in open vessels, I could, in few hours, quite destroy that blackness, and, without sensibly changing their bulk or figure, reduce them to a degree of whiteness: so much do these two colours depend upon the disposition of the little parts that the bodies wherein they are to be met with, consist of. And we find, that if white-wine tartar, or the white crystals of such tartar, are burnt, without being truly calcined, the *Caput mortuum* will be black. But if the calcination be continued till the tartar is perfectly reduced to ashes, and kept long enough in a strong fire, the remaining calx will be white. And so we see, that not only other vegetable substances, but even white woods, as the hazel, will yield a black charcoal, and afterwards whitish ashes. Thus also animal substances, naturally white, as bones and egg-shells, grow black, upon being burnt, and white again, when perfectly calcined.

5. Yet I much question whether the rule, *adusta nigra, perusta alba*, will hold as universally as is presumed; for I have several examples to alledge against it. By burning alabaster so as to make it appear to boil almost like milk, and to reduce it to a very fine powder, it would not grow black at all, but retain its pure and native whiteness: and tho', by keeping it longer than usual in the fire, I produced a faint yellow in that part of the powder which lay nearest the top of the crucible; yet a curious and experienced stone-cutter told me he had found, that if alabaster, or plaister of *Paris*, be very long kept in a strong fire, the whole heap of burnt powder would exchange its whiteness for a much deeper colour than the yellow I observed. Lead calcined with a strong fire, turns at length, to minium, whose colour we know is a deep red; and if this minium be again urged with a strong fire, you will sooner find a glassy brittle body, darker than minium, than any white calx or glass. 'Tis known among chymists, that the white calx of antimony, by a more vehement operation of the fire, may be melted into a glass; which we have obtain'd of a red colour far deeper than that of the calx of burnt antimony: and tho' common glass of antimony, being usually adulterated with borax, have its colour thereby diluted, often to a very pale yellow; yet not only ours, made more genuinely, was, as we said, of a colour less remote from black than the calx; but, by melting it once or twice more, we found the colour heighten'd. And if you burn blue unsophisticated vitriol very slowly, and with a gentle degree of heat, you may observe, that when 'tis burnt only so far as to rub to powder betwixt your fingers, it will be of a white, or whitish colour: but if you prose-

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PHYSICS. cute the calcination, this body will paſs thro' other colours, as a grey, a yellowiſh, and a red: and if you continue it in a long and vehement fire, by that time it comes to be thoroughly calcined, it will be of a dark purple, nearer to black, not only than the firſt calx, but than the vitriol, before it at all felt the fire. I might add, that *Crocus Martis*, made by the laſting violence of the reverberated flames, is not ſo near to white as the iron or ſteel that afforded it, before its calcination.

Theſe inſtances may ſuffice to ſhew, that minerals are to be excepted from the foremention'd rule; which, tho' it ſeldom fails in ſubſtances belonging to the animal or vegetable kingdom, may yet be ſuſpected even in ſome of theſe, if *Bellonius* ſay true, that charcoal, made of the wood of oxycedar, is white. And I could not find, tho' hart's-horn, and other white bodies, will turn black in retorts, by heat, that camphire would at all loſe its whiteneſs; tho' I have purpoſely kept it in ſuch a heat as made it melt and boil.

6. And tho' I could not, in cloſed glaſſes, blacken camphire by heat, but it would ſublime to the ſides and top of the veſſel in its natural form; yet being ſet on fire in the free air, it ſends out a great ſmoke: and having, purpoſely, upon ſome of it, whilſt flaming, clapt a large glaſs, almoſt in the form of a hive, with a hole at the top, it continued burning, ſo as to line all the inſide of the glaſs with a ſoot as black as ink; and in ſo great a quantity, that the cloſeneſs of the veſſel conſider'd, almoſt all that part of the camphire which took fire, ſeem'd to have been changed into that deep black ſubſtance.

7. I took rectified oil of vitriol, and, by degrees, mixed with it a convenient proportion of the eſſential oil of wormwood; and, warily diſtilling the mixture in a retort, there remain'd a ſcarce credible quantity of dry matter, black as a coal. And becauſe the oil of wormwood, tho' a chymical oil, and drawn by a virtuoſo, ſeem'd to have ſomewhat in it of the colour of the plant, I ſubſtituted, in its room, the pure and ſubtile oil of winter-ſavory; and gradually mixing it with an equal weight of the ſame oil of vitriol, and diſtilling them, as before, in a retort, even theſe two clear liquors left me a conſiderable proportion of a ſubſtance black as pitch; which I keep by me as a rarity.

8. Take a little yellow wax, ſcraped, or thinly ſliced, and putting it into a convenient glaſs, pour to it a conſiderable quantity of ſpirit of wine; and, placing the veſſel in warm ſand, increaſe the heat by degrees, till the ſpirit of wine juſt begins to boil; and by continuing that degree of heat, you will quickly find the wax diſſolv'd: then taking it off, you may either ſuffer it to cool as haſtily as with ſafety to the glaſs it can, or pour it, whilſt yet hot, into a filtre of paper; and either in the glaſs where it cools, or in the filtre, you will ſoon find the wax and menſtruum together reduced into a white ſubſtance almoſt like butter; which, by letting the ſpirit exhale, will ſhrink into a much leſs bulk, but ſtill retain its whiteneſs. 'Tis a pretty phenomenon in working of this magiſtery of wax, that the yellowneſs vaniſhes, and neither appears in the ſpirit

ſpirit

ſpirit of wine that paſſes limpid thro' the filtre, nor in the butter, if I may ſo call it, which is wax well and ſuddenly blanched. PHYSICS.

9. We took two parts of common ſublimate, and one of tin-glaſs, both finely powdered, and exactly mixed; theſe we ſublimed together, by proper degrees of fire, and there aſcended a matter of a very peculiar texture, for the moſt part made up of very thin, ſmooth, ſoft, ſlippery plates, almoſt like the fineſt ſort of fiſh-ſcales; but of ſo lovely a white, inclining to pearl-colour, and of ſo curious and ſhining a gloſs, that they appeared, in ſome reſpects, little inferior to orient pearls, and, in others ſeem'd to ſurpaſs them. They were applauded for a ſort of the pretrieſt trifles we had ever prepared to amuſe the eye. I will not undertake, that tho' a man can hardly miſs changing the colour of the tin-glaſs, that he ſhall at the firſt, or perhaps the ſecond trial, hit upon the right way of making this glittering ſublimate.

10. When, in *Aqua fortis*, we diſſolve a mixture of gold and ſilver, melted into one maſs, it uſually happens, that the powder of gold which falls to the bottom, as not being diſſoluble by that menſtruum, will not retain its own yellow, but appear of a black colour; tho' neither the gold, the ſilver, nor the *Aqua fortis* did before manifeſt any blackneſs. And many chymiſts, when they make ſolutions of minerals, are very glad to ſee a black powder precipitated to the bottom; taking it for a hopeful ſign, that thoſe particles are of a golden nature, which appear in a colour ſo common to gold, when ſeparated from other metals by *Aqua fortis*, tho' ſo obſtinate, that it is hard for the refiner to reduce the precipitated calx to its native colour. For tho' that may quickly be done by fire, which will make this gold look very charmingly, and is indeed one of the beſt ways in practice for the refining of gold; yet it requires a watchful eye, and competent skill, to give it ſuch a degree of fire, as will ſerve to reſtore its luſtre, without bringing it to fuſion. I remember, that having taken a flat bright piece of gold refined by a curious and skilful perſon, on purpoſe to try to what height of purity that metal could be brought by art; I found this very piece, as glorious as it look'd, being rubb'd a little upon fine clean linen, ſullied it with a kind of black. And the like I have obſerved in refined ſilver; which I therefore mention, becauſe I formerly ſuſpected, that the impurity of the metal might have been the only cauſe of what I have frequently obſerved in wearing ſilver-hilted ſwords, that where they rubb'd upon my clothes, when made of light-coloured cloth, they would quickly black them. And thus alſo I have found pens blacked nearly all over, upon carrying them about me in a ſilver ink-caſe. And as in theſe ſeveral inſtances of acquired blackneſs, the metals are worn off, or otherwiſe reduced to very minute parts, 'tis a circumſtance that deſerves to be remark'd.

11. That a ſolution of ſilver will dye hair of a black colour, is a known experiment, wherewith ſome perſons of greater curioſity than skill have unluckily made their friends very merry. And I lately diverted myſelf by an improvement of this obſervation; for having diſſolved ſome

PHYSICS. pure ſilver in *Aqua fortis*, and wholly evaporated the menſtruum, I caus'd a quantity of fair water to be poured upon the calx, two or three ſeveral times; and to be each time evaporated, till the calx was very dry, and all the greeniſh blue, that uſually appears in common cryſtals of ſilver, quite vaniſhed: after this, I made thoſe I had a deſign upon, to moiſten ſome part of their skin with their own ſpittle, and ſlightly rub the moiſtened parts with a little of the ſilver thus prepared; upon which they were ſurprized, to ſee that a ſnow-white body ſhould preſently produce a deep blackneſs there, as if the ſtains had been made with ink. This blackneſs could not, like that produced by ink, be readily waſhed away; but required a long time to be got off. And with the ſame calx, and a little fair water, we likewiſe ſtain'd the white hafts of knives with a laſting black, in thoſe parts where the calx was plentifully laid on; but where it was very thinly ſpread, the ſtain was not of quite ſo deep a colour.

An inquiry into the cauſe of blackneſs in the Negroes.

12. The cauſe of blackneſs, in whole nations of *Negroes*, has been long diſputed by learned men; who poſſibly had done well to have conſidered why ſome whole races of other animals, as foxes and hares, are diſtinguiſhed by a blackneſs unuſual to the generality of the ſame ſpecies. However, I ſhall freely acknowledge, that this inquiry ſeems more abſtruſe to me than it does to many others; becauſe, conſulting many authors, the accounts of voyages, and travellers, to ſatisfy myſelf in matters of fact relating to it; I have met with ſome things among them, which ſeem not to agree with the notion of the moſt claſſic writers upon this head. As it is, therefore, my preſent buſineſs to deliver rather matters hiſtorical, than theoretical, I ſhall annex ſome few of my collections, inſtead of a formal diſputation.

It is commonly preſumed, that the heat of the climates, inhabited by *Negroes*, is the cauſe of their colour; and this, principally, becauſe we plainly ſee, that mowers, reapers, and other country people, who ſpend the moſt part of the ſummer-days in the heat of the ſun, have the skin of their hands and faces, which are the parts immediately expoſed to his rays, of a dark colour, and tending to blackneſs. On the contrary, we obſerve, that the *Danes*, and other inhabitants of cold climates, and even the *Engliſh*, who feel not ſo ſevere a degree of cold, have uſually whiter faces than the *Spaniards*, *Portugueze*, and other *Europeans* of hotter countries. But this argument ſeems far more ſpecious than convincing; for tho' the heat of the ſun may darken the colour of the skin, yet experience doth not ſhew, that heat alone is ſufficient to produce a diſcolouration, which ſhall amount to a true blackneſs, like that of the *Negroes*. Beſides, in many parts of *Aſia*, under the ſame parallel, or in the ſame degree of latitude with the *African* regions, inhabited by blacks, the people are but tawny. And in *Africa* itſelf, many nations in the empire of *Ethiopia* are not *Negroes*, tho' ſituate in the torrid zone, and as near the equinoctial, as other nations, that are black. Again, I find not, by the beſt accounts, I could any where obtain of the *Weſt-Indies*, that, excepting, perhaps, one place, or two, of ſmall extent, there are any blacks, originally natives of any part

part of *America*; for the blacks, now there, were, by the *Europeans*, long ago tranſplanted thither: tho' it contains ſo great a variety of climates, and particularly reaches quite croſs the torrid zone, from one tropic to the other. 'Tis true, the *Danes* are a whiter people than the *Spaniards*; yet this may proceed rather from other cauſes, than the coldneſs of the climate; ſince the *Swedes*, and other inhabitants of thoſe cold countries, are not uſually ſo white as the *Danes*, nor whiter than other nations, in proportion to their diſtance from the pole. And, in the numerous train of an ambaſſador extraordinary from the *Ruſſian* emperor, I obſerved, tho' it were then winter, the colour of their hair and skins was far leſs whitiſh than that of the *Danes*, who inhabit a milder region; and rather, for the moſt part, of a darkiſh brown. And the phyſician to this ambaſſador told me, that, in *Muſcovy* itſelf, the generality of the people were more inclined to have dark-coloured hair, than flaxen; but ſeem'd to ſuſpect, that the true and ancient *Ruſſians*, ſome whereof he had met with in one of the provinces of that vaſt empire, were rather white, like the *Danes*, than any thing near ſo brown, as the preſent *Muſcovites*, whom he gueſſes to be deſcended from the *Tartars*, and to have inherited their colour from them.

But further, eminent authors inform us, that there are *Negroes* in *Africa*, not far from the cape of *Good-Hope*, and conſequently beyond the ſouthern tropic; and out of the torrid zone, about the ſame latitude to the north, there are many *American* nations, that are not *Negroes*; and wherein the inhabitants of *Candia*, ſome parts of *Sicily*, and even of *Spain*, are not ſo much as tawny. Nay, I find, by our lateſt accounts of *Greenland*, that the inhabitants there are olive-colour'd, or rather of a darker hue. But if the caſe were the ſame with men, and thoſe other kinds of animals, before mentioned; I ſhould offer a remark, as a conſiderable proof, that cold may do much towards making men white, or black. However it be, I ſhall ſet down the obſervation, as deſerving a place in the hiſtory of whiteneſs and blackneſs. It is affirmed by *Olaus Magnus*, and others, that in ſome parts of *Ruſſia* and *Livonia*, hares, foxes, and partridges, which were black before, or red, or grey, become white in the depth of winter, by reaſon of the great coldneſs thereof. And a virtuoſo, who lately travelled thro' *Livonia* to *Moſcow*, confirms this relation; adding, that himſelf had ſeen ſuch animals there, whilſt they were white, which the inhabitants aſſured him had been black, or of other colours, before the winter began, and that they would be ſo again, when that ſeaſon was over. But, for further ſatisfaction, I alſo conſulted one, who had, for ſome years, been an eminent phyſician in *Ruſſia*, and who, tho' he rejected ſome other traditions, that are generally believed concerning that country, told me, he ſaw no cauſe to doubt of this relation, as to foxes and hares; not only becauſe 'tis the common and unconteſted aſſertion of the natives, but alſo, becauſe he himſelf, in the winter, could never, that he remember'd, ſee foxes and hares of any other colour than white. And I myſelf, having ſeen a ſmall white fox brought out of

PHYSICS. *Ruſſia* into *England* towards the latter end of winter, foretold thoſe who ſhew'd him me, that he would change colour in the ſummer; and accordingly, coming to look upon him in *July*, I found that his back and ſides, with the upper part of his head and tail, were already grown of a dark colour; the lower part of the head and belly ſtill continuing white.

Let me add, that were it not for ſome ſcruple, I ſhould think more than what *Olaus* relates confirm'd by *Olearius*, who, in his Account of *Muſcovy*, has this paſſage: " The hares are there grey, but in ſome provin-" ces they grow white in the winter." And ſoon after—" It is not very " difficult to find the cauſe of this change, which certainly proceeds from " the outward cold; ſince I know that even in ſummer, hares will " change their colour, if they be kept for a competent time in a cellar." But in the ſame page, this author affirms the like change of colour that happens to hares in ſome provinces of *Muſcovy*, happens to them alſo in *Livonia*; and yet immediately ſubjoins, that in *Curland* the hares vary not their colour in winter, tho' the two latter countries be divided only by the river *Dugna*: for it is ſcarce conceivable, how cold alone ſhould have, in countries ſo near, ſo very differing an effect; tho' not more ſtrange, than what is believed by thoſe who aſcribe the complexion of negroes to the heat of the ſun, when they would have the river *Cenega* ſo to affect the *Moors*, that tho' on the north ſide they are but tawny, on the other ſide they are black. There is another opinion as to the complexion of the negroes, not only embrac'd by many of the more vulgar writers, but by men of eminence and learning; who would have their blackneſs an effect of *Noah*'s curſe upon *Cham*. But tho a naturaliſt may ſafely believe all the miracles atteſted by the holy ſcriptures, yet in this caſe to fly to a ſupernatural cauſe, will, I fear, look like ſhifting off the difficulty, inſtead of ſolving it; for we here enquire not into the firſt and univerſal, but the proper, immediate, and phyſical cauſe of the blackneſs in negroes. Beſides, 'tis not expreſs'd in ſcripture, that the curſe meant by *Noah* to *Cham*, was the blackneſs of his poſterity; but 'tis plain this curſe was quite another thing, clearly expreſſing that he ſhould be a ſervant of ſervants, that is, a very abject ſervant to his brethren: which accordingly, in ſome meaſure, came to paſs, when the *Iſraelites*, the poſterity of *Sem*, ſubdued the *Canaanites* that deſcended from *Cham*, and kept them in great ſubjection. But how is blackneſs a curſe? for navigators tell us of black nations, who think ſo differently of their own condition, that they paint the devil white.

Blackneſs is not inconſiſtent with beauty, which depends not ſo much upon colour, as an advantageous ſtature, a comely ſymmetry of the parts of the body, and juſt features in the face; ſo that I ſee not why it ſhould be thought ſuch a curſe to the negroes, unleſs, perhaps, they going naked in thoſe hot climates, the colour of their ſkin probably makes the ſunbeams more ſcorching to them, than they would prove to people of a white complexion. 'Tis very probable that the principal cauſe of blackneſs in negroes, is ſome peculiar and ſeminal impreſſion; for black children,

dren, brought over into these colder climates, lose not their colour. And credible authors inform us, that the offspring of negroes, transplanted out of *Africa* above a hundred years ago, still retain the complexion of their progenitors; tho' possibly, in tract of time, it will decay: on the other hand, white people removing into very hot climates, have their skins, by the heat of the sun, scorch'd into dark colours; tho' neither they nor their children are observ'd, even in the countries of negroes, to descend to a true black. Yet *Piso* tells us, that betwixt the *Americans* and negroes, are generated a distinct sort of men, which they call *Cabocles*; and that betwixt *Portugueze* men and *Ethiopian* women, he has sometimes seen twins, one whereof had a white skin, and the other a black one, with other the like effects of seminal impressions, from whence they seem to proceed; since even organical parts may receive great differences from such peculiar impressions, upon what account soever they came to be settled in the first individuals, from whom they are propagated to posterity; as we see in the thick lips and flat noses of most nations of negroes. And if we may credit what learned men deliver concerning the little feet of the *Chinese*, the *Macrocephali*, taken notice of by *Hippocrates*, will not be the only instance we might apply to our present purpose. And on this occasion it may not be impertinent to add what I have observed in other animals. Thus there is a sort of hens which want rumps; and I have seen a perfectly white raven, both in bill and feathers; which I attentively consider'd, for fear of being impos'd upon. A very ingenious physician has often told me of a young lady, a patient of his, who greatly complain'd of want of health, tho there appear'd so little cause thereof in any respect, that he concluded it wholly imaginary, and advis'd only little journeys of pleasure; when going once to visit St. *Winifrid*'s well, the lady, who was a catholic, and devout in her religion, having continued a pretty while in the water, to perform some devotions, fix'd her eyes very attentively upon the red pebble stones, which, in a scatter'd order, made a large part of those that appear'd thro the water. A while after this, she grew big, went her time, and was deliver'd of a child, whose skin was plentifully speckled with spots of the colour and magnitude of those stones: and tho' this child is many years old, yet she still retains them.

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I have but two things here to add concerning the blackness of negroes; the one is, that the seat of that colour, seems to be only the thin outward skin; for I knew a young black, who, having been lightly sick of the small-pox or measles, had, in the places of the pustules, whitish specks left behind. And *Piso* assures us, that upon dissecting many negroes in *Brazil*, he found their blackness went no deeper than the cuticle, which being removed, the *cutis* appear'd as white as that of *European* bodies. And the like has been affirm'd to me by a physician, who dissected a negroe in *England*. The other thing I shall here take notice of concerning negroes, is, that an intelligent acquaintance of mine, who keeps about three hundred, women and men, to work in his plantations in the *Indies*, told me their children came into the world almost of the like reddish colour

PHYSICS. lour with our *European* children; and that in a few days after they appear'd black. Agreeable hereto is the account lately publiſh'd by a Jeſuit, one of the miſſionaries into *South-America*, who there baptiz'd ſeveral infants; which, when newly born, he ſays, were much of the ſame colour with *European* children, but within about a week begin to put on the hue of their parents. More full is the teſtimony of our countryman, *Andrew Battel*; who being ſent priſoner to *Angola*, lived there, and in the adjoining regions, near eighteen years; for he mentioning the *African* kingdom of *Longo* peopled with blacks, ſays, according to *Purchas*, " The children in " this country are born white, and change their colour in two days to a " perfect black. The *Portugueze*, in the kingdom of *Longo*, have ſometimes " children by the negroe women; and the fathers are often deceiv'd, " thinking that when the child is born, it is theirs, and within two days " it proves itſelf the child of a negroe." And the ſame perſon has elſewhere a relation, which, if we may credit it, is very well worth our notice; ſince this, together with what we have formerly mention'd of ſeminal impreſſions, ſhews it poſſible that a race of blacks might be begun, tho' none of the ſons of *Adam* were for many precedent generations of that complexion. 'Tis ſurely as poſſible, that white parents may ſometimes have black children, as that *African* negroes ſhould ſometimes have laſtingly white ones; eſpecially ſince concurrent cauſes may more eaſily favour the production of the former, than, in the ſcorching heat of *Africa*, that of the latter. And I remember, what may a little countenance this aſſertion, that the poſſeſſor of the white raven I formerly mention'd, affirm'd to me, that in the neſt, out of which he was taken white, they found with him another young one, of as perfect a black as any common raven. But to come to the paſſage itſelf: " There are, ſays our author, ſpeaking of the " regions formerly mention'd, born in this country white children, which " is very rare among them, for their parents are negroes; and when any " of them are born, they are preſented to the king, and are call'd *Dondos*. " Theſe, who are as white as any white men, are the king's witches, " and are brought up in witchcraft, and always wait on the king. There " is no man that dares meddle with theſe *Dondos*; if they go to the mar" ket, they may take what they liſt; for all men ſtand in awe of them. " The King of *Longo* hath four of them." Yet this country in our globes is placed almoſt in the midſt of the torrid zone; and our author elſewhere tells us, that the inhabitants here are ſo fond of their blackneſs, that they will not ſuffer the whites to be buried in their land; of which he annexes a particular example, that may be ſeen in his voyage, preſerv'd by the induſtrious *Purchas*. But it is high time to return to our experiments.

Whiteneſs produced in chymical preparations. 13. The way of producing whiteneſs by chymical precipitations, is well worth our obſerving; for thereby bodies of very different colours and natures, tho' diſſolved in various liquors, are all reduced to white powders. Thus we find, that not only crabs eyes, and pearls, but red coral, and minium, being diſſolved in ſpirit of vinegar, may be uniformly

formly precipitated by oil of tartar into a white calx. Thus ſilver, and tin, ſeparately diſſolved in *Aqua fortis*, will the one precipitate itſelf, and the other be precipitated, by common ſalt-water into white powders; and ſo will crude lead, tin-glaſs, and quick-ſilver, diſſolved in *Aqua fortis*, and precipitated; and many of theſe powders may be made, at leaſt, as fair and white, if, inſtead of oil of tartar, they were precipitated with oil of vitriol. Nay, that black mineral antimony being reduced, by the ſalts that concur to compoſe common ſublimate, into the clear unctuous liquor, called rectify'd butter of antimony, will, as was before obſerved, by the bare plentiful affuſion of fair water, be ſtruck down into a ſnow-white powder; which, when well waſhed from its ſaltneſs, is term'd *Mercurius vitæ*; tho' the like powder may be made of antimony, without the addition of any mercury at all. And this whiteneſs commonly enſues, when ſpirit of wine, impregnated with thoſe parts of gums, or other vegetable concretions, ſuppoſed to abound with ſulphureous corpuſcles, is ſuddenly diluted with fair water. And on a tincture of benjamin, drawn with ſpirit of wine, and brought to be as red as blood, having poured ſome fair water, it preſently united with the tincture, and turn'd the whole mixture white. But if ſuch milky fluids be ſuffered to ſtand at reſt for a convenient time, they uſually let fall to the bottom a reſinous ſubſtance; which the ſpirit of wine, diluted, and weakned by the water, was unable any longer to ſuſtain. And ſomething of kin to this change of colour in vegetables, is that which chymiſts obſerve, in pouring acid ſpirits to the red ſolution of ſulphur, made with an infuſion of pot-aſhes, or ſome ſharp lixivium; when the precipitated ſulphur, before it ſubſides, immediately turns the red liquor white. But tho' moſt precipitated bodies are white, yet ſome are not. Gold, diſſolved in *Aqua regis*, whether precipitated with oil of tartar, or with ſpirit of ſal-armoniac, will not afford a white, but a yellow calx. Mercury alſo, tho' reduced to ſublimate, and precipitated with liquors, abounding in volatile ſalts, as the ſpirits drawn from urine, hart's-horn, and other animal ſubſtances; yet affords, as we formerly noted, a white precipitate: but, with ſome ſolutions, hereafter to be mentioned, it will let fall an orange-tawny powder. And ſo will crude antimony, if, being diſſolved in a ſtrong lixivium, you pour any acid liquor upon the ſolution, newly filtred, whilſt it is yet warm. And if, upon the filtred ſolution of vitriol, you pour a ſolution of lixiviate ſalts, there will ſubſide a large ſubſtance, very far from white, which the chymiſts call ſulphur of vitriol; ſo that the greateſt part of diſſolved bodies being, by precipitation, brought to white powders, and yet ſome affording precipitates of other colours, the reaſon of both the phenomena may deſerve to be inquired into.

Whether black receives no other colour, and white all colours?

14. Some learned moderns are of opinion, that the reaſon why whiteneſs and blackneſs ought to be called the two extreme colours, is, that black receives no other colour, but white very eaſily receives them all. And, not to diſpute about words, or expreſſions, the thing itſelf, that is affirmed

PHYSICS. affirmed as matter of fact, ſeems to be true, in moſt caſes, but not in all. For tho' it be a common obſervation among dyers, that cloth, which has once been thoroughly imbued with black, cannot well afterwards be dyed of a lighter colour; yet the experiments, lately delivered, may ſhew, that, were the change of colour in black bodies attempted, not by mixing thoſe of lighter colours with them, but by adding ſuch things, as are proper to alter the texture of thoſe corpuſcles, which contain the black; 'tis no difficult matter to effect. For inks of ſeveral kinds, may immediately be deprived of all their blackneſs; and thoſe made with log-wood, and red roſes, be changed, the one into a red, the other into a reddiſh liquor. With oil of vitriol, I have ſometimes turn'd black pieces of ſilk into a kind of yellow; and tho' the taffaty were thereby made rotten, yet that no way prejudices the experiment; the change of black ſilk, into yellow, being never the leſs true, becauſe the yellow ſilk is not good. And as for whiteneſs, I think the general affirmation of its being ſo eaſily deſtroyed, or tranſmuted, by any other colour, ought not to be received, without ſome cautions, and reſtrictions. For tho', according to what I formerly noted, lead is, by calcination, turn'd into that red powder, called minium; and tin, by the ſame means, reduced to a white calx; yet the common putty, inſtead of being, as it is pretended, and ought to be, only the calx of tin, is, by the artificers who make it, to ſave the charge of that metal, compoſed but of half tin, and half lead, if not far more lead than tin; and yet the putty, in ſpight of ſo much lead, is a very white powder, without diſcloſing any mixture of minium. And ſo, if you take two parts of copper, which is a high-coloured metal, and but one of tin, you may, by fuſion, bring them into a maſs, wherein the whiteneſs of the tin is much more conſpicuous, and predominant, than the reddiſhneſs of the copper. And a very honeſt man aſſures me upon his own experience, that if arſenic and copper be melted together in a due proportion, the arſenic will blanch the copper both within and without; and that this white mixture, skilfully kept upon the cupel, would preſently let go its arſenic, which made whiteneſs its predominant colour, and return to the reddiſhneſs of copper: ſo that ſome white mineral bodies may be very capable of eclipſing others, and of making their colour predominant in mixtures. And, as a further confirmation of this, I remember, that I had a lump of ſilver and gold, melted together, wherein, by the eſtimate of a very experienced refiner, there might be about a third part of gold; yet the yellow colour of that metal was ſo hid, that the whole maſs appeared to be ſilver; and, when rubbed upon the touch-ſtone, 'twas not eaſily diſtinguiſhed from the touch of common ſilver: tho' if I put a little *Aqua fortis*, upon any part of the white ſurface it had given the touch-ſtone, the ſilver, in the moiſtened part, being immediately taken up, and conceal'd by the liquor, the golden particles would preſently diſcloſe themſelves, and look as if pure gold alone had been rubbed upon the ſtone.

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15. Having fcraped a piece of black horn, with a piece of glafs, into many thin curled fhavings, and laid a quantity of them together; I found, that the heap they compofed, was white: and tho' if I laid it on a clean piece of white paper, its colour feem'd fomewhat eclipfed by the greater whitenefs of that body; yet if I laid it upon any thing very black, it appeared to be of a good white. This eafy experiment feems very oppofite to their doctrine, who would have colours flow from the fubftantial forms of bodies; and that of the chymifts alfo, who afcribe them to one, or other, of the three hypoftatical principles: for tho', in our cafe, there was fo great a change made, that the fame body, without being, fubftantially, either increas'd, or leffen'd, paffes immediately from one extreme colour to another, even from black to white; yet this fo great and fudden change, is effected by a flight mechanical tranfpofition of parts; there being no falt, fulphur, or mercury, added, or taken away; nor any fubftantial form generated, and deftroyed; the effect proceeding only from a local motion of the parts: which fo vary'd their pofition, as to multiply their diftinct furfaces, and qualify them to reflect far more light to the eye, than they could before they were fcraped off from the entire body of the black horn.

Colours depend not on the fubftantial forms of bodies.

16. The chymifts, we know, ufually affign, for the caufe of blacknefs, the fuliginous fteams of aduft fulphur; but many of the preceding experiments will abundantly confute this doctrine. If they here mean the fumes of common fulphur, 'tis proper to remind them, that fufion, or fublimation, does not turn that fubftance black; but it thereby becomes rather more than lefs white; and, when fired, it affords fo little foot, as fcarce, in any degree, to blacken white paper; but the fmoke of it rather blanches linen, and turns red rofes pale. Befides, I have feen a fixed kind of fulphur kept, for many weeks together, in a very violent fire, that did not, when cool'd, appear black, but of a true red. If, by fulphur, they mean the fulphureous principle; upon this fuppofition, torrify'd fulphur fhould afford more blacknefs, than moft other concretes; wherein that principle is fuppofed more deficient. Yet fpirit of wine, tho' totally inflammable, will not, by burning, difcolour white paper, held over it; and the fmoke of our *Tinby* coals has been found, rather to blanch, than blacken linen. To thefe particulars, many others, of the fame kind, might be added; but we need only look back, for further fatisfaction, to our way of making black inks, from fubftances of different colours; for how can it be faid, that, when the component liquors thereof are put together, actually cold, and continue fo, after their mixture, there intervenes any new aduftion of fulphur, to produce the emergent blacknefs? And when, by inftilling a few drops of oil of vitriol, *&c.* the blacknefs, produced in thofe experiments, is prefently deftroy'd; if the colour proceeded only from the fulphureous parts, torrify'd, in the black bodies, I demand, what becomes of them, when the colour fo fuddenly difappears? for it cannot reafonably be faid, that all thofe which fufficed to make fo great a quantity of black matter, fhould

The chymical doctrine of blacknefs confuted.

reſort to ſo very ſmall a proportion of the clarifying liquor, and be diluted by it, without giving it any blackneſs. And if it be ſaid, that the inſtilled liquor diſperſed thoſe black corpuſcles; I demand how that diſperſion comes to deſtroy their blackneſs, but by ſuch a local motion of their parts, as deſtroys their former texture? This may be a matter of ſuch moment, in caſes like ours, that I remember I have, in a few hours, without addition, from ſoot itſelf, obtain'd a large quantity of cryſtalline ſalt, and of a tranſparent liquor; yet this black ſubſtance had its colour ſo altered, by the change of its texture from the fire wherewith it was diſtilled, that it did, for a great while, afford ſuch plenty of very white exhalations, that the receiver, tho' large, ſeem'd to be almoſt fill'd with milk. Secondly, But were it granted, as it is in ſome caſes not improbable, that bodies may receive a blackneſs from a ſooty exhalation, occaſion'd by the aduſtion of their ſulphur, or oily parts; yet this is applicable only to ſome particular bodies, and will afford us no general theory of blackneſs. For if, for example, white hart's-horn, included in veſſels well luted to each other, and expoſed to the fire, be ſaid to turn black, by the infection of its own ſmoke; I think I may juſtly demand, what makes the ſmoke, or ſoot itſelf, black, ſince no ſuch colour appeared before in the hart's-horn? And, with the ſame reaſon, when we are told, that torrify'd ſulphur makes bodies black; I deſire to be told alſo, why torrifaction makes ſulphur itſelf black? Nor will there be any ſatisfactory reaſon aſſigned of theſe queries, without taking in thoſe intelligible mechanical principles of the poſition and texture of the minute parts of the body, with regard to the light and the eye; which may ſerve the turn, in many caſes, where the aduſtion of ſulphur cannot be pretended; as in the apparent blackneſs of an open window, view'd at a a diſtance, &c. in which, and many other caſes, formerly alledged, there appears nothing requiſite to the production of blackneſs, but a prevention of the incident rays of light, from being plentifully enough reflected to the eye *. In ſhort, the doctrine I here oppoſe, is pleaded for, as chymiſts commonly argue about qualities; who content themſelves to ſay in

* For the production of black, Sir *Iſaac Newton* obſerves, that "the corpuſcles of "the body muſt be much leſs than any "of thoſe which exhibit colours. For "at all greater ſizes, there is too much "light reflected, to conſtitute blackneſs. "And from hence, ſays he, may be underſtood, why fire, and the more ſubtile "diſſolver, putrefaction, by dividing the "particles of ſubſtances, turn them to "black; why ſmall quantities of black "ſubſtances impart their colour very freely, and intenſely, to other ſubſtances, "to which they are applied; the minute "particles of theſe, by reaſon of their "very great number, eaſily overſpreading "the groſs particles of others; why glaſs, "ground very elaborately with ſand, on "a copper-plate, till it be well poliſhed, "makes the ſand, together with what is "worn off from the glaſs and copper, become very black; why black ſubſtances "do ſooneſt, of all others, become hot "in the ſun's light, and burn; conſidering the multitude of refractions in a "little room, and the eaſy commotion "of ſuch very ſmall corpuſcles, &c." See *Newton. Optic.* p. 235.

what

what ingredient of a mixed body a particular quality reſides, inſtead of explaining its nature. PHYSICS.

SECT. III.

I Here propoſe to throw together the miſcellaneous experiments I have made, with relation to colours.

Many changes of colour produced by one ſimple ingredient.

I know of no way more likely to convince the generality of men, how great a ſhare the variable texture of bodies may have, in making them appear of differing colours, than by ſhewing how the addition of a ſingle ingredient, that either is colourleſs, or at leaſt has not any of the colours to be produced, is immediately able, by introducing a ſecret change of texture, to make the body 'tis put to, appear ſometimes ſo one colour, ſometimes of another; according as the parts of the body wrought upon, are diſpoſed to receive ſuch a change, as modifies the incident rays of light, after the manner requiſite to make them exhibit a blue, a green, a red, or ſome other particular colour.

For this end, I made choice of the ſpirit of ſalt, as that which is very ſimple; and which, if it be not too highly rectified, may be had clear, and colourleſs. With this ſpirit, I proceeded to make the following experiments, upon ſeveral bodies, whoſe differing textures ſeem'd to fit them for my purpoſe.

1. Some drops of well-coloured ſyrup of violets, being let fall together upon a piece of white paper; if a third, or fourth part ſo much ſpirit of ſalt be mixed with them, the ſyrup will preſently become of a red colour, uſually inclining to purple.

2. But if the liquor to be acted on, be otherwiſe diſpoſed, 'tis poſſible, with ſpirit of ſalt, to turn it from a blue colour, not to a red, but to a green; as I have ſometimes done, by letting fall into a deep ſolution of filings of copper, made with an urinous ſpirit, as that of ſal-armoniac, juſt as many drops of ſpirit of ſalt, as were requiſite to produce the change intended. A very ſmall error, either in exceſs, or defect, may leave the mixture ſtill blue, or bring it to be colourleſs.

3. Upon a few drops of good ſyrup of violets, let fall two or three drops of good ſpirit of urine, hart's-horn, or the like; and when, by mixing them well, the ſyrup has acquired a fine green colour; by putting to it a little of the ſpirit of ſalt, and ſtirring it, you may turn the green ſyrup into a red.

4. If you put a quantity of red roſe-leaves, well dried, into a glaſs vial, almoſt full of fair water; and, ſoon after, put to them as much ſpirit of ſalt as will make the water pretty ſharp; you will quickly ſee, both that liquor, and the contained leaves, brought to a fine lovely red, which they will long retain. The like effect ſpirit of ſalt will have on ſome other vegetables of a ſtiptic, or of an aſtringent nature.

5. But if, by infuſing brazil-wood in fair water, you make a tincture of it, which you may much deepen,by dropping into it a little ſpirit of hart's-horn, or of urine; and you then put to it a little ſpirit of ſalt, it will pre-

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6. If you make an infuſion of true *Lignum Nephriticum*, in ſpring water, it will appear of a deep colour, like that of an orange, when you place the vial between the window and your eye; and of a fine deep blue, when you look on it with your eye placed between it and the window: but if you ſhake into this liquor a few drops of ſpirit of ſalt, the blue colour will preſently vaniſh, and appear no more, in what light ſoever you view the vial; tho' the liquor will ſtill retain the orange-colour.

7. We took common writing-ink, and having let fall ſeveral drops of it upon a piece of white paper; ſo that, when it grew dry in the air, ſome parts of the ink lay thick, and ſome thinner, upon the paper, whereon it ſpread itſelf: we then put a few drops of ſtrong ſpirit of ſalt, ſome on one part of the black'd paper, and ſome on another; and obſerved, that in thoſe places, where the ſpirit had been put, or to which it reached, the blackneſs was quite deſtroyed, and was ſucceeded by an unpleaſant kind of colour, that ſeem'd, for the moſt part, to participate of yellow and blue, neither of them good in kind.

8. If in ſpirit of ſalt, you diſſolve filings of ſteel, and ſlowly evaporate the filtred ſolution, it will ſhoot into a kind of *Vitriolum Martis*, which appears green, as well as that which chymiſts vulgarly make with oil of vitriol. And if you take theſe cryſtals, made with ſpirit of ſalt, and, when they are dry, keep them in a crucible; you will find, that even a moderate fire, if duly apply'd, will make them, in a ſhort time, exchange their green colour for a red, like that of the finer ſort of *Crocus Martis*: as, indeed, this operation makes them referable to that ſort of medicine.

9. We took ſome mercury, precipitated *per ſe*; and tho' crude mercury is not, as far as I have tried, ſoluble in our *Engliſh* ſpirit of ſalt; yet this red precipitate readily diſſolved in that liquor, without at all imparting its own colour to it. I alſo found, that red lead, being boil'd a while in good ſpirit of ſalt, the redneſs totally diſappear'd. So that the ſame agent which produces redneſs in ſeveral bodies, in our two caſes quite aboliſh'd it. Thus alſo, the reddeſt coral being diſſolved in our menſtruum, the redneſs vaniſhes, and the ſolution appears colourleſs.

10. Take ſmall filings of copper, and having poured thereon good ſpirit of ſalt, till it ſwim, about two fingers breadth over them; keep the vial in a pretty ſtrong ſand-heat, till you perceive the menſtruum has diſſolved a competent part of the metal: then warily take out the vial, and holding it between your eye and the light, you will perceive the ſolution of copper to be of a dark and troubled colour, often inclining to a deep, but muddy red.

11. But if you pour this ſolution into a wide-mouth'd glaſs, and let it ſtand for a competent time, the expoſed liquor will appear of a green, much finer than that of the cryſtals of iron. Take

12. Take the clear limpid ſolution of ſilver, or of mercury, made in *Aqua fortis*, and drop upon it ſome ſpirit of ſalt; and you will find the clear liquor turn'd white as milk, which, after a while, will let fall a precipitate of the ſame colour.

13. And if, inſtead of a ſolution of ſilver, or quick-ſilver, you take a red ſolution, or tincture, of benjamin, or of the reſinous part of jalap root; you'll alſo have, upon the affuſion of ſpirit of ſalt, a white liquor, and a precipitate of the ſame colour.

14. Being deſirous to produce two differing colours at once, by the ſame affuſion of ſpirit of ſalt; I infuſed ſome dry'd red roſe-leaves in fair water, till it had acquired a deep colour from them: to this infuſion, poured off warily, that it might be clear, I added a conſiderable proportion of the ſweet liquor, made by digeſting ſpirit of vinegar upon red lead; by which I knew 'twould be turn'd of a bluiſh green. Upon this almoſt opake liquor, I pour'd ſpirit of ſalt, which precipitated the lead that had been diſſolved in the ſweet liquor, into a very white powder, and gave the remaining liquor, well impregnated with particles of the roſe-leaves, a very fine and durable ſcarlet colour. And if the experiment be well made, you may barely, by ſhaking together, and confounding the white powder with the red liquor, make a carnation-colour, which will appear very fine and lovely, whilſt it laſts; but, in no long time, the two ſubſtances that compoſe it, ſeparate, by degrees, and appear each in its former place and colour.

15. We took ſome ſpirit of ſalt, that, having laid long upon filings of copper, had loſt the muddy tincture it firſt acquired, by being almoſt boil'd upon them. This liquor, that look'd like common water, we pour'd into a ſmall wide-mouth'd cryſtal glaſs, and leaving it in a window, it appeared, in forty minutes, to have acquired a colour, much like that of a *German* amethyſt; and ſeem'd to have no tendency to greenneſs. But in about three hours time, it appear'd of a lovely green.

16. Precipitate a ſtrong ſolution of ſublimate made in fair water, with a ſufficient quantity of oil of tartar, *per deliquium*; put the liquor and powder into a filtre of cap-paper, and when the water is run thorough, the precipitate will remain in the filtre; which is to be ſlowly dry'd. Then take it out of the filtre, in the form of a groſs powder, and having put it into a clear glaſs, let fall on it, warily, ſome drops of ſtrong ſpirit of ſalt; and, during the conflict that will be made, the little lumps of the precipitate will loſe all their former brick-duſt colour, and turn white; tho' afterwards they will appear diſſolved into a tranſparent liquor, wherein the orange-colour is quite aboliſhed.

17. Having calcined copper, without any addition, but of fire and water; we took a quantity of it, and having poured thereon about three or four times its quantity of good ſpirit of ſalt, we obtain'd a muddy reddiſh liquor, and a white powder, whoſe quantity bore a conſiderable proportion to the part that was diſſolved; in which part itſelf, by the affuſion of common water, and the action of the air, we afterwards produced more than one change of colour.

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18. We ſometimes took a ſpoonful of the dark brown, or ſomewhat reddiſh ſolution of copper; and having put it into a cylindrical vial, that the change of colour might appear the better, we poured on it two or three ſpoons-ful of highly rectify'd vinous ſpirit; and giving the glaſs a ſhake, to mingle them, we preſently had a lovely green liquor; which, when well ſettled, was very fair, and look'd as if it were a liquid emerald.

19. Having moiſten'd a ſmall part of ſome green taffaty-ribband, twice or thrice, with good ſpirit of ſalt, we ſuffer'd it to dry of itſelf, which it did in a ſhort time; and then found, that the part wetted was no longer of a green, but a blue colour. The ſame ſpirit preſently turn'd that part of a piece of black ribband, upon which we put two or three drops of it, to a colour not unlike what is call'd *Fueille morte*.

20. 'Tis uſual to ſee books covered with paper, that looks ſometimes of a greeniſh blue colour, bordering upon purple; and ſometimes upon that of violets. The deeper coloured paper of this ſort, I have ſeveral times held in one hand, and with the other lightly touched it here and there with the end of a feather, dipped in ſpirit of ſalt; which almoſt in the twinkling of an eye, dy'd the touched parts of the paper of a lovely red, that would ſometimes long continue very vivid. And if, inſtead of a feather, I uſed a bruſh, dipt in the ſaline ſpirit, and made many drops at once fall upon the paper; 'twas pleaſant to ſee how ſuddenly it would be ſpeckled.

21. Upon well-powdered antimony, we pour'd three or four times its weight of good ſpirit of ſalt; and cauſed it to be boiled in this liquor, in a glaſs veſſel, wherein a part of it was diſſolved, and taken up into the menſtruum; when the antimony quite loſt its blackneſs. And this ſpirit of ſalt, thus impregnated, being dropped into fair water, the black mineral immediately ſubſided, in the form of a very white powder or precipitate.

Hence, by the way, it were not amiſs, if phyſicians, chymiſts, *&c.* who compound drugs, or other ingredients, would be leſs forward than they uſually are, to jumble ſeveral of them together either unneceſſarily, or without a due regard to their qualities, in reſpect of one another. For moſt of us are but too liable to be miſtaken, when we preſume before-hand to ſay what changes the coalition, or other aſſociations of differing bodies ſhall produce; eſpecially if they be either ſaline, or plentifully partake of a ſaline nature: ſince experience frequently ſhews, that by the action and re-action conſequent upon untry'd ways of compoſition, there ariſe in the mixture new and unlook'd-for conſiſtences and other qualities or accidents. And tho' it may ſometimes happen, that theſe new qualities ſhall prove advantageous, yet this may well be look'd upon but as a lucky chance; and it may ſtill be juſtly fear'd, that, ordinarily, ſuch accidental qualities of a medicine will prove to be either worſe than were expected, or at leaſt different from what was deſign'd, and conſequently leſs fit for the phyſicians or the artiſts purpoſe.

1. *Octob.*

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Experiments made in a darken'd room.

1. *Octob.*11. About ten in the morning, the weather being ſun-ſhiny, we took ſeveral ſorts of ſtain'd paper, ſome of one colour, and ſome of another, and in a darkned room, whoſe window look'd ſouthward, we with them caſt the beams that enter'd at a hole about three inches and a half in diameter, upon a white wall that ſtood on one ſide, about five foot diſtance from them. The white gave much the brighteſt reflection. The green, red, and blue being compared together, the red gave much the ſtrongeſt reflection, and manifeſtly threw its colour upon the wall; the green and blue were ſcarce diſcernible by their colours, and ſeem'd to reflect an almoſt equal light. The yellow, compared with the two laſt, reflected ſomewhat more light. The red and purple being compared together, the former manifeſtly reflected much more light than the latter. The blue and purple compared together, the former ſeem'd to reflect ſomewhat the more light, tho' the purple colour was moſt viſible. A ſheet of very well ſleek'd marbled paper being apply'd as the others, did not caſt any of its diſtinct colours upon the wall, nor throw its light with an equal diffuſion thereon; but ſent the beams unſtain'd and bright to particular parts thereof, as if its poliſh had given it the nature of a ſpeculum. But comparing it with a ſheet of white paper, we found the reflection of the latter to be much the ſtronger; this diffuſing almoſt as much light to a conſiderable extent, as the marble paper did to one part of the wall. The green and purple left us ſomewhat in ſuſpence which reflected the moſt light; only the purple ſeem'd to have ſome little advantage over the green, which was dark in its kind.

2. Though a darken'd room be generally thought requiſite to make the colour of one body appear by reflection from another that is not ſuppoſed to act as a ſpeculum; yet I have often obſerv'd, that when I wore waſtcoats lined with ſilk that was very gloſſy and vividly colour'd, but eſpecially if red, I could in an enlighten'd room plainly diſcern the colour upon the pure linen that came out at my ſleeve; as if that fine white body were more of a mirror than colour'd and unpoliſh'd bodies are uſually ſuppoſed.

3. Holding ſtain'd ſheets of paper, ſometimes one, and ſometimes another, before the hole of the window in the darken'd room, betwixt the ſun and the eye, with the colour'd ſides obverted to the ſun, we found them ſingly to be ſomewhat tranſparent, and to appear of the ſame colour as before, only a little alter'd by the great light they were placed in: but applying two of them one over another to the hole, the colours were compounded as follows. The blue and yellow ſcarce exhibited any thing but a darker yellow, which we aſcribed to the coarſeneſs of the blue paper, and its darkneſs in its kind. For applying the blue parts of the marbled paper with the yellow paper after the ſame manner, they exhibited a good green. The yellow and red look'd upon together, gave us but a dark red, a little inclining to an orange-colour. The purple and red view'd together, appear'd more deeply ſcarlet. The purple and yellow made an orange. The green and red made a dark orange-tawny. The

green

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4. The triangular priſmatic glaſs being a very commodious inſtrument to ſhew the nature of colours, we thought proper to obſerve the ſeveral reflections and refractions which the rays of light undergo in falling upon or paſſing through it. And this we imagined might be beſt done, not in an ordinary enlighten'd room, where even the curious have left particulars unregarded; but in a darken'd one, where, by placing the glaſs in a convenient poſture, the various reflections and refractions may be diſtinctly obſerv'd; and where it may appear what rays are untinged, and which they are that paint upon the bodies whereby they are ſtopt, either the primary or ſecondary iris. In purſuance hereof, we obſerv'd in a darken'd room four reflections and three refractions, that were afforded us by the ſame priſm. And notwithſtanding the rules of catoptrics and dioptrics, we thought it would not be amiſs to find, by covering ſometimes one part of the priſm, and ſometimes another, and obſerving where the light or colour vaniſhed thereon, by which reflection and by which refraction each of the ſeveral places where the light falling, or paſſing through the priſm, and appearing either pure or ting'd, gave the phenomena. But becauſe theſe and other particulars which we obſerv'd, would be tedious, and not ſo intelligible to deliver in words, I refer to the ſcheme, where all of them may be taken in at one view.

Fig. 8. PPP an equilatero-triangular cryſtalline priſm, one of whoſe edges P is placed next the ſun.

A B and $\alpha\,\beta$, two rays from the ſun falling on the priſm, at B β; and thence partly reflected towards C and γ, and partly refracted towards D and δ.

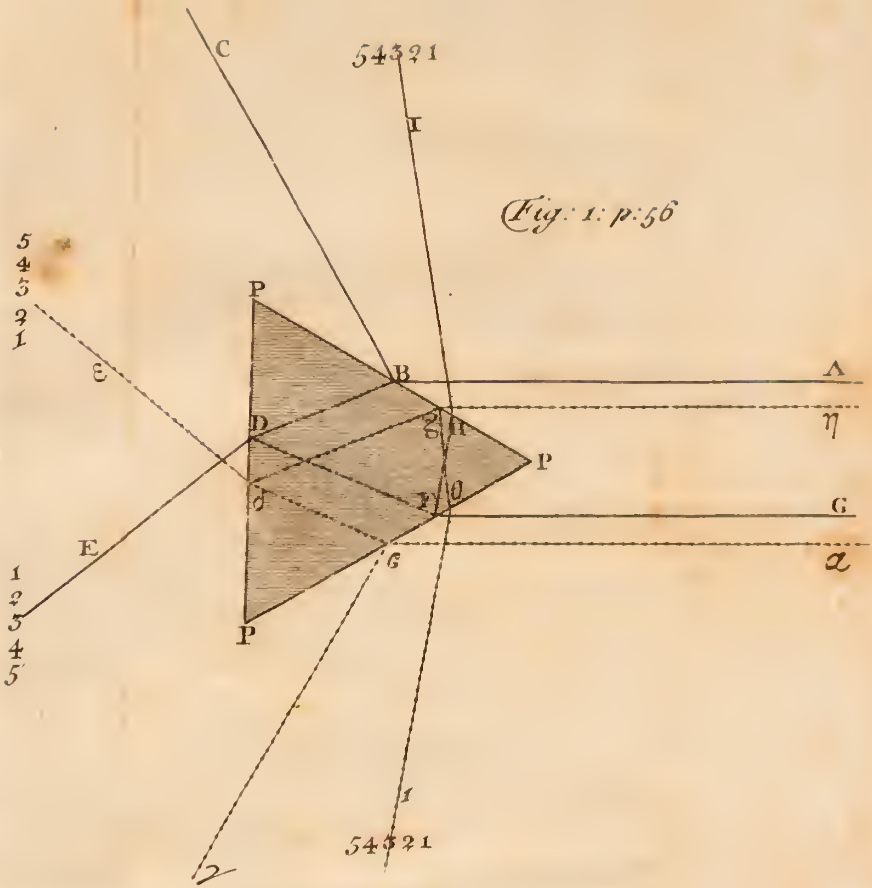
B C and $\beta\,\gamma$, thoſe reflected rays.

B D and $\beta\,\delta$, thoſe refracted rays, which are again partly refracted towards E and ϵ, and there repreſent an iris 12345, denoting the five ſucceeding colours, red, yellow, green, blue, and purple, and partly reflected towards F and ζ.

D and F, and $\delta\,\zeta$, thoſe reflected rays, which are partly refracted towards G and η colourleſs, and partly reflected towards H and θ.

F H and $\zeta\,\theta$, thoſe reflected rays, which are refracted towards I and ι, and there repreſent another fainter iris; the colours of which are contrary to the former 54321, ſignifying purple, blue, green, yellow, red; ſo that the priſm in this poſture repreſents four rain-bows.

5. We obſerv'd in a room not darken'd, that the priſmatic iris might be reflected, without loſing any of its ſeveral colours, not only from a plain looking-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looking-glaſs, and from the ſmooth ſurface of fair water, but alſo from a concave looking-glaſs; and that refraction did as little deſtroy thoſe colours, as reflection. For by the help of a large double-convex glaſs, thro' which we refracted the ſun's rays, we found that tho' one part of the iris might be made to appear either beyond or on this ſide of the other parts thereof; yet the ſame vivid colours would appear in the diſorder'd part as in the other. And by covering that ſide of the priſm obverted to the ſun, with an opake body, wherein only one ſmall hole was left for the light to paſs thro', having reduced the priſmatic iris, caſt upon white paper, into a very narrow compaſs, and view'd it thro' a microſcope, the colours appear'd the ſame, as to kind, they did to the naked eye *.

6. It may afford matter of ſpeculation, that as the colours of outward objects brought into a darken'd room, ſo greatly depend, in their viſibility, upon the dimneſs of the light they are there beheld by, that the ordinary light of the day being freely let in upon them, they immediately diſappear; ſo our experiments have ſhewn, that as to the priſmatic iris, painted on the floor by the beams of the ſun paſſing thro' a triangular glaſs, tho' the colours of it appear very vivid, even at noon-day and in ſun-ſhiny weather, yet by a more powerful light they may be made to diſappear. For having ſometimes taken a large metalline concave ſpeculum, and therewith caſt the converging beams of the ſun upon a priſmatic iris, which I had cauſed to be projected upon the floor; I found that the over-powerful light made the colours of the iris vaniſh: and if the light were ſo reflected as to croſs but the middle of the iris, the colours in that part only

* The more general phenomena of the priſm, as obſerv'd by the illuſtrious Sir *Iſ. Newton*, are, in ſhort, as follow.

1. The rays of light paſſing thro' the priſm, paint an image of various colours on the oppoſite wall; the principal whereof are red, yellow, green, blue, and violet.

2. This image is not round, but the angle of the priſm being of 60 or 65 degrees, about five times longer than 'tis broad; the reaſon whereof was unknown, till Sir *Iſ. Newton* found it owing to the different refrangibility of the rays. See *Philoſ. Tranſact.* N° 80.

3. The rays which exhibit the yellow colour, are turn'd more out of their ſtrait courſe, than thoſe which make the red; thoſe which make the green, more than thoſe which make the yellow, &c. and thoſe which make the violet, moſt of all.

4. If the priſm thro' which the rays are tranſmitted, be turn'd about its axis, ſo that the red rays, the yellow, the green, &c. may fall thro' a ſmall hole, in order, upon another priſm, placed about twelve feet from the former, and be thrown to another part of the room, the yellow rays, for inſtance, tho' they fall in the ſame manner upon the ſecond priſm with the red, yet will not be thrown to the ſame place, but beyond them. And if at the place of the ſecond priſm, they be collected by a lens, the yellow rays, the green, &c. will each in their order, be thrown to a focus at a ſhorter diſtance than the red.

5. The colours of the coloured rays, when well ſeparated, can neither be deſtroy'd, nor in any manner changed by new refractions.

6. The colours of the coloured rays remain unchanged in paſſing thro' an enlighten'd ſpace, in mutually croſſing each other, in the confine of ſhadow, and in being reflected from any natural bodies in a place otherwiſe dark.

7. All the coloured rays collected together by ſeveral priſms, by a glaſs lens, or

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Objects view'd in different kinds of light. 7. I have ſometimes thought it worth while to take notice, whether the colours of opake bodies might not appear to the eye ſomewhat diverſify'd, as well by the nature of the lucid body that ſhines upon them, as by the diſpoſition of the ſuperficial parts of the bodies themſelves, and the poſition of the eye with regard to the object and the light: I have obſerv'd a manifeſt difference in ſome kinds of colour'd bodies view'd by day-light, and afterwards by the light of the moon. Several ſheets of the colour'd paper that had been view'd in the ſun-ſhine, being look'd upon at night by the light of a pretty large candle, the changes obſerv'd therein were theſe. The yellow ſeem'd much fainter than in the day, and inclinable to a pale ſtraw-colour. The red ſeem'd little chang'd, but reflected light more ſtrongly than the other colours. A fair deep green look'd upon by itſelf, ſeem'd to be a dark blue; but view'd together with a dark blue, appear'd greeniſh; and compared with a yellow, appear'd more blue than at firſt. The blue look'd more like a deep purple or murrey, than it had done in the day-light. The purple ſeem'd very little alter'd. The red compared with the yellow, made the latter look almoſt like brown cap-paper.

8. But to ſatisfy ourſelves whether the light of the candle were not unſincere, or ting'd with a yellow colour by its fuel; we took a pretty thick cylinder of deep blue glaſs, and viewing the flame of the candle at a convenient diſtance thro' it, we perceiv'd it to look green, which is the colour that emerges from the compoſition of opake bodies, one of them ſeparately blue, and the other yellow. And this perhaps may be

or a concave ſpeculum, or by any other way whatever, make white: and being again ſeparated after this union, every one exhibits its own colour.

8. If the ſun's rays fall upon the internal ſurface of the priſm in the moſt oblique manner that any rays can poſſibly be tranſmitted, thoſe reflected will be violet, and thoſe tranſmitted, red.

9. All natural bodies, eſpecially white ones, view'd thro' a priſm, appear on the one hand fringed with red and yellow; but with violet and blue on the other.

10. Two priſms being placed together, ſo that the red of the one and purple of the other may unite upon a convenient paper in the dark, the image will appear pale; and being view'd thro' a third priſm, applied at a due diſtance from the eye, it will appear as two objects, red and purple.

11. In like manner, if two powders, a perfect red and a perfect blue, be mix'd together, and laid thick upon ſome minute body, this body will appear double thro' a priſm, and of two colours, red and blue. See *Clark. Annot. in Rohault.* Ed. 3. p. 199.

We are farther to conſider, that by other means, light is found to be a body propagated in right lines; and that it ſpends about ſeven or eight minutes in paſſing from the ſun to the earth. "This "was firſt obſerved by Mr. *Roemer*, by "means of the eclipſes of the ſatellites "of *Jupiter*. For theſe eclipſes, when "the earth is between the ſun and *Jupiter*, happen about ſeven or eight minutes ſooner than they ought to do by "the tables; and when the earth is beyond the ſun, they happen about ſeven "or eight minutes later than they ought "to do: the light of the ſatellites having "farther to go in the latter caſe than in "the former, by the diameter of the "earth's orbit." *Newton. Optic.* p. 252.

be the chief reaſon why a ſheet of very white paper, view'd by candle-light, is not eaſily, at firſt, diſtinguiſhed from a light yellow, or lemmon-colour; white bodies, as we formerly obſerved, being more of a ſpecular nature, becauſe tho', when unpoliſhed, they exhibit not the ſhape of the luminary that ſhines on them; yet they reflect its light more ſincere and undiſturbed, by either ſhades or refractions, than bodies of other colours do.

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9. We took a piece of leaf-gold, and with the edge of a knife, lightly moiſten'd, laid upon the edge of the gold, we ſo faſten'd it thereto, that it continued extended, like a little flag; which, being held very near the eye, and obverted to the light, appear'd ſo full of pores, that it ſeem'd to have ſuch a kind of tranſparency, as that of a ſieve; but the light which paſſed thro' theſe pores was in its paſſage ſo temper'd with ſhade, and modify'd, that the eye diſcerned no more a golden colour, but a greeniſh blue. And, for further ſatisfaction, we, in the night, look'd upon a candle, thro' ſuch a leaf of gold; and, by trying the effect of ſeveral proportions of diſtance betwixt the leaf, the eye, and the light, we quickly hit upon ſuch a poſition for the gold, wherein the flame, view'd thro' the leaf, appeared of a greeniſh blue, as in the day-time. But the like experiment, try'd with a leaf of ſilver, did not ſucceed.

Experiments with the tincture of Lignum Nephriticum.

10. Druggiſts have a wood they call *Lignum Nephriticum*; becauſe the inhabitants of the country where it grows, uſe the infuſion of it, made in fair water, againſt the ſtone in the kidneys: and, indeed, an eminent phyſician of my acquaintance, who has very particularly enquired into that diſeaſe, aſſures me, he found ſuch an infuſion one of the moſt effectual remedies he ever try'd, againſt that formidable diſeaſe. The moſt ancient account I have met with of this ſimple, is given us by *Monardes*, who ſays, "We have a thick, ſmooth kind of wood, "brought us from *New Spain*, which has long been uſed in the diſ-"eaſes of the kidneys and urinary paſſages. To prepare a medicine "from it, we ſteep its ſhavings in clear water, and there ſuffer them "to remain, till the whole liquor is drank up; but when it has thus "ſtood for half an hour, the water appears of a blue colour, which "gradually increaſes, the longer the infuſion is continu'd; tho' the wood "itſelf be white." This wood will afford us an experiment, which, beſides the ſingularity of it, may greatly aſſiſt to diſcover the nature of colours. The experiment, as we made it, is this. Take a handful of the thin ſlices of *Lignum Nephriticum*, and put them into two, three, or four pound of the pureſt ſpring-water; let them infuſe there for a night, tho' a much ſhorter time may ſuffice; decant the impregnated water into a clear glaſs vial; and if you then hold it directly between the light, and your eye, you will ſee it tinged, (excepting the very top of the liquor, where a ſky-coloured circle ſometimes appears) of an almoſt golden colour, if the infuſion be not made too ſtrong of the wood; for, in that caſe, it will, againſt the light, appear ſomewhat dark and reddiſh, and requires to be diluted by the addition of fair water. But if you hold it from the

 light,

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And thus far we have try'd the experiment, and found it to ſucceed, even by candle-light. But if you hold the vial over-againſt your eye, ſo that it may have a window on one ſide, and a dark part of the room before it, and on the other ſide; you will find the liquor, partly of a bluiſh, and partly of a golden colour. And if, turning your back towards the window, you pour out ſome of the liquor towards the light, and towards your eye; it will ſeem, upon coming out of the glaſs, to be perfectly blue; but when it is fallen down a little way, the drops may appear party-colour'd, according as the rays of light more or leſs fully penetrate, and illumine them. If you take a baſon, about half full of water, and having placed it ſo in the ſun-beams, that one part of the water may be illumined thereby, and the other part of it darken'd by the ſhadow of the brims of the baſon; and then drop of our tincture, made ſomewhat ſtrong, both into the ſhaded and illuminated parts of the water, you may, by viewing it from ſeveral places, and by a little agitation of the water, obſerve many pleaſing phenomena. If a little of this tincture be poured upon a ſheet of white paper, ſo that the liquor may remain of ſome depth upon it, you will perceive the adjacent drops to be partly of one colour, and partly of another, according to the poſition of your eye, with regard to the light: but if you pour off all the liquor, the paper will ſeem dy'd almoſt of a yellow colour. And if a ſheet of paper, with ſome of this liquor on it, be placed in a window, where the ſun-beams may come freely to it; then, if you turn your back to the ſun, and take a pen, or ſome ſuch ſlender body, and hold it a-thwart, betwixt the ſun and the liquor, you may perceive, that the ſhadow projected by the pen, upon the liquor, will not, all of it, be a common dark ſhadow, but, in part, curiouſly coloured; that edge of it next the body, which makes it, being almoſt of a lively golden colour; and the remoter verge of a blue one. Theſe, and other phenomena, which I have obſerved in this delightful experiment, many of my friends have beheld with wonder. And I remember an excellent oculiſt, finding, by accident, a vial full of this liquor, and having never heard of the experiment, continued apprehenſive, upon viewing it for a long time, that ſome ſtrange new diſtemper had invaded his eyes. And, I confeſs, the oddneſs of the phenomena made me very ſollicitous to find out the cauſe thereof. But tho' I am far from pretending to have diſcovered it; yet my inquiries have enabled me to give ſome conſiderable hints about it. In the firſt place, then, obſerving that this tincture, if it were too deep, kept the colours from being ſo lively, and their change from being ſo diſcernible; and finding alſo, that the impregnating virtue of this wood did, by being frequently infuſed in freſh water, gradually decay, I conjectured, that the tincture afforded by the wood, muſt proceed from ſome ſubtile parts, drawn out by the water; which, ſwimming about therein, ſo modify'd

the

the light, as to exhibit particular colours. And becauſe theſe ſubtile parts were ſo eaſily ſoluble, even in cold water, I concluded, that they muſt abound with ſalts, and perhaps contain much of the eſſential ſalt of the wood. And, to try whether theſe ſubtile parts were volatile enough to be diſtilled, without diſſolving their texture, I carefully ſubmitted ſome of the tinged liquor, in very low veſſels, to the gentle heat of a lamp-furnace; but found all that came over, to be as limpid and colourleſs as rock-water: whilſt the liquor remaining behind, was of ſuch a deep blue, that it muſt be oppoſed to a very ſtrong light, to appear of any other colour. I took, likewiſe, a vial, fill'd with ſpirit of wine, and a little ſalt of hart's-horn, and found, that there was a certain proportion to be met with, betwixt that liquor, and the ſalt, which render'd the mixture fit to exhibit ſome little variety of colours, not obſervable in ordinary liquors, as it was variouſly ſituated, with regard to the light and the eye: but this change of colour came very far ſhort of that we had admired in our tincture. However, I ſuſpected that the tinging particles abounded with ſalts, whoſe texture, and the colour thence ariſing, would probably be altered by piercing acids; and pouring into a ſmall vial of the impregnated water a very little ſpirit of vinegar, I found, that the blue colour immediately vaniſhed, while the golden one remained: and which way ſoever I turn'd the vial, either to, or from the light, I found the liquor conſtantly appear'd of a yellowiſh colour, and no other. Upon this, I imagin'd, that the acid ſalts of the vinegar, having been able to deprive the liquor of its blue colour, a ſulphureous ſalt, which is of a contrary nature, would mortify the ſaline parts of the vinegar, and deſtroy their effects. And, accordingly, having placed myſelf betwixt the window and the vial, and let fall into the ſame liquor a few drops of oil of tartar, *per deliquium*; I found, that immediately upon the diffuſion of this liquor, the impregnated water was reſtored to its former blue colour. And this oil of tartar, being very ponderous, and falling directly to the bottom of the vial; it was eaſy to obſerve, that for a little while the lower part of the liquor appeared of a deep blue, whilſt all the upper part retained its former yellowneſs; which it immediately loſt, as ſoon as the oil of tartar was diffuſed thro' the whole: and the liquor thus reſtored, being view'd, either againſt, or from the light, exhibited the ſame phenomena with the tinged water, before the adventitious fluids were poured into it.

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Kircher ſpeaks of this nephritic wood, in the following manner: "'Tis a white *Mexican* wood, and tho', generally, ſuppoſed to communicate only a blue colour to water; yet we have found, by repeated experiments, that 'twill turn it into all kinds of colours. The tree itſelf, 'tis ſaid, commonly grows to a large ſize, with a thick even trunk, like a pear-tree, leaves reſembling thoſe of rue, and ſmall oblong yellow flowers, growing in cluſters. The plant is cold and moiſt, and nearly of a middle temper. The wood of this tree, made into a cup, turns the water put into it, firſt of a perfect blue colour, like the bugloſs-flower; and the longer it ſtands herein, the deeper the colour

" grows.

PHYSICS. " grows. But if this colour'd water be put into a glaſs globe, and ex-
" poſed to the light, there will not appear the leaſt ſign of blueneſs there-
" in; but the liquor will be clear and limpid, like pure water. Yet if
" this globe of glaſs be turn'd to a more ſhady place, the whole liquor
" will appear of a pleaſant green; and if to a place ſtill more ſhaded,
" of a reddiſh colour. And thus 'twill change its hue, according to the
" objects whereto 'tis expoſed. But, after being placed in the dark,
" 'twill regain its own blue caſt." In this account, I obſerve the following particulars: Firſt, the author calls it a white *Mexican* wood; tho', not to mention that *Monardes* ſays, 'tis brought from *New Spain*, the wood that we have met with, and employ'd as *Lignum Nephriticum*, was not white, but, for the moſt part, of a dark colour, not unlike that of the ſadder-colour'd wood of juniper. 'Tis true, *Monardes* ſays, that the wood is white; and it is alſo affirmed, that what is of a ſadder colour, is adulterated, by being imbued with the tincture of a vegetable. But having enquired of the moſt eminent of our *Engliſh* druggiſts, he peremptorily deny'd it. And, indeed, having conſidered ſome of the faireſt round pieces of this wood, that I could procure, I took notice, in one or two of them, that 'twas the external part only that appeared white, whilſt the more internal part was of the other colour; the contrary whereto would, probably, have appeared, if the wood had been adulterated after the manner juſt mentioned. And I have, at preſent, by me, a piece of ſuch wood, which, for about an inch next the bark, is white, and then ſuddenly paſſes to the above-ſaid colour: and yet this wood, by the tincture it affords in water, ſeems to have its colour'd part genuine; for the white part appears, upon trial, to be much leſs endow'd with the tinging property.

Next, our author tells us, that the infuſion of this wood expos'd in a ſpherical vial to the light, looks like ſpring-water; and adds, that there is no tincture to be ſeen in it: but herein our obſervation and his do not agree; for the liquor, which oppos'd to the darker part of a room exhibits a sky-colour, did conſtantly with us, when held againſt the light, appear yellowiſh or reddiſh, according as its tincture was more or leſs deep. So that if there be no miſtake in the caſe, his white nephritic wood, and the ſadder-colour'd one, which we employ'd, were not of the ſame nature. What he mentions of the cup made of this wood, we have not try'd; but as for what he ſays, that this wood tinges the water with all ſorts of colours, that is much more than any of thoſe pieces we have hitherto employ'd was able to do: the change of colours diſcernible in a vial full of water impregnated by any of them, and directed towards a place more light or obſcure, being far from affording any ſuch variety. And as for what he tells us, that in the dark this infuſion will reſume a blue colour, I wiſh he had inform'd us how he try'd it. But having ſometimes brought a round long-neck'd vial fill'd with the tincture of this wood into the darken'd room already often mention'd, and holding it ſometimes near the ſun-beams that enter'd at the hole, and ſometimes partly in them

and

and partly out of them; varying alſo the poſition of the glaſs, and viewing it from ſeveral parts of the room, it diſclos'd a much greater variety of colours, than it does in an ordinary enlighten'd room: for beſides the uſual ones, it exhibited a red in ſome parts, and a green in others; with intermediate colours produced by the different degrees and odd mixtures of light and ſhade.

'Tis ſurprizing in this experiment, that the blue tincture may be deſtroy'd or reſtor'd, whilſt the yellowiſh or reddiſh one continues as it was. That ſalts are of a conſiderable uſe in ſtriking colours, the many experiments which the dyers trade affords us, will ſhew: and as far as we have hitherto try'd, thoſe liquors, in general, which greatly abound in acid ſalts, have the power of deſtroying the blue colour in this infuſion; as thoſe that abound in ſulphureous ones, have the virtue to reſtore it. And by the way, this obſervation hints to us a new and eaſy method of diſcovering in many liquors whether it be an acid or a ſulphureous ſalt that is predominant. That ſuch a diſcovery is often very difficult, and may frequently be of great uſe, he who is no ſtranger to the various properties and effects of ſalts, and knows of how great moment it is to be able to diſtinguiſh their tribes, will readily conceive.

To find whether an acid or a ſulphureous ſalt predominates in a liquor.

Now ſuppoſe I would try whether alum, tho' it be plainly a mix'd body, abounds rather with an acid than a ſulphureous ſalt, I turn my back to the light, and holding a ſmall vial full of the tincture of *Lignum Nephriticum* in my hand, which view'd in that poſition appears blue; then dropping into it a little of a ſtrong ſolution of alum made in fair water, and finding upon the affuſion and ſhaking of the whole, that the blueneſs formerly conſpicuous in our tincture preſently vaniſhes, I am thereby induced to ſuppoſe, that the ſalt predominant in alum is acid. But if, on the other hand, I would examine, whether ſalt of urine, or of hart's-horn, is rather of a ſalino-ſulphureous, than of an acid nature; I drop a little of the ſaline ſpirit of either, into the nephritic tincture; and finding that the blue colour is thereby rather deepened than deſtroyed, I collect, that the ſalts which conſtitute theſe ſpirits, are rather ſulphureous than acid. And, to ſatisfy myſelf yet farther in this particular, I take a ſmall vial of freſh tincture, and placing both it and myſelf, with regard to the light, as before; I drop into the infuſion juſt as much diſtilled vinegar, or other acid liquor, as will ſerve to deprive it of its blueneſs; then, without changing my poſture, I drop, and ſhake into the ſame vial, a ſmall proportion of ſpirit of hart's-horn, or urine: and finding, upon this, the tincture immediately to recover its blue colour, I am thereby confirmed in my opinion of the ſulphureous nature of theſe ſalts.

It is much doubted to what ſort of ſalt that which is predominant in quick-lime belongs; but we have been perſuaded to refer it rather to the lixiviate, than acid kind, by obſerving, that tho' an evaporated infuſion of it will ſcarce yield ſuch a ſalt as aſhes, and other alkaline bodies do; yet if we deprive our nephritic tincture of its blueneſs, by juſt ſo much diſtilled vinegar as is requiſite to make that colour vaniſh; the lixivium

of

PHYSICS. of quick-lime will, immediately, upon its affuſion, recall the baniſhed colour, tho' not ſo powerfully as either of the ſulphureous liquors, formerly mentioned. And I gueſs at the ſtrength of the liquors, thus examined, by the quantity of them; which is ſufficient to deſtroy, or reſtore the blue colour of our tincture. But whether as to liquors, wherein neither acid nor alkaline ſalts are eminently predominant, our tincture will enable us to conjecture any thing more, than that ſuch ſalts do not abound in them, I leave to further trial; for I find not, that ſpirit of wine, ſpirit of tartar, freed from acidity, or chymical oil of turpentine, have any remarkable power to deprive our tincture of its blue colour, or to reſtore it, when, upon the affuſion of vinegar, it has diſappeared.

Different colours obſerved in the ſame piece of glaſs.

11. We may alſo ſee in a mineral body, ſomething very near of kin to the changeable quality of the tincture of *Lignum Nephriticum*; for I have ſeveral flat pieces of glaſs of the thickneſs of ordinary panes for windows, one of which being interpoſed betwixt the eye, and a clear light, appears of a golden colour, not much unlike that of the moderate tincture of our wood: but being viewed, when the beams of light paſs not ſo much thro' it, as they are reflected from it to the eye, the yellow ſeems to degenerate into a pale blue, ſomewhat like that of a turquoiſe. And, what is ſtrange, if in a certain poſition, you hold one of theſe plates perpendicular to the horizon, ſo that the ſun may ſhine upon one half of it, whilſt the other half is ſhaded, you may ſee the part illumin'd, of a much fainter yellow than the ſhaded part, which will appear more richly coloured. And if the glaſs be not held perpendicular, but parallel to the horizon, you may ſee the ſhaded part of a golden colour, whilſt the other appears conſiderably blue: and as you remove any part of the glaſs, thus held horizontally, into the ſun-beams, or the ſhade, it will, in the twinkling of an eye, ſeem to paſs from one of thoſe colours to the other; and the rays paſſing thro' it, and received by a ſheet of white paper held near, colour it with a yellow, ſomewhat bordering upon a red: yet the glaſs may be ſo oppoſed to the ſun, as to throw a mixed colour upon paper, in ſome parts more inclined to yellow, and in others to a blue.

In making theſe experiments with glaſs, you muſt take notice, that as one of the ſides has its ſuperficial parts diſpoſed to reflect the blue colour, that ſide muſt be held next to the eye. I have myſelf made glaſſes proper to exhibit an experiment, not unlike the laſt mentioned, by laying ſome ſilver, very finely foliated, upon glaſs, and giving it, by degrees, a much ſtronger fire than is requiſite to tinge glaſs of other colours. And this experiment, tho made without a furnace, is the more conſiderable; becauſe a skilful painter, who allow'd 'twas with ſilver he coloured his glaſſes yellow, told me, that, when to burn them, he lays on the plates nothing but a calx of ſilver, calcined without corroſive liquors, and temper'd with fair water, the plates are tinged of a fine yellow, that looks of a golden colour, which part ſoever be turn'd to, or from the light; whilſt we have found, more than once, that ſome pieces of of glaſs, prepared after our manner, tho', when held againſt the light, they appeared of a tranſparent yellow; yet viewed with one's back turn'd to the light, exhibited an opake blue.

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The ſimple and primary colours but few.

12. There are but few ſimple, and primary colours; from the various compoſitions whereof, all the reſt reſult: for tho' painters imitate the hues of thoſe numerous different colours, to be met with in the works of nature, and of art; yet I have not found, that to exhibit this ſtrange variety, they need employ any more than white, black, red, blue, and yellow; theſe five, variouſly compounded, and re-compounded, being ſufficient to exhibit ſuch a variety, as thoſe, who are altogether ſtrangers to the painter's pallet, can hardly imagine. Thus black and white, differently mixed, make a vaſt number of lighter and darker greys; blue and yellow make a great variety of greens; red and yellow make orange-tawny; red, with a little white, makes a carnation; red, with an eye of blue, makes a purple: and, by theſe ſimple compoſitions again compounded, the skilful painter can produce what kind of colour he pleaſes; and a great many more than we have names for. But, to render the rules about the production of colours fit to be relied on, the corpuſcles, whereof the ſeveral pigments conſiſt, muſt be ſuch as do not deſtroy one another's texture; for, in caſe they do, the emerging colour may be very different from what would reſult from the mixture of other agreeing pigments of the ſame colours.

The ſun's light ſtain'd with the colours of tranſparent bodies, in paſſing thro' them.

13. It may alſo help to diſcover the nature of colours, to know, that the light of the ſun, paſſing thro' diaphanous bodies of different hues, may be tinged of the ſame compound colour, as if it proceeded from painters colours of the ſame denomination; tho' the latter be exhibited by reflection, and manifeſtly compounded of material pigments. Wherefore, to try the compoſition of colours, we provided ſeveral plates of tinged glaſs, which being laid, two at a time, upon one another; the object, view'd thro' them both, appeared of a compound colour: which agrees with what we deliver'd of looking againſt the light thro' paper of different colours. But we thought the experiment would be more ſatisfactory, if we procured the ſun-beams to be ſo tinged in their paſſage thro' plates of glaſs, as to exhibit the compound colour upon white paper. And tho', by reaſon of the thickneſs of the glaſſes, the effect was but faint, even when the ſun ſhone ſtrong; yet we eaſily remedy'd that, by collecting his rays, with a convex glaſs; which increaſed the light, at the point where they met, ſufficiently to make the compound colour very manifeſt upon the paper. By this means we obſerved, that the rays paſſing thro' blue and yellow, compoſed a green; that an intenſe and moderate red, did, with yellow, make different degrees of ſaffron, and orange-tawny; that green and blue made a colour partaking of both, like what ſome *Latin* writers call *Pavonaceus*; and that red and blue made a purple. To which we might add other colours that we produced by the combinations of glaſſes differently tinged, did I not want proper words to expreſs them in our language. And having expoſed four or five ſorts of coloured glaſs, and other tranſparent bodies to the ſun, and caſt the reflected light upon white paper, held near them; the light

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PHYSICS. appeared not manifeſtly tinged, but as if it had been reflected from the impervious parts of a colourleſs glaſs; only that reflected from the yellow, was here and there ſtained with the ſame colour; as if thoſe rays were not all reflected from the ſuperficial, but ſome from the internal parts of the glaſs. And a skilful tradeſman, who makes ſuch coloured glaſs, told me, that whereas the red pigment was but ſuperficial, the yellow penetrated to the very midſt of the plate. But, for further ſatisfaction, we foliated a plate of *Muſcovy* glaſs, and laying on it a little tranſparent varniſh of a gold-colour, we expoſed it to the ſun-beams, ſo as to caſt them upon a proper body; on which the reflected light appearing yellow, manifeſted, that, being reflected from the ſpecular part of the ſelenites, it was tinged, in its return, with the colour of the tranſparent varniſh, thro' which it paſſed.

Apparent colours compound as the genuine.

14. We now proceed to ſome experiments made in favour of thoſe colours, that are taught in the ſchools, not to be real, but only apparent, and fantaſtical. And, upon trial, we found, that theſe colours might be compounded both with true and ſtable colours; and, with one another, as well as thoſe which are unqueſtionably genuine and laſting; and that the colours reſulting from ſuch compoſitions, would reſpectively deſerve the ſame denominations. For, firſt, having, by means of a glaſs priſm, thrown an iris on the floor; I found, that, by placing a blue glaſs at a convenient diſtance, betwixt the priſm and the iris, that part of the iris, which before was yellow, might be made to appear green; tho' not of a graſs-green, but more diluted and yellowiſh. And it ſeems not improbable, that the narrow greeniſh liſt, uſually ſeen between the yellow and blue parts of the iris, is made by the confuſion of thoſe two bordering colours. And tho' the want of a ſufficient livelineſs in either of the compounding colours, or a ſmall error in the manner of making the following trials, was enough to render ſome of them unſucceſsful; yet when all neceſſary circumſtances were duly conſidered, the event was anſwerable to our expectation. As a red and blue compound a purple, ſo I could produce the latter of the three colours, by caſting, at ſome diſtance from the glaſs, the blue part of the priſmatic iris, upon a lively red. And ſometimes, when I try'd this upon a piece of red cloth, that part of the iris, which would have been blue, and, if compounded with the red of the cloth, appeared of a fair purple; did, when I came to view it near, look very oddly, as if there were ſome ſtrange reflection, or refraction, or both, made in the hairs of which that cloth was compoſed. Caſting, likewiſe, the priſmatic iris upon a very vivid blue, I found that part of it, which would otherwiſe have been the yellow, appear green. But it may ſeem more ſtrange, that tho' this iris, being made by the refraction of light thro' a body that has no colour, muſt, according to the doctrine of the ſchools, conſiſt of the pureſt emphatical colours; yet even theſe may be compounded of one another, as well as real colours in the groſſeſt pigments. For I took, at once, two triangular glaſſes, and one of them being kept fixed in the ſame poſture, that the iris it projected on the floor might not waver, I caſt on the ſame floor another iris with the other priſm, and moving it to and fro, to bring what part of the

the fecond iris I pleas'd, to fall upon what part of the firft I thought fit; we fometimes, by this means, obtain'd a green colour in that part of the more ftable iris that before was yellow or blue; and, frequently, by cafting thofe rays, that in one iris made the blue, upon the red parts of the other, we produced a lovely purple; which might be deftroyed, or re-compofed at pleafure, by feparating and re-joining the edges of the two. PHYSICS.

Experiments made with a coloured prifm.

15. Confidering the prifm as the moft ufeful inftrument hitherto employ'd about colours, and that thofe generally ufed, are made of glafs, tranfparent and colourlefs; I thought proper to try what change the fuperinduction of a colour, without deftroying the tranfparency, would produce in the phenomena exhibited by it. But, being unable to procure one of coloured glafs, and fearing alfo, that if it were not carefully made, the thicknefs of it would render it too opake; I endeavoured to fupply its place by one of clarify'd rofin, or of turpentine brought to the confiftence of a tranfparent gum. Our attempts this way were not wholly loft indeed, yet we found it fo difficult to give thefe materials their true fhape, that we chofe rather to varnifh over an ordinary prifm with fome tranfparent pigment; and this we did, firft with a yellow, and then with a red, or crimfon one, made of lac, temper'd with a convenient oil: the event was, that, for want of good tranfparent colours, of which there are but very few, both the yellow and the red rendered the glafs fo opake, tho' prudently laid on, that, unlefs I look'd upon fome very vivid object, I could fcarce difcern any colours at all, efpecially when the glafs was covered with red. And, upon viewing fuch objects, I found, that the colours of the pigment had vitiated, or drown'd, fome of thofe which the prifm would otherwife have exhibited; and, mixing with others, altered them. Thus, when covered with yellow, it made thofe parts of bright objects, where the blue fhould have been confpicuous, appear of a light green. But the nature of the colours, the degree of tranfparency, and darknefs in the pigment, with many other circumftances, fo vary'd the phenomena of thefe trials, that, till I can procure fmall coloured prifms, or hollow ones, that may be filled with tinged liquors, or obtain fome better pigments, than thofe I was reduced to employ; I fhall forbear to build any thing upon what I have done in this way: only defire the inquiry may be further profecuted.

Limpid liquors may afford coloured vapours.

16. There are fome liquors which, tho' colourlefs themfelves, yet, when they come to be elevated, and difperfed into exhalations, exhibit a confpicuous colour; which they lofe again, when re-united into a liquor. Thus, good fpirit of nitre, or ftrong *Aqua fortis*, tho' they have no appearance of rednefs, whilft in the form of a liquor; if a little heat chance to turn the minute parts of them into vapours, the fteam will appear of a reddifh, or deep yellow colour; which vanifhes, when thofe exhalations refume the form of a liquor. And if you look upon a glafs, half full of *Aqua fortis*, or fpirit of nitre, and the other half full of fteams, proceeding from either; you will fee the upper part of the glafs, of the colour juft mentioned, if turn'd to the light. But, what is much more confiderable, I have try'd,

PHYSICS. that putting *Aqua fortis* in a long clear glaſs, and adding thereto a little copper, or ſome ſuch open metal, to excite heat and fumes; the light paſſing thro' thoſe fumes being caſt upon a ſheet of white paper, appear thereon of the colour of the fumes, when directly view'd; as if the light were as well tinged in its paſſage thro' theſe fumes, as it would have been by paſſing thro' ſome glaſs, or liquor, in which the ſame colour was inherent. Having, alſo, ſometimes obſerved, whether the beams of the ſun, near the horizon, paſſing thro' a very red sky, would not exhibit the like colour; I found, that the rays falling in a room, upon a very white object, placed directly oppoſite to the ſun, diſcloſed a manifeſt redneſs, as if they had paſs'd thro' a coloured medium.

17. The reſulting of colours, upon the coalition of the particles of ſuch bodies, as were neither of them coloured like that mixture whereof they are the ingredients, is very well worth our niceſt obſervation; being of great ſervice, both in ſpeculation and practice. For the mechanical uſe of colours, among painters and dyers, very much depends upon knowing which of them are producible by mixing pigments of different hues. And 'tis of advantage to the naturaliſt to know how many, and which colours are primitive and ſimple; becauſe it both eaſes his labour, by confining his inquiry to a ſmall number of thoſe upon which the reſt depend; and aſſiſts him to judge of the nature of particular compound colours, by ſhewing, from the mixture of what more ſimple ones, and in what proportions to one another, the particular colour to be conſidered, reſults. But becauſe to inſiſt on the proportions, the manner, and the effects of ſuch mixtures, would oblige me to conſider a great part of the painter's art, and dyer's trade; I ſhall confine myſelf to mention ſeveral ways of producing green, by the compoſition of blue and yellow.

Several ways of producing a green, with blue and yellow.

Firſt, then, as painters make a green, by tempering blue and yellow, reduced to a ſoft conſiſtence, with water, oil, or ſome liquor of kin to theſe; I found, that by chuſing fit ingredients, and mixing them in the form of dry powders, I could do more this way, than if the ingredients were tempered with a liquor. But the blue and yellow powders, beſides being finely ground, muſt be ſuch, that the corpuſcles of the one may not be too unequal to thoſe of the other; leſt, by their diſproportionate minuteneſs, the ſmaller cover and hide the greater. We uſed, with good ſucceſs, a ſlight mixture of the fine powder of biſe, with that of orpiment, or good yellow oker, tho' an exquiſite mixture did not ſucceed ſo well; but, by lightly mixing the two in ſeveral little parcels, thoſe of them in which the proportion and manner of mixture was more luckily hit, afforded us a good green.

(2.) I have alſo learned, at the dye-houſe, that cloth, dy'd blue with woad, is afterwards, by the yellow decoction of *Luteola*,, turn'd to a green.

(3.) We formerly ſaid, that having in a darkned room taken two bodies, a blue and a yellow, and caſt the light reflected from the one upon the other; we thereby obtained a green.

(4.) We

(4.) We alſo formerly obſerv'd a green to be produced, when in the ſame darken'd room we look'd at the hole where the light enter'd, thro' the green and yellow parts of a ſheet of marbled paper laid one upon the other.

(5.) We found too, that the beams of the ſun paſſing thro' two pieces of glaſs, the one blue, and the other yellow, laid upon one another, did upon a ſheet of white paper, on which they were made to fall, exhibit a lovely green: and other experiments have been before deliver'd to the ſame purpoſe.

(6.) We have likewiſe contrived a way of trying whether metalline ſolutions, tho' one of them had its colour adventitious, might not, by the mixture of the menſtrua employ'd, be made to compound a green, after the manner of other bodies. To a high yellow ſolution of pure gold in *Aqua regia*, I put a due proportion of a fine deep blue ſolution of crude copper, made in ſpirit of urine; and theſe two liquors, tho' at firſt they ſeem'd to coagulate each other a little, yet being thoroughly mix'd by ſhaking, they preſently united into a tranſparent green liquor; and ſo continued for ſeveral days; only letting fall a little blackiſh powder to the bottom. This experiment we more than once repeated with ſucceſs.

(7.) And laſtly, to try whether this way of compounding colours would hold even in ingredients actually melted by the violence of the fire, provided their texture were capable of enduring fuſion; we cauſed ſome blue and yellow ammel to be long and well wrought together in the flame of a lamp, which being ſtrongly and inceſſantly blown, kept them in ſome degree of fuſion; whence at length we obtain'd an amel of a green colour.

The manner wherein this colour may poſſibly be produced.

I have ſometimes conjectured, that the mixture of the biſe and the orpiment, lately mentioned, produced a green, by altering the ſuperficial aſperity which each of thoſe ingredients had a-part; ſo that the light falling on the mixture, was reflected with different ſhades, as to quantity or order, or both, from thoſe of either of the ingredients; and ſuch wherewith the light is modify'd, when it reflects from graſs or leaves, *&c.* that we call green. And ſometimes too I have doubted whether the green thus produced, might not be partly derived from hence; that the rays reflected from the corpuſcles of the orpiment giving one kind of ſtroke upon the retina, whoſe perception we call yellow, and thoſe reflected from the corpuſcles of the biſe giving another ſtroke upon the ſame retina, like objects that are blue; the contiguity and minuteneſs of theſe corpuſcles may make the appulſe of the reflected light fall upon the retina within ſo narrow a compaſs, that the part they ſtrike being as it were a phyſical point, they might give a compounded ſtroke, and conſequently exhibit a new compound kind of ſenſation; as we ſee that two ſtrings of a muſical inſtrument being ſtruck together, give two ſounds, which arrive at the ear in the ſame time; and cauſe a differing ſenſation from either of them, and as it were one compounded of both. I ſhall only here farther obſerve, that the firſt of theſe ways of compounding a green, agrees much better with our conjectures about colours, than either with the doctrine of

PHYSICS. of the schools, or with that of the chymists. For first, in the mixture of the two powders I could, by the help of an excellent microscope, discover that what seem'd to the naked eye a green body, was but a heap of distinct, tho' very small grains of yellow orpiment and blue bise confusedly blended together; whence it appears, that the colour'd corpuscles of either kind did each retain its own nature and colour: so much may mere transposition and juxta-position of minute and singly unchanged particles of matter, contribute to produce a new colour. Secondly, the green thus made, being mechanically produced, there is no pretence to derive it from any unintelligible substantial form; nor does this green, tho' a real and permanent colour, seem to be a very inherent quality, as by that doctrine it ought to be; since each part of the mixture remains unalter'd in colour, and consequently of a different one from the heap they compose: for, if the eye be assisted by a microscope, it no longer sees a green body, but a heap of blue and yellow corpuscles. And thirdly, I demand what sulphur, salt, or mercury have to do in the production of this green? for neither the bise nor the orpiment were of that colour before; and the bare juxta-position of the corpuscles of the two powders that operate not upon each other, but might, if we had convenient instruments, be separated unalter'd, cannot with any probability be imagin'd either to increase or diminish any of the three hypostatical principles.

The mixture of every yellow and every blue will not afford a green.

18. But it is not every yellow and every blue that will by their mixture afford a green. For in case either of the ingredients has a power to alter the texture of the other, so as to indispose it to reflect the light, as bodies that exhibit a blue or a yellow, reflect it; the emerging colour may not be green, but such as the change of texture in the corpuscles of one or both of the ingredients qualifies them to give. Thus, for instance, if you let fall a few drops of syrup of violets upon a piece of white paper, tho' the syrup will appear blue thereon, yet by mixing with it two or three drops of the above-mention'd solution of gold, I obtain'd a reddish mixture from the power of the acid salts in the solution. And having made a very strong and high-colour'd solution of copper-filings with spirit of urine, tho' the menstruum seem'd fully saturated with the metal; yet having let three or four drops of syrup of violets fall upon white paper, I found that the deep blue solution proportionably mix'd with this other blue liquor, made not a blue, but, upon account of the urinous salt in the menstruum, a fair green.

The colours of the rain-bow exhibited in very thin substances.

19. To shew the chymists that colours may be made to appear or vanish where there happens no accession or change either of the sulphureous, the saline, or the mercurial principle of bodies, I shall not make use of the iris afforded by the glass prism, nor of the colours to be seen in a fair morning in those drops of dew, that in a convenient manner reflect and refract the rays of light to the eye; but remind them of what they may observe in their own laboratories. For all chymical essential oils, as also good spirit of wine, being shaken till they rise in bubbles, those bubbles appear with various fine colours, which all immediately vanish upon the relapsing

relapſing of the liquor that affords them their films, into the reſt of the oil or ſpirit of wine; ſo that a colourleſs liquor may immediately be made to exhibit a variety of colours, and loſe them in a moment, without the acceſſion or diminution of any of its hypoſtatical principles. And, by the way, 'tis worthy our notice, that ſome bodies, as well colourleſs as colour'd, by being brought to a great thinneſs of parts, acquire colours they had not before. For not to inſiſt on the variety of colours that water, render'd glutinous by ſoap, acquires, when blown into ſpherical bubbles; turpentine will, by being blown into after a certain manner, afford bubbles adorn'd with various colours; which tho' they ſoon vaniſh after the bubbles break, yet theſe would, probably, always variouſly exhibit colours upon their ſuperficies, if their texture were durable enough: for I have ſeen a perſon skill'd at faſhioning glaſſes, by the help of a lamp, blow ſome ſo vehemently, as to burſt them; whereupon we found the tenacity of the metal ſuch, that before it broke, it ſuffer'd itſelf to be reduced into films ſo extremely thin, that being kept clean, they conſtantly ſhew'd on their ſurfaces the varying colours of the rain-bow, which were exceedingly vivid. Taking, alſo, a feather of a convenient ſhape and bigneſs, and holding it at a due diſtance betwixt my eye and the ſun, when near the horizon, there appear'd to me a variety of little rain-bows, with different and very vivid colours, none of which were conſtantly to be ſeen in the feather. The like phenomenon I have at other times produced, by interpoſing, at a proper diſtance, a piece of black ribband betwixt the ſetting ſun and my eye.

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Syrup of violets, and the juice of blue-bottles, by a change of colour, diſtinguiſh an acid from an alkali.

20. Drop a little good ſyrup of violets upon white paper, and on this liquor let fall two or three drops of the ſpirit either of ſalt, vinegar, or other eminently acid liquor; and upon the mixture of theſe, you ſhall find the ſyrup immediately turn'd red. But if inſtead of ſpirit of ſalt, or that of vinegar, you drop upon the ſyrup a little oil of tartar *per deliquium*, or of the ſolution of pot-aſhes, and ſtir them together with your finger, the blue colour of the ſyrup will in a moment be turn'd into a perfect green. And this ſyrup may be ſubſtituted for the infuſion of *Lignum Nephriticum*, when we would examine whether the ſalt predominant in a liquor, or other body, wherein 'tis looſe and in plenty, belongs to the tribe of acid ſalts. For if ſuch a body turns the ſyrup of a red or reddiſh purple colour, it for the moſt part argues that body to abound with acid ſalt. But if the ſyrup be turn'd green thereby, the predominant ſalt ſeems to be of a nature contrary to acid. For as ſpirit of ſalt, oil of vitriol, *Aqua fortis*, ſpirit of vinegar, juice of lemmons, &c. will turn ſyrup of violets red or reddiſh; ſo I have found that not only all the volatile ſalts of animal ſubſtances which I have uſed, as ſpirit of hart's-horn, of urine, of ſal-armoniac, of blood, &c. but all the alkaline ſalts I have employ'd, as the ſolution of ſalt of tartar, of pot-aſhes, of common wood-aſhes, lime-water, &c. will immediately change that blue ſyrup into a perfect green. And by the ſame way, the changes that nature and time produce in the more ſaline parts of ſome bodies, may be diſcover'd;

and

PHYSICS. and ſuch bodies chymically prepared, as belong neither to the animal kingdom, nor to the tribe of alkalies, have their new nature ſucceſsfully examined. But to change the colour of this ſyrup, it requires that the body be more ſtrong of the acid or other ſalt predominant in it, than is neceſſary to work upon the tincture of *Lignum Nephriticum*: and tho' the actions of theſe contrary ſalts will deſtroy each other, yet neither of them will reſtore the ſyrup to its native blue, as in the nephritic tincture; but each of them changes it into the colour which itſelf affects.

21. By dropping on the freſh juice of blue-bottles, or the *Cyanus vulgaris minor*, a little ſpirit of ſalt, it immediately turn'd to a red. And if inſtead of the acid ſpirit, I mix'd with it a little ſtrong ſolution of an alkaline ſalt, it preſently diſcloſed a lovely green; the ſame changes being, by thoſe different ſorts of ſaline liquors, producible in this juice, as in ſyrup of violets. And finding this blue juice, when freſh, to be capable of ſerving for an ink of that colour; I attempted, by moiſtening one part of a piece of white paper with the ſpirit of ſalt, and another with ſome alkaline or volatile liquor, to draw a line on the paper after it was dry, that ſhould appear partly blue, partly red, and partly green: but the latter part of the experiment did not ſucceed well, tho' the blue and red were conſpicuous enough to ſurprize thoſe who were unacquainted with the trick. But leſt it ſhould be thought that volatile or alkaline ſalts change blue into green rather upon the ſcore of the eaſy tranſition of the former colour into the latter, than upon account of the texture, wherein moſt vegetables, that afford a blue, ſeem to be allied; I ſhall add, that having diſſolv'd blue vitriol in fair water, and put a lixiviate liquor and an urinous ſalt to diſtinct parcels of it, each of 'em turn'd the liquor not green, but of a deep yellowiſh colour, almoſt like that of yellow oker; which colour, the corpuſcles thereby precipitated, retain'd.

22. The hint of the following experiment was afforded us by the practice of ſome *Italian* painters, who counterfeit ultra-marine azure * by grinding

* The *Pruſſian* blue is allow'd to excel the ultra-marine. And the preparation of it being very curious and uſeful, we ſhall here give an extract of that receit for it which was lately communicated to the Royal Society by Dr. *Woodward*, as ſent him from *Germany*. Take of crude nitre and crude tartar, each four ounces; powder them fine, mix them together; and after decrepitation, there will remain four ounces of a ſalt of tartar: whilſt this is hot, pulverize it, and add thereto four ounces of well-dry'd ox-blood, in fine powder. Calcine the whole in a cover'd crucible, whereof it may fill two thirds. After this operation, lightly grind the matter in a mortar, and put it hot into four pints of boiling water; and let it continue boiling for half an hour. Strain the decoction, dilute the black remaining ſubſtance with water, and boil and ſtrain as before, till the water pour'd on becomes inſipid. Add the ſeveral ſtrainings together, and evaporate them to four pints. Then diſſolve an ounce of *Engliſh* vitriol, calcined white, in ſix ounces of rain-water, and filtre the ſolution. Diſſolve, likewiſe, half a pound of crude alum in two quarts of boiling water; and add this to the ſolution of the vitriol taken hot from the fire; pouring to them alſo the firſt lixivium, whilſt thoroughly hot, in a large veſſel. A great ebullition, and a green colour, will immediately enſue.

grinding verdigreaſe with ſal-armoniac and ſome other ſaline ingredients, and ſuffering them to lie for a good while together in a dunghill; for we ſuppoſed, that the change of colour wrought in the verdigreaſe by this way of preparation, muſt proceed from the action of certain volatile and alkaline ſalts, abounding in ſome of the mixed concretes, and brought to make a further diſſolution of the copper contained in the verdigreaſe; and therefore conjectured, that if both the verdigreaſe, and ſuch ſalts, were diſſolved in fair water, the ſmall parts of both being therein more ſubdivided, and ſet at liberty, would have better acceſs to each other, and thence incorporate much the ſooner. And, accordingly, we found, that if, upon a ſtrong ſolution of good *French* verdigreaſe, we poured a juſt quantity of oil of tartar, and ſhook them well together, a notable change of colour immediately ſucceeded; the mixture growing thick, and not tranſparent; but if you ſtay till the groſſer part be precipitated, and ſettled at the bottom, you may obtain a clear liquor of an exceeding delightful colour. You muſt drop in a competent quantity of oil of tartar, otherwiſe the colour will not be ſo deep and rich; but if, inſtead of this oil, you employ a clear lixivium of pot-aſhes, you may have an azure, ſomewhat lighter or paler than the former. And if, inſtead of either of theſe liquors, you make uſe of ſpirit of urine, or of hart's-horn, you will, according to the quantity and quality of the ſpirit poured in, obtain ſome further variety of blue liquors; and, by the help of this urinous ſpirit, we have made a ſurprizing blue liquor. But theſe azure-coloured liquors ſhould be freed from the ſubſiding matter, which the ſalts of tartar, or urine, precipitate out of them, rather by being decanted, than by filtration; for, in the latter method, we have ſometimes found the colour of them very much impaired.

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The production of a blue colour.

23. That roſes, held over the fume of ſulphur, may quickly be thereby deprived of their colour, and have as much of their leaves, as the fume works upon, turned pale, we have already hinted; and it is a known experiment. But it may ſeem ſtrange to one who has never conſidered the compound nature of brimſtone, that tho' the fume of it, as we

Of a red.

enſue. Whilſt this ebullition continues, pour the mixture out of one veſſel into another, and afterwards let it reſt. Then run the liquor thro' a linen, and let the pigment remain in the ſtrainer; and when 'tis thus freed from its moiſture, put it, by means of a wooden ſpatula, into a ſmall new pot; pour upon it two or three ounces of ſpirit of ſalt, and there will immediately ariſe a moſt beautiful blue. Let the matter be well ſtirr'd, and ſet to reſt for a night; and afterwards thoroughly edulcorate it with repeated affuſions of rain-water; allowing a competent time for the precipitate to ſubſide. Thus, at length, it will become exquiſitely blue. Laſtly, let it drain upon the linen ſtrainer, and dry it gently for uſe. The ſucceſs of this proceſs greatly depends upon the calcination. The crucible is firſt to be ſurrounded with coals, that it may grow gradually hot, and the matter leiſurely flame and glow. This degree of heat muſt be continued till the flame and glowing decreaſe: then the fire ſhould again be raiſed, that the matter may glow with an exceeding white heat; and but little flame appear above the crucible. The lixivia muſt be vehemently hot; and ought to be mix'd together in an inſtant. See *Philoſ. Tranſact.* N° 381. p.15——24.

PHYSICS. have ſaid, whitens the leaves of roſes, yet the oil of ſulphur *per campanam*, powerfully heightens the tincture of red roſes, and makes it more red and vivid; as we have eaſily try'd, by putting ſome red roſe-leaves, that having been long dry'd, had loſt much of their colour, into a vial of fair water: for a while after the affuſion of a convenient quantity of the oil, both the leaves themſelves, and the water they were ſteep'd in, diſcover'd a very freſh and lovely red colour.

What quantity of limpid liquor a pigment may tinge

24. It may ſerve to illuſtrate not only the doctrine of pigments, and of colours, but many other parts of mechanical philoſophy, as odors, and other qualities are explain'd by the aſſiſtance of bodies extremely minute, to examine to how much of a colourleſs liquor, a very ſmall parcel of a pigment may impart a diſcernible colour. And tho' ſcarce any thing of exactneſs can be expected from ſuch trials, yet I preſum'd I ſhou'd hence be able to ſhew a much further ſubdiviſion of the parts of matter into viſible particles, than ſeems hitherto taken notice of, or imagin'd.

The moſt promiſing bodies, for ſuch a purpoſe, might ſeem to be metals, eſpecially gold, becauſe of the multitude and minuteneſs of its parts, which might be argued from the great cloſeneſs of its texture. But tho' we try'd a ſolution of gold, made in *Aqua regia* firſt, and then in fair water, yet becauſe we were to determine the pigment we employ'd, not by bulk, but weight; and becauſe alſo the colour of gold is but weak, in compariſon of that of cochineal, we rather choſe this to make our experiments with. But, from a number of theſe, it may ſuffice to ſelect one which was carefully made in veſſels conveniently ſhap'd; to which I ſhall only premiſe, that the cochineal will be better diſſolv'd, and have its colour far more heighten'd by ſpirit of urine, than by common water, or even rectified ſpirit of wine. One grain of cochineal, diſſolved in a conſiderable quantity of ſpirit of urine, and then further diluted by degrees with fair water, imparted a diſcernible colour to ſix glaſſes of water, each containing forty-three ounces and a half; which amounts to above one hundred twenty-five thouſand times its weight.

Acid, alkaline, and urinous ſalts, change the colours of many vegetable productions

25. It may afford a conſiderable hint towards improving the art of dying, to know what change of colours are producible by the three ſeveral ſorts of ſalts we have mention'd, in the juices, decoctions, infuſions, and the more ſoluble parts of vegetables. The blue liquors lately made uſe of in our experiments, are far from being the only vegetable ſubſtances upon which acid, urinous, and alkaline ſalts, have the like operations to thoſe above recited. Ripe privet-berries, for inſtance, being cruſh'd upon white paper, tho' they ſtain it with a purpliſh colour, yet if we let fall on one part of it, two or three drops of ſpirit of ſalt, and on the other, ſome ſtrong ſolution of pot-aſhes, the former liquor immediately turns that part of the thick juice, or pulp, on which it fell, into a lovely red; and the latter changes the part whereon it falls, into a delightful green. This experiment is, perhaps, very extenſive, and ſerviceable to thoſe who wou'd know how dying ſtuffs may be wrought upon by ſaline liquors. For I have found it to ſucceed in ſo many various berries, flowers, bloſſoms, and other finer parts

parts of vegetables, that my memory will not ſerve me to enumerate them; and it is ſurprizing to ſee, by what differently colour'd flowers or bloſſoms; for example, paper being ſtain'd, may, by an acid ſpirit, be immediately turn'd red, and by any alkali, or urinous ſpirit, green; ſo that even the bruis'd bloſſoms of *Mezereon*, gather'd in froſty weather, and thoſe of peaſe cruſh'd upon white paper, how remote ſoever their colours are from green, wou'd in a moment paſs into a deep degree of that colour, upon the touch of an alkaline liquor. And either of theſe new pigments may, by a ſufficient affuſion of a contrary liquor, be preſently chang'd from red to green, and from green to red; which obſervation holds alſo in ſyrup of violets, the juice of blue-bottles, *&c.*

26. There are, however, ſome caſes wherein theſe experiments will not uniformly ſucceed. And firſt, I try'd the operation of acid ſalts upon ſuch vegetable ſubſtances as are in their own nature red; as ſyrup of clove-july-flowers, the clear expreſs'd juice of buckthorn-berries, red-roſes, infuſion of brazil, and many others; on ſome of which, ſpirit of ſalt either made no conſiderable change, or only alter'd the colour from a darker to a lighter red. And as to the operation of the other ſorts of ſalts, upon theſe red ſubſtances, I found it not very uniform; ſome red or reddiſh infuſions, as of roſes, being thereby turn'd into a dirty colour, inclining to green. Nor was the ſyrup of clove-july-flowers turn'd by the ſolution of pot-aſhes, to a much better, tho' a ſomewhat greener colour. Another ſort of red infuſion was by an alkali not turn'd into a green, but advanc'd to a crimſon; tho' there were other kinds of them, particularly the juice of buckthorn-berries, that readily paſs'd into a lovely green.

27. Among other vegetables, which ſeem'd likely to afford exceptions to the general obſervation about the different changes of colours, produced by acid and ſulphureous ſalts, we made trial upon the flowers of jeſſemin; they being both white, as to colour, and eſteem'd of a more oily nature than other flowers. Taking, therefore, only the white parts of the flowers, and rubbing them ſomewhat hard with my finger, upon a piece of clean paper, it appear'd very little diſcolour'd thereby; nor had ſpirit of ſalt, wherewith I moiſten'd one part of it, any conſiderable operation thereon: tho' ſpirit of urine, and, particularly, a ſtrong alkaline ſolution, immediately turn'd the paper, tho' it had remain'd almoſt colourleſs, of a deep greeniſh yellow; which experiment I ſeveral times repeated with the like ſucceſs. But a great degree of unctuouſneſs ſeems unneceſſary to the production of theſe effects; for when we try'd the experiment with the leaves of thoſe pure white flowers that appear about the end of winter, and are commonly call'd ſnow-drops, the event was much the ſame with the laſt mention'd.

28. Another ſort of inſtances, to ſhew how much the changes of colours effected by ſalts, depend upon the particular texture of the colour'd bodies, we have from ſeveral yellow flowers, and other vegetables, as marygold-leaves, primroſes, freſh madder, *&c.* For theſe being rubb'd upon white paper, till they imbued it with their colour, I cou'd not find,

PHYSICS. by the addition of alkaline liquors, or of an urinous ſpirit, that they would turn either green or red; even the ſpirit of ſalt would not conſiderably alter their colour, only dilute it a little: tho', in ſome early primroſes, it deſtroy'd the greateſt part of the colour, and made the paper almoſt white again. Madder, alſo, afforded ſomething peculiar; for having gather'd ſome roots of it, whilſt its yellow juice was freſh expreſs'd upon white paper, an alkaline ſolution being dropp'd upon it, turn'd it neither green nor white, but red; and the bruis'd madder it ſelf, drench'd with the like alkaline ſolution, exchang'd its yellowneſs for a redneſs.

29. It may be of uſe, towards diſcovering the nature of the changes which the alimental juices undergo in different vegetables, in their different degrees of maturity, to obſerve what operation acid, urinous, and alkaline ſalts will have upon thoſe juices. To ſhew my meaning by an example, I took from the ſame cluſter one black-berry full ripe, and another that had not yet out-grown its redneſs; and rubbing a piece of white paper with the former, I obſerved, that the juice adhering to it, was of a dark reddiſh colour, full of little black ſpecks; and that this juice, by a drop of a ſtrong lixivium, would immediately turn into a deep greeniſh colour; by as much urinous ſpirit, into a colour near allied to the former, tho' fainter; and by a drop of ſpirit of ſalt, into a fine light red: but the red berry, rubbed in like manner upon paper, left on it a red colour, which was very little altered by the ſame acid ſpirit; and from the urinous and lixiviate ſalts, received changes of colour, different from thoſe produced in the dark juice of the ripe black-berry.

I remember alſo, that tho' the infuſion of damask roſes would be heighten'd by acid ſpirits, to an intenſe degree of redneſs, and, by lixiviate ſalts, be brought to a darkiſh green; yet putting a roſe, whoſe leaves were perfectly yellow, in a ſolution of ſalt of tartar, it afforded a green bluiſh tincture; but, by means of an acid liquor, I could not obtain a red one; the ſaline ſpirit I employ'd, only a little diluting the yellowneſs of the leaves. And if I were in the iſlands of *Banda*, where cloves ſo greatly proſper, I ſhould try what operation our three differing kinds of ſalts would have upon the juice of this ſpice; which eminent authors inform us is at firſt white, afterwards green, and then reddiſh, before 'tis beaten off the tree; after which, being dry'd, it grows blackiſh, as we ſee it. And one of the lateſt botanic writers informs us, that the flower grows upon the top of the clove itſelf, conſiſting of ſmall leaves, like a cherry bloſſom, but of an excellent blue.

'Tis very proper to take notice of the particular ſeaſons wherein the vegetables, deſigned for the nicer experiments, are gathered. That diligent botaniſt, Mr. *Parkinſon*, tells us, that " of buckthorn-berries are made three " ſeveral ſorts of colours; being gathered green, and kept dry, they are " call'd ſap-berries; which being ſteep'd in alum-water, give a fair yellow " colour, uſed by painters, book-binders, and leather-dreſſers, who alſo " make a green colour, called ſap-green, taken from the berries, when " they are black; that being bruiſed, and put into a braſs kettle, and

" there

" there ſuffered to remain for three or four days, with ſome beaten alum " put to them, they are afterwards preſſed, and the liquor uſually put into " bladders, and hung up till it be dry: this, he ſays, is afterwards diſſolved " in water, or wine, but canary is the beſt to preſerve the colour from " ſtarving. The third is a purpliſh colour, made of the berries, ſuffered " to grow upon the buſhes, till the middle, or end of *November*, when " they are ready to fall of themſelves." And I try'd, with ſucceſs, to make ſuch a kind of pigment as painters call ſap-green, by a way not unlike that here delivered by our author. Much after the ſame manner, alſo, they make ſap-green in the colour-ſhops.

30. Many bodies, digeſted in well-cloſed veſſels, change their colour in tract of time, as rectify'd ſpirit of hart's-horn; and the ſame is evident in the precipitations of amalgams of gold and mercury, without addition; where, by the continuance of a due heat, the ſilver-coloured amalgam is reduced into a ſhining red powder. And many other inſtances of the like kind, might be produced. Now, in theſe operations, there appears no reaſon why we ſhould attribute the new colours to the action of a new ſubſtantial form; nor to any increaſe, or decreaſe of the ſalt, ſulphur, or mercury of the matter that acquires them. For the veſſels are cloſed, and theſe principles, according to the chymiſts, are ingenerable, and incorruptible. So that the effect ſeems to proceed from the heat, agitating and diſcompoſing the corpuſcles of the body expoſed to it; which, in proceſs of time, ſo changes its texture, that the tranſpoſed parts modify the incident light, otherwiſe than when the matter appeared of another colour.

Changes of colour by digeſtion, &c. particularly a redneſs.

31. Among the ſeveral changes of colour which bodies acquire, or diſcloſe, by digeſtion, it is very remarkable, that chymiſts find a redneſs, rather than any other colour, in moſt of the tinctures they draw; and even in the more groſs ſolutions they make, of almoſt all concretes that abound either with mineral or vegetable ſulphur; tho' the menſtruum employ'd about theſe ſolutions, or tinctures, be never ſo limpid. This we have obſerved in abundance of tinctures, drawn with ſpirit of wine from jalap, guaiacum, and many other vegetables; and not only in the ſolutions of amber, benjamin, *&c.* made with the ſame menſtruum; but alſo in ſeveral mineral tinctures. And, not to urge that familiar inſtance of the ruby of ſulphur, as chymiſts call the ſolution of flowers of brimſtone, made with the ſpirit of turpentine; nor to take notice of other more known examples of the aptneſs of chymical oils to produce a red colour with the ſulphur they extract or diſſolve; 'tis remarkable, that both acid and alkaline ſalts, tho', in moſt other caſes, of contrary operations, will, with many bodies that abound in ſulphureous, or oily parts, produce a red; as is manifeſt in the vulgar inſtances of the tinctures, or ſolutions of ſulphur, made with lixiviums, either of calcined tartar, or pot-aſhes, and other obvious examples; and in that the true glaſs of antimony, extracted with ſome acid ſpirits, yields a red tincture. And a certain acid liquor will, in a moment, turn oil of turpentine into a deep red.

Physics. red*. Among the many inſtances I could produce of the eaſy production of redneſs, by the operation of a ſaline ſpirit, as well as of ſpirit of wine, two or three of them deſerve to be particularly mentioned.

32. But, before I ſet them down, 'tis proper to premiſe, that there ſeems to be a manifeſt diſparity betwixt red liquors; for ſome of them may be ſaid to have a genuine redneſs, in compariſon of others that have it yellowiſh. Thus if a good tincture of cochineal be diluted never ſo much with fair water, it will not become a yellow liquor. But balſam of ſulphur, tho' in a large quantity, it appear to be of a deep red; yet if you ſhake the containing glaſs, or pour a few drops on a ſheet of white paper, ſpreading them on it with your finger, what falls back along the ſides of the glaſs, or ſtains the paper, will appear yellow. And there are many tinctures, ſuch as that of amber, made with ſpirit of wine, which will appear either yellow or red, according as the veſſels that contain them are ſlender or broad.

33. To come now to the experiments we deſigned. Firſt, oil or ſpirit of turpentine, tho' clear as fair water, being digeſted upon the pure white ſugar of lead, has, in a ſhort time, afforded us a high red tincture; probably a good medicine.

34. Secondly, take common brimſtone, and ſal-armoniac, of each, finely powdered, five ounces; of beaten quick-lime, ſix ounces: mix theſe powders exquiſitely, and diſtil them in a retort, placed in ſand, by degrees of fire; giving, at length, as intenſe an heat as you can in ſand; and there will come over a volatile tincture of ſulphur, which alſo may prove an excellent medicine. Now, tho' none of the ingredients here be red, the diſtilled liquor is of that colour; and, if it be well drawn, will, upon a little agitation of the vial, firſt unſtopped, ſend out a great white fume, which ſometimes ſpreads wide, and is very offenſive: and tho' the liquor itſelf be red, and its fumes white; yet it will dye the fingers black.

35. The laſt experiment, I ſhall now produce, to ſhew how apt bodies, abounding in ſulphureous parts, are to afford a red colour, is one wherein the operation of a clear ſaline ſpirit upon a white, or whitiſh body, may produce a redneſs in the twinkling of an eye. We took then, a little eſſential oil of aniſeeds congealed, and ſpread it with a knife upon a piece of white paper; when mixing with it a drop or two of well rectified oil of vitriol, there immediately emerged, with ſome heat and ſmoke, a blood-red colour.

* Mr. *Coles* found, that a certain ſulphureous ſpirit, mixed with a volatile alkali, gives a red colour in a moment. This ſpirit he made, by diſtilling two or three pound of benjamin with a little ſand, in a retort, *ad ſiccitatem*; and putting the oil, ſpirit, and flowers together in a filtre of paper, when the ſpirit came firſt thorough. Put two parts of this ſpirit to one of ſpirit of ſal-armoniac, and ſhake the containing glaſs, and the liquor will inſtantly turn red; tho' they both were clear before. The more the veſſel is ſhook, the deeper will be the red. And this effect is produced without any effervеſcence; whence the inventor conceives the experiment may be of uſe in accounting for ſanguification. *Philoſ. Tranſ.* N° 228. p. 542.

36. Let

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The degeneration of colours.

36. Let it be here obferved, once for all, that, in many of thefe experiments the colour produced is often very fubject to degenerate. However, fince the changes we have fet down happen prefently upon the operation of the bodies on each other, or at the times fpecified; that is fufficient to fhew what we intend. For it is not effential to the genuinenefs of a colour to be durable; a fading leaf, that is ready to rot and moulder into duft, may have as true a yellow as a wedge of gold. And I have feveral times obferved, that the mixture made by the oils of vitriol, and of anifeeds, tho' it acquire a thicker confiftence than either of the ingredients, quickly lofes its colour, and turns to a dark grey; at leaft in the fuperficial parts, where 'tis expofed to the air. This degeneration of colours may, in many cafes, indeed, proceed from the further action of the faline corpufcles, and other ingredients upon one another; yet much of the fudden change may often be afcribed to the air. Thus we have fometimes obferved window-curtains of a light colour, to have that part of them which was expofed to the air, when the window ftood open, of one hue; and the lower part, that was fcreen'd from the air by the wall, of another colour. And *Parkinfon* fays, of the plant turnfol, that "its berries, when "full ripe, have within them, between the outward skin and the inward "kernel, a certain juice, which rubbed upon paper or cloth, at the "firft appears of a frefh lovely green, but prefently changes into a kind "of bluifh purple. He adds, that the fame cloth, afterwards wet in "water, and wrung out, will turn the water into a claret-colour. And "thefe rags of cloth, fays he, are thofe ufually call'd turnfol in the drug-"gifts fhops." I alfo remember, that letting fome of the deep red juice of buckthorn-berries drop upon a piece of white paper, and leaving it there for many hours, till the paper was grown dry again; I found the juice degenerated to a dirty kind of greyifh colour; which, in a great part of the ftain'd paper, feem'd not to have fo much as an eye of red: tho' a little fpirit of falt, or diffolved alkali, would turn this unpleafant colour into a red or green. And, to fatisfy myfelf that this degeneration of colour did not proceed from the paper, I dropp'd fome of the fame juice upon a white glazed tile; and, permitting it to dry thereon, I found it then alfo loft its colour.

Having diffolved good filver in *Aqua fortis,* and precipitated it with fpirit of falt, upon firft decanting the liquor, the remaining matter was purely white; but, after it had lain a while uncovered, the part of it that lay contiguous to the air, not only loft its whitenefs, but appeared of a very dark, and almoft blackifh colour; but if the part that was contiguous to the air, were gently taken off, the fubjacent part of the fame mafs would appear very white; till that alfo, having continued a while expofed to the air, would likewife degenerate. Whether the air produce this effect by the means of a fubtile falt, by a penetrating moifture, by folliciting the avolation of certain parts of the bodies to which 'tis contiguous, or by fome other way, I leave to be further confidered. 'Twere eafy here to add many other inftances of rednefs, refulting from the digeftion of bodies.

PHYSICS. I have often ſeen, upon the borders of *France*, a ſort of pear, which, digeſted with a little wine in a veſſel exactly cloſed, will ſoon appear throughout of a deep red colour. Nay, even pure white ſalt of tartar, and limpid ſpirit of wine, will, by long digeſtion, acquire a redneſs. Some other obvious changes of colours frequently happen; ſuch as is the blackneſs of bodies burned in the open air, *&c.* But I ſhall not at preſent examine into the cauſes of theſe changes; tho' certainly the reaſon why the ſoots of different bodies are almoſt all of them black; why ſo much the greater part of vegetables ſhould be rather green than of any other colour; and, particularly, why gentle heat, ſo frequently, in chymical operations, produces rather a redneſs, than another colour in digeſted menſtrua, may very well deſerve a ſerious inquiry.

Different effects of an acid, in the production of colours, reconciled.

37. It may ſeem ſtrange, that if the crimſon ſolution of cochineal, the juice of black-cherries, or of ſome other vegetables that afford the like colour, be let fall upon a piece of paper; a drop or two of an acid ſpirit, ſuch as ſpirit of ſalt, or *Aqua fortis*, will immediately turn it into a fair red; whilſt an infuſion of brazil, in fair water, will have its redneſs deſtroy'd by a little ſpirit of ſalt, or *Aqua fortis*; and be turn'd either yellow or pale. But if we conſider the caſe attentively, the action of the acid ſpirit ſeems, in both theſe experiments, only to weaken the colour of the liquor wherein it falls; and ſo tho' it deſtroys redneſs in the tincture of brazil, but produce it in the tincture of cochineal, its operations may be ſtill uniform: ſince as crimſon is little elſe than a very deep red, with perhaps an eye of blue; ſo ſome kind of reds ſeem to be little elſe than heightned yellow; and conſequently in ſuch bodies the yellow ſeems to be but a diluted red. And accordingly, alkaline ſolutions, and urinous ſpirits, which ſeem diſpos'd to deepen the colours of moſt vegetable liquors, will not only reſtore the ſolution of cochineal, and the infuſion of brazil to the crimſon, from which the ſpirit of ſalt had changed them into a truer red; but will alſo heighten the yellow juice of madder into red, and advance the red infuſion of brazil to a crimſon. But perhaps it will be much ſafer to derive theſe changes from the vary'd texture, than from the peculiar kinds of bodies.

The colours of the fumes of bodies, and of the ſubſtances they form, obſerved in diſtillations, &c.

38. It might greatly contribute to the hiſtory of colours, if chymiſts would give us a faithful account of thoſe to be obſerved in the ſteams of bodies, ſublimed, or diſtilled, and of the productions made by the coalition of thoſe ſteams. Thus, for inſtance, we find in diſtilling pure ſalt-peter, that at a certain juncture of the operation, the body, tho' it ſeem either cryſtalline, or white, affords very red fumes; whilſt tho' vitriol be either green or blue, its ſpirit comes over in whitiſh vapours. The like colour I have found in the fumes of ſeveral other concretes, of different colours and natures; eſpecially when diſtill'd with ſtrong fires. And even ſoot, as black as it is, has fill'd our receivers with white fumes. No leſs obſervable may the diſtill'd liquors be, into which ſuch fumes convene: for tho', by skill and care, a reddiſh liquor is attainable from nitre, yet the common ſpirit of it, in the preparation whereof abundance of theſe red fumes paſs

over

over into the receiver, has no appearance of red. Neither the ſpirit of vitriol, nor of ſoot, is white; and the empyreumatical oils of woods, and other concretes, are either of a deep red, or of a colour between red and black. But 'tis very remarkable, that notwithſtanding the great variety of colours, to be met with in herbs, flowers, and other bodies uſually diſtill'd *in Balneo*; yet all the waters and ſpirits that firſt come over by that way of diſtillation, leave the colours of the concretes behind them; tho', indeed, there are one or two vegetables, not commonly taken notice of, whoſe diſtill'd liquors carry over the tincture of the concrete with them. And as in diſtillation, ſo in ſublimation, it were worth while to take notice of what happens to our purpoſe, by performing that operation in conveniently ſhap'd glaſſes, wherein the colour of the aſcending fumes may be diſcern'd; for it might afford a naturaliſt good information, to obſerve the agreements, or differences betwixt the colours of the aſcending fumes, and thoſe of the flowers they compoſe by their convention. It is evident, that theſe flowers do, many of them, greatly differ in point of colour, not only from one another, but often from the concretes that afford them. Thus, tho' camphire and brimſtone afford flowers much of their reſpective colours, except that thoſe of brimſtone are a little paler than the lumps that yielded them; yet the flowers of red benjamin are either white, or whitiſh. And, to omit other inſtances, even that black mineral, antimony, may be made to afford flowers, ſome of them red, others grey, and which is more ſtrange, ſome of them purely white. And glaſsmen, by exquiſitely mixing a convenient proportion of brimſtone, ſal-armoniac, and quick-ſilver, and ſubliming them together, make a ſublimate of an excellent blue. And tho', upon making this experiment, we found the ſublimate to be far from a lovely colour, yet, in ſome parts, it ſeem'd bluiſh, and was, at leaſt, of a colour very different from either of the ingredients; which is ſufficient for our preſent purpoſe. But a much finer colour is promis'd by ſome empirics, who tell us, that orpiment ſublim'd, will afford, among the parts of it that fly upwards, ſome little maſſes, which, tho' the mineral it ſelf be of a good yellow, will be red enough to emulate rubies, both in colour and tranſparency. This experiment may, for ought I know, ſometimes ſucceed; for I remember, that having in a ſmall bolt-head, purpoſely ſublim'd ſome powder'd orpiment, we could, in the lower part of the ſublimate, diſcern, here and there, reddiſh lines; tho' much of the upper part of it conſiſted of a matter not only purely yellow, but tranſparent. And we have alſo, by this means, obtain'd a ſublimate, which, tho' it conſiſted not of rubies, yet ſmall pieces of its lower part, that were numerous, afforded a pleaſant reddiſh ſparkling colour.

PHYSICS.

39. Take the dry'd buds, or bloſſoms, of the pomegranate-tree, commonly called balauſtins, pull off the reddiſh leaves, and, by a gentle ebullition, or a competent infuſion of them in fair water, extract a faint reddiſh tincture, which, if the liquor be turbid, you may clarify by the filtre; into this, if you pour a little ſpirit of urine, or ſome other

Various changes of colour, cauſed by ſaline ſpirits in the tinctures of vegetables.

PHYSICS. ſpirit, abounding in the like ſort of volatile ſalt, the mixture will preſently turn of a dark greeniſh colour: but if, inſtead of ſuch a ſpirit, you drop into the ſimple infuſion, a little rectify'd ſpirit of ſea-ſalt, the pale liquor will immediately grow more tranſparent, and acquire a high red, like that of rich claret; and this may as ſuddenly be deſtroy'd, and turn'd into a dirty bluiſh green, by the affuſion of a moderate quantity of the ſpirit of urine. This experiment gives light to two others that I met with in *Gaſſendus*. The experiments as we made them, were theſe. We took a glaſs of luke-warm water, and therein immerg'd a quantity of the leaves of ſena, upon which there appear'd no redneſs in the water; but dropping into it a little oil of tartar, the liquor ſoon became reddiſh; tho' by a little oil of vitriol, ſuch a colour could not be extracted from the infuſed ſena. On the other hand, we took ſome dryed red roſe-leaves, and ſhaking them in a glaſs of fair water, they communicated no redneſs to it; but upon the affuſion of a little oil of vitriol, the water was immediately turn'd red; which it would not have been, if inſtead of oil of vitriol, we had employ'd oil of tartar. Our author tells us, there was no redneſs either in the water, the leaves of ſena, or the oil of tartar; yet we have found that by ſteeping ſena for a night in cold water, it would afford a very deep yellow, or reddiſh tincture, without the help of oil of tartar; which ſeems to do little more than aſſiſt the water more ſuddenly to extract a plenty of that red tincture wherewith the leaves of ſena abound: for having made a tincture of ſena, only with fair water, and decanted it from the leaves before it grew reddiſh, we could not perceive, by dropping ſome oil of tartar into it, that the colour was conſiderable, tho' it were thereby a little heighten'd into a redneſs. And the ſame thing may be alledg'd in the experiment with red roſe-leaves; for we found that ſuch leaves, by bare infuſion; for a night and a day, in fair water, afforded a tincture bordering upon redneſs; and that colour being conſpicuous in the leaves themſelves, ſeem'd not wholly produc'd, but extracted by the oil of vitriol. But to improve the experiment, take the tincture of red roſe-leaves, made with a little oil of vitriol, and a large quantity of fair water, pour off this liquor into a clear vial, half fill'd with limpid water, till the water held againſt the light hath acquired a competent redneſs, without loſing its tranſparency: into this tincture, drop, leiſurely a little ſpirit of urine; and ſhaking the vial, which you muſt ſtill hold againſt the light, you will ſee the red liquor immediately turn'd into a fine greeniſh blue; a colour not to be found in any of the bodies upon whoſe mixture it enſued. And this change is the more remarkable, becauſe tho' the degeneration of blue into red, be uſual, the turning of red into blue, is very unfrequent. If, upon the falling of each drop of ſpirit of urine, you ſhake the vial containing the red tincture, you may obſerve a pretty variety of colours in the paſſage of that tincture from a red to a blue. And ſometimes we have thus obtain'd ſuch a liquor, as being view'd againſt, and from the light, ſeem'd faintly to emulate the tincture of *Lignum Nephriticum*. And if you make the tincture of red-roſes very high, and, without diluting it with fair water, pour on the

the ſpirit of urine, you may have a blue ſo deep as to render the liquor opake; but if it be dropp'd upon white paper, the colour will ſoon diſcloſe itſelf. Having, alſo, made the red, and conſequently the blue tincture very tranſparent, and ſuffer'd it to reſt in a ſmall open vial for a day or two; we found that not only the blue, but the red colour, likewiſe, was vaniſh'd; the clear liquor remaining of a bright amber colour, at the bottom whereof ſubſided a large, light feculency, almoſt of the ſame colour; which ſeems to be nothing but the ting'd parts of the roſe-leaves, drawn out by the acid ſpirits of the oil of vitriol, and precipitated by the volatile ſalt of the ſpirit of urine. This makes it the more probable, that the redneſs drawn by the oil of vitriol, was as well an extraction of the tinging parts of the roſes, as a production of redneſs. And laſtly, the colour of the tincture of roſes, may be chang'd by many other ſulphureous ſalts; as a ſtrong ſolution of pot-aſhes, oil of tartar, *&c.* tho' theſe are ſeldom ſo free from feculency, as the ſpirit of urine becomes by diſtillation. And I have a way of producing a full purple, by employing a liquor not made red artificially, inſtead of the tincture of red-roſes; for having by infuſing the powder of log-wood for a while in fair water, made that liquor red, I dropp'd into it a little urinous ſpirit, as that of ſal-armoniac, by which the colour was inſtantly turn'd into a rich and lovely purple. But care muſt be taken not to let fall into a ſpoonful of the tincture, above two or three drops of the ſpirit, leſt the colour become ſo deep as to make the liquor opake. And if inſtead of fair water, I infus'd the log-wood in water, made a little ſour by the acid ſpirit of ſalt; I obtain'd neither a purple, nor a red, but only a yellow liquor.

40. The following experiment is very ſurprizing; and, of all I have yet met with, ſeems the fitteſt to enforce our doctrine of colours, and to ſhew them not to be inherent qualities, flowing from the ſubſtantial forms of the bodies whereto they are ſaid to belong; ſince by a bare mechanical change of texture, in the minute parts of bodies, colours may, in a moment, be generated quite *de novo*, and utterly deſtroy'd: for there is this difference betwixt the enſuing experiment, and moſt of the others we deliver, that in this, the colour of a body is not chang'd into another, but betwixt two bodies, each of them, a-part, colourleſs, there is, in a moment, generated a very deep colour, which, if it were let alone, would be permanent; and yet, by a very ſmall parcel of a third body, that has no colour of its own, this otherwiſe permanent colour, will inſtantly be ſo utterly deſtroy'd, as to leave not the leaſt ſign of any colour in the whole mixture. This experiment is very eaſy, and thus perform'd. Take good common ſublimate, and fully ſatiate with it what quantity of water you pleaſe, carefully filtre the ſolution thro' clean cloſe paper, that it may drop down clear and colourleſs as fountain water. Then, when you would ſhow the experiment, put about a ſpoonful of it into a ſmall wine-glaſs, or other convenient veſſel, and letting fall therein three or four drops of good oil of tartar *per deliquium*, well filtred, that it may likewiſe be without colour, theſe two limpid liquors will, in the twinkling of an eye, turn into an opake

A colour inſtantly generated, and perfectly deſtroy'd.

PHYSICS. mixture of a deep orange-colour; which, by continually ſhaking the glaſs in your hand, you muſt prevent from ſettling too ſoon at the bottom. And when the ſpectators have view'd this firſt change, preſently drop in four or five drops of oil of vitriol, and continuing to ſhake the glaſs pretty ſtrongly, that it may the ſooner diffuſe itſelf, the whole colour will now immediately diſappear, and all the liquor in the glaſs become clear and colourleſs, as before, without the leaſt ſediment at the bottom. But for the more neat performance of this experiment, obſerve, Firſt, not to take too much of the ſolution of ſublimate, nor drop in too much of the oil of tartar, to avoid the neceſſity of putting in ſo much oil of vitriol, as to cauſe an ebullition.

Secondly, 'tis convenient to keep the glaſs conſtantly in motion, both for the better mixing of the liquors, and hindering, as we ſaid, the yellow ſubſtance from ſubſiding, which it would otherwiſe ſoon do; tho' when 'tis ſubſided, it will retain its colour, and be deprived of it by the oil of vitriol.

Thirdly, if any yellow matter ſtick to the ſides of the glaſs, 'tis but inclining the veſſel, till the clarify'd liquor can waſh along it, and the liquor will preſently imbibe it, and deprive it of its colour.

The chymical reaſon of this phenomenon.

My notion of the different natures of the ſeveral tribes of ſalts, having led me to deviſe this experiment; I can eaſily aſſign the chymical reaſon of the phenomenon. Having then obſerved, that mercury, diſſolved in ſome menſtrua, would yield a dark yellow precipitate; and ſuppoſing that common water, and the ſalts that ſtick to the mercury, would, in this reſpect, be equivalent to thoſe acid menſtrua which work upon the quick-ſilver, on account of their ſaline particles; I ſubſtituted a ſolution of ſublimate in fair water, inſtead of a ſolution of mercury in *Aqua fortis*, or ſpirit of nitre: then, conſidering that what makes the yellow colour, is indeed but a precipitate, obtain'd by means of the oil of tartar, which generally precipitates metalline bodies corroded by acid ſalts; ſo that the colour in our caſe reſults from the coalition of the mercurial particles with the ſaline ones, wherewith they were formerly aſſociated; and with the alkaline particles of the ſalt of tartar, that float in the oil. Conſidering alſo, that very many effects of lixivious liquors upon the ſolutions of other bodies may be deſtroyed by acid menſtrua, I concluded, that if I choſe a very ſtrong acid liquor, which by its inciſive power might undo the work of the oil of tartar, and diſperſe again thoſe particles which the other had, by precipitation, aſſociated into ſuch minute corpuſcles as were, before, ſingly inviſible, they would diſappear again, and conſequently leave the liquor as colourleſs as before the precipitation was made. This ſeems to be the chymical reaſon of the experiment, and is that which induced me to make it. But to give a perfect mechanical ſolution of the whole phenomenon, is more than I pretend to. However, the yellowneſs of the mercurial ſolution, and oil of tartar, ſeems produced by the precipitation occaſioned by the affuſion of the latter; as the deſtruction of the colour proceeds from the diſſipation of that curdled matter, whoſe texture is deſtroy'd, and the matter itſelf diſſolved into minute and inviſible particles, by the ſtrong acid men-

menstruum: which is the reason why there remains no sediment at the bottom; the infused oil taking it up, and resolving it into invisible parts, as water dissolves salt or sugar. PHYSICS.

But, to confirm my conjecture as to the chymical reason of our experiment, I might add, that I cannot only make the mercury precipitate out of the first simple solution quite of a different colour from that mentioned; but if, instead of altering the precipitating liquor, I altered the texture of the sublimate in such a manner, as my notion about salts requires, I can produce the same phenomenon. For having sublimed together equal parts of sal-armoniac, and sublimate, first diligently mixed, the ascending flowers dissolved in fair water, and filtred, gave a solution limpid and colourless, like that of the other sublimate; and yet an alkali dropped into this liquor, did not turn it yellow, but white. And, upon the same foundation, we may with quick-silver, unassisted by common sublimate, prepare another sort of flowers dissoluble in water, without discolouring it; which will also change its colour in the same manner. And so much does the colour depend upon the texture resulting from the convention of the several sorts of particles, that tho', in our experiment, oil of vitriol destroys the yellow colour; yet with quick-silver, and fair water, by the help of oil of vitriol alone, we may easily make a kind of precipitate, of a fair and permanent yellow; as we shall see hereafter. I here make choice of oil of vitriol, because, when well rectified, 'tis not only colourless and scentless, but extremely strong and incisive. And common *Aqua fortis* will not perform the same thing so well.

This experiment may be several times repeated with the same parcels of the liquors; for I found, that after I had clarify'd the orange-coloured mixture, by the addition of as little of the oil of vitriol as would suffice to produce the effect, I could, at pleasure, re-produce the opake colour, by dropping in fresh oil of tartar; and destroy it again, by the re-affusion of more of the acid menstruum: tho', by the addition of so much new liquor, in proportion to the mercurial particles, the colour will, at length, appear weak and faint.

The preceding experiment varied.

By boiling crude antimony in a strong and clear lixivium, you may separate a substance from it, call'd by some chymists it's sulphur; which, when let fall by the liquor, upon its refrigeration, often settles in flakes of a yellow substance. Considering therefore, that common sulphur, boiled in a lixivium, may be precipitated out of it by *Rhenish*, or white-wine, which are sourish liquors, and contain an acid salt; and having found, by tryal, that with other acid liquors I could precipitate out of lixivious solvents, some other mineral concretions, abounding with sulphureous parts, of which sort is crude antimony; I concluded it easy to precipitate the antimony, thus dissolved, with the acid oil of vitriol. And tho' common sulphur yields a white precipitate, called *Lac Sulphuris*; yet I supposed the precipitated antimony would be of a deep yellow colour; as well, if made with oil of vitriol, as by refrigeration, and length of time. From hence 'twas easy to deduce this experiment, that if we put into one glass some of this

PHYSICS. this freſh filtred ſolution of antimony; and into another, ſome of the orange-coloured mixture, made with a mercurial ſolution, and oil of tartar; a few drops of oil of vitriol, let fall into the latter glaſs, would turn the deep yellow mixture into a clear liquor; whilſt a little of the ſame oil dropped into the other glaſs, preſently turn'd the moderately clear ſolution into a deep yellow ſubſtance. But this ſucceeds not well, unleſs you employ a lixivium that has lately diſſolved the antimony, and before it ſuffers it to fall: yet, in ſummer-time, if the lixivium have been duly impregnated, and well filtred after it is quite cold, it will for ſome days retain antimony enough to exhibit, upon the affuſion of the corroſive oil, as much of a good yellow ſubſtance as is neceſſary to ſatisfy the ſpectators of the poſſibility of the experiment.

To find what kind of ſalt, whether acid, volatile or fix'd, predominates in an aſſign'd liquor, or ſaline body.

The diſtinction of ſalts into acid, volatile, and fixed or alkaline, may poſſibly be ſo uſeful in natural philoſophy, * as to render it an acceptable corollary of the preceding experiment, from thence to deduce a method of trying which, or whether any one of thoſe ſalts is predominant in chymical liquors. We have already ſhewn a way, by means of the tincture of *Lignum Nephriticum*, or of ſyrup of violets, to diſcover whether a ſalt propoſed be acid or not; yet we can thereby only find in general, that particular ſalts belong not to the tribe of acids: but cannot determine whether they belong to the tribe of the urinous; (under which I comprehend all thoſe volatile ſalts of animal, or other ſubſtances, that are contrary to acids) or to that of alkalies: for both theſe ſalino-ſulphureous ſalts will reſtore the blue colour to that tincture, and turn the ſyrup of violets green. The preſent experiment, therefore, commodiouſly ſupplies this deficiency. I found, that all thoſe chymical ſalts I thought fit to make tryal of, would, if they were of a lixivious nature, make, with ſublimate diſſolved in fair water, an orange-tawny precipitate;

* About the nature of ſalts, Sir *Iſ. Newton* thus argues. " When mercury ſublimate " is re-ſublimed with freſh mercury, and " becomes *Mercurius dulcis*, which is a " white taſtleſs earth, ſcarce diſſolvable " in water; and *Mercurius dulcis* re-ſublimed with ſpirit of ſalt, returns into " mercury ſublimate; and when metals " corroded with a little acid, turn into " ruſt, which is an earth taſtleſs, and " indiſſolvable in water; and this earth, " imbibed with more acid, becomes a " metallic ſalt; and when ſome ſtones, as " ſpar of lead, diſſolved in proper menſtrua, become ſalts; do not theſe things " ſhew, that ſalts are dry earth and watery acid united by attraction; and " that the earth will not become a ſalt " without ſo much acid as makes it diſſolvable in water? Do not the ſharp " and pungent taſtes of acids ariſe from " the ſtrong attraction, whereby the acid " particles ruſh upon, and agitate the " particles of the tongue? And when " metals are diſſolved in acid menſtrua, " and the acids in conjunction with the " metal, act after a different manner, ſo " that the compound has a different taſt " much milder than before, and ſometimes a ſweet one; is it not becauſe " the acids adhere to the metallic particles, and thereby loſe much of their " activity? And if the acid be in too " ſmall a proportion to make the compound diſſolvable in water; will it " not, by adhering ſtrongly to the metal, " become unactive, and loſe its taſt, and " the compound be a taſtleſs earth? " For ſuch things as are not diſſolvable " by the moiſture of the tongue, act not " upon the taſt. As gravity makes the " ſea flow round the denſer and weigh-" tier

tate; but if they were of an urinous nature, the precipitate would be white and milky. So that having always at hand ſome ſyrup of violets, and a ſolution of ſublimate; I can, by the help of the former, immediately diſcover whether a propoſed ſalt, or ſaline body, be of an acid nature; if it be, I need inquire no further; but if not, I can very eaſily and readily diſtinguiſh between the other two kinds of ſalts, by the white, or orange-colour, immediately produced by letting fall a few drops or grains of the ſalt to be examined, into a ſpoonful of the clear ſolution of ſublimate. Thus, for example, it has been ſuppoſed, that when ſal-armoniac, mixed with an alkali, is forced from it by the fire in cloſe veſſels, the volatile ſalt thence obtained, is but a more fine and ſubtile ſort of ſal-armoniac; which, 'tis preſumed, this operation only purifies more exquiſitely than common ſolutions, filtrations, and coagulations. But this opinion may eaſily be proved erroneous, as by other arguments, ſo particularly by our method of diſtinguiſhing the tribes of ſalts. For the ſaline ſpirit of ſal-armoniac, as it is in many other manifeſt qualities very like the ſpirit of urine; ſo, like that, it will inſtantly turn ſyrup of violets to a lovely green; a ſolution of good verdigreaſe into an excellent azure; and make the ſolution of ſublimate depoſite a white precipitate. Therefore, in moſt experiments, where I only deſign to produce a ſudden change of colour; I ſcruple not to uſe ſpirit of ſal-armoniac, when at hand, inſtead of ſpirit of urine; as indeed it ſeems chiefly to conſiſt of the volatile urinous ſalt. And by this way of examining chymical liquors, we may not only, in general, conclude affirmatively, but, in ſome caſes, negatively.

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Thus, ſince ſpirit of wine, and, as far as I have try'd, the chymical oils will not turn ſyrup of violets red, or green; nor the ſolution of ſublimate,

"tier parts of the globe of the earth; "ſo the attraction may make the watry "acid flow round the compacter particles of earth, for compoſing the particles of ſalt. For otherwiſe the acid "would not do the office of a medium "between the earth and common water, "for making ſalts diſſolvable in the water; nor would ſalt of tartar readily "draw off the acid from diſſolved metals, "nor metals the acid from mercury. "Now, as in the great globe of the earth "and ſea, the denſeſt bodies by their "gravity ſink down in water, and always endeavour to go towards the "centre of the globe; ſo in particles "of ſalt, the denſeſt matter may always endeavour to approach the centre of the particle: ſo that a particle "of ſalt may be compared to a chaos, "being denſe, hard, dry, and earthy in "the centre; and rare, ſoft, moiſt, and "watry in the circumference. And "hence it ſeems to be, that ſalts are of a "laſting nature, being ſcarce deſtroyed, "unleſs by drawing away their watry "parts by violence, or by letting them "ſoak into the pores of the central "earth by a gentle heat in putrefaction, till the earth be diſſolved by the "water, and ſeparated into ſmaller particles; which, by reaſon of their ſmallneſs, make the rotten compound appear of a black colour. Hence alſo it "may be, that the parts of animals and "vegetables preſerve their ſeveral forms, "and aſſimilate their nouriſhment; the "ſoft and moiſt nouriſhment eaſily changing its texture, till it becomes like the "denſe, hard, dry, durable earth in the "centre of each particle. But when the "nouriſhment grows too moiſt to be aſſimilated, or the central earth grows too "feeble to aſſimilate it, the motion ends "in confuſion, putrefaction, and death." *Newton. Optic.* p. 360—362.

white

PHYSICS. white or yellow, I infer, with probability, that either they are deſtitute of ſalt, or have ſuch as belongs not to either of the three grand families mention'd.

And thus, upon examining the ſpirit of oak, or of ſuch like ſubſtances, forc'd over the retort, I found by this means, amongſt others, that thoſe chymiſts are much miſtaken, who make it a ſimple liquor, and one of their hypoſtatical principles; for, not to mention what phlegm it may have, a few drops of one of this ſort of ſpirits, mix'd with a large proportion of ſyrup of violets, turn'd it purpliſh; by the affinity of which colour to redneſs, I conjectur'd that the ſpirit contain'd ſome acid corpuſcles, and accordingly found, that it would deſtroy the blueneſs of the nephritic tincture; and that being put upon corals, it would corrode them like common ſpirit of vinegar, and other acid liquors. And to examine whether there were not a great part of the liquor of other than an acid nature; having ſeparated the ſour part from the reſt, we concluded, the remaining part, tho' it had a ſtrong taſt, as well as ſmell, to be of a nature different from that of any of our three ſorts of ſalts; for it did as little as ſpirit of wine, and chymical oils, alter the colour of ſyrup of violets, and a ſolution of ſublimate: whence we alſo inferr'd, that the change made of that ſyrup into a purple colour, was effected by the vinegar, that proved one of the ingredients of the liquor which uſually paſſes for a ſimple, or uncompounded ſpirit.

And by the ſame way we may examine and diſcover many changes that are produced in bodies, either by nature or art; for both are able, by changing the texture of ſome ſubſtances, to qualify them to operate after a new manner, upon the ſyrup, or ſolution. Thus I have diſcover'd, that there are factitious bodies, which, tho' they run as readily as ſalt of tartar, belong, in other reſpects, not to the family of alkalies, much leſs to that of volatile or acid ſalts. Perhaps, too, I know a way of making a highly operative ſaline body, that ſhall neither change the colour of ſyrup of violets, nor precipitate the ſolution of ſublimate. And I can, likewiſe, conceal the liquors wherewith I make ſuch changes of colours as thoſe lately mention'd, by quite altering the texture of ſome ordinary chymical productions.

We may here obſerve, that the reaſon why our method of examining ſalts, ſucceeds in the ſolution of ſublimate, depends upon the particular texture of that ſolution, as well as upon the different natures of the ſaline liquors employ'd to precipitate it. For gold diſſolv'd in *Aqua regia*, whether it be precipitated with oil of tartar, which is an alkali, or ſpirit of ſal-armoniac, which is a volatile ſalt, will either way afford a yellow ſubſtance; tho' oil of vitriol, it ſelf, would not precipitate the metal out of the ſolution, or deſtroy the colour of it; notwithſtanding that oil will readily precipitate ſilver diſſolv'd in *Aqua fortis*. And if you diſſolve pure ſilver in *Aqua fortis*, and ſuffer it to ſhoot into cryſtals, the clear ſolution of theſe made in fair water, will afford a very white precipitate, whether occaſion'd by an alkali, or an acid ſpirit; tho' with ſpirit of ſal-armoniac, made with quick-

quick-lime, I cou'd obtain nothing like it; this volatile ſpirit, as alſo that of urine, ſcarce doing any more than ſtriking down a very ſmall quantity of matter, which was not ſo much as whitiſh; ſo that the remaining liquor, being ſuffer'd to evaporate its ſuperfluous moiſture, the greateſt part of the metalline corpuſcles, with the ſaline ones that had imbibed them, coagulated together into ſalt; as is uſual in ſuch ſolutions, when the metal has not been precipitated.

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41. From ſpirit of urine, made after fermentation, and a due proportion of copper-filings, having obtain'd a very lovely azure ſolution; I pour'd into a clean glaſs, about a ſpoonful of this liquor, and found I could, by ſhaking into it ſome drops of ſtrong oil of vitriol, immediately deprive it of its deep colour, and make it look like common water. And if into a ſmall ſpoonful of a ſolution of good *French* verdigreaſe, made in fair water, I dropp'd and ſhook ſome ſtrong ſpirit of ſalt, or dephlegm'd *Aqua fortis*, the greenneſs of the ſolution would, in a trice almoſt, totally diſappear, and the liquor, held againſt the light, ſcarce appear other than clear, or limpid; which is therefore remarkable, becauſe we know, that *Aqua fortis*, by corroding copper, the thing which gives the colour to verdigreaſe, uſually reduces it to a greeniſh blue ſolution. But if into this almoſt colourleſs liquor, you drop a juſt quantity either of oil of tartar, or ſpirit of urine, you may find, that after the ebullition is ceas'd, the mixture will diſcloſe a lively colour, tho' ſomewhat different from that the ſolution of verdigreaſe had originally.

One body changed into more, of different colours, by a colourleſs ingredient.

42. But theſe experiments tending either to alter the colour of a body, or entirely to deſtroy it, without giving it a ſucceſſor; I had a deſire alſo, to turn a body of one colour into two, very different both in conſiſtence and colour, by help of one that was colourleſs. In order to this, finding the acidity of ſpirit of vinegar to be wholly deſtroy'd, by working upon *Minium*, I concluded, that the ſolution of lead, in ſpirit of vinegar, would alter the colour of the juices and infuſions of ſeveral plants, after the manner of oil of tartar; and, accordingly, I was quickly ſatisfied, upon trial, that the infuſion of roſe-leaves would, by a ſmall quantity of this ſolution, well mix'd with it, be immediately turn'd into a ſaddiſh green.

And further, I had often found, that oil of vitriol will precipitate many bodies, both mineral and others, diſſolv'd not only in *Aqua fortis*, but particularly in ſpirit of vinegar. I had alſo found, that the powders precipitated by this liquor, were uſually fair and white. Laying theſe things together, 'twas not difficult to conclude, that if upon a tincture of red roſe-leaves, made with fair water, I dropp'd a quantity of a ſtrong ſolution of *Minium*, the liquor would be turn'd into the like muddy green ſubſtance, whereto oil of tartar would reduce it; and that a convenient proportion of oil of vitriol, would have two diſtinct operations upon the mixture; the one, to precipitate the diſſolv'd lead in the form of a white powder; the other, to clarify the muddy mixture, and both reſtore, and exceedingly heighten the redneſs of the infuſion of roſes:

and making the experiment accordingly, in a conical glaſs, that the ſubſiding powder might ſeem to poſſeſs the more ſpace, and be the more conſpicuous; I found, upon ſhaking the green mixture, that the colour'd liquor might be the more equally diſpers'd, a few drops of the rectified oil of vitriol, preſently turn'd the opake liquor into one that was clear and red, almoſt like a ruby; and threw down a large quantity of a powder, which, when ſettled, would have appeared very white, if ſome interſpers'd particles of the red liquor had not a little allay'd the purity, tho' it did not blemiſh the beauty of the colour.

And to ſhow that theſe effects flow from the oil of vitriol, as it is a ſtrong acid menſtruum, that has the property both to precipitate lead, as well as ſome other ſubſtances, out of ſpirit of vinegar, and to heighten the colour of red roſe-leaves; I have done the ſame thing, tho', perhaps, not quite ſo well, with ſpirit of ſalt, but could not do it with *Aqua fortis*; becauſe tho' that potent menſtruum, as well as the others, heightens the redneſs of roſes, yet it would not, like them, precipitate lead out of ſpirit of vinegar, but rather diſſolve it. And as, by this way, we have produc'd a red liquor, and a white precipitate, out of a dirty green magiſtery of roſe-leaves; ſo, by the ſame method, we may produce a fair yellow, and ſometimes a red liquor, and the like precipitate out of an infuſion of a curious purple colour. For, I lately intimated, that I had, with a few drops of an alkali, turn'd the infuſion of log-wood into a lovely purple; but if, inſtead of that alkali, I ſubſtituted a very ſtrong, and well-filtred ſolution of *Minium*, made with ſpirit of vinegar; and took about half as much of this liquor as there was of the infuſion of log-wood; a convenient proportion of ſpirit of ſalt, would, if the liquors were briskly ſhook together, preſently ſtrike down a precipitate like the former, and turn the liquor that ſwam above it, for the moſt part, into a lovely yellow.

But to advance this experiment a little further, I conſider'd, that in caſe I firſt turn'd a ſpoonful of the infuſion of log-wood purple, by a convenient proportion of the ſolution of *Minium*; the affuſion of ſpirit of ſal-armoniac, would precipitate the corpuſcles of lead, conceal'd in the ſolution of *Minium*, and yet not deſtroy the purple colour of the liquor; whereupon I thus proceeded. I took about a ſpoonful of the freſh tincture of log-wood, (for if it be ſtale, the experiment will not always ſucceed) and having put to it a convenient proportion of the ſolution of *Minium*, to turn it into a deep and almoſt opake purple; I then dropp'd in as much ſpirit of ſal-armoniac as I gueſs'd would precipitate about half the lead; and immediately ſtirring the mixture well together, I mix'd the precipitated parts with the others, ſo that they fell to the bottom, partly in the form of a powder, and partly in the form of a curdled ſubſtance, that retain'd, as well as the upper liquor, a bluiſh purple colour, ſufficiently deep; and then inſtantly pouring in a pretty quantity of ſpirit of ſalt, the matter firſt precipitated, was, by the conical figure of the glaſs, preſerv'd from the ſpirituous ſalt, which ſuddenly precipitated upon it a new bed of white powder, or the remaining corpuſcles of the lead, that the urinous ſpirit had

not

not ſtruck down ; ſo that there appear'd in the glaſs, three diſtinct and very differently colour'd ſubſtances ; a purple, or violet-colour'd precipitate at the bottom ; a white and carnation precipitate over that; and, at the top of all, a tranſparent liquor, of a lovely yellow, or red. PHYSICS.

Thus, tho' to ſome, I may ſeem to have hit on this, and the like experiments, by chance ; whilſt others imagine they proceed from ſome extraordinary inſight into the nature of colours ; yet, indeed, the contrivance of them need not to be look'd upon as any great matter, in one who is a little vers'd in my notions about the differences of ſalts.

Changes of colours produced in a dry, white body, by ſpring-water.

43. That the colour of a body may be chang'd by a liquor, which, of it ſelf, is colourleſs, provided it be ſaline, we have already ſhewn by a multitude of inſtances ; and the thing does not appear ſtrange, becauſe ſaline particles ſwimming up and down in liquors, have been often obſerv'd to act powerfully in the production and change of colours : but it has ſeem'd ſurprizing to many, unacquainted with chymical operations, that a white dry body ſhould immediately acquire a new rich colour, upon the bare affuſion of clear ſpring water. And yet the way of producing ſuch a change of colours, may be eaſily hit on by thoſe who often make ſolutions of mercury. For we have try'd, that tho' by evaporating a ſolution of quickſilver in *Aqua fortis*, till the remaining matter began to be tolerably dry, fair water, pour'd on the remaining calx, turn'd it but a little yellowiſh ; yet, when we took good quick-ſilver, and three or four times its weight of oil of vitriol, and in a glaſs retort drew off the ſaline menſtruum from the metalline liquor, till there remain'd a dry ſnow-white calx at the bottom ; upon pouring on it a large quantity of fair water, we did, almoſt in a moment, perceive it to paſs to one of the lovelieſt light yellows that ever we beheld. And the turbith mineral is of a colour not much inferiour to this ; tho' it be often made with a different proportion of the ingredients, and after a more troubleſome manner. But this colour, tho' ſo exquiſitely fine, and ſo greatly wanted by painters, is, I fear, too coſtly to be employ'd by them, unleſs about curious pieces ; tho' I do not know how well it will agree with every pigment, eſpecially with oil-colours.

And whether this experiment be really of another nature, than thoſe wherein ſaline liquors are employ'd, may be ſo plauſibly doubted, that whether the water pour'd on the calx, do barely, upon imbibing ſome of its ſaline parts, alter its colour, changing its texture ; or whether by diſſolving the coagulated ſalts, it becomes a ſaline menſtruum, and, as ſuch, operates upon the mercury, I leave to be conſider'd. I have, however, ſeveral times, with fair water, waſh'd from this calx abundance of ſtrongly-taſted corpuſcles, which, by the abſtraction of the menſtruum, I could reduce into ſalt.

But, to ſhew how much a real and permanent colour may be call'd out by a liquor that has neither colour, nor ſo much as ſaline, or other active parts ; provided it can but bring the parts of the body it imbibes, to convene into cluſters, diſpos'd after a particular manner, we made the following experiment.

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A permanent colour produced by a particular arrangement of parts.

44. We put ſome powder of good common vitriol into a crucible, and kept it melted in a gentle heat, till, by the evaporation of ſome parts, and the tranſpoſition of the reſt, it had quite loſt its former colour; what remain'd we took out, and found it to be a friable calx, of a dirty grey colour: on this we pour'd fair water, which it did not tinge either green or blue, but only ſeem'd to make a muddy mixture with it; then ſtopping the vial wherein the ingredients were, we let it ſtand in a quiet place for ſome days. The water having now diſſolv'd a large part of the imperfectly calcin'd body; the vitriolic corpuſcles ſwimming to and fro in the liquor, had time, by their occurſions, to conſtitute many little maſſes of vitriol, which gave the water they impregnated, a fair vitriolic colour; and this liquor being pour'd off, the remaining dirty powder, in time, communicated the like colour, but not ſo deep, to a ſecond parcel of clear water, that we pour'd on it.

Various colours produced in different parts of the ſame liquor.

45. It may contribute to ſhew how much ſome colours depend upon the leſs or greature mixture, and contemperation of light with ſhades, to obſerve how the number of particles of the ſame colour, either receiv'd into the pores of a liquor, or ſwimming up and down in it, may ſeem greatly to vary the colour of it. I could produce ſeveral inſtances of ſolid bodies, wherein, if the colour be not a light one, as white, yellow, or the like, the cloſeneſs of parts in the pigment, makes it look blackiſh; tho' when laid on but thinly, it will, perhaps, appear blue, green, or red: but I ſhall rather inſiſt upon liquors, than dry bodies.

If, then, you put a little fair water into a clear ſlender vial, and let fall into it a few drops of a ſtrong decoction, or infuſion, of cochineal, or of brazil, the ting'd drops will deſcend like little clouds into the liquor, thro' which, if by ſhaking the vial, you diffuſe them, they will turn the water of a pink colour; by dropping in a little more of the decoction, you may heighten the colour into a fine red, almoſt like that of rubies; and by continuing the affuſion, the liquor may be brought to a kind of crimſon, and afterwards to a dark opake redneſs. And whilſt the liquor paſſes from one of theſe colours to the other, you may obſerve many leſs noted colours, bordering upon red, to which it is not eaſy to aſſign names; eſpecially conſidering how much the proportion of the decoction to the fair water, and the ſtrength of that decoction, may, with other circumſtances, vary the phenomena of this experiment. But, to make it with the greater conveniency, we uſe, inſtead of a vial, a ſlender pipe of glaſs, of about a foot in length, and about the thickneſs of a man's little finger: for, if leaving one end of this tube open, you ſeal up the other hermetically, or otherwiſe exactly ſtop it, you have a glaſs, wherein may be obſerv'd, the variations of the colours of liquors, much better than in large vials; and wherein experiments of this nature, may be made with very ſmall quantities of liquor. And in this pipe, may be produc'd various colours, in the various parts of the liquor, and be kept long ſwimming upon one another, unmix'd.

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It has excited the admiration of ſome perſons to ſee what a variety of colours we have ſometimes produced in ſuch glaſſes, by the bare infuſion of brazil, variouſly diluted with fair water, and altered by the infuſion of ſeveral chymical ſpirits, and other ſaline colourleſs liquors; and when the whole mixture is reduced to an uniform degree of colour, I have made it appear to be of colours gradually differing, by pouring it into glaſſes of a conical figure. Or even take a large round vial, fill it with the red infuſion of brazil, hold it againſt the light, and you will diſcern a notable difference betwixt the colour of that part of the liquor which is in the body of the vial, and that which is more pervious to the light, in the neck.

I once had a glaſs, and a blue liquor, which was chiefly a certain ſolution of verdigreaſe, ſo fitted, that tho' in other glaſſes the experiment would not ſucceed; yet when this particular glaſs was filled with that ſolution, it appeared in the body of the vial of a lovely blue, and in the neck of a manifeſt green. I had alſo, a broad piece of glaſs, which being viewed againſt the light, ſeem'd clear enough; and, held from the light, appeared very little diſcoloured: yet it was a piece knock'd off from a great lump of glaſs, to which if we rejoin'd it, where it had been broken off, the whole maſs appeared green as graſs.

I have, likewiſe, ſeveral times uſed bottles and ſtopples, both made of the very ſame metal; and yet whilſt the bottle appeared only inclining to green, the ſtopple was of ſo deep a colour, that it could hardly be thought poſſible they ſhould be the ſame materials. And I have by me a flat glaſs, on which, if I look againſt the light with the broad ſide obverted to my eye, it appears like a good ordinary window-glaſs; but if I turn the edge of it to my eye, and ſtand in a convenient poſition, with regard to the light, it emulates an emerald.

I have ſometimes made a fluid kind of pigment, which, dropped on a piece of white paper, appears, where any quantity of it falls, of a crimſon colour; but, being ſpread thinly on the paper, preſently exhibits a fair green.

Let me add, that having made many experiments with that blue ſubſtance, called by the painters litmaſe; we have ſometimes obſerved, that being diſſolved in a due proportion of fair water, the ſolution, either oppoſed to the light, or dropped upon white paper, appeared of a deep colour, betwixt crimſon and purple; yet, when ſpread very thin on the paper, and ſuffered to dry there, the paper was thereby ſtain'd of a fine blue. This experiment alſo ſucceeded, when made on a flat piece of pure white glazed earth. And having let fall a few drops of the ſtrong infuſion of this litmaſe, in fair water, into a fine cryſtal glaſs, ſhaped like an inverted cone, and almoſt fill'd with clear water; I had the pleaſure to ſee theſe few tinged drops, variouſly diſperſing themſelves thro' the limpid water, exhibit many colours, or varieties of purple and crimſon. But when the corpuſcles of the pigment ſeem'd to have equally diffuſed themſelves thro' the whole; by adding to it two or three drops of ſpirit of ſalt, we perceived it firſt made an odd change in the colour of the liquor, PHYSICS. as well as a viſible commotion among its ſmall parts, and in a ſhort time changed it wholly into a very glorious yellow, like that of a topaz. After this, if I let fall a few drops of a ſtrong heavy ſolution of pot-aſhes, whoſe weight would quickly ſink it to the ſharp bottom of the glaſs; there would ſoon appear four very pleaſant and diſtinct colours: *viz.* a faint bright one, at the ſharpeſt part of the glaſs; a purple, a little higher; a deep and glorious crimſon, in the confines betwixt the purple and the yellow; and an excellent yellow, the ſame that before adorned the whole liquor, reaching from thence to the top of the glaſs. And if I poured, very gently, a little ſpirit of ſal-armoniac upon the upper part of this yellow, there would alſo ariſe there a purple, or a crimſon, or both; ſo that the unaltered part of the yellow liquor appeared intercepted betwixt the two neighbouring colours. Hence we need not be ſurprized at the tricks of thoſe mountebanks, who are commonly called water-drinkers. For tho' not only the vulgar, but many perſons far above that rank, have wondered to ſee a man, after drinking large quantities of fair water, return it in the form of claret, ſack, and milk; yet having by chance had occaſion to oblige a wanderer, who made a profeſſion of this, and other juggling tricks, he ingenuouſly confeſſed to me, that the art conſiſted rather in a few tricks than any great skill in altering the nature and colours of things. And I ſuſpect there may be a great deal of truth in a little pamphlet, printed long ago in *Engliſh*; wherein the author undertakes to diſcover, from the confeſſion of ſome of the accomplices themſelves, that a famous water-drinker, then much admired in *England*, performed his pretended tranſmutations of liquors by the help of two or three inconſiderable preparations and mixtures of obvious fluids; and chiefly of an infuſion of brazil variouſly diluted and made pale, yellowiſh, &c. with vinegar. And, for my part, what moſt ſurprizes me in this affair, is, that the drinkers can take down ſo much water, and ſpout it out again with violence; tho' cuſtom, and a vomit ſeaſonably taken before-hand, may in ſome of them greatly facilitate the work. But as for the changes they make in liquors, thoſe are but few and ſlight.

Changes of colour may greatly depend upon the peculiar texture of the menſtruum.

46. *Helmont* uſed to make a preparation of ſteel, which a very ingenious chymiſt ſometimes employs for a *Succedaneum* to the ſpaw-waters; diluting this *Eſſentia Martis liquida*, as he calls it, with a due proportion of water. And tho' this preparation be almoſt of the colour of a *German* amethyſt, and conſequently remote from green; yet a very few drops of it being let fall into a large proportion of *Rheniſh*, or white-wine, it immediately turns them to a lovely green. By which phenomenon we may learn how requiſite it is in experiments, about the changes of colours, carefully to regard the circumſtances of them; for water will not, as I have purpoſely try'd, concur to the production of any ſuch green; nor did it give that colour to moderate ſpirit of wine, wherein I diſſolved it: and wine itſelf is a liquor that few would ſuſpect able, of a ſudden, to work any ſuch change in a metalline preparation of this nature. And to ſatisfy myſelf that this new colour proceeds rather from the peculiar texture of the

the wine, than from any greater acidity that *Rheniſh*, or white-wine has, in compariſon of water; I ſharpened the ſolution of this eſſence in fair water, with a large quantity of ſpirit of ſalt; and then the mixture acquired no greenneſs.

To vary the experiment a little, I try'd, that if into ſome *Rheniſh* wine made green by this eſſence, I dropped an alkaline ſolution, or urinous ſpirit; the wine would preſently grow turbid, and of an odd dirty colour. But if, inſtead of diſſolving the eſſence in wine, I diſſolved it in fair water, ſharpen'd with a little ſpirit of ſalt; then either the urinous ſpirit of ſal-armoniac, or the ſolution of the fixed ſalt of pot-aſhes, would immediately turn it of a yellowiſh colour; the fixed or urinous ſalt precipitating the vitriolic ſubſtance contained in the eſſence. And as our eſſence imparts a greenneſs to wine, but not to water; *Olaus Wormius* tells us of a rare kind of turn-ſol he had, whoſe beautiful redneſs would be eaſily communicated to water, but ſcarce to wine, and not at all to ſpirit of wine: in which laſt circumſtance it agrees with our eſſence, tho' they diſagree in other particulars.

The different colours of metals in different ſtates.

47. I have often taken notice, that metals, as they appear to the eye, before they come to be altered by other bodies, exhibit colours very different from thoſe which the fire, or a menſtruum, either ſeparately or conjointly, produce in them; eſpecially conſidering, that theſe metalline bodies are, after all their diſguizes, reducible not only to their former conſiſtence, and other more eſſential properties, but to their colour too; as if nature had given them an external and an internal colour. But, upon a more attentive conſideration of this difference of colours, it ſeems probable to me, that many of thoſe we call internal, are rather produced by the coalition of metalline particles with thoſe of the ſalts, or other bodies employ'd to work on them, than by the bare alteration of the parts of the metals themſelves. Of theſe adventitious colours of metalline bodies, the chief ſorts ſeem to be three; *viz.* ſuch as are produced by the ſole action of the fire; ſuch as emerge from the coalition of metalline particles, with thoſe of ſome menſtruum employ'd to corrode, or precipitate the metal; and, laſtly, the colours afforded by metalline bodies, either melted with, or otherwiſe penetrating into others, eſpecially ſuch as are fuſible.

As to the firſt of theſe colours; 'tis well known to chymiſts, that tin calcined by fire alone, affords a white calx; and lead, by the ſame means, yields that common red powder we call *Minium*; copper alſo, calcined *per ſe*, by a long or violent fire, gives a very dark or blackiſh powder; iron, likewiſe, may, by the action of reverberated flames, be turn'd into a colour almoſt like that of ſaffron, as we ſee in the preparation of *Crocus Martis per ſe*; and mercury, by the power of fire, will be turn'd into a red powder, call'd precipitate *per ſe*.

Beſides, the ſame metal may, by the ſucceſſive operations of the fire, receive ſeveral adventitious colours; as is evident in lead, which, before it arrives at ſo deep a colour as that of *Minium*, may paſs thro' ſeveral others.

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PHYSICS. And, not only the *Calces*, but the glaſſes of metals, vitrify'd *per ſe*, may have colours different from the obvious or natural colours of the metal; as I have obſerved in the glaſs of lead, made by long expoſing that metal crude to a violent fire. I have likewiſe ſeen a piece of very dark glaſs, which an ingenious artificer, who ſhewed it me, profeſſed he made of ſilver alone, by an extreme violence of the fire.

Minerals alſo, by the action of the fire, may be brought to afford colours very different from their own; as was obſerved about the variouſly coloured flowers of antimony. To which we may add, the whitiſh grey colour of its calx, and the yellow or reddiſh one of the glaſs, into which that calx may be fluxed. And vitriol, calcined with a very gentle heat, and afterwards with higher degrees of it, may be made to paſs thro' ſeveral colours, before it deſcends to a dark purpliſh one, whereto a ſtrong fire will at length reduce it.

48. The adventitious colours produc'd in metals, by ſaline liquors, are many of them, well known to chymiſts. That gold, diſſolv'd in *Aqua regia*, communicates its own colour to the menſtruum, is a common obſervation; but the ſolutions of mercury, in *Aqua fortis*, are not generally obſerv'd to give any notable tincture to the menſtruum; tho' ſometimes, when the liquor firſt falls upon the quick-ſilver, I have obſerv'd a very remarkable greenneſs, or blueneſs to be produc'd. Tin, corroded by *Aqua fortis*, till the menſtruum will work no farther on it, becomes exceeding white; and eaſily, of it ſelf, acquires the conſiſtence not of a metalline calx, but of a coagulated matter, ſo like either to curdled milk, or curdled whites of eggs, that a perſon unacquainted with ſuch ſolutions, might eaſily be miſtaken in it. But when I purpoſely prepar'd a menſtruum that wou'd diſſolve it, as *Aqua fortis* diſſolves ſilver, not barely corrode it, and quickly let it fall again; I remember no particular colour in the ſolution: as if the more whitiſh metals did not much tinge their menſtrua, tho' the high-colour'd ones, as gold and copper, do. For lead diſſolv'd in ſpirit of vinegar, or *Aqua fortis*, gives a clear ſolution: and, if the menſtruum be abſtracted, appears either diaphanous, or white. And 'tis worth noting, that tho' when iron is diſſolv'd in oil of vitriol, diluted with water, it affords a ſalt, or magiſtery, ſo like in colour, as well as ſome other qualities, to green vitriol, that chymiſts properly call it, *Vitriolum Martis*; yet, by changing the menſtruum, and pouring upon the filings of ſteel, *Aqua fortis*, inſtead of oil of vitriol, I obtain'd not a green, but a ſaffron-colour'd ſolution, or rather a thick liquor, of a deep yellowiſh red. Common ſilver, diſſolv'd in *Aqua fortis*, yields a ſolution ting'd like that of copper; which is not to be wonder'd at, becauſe, in coining ſilver, they give it an allay of copper; and what is ſold for refin'd ſilver, is not ſo perfectly free from that ignobler metal, but that a ſolution of it in *Aqua fortis* will give its tincture to the menſtruum. But we could not obſerve, upon the ſolution of ſome ſilver perfectly refin'd, that the menſtruum, tho' held againſt the light, in a cryſtal vial, manifeſtly diſclos'd any tincture; only it ſometimes ſeem'd not quite deſtitute of a very faint bluiſh caſt.

But

But, of all the metals, there is not one which so easily and constantly discloses its colour as copper. For, in acid menstrua, as *Aqua fortis*, and spirit of vinegar, it not only gives a bluish green solution; but if it be almost any way corroded, it appears of one of those two colours. And so constant is the disposition of copper, notwithstanding the disguize artists put upon it, to discover its colour, that we have, by forcing it up with sal-armoniac, obtain'd a sublimate of a bluish cast. Nay, a famous chymist affirms, that the very mercury of it is green; but till he teaches us an intelligible way of making such a mercury, we must content ourselves to say, that we have had a cupreous body precipitated out of a distill'd liquor, which seem'd to be the sulphur of that metal, and even when flaming, appear'd of a greenish colour. And, indeed, copper is a metal so easily wrought upon, by liquors of several kinds, that, I might say, I know not any mineral which concurs to the production of such a variety of colours, as copper dissolv'd in several menstrua, *viz.* spirit of vinegar, *Aqua fortis*, *Aqua regia*, spirit of nitre, of urine, of soot, oils of several kinds, *&c.* if the variety of them were not comprehended within the limits of greenish blue, or bluish green.

But being desirous to try if I could not with crude copper make a green solution, without the bluishness that usually accompanies it, I concluded upon two menstrua, which, tho' I had not known employ'd to work on this metal, prov'd successful; the one was spirit of sugar, and the other, oil, or spirit of turpentine, which affords a fine green solution, useful on several occasions. And yet to shew that the adventitious colour may result as well from the true and permanent copper it self, as the salts wherewith 'tis corroded; if you take a piece of good *Dantzick* copperas, or any other vitriol wherein copper is predominant, and having moisten'd it with fair water, rub it upon a bright piece of iron, or steel; it will (as we have formerly said) presently stain it with a reddish colour, like that of copper.

We have sometimes try'd what colours such minerals as tin-glass, antimony, spelter, *&c.* would yield in several menstrua. The like we have also done with stones; among which, that famous one call'd by *Helmont*, *Paracelsus's Ludus*, has afforded in menstrua able to dissolve so solid a stone, sometimes a yellowish, and sometimes a red solution. And from minerals I have obtain'd, with several menstrua, very different colours; and some such, as, perhaps, would scarce be expected from such bodies.

The colours of metals may, in many cases, be further alter'd, by employing either precipitating salts, or other convenient substances, to act upon their solutions. If quick-silver be dissolv'd in *Aqua fortis*, and precipitated out of the solution, either by water impregnated with sea-salt, or the spirit of that concrete, it falls to the bottom in the form of a white powder; but if precipitated with an alkali, it affords a yellowish powder: and if no precipitation be made, and the menstruum be drawn off with a convenient fire, the corroded mercury will remain at the bottom, and may be made to appear of different colours, by different degrees of heat.

PHYSICS. Thus having abſtracted *Aqua fortis* from ſome quick-ſilver that we had diſſolv'd in it, till there remain'd a white calx: by expoſing that to ſeveral degrees of fire, and afterwards to a naked one, we obtain'd ſome new colours; and at length, the greateſt part of the calx lying at the bottom of the vial, and being brought partly to a deep yellow, and partly to a red colour, the reſt appear'd elevated to the top and neck of the vial; ſome in the form of a reddiſh, and ſome of an aſh-colour'd ſublimate. And even the ſuccedaneum to a menſtruum, may ſometimes ſerve to change the colours of a metal. The lovely red, which painters call vermilion, is made of mercury, which appears of the colour of ſilver; and of brimſtone, which reſembles that of gold, ſublim'd up together in a certain proportion.

49. The third of the principal kinds of adventitious colours in metals, is that produc'd by aſſociating them, eſpecially when calcin'd with other fuſible bodies, and eſpecially *Venice*, and other fine colourleſs ſorts of glaſs.

I formerly gave an example of a metal, imparting a colour to glaſs, very different from its own, in ſhewing how ſilver turns it to a lovely golden colour; I ſhall now add, that tho' I learn'd from one of the chief artificers in painted glaſs, that thoſe of his trade colour it yellow, with a preparation of the calx of ſilver; yet, having mix'd a few grains of ſhell-ſilver, ſuch as is employ'd with the pencil and pen, with a convenient proportion of powder'd cryſtal-glaſs, and kept them for two or three hours in fuſion, I was ſurpriz'd to find the melted maſs appear, upon breaking the crucible, of a lovely ſapphirine blue: which made me ſuſpect my ſervant might have brought me a wrong crucible; but he conſtantly affirm'd it to be the ſame wherein the ſilver was put; and conſiderable circumſtances countenanc'd his aſſertion: whence, till farther trial, I ſuſpect either that ſilver, which is not very probable, brought to a perfect fuſion with glaſs, may impart other colours thereto, than when neal'd upon it; or elſe, which is leſs unlikely, that tho' ſilver-beaters uſually chuſe the fineſt coin they can get, as that which ſpreads moſt under the hammer; yet the leaf-ſilver, whereof this ſhell-ſilver was made, might retain ſo much copper, as to give the predominant tincture to the glaſs.

Take it, as another inſtance of the adventitious colours of metals, that tho' copper, calcin'd *per ſe*, affords but a dark and baſely colour'd calx, yet the glaſs-men tinge their glaſs green therewith. And, I remember, when taking ſome crude copper, and by frequent ignition, and quenching it in water, till we had reduced it to a darkiſh-colour'd powder; and afterwards keeping it in fuſion in about a hundred times its weight of fine glaſs, we obtain'd a blue-colour'd maſs, which wou'd, perhaps, have been green, if we had hit upon the right proportion of the materials, the degree of fire, and the time wherein it ought to be kept in fuſion. But tho' copper thus gives ſomewhat near the like colour to glaſs, that it communicates to *Aqua fortis*; yet it ſeems worth inquiring, whether thoſe new colours, which mineral bodies diſcloſe in melted glaſs, proceed from the

the coalition of the corpuſcles of the mineral with the particles of the glaſs, or from the action of the alkaline ſalt (a principal ingredient of glaſs) upon the mineral body; from the concurrence of both theſe cauſes, or from any other. PHYSICS.

We may obſerve, that putty, made by calcining together a proportion of tin and lead, as it is itſelf a white calx; ſo it turns the matter of the purer ſort of glaſs, wherewith it is melted, into a white maſs; which, if it prove opake enough, is employ'd for white amel.

When the materials of glaſs, melted with calcin'd tin, have compoſed an opake white maſs; 'tis made the baſis of all thoſe fine concretes the goldſmiths, and ſeveral artificers employ in the curious art of enamelling: for this white fuſible ſubſtance receives and preſerves the colours of many other mineral ſubſtances, which, like it ſelf, will endure the fire.

And, as it appears, that ſeveral minerals will impart to fuſible maſſes, colours different from their own; ſo, by the making and compounding of amels, it is manifeſt, that many bodies will both retain their colour in the fire, and impart the ſame to ſome others wherewith they were vitrify'd. Thus we formerly ſaw, that a blue and yellow compoſed a green amel. And 'tis pretty to ſee, that ſome colours are of ſo fixed a nature, as to be capable of mixture, without receiving any damage from the fire; and that mineral pigments may be mix'd by it, almoſt as regularly and ſucceſsfully as the vulgar colours, by the help of water, in the vats of dyers.

Both metalline, and other mineral bodies, may be employ'd to give tinctures to glaſs; and 'tis worth obſerving, how ſmall a quantity of ſome mineral ſubſtances will tinge a comparatively vaſt proportion of it. We have ſometimes attempted to colour glaſs even with precious ſtones; and had no cauſe to think the experiment ill beſtow'd. 'Tis known that the artificers in glaſs tinge their metal blue, with that dark mineral, zaffora, which ſome would have to be a mineral earth; others, a ſtone; and others, again, neither; but it is confeſſedly of a dark, not a blue colour. 'Tis, likewiſe, a remarkable practice among them, that they employ manganeſe to exhibit in glaſs, not only other colours than its own, (wherein it is ſo like to the load-ſtone, that it is given by mineraliſts for one reaſon of its *Latin* name *Magneſia*) but colours different from one another: for tho' they uſe it to clarify their glaſs, and free it from that bluiſh greeniſh colour, which it wou'd otherwiſe be ſubject to; yet they alſo employ it in certain proportions, to tinge their metal, both with a red colour, and with a purpliſh, or murrey; and, by putting in a greater quantity, they alſo make with it that deep obſcure glaſs, which paſſes for black. And this agrees very well with what we formerly obſerv'd of the apparent blackneſs of thoſe bodies that are over-charg'd with the corpuſcles of ſuch pigments, as are red, blue, green, &c.

PHYSICS.

An eaſy method of examining ores.

And as by ſeveral metals, and other minerals, we can give various colours to glaſs; ſo, on the other hand, from the different colours that mineral ores, or other mineral powders, by being melted with glaſs, diſcloſe therein, a probable conjecture may be often made of the metal, or known mineral, that a propos'd ore either contains, or is neareſt allied to. And this eaſy method of examining ores, may, in ſome caſes, prove very ſerviceable; but great circumſpection is requiſite to keep it from proving fallacious, upon account of the variations of colour, producible by the different proportions that may happpen, betwixt the ore and the glaſs; by the richneſs and poorneſs of the ore it ſelf; by the degree of fire; and eſpecily by the length of time, during which the matter is kept in fuſion.

There is another way, different from thoſe already mentioned, whereby metals may be brought to exhibit adventitious colours. And, in this caſe, the metal does not ſo much impart a colour to another body, as receive one from it; or rather, both bodies, by the new texture reſulting from their mixture, produce a new colour. I will not here inſiſt upon the examples afforded us by yellow orpiment, and common ſea-ſalt; from which, ſublimed together, chymiſts unanimouſly affirm their white or cryſtalline arſenic to be made: but 'tis worth noting, that tho' yellow orpiment be acknowledged by far the moſt predominant of the two ingredients of arſenic; yet arſenic, duly added to the higheſt coloured metal, copper, when in fuſion, gives it a whiteneſs both within and without. Thus, alſo, *Lapis Calaminaris* changes and improves the colour of copper, by turning it into braſs. And I have ſometimes, by the help of zink, duly mixed after a certain manner, given to copper as rich a golden colour as ever I ſaw in the beſt true gold.

The way of making counterfeit gems.

Upon knowing the different methods of producing the adventitious colours of metals and minerals in bodies capable of vitrification, depends the pretty art of making counterfeit gems: for whilſt pure ſand, or calcined cryſtal, gives the body in their preparation; 'tis, for the moſt part, ſome metalline or mineral calx, mixed in a ſmall proportion with it, that gives the colour. Calcined lead, fuſed with fine white ſand, or cryſtal, reduced by ignitions and ſubſequent extinctions in water, to a ſubtile powder, will, of itſelf, be brought, by a due decoction, to give a clear maſs, coloured like a *German* amethyſt. But this colour may be eaſily over-powered by thoſe of ſeveral other mineral pigments, ſo that with a glaſs of lead you may emulate the freſh and lovely green of an emerald; tho', in many caſes, the colour which the lead itſelf, upon vitrification, tends to, may vitiate that of the pigment deſign'd to appear in the maſs. Theſe colours alſo depend ſo much upon the texture of the materials, that we have made the glaſs of lead itſelf, compoſed of about three parts of litharge, or *Minium*, fuſed with one of cryſtal, or ſand, very finely powdered, paſs thro' different colours, according as we kept it more or leſs in fuſion. But the degrees of coction, and other circumſtances, may ſo vary the colour produced, that, in a ſmall crucible, I have had fragments

ments of the ſame maſs; in ſome of which, perhaps, not ſo big as a hazel-nut, four diſtinct colours might be diſcerned.

Beſides the three mentioned ſorts of adventitious colours in metals, there may be others reducible under the ſame head; of which I ſhall inſtance only in two.

The firſt is afforded us from the practice of ſcarlet-dyers. A moſt famous maſter in this art aſſured me, that neither he, nor others, can ſtrike the lovely colour, called the bow-dye, unleſs their materials be boiled in veſſels of a particular metal. Secondly, metals will afford uncommon colours, by imbuing ſeveral bodies with ſolutions of them, made in proper menſtrua. Thus, tho' copper, plentifully diſſolved in *Aqua fortis*, will communicate to ſeveral bodies the colour of that ſolution; yet ſome other metals will not; as I have often try'd. Gold, diſſolved in *Aqua regia*, dyes the nails and skin, the hafts of knives, and other things made of ivory, not with a yellow, but a purple colour; which, tho' it manifeſt itſelf but ſlowly, is very durable, and can ſcarce ever be waſhed out. Thus we formerly ſaid, that the purer cryſtals of fine ſilver made with *Aqua fortis*, tho' they appear white, will preſently dye the skin and nails of a black, not to be waſhed off like ordinary ink. And many other bodies may, in the ſame manner, be dy'd, ſome of a black, and others of a blackiſh colour.

Mineral ſolutions may give colours different from their own.

Mineral ſolutions, alſo, may produce colours different from thoſe of the liquors themſelves. I ſhall not fetch an example of this from what happens in the ſalting of beef, which often appears green, and ſometimes of a reddiſh colour, when boil'd; nor ſhall I inſiſt on the practice of giving, by ſalt-peter, and a certain proportion of common ſalt, a fine redneſs to ſuch fleſh as would otherwiſe appear purely white: I rather chuſe to ſay, that I have ſeveral times found a ſolution of the ſulphur of vitriol, or even of common ſulphur, tho' the liquor appeared clear, immediately to tinge a piece of new coin, or other clean ſilver, ſometimes with a golden, ſometimes with a deeper, and more reddiſh colour; according to the ſtrength of the ſolution, and the quantity of it, that chanced to adhere to the metal. This renders it the leſs ſurprizing, that the water of the hot ſpring at *Bath*, abounding with diſſolved ſubſtances of a very ſulphureous nature, ſhould, for a while, tinge ſuch pieces of coin yellow, as are for a due time immerſed in it. And even vegetable liquors, whether by degeneration, or by altering the texture of the body that imbibes them, may ſtain other ſubſtances with colours very different from their own. 'Tis affirmed, that the green juice of *Alcanna* dyes the skin and nails of a laſting red.

The method of preparing a yellow vegetable lac.

50. Meeting, in an *Italian* author, with a way of preparing what he calls a *Lacca* of vegetables, by which the *Italians* mean a kind of extract fit for painting, like that rich *Lacca*, in *Engliſh* commonly call'd lac, employ'd by painters as a glorious red; and finding the experiment conſiderable, but very imperfectly related, we improved upon it in the following manner:

PHYSICS. manner: tho' the thing, as there delivered, is, by the *Italians*, eſteem'd a great ſecret.

Put what quantity of powdered turmeric you pleaſe into fair water; adding to every pound of that liquor a large ſpoonful of a very ſtrong lixivium of pot-aſhes, clarify'd by filtration; let all this ſimmer over a ſoft fire in a clean glazed earthen veſſel, till you find, by the immerſion of a ſheet of white paper, that the liquor is ſufficiently impregnated with the golden tincture of the turmeric: then take the decoction off the fire, and filtre or ſtrain it; and afterwards, leiſurely dropping into it a ſtrong ſolution of roch-alum, the decoction will, as it were, be curdled; and the tinged part of it either emerge, ſubſide, or ſwim up and down in little yellow flakes: when, if you pour this mixture into a funnel, lined with cap-paper, the liquor that formerly filtred yellow, will now paſs clear, and leave its tinged parts behind in the filtre; into which fair water muſt be ſo often poured, till the matter therein contain'd be dulcify'd; that is, till the water paſſes thro' it, as taſteleſs as when it was poured on. But if, without filtration, you would obtain the flakes of this vegetable lac, pour a large quantity of fair water upon the decoction, after the affuſion of the aluminous ſolution; and you ſhall find the liquor to grow clearer, and the lac to ſettle together at the bottom, or emerge to the top of the water: tho', ſometimes, having not employ'd a ſufficient quantity of fair water, the lac has partly ſubſided, and partly emerged, leaving all the liquor clear in the middle. But, to make this lac fit for uſe, it muſt, by repeated affuſions of freſh water, be dulcify'd from the adhering ſalts, as well as that ſeparated by the filtre; and be ſpread to dry leiſurely upon pieces of cloth, with brown paper, chalk, or bricks under them, to imbibe the moiſture.

Alum, being a ſtony matter diſſolved by acid, may, when uſed as a precipitate, be, itſelf, precipitated.

'Tis ſuppoſed, that the magiſtery of vegetables, obtain'd by this means, conſiſts only of the more ſoluble and coloured parts of the reſpective plant that affords it. But I muſt take the liberty to queſtion the ſuppoſition; for, according to my notion of ſalts, alum, tho' to ſenſe a homogeneous body, ought not to be reckoned among true ſalts, but is to be look'd upon as a kind of magiſtery; ſince, as native vitriol contains both a ſaline ſubſtance, and a metal corroded and aſſociated with it; ſo alum, (which is ſo near allied to vitriol, that, in ſome places of *England*, the ſame ſtone will ſometimes afford both) ſeems manifeſtly to contain a peculiar kind of acid ſpirit generated in the bowels of the earth, and a kind of ſtony matter diſſolved by it. In making our ordinary alum, 'tis true, the workmen uſe the aſhes of a ſea-weed, vulgarly called kelp, and urine; yet I am inform'd, that here in *England*, there is, beſides the factitious alum, another ſort made by nature, without the help of thoſe additions. Now, conſidering this compoſition of alum, and that alkaline ſalts precipitate what acid ones have diſſolved; I could not but ſuſpect that the curdled matter, call'd the magiſtery of vegetables, may have in it a conſiderable proportion of a ſtony ſubſtance, precipitated out of the alum by the lixi-

vium

vium wherein the vegetable had been boiled. And, to ſhew there is no neceſſity that all the curdled ſubſtance muſt belong to the vegetable; I took a ſtrong ſolution of alum, and having filtred it; by pouring in a convenient quantity of a ſtrong ſolution of pot-aſhes, I preſently turn'd the mixture into a white curdled matter; which, committed to the filtre, left a great quantity of a very white ſtony calx, that ſeem'd to be of a mineral nature, as well from other ſigns, as that little bits of it being put upon a live coal, they did neither melt, nor fly away, whilſt that was blown upon; and a quantity of this white ſubſtance, being for a long time kept in a red-hot crucible, was neither diminiſhed nor ſpoiled: hot water too, wherein I kept another parcel of ſuch calx, ſeem'd only to waſh away the looſer ſalts from the ſtony ſubſtance. And I have, by gentle exhalation, recovered from the liquor that paſſed thro' the filtre, and left this calx behind, a body, in appearance like ſalt; for 'twas very white, and conſiſted of innumerable exceeding ſlender ſhining particles, which would, in part, eaſily melt at the flame of a candle, and, in part, fly away with ſome little noiſe. I have likewiſe, with urinous ſalts, ſuch as the ſpirit of ſal-armoniac, as well as with the ſpirit of urine, and even with ſtale urine undiſtilled, eaſily precipitated ſuch a white calx out of a limpid ſolution of alum. Upon the whole, circumſpection is required in judging of the nature of aluminous liquors by precipitation; otherwiſe, we may ſometimes imagine that to be precipitated out of a liquor by alum, which is rather precipitated out of alum by the liquor.

This method of making lacs we have alſo practiſed with madder, which yielded us a red lac; and with rue, which afforded an extract nearly of the ſame colour with that of its leaves. But, becauſe 'tis here, principally, the alkaline ſalt of the pot-aſhes, which enables the water ſo powerfully to extract the tincture of the vegetables; I fear the decoction will not always be of the very ſame colour with the vegetable it is made of. For lixivious ſalts, tho', by penetrating and opening the bodies of vegetables, they prepare and diſpoſe them to part with their tincture readily; yet they not only draw out ſome tinctures, but likewiſe alter them: as will eaſily appear from ſeveral of the experiments already delivered. And tho' alum be of an acid nature, and may, in ſome caſes, deſtroy the adventitious colours produced by the alkali, and reſtore the former; yet we have produced examples, that, in many caſes, an acid will not reſtore a vegetable ſubſtance to the colour deſtroy'd by an alkaline ſalt; but make it aſſume a third, very different from both. Beſides, I have, after this manner, made magiſteries of brazil, of cochineal, and of other things, red, yellow, or green; that appeared, ſome of them, of a rich colour, and others of no bad one: in ſome, however, the colour of the lac ſeem'd rather inferior to that of the plant; and in others again, both very different, and much worſe.

But 'tis time to conclude this ſketch of a hiſtory of colours.

PHYSICS. I ſhall attempt to build no theory * upon the experiments and obſervations here delivered; but leave the ſubject to the proſecution of others. I only make it my requeſt, that the reader would not preſently conclude me miſtaken in giving the matters of fact concerning the changes of colours ſet down, tho' he ſhould not immediately find them exactly agreeable to his own experiments. For, beſides the contingencies to which this kind of tryals is obnoxious, the omiſſion or variation of a ſeemingly inconſiderable circumſtance, may hinder the ſucceſs of an experiment,

* From the diſcoveries of Sir *Iſ. Newton* we may now ſettle the whole theory of colours; which, as it regards natural bodies, is, in ſhort, this.

Firſt, 'tis found by experience, that the rays of light are compoſed of diſſimilar particles; that is, ſome of them are, probably, larger than others: as appears from their different refrangibility.

2. Thoſe particles of light which are moſt refracted, make rays of a violet colour; that is, the moſt minute particles of light, being thus ſeparately collected, probably excite the ſhorteſt vibrations in the *Retina*; which are thence propagated to the brain, along the ſolid *Capillamenta* of the optic nerves, and there excite the ſenſe of a violet colour, the moſt faint and languid of all colours; whilſt the particles which are leaſt refracted, make a red ray; that is, the greateſt particles of light make the longeſt vibrations in the *Retina*; and ſo excite the ſenſation of red, the moſt vivid colour: the other particles, according to their ſeveral intermediate degrees of magnitude and refrangibility, exciting the intermediate colours; almoſt in the ſame manner as the vibrations of the air, by their different magnitudes, cauſe different ſounds.

3. The colours of theſe rays are not adventitious modifications of them, but original, primitive, and neceſſary properties, depending, probably, upon the magnitudes of their parts; and being conſtant and immutable, they are unalterable by any farther refraction, reflexion, or other modification whatever.

4. As light is ſeparated into rays of different colours by the refractions of a glaſs priſm, and other groſs bodies; ſo it is differently ſeparable by very thin plates of any tranſparent matter: for all ſuch plates that have leſs than a determinate thickneſs, ſuffer the rays of all colours to paſs thro' them, and reflect none: but as their thickneſs is increaſed in an arithmetical proportion, they begin to reflect, firſt the blue rays, then the green, yellow, and pure red; next, the blue, green, yellow, and red, more dilute and mixed; till at length, at a certain thickneſs, they reflect all the rays of light intimately united together, as they fall thereon; that is, white. But on whatever parts theſe thin plates reflect any colour, as blue for inſtance, they there tranſmit the contrary colour; as, in this caſe, a red or yellow.

5. Now all natural bodies conſiſt of very thin tranſparent plates, which being ſo laid together, that no reflections or refractions can be made in their interſtices, the body becomes tranſparent: but if the diſtances between them are ſo great, or fill'd with ſuch a matter, or are ſo empty of matter, that many reflexions and refractions are made within the body, this body will be opake. And, therefore, thoſe opake bodies which conſiſt of the thinneſt plates, are black; thoſe which conſiſt either of the thickeſt, or of ſuch as differ greatly in their thickneſs from one another, whence conſequently they become fit to reflect all colours, are white: but ſuch as conſiſt of plates of different intermediate thickneſſes, are blue, green, yellow, or red; becauſe they ſeverally reflect the rays of thoſe colours more copiouſly than the other rays, which they either in great meaſure ſtifle and abſorb, or ſometimes tranſmit. And hence it is, that ſome liquors appear red, or yellow, by reflected light, but blue by tranſmitted light; and that leaf-gold appears yellow by the former, but green by the latter, &c. See *Clark. Annotat. in Rohault.* p. 194, 195. & *Newton. Optic.* p. 320—323, & *alibi paſsim.*

wherein

wherein no other fault has been committed. Thus in dying ſcarlet, tho' you ſhould ſee every ingredient that is uſed about it; tho' I ſhould particularly inform you of the weight of each; and tho' you ſhould be preſent at the kindling of the fire, and at the increaſing and remitting of it, whenever the degree of heat is to be altered; and tho', in a word, you ſhould ſee every thing done ſo particularly, that you would ſcarce harbour the leaſt doubt of your comprehending the whole art: yet if I ſhould not tell you, that the veſſels, which immediately contain the tinging ingredients, are to be made of, or lined with tin; you would never be able, by a due obſervance of all the other circumſtances, to bring the tincture of cochineal to give a perfect ſcarlet.

PHYSICS.

A

FREE INQUIRY

Into the Vulgar

NOTION

OF

NATURE.

SECT. I.

AS the human ſoul is, it ſelf, a true and poſitive Being, 'tis apt to conceive all other things, as ſuch. But this propenſity, I fear, makes us think and ſpeak of chimerical things, and of negations or privations, as of true and poſitive Beings.

We ſhould, therefore, be very careful of being inſenſibly miſ-led by ſuch an innate and unheeded temptation to error, as we bring into the world with us. And, perhaps, among other particulars, in which this deluding propenſity of our minds has too great an influence; it may have impos'd on us, in the notion we uſually frame of nature. Now, this notion being the fruitful parent of others; and being ſo general in its applications, and important in its influence, we ought not over-eaſily to admit it: doubtleſs, it moſt highly deſerves to be warily examin'd, before it be throughly entertain'd.

The vulgar notion of nature, prejudicial to religion and philoſophy.

I have ſometimes ſeriouſly queſtion'd, whether the vulgar notion of nature, has not been both injurious to the glory of God, and a great impediment to the ſolid and uſeful diſcovery of his works.

For,

For, it ſeems to detract from the honour of the great author and governour of the world; that men ſhould aſcribe moſt of the admirable things to be met with in it, not to him, but to a certain nature, which themſelves do not well know what to make of. 'Tis true, many confeſs that this nature is a thing of his eſtabliſhing, and ſubordinate to him; but, tho' they own it, when they are ask'd the queſtion, yet there are ſeveral, who ſeldom or never regarded any higher cauſe. And whoever takes notice of their way of aſcribing things to nature, may eaſily diſcern, that, whatever their words ſometimes are, the agency of God is little in their thoughts. Doubtleſs, it ſhews the wiſdom of God, to have ſo fram'd things at firſt, that there can ſeldom or never need any extraordinary interpoſition of his power; or the employing, from time to time, an intelligent overſeer, to regulate, aſſiſt, and control the motions of matter.

Ariſtotle, by introducing the notion of the eternity of the world, in moſt men's opinion, at leaſt, openly deny'd to God the production of it; and by aſcribing the admirable works of the divine Being, to what he calls nature, tacitly denies him the government thereof.

But my opinion hinders me not at all from acknowledging God to be the author of the univerſe, and the continual preſerver and upholder of it (which is much more than the peripatetic hypotheſis allows) for thoſe things which the ſchool-philoſophers aſcribe to the agency of nature, interpoſing upon emergencies, I aſcribe to the wiſdom of God, in the firſt fabric of the univerſe; which he ſo admirably contriv'd, that, if he but continue his ordinary and general concourſe, there will be no neceſſity of extraordinary interpoſitions: ſo that mere matter, particularly determin'd, ſhall, in certain conjunctures of circumſtances, do all that philoſophers aſcribe on ſuch occaſions, to nature.

This notion is more reſpectful to a divine Being, than to imagine, as we commonly do, that he has appointed an intelligent and powerful agent, as his vice-gerent, continually to watch for the good of the univerſe in general, and of the particular bodies that compoſe it; whilſt this Being appears not to have the skill, or the power, to prevent ſuch irregularities as often prove deſtructive to multitudes of animals, and other noble creatures, as in plagues, *&c.* and ſometimes prejudicial to greater portions of the univerſe, as in earth-quakes, *&c.*

But farther, whilſt men indulge themſelves ſo general and eaſy a way of ſolving difficulties, as to attribute them to nature; ſhame will not reduce them to a more induſtrious ſearch after the reaſons of things; nor curioſity it ſelf greatly move them to it. Thus the cauſe of the aſcent of water in pumps, and other phenomena of that kind, had never been known, if the moderns had acquieſced in that imaginary one, that the world was govern'd by a watchful Being, call'd nature, who abhors a *Vacuum*; and conſequently is always ready to do whatever is neceſſary to prevent it.

And the veneration men commonly have for what they call nature, has obſtructed and confined the empire of man over the inferiour creatures. For many have look'd upon it as impoſſible to compaſs, and

PHYSICS. others, as impious to attempt, the removing thoſe boundaries which nature ſeems to have ſettled among her productions. And whilſt they look upon her as ſuch a venerable thing, ſome make a ſcruple of conſcience to endeavour ſo to imitate any of her works, as to excel them.

'Twill here, I doubt not, be ſaid, that I contradict the ſenſe of the generality of mankind: I anſwer, that in philoſophical inquiries, we are not ſo ſollicitous about what has been, or is believ'd, as what ought to be; and certainly the ſenſe of the generality of men, ought little to ſway us in ſome particularquestions. But 'tis no wonder men ſhould be generally prepoſſeſs'd with ſuch a notion of nature, as I call in queſtion; ſince education has imbued them with it from their infancy: and even in their maturer years, they find it taken for granted, and employed by the moſt learned writers, and never hear it call'd in queſtion. Beſides, it exceedingly complies with our innate propenſity, to think that we know more than we do: to vouch nature for a cauſe, is an expedient, that can ſcarce be wanting upon any occaſion, to be produced as a reaſon for any thing we are ignorant of.

But to talk of a thing as a real and poſitive Being, and attribute great matters to it, weighs but little with me, when I conſider, that, tho' fortune be only a certain looſe and undetermin'd notion, which a modern metaphyſician would refer to the claſs of non-entities; yet not only the *Gentiles* made it a goddeſs, whom many of them ſeriouſly worſhipp'd, but eminent writers, both heathen and chriſtian, ancient and modern, and all ſorts of men, in their common diſcourſe, ſeriouſly talk of it, as of a kind of antichriſt, that uſurped a great ſhare in the government of the world; and aſcribe little leſs to it, than to nature. And to paſs over what poets, moraliſts, and divines tell us of the powers of ignorance and vice, which are but moral defects; the generality of mankind ſeriouſly attribute a great and fatal dominion to death; which, tho' ſaid to do ſo many and ſuch wonderful things, is neither a ſubſtance, nor a poſitive entity, but a mere privation. As for revelation, it countenances no ſuch notion as that vulgarly receiv'd of nature: the word is not once mention'd in ſcripture; no, not by *Moſes*, in his account of the creation. And, indeed, till the *Iſraelites* were overrun, and corrupted by idolatrous nations, there was, for many ages, a deep ſilence as to ſuch a Being.

There are two things whereof I muſt advertiſe the reader, before I proceed farther.

1. That when, in the preſent diſcourſe, I ſpeak of the opinions of *Ariſtotle*, I mean thoſe that are by the generality of ſcholars taken for his, or the *Ariſtotelian* and *Peripatetic* doctrines; by which, if he be miſ-repreſented, the blame ought to light upon his commentators, and followers.

2. That I here ſet aſide the conſideration of the rational ſoul or mind of man, all other parts of the univerſe being, according to the receiv'd opinion, the works of nature.

A

PHYSICS.

The great ambiguity of the word nature.

A conſidering perſon may well ſuſpect, that men have generally had but imperfect and confuſed notions concerning nature; ſince they apply that name to ſeveral things, and thoſe too ſuch, as have, ſome of them, very little dependance on, or connexion with the others. *Ariſtotle* has a whole chapter expreſly written to enumerate the various acceptations of the *Greek* word Φύσις, commonly render'd nature; of which, he there mentions ſix; and, in *Engliſh*, we have more ſignifications of that term. Sometimes, we uſe it,

1. For that author of nature, whom the ſchool-men call *Natural naturans*; as when 'tis ſaid, that nature hath made man partly corporeal, and partly immaterial.

2. Sometimes we mean by the nature of a thing, the eſſence, or that which the ſchool-men call the quiddity of a thing; that is, the attribute which makes it what it is, whether the thing be corporeal, or not; as when we attempt to define the nature of an angel, a triangle, or a fluid.

3. Sometimes we confound that which a man has by nature, with what accrues to him by birth; as, when we ſay, that ſuch a man is noble by nature.

4. Sometimes we take nature for an internal principle of motion; as when we ſay, that a ſtone let fall in the air, is, by nature, carried towards the centre of the earth; and, on the contrary, that fire, or flame, naturally moves upwards.

5. Sometimes we underſtand by nature, the eſtabliſh'd courſe of things; as when we ſay, that nature makes the night ſucceed the day; or that nature hath made reſpiration neceſſary to the life of men.

6. Sometimes we take nature for an aggregate of powers belonging to a body, eſpecially a living one; as, when phyſicians ſay, that nature is ſtrong, weak, or ſpent; or that in ſuch or ſuch diſeaſes, nature left to herſelf, will perform the cure.

7. Sometimes we take nature for the univerſe, or ſyſtem of the corporeal works of God; as when 'tis ſaid of a phenix, or a chimera, that there is no ſuch thing in nature.

8. Sometimes, too, and that moſt commonly, we would expreſs by the word nature, a ſemi-deity, or other ſtrange kind of Being; which is the notion we here examine.

And, beſides theſe more abſolute acceptations of the word nature, it has ſeveral others more relative. Nature is ſet in oppoſition, or contradiſtinction to other things; as when a ſtone falls downwards, we ſay, it does it by a natural motion; but that if it be thrown upwards, its motion that way is violent. So chymiſts diſtinguiſh vitriol into natural and factitious. In like manner 'tis ſaid, that water kept ſuſpended in a ſucking-pump, is not in its natural place, as that is, which ſtagnates in the well. We ſay, alſo, that wicked men are ſtill in the ſtate of nature; but the regenerate, in a ſtate of grace: that cures wrought by medicines, are natural operations; but the miraculous ones, wrought by Chriſt and his apoſtles, ſupernatural. Nor are theſe the only forms of ſpeech that might be alledg'd, to manifeſt the ambiguity of the word nature; tho' ſome of theſe already mention'd, PHYSICS. mention'd, ſhould be judged ſo near as to be co-incident. Among *Latin* writers, the acceptations of the word nature are ſo many, that I remember, one author reckons up no leſs than fourteen or fifteen. Hence we ſee how eaſy 'tis for the generality of men, without excepting thoſe who write of natural things, to impoſe upon others and themſelves, in the uſe of a word ſo apt to be miſ-employ'd.

I have often look'd upon it as an unhappy thing, and prejudicial both to philoſophy and phyſic, that the word nature hath been ſo frequently, and yet ſo unskilfully employ'd, by all ſorts of men. For the very great ambiguity of this term, and the promiſcuous uſe made of it, without ſufficiently attending to its different ſignifications, render many of the expreſſions wherein 'tis employ'd, either unintelligible, improper, or falſe. I, therefore, heartily wiſh, that philoſophers, and other leading men, would, by common conſent, introduce ſome more ſignificant, and leſs ambiguous terms and expreſſions, in the room of the licentious word nature; and the forms of ſpeech that depend on it: or, at leaſt, decline the uſe of it, as much as conveniently they can; and where they think they muſt employ it, declare in what clear and determinate ſenſe they uſe it. For unleſs ſomewhat of this kind be done, men will very hardly avoid being led into miſtakes, both of things, and of one another; whence ſuch wranglings about words and names will be ſtill kept on foot, as are uſually managed with much heat, and little advantage.

But 'tis far more difficult than any one, who hath not try'd, would imagine, to diſcourſe long of the corporeal works of God, and eſpecially of the operations and phenomena attributed to nature, and decline the frequent uſe of that term, or forms of ſpeech whereof 'tis a principal part; without frequent, and tedious circumlocutions. But to avoid, as much as poſſible, this inconvenience; I ſhall hereafter call thoſe who maintain the vulgar notion of nature *Naturiſts*: an appellation I rather chuſe than that of *Naturaliſts*, becauſe many, even of the learned among them, are not philoſophers. This inconvenience might perhaps be a little farther remedied, if,

Means of avoiding this ambiguity.

1. Inſtead of the word nature, taken for *Natura naturans*, we uſe the term God, which 'tis put to ſignify.

2. If, inſtead of nature, uſed for that which makes a thing what it is, we employ the word eſſence; ſometimes alſo we may make uſe of the word quiddity; which, tho' a barbarous term, is yet frequently employ'd, and well underſtood in the ſchools; and tho' very comprehenſive, is free from ambiguity.

3. If what is meant by the word nature, taken for what belongs to a living creature at its nativity, or accrues to it by its birth, be expreſſed, ſometimes, by ſaying, that an animal is born ſo; and, ſometimes, by ſaying, that a thing has been generated ſuch; and, ſometimes alſo, that 'tis thus or thus qualified by its original temper and conſtitution.

4. If, inſtead of the word nature, taken for an internal principle of local motion; we ſay, ſometimes, that this or that body moves as it were, or ſeems to move ſpontaneouſly, upwards, downwards, &c. or that it is put

put into this or that motion, or determined to this or that action, by the concourse of particular causes.

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5. If, instead of nature, used for the established course of things corporeal, we substitute, what it denotes, the established order, or the settled course of things.

6. If, instead of nature, taken for an aggregate of the powers belonging to a body, we employ the constitution, temper, mechanism, or the complex of the essential properties or qualities; and, sometimes, the condition, the structure, or the texture of that body: and, speaking of the greater portions of the world, we may use the terms, fabric of the world, system of the universe, *&c.*

7. If, where men employ the word nature for the universe, or the system of the corporeal works of God; we use the word world, or universe; and, instead of the phenomena of nature, substitute, the phenomena of the universe, or of the world.

8. And, lastly, if, instead of using the word nature, taken for either a goddess, or a kind of semi-deity; we wholly reject, or very seldom employ it.

Whether the nature of a thing be the law it receives from the creator.

Some, I know, will have the nature of every thing, to be only the law that it receives from the creator, and according to which it acts on all occasions. And, indeed, this opinion, tho' neither clear nor comprehensive, seems capable of a fair construction. There is often some resemblance between the orderly and regular motions of inanimate bodies, and the actions of agents, that proceed conformably to laws. And I, sometimes, scruple not to speak of the laws of motion and rest, that God has established among things corporeal, and, now and then, to call them the laws of nature.

But, in strictness, to say, that the nature of this or that body, is but the law of God prescribed to it, is an improper, and figurative expression. For this gives us but a very defective idea of nature, since it omits the general fabric of the world, and the contrivances of particular bodies; which yet, are as necessary as local motion itself, to the production of particular effects and phenomena: and, to speak properly, a law being but a notional rule of acting according to the declared will of a superior, 'tis plain, that nothing but an intellectual Being can be properly capable of receiving and acting by a law. For if it does not understand, it cannot know what the will of the legislator is; have any intention to accomplish it; or act with regard thereto. Now 'tis intelligible, that God should, at the beginning, impress determinate motions upon the parts of matter, and guide them, as he thought requisite, for the primordial constitution of things: and that ever since he should, by his ordinary and general concourse, maintain those powers, which he gave the parts of matter to transmit their motion to one another. But I cannot conceive, how a body, destitute of understanding and sense, truly so called, can moderate and determine its own motions; especially so, as to make them conformable to laws, that it has no knowledge of. And that inanimate

bodies,

PHYSICS. bodies, how ſtrictly ſoever called natural, properly act by laws, cannot be proved by their acting ſometimes regularly, and, as men think, in order to determinate ends: ſince, in artificial things, we ſee many motions very orderly perform'd, and with a manifeſt tendency to particular and deſign'd ends. Thus, in a watch, the motions of the ſpring, wheels, and other parts, are ſo fitted and regulated, that the hand upon the dial-plate moves with great uniformity, and ſeems to moderate its motion, ſo as not to arrive at the points that denote the time of the day, either a minute ſooner, or a minute later than it ſhould do. And when a man ſhoots an arrow at a mark, ſo as to hit it; tho' the arrow moves towards the mark, as it would, if it could, and did deſign to ſtrike it; yet none will ſay, that this arrow moves by a law, but by an external impulſe.

Ariſtotle's *definition of nature obſcure and unſatisfactory.*

But, poſſibly, the definition of a philoſopher may exempt us from the perplexities, to which, by the ambiguous expreſſions of common writers, we are expoſed. I therefore conſidered, with more than ordinary attention, the famous definition of nature, that is left us by *Ariſtotle*; which I ſhall recite rather in *Latin* than *Engliſh*, becauſe 'tis very familiarly known, among ſcholars in that language; and becauſe there is ſomewhat in it, that ſeems difficult to be, without circumlocution, render'd intelligibly in *Engliſh*: *Natura* (ſays he) *eſt principium & cauſa motûs & quietis ejus, in quo ineſt, primò per ſe, & non ſecundum accidens.* Now as, according to *Ariſtotle*, the whole world is but a ſyſtem of the works of nature; we might well expect, that the definition of a thing, the moſt important in natural philoſophy, ſhould be clearly and accurately deliver'd; yet, to me, this celebrated definition ſeems ſo dark, that I receiv'd no aſſiſtance from it, towards framing a clear and ſatisfactory notion of nature. For I dare not hope, that what, as to me, is not itſelf intelligible, ſhould make me underſtand what is to be explained by it. And, conſulting ſome of *Ariſtotle*'s interpreters upon the ſenſe of this definition, I found the more conſiderate of them ſo puzzled with it, that their diſcourſes about it ſeem'd to tend, rather to free the definer from tautology and ſelf-contradiction, than to manifeſt the definition itſelf to be good and inſtructive. And indeed, tho' the immoderate veneration they have for their maſter, engages them to make the beſt they can of his definition, even when they cannot juſtify it without ſtrain'd interpretations; yet what every one ſeems to defend in groſs, almoſt every one of them cenſures in parcels; this man attacking one part of the definition, and that another, with objections ſo weighty, that if I had no other arguments to urge againſt it, I might borrow enow from the commentators on it, to juſtify my diſlike thereof.

Several things are commonly received as belonging to the idea of nature, that are not manifeſtly or at all comprehended in this *Ariſtotelian* definition, which never declares whether the principle, or cauſe here mentioned, is a ſubſtance, or an accident; and if a ſubſtance, whether corporeal or immaterial: nor is it clearly contain'd in this definition, that nature does all things moſt wiſely, and ſtill acts by the ſhorteſt ways, without ever miſſing of her end; and that ſhe watches againſt a *Vacuum*, for the welfare of the univerſe.

SECT.

SECT. II.

THE best way to discover the common opinion of nature, is, to consider what axioms pass for current about her, what titles and epithets are unanimously given her by philosophers, other writers, and by the generality of men who have occasion to discourse of her, and her actions. *The receiv'd notion of nature; what?*

Of such axioms and epithets, the principal seem to be these.

Nature is exceeding wise, and all her works are perform'd with understanding. Nature does nothing in vain. Nature never fails of her purpose. Nature always does what is best. Nature always acts in the shortest manner. Nature is neither too lavish, nor too sparing in necessary things. Nature always preserves itself. Nature cures diseases. Nature always watches to preserve the universe. Nature dreads a Vacuum.

From which particulars put together, it appears, that the vulgar notion of nature may be expressed by some such description as this.

Nature is a most wise being, that acts nothing in vain, never misses of her ends, but always does what is best, and that by the most direct way, neither employing any things superfluous, nor being wanting in things necessary; teaching and inclining every one of her works to preserve itself: and as, in the human frame, she cures diseases; so, in the world, for the conservation of the universe, she abhors a *Vacuum*; making particular bodies act contrary to their own inclinations and interests, to prevent it.

If I were to propose a notion, as less unfit than any I have met with, to pass for the principal one of nature; with regard to which, many axioms and expressions, relating to that word, may be conveniently understood; I should first distinguish between the universal, and the particular nature of things. And of universal nature, the notion I would offer, should be something like this. *Nature is the aggregate of the bodies, that make up the world, in its present state, considered as a principle, by virtue whereof, they act and suffer, according to the laws of motion, prescribed by the author of things.* And this makes way for the other subordinate notion; since the particular nature of an individual consists in the general nature, apply'd to a distinct portion of the universe; or, supposing that placed as it is, in a world, framed by God, like ours, it must be a convention of the mechanical properties, (such as magnitude, figure, order, situation, and local motion) of parts, convenient and sufficient to constitute of, or entitle to its particular species, or denominations, the particular body they make up; the concourse of all these being considered as the principle of motion, rest, and changes in that body. *A new notion of nature, general and particular, advanc'd.*

The vulgar notion of nature, as it has had, doubtless, may have an ill effect on religion. The looking upon merely corporeal, and often inanimate things, as endow'd with life, sense, and understanding; and ascribing to nature, *Ill effects of the vulgar notion of nature upon religion.*

PHYSICS. and ſome other Beings, things that belong to God alone, have been grand cauſes of the polytheiſm and idolatry of the *Gentiles*.

The moſt ancient idolatry, probably, was the worſhip of the celeſtial lights, eſpecially the ſun and moon; as appears both from ſacred and profane hiſtory. The great *Hippocrates* attempts to account for the origin of the world from his immortal heat, which he eſteems as a deity. And *Galen* himſelf, who was not unacquainted with *Moſes*'s writings, and with chriſtianity, fancy'd the earth had a certain ſoul or mind imparted to it by the ſuperior bodies. And an inquiſitive perſon, who, having liv'd many years in *China*, and ſeveral of the neighbouring kingdoms, aſſured me, that, in a ſolemn conference he had with ſome of the more eminent doctors of the *Chineſe* religion; they frankly profeſſed, they believed the heavenly bodies to be truly divine, and deſerving to be worſhipped, becauſe they imparted to men ſuch good things, as light, heat, rain, *&c.* And this belief, they declared, they thought more rational than that of the *Europeans*, who worſhip a deity, whoſe ſhape, colour, and motion, and whoſe efficacy on ſublunary things, were inviſible.

Many of the ancient philoſophers held the world to be animated, and believed, that the mundane ſoul was not barely a living, but a moſt intelligent and wiſe active Being; the *Stoics* held the world to be an animal; and the notion of the ſoul of the world, and of nature, ſeem ſo near allied, that ſome of the old ſages appear to have confounded them, and to have made no other univerſal nature, than the ſoul of the world. However, the great and pernicious errors they were led into, by believing that the univerſe itſelf, and many of its nobler parts, beſides men, were endowed with life, underſtanding, and providence; may make us chriſtians jealous of admitting ſuch a Being, as that which men venerate under the name of nature: ſince they aſcribe to it as many wonderful powers and prerogatives, as the idolaters did to their adored mundane ſoul. They, alſo, ſacrilegiouſly abuſed this Being, as well under the very name of nature, as under that of the ſoul of the world: ſometimes making it the ſame with the world, at others the ſame with God; and *Orpheus* has a hymn addreſs'd to nature as a goddeſs.

Ariſtotle, indeed, and his commentators, do not ſo directly idolize nature, as did *Orpheus*; yet, I doubt, they go further than they can juſtify, when they ſo freely and often aſſert many extraordinary things of her, particularly when they call the works of God, the works of nature, and mention him and her together, not as a creator and a creature, but as two co-ordinate governors.

Thoſe who thought the ſun endowed, not only with a living ſoul, but with underſtanding and a will, muſt, if they had duly conſider'd, have been much more puzzled, to find food for ſo vaſt a body, and organs in him neceſſary to prepare and digeſt it, and to perform the other functions that belong to animal nutrition, than the philoſophers who maintain'd him to be fire. 'Tis without proof preſumed and aſſerted, that the celeſtial bodies are endow'd with underſtanding and prudence, eſpecially, ſo as to be able to know the particular con-

conditions and tranſactions of men, and hear and grant the prayers of their worſhippers. And the moon, which was anciently a principal deity, is ſo rude and mountainous a body, that 'tis a wonder ſpeculative men, who conſider'd how many, how various, and how noble functions belong to a ſenſitive ſoul, could think a maſs of matter, ſo very remote from being fitly organiz'd, ſhould be animated and govern'd by a true, living and ſenſitive ſoul. Indeed, theſe deifiers of the celeſtial globes, and the heathen diſciples of *Ariſtotle*, beſides ſeveral of the ſame mind, among the chriſtians, ſay great and lofty things of the quinteſſential nature of the heavenly bodies, and their conſequent incorruptibility; of the regularity of their motions, and of their divine quality of light, that makes them refulgent. But the perſuaſion they had, of this quinteſſential nature of the ſuperior part of the world, ſeems not grounded upon any ſolid phyſical reaſon, but entertain'd by them for being agreeable to the opinion they had of the divinity of the celeſtial bodies; of which, *Ariſtotle* himſelf ſpeaks in a way that hath greatly contributed to ſuch an exceſſive veneration for thoſe bodies, as is neither agreeable to true philoſophy, nor true religion. He himſelf takes notice, that the *Pythagoreans* held our earth to be one of the planets; and that it moved about the ſun, which they placed in the middle of the world. And ſince this hypotheſis of the earth's motion, was in the laſt age reviv'd by *Copernicus*, not only *Kepler*, *Galileo*, and *Gaſſendus*, but moſt of the eminent modern aſtronomers, have embraced it: which, indeed, is far more agreeable to the phenomena, than the doctrine of *Ariſtotle* (who was plainly miſtaken about the order and conſiſtence of the heavens) and the ancient and generally received *Ptolemaic* ſyſtem. Now, ſuppoſing the terraqueous globe to be a planet; who can conſider it as a round maſs of very heterogeneous ſubſtances, whoſe ſurface is very rude and uneven, and its body opake, (unleſs as it happens to be enlightned by the ſun, moon, and ſtars) and ſo very inorganical and unfit for ſo much as nutrition, that it ſeems wholly unfit to be an animal, much leſs a rational one; and aſcribe underſtanding and providence to it? The like may be ſaid of the celeſtial bodies.

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As for the boaſted immutability of the heavenly bodies; it may be very probably call'd in queſtion from the phenomena of ſome of the comets, that by their parallax were found to be above the moon, and conſequently in the celeſtial region of the world. But, the incorruptibility and immutability of the heavenly bodies are more diſproveable by the ſudden and irregular generation, changes, and deſtruction of the ſpots of the ſun; which ſometimes ſo ſuddenly diſappear, that in the year 1660, on *May* 8. having left, in the morning, a ſpot, whoſe motions we had long obſerv'd thro' an excellent teleſcope, with an expectation that it would continue for many days viſible to us; we were ſurpriz'd to find, that when we came to obſerve it again in the evening, it was quite diſſipated: and by comparing it to the ſun, we eſtimated the extent of its ſurface to be equal to that of all *Europe*. As to the conſtancy of the motions of the ſtars; if the earth, which we know to be inanimate, is a planet, it moves as conſtantly and

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And laſtly, as a great proof of the divinity of the ſtars is drawn from their light; ſo, tho' I grant it to be the nobleſt of ſenſible qualities, yet I cannot think it a good proof of the divine nature of bodies endow'd with it. For tho' the *Zabians* and *Chaldeans* conſidered and adored the planets as the chief gods, our teleſcopes diſcover to us, that they ſhine but by a borrow'd light; ſo that *Venus*, as vivid as it appears to the naked eye, is ſometimes horn'd like the moon. Thus alſo the earth, whether a planet or no, is enlighten'd by the ſun; and poſſibly, as a body forty times bigger, communicates more light to the moon, than it receives from her; as ſeems probable from the light on the ſurface of the moon in ſome of her eclipſes. And tho' in the night, when the darkneſs hath widened the *Pupillæ* of our eyes, and the moon ſhines with an unrival'd luſtre, ſhe ſeems exceeding bright; yet ſhe may be, for ought I know, more opake than the ſolid part of the terreſtrial globe. For I have more than once obſerv'd a ſmall cloud in the weſt, where the moon then was, about ſun-ſet; and comparing them together, the cloud reflected the light as ſtrongly to my eye, as did the moon, that ſeem'd to be not far from it; both of them appearing like little whitiſh clouds: tho' afterwards, as the ſun deſcended lower beneath the horizon, the moon grew more luminous. And, ſpeaking of light indefinitely, 'tis ſo far from arguing a divine nature in the bodies endow'd with it, (whether, as the planets, by participation from an external illuminant; or, as the ſun, from an internal principle) that a calcined ſtone, witneſs that of *Bolonia*, will afford, in proportion to its bulk, incomparably more borrow'd light than one of the planets. And a light, from internal conſtitution, may be found, not only in ſuch abject creatures as inſects, whether winged, as the *Cucupias* of *Hiſpaniola*, or creeping, as the glow-worm; but alſo in bodies inanimate, and corrupted; as in rotten wood, putrefy'd fiſh, *&c.*

Reaſons againſt admitting the vulgar notion of nature.

The reaſons that have made me backward to entertain ſuch a notion of nature, as I have hitherto diſcourſed of, may be comprized under the following.

1. Such a nature ſeems to be either aſſerted, or aſſumed, without ſufficient proof; and this ſingle reaſon, if well made out, is alone ſufficient to diſcredit it. For, in matters of philoſophy, where we ought not to take any thing upon truſt, or allow it without proof; 'tis enough to keep us from believing a thing, that we have no poſitive argument to induce us to aſſent to it; tho' we have no particular arguments againſt it. Now, I have yet met with no phyſical arguments, either demonſtrative, or conſiderably probable, to evince the exiſtence of the nature we examine; yet, ſurely, ſo univerſal an agent, ſuppoſed to act immediately, in abundance

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of phenomena, ſhould, if it really exiſted, give ſome manifeſt proofs of itſelf.

2. Such a nature is unneceſſary. For, ſince a great part of the work of true philoſophers has been to reduce the principles of things to the ſmalleſt number they can, without making them inſufficient; why ſhould we take in a principle, of which we have no need? Suppoſing the common matter of all bodies to have been at firſt divided into innumerable minute parts, by the wiſe author of things, and theſe parts to have been ſo diſpos'd, as to form the world as it now is; and, ſuppoſing the univerſal laws of motion, among the parts of matter, to have been eſtabliſh'd, and ſeveral conventions of particles contrived into the ſeminal principles of various things; all which may be effected by the mere motion of matter, skilfully guided at the beginning of the world: ſuppoſing all this, together with God's ordinary and general concourſe, which we very reaſonably may; I ſee not, why the ſame phenomena, that we now obſerve in the world, ſhould not be produc'd, without taking in any ſuch powerful and intelligent being, diſtinct from God, as nature is repreſented to be. And, 'till ſome inſtance is produc'd to the contrary, I ſhall think that the phenomena we obſerve, will genuinely follow from the mere fabric and conſtitution of the world. Thus, ſuppoſing the ſun and moon to have been put, at firſt, into ſuch motions about the earth, as experience ſhews they have; the determinate celerity of theſe motions, and the lines wherein they are performed, will make it neceſſary, that the moon ſhould exhibit ſuch ſeveral phaſes as every month ſhe doth; and that at ſome times ſhe and the ſun ſhould have a trine, or a quartile aſpect, *&c.* and that now one, and now the other of them, ſhould, at ſet times, ſuffer an eclipſe: tho' theſe eclipſes were by the *Romans*, and others of old, and are by many unlearn'd nations at this day, look'd upon as ſupernatural things. And, indeed, the difficulty we find to conceive how ſo great a fabric as the world, can be preſerved in order, and kept from falling again to a chaos, ſeems to ariſe from hence, that men do not ſufficiently conſider the unſearchable wiſdom of the divine architect, whoſe piercing eyes were able to look at once quite thro' the univerſe, and take into his view, both the beginning and end of time: ſo that, perfectly knowing what would be the conſequences of all the poſſible conjunctures of circumſtances, into which matter, divided and mov'd according to particular laws, could, in an engine ſo conſtituted as the preſent world is, happen to be put; there can nothing fall out, unleſs when a miracle is wrought, that ſhall be able to alter the courſe of things, or prejudice the conſtitution of them, any further, than he from the beginning foreſaw, and allow'd.

3. The nature I queſtion, is ſo dark and odd a thing, that 'tis hard to know what to make of it; being ſcarce intelligibly propos'd by thoſe who lay the moſt ſtreſs upon it. For, it appears not clearly, whether they will have it to be a corporeal, or an immaterial ſubſtance; or ſome ſuch thing, as may ſeem to be betwixt both; ſuch as many *Peripatetics* repreſent ſubſtantial forms, and what they call real qualities, which ſeveral ſchoolmen

men hold to be ſeparable from all matter whatſoever. If it be merely corporeal, I confeſs, I underſtand not, how it can be ſo wiſe an agent, as they would have it paſs for: if it be a body, I would gladly know what kind of body it is; and how, ſince, among bodies, there can be no penetration of dimenſions, this ſo intimately pervades, as they pretend, all the other bodies of the world? If it be ſaid, that nature is a *Semi-ſubſtantia*, as ſome modern ſchool-men call ſubſtantial forms, and real qualities; I anſwer, that I acknowledge no ſuch chymerical, and unintelligible Beings. It remains therefore, that this nature we ſpeak of, if poſitive, is an immaterial ſubſtance. But, to have recourſe to ſuch an one, as a phyſical agent, and the grand author of the motion of bodies; and that, eſpecially, in ſuch familiar phenomena, as the aſcent of water in pumps, the ſuſpenſion of it in watering-pots, the running of it thro' ſyphons, *&c.* and thence to aſſign the cauſe hereof, will, I think, prove exceeding difficult.

4. Since many naturaliſts are chriſtians, it may not be improper to add, that another thing, for which I diſlike the vulgar notion of nature, is, its being dangerous to religion, in general, and, conſequently, to the chriſtian.

For this erroneous conceit defrauds the true God of ſeveral acts of veneration and gratitude, that are due to him from men, upon account of the viſible world; and diverts them to that imaginary being they call nature, which has no title to them: for, whilſt nature is ſuppos'd to be an intelligent thing, that wiſely and kindly adminiſters all that is done among bodies, 'tis no wonder that the generality of men ſhould admire and praiſe her, for the wonderful, and uſeful things they obſerve in the world. And, in effect, tho' nature, in our preſent ſenſe of the word, be never found in the ſacred writings; yet, nothing is more frequently met with in the books of philoſophers, than nature and her effects. And, whatever has been ſaid by ſome, in excuſe of *Ariſtotle*, yet the generality of the *Peripatetics*, from whom the vulgar notion of nature is chiefly received, make the world eternal; and refer all the tranſactions among the bodies it contains, to what they call nature. Whence 'twill not be difficult to perceive, that, if they do not quite exclude God; yet, as they leave him no intereſt in the firſt formation of the univerſe, ſo they leave him but very little in the adminiſtration of the parts it conſiſts of, eſpecially in ſuch as are ſublunary. Inſtead, therefore, of the true God, they have ſubſtituted, for us, a kind of a goddeſs, with the title of nature; upon whom they look as the immediate agent and director in all excellent productions, and aſcribe to her the praiſe and glory of them.

This great error, in a point of ſuch importance, ſeems to undermine the foundation of religion. For ſince the moſt general and effectual argument which has perſuaded men that there is a God and a providence, is afforded by the conſideration of the viſible world, wherein ſo many operations and other things are obſerv'd to be managed with ſuch conduct and benignity, as cannot juſtly be aſcribed but to the wiſdom and goodneſs of a deity; they who aſcribe theſe things to mere nature, much weaken the force of of that argument, if they do not quite take away the neceſſity of acknowledging a deity; by ſhewing, that, without having recourſe to him, an account may be given of the adminiſtration of the world, and of what is perform'd among things corporeal. Indeed, when men are put upon conſidering the matter, and preſs'd to declare themſelves more clearly; they fear to affirm, that God and nature are the ſame thing, and will confeſs, that ſhe is but his vicegerent; yet, in practice, their admiration, and their praiſes, are frequently given to nature, not to God.

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In ſhort, as nature is ſo frequently recurr'd to, and ſo magnify'd in the writings of philoſophers, that the exceſſive veneration men have for her, cauſed ſome (as the *Epicureans*) to deny God; ſo, 'tis to be fear'd, that it makes many forget him. And, perhaps, a ſuſpicious perſon would add, that, were it not for other principles, this erroneous idea of nature would, too often, have a ſtrong tendency to ſhake, if not to ſubvert, the very foundations of all religion; by miſleading thoſe who are inclined to be its enemies, from overlooking the neceſſity of a God, to the denial of his exiſtence.

5. There are many phenomena, which do not agree with that notion, or repreſentation of nature, I call into queſtion. For, if there were ſuch an intelligent, powerful, and vigilant being, as philoſophers deſcribe nature to be, ſeveral things would not be done, which experience aſſures us are done.

The great vigilance of nature, for the common good of the univerſe, is pretended to be demonſtrated from the care ſhe takes to prevent, or repleniſh a *Vacuum*; which would be very prejudicial to the fabric of the world. 'Tis alledged, that nature, in pumps, and the like caſes, raiſes the heavy body of water, in ſpight of its tendency towards the centre of the earth, to obviate, or fill up a vacuity; and that out of a gardener's watering-pot, or an inverted tube, ſtopped at one end, neither water, nor quick-ſilver will fall down, leſt it ſhould leave a *Vacuum* behind it: but I demand how it comes to paſs, that if a glaſs-tube be but a foot longer than 34 or 35 feet; or an inverted tube, fill'd with quick-ſilver, but a finger's breadth longer than 30 inches, the water in the one, and the quick-ſilver in the other, will ſubſide? Is it poſſible, that nature, which, in pumps, is ſaid to raiſe up every day ſo many hundred ton of water; and, if we believe the ſchools, would raiſe it to any height; ſhould not have the diſcretion, or the power, to lift up, or ſuſtain, as much water as would ſerve to fill one foot in a glaſs tube, or as much quick-ſilver as an inch of a ſlender pipe will contain; to obviate, or repleniſh, the *Vacuum*, ſhe is ſaid to abhor? Sure, at this rate, ſhe muſt either have very little power, or very little knowledge of the power ſhe has. So, likewiſe, when a glaſs-bubble is blown very thin at the flame of a lamp, and hermetically ſeal'd whilſt 'tis very hot, the reaſon aſſign'd for its being apt to break, when it grows cold, is, that the inward air, which was before rarify'd by the heat, coming to be condenſed by the cold, leſt the ſpace deſerted by the air, that thus contracts itſelf, ſhould be

PHYSICS. be left void, nature, with violence, breaks the glaſs: but if the glaſs be blown a little ſtronger than ordinary, tho' at the flame of a lamp, the bubble, as I have often try'd, will continue unbroken, in ſpight of nature's abhorrence of a *Vacuum.* But if, tho' nature did not hinder the water, or the quick-ſilver, from falling down, there would no ſuch *Vacuum* enſue as ſhe is ſaid to abhor; why does ſhe ſeem ſo ſollicitous to hinder it; why keep three or four and thirty feet of water, in perpendicular height, contrary to the nature of all heavy bodies, ſuſpended in the tube; and why ſo furiouſly break to pieces a thin ſeal'd bubble, to hinder a *Vacuum*, if, in caſe ſhe did not break it, no *Vacuum* would enſue? On the other ſide, if we admit her endeavours to hinder a *Vacuum*, not to have been ſuperfluous, we muſt confeſs, that, where theſe endeavours ſucceed not, there is really produced ſuch a *Vacuum*, as ſhe is ſaid to abhor: ſo that, either ſhe muſt be very indiſcreet to trouble herſelf, and tranſgreſs her own ordinary laws, to prevent a danger ſhe need not fear; or elſe her ſtrength muſt be very ſmall, that is not able to fill a ſmall vacuity or to break a brittle glaſs-bubble.

Another grand inſtance given of the wiſdom of nature, and her watchfulneſs for the good of the whole world, is, the appetite ſhe has implanted in all heavy bodies, to deſcend to the centre of the earth, and in all light ones, to aſcend. But, for poſitive levity, till I ſee it better prov'd, than it hath hitherto been, I allow no ſuch thing implanted in ſublunary bodies.

A ball being let fall upon the ground, will rebound, perhaps, ſeveral times, before it reſts. If you ask why the ball being dropt out of the hand, does not move on this or that ſide, or upwards, but fall directly toward the centre of the earth, in that ſhorteſt line, the diameter of the earth produced to the centre of gravity of the ball? 'Twill be readily anſwer'd, that this proceeds from the ball's gravity, an innate appetite, whereby it tends to the centre of the earth the neareſt way. But then I demand, whence comes this rebound, this motion upwards? for, 'tis plainly the genuine conſequence of the motion downwards, and is, therefore, increas'd as that motion in the ball was increas'd, by falling from a greater height: ſo that it ſeems, that nature, in ſuch caſes, plays a very odd game, ſince ſhe forces a ball, againſt the laws of heavy bodies, to aſcend ſeveral times upwards, upon account of that very gravity whoſe office it is to carry it downwards the directeſt way: at leaſt, ſhe ſeems, in ſpite of the wiſdom aſcribed to her, to take her meaſures very ill, in making the ball move downwards with ſo much violence, as occaſions it ſeveral times to fly back from the place ſhe intended it ſhould go to. As if nature could not manage a ball without letting it be hurried on with far greater violence, than her deſign requir'd.

The ſame may be ſaid of a pendulum: for, ſince 'tis unanimouſly affirm'd, that this falls to the perpendicular, upon account of its gravity, 'tis certainly a motion proceeding from the ſame gravity, that the ſwinging weight paſſes beyond the perpendicular, conſequently aſcends, and

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often makes a multitude of vibrations; and, therefore, very frequently ascends, before it comes to reſt in the perpendicular.

There are ſeveral inſtances of perſons choak'd with a hair. The reaſon of this fatal accident, is probably, the irritation made, by the ſtay of ſo unuſual a thing in the throat; whence ariſe very violent and diſorderly motions to expel it, in the organs of reſpiration; by which means, the continual circulation of the blood, neceſſary to life, is hinder'd, the conſequence whereof is ſpeedy death. But this agrees very ill with the vulgar ſuppoſition of ſuch a kind and provident Being, as nature, who is repreſented as always at hand to preſerve the life of animals, and ſuccour them in their dangers and diſtreſſes: for ſince a hair is ſo ſlender a body, that it cannot ſtop the throat, ſo as to hinder, either the free paſſage of aliment into the ſtomach, or that of air to or from the lungs; were it not much better for nature, to let the hair ſtay there, 'till the juices of the body have reſolv'd or conſum'd it, or ſome favourable accident remov'd it, than like a paſſionate and tranſported thing, oppoſe it with ſuch blind violence, that inſtead of ejecting the hair, ſhe expels the life of the perſon?

How the care and wiſdom of nature will be reconcil'd to ſo improper and diſorderly a procedure, I leave her admirers to conſider. But it will appear very reconcileable to providence: for, in regard of the uſe and neceſſity of deglutition, and, in many caſes of coughing and vomiting, it was, in the general, moſt convenient, that the parts ſubſervient to theſe motions, ſhould be irritated by the ſudden ſenſe of things that are unuſual, tho', perhaps, they would not be otherwiſe dangerous or offenſive; becauſe it was fit that the providence of God ſhould, in providing for the welfare of animals, have more regard to that which uſually and regularly befals them, than to extraordinary caſes, or unfrequent accidents; and the like conſiderations will ſufficiently keep the divine providence from appearing to act irrationally or injuriouſly in other caſes.

It has been frequently obſerv'd, that pregnant women have been made to miſcarry by the ſmell of an extinguiſh'd candle, which would before have indeed diſpleas'd, but not endanger'd the ſame perſons: ſo that nature, in theſe caſes, ſeems very far from being ſo prudent and careful as men uſually fancy her, ſince by an odour ſhe is put into ſuch unruly tranſports: and inſtead of watching for the welfare of the woman, whoſe condition needed an extraordinary meaſure of her care and tenderneſs, ſhe violently precipitates her charge into a danger, that often proves fatal, not only to the mother, but alſo to the child.

How the groſs aberrations of nature, in the productions of monſters, will agree with that great uniformity, and excellent skill, aſcribed to her in ſeminal productions, I leave the naturiſts to conſider. Some of them, I know, lay the fault upon the ſtubbornneſs of the matter, that would not obey the plaſtic power of nature; but 'tis ſtrange it ſhould be pretended, that nature, which they make a kind of ſemi-deity, cannot mould and faſhion ſo ſmall, ſoft, and tractable a portion of matter, as that wherein the firſt model and efformation of the embryo is made; when, at the ſame time, they

PHYSICS. tell us, ſhe is able, in ſucking-pumps, to raiſe and ſuſtain whole tons of water, to prevent a *Vacuum*; and can toſs up into the air, houſes, walls, caſtles, and the rocks they are built on, to let kindled gun-powder have the expanſion its new ſtate requires.

It may be ſaid, that unleſs we admit ſuch a Being as nature, to contrive and manage corporeal things, and, in a regular, and methodical way, direct them to their reſpective ends, there will appear no viſible footſteps or proof of a divine wiſdom in the world. This argument, I confeſs, is ſo ſpecious, that it made me long heſitate what to think of the receiv'd notion of nature. But having further conſider'd the matter, I ſaw it might be anſwer'd, that the curious contrivance of the univerſe, and many of its parts, and the orderly courſe of things, with a manifeſt tendency to determinate ends, are matters of fact, and depend not upon the ſuppoſition of ſuch a Being as they call nature; but, ſetting aſide this or that hypotheſis, may be known by inſpection, if thoſe who make it be attentive and impartial. Thus when a man ſees a human body skilfully diſſected, he cannot, if he be intelligent and unprejudic'd, but acknowledge, that there is a moſt curious and exquiſite contrivance in that incomparable engine, and in the various parts of it, which are admirably fitted for diſtinct and determinate uſes. So that I cannot ſuppreſs the manifeſt tokens of wiſdom and deſign, that are to be obſerved in the wonderful conſtruction and orderly operations of the world and its parts; but endeavour to refer theſe indications of wiſdom to the true and proper cauſe. And, as in the hypotheſis of the objectors, there may be three cauſes aſſign'd of theſe foot-ſteps of wiſdom, *God*, *Nature*, and *Chance*; if, according to our doctrine, *Nature* be laid aſide, the competition will remain only between *God* and *Chance*: and ſure he muſt be very dull, or very ſtrongly prejudic'd, who ſhall think it reaſonable to attribute ſuch admirable contrivances, and ſuch regular conducts, as are obſervable in the corporeal world, rather to chance (which is no proper cauſe at all) than to a moſt intelligent Being, from which the moſt curious productions may well be expected. But if ſuch a celebrated thing, as nature is commonly thought, be admitted, 'twill not be near ſo eaſy to prove the wiſdom of God by his works, ſince theſe may have another cauſe; that moſt watchful and provident Being, which men call nature. And this will be more particularly difficult in the *Peripatetic* hypotheſis of the eternity of the world: for, according thereto, there appears no neceſſity, that God ſhould have any thing to do with it, ſince he did not make this *Automaton*, but it was always ſelf-exiſtent, not only as to matter, but as to form too: and as for the government or adminiſtration of the bodies it conſiſts of, that is the proper buſineſs of nature. If it be objected, that this Being is by its aſſertors acknowledg'd to be ſubordinate to God; I anſwer, that, as it may juſtly be queſtion'd, whether many philoſophers, and, perhaps, ſome ſects of them, who are adorers of nature, confeſs'd her to be but the ſubſtitute of a ſuperior and divine Being; ſo, this diſtinction and ſubordination is not eaſy to be prov'd againſt thoſe that ſide with the other ancient philoſophers, who either acknowledg'd no ſuch thing, or expreſly

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deny'd it. Befides, this objection fuppofes the exiftence and fuperiority of a Deity, which therefore needs to be prov'd by other ways; whereas, on my hypothefis, the fame phenomena that difcover admirable wifdom, and manifeft defigns in the corporeal world, of themfelves afford a folid argument, both of the exiftence, and of fome of the grand attributes of God, with which the reft, that properly belong to him, have a neceffary connexion. PHYSICS.

The reafons which I conceive may have induced philofophers to take up, and rely on the receiv'd notion of nature, are fuch as thefe. *The reafons whereon the vulgar notion of nature depends, examined.*

1. One of the moft obvious may be taken from the general belief, or, as men fuppofe, obfervation, that feveral bodies, as particularly the earth, water, and other elements, have each of them its natural place affign'd it in the univerfe; from which place, if any portion of the element, or a mix'd body, wherein that element predominates, happens to be remov'd, it has a ftrong inceffant appetite to return to it; becaufe, when there, it ceafes to gravitate, and is then in a place which nature has qualified to preferve it.

Now, I readily grant, that there being fuch a quantity of very bulky bodies in the world, 'twas neceffary they fhould have places adequate to their bignefs; and 'twas thought fit by the wife architect of the univerfe, that they fhould not be all blended together, but that a great portion of each fhould, at the beginning of things, be difpos'd of and lodg'd in a diftinct and convenient place. But I fee no neceffity of granting what is afferted in the argument: for inanimate bodies having no fenfe or perception, it muft be all one to them in what place they are; becaufe they cannot be concern'd to be in one place rather than in another, fince fuch a preference would require a knowledge that inanimate things have not. And, for the fame reafon, a portion of an element, remov'd by force or chance, from what they call its proper place, can have no real appetite to return thither; for, who tells it that 'tis in an undue place, and that it may better its condition by removing into another? And, who informs it, whether that place lies on this fide of it, or on that, above it, or beneath it? Some philofophers, indeed, have been aware of the weaknefs of the argument, drawn from the vulgar inftance of terreftrial bodies; which being let fall from an eminence, or thrown into the air, fall of themfelves, in a direct line, towards the centre of the earth; and therefore they have ftrengthen'd it, by pretending, that thefe bodies have not (as the ancients pretended) an appetite to defcend to the centre of the earth, but to the great mafs of their connatural bodies. I will not, therefore, accufe thefe philofophers, of the inconfiderate opinion of their predeceffors, who would have nature make all heavy things affect to lodge themfelves in the center of the earth, which being but a point, cannot contain any one of them; yet the hypothefis of thefe is liable to other weighty objections.

For, firft, it is not conceivable, how an inanimate body fhould have an appetite to re-unite with homogeneous bodies, whofe fituation, and diftance are unknown to it.

PHYSICS. (2.) It does not appear that all bodies have ſuch an appetite, as is preſum'd, of joining themſelves to greater maſſes of connatural bodies. If you file the end of an ingot of ſilver or of gold, the filings will not ſtick to their own maſs, placed ever ſo near, or made to touch them, much leſs will they leap to it, when 'tis at a diſtance from them. The like may be ſaid of almoſt all the conſiſtent bodies we are acquainted with, except the load-ſtone and iron, and bodies that participate of one or the other of them.

(3.) 'Tis obvious, that what makes terreſtrial matter fall thro' the air to the earth, is ſome general agent, which, according to the wiſe diſpoſition of the author of the univerſe, determines the motion of thoſe bodies we call heavy, by the ſhorteſt ways permitted them, towards the central part of the terraqueous globe; whether the body, put into motion downwards, be of the ſame, a like, or a quite different nature, from the greater maſs of matter, to which, when 'tis aſſoicated, it reſts. If, from the ſide of a ſhip, you let fall a chip of wood, when your arm is ſo ſtretch'd out, that the perpendicular, or ſhorteſt line, between that and the water, lies ever ſo little without the ſhip; that chip will fall into the ſea, which is a fluid body, and quite of another nature than itſelf, rather than ſwerve in the leaſt from its line of direction, to rejoin itſelf to the great bulk of wood, whereof the ſhip conſiſts. On the other ſide, if a man, ſtanding upon the ſhore juſt by the ſea, ſhall pour out a glaſs of water, holding the glaſs juſt over his feet; that water will fall into the ſand, where 'twill be immediately ſoaked up, and diſperſed, rather than deviate a little, to join itſelf to ſo great a maſs of connatural body, as is the ocean.

To the general belief, uſually made part of the preceding argument, that water does not gravitate in water, its own natural place; I reply, that water does gravitate in water, as well as out of it, and I elſewhere prove it.

2. Another argument, in favour of the receiv'd opinion of nature, may be drawn from the ſtrong appetite that bodies have to recover their uſual ſtate, when, by any means, they are put out of it, and thereby forc'd into a ſtate that is called preternatural; as air, violently compreſſed in a blown bladder, will, as ſoon as the force is remov'd, return to its firſt dimenſions: as the blade of a ſword bent, reſtores itſelf by its innate power, as ſoon as the force ceaſes; and as water, made hot by the fire, haſtens, when 'tis removed thence, to recover its former coldneſs.

But this argument, is grounded on the affections of inanimate bodies; and ſince an inanimate portion of matter is confeſſedly deſtitute of knowledge and ſenſe; it is uncapable of concern to be in one ſtate, or conſtitution, rather than another; for, it has no knowledge of that wherein it is at preſent, nor remembrance of that from which it was forc'd; and, conſequently, no appetite to forſake the former, and return to the latter. But every inanimate body, being, of itſelf, indifferent to all places and ſtates, continues in that place, or ſtate, to which the action and reſiſtance of other bodies, and eſpecially contiguous ones, effectually determine it.

And, before it be aſſerted, that water heated, returns, of itſelf, to its natural coldneſs; it were fit, that the aſſertors ſhould determine,

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what degree, or meaſure of coldneſs is natural to that liqour; and this, if I miſtake not, will prove no eaſy task. And I think it yet more difficult to determine, what degree of coldneſs is natural to water, ſince this liquor perpetually varies its temperature, as to cold and heat, according to that of the contiguous, or neighbouring bodies, eſpecially the air. And therefore the water of an unſhaded pond, for inſtance, tho' it reſts in its proper and natural place, as they ſpeak; yet, in autumn, if the weather be fair, the temper of it will much vary in the compaſs of the ſame day; and the liquor be much hotter at noon, than early in the morning, or at midnight; tho' this great diverſity be the effect only of a natural agent, the ſun, acting according to its regular courſe. And, in the depth of winter, 'tis generally confeſſed, that water is much colder than in the heat of ſummer; which ſeems to be the reaſon of what is obſerv'd by watermen, that, on rivers, boats equally laden, will not ſink ſo deep in winter as in ſummer; the cold condenſing the water, and, conſequently, making it ſpecifically heavier in the former ſeaſon than in the latter. PHYSICS.

As to the motion of reſtitution, obſervable upon the removal, or ceaſing, of the force in air violently compreſſed, and in the blade of a ſword, forcibly bent; I confeſs, it ſeems to me very difficult to aſſign the true mechanical cauſe of it: yet, I think it far more likely, that the cauſe ſhould be mechanical, than that the effect proceeds from ſuch a watchfulneſs of nature, as is pretended. For I queſtion whether we have any air here below, that is in other than a violent ſtate; the lower parts of our atmoſphere being conſtantly compreſſed by the weight of the upper.

A long narrow plate of ſilver, that has not been hammer'd, compreſſed, or made red-hot in the fire, and ſuffer'd to cool leiſurely, may be bent any way, and will conſtantly retain the laſt curve figure, that you gave it. But if, having again ſtraiten'd this plate, you give it a few ſmart ſtrokes with a hammer, it will, by that mere mechanical change, become a ſpringy body. The like may be obſerv'd in copper, but not ſo remarkably, and ſcarce at all, in lead. From theſe phenomena, I demand why, if nature be ſo careful to reſtore bodies to their former ſtate, ſhe does not reſtore the ſilver-plate to its ſtraitneſs, when it is bent this way, or that way, before it be hammer'd? and why a few ſtrokes of a hammer ſhould entitle the plate to nature's peculiar care, and make her ſollicitous to reſtore it, when bent? And why, if the ſpringy plate be again heated and cooled of itſelf, nature abandons her former care, and ſuffers it quietly to continue in what crooked poſture one pleaſes to put it? And what is the reaſon of nature's greater partiality to ſilver, copper, and iron, than to lead and gold, with regard to the motion of reſtitution? However, even in ſword-blades, it has been often obſerved, that tho' if, ſoon after they are bent, the force that bent them be withdrawn, they will ſuddenly return to their former ſtraitneſs; yet, if they be kept too long curved, they will loſe their elaſtic power, and continue in that crooked poſture, tho' the force that put them into it, ceaſes to act: ſo that, it ſeems, nature eaſily forgets the care ſhe was here preſumed at firſt to take.

3. It

PHYSICS. 3. It paſſes for current, and ſeems much to favour the opinion of the naturiſts, that what is violent, is contrary to nature, and therefore cannot laſt long. This trite ſentence is, by the ſchools, ſo particularly apply'd to local motion, that ſome of them have made it the characteriſtic, whereby to diſtinguiſh natural motions from others; that the former are perpetual, or at leaſt very durable, but the latter, being continually check'd by the contrariety of nature, continually decay, and are, within no long time, ſuppreſſed.

But it may be juſtly queſtioned, whether there is any motion among inanimate bodies, that deſerves to be called violent, in contradiſtinction to natural; ſince, among ſuch, all motions, where no intelligent agent intervenes, are made according to univerſal, and mechanical laws.

And methinks, the *Peripatetics*, who are moſt forward to employ this axiom, ſhould find but little reaſon for doing ſo, if they conſider'd how unſuitable it is to their doctrine, that the vaſt body of the firmament, and all the planetary orbs are, by the *Primum Mobile*, with a ſtupendous ſwiftneſs, whirl'd about from eaſt to weſt, in four and twenty hours, contrary to their natural tendency; and, that this violent and rapid motion, of the incomparably greater part of the univerſe, has laſted as long as the world itſelf, that is, according to *Ariſtotle*, for innumerable ages.

We may alſo obſerve here below, that the ebbing and flowing of the ſea, which is generally ſuppos'd to proceed either from the motion of the moon, or that of the terreſtrial globe, or ſome other external cauſe, has laſted for ſome thouſands of years, and probably will laſt as long as the preſent ſyſtem of our vortex continues. That other great ocean too, the atmoſphere, conſiſts of numberleſs myriads of corpuſcles, that are here below continually kept in a violent ſtate; ſince they are elaſtical bodies, whereof the lower are ſtill compreſs'd by the weight of the upper. And, to make a ſpring of a body, it is requiſite that it be forcibly bent or ſtretch'd, and have ſuch a perpetual endeavour to fly open, or to ſhrink in, that it will not fail to do either, as ſoon as the force, that hinder'd it, is remov'd. And, as for the ſtates of inanimate bodies, I ſee not, that their being or not being natural, is, with any certainty, manifeſt from their being or not being very durable. For leaves that wither in a few months, and bloſſoms that often fade in a few days, are as well natural bodies, as the ſolid and durable trees that bear them. And 'tis obvious, that, whether we make the ſtate of fluidity, or of congelation, to be natural to water, and the other violent; its change from one of thoſe ſtates into the other, and even its return to its former ſtate, is often, at ſome ſeaſons, and in ſome places, made perhaps in an hour or leſs, by cauſes acknowledg'd to be natural. Miſts, hail, whirlwinds, lightning, falling-ſtars, *&c.* tho' natural bodies, are far from being laſting; eſpecially in compariſon of glaſs, wherein the ingredients, ſand and fix'd ſalt, are brought together by great violence of fire. And the motion that a thin plate of this glaſs can exerciſe, to reſtore itſelf to its former poſition, when forcibly bent, is in great part a laſting effect of the ſame violence of the

fire.

fire: and ſo is the moſt durable perſeverance of the alcaline ſalt, one of the two ingredients of glaſs, tho' very eaſily ſoluble in water and other liquors, and even in the moiſt air itſelf. PHYSICS.

4. There is a diſtinction of local motion, into natural and violent, ſo generally received and uſed, both by philoſophers and phyſicians, that it deſerves our notice here; ſince it implicitly contains an argument for the exiſtence of the thing call'd nature, by ſuppoſing it ſo manifeſt, that an important diſtinction may juſtly be grounded on it.

This objection, I confeſs, is difficult to clear; not for any force in it, but the ambiguity of the terms, wherein the diſtinction is employ'd: for moſt men ſpeak of this diſtinction of motion, in ſo obſcure, or ſo uncertain a way, that 'tis not eaſy to know what they mean by either member thereof. Yet ſome there are, who endeavour to ſpeak intelligibly, and define natural motion to be that, whoſe principle is within the moving body; and violent motion, that which bodies are put into by an external agent. But, even according to this explication, I am not ſatisfied with the diſtinction: for, as 'tis a principle received, and frequently employ'd, by *Ariſtotle* and his followers, *That whatever moves, is put in motion by ſomewhat elſe*; it ſeems, that, according to this axiom, all motion may be called violent, ſince it proceeds from an external agent. And indeed, according to the ſchool-philoſophers, the motion of far the greateſt part of the viſible world, tho' this motion be moſt regular and laſting, muſt hence be reputed violent; ſince they aſſert, that the immenſe firmament itſelf, with all the planetary orbs, is perpetually, and againſt its native tendency, hurry'd about the centre of the world, once in twenty-four hours, by an external, inviſible agent, which they therefore call the *Primum Mobile*. And as for the criterion of natural motion, that *its principle is within the moving body*, it may be ſaid, that all bodies, once in the ſtate of actual motion, whatever cauſe firſt brought them to it, are moved by an internal principle: for inſtance, an arrow, that actually flies in the air towards a mark, moves by ſome principle or other reſiding within itſelf; for it does not depend on the bow 'twas ſhot out of, ſince 'twould continue, tho' that were broken, or annihilated; nor does it depend upon the medium, which more reſiſts than aſſiſts its progreſs. And if we ſhould ſuppoſe the air either to be annihilated, or render'd uncapable of furthering or hindring its progreſs, I ſee not why the motion of the arrow muſt neceſſarily ceaſe, ſince in this caſe there remains no medium to oppoſe its progreſs. When in a watch that is wound up, the ſpring endeavours to unbend itſelf; and when the ſtring of a drawn bow is broken or let go; the ſpring of the former, and the woody part of the latter, both return to a leſs curved line. And tho' theſe motions are occaſioned by the forcible acts of external agents, yet the watch, the ſpring, and the bow, have in themſelves, for ought appears to thoſe I reaſon with, an inward principle, by which they are mov'd, till they have attain'd their poſition. Some would add, that a ſquib, or a rocket, tho' an artificial body, ſeems, as well as a falling-ſtar, to move from an internal principle. On the other ſide, external agents are requiſite

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PHYSICS. to many motions, acknowledg'd to be natural; as the heat of the ſun to the germination and flouriſhing of ſeveral plants, hung up in the air in the ſpring. And if in the air-pump you place ſeveral inſects, and withdraw the common air from the receiver, they will lie moveleſs, as if they were dead, tho' it be for ſeveral hours, whilſt they are kept from enjoying the air: but when that is permitted again to return upon them, they will preſently revive, and be brought to move again; as if a fly, for inſtance, reſembled a little wind-mill, in being moveleſs of itſelf, and requiring the action of the air to put its wings, and other parts into motion. But, ſince motion is not eſſential to matter, as diviſibility and impenetrability are believ'd to be; the motions of all bodies, at leaſt at the beginning of things, and of moſt, the cauſes of whoſe motions we can diſcern, were impreſſed on them; either by an external immaterial agent, God, or by external portions of matter acting on them.

And tho' motion be deſervedly made a principal part of *Ariſtotle*'s definition of nature; yet men uſually call ſuch motions natural, as are very hard to diſtinguiſh from thoſe they call violent. Thus, when water falls to the ground, they tell us, this motion is natural to that liquor, as 'tis a heavy body; but when a man ſpurts water into the air, they pronounce the motion, becauſe of its tendency upwards, to be contrary to nature: yet when he draws water into his mouth thro' a pipe held perpendicularly, they will have this motion, tho' directly upwards, to be not violent, but natural. So when a blown bladder, let fall upon a floor, rebounds, the deſcent and aſcent are both ſaid to be natural motions; tho' the former tends towards the centre of the earth, and the latter from it. And ſo, if, from a conſiderable height, you let fall a ſphere of ſome cloſe wood, not too heavy, into a deep veſſel of water, it will deſcend far in that liquor, by a natural motion; yet its contrary motion, upwards, ought not to be eſteem'd violent, ſince, according to the ſchools, being lighter *in ſpecie* than water, 'tis natural to it to affect its proper place; for which purpoſe, it muſt aſcend to the top of the liquor, and float. 'Tis, however, from theſe tendencies to oppoſite points, that men judge many motions of bodies to be natural, or violent.

And, indeed, ſince it muſt be indifferent to a lifeleſs and inſenſible body, to what place 'tis made to move, all its motions may, in one reſpect, be ſaid to be natural; and, in another, violent: for as very many bodies, of a viſible bulk, are ſet in motion by external impellents, upon which account, their motions may be ſaid to be violent; ſo the generality of impell'd bodies, move upwards, downwards, *&c.* toward any part of the world, in what line ſoever they find their motion leaſt reſiſted; which impulſe and tendency being given by virtue of what they call the general laws of nature, the motion may be ſaid to be natural.

The vulgar diſtinction of motion, therefore, into natural and violent, is not ſo clear and well-grounded, as to oblige us to admit that there is ſuch a Being as the naturiſts aſſert.

5. I come now to consider the argument that may be drawn in favour of the receiv'd notion of nature, from the critical evacuations, which happen in diseases; and the strange stratagems that nature is said to make use of, to free herself from the noxious humours that oppress her. PHYSICS. *The vulgar notion of a crisis examin'd.*

This argument I willingly acknowledge to be very considerable; for we really see, that in continual fevers, especially in hot climates, there usually happen, at certain times of the disease, notable commotions, or conflicts, after which the morbific matter is disposed of, and discharged by ways strange and surprizing; to the great and sudden relief of the patient. Upon this account, I take the argument drawn from crises to be much the strongest that can be urged for the opinion from which I dissent.

But the body of a man is an incomparable engine, which the most wise author of things has so skilfully framed, for lasting many years, that, if there were in it an intelligent principle of self-preservation, (as the naturists suppose there is) things would not, in most cases, be better, or otherwise managed, for the preservation of the animal, than they generally are. So that the question is not, whether there be a great deal of providence and wisdom exercised in the crises of diseases; but upon what account it is, that these apposite things are perform'd? 'Tis the universal opinion of physicians, that 'tis the intelligent principle they call nature; which, being sollicitous for the welfare of the patient, and distressed by the quantity, or hurtfulness of the morbific matter, watches her opportunity to expel it hastily out of the body, by the most safe and convenient ways, which, in the present condition of the patient, can be taken. I, on the other side, attribute crises to the wisdom and ordinary providence of God, exerting itself by the mechanism of that great machine, the world; and of that smaller engine, the human body, as constituted in the patient's circumstances. The reasons that hinder me from acquiescing in the general opinion of physicians, about crises, are principally these.

Crises, properly so call'd, very seldom happen, except in fevers, and the like acute diseases; where, according to the common course of things, the disorder is soon terminated, either by recovery, death, or a change into some other disease: but chronical sicknesses, unless they happen to be accompany'd with feverish distempers, usually have no crises. This argues, that nature doth not make critical evacuations, upon account of such care and watchfulness, as physicians ascribe them to; since she neglects to employ so wholesom an expedient in diseases, that are often no less dangerous and mortal than acute ones, which she attempts to cure by crises.

Again, critical evacuations may be procured by the bare mechanism of the body; for, by virtue of that, the fibres, or motive organs of the stomach, *Viscera*, and other parts, being distended, or vellicated, by the abundance, or acrimony, of the peccant matter, will, sometimes, by such an irritation, be made to contract themselves vigorously, and to throw out the matter that offends the part, either by the emunctories of the body, or by whatever passages it can, with most ease, be discharged. Thus,

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PHYSICS. when ſome men find their ſtomachs burden'd with aliment, they provoke it to unload itſelf with a feather, or the like, without being beholden to nature's watchfulneſs for a criſis, which, probably, ſhe would not attempt. And as 'tis uſual for criſes to be made in fevers by large hemorrhagies at the noſe, and other parts; which is aſcribed to nature's care for the patient's recovery; I muſt add, that it hath been often obſerved, that even, after death, large bleedings have happen'd at ſeveral other parts of the body; which ſhews, that ſuch excretions may be made by virtue of the ſtructure of the machine, or the turgeſcence and acrimony of the humours, without any deſign of nature to ſave the life of the patient.

Indeed, if it appear'd, by experience, that all criſes of diſeaſes either expell'd the morbific matter, or notably relieved the patient; the critical attempts of nature would much favour the opinion men have conceiv'd of her vigilance and conduct: but unwelcome inſtances daily ſhew, that, as ſome criſes are ſalutary, ſo others prove mortal. And, among thoſe that do not directly kill the patient, there are ſeveral which leave him in a worſe condition than he was before. I, therefore, wonder not, that phyſicians have thought themſelves obliged to lay down ſeveral circumſtances, as neceſſary requiſites of a laudable criſis; if any of which be wanting, 'tis not thought of the beſt kind: and if the contrary to ſome of them happen, 'tis to be judged either pernicious, or hurtful. For, as there are two general ways, ſuppoſed to be employ'd by nature, in making criſes; the one by expulſion of the peccant matter out of the body; and the other, by the ſettling of the matter ſomewhere within it; neither of theſe is conſtantly ſucceſsful.

Whence experience hath obliged phyſicians to divide criſes not only into perfect, that fully determine the event of the diſeaſe; and imperfect, that only alter it for the better or the worſe; but into ſalutary, that quite relieve the patient; and mortal, which deſtroy him. And, to a perfect and ſalutary criſis, ſome require no leſs than ſix conditions; that it be preceded by ſigns of coction; that it be made by a manifeſt and ſufficient excretion, or tranſlation; that it happen upon a critical day; that it leave no relicks behind, to endanger a relapſe; that it be without dangerous ſymptoms; and, laſtly, ſuitable to the nature of the diſeaſe, and the patient.

Hence it may appear no common thing to meet with a perfect and ſalutary criſis, ſince ſo many laudable conditions muſt concur in it. And, indeed, nature uſually takes up but with imperfect ones; and it were happy if ſhe made no better, provided ſhe made no worſe. But 'tis found, by ſad experience, that ſhe rouzes herſelf up to make a criſis, not only upon improper days, but alſo when there appear no ſigns of coction; and, by theſe unſeaſonable attempts, weakens the patient and encreaſes the malady, or, perhaps, makes it ſpeedily mortal. Nor will it juſtify nature, to ſay, that theſe attempts are accidentally brought on, by the acrimony, or importunity of the morbific matter, by which ſhe is provoked, before the time, to endeavour an expulſion of it. For if nature be ſo prudent and

and watchful a guardian, as is thought, ſhe ſhould not ſuffer herſelf to be provoked to act prepoſterouſly, and make furious attempts, that laviſh the little ſtrength the patient hath ſo much need of. And, therefore, phyſicians often do very well, when, agreeably to the dictates of prudence, they forget how much wiſdom they are apt to aſcribe to nature, and employ their beſt skill and remedies to ſuppreſs, or moderate, the inordinate motions, or the improper and profuſe evacuations, that ſhe, when irritated, raſhly begins to make. And tho' the criſes that are made by a tranſlation of the peccant matter, or by lodging it in ſome particular part of the body, be often, when they are not ſalutary, ſomewhat leſs hurtful than thoſe made by excretion; yet theſe, frequently, prove dangerous; ſometimes producing inward impoſthumes, or external tumors, in parts either noble by their functions, ſituation, connexion, or ſympathy with others, that are not to be without hazard, or great inconvenience, oppreſs'd.

I know that phyſicians make it a great argument of nature's providence and skill, that ſhe watches for the concoction of the peccant matter, before ſhe attempts to expel it by a criſis. But what is to be meant by this coction of humours, they do not clearly declare. As I underſtand it, when they ſay, that a portion of peccant matter is brought to coction, they mean, that it has acquir'd ſuch a diſpoſition, as makes it more fit than before, to be ſeparated from the ſounder portion of the maſs of blood, or from the conſiſtent parts to which it, perhaps, formerly adhered; and to be afterwards expell'd out of the body. As where the lungs are affected by a freſh taken cold, we ſee, that, after a few days, the phlegm is made more fluid, and that lodg'd in them, is eaſily brought up by the coughing, which could not diſlodge it before.

And, in fevers, that ſeparation in the urine, which phyſicians look upon as a good ſign of coction, ſeems to be produc'd by ſome part of the peccant matter, that, beginning to be ſeparated from the blood, mixes with the urine, and is not uſually diſtinguiſh'd from it, whilſt this liquor is warm; but when grown cold, on account of its weight, or texture, appears in a diſtinct form; as of a cloud, ſediment, &c.

But, whatever they mean by coction, 'tis plain that, on many occaſions, nature waits not for it, but unſeaſonably, and often with danger, attempts to throw off the matter that offends her, before it be duly prepar'd for expulſion.

'Tis a circumſtance of criſes, thought the moſt wonderful, that nature often, by very unuſual ways, and at unexpected places, diſcharges the matter that offends her, and thereby either cures, or notably relieves the patient.

But, ſetting aſide extraordinary caſes, it ſeems probable, that the performances of nature, in common criſes, are, in ſome meaſure, referable to the particular condition of the matter to be expell'd, but principally to a peculiar diſpoſition in the primitive fabric of ſome parts of the patient's body; or ſome unuſual change made in the conſtruction of theſe parts by the diſeaſe itſelf, or

PHYSICS. other accidents: which original, or adventitious difpofition being invifibl to us, at leaft whilft the patient lives, we are apt to afcribe the unexpect accidents of a crifis, if it prove falutary, to the wonderful providence of nature; and if it happen otherwife, to overlook them. Plentiful evacuations, procured by medicines, are a kind of artificial crifes. Some bodies, we fee, are fo conftituted, that tho' the peccant humour, wrought on by the medicine, ought, as the phyfician thinks, to be expell'd by ftool, in the generality of thofe who take rhubarb or fena; yet the peculiar difpofition of the patient's ftomach will make that an emetic, which was intended for a cathartic. Nor does this conftitution of the ftomach equally regard all purging medicines; for the fame ftomach that will reject them in the form of a potion, will quietly retain them, in that of pills. Thus mercury, which tho', when duly prepared, is ufually given to procure falivation, efpecially to fucculent bodies; yet there are fome patients, wherein, inftead of falivating, 'twill work violently downwards, like a purge; or make fome other unexpected evacuation. And I have feen a patient, who, tho' young and very fat, could not be brought to falivate, either by the gentler ways; by turbith-mineral, or other harfher medicines, tho' adminiftred by very skilful phyficians and chirurgeons.

And this peculiarity may be as well contracted, as native: for fome perfons, efpecially after furfeits, having been ruffly dealt with, or, at leaft, tired out with a medicine of this or that kind of form, will afterwards naufeate and vomit up the like, tho' in other bodies it be very far from emetic.

We fee, alfo, that fometimes fudorific medicines, inftead of procuring fweat, prove briskly diuretic, and fometimes either purging or vomitive.

From all this we may argue, that the qualities of the irritating matter, and much more the particular difpofition of the patient's body, may procure evacuations at unexpected places.

There are alfo inftances of periodical and critical evacuations, at very inconvenient, and unufual vents. Thus fome women are recorded to have had their menfes, fometimes at the eyes, fometimes at the navel, fometimes at the mouth, *&c.* of which there feems no caufe fo probable, as fome peculiar ftructure, whether native or adventitious, of the internal parts concern'd in that difcharge: and of fuch unufual ftructures, anatomifts muft have feen many, fince I myfelf have obferv'd more than one or two. Now if thefe uncommon ways of difpofing of the morbific matter, we real ways falutary to the patient, the argument grounded on them would have more weight: but tho' moft men take notice of this fort of crifes, only when they are lucky, yet an impartial obferver fhall often find, that ill-condition'd and hurtful ones, may be made by unufual and unexpected ways. And, in fome tranflations of the morbific matter to diftant and nobler parts, perhaps it will be as difficult to fhew, by what channels or known ways the matter pafs'd from one to another, as 'tis to determine, how it was conducted to the parts that moft happily vented it.

'Tis

'Tis my settled opinion, that divine prudence is, at least, often concern'd, in a peculiar manner, about the actions of men, and the things that happen to them, or has a necessary connexion with the one, the other, or both. And tho' I think it probable, that in the conduct of that far greatest part of the universe, which is merely corporeal, the wise author of it seldom manifestly procures a recess from the settled course of the universe, and especially from the most general laws of motion; yet, where men, who are creatures, that he is pleas'd to endow with free-will (at least with regard to things not spiritual) are nearly and highly concern'd; he may, not only sometimes by those signal and manifest interpositions we call miracles, acted in a supernatural way; but, as the sovereign lord and governour of the world, frequently give, by the intervention of rational minds, as well united, as not united to human bodies, several such determinations to the motion of parts in those bodies, and of others, which may be affected by them, as by laws merely mechanical, those parts of matter would not have had: and by motions so determin'd, either salutary or fatal crises, and many other things, conducive to the welfare or detriment of men, may be produced.

The interposition of divine providence, in cases of life and death, might be easily shewn to christians, from several passages of scripture. And, I think, it becomes a christian philosopher, to admit in general, that God sometimes in a peculiar, tho' secret way, interposes in the ordinary phenomena and events of crises; but that this is done so seldom, at least in a way that we can certainly discern, that we are not hastily to have recourse to an extraordinary providence, and much less to the strange care and skill of that question'd Being call'd nature, in a particular case, if it may be probably accounted for by mechanical laws, and the ordinary course of things.

When we consider the world, and the physical changes that happen in it, with regard to the divine wisdom and providence; the arguments for the affirmative ought, in their kind, to have more force than those for the negative. For it seems more allowable, to argue a providence from the exquisite structure and symmetry of the mundane bodies, and the apt subordination and train of causes, than to infer from some physical irregularities, that things are not fram'd and administred by a wise author and governour. For the characters and impressions of wisdom, conspicuous in the curious fabric, and orderly train of things, can, with no probability, be referr'd to blind chance, but to a most intelligent and designing agent. Whereas, on the other hand, the irregularities we speak of, are incomparably fewer than those things which are regular, and produc'd in an orderly way; and the divine maker of the universe being a most free agent, and having an intellect infinitely superiour to ours, may, in the production of seemingly irregular phenomena, have ends unknown to us, which even these irregularities may be very fit to compass.

SECT.

SECT. III.

Axioms about nature, how far, and in what ſenſe to be admitted.

TO proceed, the moſt general and current axioms concerning nature, employ'd in the writings of philoſophers, may have a fair account given of them, agreeably to the doctrine hitherto propos'd; tho' ſome of theſe axioms ſuppoſe, and others ſeem ſtrongly to ſupport the receiv'd notion of nature.

But before I conſider the particular maxims receiv'd concerning nature's actions, let us ſee in what ſenſe nature may, or may not, be ſaid to act at all, or to do this or that.

For ought I can clearly diſcern, whatſoever is perform'd in the merely material world, is really done by particular bodies, acting according to the laws of motion, *&c.* ſettled and maintain'd by God, among things corporeal. In which hypotheſis, nature ſeems rather a notional thing, than a true phyſical, diſtinct efficient; ſuch as would be, in caſe *Ariſtotle*'s doctrine were true, one of thoſe intelligences he preſum'd to be the movers of the celeſtial orbs. But men often expreſs themſelves ſo very ambiguouſly, or intricately, when they ſay, that nature does this, or acts thus, that 'tis ſcarce poſſible intelligibly to tranſlate their expreſſions into any forms of ſpeech, adequate to the originals.

1. In this axiom, every nature preſerves it ſelf; by the word nature, I ſuppoſe, is underſtood a natural body, otherwiſe I know not what it means. Thus, then, it may either ſignify, that no one body tends to its own deſtruction, or that, in every body, there is a principle call'd nature, upon account whereof the body is vigilant and induſtrious to preſerve its ſtate, and to defend itſelf from the violence and attempts of all other bodies that oppoſe, or endeavour to deſtroy or harm it.

In the former ſenſe, the axiom may be admitted, without any prejudice to our doctrine: according to which, inanimate bodies can have neither appetites, hatreds, nor deſigns; ſo that we may eaſily grant, they have none to deſtroy themſelves.

But, according to the other ſenſe of the axiom, 'twill import, that every body has within it ſelf a principle, whereby it deſires, and with all its power endeavours to compaſs its own preſervation; and both to do thoſe things, that tend thereto, and oppoſe all endeavours, that outward agents, or internal diſtempers, may uſe to compaſs the deſtruction of it.

And, as this is the moſt vulgar ſenſe of the axiom, ſo 'tis chiefly in this, that I am concern'd to examine it.

I conceive, then, that the moſt wiſe creator of things at firſt ſo framed the world, and ſettled ſuch laws of motion between the bodies, which, as parts, compoſe it; that by the aſſiſtance of his general concourſe, the portions of the univerſe, are lodg'd in ſuch places, and furniſh'd with ſuch powers, that, by the help of his general providence, they may have their beings continued and maintained, as long as the courſe he thought fit to eſtabliſh amongſt things corporeal, requires.

Upon this ſuppoſition, which is but reaſonable, there will appear no neceſſity to have any recourſe, for the preſervation of particular bodies, to ſuch an internal appetite and in-bred knowledge in each of them, as the naturiſts preſume: ſince, by virtue of the original frame of things, and eſtabliſhed laws of motion, bodies are neceſſarily determined to act on ſuch occaſions, after the manner they would, if they had really an aim at ſelf-preſervation. PHYSICS.

Thus, 'tis all one to a lump of dough, whether you make it into a round loaf, a long roll, or a flat cake: for in whatever figure the hands leave it, that it retains without having any appetite to return to the former. The mariner's needle, before it is excited, may have no particular propenſity to one part of the heavens more than another; but when it has been duly touch'd upon a load-ſtone, the flower-de-luce will be determin'd to the north, and the oppoſite extreme to the ſouth. So that, if the lily be drawn aſide, towards the eaſt or the weſt, as ſoon as the force that detain'd it, is remov'd, it will return to its former poſition, and never reſt, till it regard the north. But, in ſpite of this ſeeming affection of the lily to that point of the horizon, yet, if the needle be duly touch'd upon the contrary pole of a vigorous load-ſtone, the lily will preſently loſe its former inclination, and regard the ſouth part of the heavens.

2. Another received axiom concerning nature, is, that ſhe never fails of her end. This propoſition is very ambiguous. However, if by nature we here underſtand the Being, that the ſchool-men ſtyle *Natura naturans*; I allow that nature never miſſeth its end. For the omniſcient and almighty author of things having once framed the world, and eſtabliſh'd in it the laws of motion, which he conſtantly maintains, there can no irregularity happen, eſpecially among the greater mundane bodies, that he did not from the beginning foreſee and permit. And tho', on ſome ſpecial occaſions, this inſtituted order, either ſeemingly or really, has been violated; yet theſe things happening but rarely, and for weighty ends and purpoſes, by the peculiar intervention of the firſt cauſe, either guiding or over-ruling the propenſities and motions of ſecondary agents; it cannot be ſaid, that God is fruſtrated of his ends by theſe exorbitances, whereby he moſt wiſely and effectually accompliſhes them. But, if by nature be meant ſuch a ſubordinate principle, as men uſually underſtand by that name, I doubt the axiom is in many caſes falſe: for tho' the material world is ſo conſtituted, that, for the moſt part, things are brought to paſs by corporeal agents, as regularly, as if they deſigned the effects they produce; yet there are ſeveral inſtances wherein things happen quite otherwiſe.

Thus when a woman is pregnant, the aim of nature is, to produce a perfect human fœtus; yet we often ſee, nature widely miſſing her mark, inſtead of that, produces a monſter. Thus the ſap, that nature raiſes with intent to feed the fruit of a white-thorn, for inſtance, is by graffing brought to nouriſh a fruit of quite another kind. So, when the malſter makes barly to ſprout, whence nature intends to produce ſtalks and ears, 'tis perverted to a very different purpoſe.

3. Another

PHYSICS. 3. Another celebrated axiom concerning nature, is, that ſhe always acts by the ſhorteſt ways. But this rule, as well as ſeveral others, requires to be explained and limited, before it be admitted. 'Tis true, the omniſcient author of the univerſe has ſo framed it, that moſt of its parts act as regularly in order to the ends propoſed, as if they did it with deſign. But ſince inanimate bodies have no knowledge, it cannot reaſonably be ſuppoſed, that they moderate and vary their own actions, according to the exigency of particular circumſtances, wherewith they muſt of neceſſity be unacquainted; it were therefore ſtrange, if various occurrences did not determine them to act by other than the ſhorteſt ways, that lead to particular ends, if thoſe other ways be more agreeable to the general laws or cuſtoms eſtabliſhed among things corporeal. This I prove by inſtances taken from gravity itſelf, a quality perhaps very probably refer'd to an innate power and propenſion. For if a heavy body be let fall into the free air, 'twill take its courſe directly towards the centre of the earth; and, if it meet with an inclining plane, which puts it out of its way, it will not loſe its tendency towards the centre, but run along that plane, by which means its tendency downwards is proſecuted, tho' not, as before, in a perpendicular line, yet in the ſhorteſt way it is permitted to take. Theſe obvious phenomena, I confeſs, agree very well with the vulgar axiom, and poſſibly were the chief things that induced men to frame it. But now let us ſuppoſe, that a little ſphere of marble or ſteel, after having long fallen thro' the air, lights upon a pavement of hard ſtone, that lies horizontal; in this caſe, experience ſhews, that the falling globe will rebound to a conſiderable height, and falling down again, rebound again, and ſo for ſeveral times ſucceſſively, before it approaches as near as is permitted it, to the centre of heavy bodies. But if nature acted in all caſes by the ſhorteſt ways, this ſphere ought not to rebound at all. And having taken a good ſea-compaſs, and ſuffer'd the magnetic needle to reſt north and ſouth; if I held the proper pole of a good load-ſtone at a convenient diſtance, on the right or left hand of the lily, this would be drawn aſide from the north point towards the eaſt or weſt, as I pleaſed; and then the load-ſtone being quite remov'd, the lily of the needle would indeed return northward, tho' not ſtop in the magnetic meridian, but paſſing on ſeveral degrees beyond it, 'twould thence return without ſtopping at the meridian line: and ſo, by its vibrations, deſcribe many arches ſtill ſhorter and ſhorter, till at length it came to ſettle on it, and recover that poſition, which, if nature always acted by the moſt compendious ways, it ſhould have reſted at the firſt time it had regain'd it. The truth is, that, at leaſt, inanimate bodies, acting without knowledge or deſign of their own, cannot ſtop or moderate their own actions, but muſt neceſſarily move as they are determined by the general laws of motion; according to which, in one caſe, the impetus, that a body acquires by falling, is more powerful to carry it on beyond the line of direction, than the action of the cauſes of gravity is to ſtop it, as ſoon as it comes to the neareſt place they can give it to the centre of the earth. And ſomething like this happens in levity, as well as gravity;

gravity; for if you take an oblong and conveniently ſhaped piece of light wood, and having ſunk it to the bottom of deep ſtagnant water, give it liberty to aſcend, it will not only regain the ſurface of the water, where, by the laws of gravity, it ought to reſt, and did reſt before it was forc'd down; but riſe far beyond that ſurface, and in part, as it were, ſhoot itſelf up into the incumbent air, then fall down again, and riſe a ſecond time, and perhaps much oftner, and fall again, before it ſettles in its due place, wherein it is in an equilibrium with the water, that endeavours to preſs it upwards. PHYSICS.

4. Another maxim, generally receiv'd concerning nature, is, that ſhe always does what is beſt. But of this it will not be ſafe to deliver an opinion, till I have remov'd the ambiguity of the words; for they eaſily admit of two different ſenſes. They may ſignify, that nature, in the whole univerſe, does always that which is beſt for the preſervation of it in its preſent ſtate; or that, with regard to each body in particular, nature ſtill does what moſt conduces to the preſervation and welfare of that body. In the firſt of theſe ſenſes, the axiom will be leſs liable to exception; but then, I fear, it will be difficult to be poſitively made out, by ſuch inſtances as prove that nature acts otherwiſe than neceſſarily according to mechanical laws. And, therefore, till I meet with ſuch proofs, I ſhall proceed to the other ſenſe; which, tho' the moſt uſual, I cannot admit, without it be both explain'd and limited. I readily grant, as I have often occaſion to repeat, that the all-wiſe author of things corporeal has ſo framed the world, that moſt things happen in it, as if the particular bodies that compoſe it, were watchful both for their own welfare, and that of the univerſe. But I think, withal, that particular bodies, at leaſt ſuch as are inanimate, acting without either knowledge or deſign, their actions tend not to what is beſt for them in their private capacities, any further than ſuits with the general laws of motion, and the important cuſtoms eſtabliſh'd among things corporeal: ſo that, in conformity hereto, ſeveral things are done, that are neither the beſt, nor ſo much as good, with regard to the welfare of particular bodies.

We often ſee, that fruit-trees, eſpecially when they grow old, will, for one ſeaſon, be ſo overcharg'd with fruit, that they decay and die ſoon after; and even, whilſt they flouriſh, the exceſſive weight upon them, ſometimes breaks off the branches, and thereby both hinders the maturity of the fruit, and haſtens the death of the tree. Now this fatal profuſion would have been prevented, if a wiſe nature, harbour'd in the plant, did, as is preſumed, ſollicitouſly watch for its welfare.

We ſee, alſo, in ſeveral diſeaſes, and in the unſeaſonable and hurtful criſes of fevers, how far, what men call nature, often is from doing that which is beſt for the patient's preſervation. In many diſeaſes, a great part of the phyſician's work is to appeaſe the fury, and to correct the errors of this pretended nature; which being, as 'twere, tranſported with a blind and impetuous paſſion, unſeaſonably produces thoſe dangerous

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And if nature be so provident and watchful for the good of men, and other animals, and of that part of the world wherein they live; how comes she, from time to time, to destroy such multitudes of men and beasts, by earth-quakes, pestilences, famine, &c.

5. The word *Vacuum* being ambiguous, and used in differing senses; 'tis requisite, before I declare my opinion about the receiv'd axiom of the schools, that nature abhors a *Vacuum*, to premise the chief acceptations, in which I have observ'd the term *Vacuum* to be used for it has sometimes a vulgar, and sometimes a philosophical, or strict signification. In common speech, to be empty, usually denotes, not to be destitute of all body whatsoever; but of that body men suppose should be in the thing spoken of, or of that which it was framed, or design'd to contain. Thus, we say, a purse is empty, if there be no money in it; or a bladder empty, when the air is squeez'd out, &c. The word *Vacuum* is, also, taken in another sense, by philosophers who speak strictly, when they mean by it a space within the world, wherein there is not contain'd any body whatsoever.

Now the chief, if not the only, reason that moves the generality of philosophers to believe, nature abhors a *Vacuum*, is, that, in some cases, they observe an unusual endeavour, and, perhaps, a forcible motion in water, and other bodies, to oppose a *Vacuum*. But I am not apt, without absolute necessity, to ascribe to inanimate, and senseless, bodies, the appetites and hatreds that belong to rational, or sensitive, Beings; and, therefore, think it a sufficient reason to decline employing such improper causes, if, without them, the motions, ascribed thereto, can be accounted for.

If the *Cartesian* notion of the essence of a body, consisting in three dimensions, be admitted, it can scarce be deny'd, that nature does not produce these great, and irregular efforts to hinder a *Vacuum*; since, it being impossible there should be any, 'twere a fond thing to suppose, that nature, who is represented to us as a most wise agent, should take pains, and do extravagant things, to prevent an impossible mischief.

If the atomical hypothesis be admitted, it must be granted, that nature is so far from abhorring a *Vacuum*, that a great part of the things she does, require it; since they are brought to pass by local motion; and there are very many cases, wherein, according to these philosophers, the necessary motions of bodies cannot be perform'd, unless the corpuscles, that lie in their way, have little empty spaces to retire, or be impell'd into; when the body, that impels them, endeavours to displace them. Whence the axiom, that nature abhors a *Vacuum*, agrees with neither of the two great sects of the modern philosophers.

But, for ought appears by the phenomena employ'd to demonstrate nature's abhorrence of a *Vacuum*; either nature does not abhor a *Vacuum*, even when she seems sollicitous to hinder it; or, she has but a very moderate hatred of it, in that sense wherein the vulgar philosophers take the word *Vacuum*.

For,

For, in almoſt all viſible bodies here below, and even in the atmoſphere itſelf, there is more or leſs of gravity, or tendency towards the centre of our terraqueous globe; whence nature need not diſquiet herſelf, and act irregularly, to hinder a *Vacuum:* ſince, without her abhorrence of it, it may be prevented or repleniſhed, by her affecting to place all heavy bodies as near the centre of the earth, as heavier than they will permit. And even, without any deſign of her's, a vacuity will be as much oppos'd, as we really find it, by the gravity of moſt, if not of all bodies here below, and the fluxility of liquors. For, by virtue of their gravity, and the minuteneſs of their parts, they will be determin'd to inſinuate themſelves into, and fill all the ſpaces, that they find not already poſſeſſed by other bodies, either more ponderous, *in ſpecie*, than themſelves, or, by reaſon of their firmneſs of ſtructure, capable of reſiſting, or hindering, their deſcent. Accordingly, we obſerve, that, where there is no danger of a *Vacuum*, bodies may move, as they do, when they are ſaid to endeavour its prevention. Thus, if you would thruſt your hand deep into a veſſel of ſand, and afterwards draw it out again; there will need nothing but the gravity of the ſand to make it fill up the greateſt part of the ſpace deſerted by the hand. And if the veſſel be repleniſhed, inſtead of ſand, with an aggregate of corpuſcles more minute and ſmooth than the grains of that, as, for inſtance, with quick-ſilver, or with water; then the ſpace, deſerted by the hand, will be, to ſenſe, compleatly fill'd by the corpuſcles of the fluid. And if you take a glaſs-pipe, whoſe cavity is too narrow to let water and quick-ſilver paſs by one another in it; and having lodg'd a ſmall cylinder of mercury, of about half an inch long, in the lower part, you carefully ſtop the upper orifice with your finger, the quick-ſilver will remain ſuſpended in the pipe: and if, then, you thruſt the tube directly downwards, into a deep glaſs of water, till the quick-ſilver be depreſſed about a foot beneath the ſurface of the water; and then take off your finger from the orifice of the pipe, the quick-ſilver will, immediately, aſcend ſwiftly, for five or ſix inches, and remain ſuſpended at this new ſtation. Here, therefore, we have a ſudden aſcent, of ſo heavy a body as is quick-ſilver, and a ſuſpenſion of it in the glaſs, not produced to prevent, or fill a *Vacuum*; for the pipe was open at both ends: the phenomena being but genuine conſequences of the laws of the equilibrium of liquors.

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Conſidering how great a power the ſchool-philoſophers aſcribe to nature, I am the leſs inclined to think her abhorrence of a *Vacuum* ſo great as they imagin'd. I have ſhewn, that her averſion to, and watchfulneſs againſt it, are not ſo vehement, but that, in the ſenſe of the *Peripatetics*, ſhe can quietly admit it, in ſome caſes; where, with a very ſmall endeavour, ſhe might prevent, or repleniſh it. When the *Torricellian* experiment is made, tho' it cannot, perhaps, be fully prov'd, either againſt the *Carteſians*, or ſome other *Pleniſts*, that, in the upper part of the tube, deſerted by the quick-ſilver, there is a *Vacuum*, in the ſtrict philoſophical ſenſe of the word; yet, as the *Peripatetics* declare their ſenſe, by their reaſonings

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againſt a *Vacuum*, 'twill appear very hard for them to ſhew, there is not one in that tube. And, as by the ſcholaſtic way of arguing, nature's hatred of a *Vacuum*, from the ſuſpenſion of water, and other liquors, in tubes and conical watering-pots, it appears, they thought that any ſpace here below, deſerted by a viſible body, not ſucceeded by another viſible body, or at leaſt by common air, may be reputed empty ; ſo, the ſpace deſerted by the quick-ſilver, at the top of of a barometer, thirty-one inches long, will invite one to doubt, whether a *Vacuum* ought to be thought ſo formidable a thing to nature, as they imagine ſhe thinks it? For, what miſchief would enſue to the univerſe, upon the production or continuance of ſuch a *Vacuum*, tho' the deſerted ſpace were much more than an inch, and continued many years, as has ſeveral times happen'd in the taller ſort of mercurial barometers ? And thoſe *Peripatetics* who tell us, if there were a *Vacuum*, the influences of the celeſtial bodies, that are abſolutely neceſſary to the preſervation of the ſublunary ones, would be intercepted, ſince motion cannot be made *in vacuo*, would do well to prove ſuch a neceſſity ; and to conſider, that in our caſe the top of the quick-ſilver, to which the *Vacuum* reaches, uſually appears protuberant ; which ſhews, that the beams of light are able to paſs thro' that *Vacuum*, being, in ſpite of it, reflected from the *Mercury* to the eye. And in ſuch a *Vacuum*, as to common air, I have try'd, that a load-ſtone will emit its effluvia, and move iron or ſteel placed therein.

In ſhort, it is not evident, that, here below, nature ſo much endeavours to hinder or fill up a *Vacuum*, as to manifeſt an abhorrence of it : and, without much peculiar ſolicitude, a *Vacuum*, at leaſt a philoſophical one, is as much provided againſt, as the welfare of the univerſe requires, by gravity and the fluxility of the liquors, and other bodies.

6. I come now to the celebrated ſaying, that nature cures diſeaſes, taken from *Hippocrates*, who expreſſes it in the plural, νȣσῶν φυσιὲς ἰητροί. And becauſe this axiom is generally receiv'd among phyſicians and philoſophers, and ſeems to be one of the principal things, that has made them introduce ſuch a Being as they call nature ; I ſhall attentively conſider, in what ſenſe, and how far, this famous axiom may, or ſhould not, be admitted.

Firſt, then, I conceive it may be taken in a negative ſenſe, ſo as to import, that diſeaſes cannot be cured in ſuch perſons, in whom the aggregate of the vital powers, or faculties of the body, is ſo far weaken'd or depraved, as to be utterly unable to perform the functions neceſſary to life ; or at leaſt to actuate and aſſiſt the remedies employ'd by the phyſician, to preſerve or recover the patient.

This I take to be the meaning of ſuch uſual phraſes as theſe, *Phyſic comes too late. Nature is quite ſpent,* &c. And in this ſenſe I readily acknowledge the axiom to be true. For, where the engine has ſome neceſſary parts, whether fluid or ſolid, ſo far deprav'd or weakned, as to render it altogether unable to co-operate with the medicine, it cannot be rationally expected, that the adminiſtration of that medicine, ſhould prove effectual. But

there

there is a poſitive ſenſe of our axiom, wherein it is the moſt uſually employ'd; for men commonly believe there reſides in the body of a ſick perſon, a certain provident or watchful Being, that induſtriouſly employs itſelf, by its own endeavours, as well as by any occaſional aſſiſtance, to rectify whatever is amiſs, and reſtore the diſtemper'd body to its priſtine ſtate of health.

Now, I conceive that the wiſe and beneficent maker of the world, intending that men ſhould, for the moſt part, live a conſiderable number of years, in a condition to act the parts aſſign'd them; he was pleas'd to frame human bodies ſo, that, with the ordinary ſuccours of reaſon, *&c.* they may in many caſes recover a ſtate of health, if they chance to be put out of it by leſs accidents than thoſe, that God, in compliance with the great ends of his general providence, did not think fit to ſecure them from, or enable them to ſurmount. Many things, therefore, that are commonly aſcrib'd to nature, may be better aſcribed to the mechaniſm of the univerſe, and the human body.

The cauſes that diſorder the human frame, are often tranſient, but the ſtructure of the body it ſelf, and the cauſes that conduce to the preſervation of its ſtructure, are more ſtable and durable; and, on that account, may enable the engine to out-laſt many things that are unfriendly to it, as ſlight hurts, and diſeaſes.

Several phenomena of diſeaſes may be illuſtrated, by ſuppoſing dirt thrown into a vial of fair water, and then the vial to be well ſhaken; whereby the water will loſe its tranſparency, upon a double account; that of the dirt, whoſe opake particles are confounded with it; and that of the bubbles, which ſwim at the top of it: yet to purify this water, and make it recover its former tranſparency, there needs no particular care or deſign of nature; but, according to the common courſe of things, after ſome time the bubbles will break and vaniſh at the top, and the earthy particles, that compoſe the mud, will, by their gravity, ſubſide to the bottom, and ſettle there, whence the water becomes clear again.

There is indeed one thing, to which the maxim, *Nature cures diſeaſes*, may be very ſpeciouſly applied; and that is the healing wounds; which, if but in the fleſh, may often be cured without medicines. However, this healing ſeems to be only an effect or conſequent of that fabric of the body, on which nutrition depends. For the alimental juice being, by the circulation of the blood and chyle, carried to all parts of the body; if it meets any where, either with preternatural concretions, or with a gap made by a cut or wound, its particles there concrete into a kind of fleſh, or other body, which that juice, in the place and other circumſtances 'tis in, is fitted to conſtitute. Thus not only wens and ſcrophulous tumors are nouriſh'd in the body, but miſ-ſhapen mola's grow in the womb, as well as embryo's. And we ſee, in wounds, that fungus's are as well produced and nouriſh'd by the aliment brought to the wounded part, as the true and genuine fleſh; ſo that either nature ſeems much miſtaken, if ſhe deſigns the production and maintenance of ſuch ſuperfluous and inconvenient bodies;

PHYSICS. bodies; or the chirurgeon is much to blame, who induſtriouſly deſtroys them. But, for ought appears, nature is not ſo ſhy and reſerv'd in her bounty, but that ſhe ſends nouriſhment, to repair as well things that do not genuinely belong to the body, as to reſtore fleſh to wounded parts. This appears by warts and corns, that grow again after they are cut. And I have ſeen a woman, in whoſe forehead nature was careful to nouriſh a horn, above an inch in length; which I fully examin'd, whilſt it was yet growing upon her head, to avoid being impoſed upon.

But there are many diſeaſes, as well acute as chronical, wherein, 'tis confeſs'd, that nature alone does not work the cure; ſo that the aphoriſm, which makes nature the curer of theſe, is not true, but in a limited ſenſe. 'Tis, indeed, pretended, that even here, nature is the principal agent, by whoſe direction the phyſician acts in ſubſervience to her deſigns; and phyſicians themſelves acknowledge, that they are but nature's miniſters. Let us, therefore, ſee in what ſenſe it is fit, according to our doctrine, to admit theſe aſſertions.

One great cauſe of the common miſtakes about this matter, is, that the body of a man hath been look'd upon, rather as a ſyſtem of parts, whereof moſt are groſs and conſiſtent, and not a few hard and ſolid too, than as, what indeed it is, a very compounded engine; that, beſides theſe parts, conſiſts of the blood, chyle, gall, and other liquors; and of more ſubtile fluids, as ſpirits and air: all which are inceſſantly and variouſly moved, and thereby put ſeveral of the ſolid parts into frequent and differing motions. When, therefore, the conſtitution, or the motions, that in a ſound body, regularly belong to the fluids, happen, the former to be deprav'd, or the latter to grow irregular; the engine is immediately out of order, tho' the groſs ſolid parts were not primarily affected: ſo, when by proper remedies the vitiated texture of the blood, or other juices, is corrected, and the inordinate motions of them and the ſpirits are calm'd and rectify'd, the groſſer and more ſolid parts of the body, and ſo the whole animal œconomy, will be reſtored to a more convenient ſtate.

The phyſician, in my opinion, is to look upon his patient's body, as an engine out of order; yet ſo conſtituted, that, by concurring with the endeavours, or tendencies, of the parts of the machine, it may be brought to a better ſtate. If, therefore, he find, that, in the preſent diſpoſition of the body, there is a propenſity to throw off the matter that offends it, in a convenient way, and at commodious places; he will then act ſo, as to comply with, and further that way of diſcharge, rather than another. As, if there be a great appearance, that a diſeaſe will quickly have a criſis by ſweat; he will rather further it by covering the patient with warm clothes and giving ſudorific medicines; than, by endeavouring to carry off the peccant matter by purging or vomiting, unſeaſonably hinder a diſcharge, that probably will be beneficial. And, in this ſenſe, men may ſay, if they pleaſe, that the phyſicians are miniſters or ſervants of nature; as ſea-men, when the ſhip ſails before a good wind, will not ſhift their ſails, nor alter the ſhip's motion, becauſe they need not. But to ſhew,

that

that 'tis, as 'twere, by accident, that the phyſician, in this caſe, obeys nature, (to ſpeak in the language of the naturiſts;) there are many other caſes, wherein the phyſician, if skilful, will be ſo far from taking nature for his guide, to direct him by her example; that a great part of his care and skill is employ'd, to hinder her from doing what ſhe ſeems to deſign; and to bring to paſs other things very differing from, if not contrary to, what ſhe endeavours.

Thus, tho' nature, as 'tis call'd, importunately craves for drink in dropſies, the phyſician thinks himſelf obliged to deny it; as he does what his patients greedily deſire in the *Pica*, *&c.* Thus alſo the chirurgeon often hinders nature from cloſing up the lips of a wound, as ſhe would unskilfully do, before it be well and ſecurely heal'd at the bottom. So the phyſician, by purging or phlebotomy, carries off the matter, that nature would more dangerouſly throw upon the lungs.

In ſhort, I look upon a good phyſician, not properly as a ſervant to nature, but a counſellor and a friendly aſſiſtant; who, in his patient's body, furthers theſe motions, and other things, that he judges conducive to the welfare and recovery of it: but as to thoſe, that he perceives likely to be hurtful, either by increaſing the diſeaſe, or otherwiſe endangering the patient, he thinks it his part to oppoſe or hinder, tho' nature manifeſtly ſeems to endeavour to exerciſe or carry on thoſe hurtful motions.

Hence, I fear, the commendations which *Hippocrates* gives of nature, in the cure of diſeaſes, make many phyſicians leſs courageous and careful than they ſhould be, to employ their own skill, on ſeveral occaſions that greatly require it.

As, in ſome caſes, the phyſician relieves his patient in a negative way, by oppoſing nature in her unſeaſonable, or diſorderly attempts; ſo, in others, he may do it in a poſitive one; by employing medicines that either ſtrengthen the parts, or make ſenſible evacuations of matter, neceſſary to be proſcribed by them; or, by uſing remedies, that by their manifeſt qualities oppoſe thoſe of the morbific cauſe. And, perhaps, in ſome caſes, the phyſician may, in a poſitive way, contribute more to the cure even of an inward diſeaſe, than nature herſelf ſeems able to do: for, if any ſuch medicine may be prepared by art, as *Helmont* affirms to be attainable from *Paracelſus's Ludus*, by the alkaheſt; or, as *Cardan* relates, that an empiric had in his time, who travell'd up and down *Italy*, curing ſuch where-ever he came, as were tormented with the ſtone of the bladder; the phyſician might, by ſuch inſtruments, perform that which, for ought appears, is not to be done by nature herſelf; ſince we never find, that ſhe diſſolves a confirm'd ſtone in the bladder. Nay, ſometimes, the phyſician does, even without the help of a medicine, control and over-rule this nature, to the great and ſudden advantage of the patient, as in the caſe of ſwooning.

It appears from the whole, that as there are ſome phenomena, which ſeem to favour the doctrine of the naturiſts, about the cure of diſeaſes; ſo there are others, that more manifeſtly agree with our hypotheſis. And both

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PHYSICS. these ſorts of phenomena, being conſider'd together, may well ſuggeſt a ſuſpicion, that the moſt wiſe and free author of things, having framed the firſt individuals of mankind, ſo as to be fit to laſt for many years, and endow'd thoſe originals with the power of propagating their ſpecies; it thence comes to paſs, that in the ſubſequent engines we call human bodies, when neither particular providence, nor the rational ſoul, nor over-ruling impediments interpoſe, things are generally perform'd according to mechanical laws and courſes; whether the effects and events of theſe prove conducive to the welfare of the engine itſelf, or elſe cheriſh and foment extraneous bodies or cauſes, whoſe preſervation and increaſe are hurtful to it. Hence the happy things, referr'd to nature's prudent care for the recovery and welfare of ſick perſons, are uſually genuine conſequences of the mechaniſm of the world, and the patient's body; which effects luckily happen to be co-incident with his recovery, rather than to have been purpoſely and wiſely produced in order to it; ſince what is called nature, ſeems careful to produce, preſerve, and cheriſh things hurtful to the body, as well as thoſe beneficial to it.

Thus, not only worms, but frogs and toads, taken in their ſpawn, with corrupted water, have been cheriſh'd in the ſtomach, till the eggs being grown compleat animals, have produced horrid ſymptoms in the body. And if, according to the receiv'd opinions of phyſicians, ſtubborn quartans are produced by a melancholy humour ſeated in the ſpleen; it may be ſaid, that nature ſeems to buſy herſelf to convert ſome parts of the fluid chyle into ſo tenacious a juice, that, in many patients, notwithſtanding the neighbourhood of the ſpleen and ſtomach, neither ſtrong emetics, purges, nor other uſual remedies, are able, in a long time, to diſlodge, reſolve, or correct it.

But the poiſon of a mad-dog, nature ſometimes ſeems induſtriouſly to preſerve; for we have inſtances of a little foam convey'd into the blood by a ſlight hurt, which, notwithſtanding the conſtant heat and perſpirable make of the human body, and the diſſipable texture of the foam, ſo preſerved, and ſometimes, too, for many years, that, at the end of that time, it has broke out, and diſplay'd its fatal efficacy with as much vigour and fury, as if it had but newly been receiv'd into the body.

Thus, tho' the quantity of poiſon, in the bite of the *Tarantula*, is ſcarce viſible, being communicated by the tooth of ſo ſmall an animal as a ſpider; yet, in many patients, 'tis preſerv'd during a great part of their lives, and manifeſts its continuance in the body by annual paroxyſms. And, a perſon of great quality, complain'd to me, that being in the *Eaſt*, the biting or ſtinging of a creature, whoſe offenſive arms were ſo ſmall, that the eye could very hardly diſcern the hurt; had ſo laſting an effect upon him, that, for about twelve years after, he was reminded of his miſchance, by a pain he felt in the hurt place, about the ſame time of the year that the miſchief was firſt done him. And, in ſome hereditary diſeaſes, as the gout, the falling-ſickneſs, *&c.* nature ſeems to act as if ſhe did, with care and skill, tranſmit to the unhappy child ſuch morbific ſeeds, or impreſ-

impreſſions of the parents, that, in ſpite of all the various alterations the younger body paſſes thro', during the courſe of many years, this protected enemy is able to exert its power and malice, after forty or fifty years concealment.

In the ſame manner, but with light variations, other axioms about nature might be eaſily explain'd.

But tho' we could not intelligibly explain all the particular axioms about her, and the phenomena of inanimate bodies, that are thought to favour them, by mechanical principles; it would not follow, that we muſt therefore yield up the whole cauſe to the naturiſts. For we have already ſhewn, that the ſuppoſition of ſuch a Being as they call nature, is far from enabling her patrons to give intelligible accounts of theſe and other phenomena of the univerſe. And tho' our doctrine ſhould be inſufficient to give a good account of things corporeal; yet a leſs degree of probability may ſerve in arguments employ'd but to juſtify a doubt, than is requir'd in thoſe that are to demonſtrate an aſſertion.

'Tis true, the naturiſts tell us, that the nature they aſſert is the principle of all motions and operations in bodies; which infers, that in explaining them, we muſt have recourſe to her.

But before we acquieſce in, or confidently employ this principle, it were very fit we knew what it is: I, therefore, demand of thoſe who aſſert ſuch a nature as is vulgarly deſcribed, whether it be a ſubſtance, or an accident? If it be the latter, it ſhould be declar'd, what kind of accident it is; how a ſolitary accident can have right to all thoſe attributes, and produce thoſe numerous and wonderful effects, that they aſcribe to nature: and why a complex of ſuch accidents, as are the mechanical affections of matter, may not altogether as probably as that accident they call nature, be conceiv'd to have been inſtituted by the perfectly wiſe author of the univerſe, to produce thoſe changes among bodies, which are intelligibly referable to them. And if things be not brought to paſs by their intervention, 'twere very fit we ſhould be inform'd, by what other particular and intelligible means nature can effect them better.

But if it be ſaid, as by moſt it is, that the principle call'd nature, is a ſubſtance; I demand, whether it be corporeal or immaterial? If it be immaterial, I further ask, whether it be created, or not? If not, we have God under another name, and our diſpute is at an end, by the removal of its ſubject; which is ſaid by the ſchools, to be God's vicegerent, not God himſelf. But if nature be affirm'd (as ſhe is by all chriſtian philoſophers) to be a created Being; I then demand, whether ſhe be endow'd with underſtanding, ſo as to know what ſhe does; for what ends, and by what laws ſhe ought to act? If the anſwer be negative, the ſuppoſition of nature will be of very little uſe to afford an intelligible account of things. And if it ſhould be ſaid, that nature is endowed with underſtanding, and performs ſuch functions as many of the ancients aſcribe to the ſoul of the world; this hypotheſis is near of kin to heatheniſm; and I do not think, that they who ſuppoſe a kind of ſoul of the univerſe, will

PHYSICS. find this principle ſufficient to explain the phenomena of it. For tho' nature be admitted to have reaſon, yet a multitude of phenomena may be mechanically produced, without her immediate intervention; as, we ſee, that in man, tho' the rational ſoul has ſo narrow a province to take care of, as the human body, and is ſuppoſed to be intimately united to all the parts of it; yet abundance of things are done in the body, by the mechaniſm of it, without being produced by the ſoul. In ſleep, the circulation of the blood, the regular pulſation of the heart, digeſtion, nutrition, reſpiration, *&c.* are perform'd without the immediate agency, or ſo much as the actual knowledge of the mind. And, when a man is awake, many things are done in his body, not only without the direction, but againſt the bent of his mind; as often happens in cramps, and other convulſions, coughing, yawning, *&c.* Nay, tho' ſome brutes, as, particularly, apes, have the ſtructure of many parts of their bodies very like that of the correſpondent ones in men; yet that admirable work of the formation and organization of the fœtus in them, is granted, by philoſophers, to be made by the ſoul of the brute, which is neither an incorporeal, nor a rational ſubſtance. And even the human fœtus, if we admit the general opinion of philoſophers, phyſicians, divines, and lawyers, is form'd without the intervention of the rational ſoul, that is not infuſed, till this hath obtain'd an organization, fitting to receive ſuch a gueſt; which it is reputed to do, about the end of the ſixth week, or before that of the ſeventh.

If it be urged, that nature, being the principle of motion in bodies; their various motions, which amount to a conſiderable part of their phenomena, muſt be explained, by having recourſe to her: I anſwer, 'tis very difficult to conceive how a created, immaterial ſubſtance, can, by a phyſical power, move a body; the agent having no impenetrable part, wherewith to impel the corporeal *Mobile.* God, indeed, who is an immaterial ſpirit, ought to be acknowledg'd the primary cauſe of motion in matter; becauſe motion neceſſarily belongs not to corporeal ſubſtances. But, then, there is that infinite diſtance between the incomprehenſible creator, and the leaſt imperfect order of his creatures, that we ought to be very cautious how we make parallels between them; and draw inferences from his power, and manner of acting, to theirs: ſince he, for inſtance, can immediately act upon human ſouls, as having created them; whilſt they are not able ſo to act upon one another. And it ſeems the more difficult to conceive, that, if nature be an incorporeal ſubſtance, ſhe ſhould be the great mover of the mundane matter; becauſe we ſee, that, in a human body, the rational ſoul, tho' vitally united to it, can only determine the motion of ſome of the parts; not give motion to any, or ſo much as regulate it in moſt. And, if nature be ſaid to move bodies, in another than a phyſical way; I doubt, whether the ſuppoſition of ſuch a principle will be of much uſe to philoſophers, in explaining phenomena. I ſhould ſcarce think him a judicious phyſician, who imagines, that he gives an intelligible and particular account of the ſurprizing ſymptoms of

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thoſe ſtrange diſeaſes, that many impute to witchcraft, when he ſays, that their phenomena are produced by a wicked immaterial ſpirit, call'd a devil. PHYSICS.

But, 'tis thought, that the greater number of philoſophers, at leaſt among the moderns, take nature to be corporeal: I, therefore, demand whether theſe philoſophers believe nature, tho' corporeal, to act knowingly, that is, with conſciouſneſs of what ſhe does, and for deſigned ends; or elſe to be blindly and neceſſarily moved and directed by a ſuperior agent, endow'd with an excellent underſtanding?

The *Carteſians* would ask, how, if nature be a corporeal ſubſtance, we can conceive her capable of thinking; and of being a moſt wiſe and provident director of all the motions that are made in the corporeal world?

And a philoſopher may juſtly demand, how a corporeal Being can ſo pervade the univerſe, as to be intimately preſent with all its minute parts, whereof 'tis ſaid to be the principle of motion?

We may, alſo, demand, whence nature, being a material ſubſtance, comes itſelf to have motion; ſince motion does not belong to matter, in itſelf? For a body is as truly a body when it reſts, as when it moves. If it be anſwer'd, that God, at firſt, put it into motion; I reply, that the ſame cauſe may as probably be ſuppoſed to have put the unqueſtion'd mundane matter into motion; without the intervention of another corporeal Being, in the conception whereof as matter, motion is not involv'd.

It may, likewiſe, be ask'd, how the laws of motion come to be obſerv'd, or maintain'd, by a corporeal Being, which, as merely ſuch, is either uncapable of underſtanding them, or of acting with reſpect to them; or, at leaſt, is not neceſſarily endow'd with any knowledge of them, or power to conform to them, and to make all the parts of the unqueſtion'd mundane matter do ſo too?

And I ſee not, how the taking in ſuch an unintelligible, and undeſigning principle, will free our underſtandings from great difficulties, when we come to explain the phenomena of bodies. For, if nature be a bodily creature, and acts neceſſarily; I ſee no cauſe to look upon it as other than a kind of engine: and the difficulty may be as great, to conceive how all the ſeveral parts of this ſuppos'd engine, call'd nature, are, themſelves, framed, and moved, by the great author of things, and how they act upon one another, as well as upon the undoubted mundane bodies; as 'tis to conceive, how, in the world itſelf, which is, manifeſtly, an admirably contriv'd automaton, the phenomena may, by the ſame author, be produced, in conſequence of the primitive conſtruction, and motions, that he gave it; without the concurrence of ſuch a thing, as they call nature. For this, as well as the world, being a corporeal creature; we cannot conceive, that either of them act otherwiſe than mechanically. And it ſeems very ſuitable to the divine wiſdom, to employ in the world, already framed and completed, the feweſt, and moſt ſimple means, by which the phenomena, deſign'd to be exhibited, could be produced. Nor need we be much mov'd, to hear ſome naturiſts ſay, that nature, tho' not an in-

PHYSICS. corporeal Being, is of an order ſuperior to mere matter. For, who can clearly conceive an order, or kind of Beings, that ſhall be real ſubſtances, and yet neither corporeal, nor immaterial? Nor do I ſee, how the ſuppoſition of this unintelligible, at leaſt unintelligent Being, tho' we ſhould grant it to have a kind of life, or ſoul, will much aſſiſt us to ſolve phenomena.

To draw, at length, towards a concluſion; what we have here deliver'd upon this ſubject, may, perhaps, do ſome ſervice both to natural philoſophy, and to religion.

The uſe and advantages of this inquiry.

We have attempted to diſſuade philoſophers from often employing, and without great need, a term, which, by reaſon of its great ambiguity, and the little care taken by ſuch as uſe it, to diſtinguiſh its different acceptions, occaſions both a great deal of darkneſs, and confuſion, in what men ſay and write about things corporeal; and a multitude of controverſies, wherein really they wrangle about words. And this diſcourſe may, poſſibly, wean many from the fond conceit they cheriſh, that they underſtand, or explain a corporeal ſubject, or a phenomenon, when they aſcribe it to nature. For, to do that, one need not be a philoſopher; ſince a peaſant may eaſily do the ſame.

A man has never well perform'd the part of a true philoſopher, till he circumſtantially, or particularly, deduces the phenomenon he conſiders, by intelligible ways, from intelligible principles; which he will be conſtantly put in mind of doing, or diſcover that he hath not done, if, by forbearing general and ambiguous terms, he endeavours to explain things, by expreſſions that are clear to all attentive readers. And this perſpicuous way of philoſophizing ſhould be greatly recommended, by the valuable diſcoveries which thoſe who employ'd it, have happily made, in hydroſtatics, optics, anatomy, botanics, and many other parts of real learning.

Our doctrine may, alſo, be ſerviceable to religion in three reſpects.

And, firſt, it may keep many, who were inclined to have an exceſſive veneration for what they call nature, from running into thoſe extravagant and ſacrilegious errors, that have been, upon plauſible pretences, embraced, not only by many of the old heathen philoſophers, but by ſeveral modern profeſſors of chriſtianity.

Secondly, it may conduce to juſtify ſome remarkable proceedings of divine providence, againſt thoſe who boldly cenſure it, upon the account of ſome things they judge to be phyſical irregularities; ſuch as monſters, earth-quakes, floods, eruptions of vulcano's, famines, *&c.* For, according to us,

1. God is a moſt free agent, and created the world, not out of neceſſity, but voluntarily; having framed it as he pleaſed, and thought fit, at the beginning of things; when there was no ſubſtance but himſelf, and, conſequently, no creature, to which he could be obliged, or by which he could be limited.

2. God having an underſtanding infinitely ſuperior to that of man, in extent, clearneſs, and other excellencies; he may, rationally, be ſuppos'd to have framed ſo great and admirable an automaton as the world, and the

the ſubordinate engines compriżed in it for ſeveral ends and purpoſes; ſome of them relating chiefly to his corporeal, and others to his rational creatures: of which ends, he hath vouchſafed to make ſome diſcoverable by our dim reaſon; whilſt others are, probably, not to be penetrated by it, but lie conceal'd in the deep abyſs of his unſearchable wiſdom. PHYSICS.

3. It ſeems probable, that this moſt excellent and glorious Being thought fit to order things ſo, that both his works and actions might bear ſome marks of his attributes; and, eſpecially, to ſtamp upon his corporeal works ſome tokens, or impreſſes, of his divine wiſdom, diſcernible by the human mind.

4. Upon this ſuppoſition, it became the divine author of the univerſe to give it ſuch a ſtructure, and ſuch powers, and to eſtabliſh among its parts ſuch general and conſtant laws, as beſt ſuited his purpoſes in creating the world; and to give theſe univerſal laws, and particular parts, or bodies, ſuch ſubordinations to one another, and ſuch references to the original fabric of the grand ſyſtem of the world, that, on all particular occaſions, the welfare of inferior, or private portions of it, ſhould be only ſo far provided for, as their welfare is conſiſtent with the general laws by him ſettled in the univerſe; and with ſuch of thoſe ends, that he propos'd to himſelf in framing it, as are more conſiderable than the welfare of thoſe particular creatures.

5. The laſt ſervice that, I hope, our doctrine may do religion, is, to induce men to pay their admiration, their praiſes, and their thanks, directly to God himſelf; who is the true and only creator of the ſun, moon, earth, and thoſe other creatures, that men call the works of nature. And in this way of expreſſing their veneration of the true God, and their gratitude to him, they are warranted by the examples of the ancient people of God, the *Iſraelites*; and not only by the inſpired perſons of the old teſtament, but by the promulgators of the new, and even by the celeſtial ſpirits; who, in the Revelation of St. *John*, are introduced praiſing and thanking God himſelf for his works; without taking any notice of his pretended vicegerent, nature.

AN

INQUIRY

INTO THE

FINAL CAUSES

OF

Natural Things.

SECT. I.

I Here propoſe to deliver my thoughts upon theſe four queſtions. 1. Whether, indefinitely ſpeaking, there be any final cauſes of things corporeal, knowable to men ? 2. Whether, if that queſtion be reſolv'd in the affirmative, we may conſider final cauſes in all ſorts of bodies, or only in ſuch as are peculiarly qualified ? 3. Whether, or in what ſenſe, the acting for ends may be aſcribed to an unintelligent and inanimate body ? 4. And laſtly, how far, and with what cautions, arguments may be framed upon the ſuppoſition of final cauſes ?

Whether the final cauſes of natural things are knowable to men ?

To begin with the firſt queſtion. Thoſe who would exclude final cauſes from the conſideration of the naturaliſt, ſeem to do it, either becauſe, with *Epicurus*, they think the world was produc'd by atoms and chance, without the intervention of a Deity ; and, conſequently, that 'tis in vain to ſeek for ſuch cauſes: or becauſe, with *Des Cartes*, they imagine, that God being omniſcient, 'tis raſh and preſumptuous for men to think they know, or can diſcover what ends he propos'd to himſelf, in his creatures. The ſuppoſition on which the *Epicureans* have rejected final cauſes, has been diſallow'd by the philoſophers of almoſt all other ſects ; and ſome have written ſufficient confutations of it : I ſhall, however, in the progreſs of our work,

work, make ſuch occaſional obſervations, as may ſerve to diſcredit ſo unreaſonable an opinion. But the *Carteſian* argument having been ſo prevalent among learned and ingenious men, I ſhall beſtow the more pains in the conſideration of that.

One thing, perhaps, that kept ſo great a philoſopher as *Des Cartes*, from allowing the conſideration of final cauſes in phyſics, was, that the ſchool-philoſophers, and others, are apt to propoſe it too unwarily; as if there were no creature in the world, that was not ſolely, or chiefly deſign'd for the ſervice of man. And, indeed, I have ſeen a body of divinity, publiſh'd by a famous author, wherein he urges this argument for the annihilation of the world; "That ſince the world was made for the ſake of "man, in his travelling capacity; when once man is poſſeſs'd of his ever-"laſting ſtate of happineſs, or miſery, there will be no further uſe of the "world." The opinion that gives riſe to ſuch preſumptuous and unwarrantable expreſſions, was, as I conjecture from his objection, very ſhocking to *Des Cartes*; but the indiſcretion of men ought not to prejudice truth, which is not to be thrown away with the groundleſs conceits that ſome people have pinn'd upon it.

Since then, I cannot cloſe in either with the doctrine of the *Epicureans*, or *Carteſians*, I ſhall leave each party to maintain its reſpective opinion, and proceed to declare my own: but, to clear the way, 'tis neceſſary to premiſe a diſtinction.

Final cauſes, what they may ſignify.

In ſpeaking of the ends which the author of nature is ſaid to have in things corporeal, any of theſe four particulars may be ſignified.

Firſt, Some grand and general ends of the univerſe; ſuch as exerciſing and diſplaying the creator's wiſdom; the communication of his goodneſs, and the admiration and thanks due to him from his intelligent creatures, *&c.* And theſe ends, becauſe they regard the whole creation, I call the univerſal ends of God, or nature.

Secondly, In a more reſtrain'd ſenſe, the ends deſign'd in the number, fabric, ſituation, and motion of great maſſes of matter, that make large parts of the world; ſince 'tis very probable, that theſe bodies, ſuch as the ſun, moon, fix'd ſtars, and the terraqueous globe, were ſo framed, and placed, as not only to perſevere in their own preſent ſtate, but alſo to conduce to the univerſal ends of the creation, and the good of the whole, whereof they are conſiderable parts. Upon which accounts, theſe ends may be call'd coſmical, or ſyſtematical; as they regard the ſymmetry of the great ſyſtem of the world.

Thirdly, ends that more peculiarly concern the parts of animals, and, perhaps, plants too; or thoſe to which the particular parts of animals are deſtined for the welfare of the whole creature, conſider'd as an entire and diſtinct ſyſtem of organiz'd parts, deſign'd to preſerve himſelf, and propagate his ſpecies, upon that ſtage to which his ſtructure, and circumſtances determine him to act his part. And theſe ends, to diſtinguiſh them from others, may be call'd animal ends.

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Laſtly, The ſame expreſſion may ſignify another ſort of ends, which, becauſe they relate particularly to man, may be called human ends; and are thoſe aimed at by nature, where ſhe is ſaid to frame animals, vegetables, *&c.* for the uſe of man. And theſe ends may farther be diſtinguiſh'd into mental and corporeal; not only as man is an animal framed, like others, for his own preſervation, and the propagation of his ſpecies; but alſo, as he is made to have dominion over other animals, and works of nature, and fitted to make them ſubſervient to his purpoſes. This diſtinction being thus ſettled, I declare my diſſent, as well from the vulgar notion of final cauſes, which allows of none but thoſe we have call'd human ones; as from theirs who wholly reject them all.

'Tis an acknowledg'd principle of the *Carteſian* philoſophy, that there is always the ſame quantity of motion in the world *; becauſe, ſay they, there is no reaſon why God, who is immutable, ſhould, at the beginning of things, have given ſuch a quantity of motion to matter, as ſhould need to be afterwards augmented, or leſſen'd. But do not thoſe who employ this negative argument, take upon them to judge of the ends that God may have propos'd to himſelf in natural things? For, without ſuppoſing that they know what God deſign'd in ſetting matter in motion, 'tis hard for them to ſhew, that his deſign could not be beſt accompliſh'd by ſometimes adding to, and ſometimes taking from the quantity of motion, he originally

* Sir *Iſ. Newton* makes it evident, that there is not always the ſame quantity of motion in the world. "The *vis inertiæ*, ſays "that great philoſopher, is a paſſive principle, by which bodies perſiſt in their "motion, or reſt; receive motion in proportion to the force impreſſing it, and "reſiſt as much as they are reſiſted. By this "principle, alone, there could never have "been any motion in the world. Some "other principle was neceſſary for putting bodies into motion; and now they "are in motion, ſome other principle is "neceſſary for conſerving the motion. "For, from the various compoſition of "two motions, 'tis very certain, there is "not always the ſame quantity of motion in the world. For, if two globes "join'd by a ſlender rod, revolve about "their common centre of gravity, with "an uniform motion, while that centre "moves on uniformly, in a right line, "drawn in the plain of their circular "motion; the ſum of the motions of "the two globes, as often as the globes "are in a right line, deſcribed by their "common centre of gravity, will be bigger than the ſum of their motions, "when they are in a line perpendicular "to that right line. By this inſtance it "appears, that motion may be got or "loſt. But by reaſon of the tenacity of "fluids, and attrition of their parts, "and the weakneſs of elaſticity in ſolids, "motion is much more apt to be loſt, "than got; and is always upon the decay. For, bodies which are either abſolutely hard, or ſo ſoft as to be void "of elaſticity, will not rebound from "one another. Impenetrability makes "them only ſtop. If two equal bodies "meet directly *in vacuo*, they will, by "the laws of motion, ſtop where they "meet, and loſe all their motion, and "remain at reſt; unleſs they be elaſtic, "and receive new motion from their "ſpring. If they have ſo much elaſticity "as ſuffices to make them rebound with "$\frac{1}{4}$, or $\frac{1}{2}$, or $\frac{3}{4}$ of the force with which "they come together, they will loſe $\frac{3}{4}$, "or $\frac{1}{2}$, or $\frac{1}{4}$ of their motion. And this "may be tried, by letting two equal "pendulums fall againſt one another from "equal heights. If the pendulums be of "lead, or ſoft clay, they will loſe all, "or almoſt all their motions; if of elaſtic

originally communicated to matter. And, I think, it may be worth considering, whether by this doctrine of theirs, the *Cartesians* do not more take upon them, than other philosophers, to judge of God's designs. For, if a man be known for very wise, and to have various ways of compassing his several ends; he who, seeing some of those ways have a direct tendency to a rational end, shall conclude that end to be one of the number intended, thereby expresses more respect to that man, than he who should conclude, his end cannot be such; and that he has no other design knowable, except a certain general one. And, indeed, it seems more easy to know, that a particular end, for which an engine is proper, may, among others, be intended by the artificer, tho' never so skilful; than to know negatively, that he can have no other than such a certain end.

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And how will a *Cartesian* be sure, that among the many ends he grants God may have proposed to himself, in the production of his mundane creatures, one is not, that we, whom he has made intelligent Beings, and capable of admiring and praising him, should find just cause to do so, for the wisdom and goodness he has display'd in the world? Which attributes we could not well discern, and celebrate, unless we knew that the creatures were made for such uses, and that they are exceedingly well fitted to them. God's immutability is, indeed, alledg'd to prove, that the quantity

" stic bodies, they will lose all but what " they recover from their elasticity. If " it be said, that they can lose no motion " but what they communicate to other " bodies, the consequence is, that *in* " *vacuo* they can lose no motion; but, " when they meet, must go on, and penetrate one another's dimensions. If three " equal round vessels be fill'd, the one " with water, the other with oil, and " the third with melted pitch; and the " liquors be stirred about alike, to give " them a vortical motion; the pitch, by " its tenacity, will lose its motion quickly; the oil, being less tenacious, will " keep it longer; and the water, being " less tenacious, will keep it longest; but " yet will lose it in a short time. Whence " it is easy to understand, that if many " contiguous vortices of melted pitch, " were each of them as large, as some " suppose to revolve about the sun and " fix'd stars; yet these, and all their " parts, would, by their tenacity and stiffness, communicate their motion to one " another, till they all rested among " themselves. Vortices of oil, or water, " or some fluider matter, might continue longer in motion; but, unless that " matter were void of all tenacity, and attrition of parts, and communication of " motion, the motion would constantly " decay. Seeing, therefore, the variety of motion which we find in the " world, is always decreasing, there is " a necessity of conserving and recruiting it by active principles; such as are " the cause of gravity, by which planets " and comets keep their motions in their " orbs, and bodies acquire great motion " in falling; and the cause of fermentation, by which the heart and blood of " animals are kept in perpetual motion, " and heat; the inward parts of the earth " are constantly warm'd, and, in some " places, grow very hot; bodies burn and " shine; mountains take fire; the caverns of the earth are blown up; and " the sun continues violently hot, and " lucid, and warms all things by his light. " For, we meet with very little motion " in the world, besides what is owing to " these active principles. And, if it were " not for these principles, the bodies of " the earth, planets, comets, sun, and " all things in them, would grow cold, " and freeze, and become inactive masses; " and all putrefaction, generation, vegetation, and life, would cease; and the " planets and comets would not remain " in their orbs." *Newton. Optic.* p. 372, —375.

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To come then to the thing itſelf; M. *Des Cartes* objects, that 'tis a preſumption in man to pretend he is able to diſcover the ends that the Omniſcient propoſed to himſelf in the making of his creatures. Now I conſider, by way of anſwer, that there are two very different ways, wherein a man may pretend to know the ends of God in his viſible works; for he may either pretend to know only ſome of God's ends in ſome of his works, or he may pretend to know all his ends. He who preſumes to diſcover God's ends, in this latter ſenſe, will ſcarce be excuſed from a high preſumption, and no leſs a degree of folly. But to pretend to know God's ends, in the former ſenſe, is no preſumption; and, to take notice of them, rather a duty. For there are ſome things in nature, ſo curiouſly contrived, and ſo exquiſitely fitted for certain operations and uſes, that it ſeems little leſs than blindneſs in him who acknowledges a moſt wiſe author of things, not to conclude, that, tho' they may, perhaps, have been deſign'd for other uſes, yet they were alſo deſign'd for theſe. Thus he who conſiders the admirable fabric of the coats, humours, and muſcles of the eye, and how excellently all the parts are adapted to compoſe an organ of viſion, can ſcarce help confeſſing, that the author of nature intended it ſhould ſerve the animal, to which it belongs, to ſee with. The *Epicureans*, indeed, who believe the world to have been produced merely by a caſual concourſe of atoms, may have a kind of excuſe, which other philoſophers, who acknowledge a deity, want. Thus, the very ſuppoſition, for inſtance, that a man's eyes were made by chance, argues, that they need have no relation to a deſigning agent; and the uſe that a man makes of them, may be either caſual too, or at leaſt an effect of his knowledge, not of nature's. But when, upon the anatomical diſſection, and the optical conſideration of this part, we find it to be as exquiſitely fitted for an organ of ſight, as the beſt artificer could have framed a little engine, purpoſely deſign'd for the uſe of ſeeing; it ſounds very harſh to ſay, that an artificer, who is too intelligent to do things by chance, or to make a curious piece of work, without knowing to what purpoſe, ſhould not deſign it for an uſe to which 'tis wholly adapted.

'Tis poſſible, indeed, he may have deſign'd more uſes of it than one; and, perhaps, ſuch as we cannot find out: however, among its ſeveral uſes, this, to which we ſee it ſo admirably adapted, ſhould be thought one. And I ſee not how it magnifies God's wiſdom, or expreſſes our veneration of it, to exclude from the number of his ends, in framing human eyes, that moſt obvious and ready uſe, which, we are ſure, is made of them; and which they could not be better fitted for. Thus, ſuppoſe a countryman, brought into the garden of a mathematician, ſhould there ſee one of thoſe curious gnomonic inſtruments, that ſhew, at once, the place of the ſun in the zodiac, his declination from the equator, the day of the month, the length of the day, *&c.* it would, indeed, be preſumption in him, whilſt unacquainted both with mathematics, and the ſeveral intentions of the artiſt, to pretend himſelf able to diſcover all the ends for which ſo curious and elaborate a piece was framed: but, when he ſees it furniſh'd with a ſtile, with horary lines, with numbers, and, in ſhort, with all the requiſites of a ſun-dial; and manifeſtly perceives the ſhadow to mark, from time to time, the hour of the day; 'twould be no preſumption in him to conclude, it a ſun-dial; whatever other uſes it was deſign'd for.

And I demand of thoſe, who will not allow that any natural things are directed to ends knowable by men, whether, if the divine author of them had really deſign'd them for ſuch ends, the things themſelves are not ſo framed and directed, as in that caſe they ought to be? And whether the fabric and management of natural things do really countenance, or contradict our ſuppoſition?

For my part, I confeſs, I think it no diſparagement to the wiſdom of any agent whatſoever, to ſuppoſe his productions deſign'd for ſuch ends, among others, as they are excellently fitted for; unleſs it appear'd that ſuch ends were unworthy the wiſe agent. But that cannot be juſtly ſaid in our caſe; ſince 'tis not injurious to the divine author of things, to believe that ſome of the ends, to which he deſtined ſeveral of his corporeal works, were to exert and communicate his goodneſs; and to receive from his intelligent creatures an ardent love, a high admiration, and an obſequious gratitude for having diſplay'd ſo much wiſdom and beneficence in them.

And, indeed, I can by no means aſſent to the aſſertion of *Des Cartes*, that none of God's ends in his corporeal works are more manifeſt than others, but that all of them lie equally hid in the abyſs of the divine wiſdom; for there are many of his creatures, ſome of whoſe uſes are ſo manifeſt and obvious, that the generality of mankind have in all ages, and almoſt in all countries, acknowledg'd them. And as to what he adds, that, in phyſics, all things ought to be made out by certain and ſolid reaſons; I anſwer, firſt, that I ſee not how God's deſigning ſome of his works for particular uſes, amongſt others, is inconſiſtent with the phyſical accounts of their creation. Thus, a man may give a mechanical reaſon of the ſtructure of every wheel, and other part of a watch, and of their way of acting upon one another, when rightly ſet together; and, in ſhort,

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PHYSICS. of the contrivance and phenomena of the little machine; tho' he supposes that the artificer design'd it to shew the hours of the day, and tho' he has that use in view, whilst he explains the fabric and operations of the watch. Secondly, I readily admit, that, in physics, we should ground all we deliver upon as solid reasons as possible; but there is no necessity that those reasons should be always precisely physical, especially when we are treating not of any particular phenomenon, produced according to the course of nature established in our system; but of the first and general causes of the world itself: from which causes, I see not why the final uses that appear manifestly to have been design'd, should be excluded. And to me 'tis not very material, whether in physics, or any other science, a thing be prov'd by the peculiar principles of that science; provided it be firmly prov'd by the common grounds of reason. And here let me observe, that the fundamental tenets of *Des Cartes*'s own philosophy, are not prov'd by arguments strictly physical; but either by metaphysical ones, the more general dictates of reason, or the particular testimonies of experience. Thus, when, for instance, he truly ascribes to God all the motion that is found in matter, and consequently all the phenomena that occur in the world; he proves it not by a precise physical argument, that God, who is an immaterial agent, is the efficient cause of motion in matter; but only hence, that since motion does not belong to the essence and nature of matter, matter must owe its motion to some other Being: and then 'tis most agreeable to common reason to infer, that since matter cannot move itself, but must be mov'd by some other Being, that Being is immaterial; since, otherwise, some matter must, contrary to the hypothesis, be able to move itself. And when *Des Cartes* attempts to demonstrate, that there is always the same quantity of motion in the universe; and, consequently, that as much motion as one body communicates to another, it loses itself; he proves it by the immutability of God, which is not a strict physical argument, but rather a metaphysical one; as he had before prov'd, that God was the cause of all motion in matter, not by principles peculiar to physics, but by the common grounds of reason.

But tho' *Des Cartes* speaks very dogmatically and universally against mens pretending to know any final cause in natural things; yet I have met with a passage in his writings, where he seems to speak more cautiously, and opposes their opinion, who teach, that God had no other end in making the world, than that of being praised by men. His words are these. "'Tis self-evident, that we cannot know the ends of God, unless "he reveals them to us; and supposing it morally true, that all things "were made to the end that we should praise God for them; in which sense "it may be said, that the sun was made to give us light; yet it would be "very childish and absurd to say, metaphysically, that God, like a proud "man, had no other end in making the world, but to be prais'd by men "for it; and that the sun, which is vastly larger than the earth, was "wholly design'd to give us light, who inhabit but a very small part of the "surface of the terraqueous globe." But he, at the same time, delivers two

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or three other things, wherein I cannot acquiesce. As, first, that 'tis self-evident, we cannot know the ends of God, unless he himself reveals them to us. For what he here says to be self-evident, is not so to the generality of mankind, or even of philosophers; and, therefore, I think it ought not to be barely asserted, but prov'd. In the next place, he does not shew how we are obliged to praise God for his works, if God had no intentions that we should do so, or that we should discover none of the ends for which he design'd them. If a judicious man should see a book written in some language which he is an utter stranger to, and should know nothing farther of it than that 'twas composed by a very intelligent physician; he might, indeed, conclude, that it was not written by chance; but could not, by inspecting the book itself, be convinced, that it was form'd with great skill and kindness, and deserv'd his praise and thanks; since he was unable to know any of the particular ends to which the several chapters of it were design'd; and, consequently, could not see how well they were fitted to answer such ends. What *Des Cartes* says, that it is childish and absurd to think God had created the sun, which is vastly bigger than the earth, only to afford light to a small number of men, is somewhat invidiously propos'd; for there are few eminent writers who confine the utility of the sun directly to its affording light to man: and the small bulk of mankind ought not to make it seem absurd, that God may have had an especial eye to their welfare in framing that bright globe; since that most excellent machine, the human body, appears to be a more admirable thing than the sun: besides, the rational and immortal soul that resides in it, is incomparably more noble than a thousand masses of brute unorganized matter can be justly reputed. And since, in this very discourse, our author confesses that we may know the ends of God's corporeal works, if he reveal them to us; a christian philosopher may be allow'd to think the sun was made, among other purposes, to enlighten the earth, and for the use of man: since the scripture teaches, that not only the sun and moon, but the stars of the firmament, which *Des Cartes*, not improbably, thinks to be so many suns, were made to give light to the earth, and were divided to all the nations that inhabit it. Perhaps it were not rash to add, that we may know some of God's ends in things corporeal, without lessening our veneration of his wisdom; as we know some of them in other matters, of which the scripture furnishes us with a multitude of instances; as, particularly, *Job*'s sacrificing for his friends, and the declared uses of the *Urim* and *Thummim*: since God may, if he pleases, declare truths to men, and instruct them by his creatures and actions, as well as by his word. And thus he taught *Noah* by a rain-bow, and *Jonah* by a gourd and a worm; and regulated the encampents of the *Israelites* by the guidance of a cloud, and a fiery pillar. Lastly, *Des Cartes* objects, that those he dissents from, talk as if they look'd upon God as a proud man, who design'd his works only to be praised for them; but I know not whether he herein speaks so cautiously and reverently of God, as he ought, and elsewhere uses to do. For as humility, tho' a virtue in men, is

PHYSICS. is extremely remote from being any of God's perfections; fo that may be pride in a man, who is but a creature imperfect, dependant, and having nothing of his own, which wou'd be none at all in the creator, who is incapable of vice, and who may, if he pleafes, juftly propofe to himfelf his own glory for one of his ends, and both require and delight to be prais'd by men for his works; fince he is moft worthy of all praife, which it is their duty and reafonable fervice to pay him.

'Tis not without concern, that I find myfelf obliged thus to oppofe fome fentiments of M. *Des Cartes*, for whom I have, otherwife, a great efteem; for I am not at all of their mind, who think that author a favourer of atheifm, which, to my apprehenfion, would fubvert the very foundation of his own philofophy. But, judging that his doctrine, as generally underftood, as to the rejection of final caufes from the confideration of philofophers, tends to weaken one of the beft, and moft fuccefsful arguments for the being of a God, whom they ought to admire, praife, and reverence; I think it my duty to prefer an important truth before my refpect to any man, how eminent foever, who oppofes it; and to confider more the glory of the great author of nature, than the reputation of any of her interpreters.

And to confirm what I have hitherto deliver'd, 'tis remarkable that the excellent contrivance of the great fyftem of the world; and efpecially the curious fabric of animal bodies, with the ufes of their feveral fenfes and parts, have been the great motives, which in all ages and nations induced philofophers to acknowledge a Deity; and that the nobleft and moft intellectual praifes have been paid him by the priefts of nature; as proceding from the tranfcendent admiration rais'd in them by the attentive contemplation of the fabric of the univerfe, and of the curious ftructures of living creatures. And, therefore, it feems injurious to God, as well as unwarrantable in itfelf, to banifh from natural philofophy the confideration of final caufes; from which, chiefly, I cannot but fuppofe that God muft reap the honour due to thofe glorious attributes, his wifdom and his goodnefs. And, I confefs, I fomewhat wonder, that the *Cartefians*, who have generally, and fome of them with skill, maintain'd the exiftence of a Deity, fhould endeavour to make men throw away an argument, which the experience of all ages fhews to have been the moft fuccefsful, and in fome cafes the only prevalent one, to eftablifh, among philofophers, the belief and veneration of God. I know the *Cartefians* fay, that their mafter has demonftrated the exiftence of a God, by the innate idea men have of a Being infinitely perfect, who left it upon the human mind, as the mark of an artift imprefs'd upon his work; and that they alfo afcribe to God the creation of matter out of nothing, and its motion; which fufficiently argue the immenfity of his power. But tho' I would by no means weaken the argument drawn from the innate notion of God, fince many learned men have acquiefced in it; yet why may we not reafonably think, that God, who has taken care men fhould acknowledge him, may have alfo provided for the fecuring a truth of fo great confequence, by ftamping characters, or leaving impreſſions of his wiſdom and goodneſs, as well externally upon the world, as internally upon the mind? The bare contemplation of the fabric of the world, without conſidering any part of it as deſtin'd to certain uſes, may ſtill leave men unconvinced that there is any intelligent, wiſe, and provident author and diſpoſer of things; ſince we ſee, the *Ariſtotelians*, notwithſtanding the extent, ſymmetry, and beauty of the world, generally believed it to have been eternal. And tho' they did not allow it to have been created by God, yet becauſe they aſſerted that animals, plants, &c. act for ends; they were obliged to acknowledge a provident and powerful Being, that maintain'd and govern'd the univerſe, which they call'd *Nature*; tho' they often dangerouſly miſtook, by confounding this Being with God himſelf; and at other times ſpeaking of it as coordinate with him; as in that famous axiom of *Ariſtotle*, "God and nature "do nothing in vain." I acknowledge, therefore, that as I ſet a juſt value upon the *Carteſian* proof of God's exiſtence; ſo I ſee no reaſon why we ſhould give up any other ſtrong argument, that proves ſo noble and important a truth; eſpecially ſince the *Carteſian* way of conſidering the world, tho' very proper, indeed, to ſhew the greatneſs of God's power, does not, like the way I plead for, manifeſt that of his wiſdom and beneficence. For while a *Carteſian* only ſhews, that God is admirably wiſe, upon the ſuppoſition of his exiſtence; the ſame thing is manifeſted in our method, by the effect of a wiſdom as well as power, that cannot reaſonably be aſcribed to any other than a moſt intelligent and potent Being: ſo that by this means men may, at once, be brought to acknowledge God, to admire him, and to praiſe him.

SECT. II.

Whether final cauſes are to be expected in all, or only in ſome particular bodies?

BEFORE I proceed to the ſecond queſtion; Whether we may conſider final cauſes in all ſorts of bodies, or only in ſuch as are peculiarly qualified? I muſt divide natural bodies into animate and inanimate. The former term I here take in the larger ſenſe, to comprehend not only animals, but vegetables; tho' I ſhall not reject the opinion of thoſe, who are unwilling to allow plants ſuch a ſoul or life, as is confeſſedly granted to animals.

Of the inanimate bodies of the univerſe, the nobleſt, and thoſe which chiefly deſerve to be conſider'd on this occaſion, are the ſun, planets, &c. For when men ſaw thoſe vaſt luminous globes, and eſpecially the ſun, to move ſo conſtantly and regularly about the earth; diffuſing thereon both light and heat, and by their various revolutions to produce day and night, ſummer and winter, and the viciſſitudes of ſeaſons; they concluded, that theſe motions were guided by ſome divine Being, and deſign'd for the benefit of man. Whether this be a demonſtrative proof, I ſhall not now debate; but ſurely it may carry thus much of probability with it, that in caſe a man ſhall think the fabric of the celeſtial bodies was the production of an intelligent and divine agent, the irregular phenomena will not contradict him; ſince

there

PHYSICS. there is nothing in that fabric unworthy a divine author: and the motions and effects of the sun and stars may well allow us to think, that, among other purposes, they were made to illuminate the terrestrial globe, and bring heat and other benefits to the inhabitants of it. So that the contemplation of the heavens, which so manifestly declare the glory of God, may justly excite men both to admire his power and wisdom in them; and to return him thanks and praises for the great advantages we thence receive. On the other hand, it may be said, that in bodies inanimate, whether the portions of matter they consist of, be greater or less; the contrivance is very rarely so exquisite, but that the various motions and occursions of their parts may, without much improbability, be suspected capable, after many essays, to cast one another into several of those circumvolutions call'd by *Epicurus* συστροφάς, and by *Des Cartes*, *Vortices*; which being once made, may continue a long time, after the manner explain'd by the latter. But allowing this hypothesis to be possible; when I consider that we are not yet sufficiently acquainted with the true system of the world, nor usually sensible how small a part the terrestrial globe makes of the universe; I am apt to suspect, that men often assign the systematical ends and uses of the celestial bodies upon slender evidence; concluding them made and moved only for the service of the earth, and its inhabitants. And tho' I will not deny, that as we actually receive benefits by the established order and motion of the heavenly bodies, so one of the several uses intended by the author of nature, may particularly regard our species; yet perhaps 'twill not be easy to prove, that some of those bodies and motions are not rather intended for other purposes, than to cast their beams, or shed their influences upon the earth: at least, it seems probable to me, that the situations of the celestial bodies do not afford, by far, so clear and cogent arguments of the wisdom and design of the author of the world *, as do the bodies of animals and plants. And for my part, I think I see more of admirable contrivance in the muscles of a human body, than in what we yet know of the astronomical world: and the eye of a fly, as far as appears to us, seems to be a more curious piece of workmanship, than the body of the sun itself.

* As a noble instance of wisdom and design in the situation of the planets, take an observation of the great Sir *Is. Newton.* "Those planets, *says he*, have the greater density, *cæteris paribus*, which are placed nearest the sun. Thus *Jupiter* is more dense than *Saturn*, and the earth more dense than *Jupiter*. For it was necessary to place the planets at different distances from the sun, that each might receive a greater or a less degree of his heat, according to its density. If our earth were placed in the orbit of *Saturn*, the water of it would be frozen up; and if in the orbit of *Mercury*, 'twould presently exhale in vapour. For the sun's light, to which his heat is proportionable, is seven times more dense in the orbit of *Mercury*, than with us; and I have found, by the thermometer, that water will boil with a heat seven times as great as that of the summer's sun. But the matter of *Mercury* is, doubtless, fitted for heat; and therefore must be denser than that of our earth: since all dense matter requires a greater degree of heat, to perform the operations of nature." *Newton. Princip.* p. 372.

As

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As for other inanimate bodies, the matter whereof seems unorganiz'd, tho' there be no absurdity in supposing that these were also made for distinct and particular purposes, if not also for human uses; yet most of them are of such easy and unelaborate textures, as to make it seem possible, that various occursions and justlings of the parts of the universal matter might, at one time or other, have produced them: since we see, that in some chymical sublimations and crystallizations of mineral and metalline solutions, and other phenomena, where the motions appear not to be particularly guided and directed by an intelligent cause, bodies of as various textures as those are producible.

If it be said, that supposing chance, or any thing else, without the particular guidance of a wise and all-disposing cause, can make a finely-shaped stone, or a metalline substance, growing, as silver sometimes does, in the form of a plant; ought we not to allow, that chance may also make vegetables and animals? I can by no means grant the consequence. For there are some effects so easy, and readily to be produced, that they do not infer any knowledge or intention in their causes; whilst there are others that require such a number and conjunction of conspiring causes, and such a continued series of motions or operations, that 'tis utterly improbable they should be produced without the superintendency of a rational agent, wise and powerful enough to range and dispose the several intervening materials and instruments, in a manner necessary to produce such a remote effect: it will not therefore follow, that, if chance could produce a slight contexture in a few parts of matter, we may safely conclude it able to produce so exquisite and admirable a contrivance, as that of the body of an animal. What does it argue then, if sometimes, in sawing pieces of variegated marble, we happen to meet with the delineations or pictures of towns, woods, and men? For, besides that the delightfulness and rarity of such spectacles inclines the imagination to favour them, and to supply their defects; would any wise man conclude from hence, that a real town or wood, much less numbers of men, should be made by such a fortuitous concourse of matter? What comparison is there betwixt the workmanship that seems to be express'd in a few irregular lines, drawn upon a plane, with perhaps two or three colours luckily placed, and the great multitude of nerves, veins, arteries, ligaments, tendons, membranes, bones, glands, &c. required to compose a human body; every one of the numerous parts whereof, must have its determinate size, figure, consistence, situation, connexion, &c. and many, or all of them together, conspire to exercise and perform determinate functions and uses? And, indeed, I never saw any inanimate production of nature, or, as they speak, of chance, whose contrivance was comparable to that of the meanest limb of the most despicable animal; nay, there is infinitely more art express'd in the structure of a dog's foot, than in that of the famous clock at *Strasburg*.

Evident marks of design in the structure of the eyes, and other parts, of animals.

And tho' the paw of a dog be far inferior in its structure to the hand of a man, yet even this is inconsiderable, if compared to the eye; the several parts

 parts whereof, how numerous ſoever, could none of them be ſpared or alter'd, unleſs for the worſe; as may appear from thoſe many diſeaſes obſerv'd in that ſmall admirable organ: for each of thoſe diſeaſes conſiſts in this, that the humours, or other parts of the eye, are brought to a ſtate different from that whereto nature had deſign'd it. 'Twould be tedious to enumerate the ſeveral diſtempers of the eye; wherefore I ſhall only mention two or three particulars, wherein one would ſcarce imagine that a ſmall receſs from the natural ſtate could bring any conſiderable or ſenſible inconvenience. That which we call the pupil, is not a ſubſtantial part of the eye, but only an aperture of the uvea; almoſt perpetually changing its bigneſs, according to the different degrees of light the eye chances to be expoſed to. And therefore, it ſhould ſeem, that whilſt this hole remains open, it performs its office, by giving entrance to the incident rays of light. And yet I lately ſaw a woman, who, after a fever, was not able to dilate the *Pupillæ* of her eyes, as before; and tho' they were but very little narrower than ordinary, yet ſhe complain'd ſhe had thereby almoſt loſt her ſight. The preternatural conſtriction of the pupil is indeed no frequent diſtemper; yet phyſicians have given it a place among the ſtated diſeaſes of the eye. And, on the other ſide, tho' a competent wideneſs of the pupil be requiſite to a clear and diſtinct viſion; yet if its dilatation exceeds the due limits, there is thereby produced a diſtemper worſe than the former; becauſe it often almoſt totally deprives the patient of ſight. It may ſeem alſo but a ſlight circumſtance, that the tranſparent coats of the eye ſhould be devoid of colour; and of as little moment, that the cornea ſhould be very ſmooth, provided it remain tranſparent: yet when either of theſe circumſtances is wanting, the ſight may be greatly vitiated. Thus we ſee, that in the yellow jaundice, the adventitious colour wherewith the eye is ting'd, makes the patient think he ſees many objects yellow, which are of a contrary colour. And I know a gentleman, who having had a ſmall puſtule excited and broken upon the cornea, tho' the eye has long been cicatriz'd; yet a very little inequality or depreſſion, that ſtill remains upon the ſurface of that tranſparent coat, ſo affects him, that when he comes into the open fields, or the ſtreets, he for a pretty while thinks he ſees objects very glaring, and as many others as men uſually do ſtones at the bottom of clear water: which I impute to the want of uniformity in the refraction of thoſe reflected rays of light, that fall upon the unequal ſurface of the cornea. To give a further proof, that the eye was made with deſign, I ſhall here take notice of an obſervation or two, that do not occur in the diſſection of that part, and are therefore often unobſerv'd by anatomiſts.

I have found in frogs, that, beſides thoſe parts of the eye which they have in common with men, dogs, cats, and moſt other animals, there is a peculiar membrane or cartilage, which is not commonly perceiv'd; wherewith they can at pleaſure cover the eye, without too much hindering the ſight, becauſe the membrane is both tranſparent and ſtrong; ſo that it may paſs for a kind of moveable cornea, or occaſional ſafe-guard to the eye. In furniſhing frogs with this ſtrong membrane, the providence of nature ſeems very conſpicuous; for, they being amphibious creatures, deſign'd to paſs their lives in watery places, which, for the moſt part, abound in plants, endow'd with ſharp edges, or points; and the progreſſive motion of this animal, being not by walking, but by leaping, if his eyes were not provided with ſuch a caſe, he muſt either ſhut them, and ſo leap blind-fold; or, by leaving them open, muſt run the risk of having the cornea cut, prick'd, or otherwiſe offended; whilſt this membrane, as was ſaid, is like a kind of ſpectacle, that covers the eye, without taking away the ſight: and as ſoon as the occaſion for it is over, the animal withdraws it into a little cell, where it reſts till its uſe be again required. This membrane becomes viſible, by applying the point of a pin, or any ſuch ſharp thing, to the eye of a frog, whilſt his head is held ſteady; for to ſcreen his eye, he will preſently cover it therewith, and afterwards withdraw it, upon a removal of the ſuſpected danger. And becauſe many birds are deſtin'd to fly among the branches of trees, and buſhes; leſt, by this means, the prickles, twigs, leaves, *&c.* ſhould wound or offend their eyes, nature hath alſo given them ſuch another kind of horny membrane, as we find in frogs.

'Tis known that men, and moſt four-footed beaſts, and birds, have ſeveral muſcles belonging to their eyes, by the help of which, they can turn them any way; and ſo obvert the organ of ſenſe to the object. But nature, not having given that mobility to the eyes of flies, ſhe, in recompence, furniſhes them with a multitude of little protuberant parts, finely rang'd upon the convex of their large bulging eyes: ſo that by means of theſe numerous little ſtuds, numberleſs rays of light are reflected from objects placed on either hand, above, or beneath the level of the eye, and conveniently fall upon that organ, to render the objects they come from, viſible to the animal; and by the help of a good microſcope, and a clear light, ſome hundreds of theſe little round protuberances may be diſcover'd, curiouſly rang'd, on the convexity of a ſingle eye of an ordinary fleſh-fly.

But ſome may here pretend, that all organs of ſight ought to be conform'd to thoſe of men; theſe being the beſt, and moſt perfect. And, indeed, man being juſtly reputed the moſt perfect of animals, it is not ſtrange he ſhould preſume, that his eyes, and other parts of his body, are the beſt contriv'd of any to be found in nature; yet we cannot, from hence, ſafely conclude, that all eyes, which, in other animals, are of different ſtructures from thoſe of man, ſhould, for that reaſon, be defective. For, firſt, the admirable wiſdom diſplay'd by the author of nature, in fitting the eyes, and other organical parts of animals, to their ſeveral uſes, and the reſpective functions we ſee them exerciſe, may juſtly perſuade us, that the things whoſe reaſons and uſes we do not alike diſcern, are, nevertheleſs, moſt wiſely conſtituted: God having too much knowledge to do any thing unskilfully; and we having to much preſumption, if we ſuppoſe he had no ends in framing his creatures beyond the reach of our diſcovery. And, ſecondly, the eye is not to be conſider'd abſtractedly, as an inſtrument of

Physics. viſion; but as an inſtrument belonging to an animal, that is to make uſe of it in particular circumſtances. And, therefore, it ought highly to recommend the wiſdom and providence of the great author of things, that he has furniſh'd various ſpecies of animals, with organs of ſight, very differently framed and placed; ſince this diverſity nobly manifeſts his great providence and knowledge, in having ſo admirably ſuited the eyes of all animals, both to the reſt of their bodies, and to thoſe parts of the great theatre of the world, on which he deſign'd they ſhall live and act. Thus, tho' ſeveral beaſts, as horſes, oxen, *&c.* have their eyes furniſh'd with a ſeventh muſcle, beſides the ſix they ſhare in common with men; we muſt not conclude, that either the organs of viſion are imperfect in men, or that thoſe of theſe beaſts have ſomething ſuperfluous: for they being to feed, for the moſt part, on the graſs of the field; and that they may the better chuſe their food, being obliged to turn their eyes, for a long time together, downwards; the ſeventh muſcle excellently ſerves them for that purpoſe, by enabling them to continue unwearied by ſuch a poſture; whilſt man, who has no ſuch neceſſity of looking aſſiduouſly downwards, would be only incumber'd by a ſeventh muſcle.

On the other hand, the deficiency obſervable in the eyes of ſome animals, compared to thoſe of man, may be aſcribed to the juſt contrivance of nature, that, on moſt occaſions, declines doing what is unneceſſary to the particular ends ſhe aims at in the fabric of a part. Thus moles, being deſtin'd to live, for the moſt part, under ground, have their eyes ſo little, in proportion to their bodies, that 'tis commonly believ'd they have none at all; but, tho' I have found the contrary, yet their eyes are very different from thoſe of other four-footed beaſts; which need not be wonder'd at, conſidering that nature deſign'd theſe creatures to live under ground, where ſight is of no uſe; and where large eyes would be more expos'd to danger: beſides, their ſight, as dim as it is, ſerves them to perceive when they are no longer under ground; which ſeems to be the moſt neceſſary intelligence they want from their eyes.

'Tis obſerved, that the organs of viſion in a camelion, are of a very uncommon ſtructure; ſince, to omit other conſiderable peculiarities, his eyes often move independently on each other; ſo that, for inſtance, he can look directly forward with the right eye, and, at the ſame time, directly backward, towards his tail, with the left; or may turn the pupil of the former ſtrait upwards, whilſt the other is turn'd directly downwards. This peculiar power ſeems to have been granted him by providence, that as he is a very low animal, and deſtin'd to live, for the moſt part, in trees and buſhes, and there to feed chiefly on flies; he may perceive them, which way ſoever they chance to come, within the reach of his tongue, which, being of a great length, he ſuddenly darts out, and therewith catches his prey.

Many fiſh have the cryſtalline humour of their eyes almoſt ſpherical; and, conſequently, much rounder than it is found in man, and other terreſtrial animals. And this difference of figure, tho' it would be inconvenient for us, is very well accommodated to them; ſince they, living in the water, which, as a thicker medium, much more refracts the rays of light than the air, thro' which they paſs to our eyes; 'twas fit their cryſtalline humour ſhould be of that figure, to refract the rays already refracted by the water, and thereby make them converge, ſo as to paint the images of objects at the bottom of the eye. PHYSICS.

Should a perſon of curioſity ſurvey and conſider the various ſtructures of the organs of viſion, in different animals, and compare them with the other parts of the reſpective animal; the ſcene he is deſign'd to act on, and the uſes each of them is to make of his eyes, in the moſt ordinary circumſtances; he might, doubtleſs, offer a probable reaſon of the differences in thoſe organs, which, to a common obſerver, would ſeem to be errors, or defects in nature. Thus, tho' the pupil of the eye be oblong in horſes, oxen, and ſome other quadrupeds, as well as in cats; yet, in the former kinds of animals, it lies tranſverſly from the right ſide of the eye to the left, whilſt, in cats, its ſituation is perpendicular: for horſes and oxen, being uſually obliged to find their food on the ground, they the more conveniently receive the images of the graſs, &c. in a horizontal view, by having their *Pupillæ* tranſverſly placed; whilſt cats, being to live chiefly on rats and mice, which are animals that uſually climb, and run upon ſteep places; the moſt commodious ſituation of their pupil, for diſcovering and purſuing theſe objects, is the perpendicular. But, to proceed: the different ſtructures, and ſituations of the eye, in different animals, wonderfully ſhew a great variety in the skill of the divine author. And, indeed, if I might preſume to gueſs at any of God's ends, that are not manifeſt; I ſhould think that the delightful variety, we may obſerve, not only in animals themſelves, conſider'd as entire ſyſtems; but in thoſe parts of them which appear deſtin'd for the ſame function; as particularly, that the organ of viſion was deſign'd, at leaſt, among other ends, to diſplay the great creator's manifold wiſdom; and to ſhew, that his skill is not confined either to one ſort of living engines, or in their parts of the ſame kind, to the ſame contrivance; but is able to make a multitude of ſurprizing organs, all of them curious, and exquiſite in their kind, with regard to their different uſes.

To be able to frame clocks and watches, ſhips, mills, &c. manifeſts a far greater skill in an artificer, than the power of making but one of thoſe engines, how perfectly ſoever he contriv'd it. And the ſame ſuperiority of knowledge would be diſplay'd, by contriving engines of the ſame kind, or for the ſame purpoſes, after very different manners.

Thus weights are of great uſe and neceſſity in the famous clock of *Straſburg*; and therefore it recommends the contrivers of watches, that they give them a very little and portable bulk, which is wanting in the *Straſburg* machine; and ſtill more, that they can make a clock without weights, and ſubſtitute a ſpring to perform their office.

And thus tho' it ſeems abſolutely neceſſary, that an animal ſhould be furniſh'd with feathers, in order to fly; yet the wiſe creator hath ſhewn, that he is not

PHYSICS. not confined to apply them for that purpose; since a flying-fish is able to move a great way in the air. And the *Indies* have lately afforded a sort of flying squirrels, one whereof I saw, alive, at *Whitehall*. And tho' the flight of these is small, yet there is another kind of animal, without feathers, that long continues upon the wing, and that is the bat; some whereof I have seen but little less than hens: and was assur'd, by a credible eye-witness, that, in the kingdom of *Golconda*, he had seen much bigger.

This consideration is alone sufficient to justify the wisdom of the creator; who, being a most free, as well as a most wise agent, men ought not to find fault, if he think fit to recommend his wisdom, by displaying it in very different manners: tho' there are many cases wherein the less perfect fabric, or situation, of an eye, or other organical part, may be more convenient than the correspondent organ of man, to obtain the ends for which it was given to an animal design'd to act upon its respective stage, and live by its peculiar provision. Besides, an organical part may, in some animals, be intended for more uses than in others; and, therefore, require a different structure: as, in moles, the feet are differently framed, or situated, from those of other quadrupeds; because the chief use they are to make of them, is not to walk upon the ground, but to dig themselves ways under it; provident nature wisely suiting the fabric of the parts to the uses wherein they were to be employ'd: as, a mechanic employs one contrivance of his wheels, pinions, &c. when he makes a mill to be driven by water; and another, when it is to be mov'd by the wind. The camelion has a tongue, both peculiarly shaped, and of a length disproportionate to that of his body; because, as we before observed, he is to take his prey, which are flies, by shooting out that instrument; and could not often, otherwise, approach very near them, without frighting them away. And, in many cases, where this reflection does not so properly take place, we may observe, there is a wonderful compensation made for that which seems a defect in the parts of an animal, of a particular species, compar'd with the correspondent ones of an animal of some other species. Thus birds, which want teeth to chew their food, are not only furnish'd with hard bills to break it; and, birds of prey, with crooked ones to tear it; but, which is more considerable, have crops to prepare and soften it, and very strong muscular stomachs, to digest and grind it: in which work, they are usually assisted by gravel, and little stones, that they are led, by instinct, to swallow; great quantities whereof are often found in their stomachs.

Chance an imaginary Being. And let it be here observ'd, that chance is really no natural cause, or agent, but a creature of man's own making. For the things that are done in the corporeal world, are really done by the parts of the universal matter, acting and suffering according to the laws of motion, established by the author of nature. But we men, looking upon some of these parts as directed in their motions by God, or nature, and as disposed to the attainment of certain ends; if, by the intervention of other causes, that we are not aware of, an effect be produced, very different from that which

we

we suppos'd was intended; we say that effect was produced by chance: so that chance is indeed but a notion of ours, and signifies no more than that in our apprehensions the physical causes of an effect did not tend to the production of what they have, nevertheless, produced. And therefore I wonder, that the philosophers who preceded *Aristotle*, never treated of chance among natural causes.

And as some stones, of the most curious shapes, have embolden'd many favourers of *Epicurus* to set them in competition with those animals, or parts of animals, from their likeness whereto they have receiv'd their names; we ought to consider, that several learned men have, of late, made it very probable, that these stones were once really the animals, or those parts of them which they resemble, and were afterwards turn'd into stones by the supervention of some petrescent matter, or petrifying cause. And, allowing some of these sorts of stones to be the production of the mineral kingdom; yet it would not clearly follow, that they owe their shapes to chance, since 'tis no absurdity to admit seminal principles in the more elaborate sorts of fossils. However, I think it very injurious to make these productions rival the animals to which they are compared. For the shape, in which alone they and the animals agree, being an external thing, is not worthy to be mention'd, in comparison of that wherein they differ; the rude and slight texture of the best shaped stone being immensely inferior to the internal contrivance of an animal; which must consist of a multitude of parts of a determin'd figure, bulk, situation, &c. as is obvious to those who have seen dissections skilfully made. And 'tis not only in the firm and quiescent parts that this great internal difference between stones, and the animals they resemble, is to be found: there appears a far greater difference between a living animal, and a stone, than any the anatomical knife can shew us betwixt a dead one, and a stone, tho' ever so curiously figured. For there are numberless liquors, spirits, digestions, secretions, coagulations, motions of the whole body, of the limbs and other parts, which are lodg'd and perform'd in a living body, and are, perhaps, more admirable than the structure of the solid and quiescent parts themselves: so that, tho' a stone, in external appearance very like a shell-fish, were made by chance; yet from thence to conclude, that chance may make a living shell-fish, would be to argue worse than he who should pretend, that because an unskilful smith may make a hollow piece of metal like a watch-case, and fill it with some rude stuff, he must, therefore, be able to make a watch; for there is less difference betwixt the skill express'd in making the case of a watch and the movement, than in making a body like a shell, and the internal parts of a real fish: or, that because putrefaction and winds have sometimes made trees hollow, and blown them into the water, where they swim like boats; therefore the like causes may make a regular galley, according to the laws of naval architecture, man it, steer it, excite and guide all its motions to the best advantage, for the preservation, and various uses of the vessel. In short, if chance sometimes does strange things, 'tis in regard to what she herself, not to what nature uses to perform.

And

PHYSICS. And now, to give my thoughts upon the ſecond queſtion. 1. I think he naturaliſt may draw arguments from the ends and uſes of the parts of living bodies, provided he proceeds herein with due caution. 2. 'Tis my opinion, that the inanimate bodies here below, proceeding not from ſeminal principles, have but a more ſlight texture; ſuch as earths, liquors, flints, pebbles; and will not ſufficiently warrant reaſonings drawn from their ſuppoſed ends. 3. It ſeems to me, that the celeſtial bodies abundantly declare God's power and greatneſs, by the immenſity of their bulk, and, if the earth ſtand ſtill, the celerity of their motions; and alſo argue his wiſdom, and general providence, with regard to them: becauſe he has, for ſo many ages, kept ſuch a variety of vaſt vortices, or other maſſes of matter in motions immenſely rapid, without permitting them to deſtroy one another, or loſe their regularity. And I ſee no abſurdity in ſuppoſing that, among other uſes of the ſun, and of the ſtars, the ſervice of man might be intended; tho' I doubt whether, from the bare contemplation of the heavens, and their motions, it can be cogently inferr'd, that the chief end of them all, is to enlighten the earth, and bring benefits to the creatures that live upon it.

Revelation allows us to ſpeak more poſitively of final cauſes, than natural philoſophy.

Hitherto I ſuppoſe the naturaliſt to diſcourſe merely upon phyſical grounds; but if revelation be admitted, we may rationally believe more, and ſpeak leſs doubtfully of the ends of God, than bare philoſophy will warrant us to do. For if God is pleas'd to declare any thing to us concerning his intentions in the making of his creatures, we ought to believe it, tho' the conſideration of the things themſelves did not give us the leaſt ſuſpicion of it; which yet, in our caſe, they do. The ſcriptures expreſly teach us, that "God made the two great luminaries, the greater to rule "the day, and the leſſer to rule the night;" that "he made the ſtars al-"ſo, and ſet them in the firmament, or expanſum of heaven, to give "light upon the earth." And theſe are reckon'd among the uſes of theſe luminaries: "to divide the day from the night; and to be for ſigns, and "for ſeaſons, and for days and years." And *Moſes*, diſſuading the *Iſraelites* from worſhipping the ſun, the moon, and the ſtars, ſays, that "the Lord "had imparted them unto all nations under the heaven." And therefore thoſe *Carteſians*, who admit the authority of holy ſcripture, ſhould not reject the conſideration of ſuch final cauſes as revelation diſcovers to us; ſince 'tis certainly no preſumption to think we know God's ends, when he himſelf acquaints us with them: nor to believe that the ſun, tho', generally, eſteem'd a nobler body than the terreſtrial globe, was made, among other purpoſes, to enlighten it. 'Tis recorded, in the book of *Geneſis*, that God's deſign in making man, was, that he ſhould "ſub-"due the earth, and have dominion over the fiſh of the ſea, and over the "fowl of the air, and over the cattle, and over all the earth, and over "every living thing that moveth upon the earth." And the ſame book informs us, that, after the deluge, God "deliver'd all terreſtrial beaſts, "and fowl, and fiſh, and every moving thing that lives," into the hands of men, and intended that they ſhould eat animals; as, before the flood, he

had

had appointed them all the sorts of wholesom vegetables for their food. And, since God was pleased to appoint, that men should live on these creatures; it cannot be absurd to say, that, among other purposes to which he destin'd the sun, his shining upon the earth was one; since, without his light and heat, men could not provide for themselves; and neither those plants, that men and cattle must feed upon, could grow and ripen; nor, consequently, those animals that were to be their principal food, and serve them for other uses, could be sustain'd, and provided for. Many other texts might be here alledged to the same purpose; but I shall content myself to mention that of the royal prophet, when, speaking of man to his maker, he says, " Thou hast made him a little lower than the " angels, and hast crown'd him with glory and honour. Thou madest him " to have dominion over the works of thine hands, and hast put all things " under his feet."

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Indeed, if we consider only that visible part in man, his body; the smallness thereof may make it seem improbable, that portions of the universe, incomparably greater than he, should be intended for his service. But christians will not think this incredible, if they consider man, as he chiefly consists of a rational mind; which proceeds immediately from God, and is capable of knowing him, loving him, and being eternally happy with him. They who despise man, consider'd in this capacity, very little know the worth of a rational soul. But God, who is the best judge in this case, was pleas'd to consider men so much, that it gave *David* cause to admire, as we just now saw; and not only to form them in his image, at their first creation; but, when they had wilfully lost, and forfeited it, he vouchsafed to redeem them by the sufferings and death of his own son, who is incomparably more excellent than the whole world. And 'tis not incredible, that God should have intended many of his other works to be serviceable to man; since, by miraculous operations, he hath, sometimes, suspended the laws of nature; and, sometimes, over-ruled them for his sake: as appears by the flood, by the passage of the *Israelites* thro' the red-sea, and the river of *Jordan*; by the standing-still of the sun and moon, at *Joshuah*'s command; by the inefficacy of the burning fiery-furnace on *Daniel*'s three companions; and, in short, by the stupendous eclipse of the sun, at the crucifixion of the Messias.

And we shall the less scruple to admit, that such vast bodies, as the sun and moon may tend to serve mankind, if we consider, 'tis far from being a constant rule, that a thing more excellent cannot be employ'd for the good of a less excellent. Thus an angel was sent to relieve *Hagar* in the wilderness; another had regard to the life of a prophet's ass; and many more were employed on earth, in doing good offices to particular persons: and, of all the angels in general, the epistle to the *Hebrews* informs us, that " they are ministring spirits, sent forth to minister unto " them who shall be heirs of salvation."

SECT. III.

How inanimate bodies may act for ends, wherewith they are unacquainted.

PReparatory to the discussion of the third question, "Whether, and in "what sense, the acting for ends may be ascribed to an unintelligent, and inanimate body?" it will be necessary to clear the grand difficulty that has before, and ever since the time of *Aristotle*, perplexed those who allow the consideration of final causes in natural philosophy. This difficulty is obvious enough; for much the greater part of bodies being senseless, and, most of them, lifeless too; it seems inconceivable how they should constantly act for ends they are not capable of pre-designing, and, appositely employ means they have no knowledge to make choice of.

Aristotle, who expresly teaches, that nature does nothing in vain; and, rightly judg'd, that the actions of natural agents had a tendency to certain ends; takes no notice of this difficulty, but seems rather to shift it off than resolve it.

But, to consider the difficulty itself, there are two accounts on which the actions of natural agents may be said to tend to a certain end; as, either when the agent has a knowledge of that end, and acts with an intention to obtain it; or, when the action of the proximate agent is directed as it ought, to obtain an end, which, yet, is neither known, nor intended, by that proximate agent; but by a remoter, that is intelligent. In the former of these senses, I cannot admit, that any inanimate body acts for ends; since that pre-supposes the agent both to know the end he is to obtain, and to purpose to obtain it: things whereof inanimate bodies are incapable. And, to fancy with some, that they may have a knowledge, *sui generis*, as they speak; which, tho' confined to the actions proper to a particular kind of body, shall yet suffice to determine them to those actions, is, to offend against that rational, and receiv'd rule in philosophy, that Beings are not to be multiply'd, without there appears a necessity for them; and to introduce a sort of knowledge that seems unintelligible.

It remains, then, that I embrace the second sense, in which 'twas formerly said, natural things may work for an end; tho', in this case too, we must speak somewhat improperly: for the action may more justly be attributed to the remote intelligent, than to the immediate agent, which is but, as it were, the instrument of the other. Now, it appears to me, that the most wise, and powerful author of nature, whose piercing sight is able to penetrate the whole universe, and survey all the parts of it at once; did, originally, frame material things into such a system, and settle among them such laws of motion, as he judged suitable to the ends he proposed to himself in making the world. And as, by virtue of his vast and boundless intellect that he, at first, employ'd; he was able, not only to see the present state of things he had made, but to foreknow all the effects that particular bodies, peculiarly qualify'd, and acting according to the laws of motion, by him established, would, in particular circumstances, have on one another; so, by the same omniscient power, he he was able to contrive the whole fabric, and all its parts, in ſuch a manner, that, whilſt his general concourſe maintain'd the order eſtabliſhed, each part of this great engine ſhould, without either intention, or knowledge, as regularly and conſtantly act towards the attainment of the reſpective ends he deſign'd them for, as if themſelves really underſtood, and induſtriouſly proſecuted thoſe ends. Thus, in a well-made clock, the ſpring, the wheels, the balance, *&c.* tho' each of them act according to the impulſe it receives, and the determination that is given it by the other parts of the engine, without knowing what the neighbouring parts, or what themſelves perform; yet their tendencies are ſo determined, and over-ruled, and their motions ſo regulated, by the ſtructure of the machine, that the whole could not proceed more conveniently, nor better perform the office of a clock, if they knew they ought all to conſpire, and were deſign'd to make the index truly mark the hours. 'Tis not eaſy, indeed, to conceive, how one agent ſhould, by ſo ſimple an inſtrument as local motion, direct ſuch a multitude of others, as make up a world, to act with the ſame regularity, as if each of them went upon its own particular deſign, and yet all conſpire to obey the laws of nature. But if we conſider, that this great work is aſcribed to an omniſcient, and almighty agent, it will not appear incredible; eſpecially ſince 'tis manifeſt, that a multitude of bodies act as we have ſuppoſed: and that, if we will not aſcribe to God the direction, and ſuperintendency of the motions that are manifeſtly fitted for the attainment of ends, we muſt aſcribe them to nature; which will not leſſen, but increaſe the difficulty. And, upon viewing a great engine, wherein the works of many trades, and a great variety of other motions were perform'd by little puppets, that managed the tools of the ſeveral artificers; whilſt all of them were ſet on work by a ſingle ſpring, which communicated motions, regulated and determined by the particular ſtructure of the little ſtatues, *&c.* I could not think it impoſſible, that the great creator ſhould be able, by the motions and ſtructures of matter, to ſet very many partial, and ſubordinate, engines a-going. For 'twill not, I hope, be ſaid, that the multitude of theſe, any thing near ſurpaſſes that of thoſe which I ſaw in the hand of an illiterate tradeſman, ſo far as the narrow knowledge of that artificer is ſurpaſſed by the boundleſs underſtanding of an omniſcient artiſt. And God's wiſdom and skill is more diſplay'd in making ſo many various bodies act according to their particular deſignations, while they all conſpire to the general ends of the univerſe; than barely in cauſing bodies to act, appoſitely, for ends to themſelves unknown. For, if moving bodies be duly diſpoſed, and have a ſufficient connection, 'tis not difficult to direct a few of them to the attainment of an end propoſed by an underſtanding agent, tho' unknown to the immediate agents: as, anciently, among the *Jewiſh* husbandmen, and, at this day, in ſome parts of the eaſt, the ox, by treading the corn, ſeparates the grain from the ſtraw, as well as the labourer who thraſhes it, on purpoſe to make that ſeparation; and a

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This doctrine, however, is not inconsistent with the belief of any true miracle; for it supposes the ordinary and settled course of nature to be maintain'd; without at all denying, that the most free and powerful author of nature is able, whenever he thinks fit, to suspend, alter, or contradict those laws of motion, which he, alone, at first establish'd, and which requires his perpetual concourse to uphold.

SECT. IV.

How final causes are to be consider'd.

TO come to our last question, "With what cautions, final causes are to be consider'd by the naturalist?" I must observe, that the cases whereto it may relate, are so many and various, that I can only touch upon some few of them.

And to make way for what I am to offer, by a distinction; there are two ways of reasoning from the final causes of natural things, that ought not to be confounded. For sometimes men draw arguments from the use of bodies, that relate to the author of nature, and the general ends he is supposed to have intended in things corporeal: as when from the manifest usefulness of the eye, and all its parts, for vision, 'tis inferr'd that the eye was originally framed by a very intelligent Being, with a particular care that animals should be furnish'd with the fittest organ of so necessary a sense. And sometimes, also, men ground arguments upon the supposed ends of things, as to the peculiar nature of the things themselves; and conclude, that this affection of a natural body or part ought to be granted, or that deny'd; because by this, and not by that, or by this more than by that, the end design'd by nature may be best and most conveniently obtain'd. The latter sort of arguments I usually call purely physical; and those of the former may be stiled physico-theological, or, by a shorter name, metaphysical ones.

But in order to be the more clear upon this subject, I shall refer my thoughts of it to the five following propositions.

PROP. I.

As to the generality of the celestial bodies, it seems unsafe to draw arguments of their nature, on a supposition of particular ends, at least of the human ones, design'd by God in their formation.

As to the celestial bodies.

I am by all means for encouraging the contemplation of the celestial part of the world, and the shining globes that adorn it, and especially the sun and moon, in order to raise our admiration of the stupendous power and wisdom of him, who was able to frame such immense bodies; and, notwithstanding their vast bulk, and scarce conceivable rapidity, keep them for so many ages constant both to the lines and degrees of their motion, without interfering with one another. And, doubtless, we ought to return

return thanks and praises to the divine goodness, for having so placed the sun and moon, and determin'd the former, or else the earth, to move in particular lines, for the good of men, and other animals. And how disadvantageous would it have been to the inhabitants of the earth, if the luminaries had moved after a different manner? I dare not, however, affirm, that the sun, moon, and other celestial bodies, were made, solely, for the use of man; much less presume to prove one system of the world to be true, and another false; because the former is better fitted to the conveniency of mankind, or the other less suited, or perhaps altogether useless to that end. Thus men sometimes alledge, that the sun ought to be in perpetual motion, to shine upon the earth; because, as they fancy, 'tis more convenient for man, that those distant bodies, than that the earth, which is his habitation, should be kept in motion. But considering things as mere naturalists, it seems not very likely, that a most wise agent should have made such vast bodies as the sun and the fix'd stars, (especially, if we suppose them to move with such a prodigious rapidity, as vulgar astronomers assign them) chiefly to illuminate a little globe, that, without an hyperbole, is but a physical point in comparison of the immense celestial space; while those lights might as well illuminate the earth, if they were a thousand times less than they are; provided they were placed at a proportionable distance from it. And 'twill be very hard to say, what considerable use the terrestrial globe, or its inhabitants, derive from that multitude of celestial spheres which compose the milky way; since each of those stars is so far from being singly able to enlighten the earth, that *Aristotle*, and the generality of philosophers, for many ages, took the whole number of them for a meteor. And what light, or other known advantage, can the earth, or its inhabitants, receive from those many fix'd stars, that the telescope, only, can discover, among the six or seven conspicuous ones of the *Pleiades*; or among those, which the naked eye discovers in the belt or girdle of *Orion* *?

I foresee, it may be said, that these and the like celestial bodies are, at least, thus far useful to man, as to discover to him the power and greatness of the divine maker. And, indeed, tho' perhaps his wisdom appears as great to us men in the structure of a glow-worm, as in the disposition of the small stars, that make up the galaxy; yet the immensity of his power could

* It seems difficult to say, what advantages the earth can receive from the comets; yet late discoveries have led us to conjecture at their use: and that most sagacious philosopher, Sir *Is. Newton*, supposes that one final cause of comets, is, "to recruit the seas, and the moisture "in the planets, by a condensation of "their vapours, and exhalations there-"on. For as seas are absolutely necessa-"ry to the constitution of our earth, to "the end that the heat of the sun may "thence raise vapours in great plenty, "which gathering into clouds, fall down "in rain, and thereby water the earth, "and fit it for vegetation; or else being "condensed by the cold tops of the "mountains, distil down in springs and "rivers; so comets seem necessary to "preserve the waters in the planets, by "continually repairing, and supplying "with their exhalations and condensed "vapours, all that liquor, which being "wasted in vegetation and putrefaction, "is

PHYSICS. could not poſſibly be ſo well declared by leſs productions. Theſe arguments, nevertheleſs, are not purely phyſical, but of that ſort which I call phyſico-theological, whoſe inferences relate to the general deſigns of God in the univerſe, which I therefore ſtile coſmical ends; but do not reach to prove any thing about the determinate nature of particular bodies. And ſince the utmoſt that philoſophy teaches, is, that in general, the good of man was one of the ends deſign'd by God in framing the world as we ſee it, ; there may, by the ſame omniſcient author of nature, be other ends deſign'd of thoſe teleſcopical, and other ſmall or remote ſtars, whoſe uſes to us are doubtful or inconſiderable; to attain which ends, thoſe celeſtial bodies and motions may be admirably contrived and directed. And we being unable by mere reaſon to diſcover what thoſe ends are, tho' we have nothing near ſo great cauſe to think there may not be ſuch ends, as the infinite wiſdom of God gives us to think there may; 'tis preſumptuous to judge of the ſyſtem of the world, and of the deſign of vaſtly remote fix'd ſtars, by its being greater or leſs advantageous to us: eſpecially ſince tho' it were certain, that, among other uſes, God intended they ſhould in ſome ſort be ſerviceable to us; yet he has no way declared in what capacity, or to what degree, they ſhall be ſo. And therefore, if they prove ſerviceable in any meaſure; that is, ſo far as we know, all he deſign'd they ſhould be: and that itſelf, being an unmerited favour, deſerves our humble thanks. And it ſeems very likely, that God did not deſign equal advantages to all the parts of the earth, from the preſent ſyſtem of the univerſe; ſince the countries inhabited by the *Samoids* and *Nova-Zemblans*, and other nations that lie very near the arctic pole, want many conveniencies and advantages enjoy'd by the inhabitants of the temperate zones.

But tho' bare philoſophy does not favour this bold opinion, yet I know 'twill be pretended that revelation does. And I readily confeſs, that the terraqueous globe and its productions, eſpecially the plants and animals 'tis furniſh'd with, appear, from the ſcripture, to have been deſign'd for the uſe and benefit of man; who has, therefore, a right to employ any of them he is able to ſubdue; and that the ſun and moon were appointed by God to give light upon the earth, and be uſeful to all the nations that inhabit it: and that therefore the royal prophet had reaſon to exclaim,

" is turn'd into dry earth upon them. " For all vegetables wholly grow from " liquids, and afterwards, in great mea- " ſure, change into dry earth by putre- " faction; and a terreſtrial part perpe- " tually falls from putrefied fluids. " Hence the bulk of dry earth is con- " ſtantly upon the increaſe, and the " fluids, unleſs by ſome means ſupplied, " muſt continually decreaſe, and at length " be exhauſted. But the vapours of co- " mets being continually rarified in the " empty celeſtial ſpace, are diffuſed eve- " ry way therein, and ſpread thro' the " whole heavens; afterwards they are " gradually attracted by the planets, from " their principle of gravity, and proba- " bly intermix'd with their atmoſpheres. " I farther ſuſpect," ſays this great philoſopher, " that the ſpirit, which is the " leaſt, the moſt ſubtile, and the beſt part " of our air, and which is neceſſary to the " life of all things, comes principally " from the comets." *Newton. Princip.* p. 472, 473.

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" how manifold are thy works, O Lord! how wisely hast thou made them " all!" For he applies these expressions to the terraqueous globe and its inhabitants; as he elsewhere justly says, that " the heavens declare the " glory of God, and the firmament sheweth his handy-work." But these general declarations, tho' they properly excite our wonder and thankfulness, yet I fear, are not good topics, from whence to draw such physical conclusions in particular cases, as some learned men venture upon. For I do not remember 'tis any where declared in scripture, that the service of man was the only, or principal use, of all the celestial bodies. And this single consideration should make us very cautious, how we estimate the great system of the world by our conveniencies. And if it be said, that man alone has a rational faculty, whereby to refer the great works of God to the glory of their maker; I answer, that tho' this has been affirm'd by many, yet I have never found it prov'd. And I somewhat wonder, that divines should, on this occasion, overlook that passage in *Job*, which they generally interpret of the angels. For the question which God there puts to *Job*, may be justly applied to *Adam* himself; " Where wast thou, when " I laid the foundations of the earth? declare, if thou hast understanding. " When the morning-stars sang together, and all the sons of God shouted " for joy?" And, indeed, if we may presume to conjecture at such things, it seems reasonable to me, that God created the angels before the material world, that he might have intelligent Beings to pay him the just tribute of praises for so admirable a spectacle, as that of the rising world, or the beginning and progress of the creation. However, it appears from these words in *Job*, that before man was made, God wanted not intelligent spectators and applauders of his corporeal works. And since the angels are a nobler order of intellectual creatures than men, and no unconcern'd spectators at the works of God; how do we know, that in the system of that part of the heavens which is invisible to us without the help of telescopes, and in the plants, animals, or other furniture, whatever it be, of those particular stars that serve us men, barely for declarations of their maker's power; such intelligent spirits as angels may not discern as wise designs, and as admirable contrivances, as those manifested in forming and furnishing the earth? And in this case, God will lose none of the glory due to the divine attributes display'd in the fabric of the celestial part of the world, tho' the fix'd stars should not be principally design'd for the service of man.

But, secondly, 'tis yet more unsafe to form arguments upon the nature of particular inanimate bodies in the sublunary world, from the uses we think them design'd for.

And those that are terrestrial.

This will be made evident, by considering how little we know of the particular purposes of nature, in those terrestrial bodies, which being inorganical, cannot, by their curious structure, disclose to us the particular ends to which they were ordain'd. And their motions wanting that constancy and regularity of the celestial bodies, the caution given about drawing arguments from the astronomical system, will not, sure, be thought unfit to take place in clays, chalks, stones, *&c.* whose textures, compared with

PHYSICS. with thoſe of living creatures, are very ſimple, ſlight, and ſeldom more curious than may be made artificially, by diſſolving ſtones and metals in chymical menſtrua, and afterwards cryſtallizing the ſolutions. 'Tis true, revelation ſpeaks rather of God's having deſtin'd animals and vegetables, than other inanimate bodies, to the ſervice of men; yet there is no abſurdity, to conceive, in the general, the ſame to be one of the ends deſign'd by the author of nature, in making metals, ſtones, and thoſe other inanimate parts of the terreſtrial globe, that man is able to maſter and make uſe of. But 'tis very unlikely, that the internal part of the earth, which may, for ought we know, contain great varieties of foſſils, and other creatures, ſhould be made chiefly for the ſervice of men, from whoſe ſight they lie hid; and who will, in all probability, never deſcend to a thouſandth part of the depth requiſite to diſcover them, and do not ſo much as know what kind of bodies they are. And tho' it will not hence follow, that the terraqueous globe was made by chance, any more than that the other plants were ſo; becauſe the admirable ſtructure of plants and animals proves the exiſtence and providence of a moſt wiſe and powerful author of things, who may juſtly be ſuppoſed to have made nothing in vain, even among the inanimate portions of our globe; yet that thoſe inanimate portions were made for determinate ends, is more eaſily deduced from the knowledge we have, by other means, of their being produced by a wiſe author, than from the contemplation of thoſe bodies themſelves. And, perhaps, it is worth inquiry, whether ſome things may not be made, even by a wiſe agent, not out of a primary intention, but as productions that will naturally follow, upon the eſtabliſhment and preſervation of thoſe grand laws and rules of motion, that were moſt fit to be ſettled among things corporeal. And 'tis very poſſible, that, according to ſuch a general eſtabliſhment, many parts of the terreſtrial globe are ſo diſpoſed of, as not to be ſerviceable to men; becauſe the whole maſs could not otherwiſe be ſo well ſuited to the general ends of the univerſe. Thus, tho' the eclipſes of the ſun and moon be uſually unwelcome, and, if aſtrologers may be credited, often prejudicial to men; yet "the great former of all things" did not think fit to alter the tracts or lines of motion that he aſſign'd the luminaries, to avoid the eclipſes that muſt yearly enſue upon their moving in ſuch lines. Whence we alſo learn, that ſome phenomena may not belong to the primary intention of nature; but are only the neceſſary conſequences and effects of the primitive conſtitution of the world, and the univerſal laws of motion.

But if it be here demanded, to what end the deep and hidden parts of the terraqueous globe, and the teleſcopic ſtars of the firmament, were made, if not for the uſe of man? I freely acknowledge, that I cannot tell: and perhaps ſuch an anſwer may be more expreſſive of the profound reverence we owe the great author of nature, than their opinion, who would have all theſe made for the ſole uſe of man.

PROP.

PROP. II.

'Tis often allowable for a naturalist, from the manifest and appofite uses of the parts of animal bodies, to collect some of the particular ends for which the creator design'd them: and in some cases we may, from the known nature and structure of the parts, draw probable conjectures about the particular offices of them.

To obviate mistakes, it must be here observ'd, that I speak only of those ends and uses of the parts of an animal, that relate to the welfare and propagation of the animal itself, and which, therefore, I call animal ends; tho' I do not thereby deny any declaration made in the holy scriptures, that God design'd the entire animals, as well as their parts, to be serviceable many ways to man. This premised, I come to consider distinctly the two parts of the proposition.

And, first, there is no work of nature known to us, wherein the consideration of final causes may so justly take place, as in the structure of animal bodies. For my own part, I confess, that when I assist at a skilful dissection, I cannot but wonder there should be philosophers, who ascribe the admirable contrivance of a human body to blind chance. The *Stoic*, who in *Cicero*, ask'd an *Epicurean* why chance did not make palaces, and erect other buildings, seems to have proposed a pertinent question. But the most commodious houses are far less curious structures than the human machine: for the materials of a palace are few, in comparison to the parts of an animal body; and their disposition exceeding slight, compared to the curious and elaborate contrivance of the numerous solids and fluids of the human structure: the former whereof, alone, are above some hundreds; yet in every one of these parts, the bulk, figure, consistence, texture, situation, connexion, and aptness for motion, are the most commodious that can possibly be devised; whilst all of them are wonderfully symmetrical, both to one another, and the whole body. And this number of parts is so artificially contrived and set together, that tho' no room be lost, many of them, at the same time, exercise very different motions, while each moves freely, and rather promotes than hinders the motion of others.

Hurts or diseases may shew how excellently all the parts of our bodies are contrived, in order to our welfare. For if even a finger be swell'd, displaced, kept in a wrong posture by contractions, have its continuity violated, its tone chang'd by strains or contusions, its sense or motion taken away, its membranes fretted by sharp humours, or its motions disorder'd by convulsions; we quickly find, how commodiously the parts affected were framed or disposed; their natural figure, connexion, tone, *&c.* being now alter'd.

The eye, to single out again that part for an instance, is so exquisitely adapted for seeing, and so little fitted for almost any other office in the body; and that use is so necessary for the welfare of the animal, that it may well be doubted, whether any considering man can really think it not designed for that use. The six or seven muscles, which move the whole ball of

PHYSICS. of the eye upwards, downwards, to the right-hand, to the left, and to various oblique positions; and the several coats and humours that make up the organ, have not only their magnitude, figure, consistence, situation, and connexion, admirably adapted to that end; but the transparency of the cornea, and the three humours, the opacity of the uvea, the semi-opacity of the retina, and the several motions of the parts of the eye, which are requisite to receive, transmit, refract, and dispose the visual rays, that come from the object, after the manner necessary to make the liveliest picture of it in the bottom of the eye; wonderfully conspire to compleat this matchless instrument of vision: whence we may as properly conclude, that an eye, as that a telescope, is made to view objects with. But, in that admirable perforation of the uvea, which we call the pupil, nature has greatly exceeded art. For tho' we are obliged to employ opake bodies, with several circular apertures, to the object-glasses of telescopes, that some may let in less light, and others more, according as the objects require to be illumin'd; that part of the uvea, which is suspended in the aqueous humour, is an aperture that widens and contracts it self, in an instant, according to the exigency of the object.

But for the uses of the several parts of the eye, I refer my reader to *Scheiner*'s *Oculus*, and *Des Cartes*'s *Dioptrics*; whence it will appear, that, in forming this part, nature not only acted with design, but with so great skill in optics, that a more than ordinary acquaintance with that science is necessary to understand the wisdom of the several contrivances; which, perhaps, no degree of skill whatever in it, would enable a man to alter for the better.

'Twere tedious to mention other parts of the body, that manifestly appear to have been ordain'd to certain uses. The books of anatomists are full of passages to this purpose: of which I shall only say in general, that tho' what they deliver is sufficient to shew all the parts of the body to be the effects of an intelligent cause; yet, unless their descriptions, and reflections, be improved by mathematics, mechanics, and chymistry, we shall have but an imperfect notion how intelligent that cause is; or how much wisdom is display'd in the structure of a human body, and its several parts.

I know 'tis objected, by the *Epicureans*, that the parts of animals were first made, and their uses afterwards discover'd, by the sagacity of men. But this is a sophistical objection. For, first, many of the internal parts perform their functions, without our having any knowledge of their structure, or situation; so far are they from being applied to such uses by our sagacity. And as for the limbs, and other parts, which we move at pleasure, 'tis true, they cannot be employ'd to their respective uses, till actually form'd; nevertheless, they might be originally so form'd, as, in due time, to be fit for such uses. And, in effect, we see that a chick is furnish'd with compleat eyes and wings, before it be hatch'd; tho' whilst inclosed in the egg, it can make no use of them, either to see, or fly. And why was it, do the *Epicureans* think, that nature provided a whole sett of temporary

temporary parts for pregnant females, and animals in the womb; which, when they are come into a freer ftate, partly fhrink away of themfelves, and partly turn to a ligament, fitted no longer for the former, but for a more feafonable ufe? And 'tis to be noted, that thefe umbilical veffels, and the placenta to which they are faften'd, is of no neceffity or ufe to the female before conception; and therefore, thofe temporary parts, appear to have been defign'd by nature for the propagation of the fpecies. PHYSICS.

And tho' the fagacity of men, may have found out fome ufes of the particular parts of their bodies, which feem not to have been primarily intended by the author of nature; yet this does not prove, that thofe ufes were undefign'd: for the prefcience, and goodnefs of God, are fufficient to render it probable, that he who gave man both the limbs of his body, and the endowments of his mind, did both forefee what ufes men might, according to their fagacities, and emergencies, make of thefe parts; and fo contrive the parts, that they fhould be applicable to fuch ufes.

And, we may now proceed to the latter part of our propofition; which afferts, that, in fome cafes, from the known ends of nature, as well as from the ftructure of the parts, probable conjectures may be form'd about the particular offices of them.

This could not have been feafonably fpoken to before; becaufe the arguments that were founded on the ufes of the parts of animals, fuppofe thofe parts to have been deftin'd to particular ufes, knowable by us; and that the feveral parts of the body were contrived as wifely, and commodioufly, as men are able to devife, in order to the ends of nature: which muft always be underftood to have united in her defigns, the ufes of the parts, and the welfare of the whole.

And, indeed, if we confider how admirable a fitnefs there is in the parts of a human body, to thofe particular ends we can difcover them to have been defign'd for; it feems allowable to conjecture, that fuch a part was not primarily defign'd to fuch an ufe, if it is, on the account of its ftructure, or otherwife, lefs fitted for it than the conftant wifdom of nature feems to require; efpecially if there be any other parts, by which the office may be more commodioufly performed. And, on the other fide, it feems probable, that fuch a part was deftin'd to fuch an ufe, if the ufe itfelf appear to be neceffary, and the part better fitted for it than any other.

Thus, tho' anatomical and optical writers, for many ages, unanimoufly concluded, the cryftalline humour to be the principal feat of vifion; yet the induftrious *Scheiner* juftly rejects that receiv'd opinion, upon fhewing, that it fuits not with the skill and providence of nature, that it fhould be fo, fince it wants many requifite qualifications for that purpofe; and efpecially fince moft of thefe are to be found in the retina. And, I remember, upon asking our famous *Harvey*, what induced him to think of a circulation of the blood; he faid, that obferving the valves in the veins of many parts of the body, fo placed, as to give free paffage to the blood towards the heart; but to oppofe the paffage of the venal blood, the contrary way; he

PHYSICS. imagin'd that so provident a cause, as nature, had not thus placed so many valves without design: and as no design seem'd more probable than that, since the blood could not well, because of the interposing valves, be sent by the veins, to the limbs, it should be sent thro' the arteries, and return thro' the veins, whose valves did not oppose its course that way.

Thus, tho' the ancient anatomists, and physicians, believ'd the parts were nourished by the venal blood; the modern writers teach them to be nourished by the blood, in its passage thro' the arteries. Not that they think the blood, which runs thro' the veins, altogether unfit to supply the parts with that vital liquor; but because they judge the veins to be less fit for this purpose, than the arteries; into the latter whereof the blood comes immediately from the left ventricle of the heart, agitated, and spirituous, and, by a brisk impulse, better suited to answer this end.

But the writings of physicians, and anatomists, being filled with instances of this kind, I forbear to mention any more.

PROP. III.

It is rational, from the manifest fitness of some things, to cosmical, or animal ends, to infer, that they were thereto ordained by an intelligent agent.

Care seems to have been taken, that the body of an animal should be provided not only with all things that are ordinarily necessary, and convenient; but with some super-abundant provision against accidents. Thus, tho' a man may live, and propagate his species, after the loss of an eye; yet nature furnishes us with two, that, in case one be destroy'd, the other may suffice for vision. And the like may be said of the ears. In short, nature has furnish'd men with double parts of the same kind, where it is highly useful, and may be permitted, without prejudice, to the rest of the body. And this is the more considerable, because, in other parts, nature appears to husband things so, as to avoid doing what is superfluous. Thus, within the skull, some vessels, that would, in other parts of the body, have double coats, are very thin; the skull being ordinarily sufficient to defend them from external injuries.

Another argument, that nature acts with design, about animals, may be drawn from what anatomists observe of those parts of the womb, or the fœtus, that are to be found but at certain times, when there is need of them; and not at others, when they would be useless. Thus, when a woman is with child, the *Vasa umbilicalia* are produced to be canals, either for the blood, or alimental juice, and spirits, that then ought to pass between the womb and the fœtus, by means of the placenta. And tho', as long as the child continues in the womb, these temporary parts continue with him; yet, as soon as he comes into the world, the umbilical vessels, particularly the two arteries, and the vein, together with the membranes they are wrapp'd up in, with the chorion, and the amnios, that involve the fœtus, are thrown off, as unnecessary, and expell'd in the after-birth; there remaining only that part of the umbilical vessels that

lies

lies within the child's abdomen, between the navel and the liver; where its uſe is conſiderable, tho' new; for it ſerves now no longer to convey blood, or an alimental liquor, to and fro, but degenerates into a ligament.

Thus, alſo, the *Foramen ovale*, gives paſſage to the blood from the right ventricle of the heart to the left, that the circulation of it may be maintain'd; for it cannot in the embryo, as in a born child, paſs thro' the veſſels of the lungs, from one of the ventricles to the other: whence this contrivance ſeems to be an expedient that nature employs, till the fœtus is excluded; when that temporary conformation is obliterated. For the child, now breathing the free air, is in a condition to make the blood circulate thro' the pulmonic veſſels, according to the primary intention of nature. From which, and the like inſtances, we may infer, that theſe temporary parts were framed by a fore-knowing, as well as a deſigning agent; who intended they ſhould ſerve for ſuch a purpoſe, and then be laid aſide: for 'tis uttterly improbable, that an undeſigning agent ſhould ſo appoſitely, and exquiſitely, frame ſcaffolds for a future building, if he did not, before-hand, deſtine both the one and the other, to concur to the ſame ultimate effect.

Another argument, for our preſent purpoſe, may be drawn from the conſideration of what, in animals, is commonly call'd inſtinct: which, in ſome caſes, more directly regards the welfare of the creature; in others, the propagation of the ſpecies; and, ſometimes again, reſpects both. The writers of voyages, and natural hiſtory, recount ſtrange inſtances of the inſtinct obſervable in certain animals. But we need not lay the ſtreſs of our argument upon dubious, or ſuſpected relations; ſince what I have met with, in authors of good authority, or receiv'd from the mouths of credible travellers, may ſerve my preſent turn; eſpecially, if we may take the word inſtinct in a latitude, ſo as to comprize thoſe untaught methods, and expedients, that are made uſe of by ſome animals, to avoid dangers, provide for their future neceſſities, or to catch their prey.

Surprizing things are related, not only by poets, but by more credible writers, about the ſagacity and government of bees; in point, both of œconomy, and politics. But tho' I ſhall not build any thing upon ſuſpected authorities, yet, having long kept a tranſparent hive, and thereby gained the opportunity to make frequent obſervations of the actions of theſe little animals; I confeſs, I diſcover'd ſome things that I did not believe before: which induced me to look upon them, as very fit inſtances of creatures endow'd with natural inſtinct and providence. For, 'twere hard for a mathematician, in contriving ſo many cells as bees make in the area of one of their combs, to husband ſo little ſpace more skilfully than theſe animals. And they not only carefully, and ſeaſonably, lay up their honey, to ſerve them all the winter; but, curiouſly, cloſe the particular cells with covers of wax, that keep the included liquor from ſpilling, and from external injuries. I do not here mention the prognoſtication of weather that may be made in the morning, by their keeping within their hives, PHYSICS. hives, or flying early abroad, to furnish themselves with wax, or honey; or, by their unexpected return before a storm: because, I suspect, that these things are not so much the effects of instinct, as of a tenderness, and quickness of sense; something analogous whereto, may be seen in a good weather-glass; and is, also, to be found in many wounded, and valetudinary persons, who are affected with such beginning alterations in the air, as other men perceive not. But, among the peculiarities to be observ'd in the conduct of bees; 'tis very remarkable, that, after a fight, they take up the dead which lay on the ground, and, as I have observ'd, fly away with them far from their hive.

Another obvious instance of the instinct that nature has given to some despicable insects, may be taken from ants. For, 'tis known, that these little creatures do, in the summer, hoard up grains of corn against the winter. And their sagacity is the more considerable, if it be true, what many learned men affirm, that they eat off the germen of the grains they lay up, lest the moisture of the earth, exposed to the rains, should make them sprout. But, whatever become of this tradition, these insects perform some other actions, greatly resembling those proceeding from sagacity and industry.

The natural skill of spiders, in weaving their webs, that are so fitly contrived, both to catch their prey, and give them immediate notice of its being caught, is a thing which, if it were not familiar, would be look'd upon as admirable. And this skill is not, as some imagine, an effect of imitating their parents; for if the eggs be taken away, and enclosed in a glass; when they come to be hatch'd by the heat of the sun, the little creatures will, immediately, fall to spinning in the glass itself; as was related to me by an eminent mathematician, who made the experiment. And I saw the less reason to distrust it, because having, by an external heat, hatch'd many eggs of silk-worms, in a place where there had not been any of a long time before, nor, probably, ever, till then; yet the worms, produced by these eggs, did, in autumn, of their own accord, climb up to such convenient places as I had prepared for them, and there weave those curious oval prisons, wherein they enclose themselves, and which are unravel'd into extremely fine silk.

But this provident industry is not confined to insects, for 'tis to be found in many of the greater animals; particularly in the beaver; a creature whereof, indeed, many fabulous stories are related: yet sober, and judicious persons, who were either born, or lived in *New-England*, where these animals abound, have assured me, that the beavers, with their sharp teeth, cut pieces of wood, and fit them to their purpose; that, by joining their labours, they lay these together, so as to build themselves strong winter-houses; in which there is, sometimes, a kind of second story for the inhabitants to retire to, when the water chances to overflow; that, for these houses, they chuse a very convenient situation, just by some river, or other water, that can furnish them with fish; and that the hole belonging to each house, is placed just by the water, that

they

they may, immediately, flounce into it, and ſo ſave themſelves, when their houſes are attack'd. And, to facilitate their ſwimming, and the catching of their prey in the water; nature has given them two feet, not made like thoſe of dogs, or cats, or in the manner of their other two; but furniſh'd with broad membranes betwixt their toes, like the feet of geeſe, ducks, and other aquatic animals, that are to uſe them as oars, to thruſt away the water, and facilitate their motions.

The various arts employ'd by animals of different kinds, about the materials, the conſtruction, and the ſituation of their neſts, is uſually remarkable, and, ſometimes, wonderful. Of this skill, we have many eminent inſtances; but 'tis particularly remarkable, that, in countries abounding with apes and monkeys, creatures very greedy of birds eggs, there is a ſort of bird, whoſe eggs they, peculiarly, affect, that hang their neſts near the end of ſome long flexible branches, which reach over the water; and, by that means, avoid their enemies, who cannot ſwim.

In the neſts of waſps, which they often, for greater ſecurity, make under ground; I have obſerv'd a very curious, and artificial ſtructure, to conceal, and ſhelter their young ones, till they are ready to fly.

But the inſtinct that nature has planted in animals, for their own preſervation, is much inferior to that providence ſhe has furniſhed them with, for the propagation of their ſpecies.

There are many remarkable things to be met with in the neſts of ſeveral birds, both as to the materials, the ſtructure, and the ſituation of the places wherein they are built. I have ſeen neſts, and, particularly, ſome made in the *Indies*, which would raiſe a man's wonder how the birds ſhould find ſuch odd, but commodious materials to build with. There are birds in the eaſt, which make their neſts of a white ſubſtance, that looks almoſt like iſing-glaſs, diſſoluble in liquors, and ſo very well taſted, that it makes the chief ſauce uſed in the ſouthern parts of *India*. The ſtructure, alſo, of the neſts of ſeveral birds, both as to their figure, magnitude, and accommodations, wherewith they are furniſh'd, for warmth and ſoftneſs, may deſerve the applauſe of mathematicians; eſpecially if it be conſider'd, that theſe little untaught architects had no tools to make their curious buildings with, except their bills and feet. Much more foreſight, however, appears in the ſituation of the place, that ſome birds make choice of, to build their neſts in; as may be obſerv'd, not only in the pendulous neſts of ſwallows, and the ſecret ones of ſome *European* birds; but very conſpicuouſly in the hanging neſts that we lately mention'd, to be ſo oddly placed by ſome birds, to ſecure their eggs from apes and monkeys; and by the ſituation of the neſts uſed for ſauce as we juſt now related, to be found only upon high and ſteep rocks: which are ſo faſten'd to the concave parts of them, that look downwards, and, commonly, hang directly over the ſea; that there is no getting them, without much trouble, and danger: upon which account, as well as that of their delicacy, they are very dear, even in the *Eaſt-Indies*, where they are found.

The

PHYSICS. The like care to contrive their nefts advantageoufly, and make them in fecure places, is vifible in infects; as may be obferv'd in the fubterraneal nefts of the wafps, formerly mention'd, and in the eggs of fnails, which I have fometimes found hid under-ground; and have hatch'd in glaffes furnifhed with the fame earth wherein they were found.

If I fhould here fet to view the feveral effects and arguments of the wonderful providence, that the moft wife author of nature exercifes about the propagation of animals, by diftinguifhing them into male and female; by furnifhing both fexes with mutual appetites and organs exquifitely adapted to the increafe of their kind; by the admirable formation of the fœtus in the womb, without the female's knowing how it is perform'd; by the ftrange fubtilties and courage, that feveral creatures, either oviparous or viviparous, have, to hide and defend their young; by the full provifion that is made for the nourifhment of the fœtus, and the welfare of the female after fhe has brought forth; and the like: I might much enrich and adorn my argument. But inftead of purfuing a fpeculation, that would lead me too far, I fhall look back upon the intimation I lately gave, that even thofe meaner parts of animals, which feem to have been framed with the leaft care and contrivance, are yet worthy of their author. For tho' the teeth be fome of the leaft elaborate parts of the human body, yet even thefe afford numerous phenomena applicable to our prefent purpofe: but I fhall only tranfiently confider a few of them.

And firft, 'tis remarkable, that tho' when a man comes to his full ftature, all the other bones of the body ceafe to grow, the teeth continue to increafe, in length, during his whole life. This growth of the teeth appears not only by their continuing for many years of the fame length, but by the unfightly fhooting of one tooth, when that which was oppofite to it in the other jaw is wanting; whence it has liberty to fprout without oppofition. Of this difference in point of growth betwixt the teeth and other bones of the body, what reafon can be fo properly affign'd, as its being intended to repair the daily wafte of fubftance, proceeding from the frequent attritions of the upper and lower jaw in maftication?

2. Tho' the bones of the body are generally invefted with a very thin fenfible membrane call'd *Periofteum*; that part of each tooth, which is not cover'd by the gums, wants it; which would be fubject to very frequent and painful compreffions and lacerations.

3. To enable the teeth to break and make comminutions of the more folid kind of aliment, nature has providently framed them of a clofer and harder fubftance than almoft any other bones in the body; tho' thefe are fo numerous, that anatomifts reckon above three hundred of them. And I have met with it in authors of good credit, that fome mens teeth have been fo hard, as, when ftruck againft another fit body, to produce fparks of fire.

4. That thefe bones, whofe ufe is fo great, may themfelves be continually nourifh'd and fupplied; tho' they are fet in bone, the wife author of things has admirably contriv'd a cavity on each fide of the jaw-bone, wherein

wherein are lodg'd an artery, a vein, and a nerve; which, thro' lesser cavities, send their twigs to each particular tooth. PHYSICS.

5. As infants are design'd, by nature, to feed, for a considerable time, on milk, they remain long without teeth; whilst several animals, which are often early reduced to seek aliment, that is neither fluid nor soft, come into the world with teeth already form'd in their jaws.

6. The bony substance appointed for the comminution of the aliment, ought not, for several reasons, to be in either jaw entire, or all of one piece; and therefore nature has providently made, for that use, a competent number of distinct bones in either jaw. And because men may have occasion to feed often upon very different sorts of aliment; and because usually the same aliment may require different preparations in the mouth, to facilitate the digestion of it in the stomach; nature has provided men with two rows of teeth, for the most part equal in number, and corresponding to each other, yet of different shapes, for different uses; the fore-teeth are broader, and have a kind of edge, to cut the more yielding sort of aliment, whence they are call'd *Incisores*; others being stronger, and more fitly shaped to tear the tougher food, are term'd *Canini*. There is also a third sort, whose principal office it is to grind the aliment cut or torn by the others: and, for this purpose, they are made much broader, and somewhat flattish, with their upper surfaces uneven and rugged; that by their knobs and little cavities, they may the better retain, grind, comminute, and mix the aliment; and for this reason they are call'd *Molares*.

7. And because the operations to be perform'd by the teeth, often require a considerable firmness and strength, partly in the teeth themselves, and partly in the instruments that move the jaw, wherein the lower sett of them is fix'd; nature has provided the lower, or moveable jaw, with strong muscles, that it may bear forcibly against the upper; and has not only placed each tooth in a distinct cavity of the jaw-bone, as it were in a close, strong, and deep socket; but furnish'd the several sorts of teeth with hold-fasts, suitable to the stress, that, by reason of their different offices, is to be laid on them. And therefore the *Incisores* and *Canini* have usually but one root; whilst the *Molares*, that on many occasions are employ'd to break hard bodies before they can be ground, are furnished with three roots, and often with four, in the upper jaw, whose substance is somewhat softer, and whose grinders serve as so many little anvils for those of the lower to strike or press against.

If it shall be said, that allowing, tho' man is indeed advantageously furnish'd with teeth, yet there are many other animals, some whereof have not near so many teeth, nor so commodiously shaped and placed as his; others, that are not furnish'd with any teeth at all; and that several animals have some of their other parts less convenient in their kind, or are not otherwise near so well provided for as they would be, if they were not rather the productions of chance, than of an intelligent and designing agent: I must declare, that many particulars might be farther alledg'd, to shew the final causes of things corporeal, as the consideration of them leads

PHYSICS. to a high veneration of their divine author; and to manifest, that when his providence is deny'd or condemn'd, 'tis for want of due consideration.

But I have already, in great measure, prevented myself in the answering this objection, by obviating some exceptions relating to the eyes of man and other animals. For the considerations that have been alledg'd to that purpose, may, *mutatis mutandis*, be applied to the varieties to be found in the teeth, and other parts, of different kinds of creatures.

'Tis known, that oxen, sheep, &c. are not furnish'd with near so many teeth as men, horses, dogs, &c. But this defect, if it be any, is supplied partly by the power and instinct they have to chew the cud, and thereby make a second attrition of the aliment, that is already greatly soften'd; and partly by the successive cavities, or stomachs, thro' which that aliment is transmitted, and by that means elaborated and fitted for further uses. The beaver, the tortoise, the bee, and the humming-bird, which sucks the exudations of flowers with his little long bill, like the bee, and many other animals, have their mouths, and their ways of preparing their aliment for the stomach, very different from what is observ'd in men; and yet each respectively very convenient; all circumstances consider'd.

These, and the like expedients, are in many animals such as afford no cause of taxing the author of nature, for not having given some of them all the same parts wherewith others are furnish'd; but rather, the providence and wisdom of God, in the contrivance of his visible works, may be as well discover'd by the seeming omission of this or that part, useful to other animals, but unnecessary to those wherein it is not found; as by granting those parts to such animals, whose compleatness or welfare they are necessary or highly conducive to: and therefore 'tis not strange, that he has not given to men, and many sorts of birds, such tough, transparent, and moveable membranes, as frogs are provided with, to defend their eyes from such accidents as the others are not usually exposed to.

Bats are esteem'd a contemptible sort of creature, yet they may afford us a considerable argument to our present purpose. For we have here an animal that flies like a bird, tho' it wants feathers, and has a fabric quite different from that of birds. And here too, we may observe the compensation that is made for parts that seem either deficient, or less advantageous, than those of the same denomination in other birds; as also the regard which the divine artist appears to have to the symmetry of parts in his animated works, and to their fitness to the places they are to frequent. For the bat being to act sometimes like a bird, and on some occasions like a terrestrial animal, or mouse, he ought to be furnish'd with parts suitable to such different designs: and, therefore, the want of feathers in his wings is supplied with a broad membranous expansion, and a kind of toes furnish'd with articulations. And because this animal was to forbear settling on the ground, otherwise than his occasions required, each of his wings is furnish'd with a strong crook, like the claw of a bird's foot; by the help of which, he can fasten to trees, walls, &c. and there keep himself at what distance he pleases from the ground. And as he is furnish'd

with

with teeth, which other birds want, to chew his food, and thereby prepare it for digeſtion; he needs no crop, or ſuch a ſtrong muſcular ſtomach, as is uſually found in birds.

In ſhort, to omit the peculiar ſtructure of other internal parts, wherein the bat differs from other birds; ſince the female was not, like them, to be oviparous; but, like mice, and other quadrupeds, to bring forth her young ones alive; ſhe is not only furniſhed with an uterus, fitted for that purpoſe; but, in regard ſhe does not exclude, together with the fœtus, a competent ſtock of aliment, to nouriſh it, till it can ſhift for it ſelf; the bat is furniſhed with teats to give ſuck: and 'tis obſerv'd, that as ſhe has but two of theſe, ſo ſhe brings forth but two young ones at a time; while mice are much more prolific.

The writers of natural hiſtory, and travellers, afford numerous inſtances of the various, and excellent contrivances, that are to be found in animals differing from man, in the fabric of the mouth, and other parts, ſubſervient to the preparation and digeſtion of their aliment.

But, to be ſilent, as to the mouths of tortoiſes, camelions, &c. the hardneſs of whoſe gums, with regard to their aliment, ſupplies the want of teeth, there is an *American* beaſt, which ſerves as a notable inſtance, to manifeſt how the wiſe architect can compenſate the want of teeth, by the rare ſtructure of the mouth and tongue; and their fitneſs to ſeize, and make uſe of that aliment, which, tho' uncommon for a beaſt of his bulk, he ſeems to have been deſtined to live upon. The animals of this kind are, by *Hernandus*, call'd, *Achoas*, and *Tamendoas*; and, of theſe, he ſays, " they are quadrupeds, about the ſize of lambs, whoſe bellies almoſt " touch the ground; that they have no teeth at all, but live upon ants; " whoſe hillocks they turn up with the two large claws they have fixed " to their fore-feet, and then thruſting out their long rough, cylindrical " tongue, they roll it upon as many as they can, and ſo draw them into " their mouth, which is exceeding narrow." *Piſo* adds, that " their " tongue, which is ſometimes two feet in length, lies doubled up in a " cavity, between the ſides of the lower jaws; which, in order to catch " their prey, they thruſt into the trunks of trees, and there ſuffer it to " continue, with a moiſture upon it, till cover'd with ants; and then ſud- " denly draw them with it into their mouths."

'Tis alſo to be conſider'd, that many things may be uſeful, in an organical part, beſides thoſe whereby its office is primarily and chiefly perform'd. Thus, tho' the eye-lids, and their motions, together with the little glands that belong to them, are not at all neceſſary to the act of viſion; yet they are to the compleatneſs, and welfare of the eye: as is manifeſt by the pain and prejudice that organ receives, if the eye-lids be conſiderably diſorder'd.

But, beſides thoſe uſes of the parts of a human body, which I call anatomical, becauſe they are ſuch as anatomiſts have diſcover'd, by mere diſſection; there may be ſeveral others, which I term chymical, becauſe theſe parts prepare ſpirits of ſeveral ſorts, and, perhaps, perform other

PHYSICS. operations, very important, if not neceſſary, to the welfare of a living man.

And there may ſtill be other uſes, very proper to be conſider'd, in ſome parts of a human body; as the mechanical advantages, for which the various ſhapes, and ſtructures of different muſcles, the ſeeming irregular fabric of the bones, and, eſpecially, of the proceſſes, and protuberances, are admirably fitted. And, in ſome parts, too, there are peculiar ones; as for inſtance, optical ones in the eyes; which muſt be conſider'd, before the deſign of nature therein, can, without raſhneſs, be cenſured.

Thus, tho' the figure of the cryſtalline humour, be much more ſpherical in moſt fiſh, than in men, and other terreſtrial animals; yet he who underſtands the doctrine of refractions, and conſiders, that fiſh under water, are to ſee thro' a far thicker medium than air, will readily acknowledge, that this difference between their eyes, and thoſe of men, is not an imperfection in the former; but, whilſt thoſe creatures are in their own element, a great advantage.

And, to be ſhort, I think there are ſo many ſciences, and other parts of knowledge, ſome of them, perhaps, ſcarce yet diſcover'd, that may be requiſite to warrant a man in cenſuring the ends of God, in the bodies of animals; that very few men are qualified to condemn them juſtly; and thoſe who have knowledge enough to judge right, will not be forward to condemn, but admire them. Thus men may eaſily be too raſh, in thinking a part inartificially framed, upon ſuppoſition, that by the anatomical diſſection of it, they know all the uſes whereto the skill of the divine artiſt could deſign it.

And, it will not neceſſarily follow, that becauſe in ſome particular bird, or beaſt, or fiſh, we are unable to ſay why this, or that part is not to be found; or why it is otherwiſe framed, or ſituated, than that which is analogous thereto in man; it muſt, therefore, be caſually, or undeſignedly framed, or placed: ſince we cannot expect from brute animals, anſwers to thoſe proper queſtions about their own bodies, which we can receive from men about theirs. Nay, notwithſtanding the great diligence, with which the more curious phyſicians are obliged to cultivate anatomy, and the frequent opportunities they have to do it, and to inquire of living men about what they obſerve, and feel, when the natural uſe of their parts is hinder'd, or perverted; yet we are, to this day, at a loſs, as to the true uſes of the viſible parts of the body; to ſay nothing of the inviſible ones. So that 'tis no wonder, if, in animals, whoſe fabric we have much leſs concern to inquire into, and much leſs opportunity to examine, we, ſometimes, find parts, of whoſe uſes and fitneſs, men are, hitherto, unable to give a ſatisfactory account. For even in man, himſelf, tho' there be numerous valves found in his veins, yet, for thoſe many ages, that the true uſes of them were unknown, an *Epicurean* phyſician might have thought he had reaſon to look upon them as ſuperfluous parts; which, ſince the circulation of the blood is diſcover'd, are acknowledg'd of the greateſt utility.

And,

And, since God is both a most free, and a most wise agent, it need not seem strange, that he should adorn some animals with parts, or qualities, that are not necessary to their welfare, but that seem design'd for their beauty; such as the disposition of the camelion to change his colour; and the lovely green, blue, yellow, and other vivid colours, that adorn some sorts of pigeons, parrots, and the smaller birds; and especially, that admirable little creature, the humming-bird. Physics.

On the other side, God's wisdom sometimes seems to be provident, and sollicitous, not to bestow on an animal, or a part of it, more than is necessary for the use whereto either is design'd. Thus the veins are, by anatomists, observ'd to have but one coat, and usually to lie more exposed than the arteries that accompany them, which have stronger, and double coats, because they are to convey a more important liquor; which liquor, besides that 'tis more agitated, and spirituous, is forcibly impell'd into them, by the strong muscular contraction of the heart. And, 'tis observable, that tho' the nerves usually lie deep, to be kept both safe and warm; being very liable to be offended both by cold, and the contact of external bodies; yet, as 'tis necessary that the optic nerve should expand itself into the eye, the membranes which invest that nerve, and other coats of the eye, except the retina, are made vastly more firm than the *Dura*, and the *Pia mater*, whence they proceed; and tho' exposed to the free air, are less sensible of the cold, than most parts of the body; and will bear, without danger, several liquors, and other offensive things, whose pungency would put other nerves of the body into convulsive motions.

This conduct makes it seem, as if God, like an excellent writing-master, did, in the great volume of his creatures, intend to bestow on some of them, things rather ornamental, than necessary, as flourishes on the capital letters of nature; and sometimes to employ characters of different shapes, to stand for the same letters; and sometimes, also, to employ abbreviations, to express, in short, that which might be very justifiable, had it been more fully delineated.

And, if we allow, as 'tis very probable, that God design'd, by the great variety of his works, to display to his intelligent creatures, the great abundance of his wisdom; 'tis obvious, that much of the variety, observable in the analogous parts of animals, may be very conducive to so comprehensive a view; whereto the beauty of some creatures, and parts, as well as their more necessary, or convenient structure, may be subservient: especially if the innocent delight of man be also intended; as it seems to be, in the curious colours, and shapes of several flowers; in the melodious music of birds; and, in the vivid, and curiously variegated colours of the feathers of several winged animals; particularly those that make up the peacock's train.

'Tis worth our observing, that we are not near such competent judges of wisdom, as of justice and veracity: which are estimated by eternal and fixed bounds, or rules, very intelligible to a moderate understanding. But, as for wisdom, the more profound it is, the less we are able to look

thro'

PHYSICS. thro' it, penetrate to the bottom of it, and judge knowingly of its actions. And therefore, tho' we may ſafely conclude, that God acts wiſely, when he does any thing that has an admirable tendency to thoſe ends, we juſtly ſuppoſe him to have deſign'd; yet we cannot ſafely conclude in the negative, that this, or that, is unwiſe, becauſe we do not diſcern a wiſe tendency therein. For ſo wiſe an agent may have other deſigns than we know of, and further aims than we can diſcern, or ſuſpect; and may have at hand, or furniſh himſelf with ſuch means to compaſs his ends, and that even by the co-operation of what we think uſeleſs, or improper, as are far above the reach of our conjectures; and without the knowledge of which, we raſhly cenſure the wiſdom of his proceedings.

In the double horizontal dial, formerly mention'd, it would be raſh of any to condemn, or deſpiſe, the various lines they find traced upon that uſeful inſtrument, becauſe they ſee they are not neceſſary to ſhew the hour of the day; ſince the mathematician, who drew thoſe lines, may be well ſuppoſed to have more ends than one, or two, in making the inſtrument; and not to have drawn them by chance, or unskilfully, tho' the inconſiderate cenſurers do not know for what other, or farther purpoſes, the artiſt may have deſign'd them.

Suppoſe ſome *Indian* fiſher-man, unacquainted with *European* arts and affairs, ſhould come on board a man of war, under ſail; he would quickly perceive, by the uſe made of the tackling, that this floating building was very artificially contrived; yet, if he ſhould fix his eyes upon one of the guns, or the anchors, and perceive that no uſe was made, or likely to be made of them, in ſailing, he would be ſtrongly tempted to think, that thoſe heavy maſſes were uſeleſs clogs, and burdens to the veſſel. But if he were told the neceſſity, and uſefulneſs of the guns, for defence; and of the anchors, to ſtay the ſhip in ſtorms; he would preſently alter his mind, and confeſs, that he had blamed the contrivers for that, which nothing but his ignorance kept him from commending.

I have dwelt the longer on this third propoſition, becauſe I think it a duty, that our reaſon owes to its author, to endeavour to vindicate his manifold wiſdom, in this libertine age; wherein too many, who have more wit than philoſophy, labour upon *Epicurean*, and ſome even upon *Carteſian* principles, to depreciate the wiſdom of God; whilſt others preſume to cenſure his contrivances, in the bodies of animals.

I had alſo a deſire, that the reader ſhould not barely obſerve the wiſdom of God, but actually be convinc'd of it. To which purpoſe, in my opinion, 'tis very conducive, if not neceſſary, beſides general notions, to obſerve, with attention, ſome particular inſtances of the divine skill, wherein it is conſpicuouſly diſplay'd. 'Tis true, that in the idea of a Being infinitely perfect, boundleſs wiſdom is one of the attributes included: but, for my part, I am of opinion, that this general and indefinite idea of the divine wiſdom, will not give us ſo great a veneration for it, as may be produced in our minds, by knowing, and conſidering the admirable

rable contrivances of the particular productions thereof; and their exquisite fitneſs for thoſe ends and uſes, to which they appear to have been deſign'd.

PROP. IV.

We ought not to be haſty in concluding upon the particular uſe of a thing, or the motive which induced the author of nature to frame it in a peculiar manner.

It has been already ſhewn, that ſome parts are ſo excellently, and manifeſtly fitted for a certain uſe, and ſo much better adapted to that, than to any other; as to make it ſeem perverſe, to doubt of its being deſign'd thereto. But the like cannot be ſaid, in general, of all the parts of the body; eſpecially of the internal. There are many uſes, either neceſſary, or highly conducive to the welfare of the animal, that have no part ſo much more conſpicuouſly fitted for them than another, as to leave it eaſy to determine the true, and primary offices thereof; eſpecially, with ſo much certainty, as thereon to ground philoſophical inferences. And, of this difficulty, I conceive, there may be four reaſons; tho' they do not all occur in each particular caſe.

And, firſt, the whole animal itſelf, that we conſider, is a part of the univerſe; and, therefore, cannot well be ſuppoſed to have been framed, and furniſh'd with the parts it conſiſts of, merely for its own ſake. And, when we ſay, that all its parts are contriv'd to the beſt advantage to the animal; I conceive, this is to be ſo underſtood, in a limited ſenſe; that the parts are excellently framed for the welfare of the animal, as far as that welfare is conſiſtent with the general ends of the author of nature, in the conſtitution, and government of the univerſe; which ends, becauſe they relate to the whole world, I call coſmical. It has not, indeed, been prov'd, that none of theſe coſmical ends are diſcoverable by us: however, to diſcover them all, is no eaſy task. Yet, it ſeems preſumptuous to ſuppoſe, that the welfare of particular animals is any farther deſign'd, and provided for, than will conſiſt with the coſmical ends of the univerſe, and the courſe of God's general providence; to which his particular providence ought, in reaſon, to be ſubordinate. And, tho' it ſeems great raſhneſs in men, to determine poſitively, and excluſively of others, what ends the omniſcient creator propoſed to himſelf, in giving to the world its preſent frame; yet, as far as I can hitherto diſcern, I ſee nothing more likely to have been one grand motive of ſo great a variety, as we obſerve in his corporeal works, eſpecially in animals, than that he might, by ſo many very different contrivances as are to be met with in the ſtructure of men, quadrupeds, birds, fiſh, reptils, *&c.* exerciſe and diſplay his manifold wiſdom. As man, even upon account of his body, is acknowledg'd the moſt perfect of animals; if God had barely deſign'd to give every creature the moſt advantageous ſtructure, it ſeems, that he ſhould have made no other animals than men. But, then, there could not have been that diverſity of contrivance among living automata, which ſo greatly recommends

PHYSICS. mends the wiſdom of him, who could frame ſo many, and ſo differing animals; tho' not all equally perfect, yet all admirably furniſh'd for thoſe purpoſes to which he deſign'd them. It does not, therefore, argue any want of providence, that he has not furniſh'd man with wings, fiſh with feet, and birds with fins and ſcales; becauſe theſe parts would have been either ſuperfluous, or burdenſom, or unſuitable to his deſign of fitting ſome animals to live on the earth, and others in water: and if he deſign'd any to inhabit both, he furniſhes them with parts of a peculiar ſtructure; as was formerly noted of the beaver. Were it not for this conſideration, it would be hard to aſſign the reaſon why vegetables are not the food of all animals; but that ſome ſhould be carnivorous, and furniſh'd with appetites, and organs, to devour others, and live, as birds and beaſts of prey, upon the weaker. And 'twill be hard to ſhew, why, even in animals of the ſame kind, the ſafety of ſome ſhould be ſo much better provided for than that of others; as we ſee ſome ants, and glow-worms, furniſh'd with wings, and others not. And, even in our own ſpecies, thoſe of the female ſex are not ſo happily framed, in order to their own welfare, as males; ſince the uterus, *&c.* which are not neceſſary to the good of individuals, but to the propagation of the ſpecies, ſubject that tender ſex to a ſett of diſeaſes peculiar to them. So that men may, ſometimes, miſtake, when they peremptorily conclude, that this, or that part of an animal, muſt, or cannot have been framed for ſuch a particular uſe; without conſidering the coſmical, primary, and over-ruling ends, that may have been deſign'd by nature, in the conſtruction of the whole animal.

2*dly*, Men ſometimes, erroneouſly, conclude, that ſuch an office cannot belong to ſuch a part, becauſe they think it is not ſo commodiouſly framed for it, as might be wiſh'd; without conſidering, whether the ſtructure they propoſe, would not, in ſome other as conſiderable reſpect, oppoſe the welfare of the animal; or, whether it would be conſiſtent with the other uſes, deſign'd by nature, in that ſort of creature. For, in the living works of ſo excellent an architect as nature; it muſt not be expected, that any particular end ſhould be proſecuted, to the prejudice of the whole: but it muſt rather be ſuppos'd, that ſhe aims, not only at particular expedients, but univerſal ſymmetry; while ſhe excellently fits the ſeveral parts for their reſpective offices; tho' only ſo far as a due regard to the deſign, and welfare of the whole, will permit. Thus, as we before obſerv'd, tho' man be allow'd the moſt perfectly framed of any animal in the world; yet his body is not made the model for nature to form the correſpondent parts of other animals by. The lungs of a dog, a bird, a frog, a viper, *&c.* are of a ſtructure very different from thoſe of man. He is not furniſh'd with ſo many ſtomachs as an ox, or a ſheep; becauſe nature intended not that he ſhould ruminate like them. And tho' his gall be lodg'd in a peculiar bag, within the liver; yet 'twere raſh to ſay, that the ſecretion of the bile is none of the uſes of thoſe livers, wherein ſuch a cyſtis is not to be met with: ſince, in ſome animals, as in in horses and pigeons, that bitter humour is not usually, as in man, collected into one bag: and tho' it be so in vipers; yet, as far as I have observ'd, the containing cystis does not at all touch the liver.

3dly, 'Tis difficult to determine the true and primary use of a part; because nature often fits one part for several uses.

And, *4thly*, it sometimes increases the difficulty, that nature may compass the same ends by several means; each of them sufficiently commodious. I join these two observations together, because, in effect, they often concur to render it hard to determine the true use of a part. Neither does nature constantly employ only one part to perform a particular office; but the design'd effect is, sometimes, produced by a series of successive operations, to which several parts may differently contribute.

Neither the mechanism of a human body, nor of very considerable parts thereof, is to be assign'd from the bare structure of the visible matter, whether solid, or fluid, to be found in the vessels, and cavities of a dead body, skilfully dissected. For the body of a living man is a very compounded engine, many of whose functions are perform'd, not by the blood, and other visible fluids, barely as they are liquors; but, partly, by their circulation, and other motions; and, partly, by a very agile, and invisible, sort of fluids, call'd spirits animal and vital; partly, perhaps, by little springy particles; by somewhat that may be call'd the vital portion of the air; and, by things analogous to local ferments: the important operations of all which, cease with life; and the agents themselves are not to be discern'd in a dead carcass. So that, besides those manifest uses, which the visible fabric of the engine may suggest to an anatomist; there may be chymical uses of some parts, that serve for the elaboration of spirits, and other fluids: which uses, as we formerly observ'd, are not suggested to the anatomist, by the inspection of the structure of the parts; but, to discern them, may require no ordinary skill in chymical principles, and operations.

Such considerations as these, lead me to think it exceeding difficult to determine, with any certainty, the principal use of many particular parts; especially, if other uses be excluded. It is not enough to secure us, that we understand the chief function, and end, of a part, to know, that it is contrived for such a purpose; since this fitness hinders not, but that the primary use of the part may be another more conducive to the general welfare of the animal, or the cosmical ends of nature. And it ought not to seem strange, that some pieces of workmanship, consisting of many parts, all of them curiously contrived, should, by one learned man, be guess'd to be design'd for this use; and, by others, for that; while both of them may be worthy of the artificer.

When some very politic prince does a great thing, without declaring why; the guesses of the statesmen are often very different; tho' none of them ascribe to him a design unbecoming a wise man. So, when a learned author expresses himself ambiguously; one reader interprets his words to this sense; and another, to that: yet both of the senses pitch'd on, PHYSICS. may fairly agree with the context, and the main ſcope of the writer. This I ſay, becauſe I would, by no means, diſparage the wiſdom of nature, by propoſing the difficulties I have hitherto mentioned; tho', I confeſs, that, upon the account of them, and ſome others; I look upon many of the arguments that ſeveral authors draw from final cauſes, but as conjectures. And, in many caſes, I allow what is ſuggeſted to me, upon the ſuppoſition of the intended uſes of particular parts, rather as proper hints to excite, and direct, a more thorough inquiry, than as ſafe grounds to build phyſical concluſions on.

PROP. V.

The naturaliſt ſhould not ſuffer the ſearch, or diſcovery, of final cauſes, to make him undervalue, or neglect, the inquiry after their efficients.

'Tis true, to inquire for what purpoſe nature would have particular effects produced, is a curioſity worthy of a rational creature. But this is not the proper task of a naturaliſt; whoſe work is not ſo much to diſcover the manner wherein, as for what reaſon particular effects are produced. Thus, an ignorant ruſtic, in *England*, knows ſomething of a watch, if he is able to tell you, that 'tis an inſtrument made to meaſure time; which is more than every *American* ſavage would be able to ſay; and more than thoſe civiliz'd *Chineſe* knew, who took the firſt watch they ſaw for a living creature: yet the *Engliſh* ruſtic, who knows no more of a watch, than that 'twas made to ſhew the hour of the day, underſtands but very little of the nature of that machine. And, as the two things men aim at in phyſics, are to underſtand after what manner nature produces the phenomenon we contemplate; and, in caſe it be imitable by us, how we may, if occaſion requires, produce the like effect, or come near it; theſe ends cannot be attain'd by the bare knowledge of the final cauſes of things, nor of the general efficient. But, to anſwer thoſe intentions, we muſt know the particular efficients, with the manner, and progreſs, of their operating; and what diſpoſitions they either find, or produce in the matter they work on. Thus, he who would thoroughly underſtand the nature of a watch, muſt not reſt ſatisfy'd with knowing, in general, that a man made it for ſuch uſes; but he muſt, particularly, know of what materials the ſpring, the wheels, the chain, and the balance are made; he muſt know the number of the wheels, their magnitude, ſhape, ſituation, and connexion, in the engine; and after what manner one part moves another, in the whole ſeries of motions; from the expanſive endeavour of the ſpring, to the revolutions of the index that points the hours. And much more muſt a mechanic know all this, if he means to make a watch himſelf, or to give ſufficient inſtructions for it to be made. In ſhort, the neglect of efficient cauſes, would render philoſophy uſeleſs; but the ſtudious ſearch after them, will not prejudice the contemplation of final cauſes. For the wiſe author of nature has ſo excellently contriv'd the univerſe, that the more clearly, and particularly, we diſcern how ſuitable the means are to the ends

ends to be obtain'd by them; the more plainly we discern the admirable wisdom of the author of things; who is "wonderful in counsel, and excellent in working." Nor will the sufficiency of the intermediate causes make it needless to admit a first, and supreme cause; since that order of things, by virtue whereof these means become sufficient to such ends, must have been, at first, instituted by an intelligent cause. And if it be irrational to ascribe the excellent fabric of the universe, and the actions that have manifest tendencies to determinate useful ends, to so blind a cause as chance; it will be rather more irrational to ascribe to chance, the first formation of the universe; of which the present state of things is but the natural consequence, or effect. For it may, indeed, be plausibly said, that, in the present state of things, the several parts of the universe are, by the contrivance of the whole, determin'd, and thereby qualify'd to obtain their ends; but it cannot be, rationally, pretended, that, at the first formation of the world, there was a sufficiency in the senseless materials of it, without any particular guidance of a wise superintendent, to frame bodies which are so excellently contriv'd, and fitted to their respective ends.

Upon the whole, it appears, that all consideration of final causes, is not to be banish'd from natural philosophy; but that 'tis rather allowable*, and, in some cases, commendable, to argue, from the manifest uses of things, that the author of nature pre-ordain'd them; that the sun, and moon, with the other celestial bodies, excellently declare the power, wisdom, and glory of God; and were, some of them, among other purposes, made to be serviceable to man; that, from the suppos'd ends of inanimate bodies, whether celestial, or sublunary, 'tis very unsafe to draw arguments, to prove the particular nature of those bodies, or the true system of the universe; that, as to animals, and the more perfect sort of vegetables, 'tis warrantable to say, particular parts were pre-ordained to particular uses, relating to the welfare of the animal, or

* Final causes are allowable in philosophy; and Sir *Isaac Newton* himself, scruples not to assign them in the structure and contrivance of the universe. And of this we have a remarkable example in his doctrine of comets. "Comets, (says "that admirable philosopher) by reason "of the great number of them, the vast "distance of their aphelia from the sun, "and the considerable stay they make "there, must necessarily receive some di-"sturbance from their mutual gravita-"tions to each other. Whence their ec-"centricities, and times of revolution, "will sometimes be a little increas'd, and "at others, diminish'd. We are not, "therefore, to expect, that the same co-"met shall always exactly return in "the same orbit, at the same period "of time.—— And hence appears the "reason why comets move not in the "zodiac, as the planets do; but stray "from it, in various motions, to all the "parts of the heavens: for, in their a-"phelia, where their motion is slowest, "they ought to be at the greatest di-"stance from one another, that their "mutual attractions may be the less. *Newton. Princip.* p. 480.

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plant itſelf, or to its ſpecies; but that ſuch arguments may eaſily deceive, if thoſe who form them are not very cautious to avoid miſtaking, among the various ends that nature may have in the contrivance of an animal body, and the various ways which ſhe may ſucceſsfully take to compaſs the ſame ends; and laſtly, it appears, that a philoſopher, nevertheleſs, muſt not let the ſearch, or knowledge of final cauſes, make him neglect a careful inquiry after efficients.

THINGS ABOVE *REASON*, CONSIDER'D.

SECT. I.

IT appears to me, that, among the objects of our reason, there are some, whose nature we cannot comprehend; others, whose attributes, or actions, we cannot reconcile to the subject; and, lastly, others, that we cannot conceive how they should consist with some acknowledg'd truth: and these may be call'd things above reason. By which I here understand, not such things as are false, and absurd; but such as, tho' the understanding sees sufficient cause to assent to, yet finds itself reduced to this assent, with a remarkable, and peculiar, disadvantage. And this disadvantage, usually, proceeds either from the nature of the thing proposed; which is such, that we cannot, sufficiently, comprehend it; or, from our being unable to conceive the manner of its existence, and operation; or, because it involves some notion, or proposition, that we see not how to reconcile with some other, which, we are persuaded, is true. The first of these three sorts of things, may be call'd incomprehensible; the second, inexplicable; and the third, unsociable. *Things above reason, of three kinds.*

The first consists of those things, whose nature is not distinctly, and adequately, comprehensible by us; to which, perhaps, we may refer all such intellectual Beings, if any be granted, as are, by nature, of an higher order than human souls; at least, we may refer to this head the great author of nature. *Incomprehensible.*

The second sort of things above reason, consists of such as, tho' we do not deny to exist, yet we cannot clearly, and satisfactorily, conceive how they can be what we acknowledge they are: as, how matter can be infinitely divisible; and how there should be such an incommensurability betwixt the side, and diagonal, of a square, that the smallest line cannot, adequately, measure them both. *Inexplicable.*

The

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Unsociable.

The third sort, are things incumber'd with difficulties, and objections, that cannot directly, and satisfactorily, be removed by those who acquiesce in the receiv'd rules of inferior sciences, and reason but at the common rate; such objects of contemplation having something belonging to them, that appears irreconcilable with some very manifest, or acknowledg'd truths.

Thus, that man has free will, at least with regard to civil matters, is the general confession of the species: all the laws that forbid, and punish, murder, adultery, theft, and other crimes, being founded upon a supposition, that men have a power to forbear committing them: and the sense men have of being possessed of this power over their own actions, is great enough to make malefactors acknowledge their punishments just; being condemned by their own consciences, as well as by their judges. Yet the generality of mankind ascribe to God an infallible prescience of human actions. But how a certain fore-knowledge can be had of contingent things, and such as depend upon the free-will of man, is that which we are unable clearly to comprehend. And, doubtless, 'tis hard to conceive, how an infinitely perfect Being should want prescience; or, that our will should want the liberty we almost perpetually exercise.

Again, geometricians teach the divisibility of quantity *ad infinitum*, to be mathematically demonstrable. Suppose, then, a strait line, of three feet long, divided into two parts, the one double to the other; now a line of two feet is divisible into infinite parts, or it is not; if you say it is not, you contradict a demonstration; if you say it is, you must confess, either that the line of one foot is divisible into as many parts as the line of two feet; or else, that the infinite parts into which the line of one foot is granted to be divisible, is exceeded in number by the parts into which the line of two feet is divisible; and, consequently, that the line of two feet has a multitude of parts, greater than infinite. Which reasoning may let us see, that we may be reduced, either to reject inferences fairly drawn from manifested, or granted truths, or to admit conclusions that appear absurd; if we will have all the common rules, whereby we judge of other things, to be applicable to infinites. Hence it is clear, in the general, that there may be things which surpass our reason; at least, so far, as not to be judg'd of by the measures and rules used in judging of ordinary occurrences: for which reason, I shall call them privileg'd things.

Privileg'd things what?

The imperfection of the human mind.

But, to manifest the imperfections of our reason, with regard to privileg'd things, we need not have recourse to the divine nature: a mathematical demonstration may serve the turn. For, suppose a great circle, divided into its 360 degrees; and suppose any number of strait lines, drawn from the several assignable parts of some one of these degrees, to the centre; 'tis manifest, that the degrees being equal, as many lines may be drawn from any other; and so from every one of the rest.

Then, suppose a circular arch, equal to the assumed degree, to be further bent into the circumference of a little circle, concentrical with the great

great one; it follows, from the nature of a circle, that the ſemi-diameters of it, how many ſoever they be, can no where touch one another, but in the centre. Whence 'tis evident, that all the lines, drawn from the circumference to the centre of the greater circle, muſt paſs thro' different points of the circumference of the ſmaller; and, conſequently, that as many lines as can, even mentally, be drawn from the ſeveral points of the circumference of the great circle to the common centre of both circles, muſt all paſs thro' different points of the little circle; and thereby divide it into as many parts as the greater circle is divided into. Here, then, the circumference of the leſſer circle preſents us with a curve line, which was not poſſibly diviſible into more parts than an arch of one degree, or the 360th part of the circumference of the greater circle; yet, without being lengthen'd, it becomes diviſible into as many parts as the whole circumference of the ſame greater circle. And, tho' we ſhould ſuppoſe the circumference of the internal circle not to exceed one inch; and that of the exterior circle to exceed the circumference of the terreſtrial globe, or *Orbis magnus* itſelf; ſtill the demonſtration would hold.

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And, ſurely, 'tis very agreeable, both to the nature of God, and to that of man, that ſuch things as theſe ſhould prove true; for we miſtake, and flatter human nature too much, when we think our underſtanding ſo unlimited, both in point of capacity, and of extent; and ſo free, and unprepoſſeſs'd, as many philoſophers ſeem to ſuppoſe. For, whatever our ſelf-love may incline us to imagine, we are really but created, and finite Beings, and come into the world but ſuch as it pleas'd the moſt free author of nature to make us. And, from this dependance, and limitation of our natures, it follows, not only that we may be born with certain impreſſions, appetites, or tendencies of mind; but alſo, that the means given us to employ in the ſearch, or judging of truth, are only proportionable to the deſign of our make; and, therefore, may, probably, be ſuppoſed not able to reach all kinds of truths; many of which may be unneceſſary for us to know here: and ſome may be reſerv'd, to render us ſenſible of the imperfections of our natures, and to make us aſpire to that condition, wherein our faculties ſhall be much enlarged, and heighten'd. It ſeems not, therefore, unreaſonable to think, both that God has ſo limited our faculties, that, in our preſent ſtate, there ſhould be ſome objects beyond the comprehenſion of our underſtandings, or, that ſome of his creatures ſhould not be able perfectly to underſtand ſome others; and yet, that he has given us light enough to perceive, that we cannot attain to a clear, and full knowledge of them.

It may here, indeed, be objected, that tho' the inſtances given, have not been hitherto clear'd by the light of reaſon, yet, 'tis probable, they may be ſo hereafter; conſidering how great a progreſs is, from time to time, made in the diſcoveries of nature. I anſwer, that I imagine, what future diſcoveries are made, will chiefly concern thoſe things which either we are ignorant of, for want of a competent hiſtory of nature; or miſtake, thro' erroneous prepoſſeſſions, or for want of freedom, and attention, in our

ſpecu-

PHYSICS. ſpeculations. But as to all metaphyſical difficulties, wherein neither matters of fact, nor the hypotheſes of ſubordinate parts of learning are of force, we have little to expect. And, however it be as to other abſtruſe objects; I am very apt to think, there are ſome things relating to the infinite Being, which will ſtill remain incomprehenſible, even to philoſophical underſtandings. I can ſcarce hope to ſee thoſe obſtacles ſurmounted, that proceed not from any perſonal infirmity, or evitable faults, but from the limited nature of the human mind. Beſides, as mankind may hereafter explain ſome of thoſe grand difficulties, which have hitherto perplex'd philoſophers; ſo their inquiries may, poſſibly, lead them to diſcover new difficulties more perplexing than the firſt. For, even amongſt the things wherewith we are already acquainted, there are many which we think we know, only becauſe we never, with due attention, try'd whether we can frame ſuch ideas of them, as are clear, and worthy for a rational mind to acquieſce in. This appears from the great intricacy that conſidering men find in the notions commonly receiv'd of ſpace, time, and motion, *&c.* and the difficulties of framing clear, and ſatisfactory apprehenſions, even of ſuch obvious things. We ſee alſo, that the angle of contact, the doctrine of aſymptotes, and that of ſurd numbers, and incommenſurable lines, all which give no concern to common accomptants and ſurveyors, perplex the greateſt mathematicians. Hence the growing curioſity of mankind is not more likely to ſolve ſome difficulties, than to raiſe others; which may prove more inſuperable than they.

The motion of a coach-wheel is ſo obvious, and ſeems ſo plain a thing, that the coach-man never looks upon it with wonder; yet after *Ariſtotle* had taken notice of the difficulty that occurr'd about it, this trivial phenomenon has perplex'd even famous mathematicians, and continues yet to do ſo: there being ſome circumſtances in the progreſſive motion and rotation of the circumference of a wheel, and its nave; or of two points aſſign'd, the one in the former, and the other in the latter, that have appear'd too ſubtile for modern writers.

After what manner human reaſon acts.

And here we may obſerve, that reaſon operates according to certain ideas, axioms, and propoſitions, wherewith, as by rules and meaſures, it conceives, eſtimates, and judges of things. And, indeed, when we ſay, that a thing is conſonant or repugnant to reaſon, we mean uſually, that it is either immediately or mediately deducible from, or at leaſt conſiſtent with, or contradictory to one or other of theſe ſtandard-rules or notions. But if theſe rules and notions be ſuch as are drawn only from finite things, or are agreeable but to thoſe; they may prove uſeleſs or deceitful, when we go to ſtretch them beyond their meaſure, and apply them to infinites.

To illuſtrate and confirm this notion. All the things that we naturally do or can know, may be divided into ſuch as we may acquire without a medium, and ſuch as we cannot attain to, but by the intervention of a medium, or by a diſcurſive act. To the firſt belong ſuch notions as are ſuppoſed to be connate; as, that "Two contradictories cannot be both true;" "The whole is greater than a part thereof;" "Every whole number is either

either even or odd ;" *&c.* as alſo, thoſe other truths, that are aſſented to upon their own account, without needing any medium to prove them ; becauſe that, as ſoon as by clear terms, or fit examples, they are plainly propoſed to the underſtanding, they diſcover themſelves to be true ſo manifeſtly by their own light, that they want no propoſition to make the underſtanding acquieſce in them. Of this kind are ſome of *Euclid*'s axioms ; for inſtance, " If to equal things equals be added, the totals will be equal." PHYSICS.

To the ſecond ſort of things knowable by us, belong all that we acquire by the act of reaſoning ; wherein, by means of propoſitions or mediums, we deduce one thing from another ; or conclude, affirmatively or negatively, one thing of another. This being ſuppoſed, and we being conſcious to ourſelves that we are not the authors of our own nature ; all the experience we have hitherto had, leads us to think, that the meaſures ſuggeſted to us, either by ſenſations, the reſults of ſenſible obſervation, or the other inſtruments of knowledge, are ſuch as fully reach but to finite things ; and, therefore, are not ſafely applicable to others. And many of thoſe principles that we think very general, may be only gradual notions of truth ; and but limited and reſpective, not abſolute and univerſal.

And tho' perfect ſyllogiſms be counted the beſt and moſt regular forms, that our reaſonings can aſſume ; yet even the laws of theſe are grounded on the doctrine of proportions ; for even between things equal, there may be a proportion. Upon this ground, I ſuppoſe, it is, that mathematical demonſtrations have been publicly propoſed of the grand ſyllogiſtical rules. And, in conſequence hereof, geometricians tell us, there is no proportion between a finite line and an infinite ; becauſe the former can never be ſo often taken, as to exceed the latter ; which, according to *Euclid*'s definition of proportion, it ſhould be able to do. Since then, the underſtanding operates but by the notions and truths 'tis furniſh'd with, and ſince theſe are its inſtruments, by proportion to which, it takes meaſures, and makes judgments of other things ; ſuch inſtruments may be too diſproportionate to ſome objects, to be ſecurely employ'd to determine ſeveral particulars about them. Thus, the eye being an inſtrument which the underſtanding employs to eſtimate diſtances, we cannot by that, ſafely take the breadth of the ocean ; becauſe our ſight reaches not far enough, to diſcover the extent of ſo vaſt an object. And the common inſtruments of ſurveyors, that would ſerve to meaſure the height of a houſe or a ſteeple, or even of a mountain, cannot enable them to take the diſtance of the moon. But when aſtronomers take, by ſuppoſition, a line that reaches from the ſurface to the centre of the earth, tho' by the help of this, and the parallax, they may tolerably meaſure the diſtance of the moon ; yet, with all their great induſtry, they cannot, by the ſame way, with any tolerable accuracy, meaſure the diſtance of the ſtars ; the ſemi-diameter of the earth bearing no ſenſible proportion to that of ſo vaſt a ſphere, as makes their parallax vaniſh : it being all one to ſenſe, whether, at ſo great a remove, a ſtar be obſerv'd from the centre, or from the ſurface of the earth. Thus when I

PHYSICS. think of a triangle or a ſquare, I find in my imagination, an intuitive idea of thoſe figures; that is, a picture clear and diſtinct, as if a figure of three ſides, or of four equal ſides and angles, were placed before my eyes. But if I would fancy a myriagon, or a figure conſiſting of ten thouſand equal ſides, my imagination is overpower'd with ſo great a multitude; and frames but a confuſed idea of a polygon, with a very great many ſides. For if a man ſhould endeavour to frame ideas of a myriagon, or a chiliagon, they would be both ſo confuſed, that his imagination would not be able clearly to diſcriminate them; tho' the one has ten times as many ſides as the other. Thus, if you would imagine an atom, of which perhaps ten thouſand would ſcarce make up the bulk of one of the light particles of duſt, that plays in the ſun-beams; ſo extraordinary a minuteneſs, not having fallen under any of our ſenſes, cannot truly be repreſented in our imagination. So, when we ſpeak of God's omnipotence, and other of his infinite attributes and perfections; we have ſome conceptions of the things we ſpeak of, but may very well diſcern them to be inadequate. And tho' ſeveral propoſitions, relating to things above reaſon, ſeem clear enough to ordinary capacities; yet he who ſhall, with a competent attention, curioſity, and skill, conſider and examine them, will find that either their parts are inconſiſtent with one another, or that they involve contradictions to ſome acknowledg'd or manifeſt truth, or are veil'd over with darkneſs, and encumber'd with difficulties, from whence we are not able to reſcue them. Thus, when the ſide and diagonal of a ſquare are propoſed, we have clear and diſtinct ideas of each of them a-part; and when they are compared, we may have a conception of their incommenſurability; yet this negative notion, if it be thoroughly conſider'd, and far enough purſued, clearly contains that of a ſtrait line being diviſible *in infinitum*: and this diviſibility is encumber'd with ſo many difficulties, and is ſo hard to be reconciled to ſome confeſs'd dictates of reaſon, that philoſophers and geometricians, tho' convinc'd of the truth, are to this day labouring to extricate themſelves out of theſe perplexing intricacies.

'Tis evident, that ſome ſubſtance or other, whether God, the world, or matter, never had a beginning; that is, has been for ever. But when we ſpeak of an eternity *à parte ante*, as they call it, we do not ſpeak of a thing whereof we have no conception at all; as will appear to a conſidering perſon: yet the general notion we have, is ſuch, that when we come attentively to examine it, by the ſame ways whereby we judge of almoſt all other things, the underſtanding is confounded: for we muſt conceive, that the time efflux'd ſince the firſt man began to live, bears no greater proportion to the duration of God, or of matter, than a moment. And as there are ſome things, whoſe nature and conſequences puzzle our faculties; there are others, whereof tho we have a notion, yet the *Modus operandi* is beyond our comprehenſion. I do not mean only the true and certain *Modus operandi*, but even an intelligible one. Thus, tho' many, eſpecially *Carteſians*, and that upon a philoſophical account, aſſert that God created the world; yet, how a ſubſtance could be made out of nothing, I fear, we cannot conceive.

And

And tho' all philosophers, very few excepted, believe that God made the world, out of pre-existent matter; yet how he could make it, but by locally moving the parts of the matter it was to consist of; and how an incorporeal substance can move a body, which it may pass thro' without resistance, will be found hard to explain. If it be said, that the soul being an immaterial substance, can nevertheless move the limbs of the human body, rightly disposed; I answer, it does not appear that the rational soul gives any motion to the parts of the body, but only guides or regulates that which she finds in them already. PHYSICS.

Thus then, by making observations of such things as are the proper objects of our faculties, and by drawing just consequences from such observations, and from our other knowledge, we may come to be certain that some things exist, and so have general and dark ideas of them; when, at the same time, we are at a loss to conceive how they can be such, or how they can operate and perform what they do; supposing the truth and sufficiency of some other things we are convinc'd of. To be short, negative apprehensions we may have of some privileged things, and positive, but indistinct apprehensions of others; and that is enough, in some sort, to make us understand ourselves, and one another, when we speak of them: tho' when we sufficiently consider what we say, we may find, that our words are not accompany'd with clear, distinct, and symmetrical conceptions of those abstruse and perplexing things we speak of. And since we find, by experience, that we are unable sufficiently to comprehend things, which by clear and just consequences may be proved to exist; this strongly argues, that some of our conceptions may be of things whereto somewhat belongs, that transcends our reason, and surpasses our comprehension. So that when natural philosophy had taught men to believe God to be an infinitely perfect Being, we ought not to say, that they had no idea of such a Being, because they had not a clear and adequate one. And since *Aristotle* professedly discourses of infinity, and cites the ancienter philosophers for having done so before him; and since *Democritus*, *Epicurus*, *Gassendus*, and others, maintain either that the world is boundless, or that space is not finite in extent, or that the world consists of atoms, infinite in number; we must not suppose, they said they knew not what, as they must have done if they wrote without ideas of the things they treated of: tho' we may justly say, that the subject being infinite, the ideas they framed of it, could not be comprehensive and accurate. Thus, when the eye looks into a deep sea, tho' it may pierce a little way into it; yet when it would go deeper, it only discovers somewhat dark and indistinct, which affects the organ so differently from what other more genuine objects do, that we thence easily discern our sight fails us in the way, before it arrives at the bottom; and consequently, that there may be many things conceal'd there, which our sight is unable to reach. But if we really find there are things which our reason cannot comprehend, then, whether the account I have given, why our faculties prove insufficient for these things, be good or not; there must be some true account or other of that insuffi-

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ciency. And some things must appear to us so sublime and abstruse, that we are unable to comprehend them, and to discern so much as the reason why they cannot be comprehended by us.

Upon the whole, then, we may reasonably suppose, that the great author of nature so framed man, as to have furnish'd his intellective faculty with a light, whereby it can, not only estimate the power of a multitude of other things, but also judge of its own nature and power, and discern some of the limits, beyond which it cannot safely exercise its act of judging and defining. But the rational soul does not only pass judgment of the things without her, but about herself, and what passes within her: she searches out, and contemplates her own spirituality, and union with the body. The intellect judges wherein its own nature consists, and whether or no itself be a distinct faculty from the will. And to come yet closer, logic and metaphysics are the works of the human mind; which, by framing those disciplines, manifests that it not only judges of reasoning, but of the very principles and laws thereof; teaching what things are necessary to obtain evidence and certainty, and what kind of mediums they are, from whence we must not expect any demonstrative arguments relating to a subject. Thus, if we compare the bodily eye with the understanding, which is the eye of the mind; we must allow this difference, that the intellect is as well a looking-glass as a sensory; since it does not only see other things, but itself too, and can discern its own blemishes, bad conformation, or whatever other infirmities it labours under. The soul, therefore, when duly excited, is furnish'd with a light that may enable her to judge, even of many of those original notions, by which she judges of other things. In short, the soul, upon trial, may find, by an inward sense, that some things surpass her force; as, a blind man being set to lift up a rock, would quickly find it too unwieldy to be managed by him: and the utmost exercise of his strength, would but convince him of the insufficiency of it to surmount so great a weight. We do not then pretend, that the eye of the mind should see invisibles; but only, that it shall discern the limits of that sphere of activity, within which nature hath bounded it; and consequently, that some objects are disproportionate to it. *Aristotle* himself says, that the eye sees both light and darkness; which expression, tho' somewhat odd, may be defended: since, tho' darkness is a privation, not a Being, it cannot properly be the object of sight; yet it may be perceived by means of the eye, from the very different affection which that organ suffers, when impress'd by luminous objects, and when it is made useless to us by darkness.

Whether men may, with justice, discourse of things above reason?

But it may be ask'd, how can we justify our discoursing, at all, of things transcending reason?

The better to clear this matter, I must make some distinctions of the notions or conceptions of the mind; and, for brevity sake, give names to those I have now occasion to employ. I consider then, that whether the conceptions or ideas we have of things, be simple or compounded, they may be distinguish'd into such as are particular or distinct, and such as are only

only general, dark, and confused, or indistinct. When a navigator to unknown countries, first gets a sight of land, tho' he may be satisfied that it is land, yet he has but a very dark and confused picture of it made in his eye, and cannot descry whether the shore be rocky, or what creeks or harbours it may have in it; much less, whether the coast be well inhabited; and if it be, what kind of buildings it has: all which he may plainly and distinctly see, upon going on shore. And of some things we have an adequate, of others but an inadequate conception: thus, if we suppose the same navigator should look towards the main sea, tho' he might see a great way distinctly, yet at length it would appear so dark and confusedly to him, that at the verge of the sensible horizon, his sight would make him judge the sea and sky came together; he would, however, conclude, that the utmost part of the sea he could descry, was but a part of the ocean, which may, for ought he knows, reach to a vast extent beyond the visible horizon. To our confused, and often, also, to our inadequate conceptions, belong many of those that may be call'd negative; which we employ, when we speak of privations or negations, as blindness, ignorance, death, *&c.* We have a positive idea of things that are square and round, black and white; and, in short, of other things, whose shapes and colours make them the objects of our sight. But when we say, for instance, that a spirit or an atom are invisible; those words are attended with a negative conception, which is commonly but dark and confused, because 'tis indefinite, and removes, or lays aside those marks, by which we clearly perceive and distinguish visible substances: as, when we say, that such a thing is impossible, we have some kind of conception of what we speak; but 'tis a very obscure and indistinct one, at best; exhibiting only a general and very confused representation of some ways, whereby one might think the thing likely to be effected, if it were at all performable; and accompanied with a perception of the insufficiency of those ways. There is yet another difference in the notions we have of things, which, tho' not usually observ'd, is too important to be here omitted. Of some things we have a knowledge, that for want of a fitter term, may be call'd primary or direct; and of others, the knowledge we have, is acquired but by inferring it from some more known or clearer truth; and so may be call'd inferr'd or illative knowledge. Thus, when a geometrician defines to me an hyperbola, I quickly gain a clear and distinct idea of it: but when he proves to me, that this hyperbola may have such a relation to a strait line, which he calls an asymptote, that this line being continued, still comes nearer and nearer to the prolong'd side of the hyperbola, and yet, how far soever both be drawn, will never come to touch it; his subtile demonstrations present me with an inferr'd or illative truth, at which we arriv'd not, but by the help of a train of reasonings; and on which if we exercise our imagination, we shall find this factitious truth, if we may so call it, accompanied but with a very dim and confused idea. To these distinctions we may add, lastly, what belongs chiefly to the notions we have of true or false propositions, that of our conceptions of things, some are symmetrical, or every way consistent; by which I mean, those which have

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PHYSICS. have theſe two qualifications; the one, that all the parts are conſiſtent among themſelves; the other, that the entire idea is conſiſtent with all other truths: and ſome are chimerical, or aſymmetrical; by which I underſtand thoſe that are either ſelf-deſtructive, by the contrariety of the parts themſelves, whereof they are made up, as if one ſhould talk of a triangular ſquare, or a ſun-ſhiny night; or being extravagant, lead to ſome manifeſt abſurdity that may be juſtly inferr'd from them; or into inextricable difficulties; or elſe involve a real repugnancy to ſome acknowledg'd truth, or rule of reaſon.

Now the mind of man is ſo framed, that, when duly inſtructed, it can perceive a want of light in itſelf for ſome purpoſes; or of clearneſs, and completeneſs, in the beſt ideas it is able to frame of ſome things; and, on this account, can ſo far take notice of the extent, and imperfection of its own faculties, as to diſcern, that ſome objects are diſproportionate to it. As, when we attentively conſider the dimenſions of ſpace, or thoſe of the univerſe; we may, by trial, find, that we cannot conceive them ſo great, but that they may be yet greater; or may exceed the bounds, how remote ſoever, which our former conception preſumed to aſſign them. Thus, when the eye looks upon the main ſea, we eaſily grow ſenſible, that, how much ſoever we can diſcover of it, yet our ſight falls far ſhort of the extent of that vaſt object. And 'tis by the ſenſe which the mind has of its own limits, and imperfections, on certain occaſions, that we may eſtimate what things ought not, and what ought to be look'd upon as things above reaſon; for, by that term, I mean not ſuch things as our rational faculty cannot at all reach to, or has no kind of perception of: for, of ſuch things, we cannot, in particular, either think, or ſpeak, like men: but my meaning is this, that the rational ſoul, being conſcious of her own actions, and feeling that ſhe knows ſeveral ſorts of things truly, and clearly; and, thereby, juſtly concluding them to be within the compaſs of her faculties; when ſhe contemplates ſome few things, which ſeem to be of another order, ſhe is convinced, that, however ſhe ſtrains her power, ſhe has no ſuch ideas, or perceptions, of them, as ſhe has, or may have, of thoſe objects, which are not diſproportionate to her faculties.

Again, the nature of the mind is ſuch, that its faculty of drawing conſequences from known truths, is of greater extent, than its power of framing clear, and diſtinct, ideas of things; ſo that, by ſuitable, or ſucceſſive inferences, it may attain to a clear conviction, that ſome things exiſt, of whoſe nature, and properties, it can frame no clear, and ſatiſfactory conceptions. And, that men ſhould be better able to infer propoſitions about ſeveral things, than to penetrate into their nature, is the leſs ſtrange, becauſe 'tis oftentimes ſufficient for our uſes, to know that ſuch things exiſt; tho' that knowledge be not accompany'd with a clear and diſtinct idea; and becauſe the rules, as this, for example, "whatever is produced, muſt have a cauſe," are often clear, and eaſy, that enable the mind to infer concluſions about things, whoſe nature is very abſtruſe.

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To apply, then, these notions to the three sorts of things that I have represented, as being, in some sense, above reason. There are objects of so immense, and peculiar a nature, that, by an easy view of the mind, without subtile, and laborious inquiry, the soul discerns, and, as it were, feels the object to be disproportionate to her powers; and, accordingly, if she tries, she quickly finds herself unable to frame conceptions thereof fit to be acquiesced in: and this sort of objects, I, upon that account, call inconceivable, or supra-intellectual.

But when, by attentively considering the attributes, and operations of a thing, we, sometimes, find, that it hath a property belonging to it; or performs somewhat, which, by reflecting on the Beings, and ways of working that we know already, we cannot discern to be reducible to them, or derivable from them; we then conclude this property, or this operation, to be inexplicable; that is, such as cannot so much as, in a general way, be intelligibly accounted for: and this makes the second sort of our things above reason. But this is not all; for the rational soul, that is already furnish'd with primitive ideas, and rules, of true and false; when she comes to examine certain things, and make successive inferences about them, she finds, that she cannot avoid admitting some consequences as true, and good, which she is not able to reconcile to some other manifest truth, or acknowledg'd proposition. And other truths are so harmonious, that there is no disagreement between any two of them. The heterogeneous truths, I speak of, appear not symmetrical with the rest of the body of truths; and we see not how we can, at once, embrace these, and the rest, without admitting the grand absurdity, which subverts the very foundation of our reasonings, that contradictions may be true. Thus, in the controversy about the endless divisibility of a strait line; as 'tis manifest, that a line of three feet, for instance, is thrice as long as a line of one foot, so that the shorter line is but a third part of the longer; it should follow, that a part of a line would contain as many parts as the whole, since each of them is divisible into infinite parts; which seems repugnant to common sense; and, to contradict one of those axioms in *Euclid*, whereon geometry itself is built. Upon which account, I have ventur'd to call this third sort of things above reason, asymmetrical, or unsociable; of which eminent instances are afforded us, by those controversies, wherein, which side soever of the question you take, you will be unable, directly and truly, to answer the objections that may be urg'd to shew, that you contradict some primitive, or other acknowledg'd truth.

These are some of the considerations, by which I have been induced to distinguish the things that, to me, seem to over-match our reason, into three kinds. Of those I stile inconceivable, our ideas are such, that a moderate attention suffices to make the mind sensible she wants either light, or extent enough, to have a clear and full comprehension of them; those things I have called inexplicable, are such as we cannot perceive to depend upon the ideas wherewith we are furnish'd;

PHYSICS. and to resemble, in their manner of working, any of the agents, whose nature we are acquainted with; and, lastly, those things which I have named unsociable, are such as have notions belonging to them; or have conclusions deducible from them, that appear either disagreeable to our primitive ideas, or, when driven home, inconsistent with the manifest rules we are furnish'd with, to judge of true and false.

But, by sorting things above reason, into three kinds, I do not deny that 'tis possible one object may, in different regards, be referr'd to more than one of these sorts; as may sufficiently appear in that noblest of objects, God; who, when he made the world out of nothing; or, when he discerns the most secret thoughts and intentions of the mind; or, when he unites an immaterial spirit to a human body, and maintains, perhaps, for very many years, that matchless union, with all the wonderful conditions he has annex'd to it; he supplies us with instances of things that are inexplicable: for such operations are not reducible to any of the ways of working known to us, since our minds can but modify themselves by various manners of thinking. And, as for things without us, all that one body can do to another, by acting on it, is to communicate motion to it; and thereby produce in it the natural consequences of such motion; in all which, there is no action like any of those I just now ascribed to God. And since the prescience of those future events, which we call contingent, being a perfection, is not to be deny'd to God; and since this seems irreconcileable to the freedom of human actions, it will afford us an instance of truths, whose consistency, and whose symmetry with the body of other truths, our reason cannot discern; and which, therefore, ought to be referr'd to that sort of things above reason, which I call unsociable.

I come now to the third sort of these things, that mention'd under the name of incomprehensible, or supra-intellectual; which title, whether it belongs to any other object, or no, certainly belongs to God, whose nature comprehending all perfections, in their utmost possible degrees, is not likely to be comprehensible by our minds, who wholly want several of those perfections, and have but moderate shares of the rest. We are, indeed, born with, or, at least, have a power, and several occasions to frame an idea of a Being infinitely perfect; and, by this idea, we may sufficiently discriminate the origin of it, God, from all other objects whatsoever: but, when we come to consider attentively, and minutely, what is contain'd in the notion of omnipotence, omniscience, eternity, and those other attributes that are all united in that great affluence of perfections, God; we may be sure to find, that our faculties are exceedingly surmounted by the vastness and glory of that unlimited, and unparallel'd object; about which, as we can discover that it exists, and that it possesses all the perfection we can conceive; so we may, at the same time, discern, that it must have degrees of perfection, which, because of the inferiority of our nature, we are not able to comprehend. Yet this discovery of God's incomprehensibility, may be made, without subtile inquiries, and without trains of consequences, tho' not without attention, by a direct view of the mind; which finds

finds itfelf, upon trial, as unable, fully to meafure the divine perfections, as the dimenfions of fpace, which we can conceive to be greater and greater, without ever being able to determine any extent beyond whofe limits they cannot reach. PHYSICS.

'Tis indeed, therefore, arrogance to talk of infinite, or of privileg'd things, withthe fame confidence, or to pretend to do it with the fame clearnefs, wherewith knowing men may fpeak of things, unqueftionably within the compafs of our underftandings : but this need not hinder us, nor doth difable us from fpeaking rationally of privileg'd things themfelves. For all allowable notions, are not of the fame fort, or order ; and if none were to be admitted but thofe that enable us to comprehend the object, that is, which give us a clear and diftinct knowledge of all that it contains, or that belongs to it ; I muft confefs, we have no good notions of privileg'd things in particular. I muft add, that I fear we have few or none, even of many things, that we think ourfelves very knowing in. And when we fpeak of things, as being above reafon, tho' we have no clear, diftinct, and adequate idea of them ; yet we may have a general, confufed, and inadequate notion of them ; which may fuffice to make us diftinguifh their refpective objects from all elfe, and from one another; as may be obferv'd in feveral ideas that are negatively framed ; fuch as thofe we have of invifible, incomprehenfible; and in others, which I formerly call'd inferr'd, becaufe they accompany the remote inferences whereby one truth is concluded from another ; as when geometricians infer from fome propofitions in *Euclid*, that any ftrait line may be divided farther and farther, without ftop. For, of this, and fome other propofitions, about privileg'd things, we are not quite deftitute of allowable notions, as may appear by fome admirable fpeculations of mathematicians, about the affections of furd numbers, and incommenfurable magnitudes, of fome of which we have no fuch clear and fymmetrical conceptions, as we have of many other things that are of a nearer, and more intelligible order. I fhall not, therefore, fcruple to acknowledge, that by my own experience, the confeffions of others, and by their unfuccefsful attempts, I am induced to think, that God, who is a moft free agent, having been pleafed to make intelligent Beings, may, perhaps, have made them of different ranks, or orders, whereof men may not be the principal ; and that, whether there be fuch orders, or no, he hath, at leaft, made us men of a limited nature, in general, and of a bounded capacity ; and accordingly, hath furnifh'd man either with certain innate ideas, or models, and principles ; or, with a faculty, or power, and difpofition, eafily to frame them, as it meets with occafions to excite them. But, becaufe God intended the mind of man of a limited capacity, his underftanding is fo conftituted, that the in-bred, or eafily acquired ideas, and primitive axioms, wherewith it is furnifh'd ; and by relation, or analogy whereto, it judges of all other notions and propofitions, do not extend to all knowable objects whatfoever ; but reach only to fuch as have a fufficient affinity, or bear fome proportion to thofe primary ideas, and rules of truth, which are fufficient, if duly improv'd, Physics. to help us to attain, tho' not the perfect knowledge of truths of the highest order, yet the competent knowledge of as much truth, as God thought fit to allow our minds, in their present state of union with our bodies.

And, indeed, I see no reason to repine at the limits which the author of nature assigns to human knowledge. For the number of privileg'd things, is altogether inconsiderable, in comparison of the multitude of others, to which our knowledge may be improv'd to reach; and which it far more concerns us well to know, than it doth to resolve puzzling questions, about things incomprehensible; there being, within the compass of those truths, enough to employ, and reward our diligence, without straining and tiring our reason about objects that transcend it. Yet, even about these, some inquiries may be allowed; for an object that is, on account of some of its properties, privileg'd, may have several others belonging to it, that do not surpass our reason; and whose knowledge may, therefore, be attain'd by the due employment of it. Thus we usefully study the nature of bodies, which make up the object of natural philosophy; tho' the true notion of body, in general, be a thing so difficult to frame, that the best of our modern philosophers can, by no means, agree about it. This I do not wonder at, because, if we pursue the notion of body to the utmost, 'twill lead us to the perplexing controversy *de compositione continui*; and there the understanding will be left in the dark. Thus surveyors, carpenters, architects, and many others, know several properties of the square figure, that are of great use to them in their respective employments; tho' this, that its side, and diagonal, are incommensurable, be unknown to most of them; and if they were told of it, and should prosecute the speculation, 'twould involve them in exceeding great, and, probably, insuperable difficulties.

And, even about privileg'd things, our inquiries, if discreetly manag'd, may not only be allowable, but, sometimes, profitable. For, of such subjects, a studious search may bring us to know more than we did, tho' not so much as we would, nor enough to be acquiesced in. This may, probably, teach us to know both the objects better, and ourselves the better too; by giving us such a sensible discovery of the insufficiency of our understandings, to comprehend all sorts of things, as may be very useful, tho' not pleasing; and may richly reward the pains that ended in so instructive a disappointment. Thus in the noblest instance that can be given, the contemplation of God himself, tho' he has so order'd all things, that 'tis scarce possible for us to be destitute of an idea of him; yet when we come, with a sufficient application of mind, to pry into the wonderful attributes of this most singular, and adorable Being, we are sure to find ourselves unable to comprehend so unbounded an object. This, however, ought not to discourage us from so noble a study, since we are allowed the great contentment and honour to make further and further discoveries of the most excellent of objects, by that very immensity of his perfections that renders it impossible for us to reach to the bounds of his excellency, or rather to discover that it has any bounds at all.

SECT.

SECT. II.

I Proceed, in the next place, to offer ſome rules and directions, whereby to regulate, and eſtimate the reaſonings we meet with, concerning things above reaſon. *Rules for judging of things above reaſon.*

And, firſt, I would obſerve, that as to privileg'd ſubjects, we ſhould not admit any affirmative aſſertion, without ſuch proofs as are ſufficient in their kind. *The firſt rule.*

For, 'tis not reaſonable to give aſſent to any thing, as a truth, without a ſufficient ground for that aſſent. And tho' it may well be granted in the general, that a thing, which ſurpaſſes our reaſon, may have belonging to it ſome property that is alſo above reaſon; yet we are not, in particular, to believe, that this, or that affection belongs to it, without particular and competent proof. For, ſince about a privileg'd thing, as well as about any other, propoſitions may be framed, and often are ſo, contrary to one another; to aſſent to both, were certainly to believe one falſity, if not two. And if we will aſſent but to one, we muſt either judge at adventures, or allow ourſelves to examine the mediums of probation, employ'd on both ſides; and thereupon judge why one of the propoſitions is to be aſſented to, and the other rejected. This manly freedom muſt be allow'd; without which, our underſtandings were liable to be impos'd on, in matters of the higheſt concern: for, there ſcarce ever did, or, I fear, ever will, want ſome men, who, either out of ignorance, and paſſive deluſion, ſelf-confidence, or deſign, take upon them, with great boldneſs, to affirm what they pleaſe about privileg'd ſubjects: and, when they are oppoſed in their extravagancies, by reaſonings they cannot anſwer, they urge, that theſe things, being above reaſon, are not to be judg'd of by it. But, of ſuch men as theſe, I uſually demand, whether their own aſſent to the things they would have us believe, be grounded upon ſome rational argument, or not. If they ſay, 'tis not, they are fools to believe it themſelves; and I ſhould add to the number, if, after this acknowledgment, I believ'd them. But, if they ſay, it is, I deſire them to produce their argument; for, ſince 'tis framed by a human underſtanding, the force of it may, alſo, be comprehended, and judg'd of by a human underſtanding: and 'tis to no purpoſe to ſay, the ſubject ſurpaſſes human reaſon; for, if it do ſo indeed, it will ſurpaſs theirs, as well as mine, and ſo leave us upon even terms. And, let the thing aſſented to, be what it will, the aſſent itſelf ought to be founded upon a ſufficient reaſon; and, conſequently, upon one that is intelligible to the human underſtanding, that is wrought on by it.

The poſitive proofs requir'd for an aſſertion, about a privileg'd thing, muſt, we ſay, be ſufficient in their kind; but we are not to expect rigid demonſtrations of ſuch aſſertions: for, ſince 'tis manifeſt, that there are many truths, ſuch as hiſtorical and political ones, that, by the nature of the things, are not capable of mathematical, or metaphyſical demonſtrations, PHYSICS. tions, yet, being really truths, have a juſt title to our aſſent; it muſt be acknowledg'd, that a rational aſſent may be founded upon proofs that reach not to rigid demonſtrations: it being ſufficient, that they are ſtrong enough to make a wiſe man acquieſce in them. And therefore, if any things can be made out to be reveal'd by God, concerning his own nature, actions, or decrees, we ought to receive them; becauſe, of ſome of thoſe things, as his preſcience, mercy, *&c.* we can have no better proofs; and of others, as, what he did before our world was made, and what he will do with us after we are dead, we can have no other conſiderable proofs at all. There is no reaſon to think, that becauſe an object ſurpaſſes the human underſtanding, it muſt, therefore, ſurpaſs the divine intellect itſelf. Even in the things that are tranſacted in the mind of man, I may learn from another, who is not my ſuperior, what I can by no means attain to know, unleſs he be pleas'd to diſcover it to me.

Intelligibility to the human underſtanding, ſeems no more neceſſary to the truth, or exiſtence of a thing, than that viſibility, to a human eye, ſhould be neceſſary to the exiſtence of an atom, or of a corpuſcle of air, or of the effluvia of a load-ſtone, or the fragrant exhalations of amber-greece, or musk. The natural incapacity of a child, to underſtand the abſtruſe properties of parabolas, hyperbolas, and the incommenſurable lines of a ſquare, hinders not thoſe figures from exiſting, or their properties from being true and demonſtrable. And, tho' we do admit ſome privileg'd things, yet there is no neceſſity that we ſhould be debarr'd from all knowledge of thoſe ſublime objects, in which there are many things, whereof we muſt confeſs ourſelves ignorant. The ancient geometricians knew very well what a rectangular triangle was, when they conceiv'd it to be a figure conſiſting of three ſtrait lines, two of which, comprize a right angle; tho' probably, for a great while, they did not know all its chief properties; ſince, for ought appears, before *Pythagoras*, it was not known that the ſquare of the hypothenuſe, is equal to the ſquares of both the other ſides taken together; and much more likely it is, that they were not able to ſolve thoſe difficulties which attend the endleſs diviſibility of lines inferable from that equality.

But every thing hard to be underſtood, or contrary to the common rules of probability, has not a right to paſs for privileg'd; * for then, the paradoxes about ſurd quantities, the duplicate proportion, and ſeveral other ſurprizing doctrines, capable of mathematical demonſtrations, would be

* " Let it be propoſed (ſays the ingenious Mr. *Hauksbee*) to repreſent the figure of an object, placed behind an opake body, upon the contrary ſide of that opake body; and this without the help of optic-glaſſes, or any foreign adventitious lights: perhaps the ſolution might be thought impoſſible; or the very terms of the problem abſurd and contradictory. For the body, on which the figure is to be ſeen, muſt be opake by the hypotheſis, and the object placed on the contrary ſide to that whereon 'tis ſeen; ſo that either the light muſt be tranſmitted thro' this body, and then 'tis not opake, contrary to the ſuppoſition; or elſe the light muſt not be tranſmitted, and then no figure could " be

be privileg'd. Nor are all those worthy of this title, that are by many proposed and embraced as philosophical mysteries; such as substantial forms, which are really only scholastic chimeras. But tho' I shall not presume, positively, to set down the discriminating bounds, and signs of privileg'd things; yet most, if not all of them, are either primary in their kind, as God himself, and the things whose nature flows immediately from him; or else, things that, if thoroughly inspected, necessarily involve the consideration of some kind of *Infinitum*; or else, are such, that tho' in some principal questions about them, one side must be taken, both sides are encumber'd with absurdities, or scarce superable difficulties. These being the usual marks that belong to privileg'd things, their number cannot be very great; and therefore, we shall not, by allowing them, want objects whereon to exercise our faculties.

A second rule.

2*dly*, We should not be hasty to frame negatives about privileg'd things; or to reject explications, or propositions of them, as if they were absurd, or impossible.

We observe, that even in natural things, 'tis very unsafe to affirm, or reject opinions, before men have a competent historical information of what belongs to the subject they take upon them to judge of. And therefore, it must, in reason, be thought much more unwary to be forward to resolve upon negative propositions, about things which, since we ourselves acknowledge to be above the reach of human reason, 'twill become us, at least to forbear a rude, and insulting way of rejecting the opinions of learned men, who dissent from us, about such things; for the sublimity of the subject may render mistakes the more excusable, because difficult to be avoided; and our own sharing in the inability of penetrating such abstruse things, should keep us from being over-confident, that we, also, may not be mistaken; and incline us to tolerate other men's opinions, about matters wherein ourselves have only opinion, not science.

But, I must not be understood to speak against all framing of negative propositions, about privileg'd things; my design being to dissuade from doing it hastily: for, sometimes, 'tis much more easy and safe, to deny things,

" be seen; for all distributions of light, " by optical artifices, are excluded. " The thing, however, is plain matter of " fact;" as he shews, by lining a globe of glass, in part, with melted sealing-wax, common sulphur, or melted pitch, and giving it a circular motion; for when his hand was then applied to the lined part, he distinctly, and perfectly saw the shape and figure thereof, upon the concave surface of the wax, &c. within. Hence that Gentleman thinks it may be useful to observe, " that many odd effects, and ap- " pearances, against the possibility where- " of, men may seem to argue very plau- " sibly, and to find downright absurdi- " ties, and contradictions in, may yet be " brought about by the genuine force " of nature, acting in convenient circum- " stances, upon proper and suitable bo- " dies;" and therefore, that " we should " not, upon such occasions, proceed to " conclude too peremptorily, what may, " or may not be done; and think that every " difficulty, or apparent impossibility to " us, is a real one to nature." See *Hauksb. Experiment.* p. 167,—171, & p. 268.

than

PHYSICS. than to affirm them to belong to a fubject that furpaffes our reafon. And, the obfervation may be of ufe, efpecially in two cafes; one, when the negative, we affert, is grounded not upon axioms taken from the ufual courfe of nature, or upon propofitions dubious, or remote from the firft principles of knowledge; but upon either univerfal, or metaphyfical axioms; or elfe upon truths manifeftly flowing from fome clear, tho' inadequate notion, we have of the nature of the things we treat. The other cafe is, when we have a clear and fufficient proof by revelation, or otherwife, of the pofitive attributes of the things we contemplate; for then we may fafely deny of that fubject, any other thing that is really inconfiftent with the pofitive attribute. Upon which account, tho' we do not fully comprehend what God is, yet knowing by the clear light of nature, that he is a Being intelligent, and infinitely perfect, we may fafely deny, againft *Epicurus*, *Vorftius*, and Mr. *Hobbs*, that he is a corporeal fubftance; as alfo that he is mortal, or corruptible.

A third rule for judging of things above reafon.

3*dly*, A matter of fact, or other propofition about privileg'd things, being prov'd by arguments competent in their kind, we ought not to deny it merely becaufe we cannot explain, or, perhaps, fo much as conceive the *Modus* of it.

I have obferv'd a want of clearnefs in feveral difcourfes, where the term *Modus* has been employ'd. Sometimes we would deny by it, fo much as a poffibility that one thing can belong to, or be truly faid of another; as when we fay, we underftand not how one creature can create another, or how there can be a line that is neither ftrait nor crooked, or a finite whole number that is neither even nor odd. But moft commonly we mean, by our not underftanding the *Modus* of a thing, that we do not clearly and diftinctly conceive after what manner the property, or other attribute, of a fubject belongs to it, or performs its operations. The firft kind may be call'd a poffible *Modus*, and the other an actual *Modus*. Now in both the foregoing acceptations of the term, we may find inftances fit for our prefent purpofe. For we cannot imagine, how a fhort line, or other finite quantity, fhould be endlefly divifible; or, on the contrary, how infinite parts fhould make but a finite total: and yet geometry conftrains us to admit that it is fo. But tho' there be few inftances of this kind, yet of the other fort of our ignorance of the *Modus* of things, there may be found more inftances than we could wifh there were; for even in natural and corporeal things, the eager difputes of the beft philofophers, and the ingenuous confeffion of the moft judicious and moderate, fufficiently manifeft, that as yet we know not the manner of operation, whereby feveral bodies perform, what we well know they bring to pafs. And not to enter into thofe nice and tedious difputes, about the caufe of the cohefion of the parts of matter in the fmalleft, moft principal, and moft primary bodies; perhaps the way whereby the rational foul exercifes any power over the human body, and the way whereby the underftanding and the will act upon one another, have not yet been intelligibly explain'd by any. The like I may fay of the phenomena of the memory; efpecially in thofe who

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are eminent for that faculty. 'Tis hard to conceive, how in ſo narrow a compaſs as part of a human brain, there ſhould be ſo many thouſand diſtinct cells or impreſſions, as are requiſite to harbour the characters or ſignatures of ſeveral languages, each of them conſiſting of many thouſand different words; beſides the images or models of ſo many thouſand faces, ſchemes, buildings, and other ſenſible objects, and the ideas of ſo many thouſand notions and thoughts, and the diſtinct traces of multitudes of other things: and how all theſe ſhall, in ſo ſmall a ſpace, have ſuch deep and laſting impreſſions made for them, and be oftentimes lodg'd ſo exactly in the order wherein they were at firſt committed to the memory, that upon a ſudden command of the will, or a ſlight caſual hint, a whole ſett of words, things, and circumſtances, will in a trice, as it were, ſtart up, and preſent themſelves, even in the very ſeries, order, and manner, wherein they long before were ranged. And I doubt not, that beſides thoſe abſtruſe things, about the *Modus* of which, the more candid philoſophers have confeſs'd their ignorance, there would others have been taken notice of, if we did but as fully and impartially inquire into the nature of all the things we think we know. And conſidering the yet depending diſputes between philoſophers and mathematicians, about the nature of place and motion, which are things obvious and familiar to us; one might, without other inducements, be inclined to think, we ſhould find many difficulties in many other ſubjects, wherein we do not now take notice of any, if we particularly ſtudied their nature; and that our acquieſcing in what we have learn'd about many things, proceeds not from our greater knowledge of their nature, but from our having exerciſed leſs curioſity and attention in conſidering it. And if in things corporeal, that are the familiar objects of our ſenſes, we are often reduced to confeſs our ignorance of the *Modus* of their exiſting, or operating; it will not be denied, that to a Being wholly unapproachable by our ſenſes, natural philoſophy may be allow'd to aſcribe ſome things, whoſe *Modus* is not attainable by our underſtanding: as the divine preſcience, which, as 'twere impious to deny, ſo, I fear, 'tis impoſſible to explain, as to the *Modus* of it.

And, perhaps, upon the ſame grounds, 'tis as inconceivable, that God ſhould ſee our thoughts, as how he can know our outward actions; for ſince we have no way of diſcerning the particular motions of mens bodies, but by ſome of our ſenſes, eſpecially our ſight; and ſince thoſe ſenſations themſelves neceſſarily require organs duly conſtituted, that is, made up of ſeveral parts, framed, and join'd together after a determinate manner; I ſee not how we can explain the perception of viſible objects, without an eye, or ſo much as any corporeal organ or ſubſtance; eſpecially, ſince 'tis very juſtly aſſerted, that the deity is not united to any portion of matter, as the human ſoul is to the human body.

We do not, however, deny to God either the power of moving matter, or that of perceiving all its motions; for which there is ſome poſitive proof, competent in its kind. For if there be an effect, that we diſcern muſt proceed from ſuch a cauſe or agent, we may conclude, that ſuch a cauſe there

PHYSICS. there is; tho' we do not particularly conceive how, or by what operation, 'tis able to produce the acknowledg'd effect. Thus, tho' a man, otherwise of good judgment, being wholly a stranger to mathematics, cannot conceive how a skilful astronomer should, many years before-hand, foretel eclipses to a day and hour, and perhaps to a few minutes; yet when the success, as it often happens, verifies such predictions, he will be satisfied that the maker of them had the skill to foreknow the things foretold in them. And so the generality of learned men among us, who are not much acquainted with that part of navigation, which some moderns call *Limeneuretica*, or the art of steering to harbours, cannot well conceive how a ship that is, for instance, in the vast *Atlantic* ocean, above a thousand miles from any shore, should be so directed, as to arrive just at a little harbour, not cannon-shot over; which perhaps neither the pilot, nor any other in the ship, ever saw. And yet, as little as we distinctly conceive how such an art of finding ports can be framed, we scruple not to allow there is such an one, because navigators to the *East* and *West-Indies* could not, without it, find the remote ports they are bound for.

There is a second sort of positive proofs, consisting of those consequences that are clearly and justly drawn from any manifest, acknowledg'd, or already demonstrated truth. To this belong several mathematical propositions and corollaries; which, tho' being nakedly proposed, they seem incredible to the generality of learned men, and sometimes to mathematicians themselves, are yet fully assented to, because they clearly follow from either manifest or demonstrated truths. Thus, many cannot conceive how 'tis possible there may be millions of circles, whose circumferences shall each of them come nearer and nearer to another, and to a strait line assign'd; and yet none of them touch, and much less cut either any other circle, or that line, but in the same point: and yet this is one of those odd propositions that geometricians have rightly deduced, as corollaries from a proposition of *Euclid*. And, tho' we cannot clearly conceive, how two lines, that, at their remotest ends, are but little distant from each other, should perpetually incline to each other, without ever meeting; yet geometricians, that is, the most rigid reasoners we know of, have been compell'd to admit this in the conchoid of *Nicomedes*.

A fourth rule. 4*thly*, When we treat of privileg'd subjects, we are not bound to suppose every thing false, that seems to oppose some receiv'd dictate of reason.

For, it being evident, that a great part of the dictates of reason are negative, and that negative propositions usually spring from the repugnancy we judge some things have to a positive dictate of reason; if those positive dictates contain but gradual and limited truths, and come to be unduly extended to privileg'd subjects, it may very possibly happen, that a thing, really true, will appear false, if judg'd of by its agreement to one of those limited, and but respective dictates. 'Tis also clear, that, in natural philosophy, the usual ground on which we reject many things, is, that we judge them unintelligible. And, I censure not the practice in general, but think

think it may eaſily miſlead us, when extended to things that apparently tranſcend our reaſon, as, for ought yet appears, ſome of the modus's, even of things corporeal, are found to do. And, we ſometimes think we have made compleat enumerations of the ſeveral ways of the exiſtence of an attribute in a ſubject, or of the operation of one thing upon another, when, indeed, we have over-look'd one or other of them ; and, perhaps, that which we have thus omitted, may be the true one ; tho', poſſibly, no attention, and diligence of ours, could, in ſome caſes, have ſerv'd the turn ; the *Modus* inquired after, being not conceivable by us, tho' it may be by a higher than a human intellect.

The ſchool-philoſophers, for many ages, in the catalogues they made of the means of a body's working upon another at a diſtance, did not think of the true ways by which odours and ſounds are communicated to us ; and therefore had recourſe to certain unintelligible things, which they call'd intentional ſpecies. But the moderns acknowledge, that odours are communicated by effluvia exhaled from the odorous body, and fitted to affect our noſtrils ; and that ſounds are tranſmitted to the ear, by the undulating motion which the air is put into by the impulſe of the vibrating, or otherwiſe agitated parts of the ſonorous body.

And ſuppoſing the reaſonable ſoul to be an immaterial ſubſtance ; tho', men think they have ſufficiently expreſs'd the ways of determining the motion of a body, by ſaying, that the determination muſt be either in the line wherein the impellent made it move, or in the line wherein it was determin'd to move by the ſituation of the reſiſting body ; yet the motions of the animal ſpirits, if not alſo ſome other internal parts of the body, may, the body being duly diſpoſed, be determin'd by the human will ; which is a way quite different from the other. And how the power of determining the motion of a body, without any power to impart motion to that body, ſhould belong to an immaterial creature, which has no corporeal parts to reſiſt the free paſſage of a body, and thereby change the line of its motion ; is not yet, nor, perhaps, ever will be, in this life, clearly conceived by men : tho' there is no doubt, that he who endow'd the ſoul with this attribute or power, perfectly underſtands both how it exiſts in the ſoul, and how the ſoul, by exerting it, operates on the body.

I am, however, in no wiſe againſt rejecting opinions that are found contrary to thoſe rules of reaſon, at the framing of which, the things in queſtion were duly conſider'd : but in caſes not thought on, when ſuch rules were deviſed, we are not always bound to ſubmit to be judg'd by them ; and to maintain an opinion unconformable to ſuch a rule, may be not to oppoſe a genuine and abſolute dictate of reaſon, but to rectify one that is erroneouſly thought ſo ; by ſhewing that the rule is expreſs'd in more general and indefinite terms than it ought to have been. And, doubtleſs, of two opinions, that is the moſt rational, which is moſt agreeable to thoſe rules of reaſon, which are framed upon the fulleſt information. 'Tis eaſy to ſee, that in the rule I propoſe, very few of the caſes occurring in ordinary diſcourſe, or even in that of philoſophers, will be at all concern'd.

PHYSICS. cern'd. And in thofe few cafes, wherein I intend the rule fhould take place, I obviate inconveniencies by a double caution. The firft, by fuppofing that the opinion, which claims an exemption from the common rules, it not an arbitrary or precarious tenet, but fufficiently made out by proper arguments: and the fecond, by declaring that 'tis not to contradict right reafon, but bad reafoners, to give limitation to rules which have been too haftily framed and conceived, in too general terms, by men who either were not competently inform'd of the variety of particulars, when they took upon them to make analyfes and enumerations; or elfe prefumed to infer, that a thing was not, becaufe they did not underftand the *Modus* of its exiftence or operation. And I have often thought, that the caufes of the great clamour that is made againft fome men, for not obfequioufly fubmitting to what fome others call the rules of reafon, are, that men do not fufficiently underftand the nature of things, and of themfelves; but entertain too narrow conceptions of the former, and too high an opinion of the latter.

Reafon, what? Reafon is often taken for a fett of notions and propofitions, employ'd and acquiefced in by this or that fort of reafoners, who receive names from this or that particular difcipline, as aftronomy, chymiftry, optics; of whofe receiv'd doctrines they are fuppofed to be intirely maintainers. But it is alfo, with at leaft as much propriety, ufed to fignify the rational faculty itfelf, furnifh'd with the light that accompanies it, when it is rightly difpofed and inform'd. In the firft of thefe two fenfes, it feems but equitable to allow, that fome things ought not to be judg'd by all the fame rules employ'd to judge other things by; for fome of thefe rules were framed upon a flight confideration of common and familiar things, either by the vulgar, or by men who, for want of skill or application of mind, did not critically regard the diftinct natures of things; and yet prefumed to fettle rules, which other mens inadvertence or lazinefs has made them receive for certain dictates of reafon: whereas, other natures fhould have been then confider'd, as well as thofe; but not having been fo, the rules I fpeak of, are not always proper and fafe, when applied to thefe overlook'd natures. Thus, fucceffive Beings, as time and local motion, in fome cafes require to be eftimated by other meafures than fubftances, whether material or incorporeal. And fo, likewife, the more nice metaphyficians, efpecially among the moderns, have thought themfelves obliged to difcourfe of modus's, relations, privations, extrinfecal denominations, *&c.* in a very different way from that which belongs to bodies and fpirits; tho' the unskilful are ftill apt to confound all thefe fubjects, by applying to them, indifcriminately, the fame rules, or, as they imagine, dictates of reafon.

The generality of philofophers, after *Ariftotle*, conceive place to be the immovable and immediately contiguous concave furface of the ambient body; fo that 'tis a kind of veffel, which every way contains the body lodg'd in it; but with this difference, that a veffel is a kind of moveable place; as, when a bottle of wine is carried from the cellar to the table: but

but place an immoveable veſſel, or a veſſel conſider'd as immoveable. PHYSICS.
Now ſuppoſing, with *Ariſtotle*, and the generality of philoſophers, the plenitude of the world; it may be truly ſaid, that all plants, animals, minerals, ſtars, and other bodies, are each of them in ſuch an *Ariſtotelian* place; whence it has been uſually affirm'd, that what is in no place, is not at all: yet it appears not, how the outermoſt heaven can be properly ſaid to be in a place; ſince theſe philoſophers, aſſerting the world to be finite, muſt grant there is no ambient body without it, to contain it. And if the outermoſt heaven ſhould be impell'd, by the power of God, in a ſtrait line this way or that way, there would enſue a motion without change of place; for the outermoſt heaven was in none before, and does not, by its progreſſion, come to be contained by a new ambient body. And in this caſe, even according to thoſe modern favourers of *Ariſtotle*, who approve *Des Cartes's* definition of local motion, the world may be ſaid to move, without changing place; for it does not paſs from the neighbourhood of ſome bodies to that of others; ſince comprizing all bodies, and yet being bounded, there is no body for it to leave behind, nor any beyond, for it to approach: and therefore, tho' every particular body in the univerſe is naturally capable of local motion, yet the univerſe itſelf is not; and tho' every particular body in the world has ſome determinate figure, yet the world itſelf, if it be, according to the *Carteſians*, indefinite, has none.

Ariſtotle, and the philoſophers ſince his time, have generally admitted the diviſion, eſtabliſh'd by him, of all Beings into ſubſtance and accident; and accommodated their rules to one of them, or to both. But *Gaſſendus*, and his followers, have introduced a third ſort of Beings, as not being either ſubſtances or accidents. Of this kind of things, they make place or ſpace to be; for they will not allow it to be a ſubſtance, becauſe it is neither body nor ſpirit; but only ſomewhat that has a capacity to receive or contain bodies; and would ſubſiſt, tho' God ſhould annihilate all the ſubſtances he has created. And, for the ſame reaſon, it is not to be call'd an accident, ſince that neceſſarily requires a ſubſtance to reſide in; whereas, in caſe of the annihilation of the world itſelf, and conſequently all the ſubſtances that compoſe it, their place or ſpace would ſtill remain, and be capable of admitting a new world of the ſame extent; if God ſhould be pleaſed to create it: whence *Gaſſendus* wittily infers, that bodies are rather accidental in reſpect of ſpace, than ſpace in reſpect of bodies. But he who ſhall, with an heedful and unprejudiced eye, ſurvey the ſeveral hypotheſes, or ſyſtems, maintain'd by the different ſects of philoſophers, may find, that tho' the inſtances will not be all of them the ſame, yet there are none of theſe ſyſtems, wherein there may not be obſerv'd ſomething or other, whereto every one of the rules, which reach to the other ſubjects treated of in that philoſophy, cannot ſafely be applied. And, indeed, the mind of man being, naturally, far more deſirous to know much, than to take the pains requiſite to examine whether it does ſo or not; is very apt to think, that any ſmall number of things it has not diſtinctly conſider'd, muſt be of the ſame nature and condition with the reſt, that it judges of

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PHYSICS. the ſame kind. For by thus eaſily attaining to the knowledge of things, the mind gratifies, at once, both its vanity, and its lazineſs; looking upon theſe concluſions, as marks of the excellency of its rational faculty; whilſt they rather proceed from a want of the due exerciſe of it.

The internal light of the mind.

But, ſince there is no progreſs *ad infinitum* in the *Criteria* of truth; and ſince our faculties are the beſt inſtruments that God has given us, to diſcover, and to examine it by; I think, a clear light, or evidence of perception, ſhining in the underſtanding, affords us the greateſt aſſurance we can have, in a natural way, of the truth of the judgments we paſs upon things. And, it is not by induction, but by evidence, that we know nothing but truth follows from truth. By which it appears, that the innate light of the rational faculty, is more primary than the very rules of reaſoning; ſince, by that light, we judge even of this axiom, which is itſelf the grand principle of reaſoning by inference. And, as the underſtanding is uſually look'd upon to be the eye of the mind, there is this analogy between them, that there are ſome things which the eye may diſcern mechanically, or by the help of inſtruments; as when it judges a line to be ſtrait, by the application of a ruler to it, or to be perpendicular, by the help of a plumb-line; or a circle to be perfect, by the help of a pair of compaſſes: but there are other things which the eye perceives and judges of immediately, by intuition, and without the help of organs, or inſtruments; as when, by the bare evidence of the perception, it knows, that this colour is red, and that blue; that ſnow is white, not black; and a coal, black, not white. For, thus there are ſome things, which the intellect uſually judges of in a kind of organical way; that is, by the help of certain rules, or hypotheſes, ſuch as are a great part of the theorems, and concluſions in philoſophy, and divinity; but there are others, which it knows without the help of theſe rules, more immediately, and, as it were, intuitively, by evidence, or perception; as that "two contradictory propoſitions cannot both be true;" that "from truth, nothing but truth can juſtly be deduced," *&c.* 'Tis alſo, upon this evidence of perception, that we receive, with an undoubted aſſent, many primitive ideas and notions; ſuch as thoſe of extended ſubſtance, or body; diviſibility, or local motion, a ſtrait line, a circle, *&c.*

And, it ſeems to me, that the internal light, which the author of nature has ſet up in the mind of man, qualifies him, if he makes a right uſe of it, not only to apply the inſtruments of knowledge, but alſo to frame, and to examine them. For, by the help of this light, the underſtanding is enabled to look about, and both to conſider a-part, and compare together, the natures of all kind of things; without being neceſſitated to employ, in its ſpeculations, the rules, or dictates of any particular ſcience, or diſcipline; being ſufficiently aſſiſted by its own light, and thoſe axioms and notions that are of a general nature, and perpetual truths; and ſo, of a higher order than the dictates or rules of any particular, or ſubordinate ſcience, or art. And, by theſe means, the underſtanding may perceive the imperfection and falſity of ſuch rules, or theorems, as thoſe men who look no

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higher, nor further than their own particular ſcience, or art, embrace for certain and unqueſtionable. Thus, philoſophers obſerving, that they could frame a clear notion of a thing, without conſidering, whether it were actually in being, or not; or even when they ſuppoſed, that 'twas not actually in being; as we can frame a clear conception of a roſe in winter, when there are none to be found growing; and have a notion of a myriagon, tho', very likely, there is no ſuch figure really exiſting in the world; men have generally concluded, that the eſſence of things is different, and ſeparable from their exiſtence: yet, when we conſider, that God is a Being infinitely perfect; and, that actual exiſtence, being a perfection, muſt belong to him; we may, by the ſame light of reaſon, that dictated eſſence and exiſtence to be two ſeparable things in all other Beings, diſcern, that they muſt be inſeparable in God; and, conſequently, that the fore-mention'd rule, tho' more general than almoſt any other, is not abſolutely univerſal, but muſt be limited by the light of reaſon. And thus, alſo, philoſophers conſidering, that not only all ſorts of bodies, but the immaterial ſouls of men, are endow'd with qualities, which are accidents, have included it in the very notion of a ſubſtance, to be the ſubject of accidents. But the free intellect, finding in itſelf a notion of an abſolutely perfect, and, therefore, exiſtent Being; and conſidering that to be the ſubject of accidents, is not a thing agreeable to the higheſt perfection poſſible, it concludes, that in God there are no accidents. And this concluſion has been embraced as a part, not only of chriſtian, but of natural theology; and maintain'd, by ſeveral philoſophers themſelves, upon metaphyſical, and other merely rational grounds. In ſhort, the native light of the mind may enable a man, who will make a free, and induſtrious uſe of it, both to paſs a right judgment of the extent of thoſe very dictates, that are commonly taken for rules of reaſon; and to frame others on purpoſe for privileg'd things.

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5*thly*, Where privileg'd things are concern'd, we are not bound to reject, as falſe, whatever we know not how to reconcile with ſomething that is true. *A fifth rule.*

I think, it will not be doubted, that there are, or may be conceiv'd, ſtrait lines, whereof one is a hundred, or a thouſand times longer than another; 'tis alſo generally granted, that a longer line conſiſts of, or may afford more parts, than a ſhorter: and, laſtly, 'tis generally acknowledg'd, that no number can be greater than infinite. Now, I ſhould gladly ſee theſe propoſitions reconciled to the demonſtrations of geometricians, about the endleſs diviſibility of all ſtrait lines; whence they deduce, that tho' they be very unequal among themſelves, yet the ſhorteſt of them contains, or may afford, infinite parts. I am far from affirming, that one truth can really contradict another; yet, I think, that which is but gradual, or limited truth, may, in ſome few caſes, be irreconcileable, by us, to an abſolute and univerſal truth. We may diſtinguiſh thoſe propoſitions, we call true, into axioms metaphyſical, or univerſal, that hold in all caſes, without reſerve; and axioms collected, or emergent; by which, I mean ſuch

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PHYSICS. as result from comparing together many particulars, that agree in something common to them all. And some of these, tho' they be so general, that, in the usual subjects of our reasoning, they admit of no exceptions, yet may not be absolutely, and without limitation, true. Of this we have an instance, even in that axiom which, almost, all natural philosophers have supposed, and built on, that something cannot be made out of nothing; which, tho' at least, one of the highest, or gradual truths, may yet not be universally true; since, for ought we know, God, who is acknowledg'd a Being infinitely perfect, may possess, and have exercis'd the power of creating. And, in such cases as this, not to be able to reconcile a truth, concerning a privileg'd thing, with a proposition that generally passes for true, will not presently oblige us to reject either proposition, as false; but, sometimes, without destroying either, only to give one of them a due limitation, and restrain it to those sorts of things, on which 'twas at first grounded; and to which 'twas, because of man's ignorance, or inconsiderateness, not at first confined. And, if the miracles vouch'd for any religion, be any of them granted true, it cannot well be denied, that physical propositions are but limited, and such as I call collected truths; being gather'd from the settled phenomena of nature, and liable to this limitation, or exception, that they hold, only where the irresistible power of God, or some other supernatural agent, is not interpos'd, to alter the course of nature.

Now, the reason why we judge things are repugnant, being, that the notions, or ideas we have of them, seem to us inconsistent; if either of these notions be wrong framed, or be judg'd of, by an unfit rule, we may think those propositions to be contradictory that really are not. Thus those used to employ their imaginations about things, which are the proper objects of the intellect, are apt to pronounce others to be inconceivable, only because they find them unimaginable; as if the fancy, and the intellect, were faculties of the same extent. Upon which account, some have so grosly erred, as to deny all immaterial substances; and chose, rather, so far to degrade the Deity itself, as to impute to it a corporeal nature, than to allow any thing to have a being, that is not comprehensible by their imagination; which themselves acknowledge to be but a corporeal faculty. But, besides this mistake of things repugnant, which arises from the misapplication, or mismanagement of our discerning faculties, there may be another, that proceeds from the imperfection, and limitation of our understanding; which being unable to judge of privileg'd things, as it does of other objects, may, sometimes, be unable to discover the reconcileableness, which a more illuminated, and penetrating faculty, may discern. This may be illustrated, by what usually happens at sea, where there is a free prospect; when, looking towards the main, the sky, and the waters, seem to meet at the edge of the sensible horizon; tho', indeed, they are as far distant, as the heavens from the earth. On the other hand, if you skilfully mix together, the dry and fine powder of orpiment, and that of indigo, you will produce a green colour, as is known to painters; wherein in the eye takes notice but of an uniform mixture, in which it diſtinguiſhes neither blue, nor yellow; but if you look on this mixture thro' a microſcope, the former colour diſappears, and you will plainly ſee, inſtead of it, blue and yellow grains of the powder, diſtinct from one another. Theſe inſtances may ſerve to ſhew the weakneſs of our viſive faculty; and the latter of them teaches, that a thing may appear one and different, as 'tis look'd upon by a more or leſs diſcerning eye. But an inſtance, more to our preſent purpoſe, is afforded by yellow diamonds, which, becauſe of their colour, the generality of goldſmiths take to be counterfeit gems; tho' very skilful lapidaries will, by ſure ſigns, diſcover, and acknowledge them to be true diamonds. Whence we learn, that a more skilful judge may diſcern an agreement in things that almoſt all other men think they ſee manifeſtly to be of different natures.

I have, alſo, ſeveral times obſerved, that men judge two things to be irreconcileable, not only when they are both of them repreſented to the underſtanding in the form of propoſitions, but when one of them is only a notion, or a current definition. For ſeveral of theſe notions contain in them a propoſition, or are equivalent to it: as, when a circle is defined to be a figure contain'd by a line, all whoſe parts are equally diſtant from a point in the middle; this definition contains an affirmation of the eſſential property of a circle; which, by the generality of geometricians, is, therefore, diſtinguiſh'd from that conic ſection they call an ellipſis; tho' that be alſo a figure terminated by one curve line.

But, if a man has not genuine, and adequate notions of the things he judges of; he may confidently, and even upon very probable grounds, take things to be inconſiſtent, that, in reality, are not ſo. If an ordinary mathematician ſhould hear one man ſay, that ſuch a figure is an ellipſis, and another affirm it to be a circle; he would think their aſſertions to be inconſiſtent, having his mind prepoſſeſſed with an ellipſis being a conic ſection, whoſe properties muſt, therefore, he ſuppoſes, be very different from thoſe of a circle; whereas, ſuch wary geometricians, as the learned Dr. *Wallis*, will tell him, that the vulgar notions of conic ſections are not adequate to the figures producible by them. 'Tis true, when a right cone is cut quite thro' by an inclining plane, the figure produced by the ſection, agrees well with the receiv'd notion of an ellipſis, in which the diameters are of an unequal length; yet if the plane cut the cone parallel to the baſis, that conic ſection will be a true circle, having all its diameters equal.

'Tis, indeed, an uncommon, and unheeded account, but ſuch an one upon which I have obſerv'd, not only logicians, but philoſophers themſelves, to err about judging things reconcileable, or inconſiſtent; that if a man be not ſufficiently acquainted with the nature of either of the two things under conſideration, he may think there is a contradiction between them, when there is none to a ſuperior underſtanding. For, taking it for granted, that he knows one thing for a truth; if ſome other thing be affirm'd to be ſo, which he has not skill enough to ſee how to reconcile to

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take for a truth; provided the distrusted proposition be such as we would acquiesce in, if we could reconcile it to the supposed truth. PHYSICS.

Upon the whole, it appears, that when two propositions are laid down, one whereof is made evident to us by experience, or by reason, acting within its own jurisdiction, or compass; and the other sufficiently prov'd, by being mathematically demonstrated, or duly attested by divine revelation; we ought not to reject either of these propositions as no truth, merely because we do not yet know how to reconcile them: but we should rather think, that the collected proposition is only a gradual, or limited truth; or else consider, that we knowing but so imperfectly as we do, the particular natures of privileged subjects, there may, possibly, be a superior intellect, able to discern an agreement between what is deliver'd about that subject, and the affirmation which seems repugnant to it; tho' we are not quick-sighted enough to perceive this agreement.

Nor will this doctrine, if duly limited to the subjects wherein alone I would have it admitted, expose us to have falsities imposed on us at the pleasure of bold, and dictating men. For, if neither of the things be privileged, but both in the jurisdiction of ordinary reason; I require, that the propositions framed about them, be estimated according to the common dictates of reason. And, even in cases where one of the propositions is about a privileged thing; I do not at all think fit, that it should be receiv'd, in spite of its being repugnant to the gradual truth deliver'd in the other; unless it can, by some other argument, sufficient in its kind, be prov'd to be true. Thus, tho' men know not how to reconcile the liberty of their own will with the infallible knowledge that God has of those actions that flow from it; yet they have unanimously judg'd it reasonable to believe both free will, and prescience: the former, because they felt it in themselves; and the latter, because the fore-knowledge of things, being manifestly a perfection, ought not to be deny'd to God, whom they look'd upon as a Being supremely perfect; and because some actions, and events, that they all judg'd to flow from mens free will, were, as the generality believ'd, foretold by prophetic oracles. But, since we have scarce any way of discovering a falsity, but by its being repugnant to somewhat that is true; to deny, that, in cases within the jurisdiction of ordinary reason, the repugnancy of a proposition to any manifest truth ought to sway our judgments, were to deprive us of the most useful criterion to distinguish between truth and falshood.

The sixth and last rule for judging of things above reason.

Lastly, in privileged things, we ought not always to condemn that opinion which is liable to ill consequences, and encumber'd with great inconveniences, provided the positive proofs of it be sufficient in their kind.

That this rule may be the more easily admitted, I shall, separately, suggest three things, which, I desire, may be, afterwards, consider'd all together.

First, clear positive proofs, proportionate to the nature of things, are genuine, and proper motives to induce the understanding to assent to a

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The fecond thing I would offer, is, that we have fhewn there are fome things which our imperfect underftandings either cannot, or, at leaft, do not perfectly comprehend; and that neverthelefs men have not refrain'd from prefuming to dogmatize, and frame notions and rules about fuch things, as if they underftood them very well. Whence it muft needs come to pafs, that if they were miftaken, as in things fo abftrufe 'tis very likely they often were; thofe who judge by the rules they laid down, muft conceive the propofitions oppofite to their miftakes to be liable to very great, if not infuperable difficulties and objections.

Thirdly, as we need not wonder that privileg'd things, which are ufually fo fublime, as to have been out of the view of thofe who framed the rules whereby we judge of other things, fhould be thought liable to great objections by them who judge of all things only by thofe rules; fo we fhould not require or expect more evidence of a truth relating to fuch things, than that there are for it fuch fufficient pofitive reafons, as notwithftanding objections and inconveniencies, make it, upon the whole, worthy to be embraced.

Euclid, indeed, befides that more fatisfactory way of direct probation, which perhaps he might have oftener employ'd than he did, has fometimes, where he thought it needful, had recourfe to a way of arguing to an abfurdity. But in thefe cafes, he never goes out of the difcipline he treats of; and confining himfelf to arguments drawn from quantity, he urges nothing as abfurd, but what is undeniably repugnant to fome truth he had already demonftrated, or to thofe clear and undifputed definitions, axioms, or *Poftulata*, which he fuppofes to have been already granted by thofe he would convince. But tho' he thus argues, to prove that his readers cannot contradict him without contradicting themfelves; yet we find not that he was at all folicitous to clear thofe difficulties, that a man fo quick-fighted could not but know fome of his theorems were attended with; but he is contented to demonftrate the incommenfurability of the side and diagonal of a square, without troubling himself to take notice of the seeming absurdities that attend the endless divisibility of a line, which would follow from what he demonstrated.

'Tis true, about some privileg'd things there are, and about some others there may be, contradictory opinions maintain'd. Now, as both of these cannot be true, one of them must be so; as, tho' it be hotly disputed whether quantity be endlesly divisible, yet certainly it either must, or must not be divisible without end: and, as was formerly observ'd, which side soever you take, the inconveniencies will be exceeding great; and, perhaps, there will lie objections against it scarce to be directly answer'd. And since one of the two opposite opinions must be true, it will not always be necessary that an opinion must be false, which is encumber'd with great difficulties, or liable to puzzling objections. And, therefore, if the positive proofs on one side be clear and cogent, tho' there be perplexing difficulties objected by the other; the truth ought not, for their sake, to be rejected: because such difficulties proceeding, usually, either from notions that men presume to frame about things above their reach, or from rules that were not made for such points as are in dispute; the objections are not to be judg'd so well founded, as is that acknowledg'd principle in reasoning, "from truth nothing but truth can be justly infer'd."

I confess, I have always thought it reasonable, in such cases, to compare as well the positive proofs of one opinion with those of the other, as the objections that are urg'd on either side; and thence make my estimate, upon the whole, tho' with a peculiar regard to that opinion which has a great advantage in point of positive arguments; because those are, unquestionably, the proper inducements to assent. And, then, the objections may well enough be suspected to proceed from the abstruse nature of privileged things, and the great narrowness of the rules whereby men usually judge of things. For we may have a sufficiently clear proof, that a thing is, whilst we have no satisfactory conception of its manner of existing, or operating; our infer'd knowledge being clearer, and extending farther, than our intuitive, or apprehensive knowledge.

But, even about things that we cannot sufficiently understand, we may, in some cases, exercise our reason, in answering objections that are thought unanswerable, because not directly so. For we may, sometimes, shew, by framing in another case a like argument, which the adversary must confess does not conclude well; that neither does the argument, containing his objection, conclude right.

However, we must not expect to be able, as to privileged things, and the propositions that may be framed about them, to resolve all difficulties, and answer all objections; since we can never directly answer those which require for their solution a perfect comprehension of what is infinite. As a man cannot well answer the objections that may be made against the *Antipodes*, the doctrine of eclipses, that of the different phases of the moon, and of the long days and nights of some months near the

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poles, the theory of the planets, *&c.* unlefs he underftand the nature of the fphere, and fome other principles of cofmography, and aftronomy. So that where privileged things are concern'd, clear and pofitive arguments ought to be of great weight, in favour of the opinion they conclude for; even when, on the contrary fide, we may difcourfe ourfelves into fuch difficulties, as perplex, and, perchance, puzzle our limited underftandings.

THE

Philosophical Difficulties

Relating to the

RESURRECTION,

CONSIDER'D.

THE question, I here design to consider, is, Whether to believe the resurrection of the dead, which the christian religion teaches, be to believe an impossibility? *Preliminary observations.*

I do not pretend, that the resurrection is a thing knowable, or directly provable, by the mere light of nature; nor that it may possibly be effected by mere physical agents: so that, treating of the possibility of the general resurrection, I here take it for granted, that God has been pleased to promise and declare, that there shall be one; and that it shall be effected, not according to the ordinary course of nature, but by his own immediate power.

It must also be observed, that the resurrection taught by the christian religion, is not here meant, in such a latitude, as to comprize all that any particular church, or sect of christians, much less what any private writer hath taught about it; but only what is plainly deliver'd as to this point, in the scripture.

I must further premise, that 'tis no easy matter to determine what is absolutely necessary, and but sufficient to make a portion of matter, consider'd at different times, or places, fit to be reputed the same body. That the generality of men, in vulgar discourse, allow themselves a great latitude, in this case, will be easily granted, by him who observes the receiv'd forms of speaking. Thus *Rome* is said to be the same city, tho' it hath been often taken, and ruin'd by the *Barbarians*, and others. Thus, an university is said to be the same, tho' some colleges fall to ruin, and new ones are built; and, tho' once in an age, all the persons who compos'd it, are succeeded by others. Thus, the *Thames* is said to be the same river, that it was in the time of our fore-fathers; tho' indeed, the water, *Identity; the difficulties of conceiving it.*

that

 that now runs under *London-bridge*, is not the ſame that ran there an hour ago, and is quite other than that which will run there an hour hence. And, ſo the flame of a candle, is ſaid to be the ſame, for many hours together, tho' it, indeed, be every minute a new body; and tho' the kindled particles that compoſe it, at any time aſſign'd, are continually putting off the form of flame, and are repair'd by a ſucceſſion of the like.

Nor is it by the vulgar only, that the notion of identity has been hard to be obtain'd; for, it ſeems, that even the ancient philoſophers have been puzzled about it; witneſs their diſputes, whether the ſhip of *Theſeus* were the ſame, after it had been ſo far repair'd, from time to time, to preſerve it as a monument, that ſcarce any plank remain'd of the former timber. And, even, in metaphyſics, I think it no eaſy task, to eſtabliſh a true and adequate notion of identity; and clearly to determine, what is the true principle of individuation. And, this is not ſurprizing; for, almoſt every man, who thinks, conceives in his mind, this, or that quality, relation, or aggregate of qualities, to be that which is eſſential to a certain body, and proper to give it a peculiar denomination; whereby it comes to paſs, that, as one man chiefly reſpects this thing, and another that, in a body that bears ſuch a name; ſo one may eaſily look upon a body as the ſame, becauſe it retains what he chiefly conſider'd in it; whilſt another thinks it to be chang'd, becauſe it has loſt that which he thought was the denominating quality, or attribute. Thus philoſophers, and phyſicians diſagree about water, and ice; ſome taking the latter to be but the former diſguiz'd; becauſe they are both of them cold, and ſimple bodies; and the latter eaſily reducible to the former, by being freed from the exceſſive adventitious degree of coldneſs; whilſt others, looking upon fluidity, as eſſential to water, think ice, upon account of its ſolidity, to be a diſtinct ſpecies of body. And ſo the *Peripatetics*, and chymiſts often diſagree about the aſhes and *Calces* of burnt bodies; the firſt referring them to earth, becauſe of their permanency, and fixedneſs; and many chymiſts taking them to be bodies *ſui generis*, becauſe common aſhes uſually contain a cauſtic ſalt; whereas earth ought to be inſipid. And, the like may be ſaid of ſome wood-aſhes, and lime-ſtone, and even coral; which when well calcin'd, and freſh, have a pungent taſt: beſides, ſome of them, that are inſipid, may be eaſily reduced into metals, as the *Calces* of lead and copper.

Theſe difficulties, about the notion of identity, being obſerv'd, will render it leſs ſtrange, that, among the ancient *Hebrews* and *Greeks*, whoſe languages were ſo remote, in ſeveral reſpects from ours, the familiar expreſſions, employ'd about the ſameneſs of a body, ſhould not be ſo preciſe, as thoſe who maintain the reſurrection, in the moſt rigid ſenſe, would have them.

In the next place, I obſerve, we may, agreeably with ſcripture, ſuppoſe, that a comparatively ſmall quantity of the matter of a body, being increas'd, either by aſſimilation, or other convenient appoſition of parts, may bear the name of the former body; as a large crop of corn, ariſing from a comparatively ſmall quantity of ſeed ſown.

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And, here it may be proper to mention an experiment, made by two acquaintance of mine, men of great veracity and judgment; who both aſſured me, that having ſown in a garden, ſome aſhes of a plant, like our *Engliſh* red poppy, they, ſooner than was expected, produced certain plants, larger and fairer than any of that kind, which had been ſeen in thoſe parts. Now, this ſeems to argue, that in the ſaline and earthy, that is, the fix'd particles of a vegetable, that has been diſſipated, and deſtroyed by the violence of the fire, there might remain a plaſtic power, enabling them to contrive diſpos'd matter ſo, as to re-produce ſuch a body as was formerly deſtroy'd. But, to this plaſtic power, reſiding in any portion of the deſtroy'd body itſelf, it will not, perhaps, be neceſſary to have recourſe, in our preſent undertaking; ſince an external, and omnipotent agent, can, without it, perform all that I need contend for.

The firſt objection againſt the reſurrection.

To come, then, to a more cloſe conſideration of thoſe difficulties, which are ſaid to demonſtrate the impoſſibility of the reſurrection.

'Tis ſaid, when a man is once really dead, many parts of his body will, according to the courſe of nature, reſolve themſelves into multitudes of ſteams, that wander in the air; and the remaining parts, which are either liquid, or ſoft, undergo ſo great a corruption, and change, that 'tis not poſſible, ſo many ſcatter'd corpuſcles ſhould be again brought together, and re-united, after the ſame manner wherein they exiſted in a human body, whilſt it was yet alive. And, ſay they, much more impoſſible is it, to effect this re-union, if the body have been, as it often happens, devour'd by wild beaſts, or fiſh; ſince, in this caſe, tho' the ſcatter'd corpuſcles of the carcaſs might be recover'd, as particles of matter; yet, having already paſs'd into the ſubſtance of other animals, they are quite tranſmuted by the new form of the beaſt, or fiſh, that devour'd them; and of which they now make a ſubſtantial part.

Yet, far more impoſſible will this renewal be, if we put the caſe, that the body was devoured by *Cannibals*; for then, the ſame fleſh, belonging ſucceſſively to two different perſons, 'tis impoſſible that both ſhould have it reſtored to them at once; or, that any footſteps ſhould remain of the relation it had to the firſt poſſeſſor.

Anſwer'd.

But, in anſwer to this grand objection, I have ſeveral things to offer.

And, 1*ſt*, A human body is not like a ſtatue of braſs, or marble, that may continue, as to ſenſe, whole ages, in a permanent ſtate; but is in a perpetual flux, or changing condition; ſince it grows, in all its parts, and all its dimenſions, from a corpuſcle no bigger than an inſect, to the full ſtature of a man; which could not happen but by a conſtant appoſition, and aſſimilation of new parts, to the primitive ones, of the little embryo. And ſince men, as other animals, grow but to a certain degree, and till a certain age, and therefore muſt diſcharge a great part of what they eat and drink by inſenſible perſpiration, which *Sanctorius*'s ſtatical experiments, and mine, aſſure me to be ſcarce credibly great, as to men, and ſome other animals, both hot and cold; it will follow, that in no very great compaſs

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PHYSICS. of time, a large part of the ſubſtance of a human body, muſt be changed. Yet 'tis conſiderable, that the bones are of a ſtable and laſting texture; as I found, not only by ſome chymical trials, but by the skulls and other bones of men, whom hiſtory records to have been kill'd an exceeding long time ago.

2*dly*, There is no determinate bulk, or ſize, neceſſary to make a human body paſs for the ſame; and a very ſmall portion of matter, will ſometimes, ſerve the turn. Thus an embryo, for inſtance, in the womb, a new-born child, a man at his full ſtature, and a decrepit, aged perſon, notwithſtanding the vaſt difference of their ſizes, are ſtill reputed to be the ſame perſon; as is evident by the cuſtom of crowning kings and emperors in the mothers womb; and by putting malefactors to death in their old age, for crimes committed in their youth. And, if a very tall, and unwieldy fat man, ſhould, as it ſometimes happens, be reduced by a conſumption, almoſt to a skeleton; yet none would deny that this waſted man, were the ſame with him that had once ſo vaſt a body.

3*dly*, A body may either conſiſt of, or abound with ſuch corpuſcles, as may be variouſly aſſociated with thoſe of other bodies, and exceedingly diſguized by the mixtures, yet retain their own nature: and of this we have various inſtances in metals. Thus gold, for example, when diſſolv'd in *Aqua regia*, paſſes for a liquor; and, when dextrouſly coagulated, appears a ſalt, or vitriol; by another alteration, I have made it part of the fewel of a flame; being dextrouſly conjoin'd to another mineral, it may be reduced to glaſs; well precipitated with mercury, it makes a glorious tranſparent powder; precipitated with ſpirit of urine, or oil of tartar *per deliquium*, it makes a fulminating calx, that goes off very eaſily, yet is far ſtonger than gun-powder; precipitated with another certain alkali, the fire turns it to a fix'd and purple calx. But notwithſtanding all theſe, and various other diſguizes, the gold retains its nature, as may be prov'd by chymical operations, eſpecially by reductions. And mercury, is of a more changeable nature than gold; ſometimes putting on the form of a vapour; ſometimes appearing in that of an almoſt inſipid water; ſometimes aſſuming the form of a red powder; ſometimes that of a white, or yellow one; of a cryſtalline ſalt; of a malleable metal; and of what not? Yet, all theſe are various dreſſes of the ſame quickſilver, which a skilful artiſt may eaſily make it put off, and appear again in its native ſhape *.

And,

* If groſs bodies, and light, be mutually convertible into each other, as Sir *I. Newton* ſeems to think they are; what tranſmutations may there not be, in the compaſs of nature and art? Nature, as Sir *Iſaac* obſerves, ſeems delighted with tranſmutations: "Water (ſays he) ſhe changes into vapour; "which is a ſort of air; and, by cold, "into ice; which is a hard, pellucid, "fuſible, brittle ſtone; and this ſtone returns into water by heat; and vapour "returns into water by cold. Earth, by "heat, becomes fire; and, by cold, returns into earth. Denſe bodies, by fermentation, rarify into ſeveral ſorts of "air; and this air, by fermentation, and "ſometimes without it, returns into denſe "bodies. Mercury ſometimes appears in "the

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And, tho' it be true, that inſtances of the permanence of corpuſcles undergoing ſucceſſive diſguizes, may be much eaſier found among metals and minerals than vegetables and animals; yet there are ſome to be met with among theſe too: for, not to mention *Hippocrates*'s affirmation, about purging a child with the milk of an animal, that had taken elaterium, I once, in *Savoy*, obſerv'd all the butter, that was made in ſome places, during the ſpring ſeaſon, taſted very much of a certain weed, which, at that time, abounds in the fields there. And, conſidering how many elaborate alterations the rank corpuſcles of this weed muſt have undergone, in the various digeſtions in the cow's ſtomach, heart, udder, *&c.* and that afterwards, two ſeparations, at leaſt, were made; the one of the cream from the reſt of the milk; and the other of the unctuous parts of the cream, from the ſerum; it will ſcarce be deny'd, that vegetable corpuſcles may, by aſſociation, paſs thro' various diſguizes, without loſing their nature; eſpecially ſince the eſſential attributes of ſuch corpuſcles, may remain undeſtroy'd, tho no ſenſible quality ſurvive, to make proof of it, as is afforded by our example in the offenſive taſt. And, beſides what we commonly obſerve on the ſea-coaſt, of the fiſhy taſt of thoſe ſea-birds, that feed only upon fiſh; I purpoſely inquired of an obſerving man, who lived upon a part of the *Iriſh* coaſt, where the cuſtom is to fat their hogs with a ſort of ſhell-fiſh, wherewith that place very much abounds, about the taſt of their pork; and he aſſured me, that the fleſh had ſo ſtrong and rank a ſavour of the fiſh, that ſtrangers could not endure to eat it. There is a certain fruit in *America*, well known to our *Engliſh* planters, which many of them call the pricklepear, whoſe very red juice, being eaten with the pulp of the fruit, of which it is a part, paſſes thro' the various ſtrainers, and digeſtions of the body, ſo unalter'd, as to render the urine red enough to perſuade thoſe, who are unacquainted with this property, that they make bloody urine; as I have been ſeveral times aſſured, by unſuſpected eye-witneſſes. But, more odd is that, related by a learned man, who ſpent ſeveral years upon the *Dutch* and *Engliſh* plantations, in the *Charibbe* iſlands; when ſpeaking of

" the form of a fluid metal, and ſometimes " in the form of a hard, brittle metal; " ſometimes in the form of a corroſive, " pellucid ſalt, called ſublimate; ſometimes in the form of a taſtleſs, pellucid, volatile white earth, call'd, *Mercurius dulcis*; or, in that of a red, opake, " volatile earth, call'd, cinnabar; or, in " that of a red, or white precipitate; or, " in that of a fluid ſalt: and diſtillation " turns it into vapour; and being agitated *in vacuo*, it ſhines like fire. And, " after all theſe changes, it returns into its " firſt form of mercury. Eggs grow from " inſenſible magnitudes, and change into " animals; tadpoles into frogs; and worms " into flies. All birds, beaſts, and fiſhes, " inſects, trees, and vegetables, with their " ſeveral parts, grow out of water, and watry tinctures, and ſalts; and, by putrefaction, return again into watry ſubſtances. Water, ſtanding a few days, in " the open air, yields a tincture, which " (like that of malt) by ſtanding longer, " yields a ſediment, and a ſpirit; but, " before putrefaction, is fit nouriſhment " for animals and vegetables. And, among ſuch various and ſtrange tranſmutations, why may not nature change " bodies into light, and light into bodies?" *Newton. Optic.* p. 349, 350.

PHYSICS. a fruit called *Janipa*, or *Junipa*, growing in ſeveral of thoſe iſlands, he ſays, that, at the ſeaſon when this fruit falls from the tree, the hogs, which feed on it, have both their fleſh, and fat, of a violet colour, as experience witneſſeth; which colour is the ſame that the juice dyes. And the like happens to the fleſh of parrots, and other birds, that feed upon it.

Having thus ſhewn, that the particles of a body, may retain their nature, under various diſguizes, I proceed to add, that they may be ſtripp'd of thoſe diſguizes; or, to ſpeak without a metaphor, be extricated from thoſe compoſitions wherein they are diſguized; and, that, ſometimes, by ſuch ways, as thoſe who are ſtrangers to the nicer operations of nature, would never have thought on; nor will not, perhaps, judge probable, when propoſed.

Tho' vitrification be look'd upon, by chymiſts, as the ultimate action of the fire, and the moſt powerful way of making inſeparable conjunctions of bodies; yet, even out of glaſs of lead, for inſtance, made of ſand, and the aſhes of a metal; (tho' the tranſmutation ſeems ſo great, that the dark and flexible metal, is turn'd into a very tranſparent, and brittle maſs) we have recover'd opake, and malleable lead. And, tho' there be ſeveral ways, beſides precipitations, of divorcing ſubſtances, that ſeem ſtrictly, if not inſeparably, united; yet, by precipitation alone, if a man have the skill to chuſe proper precipitants, ſeveral ſeparations may be eaſily and thoroughly made, that every one would not think of: for, 'tis not neceſſary, that, in all precipitations, as is obſerv'd in moſt of the vulgar ones, the precipitant ſhould, indeed, make a ſeparation of the diſſolv'd body, from the maſs or bulk of that liquor, or other adjunct, whereto 'twas before united, and not be able to perform this, without aſſociating its own corpuſcles with thoſe of the body it ſhould reſcue, and ſo making, in ſome ſenſe, a new and farther compoſition. That ſome bodies may precipitate others, without uniting themſelves with them, is eaſily prov'd by the experiment of refiners in ſeparating ſilver from copper; for the mixture being diſſolv'd in *Aqua fortis*, if the ſolution be afterwards diluted, by adding fifteen or twenty times as much common water, and you put into this liquor a copper-plate, you ſhall quickly ſee the ſilver begin to adhere to the plate; not in the form of calx, as when gold is precipitated to make *Aurum fulminans*, or tin-glaſs, to make a fine white powder for a fucus; but in the form of a ſhining metalline ſubſtance, that needs no farther reduction, to be employ'd as good ſilver. And, by a proper precipitant, I have alſo, in a trice, reduced a large quantity of well-diſguized mercury, into running quick-ſilver. And, if one can well appropriate the precipitants to the bodies they are to recover, very ſlight, and unpromiſing agents, may perform great matters in a ſhort time.

If you let a piece of camphire lie a while upon oil of vitriol, ſhaking them now and then, it will be ſo corroded by the oil, as totally to diſappear therein, without retaining ſo much as its ſmell, or without any manifeſt ſign of there being camphire in that mixture; yet that a vegetable ſubſtance,

ſubſtance, thus ſwallow'd up and changed by one of the moſt fretting and deſtroying ſubſtances known in the world, ſhould not only retain the eſſential qualities of its nature, but be reſtorable to its obvious and ſenſible ones in a minute, and that by ſo unpromiſing a medium, as common water, will appear by pouring the ſolution into a large proportion of that fluid, to whoſe upper part there will immediately riſe white, brittle, ſtrong-ſcented, and inflammable camphire, as it was before. PHYSICS.

'Tis here a principal conſideration, that all bodies being but parcels of the univerſal matter mechanically different, they may ſucceſſively put on forms, in a way of circulation, till they return to their original form, whence they firſt begun; by having only their mechanical properties alter'd.

That all bodies agree in one common matter, the ſchools themſelves allow; making what they call the *Materia prima* to be the common baſis of them all; and their ſpecific differences to ſpring from their particular forms: and ſince the true notion of body conſiſts either alone in its extenſion, or in that and impenetrability together, it will follow, that the differences which make the varieties of bodies we ſee, muſt not proceed from the nature of mere matter, of which we have but one uniform conception; but from certain attributes, ſuch as motion, ſize, poſition, *&c.* that we call mechanical affections. Hence a determinate proportion of matter being given, if we ſuppoſe that an intelligent and otherwiſe duly qualify'd agent were to watch this portion of matter in its whole progreſs thro' the various forms it is made to put on, till it come to the end of its courſe or ſeries of changes; and that this intelligent agent ſhould lay hold of this portion of matter, cloth'd in its ultimate form; and, extricating it from any other parcels of matter wherewith it may be mix'd, make it exchange its laſt mechanical properties for thoſe which it had when this agent firſt began to watch it: in ſuch a caſe, I ſay, this portion of matter, how many changes and diſguizes ſoever it may have undergone in the mean time, will return to be what it was; and if it were before part of another body to be reproduced, it will become capable of having the ſame relation to it, that formerly it had.

Thus, ſuppoſe a man to cut a large ſphere of ſoft wax into two equal parts, and of the one to make cones, cylinders, rings, ſcrews, *&c.* and kneading the other with paſte, make an appearance of cakes, vermicelli, wafers, biskets, *&c.* 'tis plain, that one may, by diſſolution, and other ways, ſeparate the wax from the paſte, and reduce it in a mould to the ſame hemiſphere of wax it was before; and ſo we may deſtroy all that made the other part of the wax paſs for cones, cylinders, rings, *&c.* and reduce it in a mould to one diſtinct hemiſphere, fit to be re-conjoin'd to the other; and ſo to recompoſe ſuch a ſphere of wax, as they conſtituted before the ſeparation was made. And to view precipitate, carefully prepared *per ſe*, one would think that art had here made a body extremely different from common mercury; this being conſiſtent, like a powder, very red in colour, and purgative, and for the moſt part vomitive in operation, tho' given but in the quantity of four or five grains: yet if you but urge this powder with a due heat; by putting the component particles into a new and fit motion, you may re-unite them together, so as to re-produce the same running mercury you had before the precipitate *per se* was made of it.

4thly, But the christian doctrine doth not ascribe the resurrection to nature, or any created agent, but to the peculiar and immediate operation of God; who has declared, that he will raise the dead. Wherefore, when I mention chymical ways of recovering bodies from their various disguizes, I am far from desiring, that such ways should be thought the only ones, or the best that can possibly be employ'd to such an end. For, as the generality of men, without excepting philosophers themselves, would not have believed or thought, that by easy chymical ways, bodies, which are reputed to have pass'd into quite another nature, should be reduced or restored to their former condition; so, till chymistry, and other parts of natural philosophy, be more thoroughly understood, and farther promoted, 'tis probable, that we can scarce now imagine, what expedients to re-produce bodies, a further discovery of the mysteries of art and nature may lead us to. And much less can our slender knowledge determine, what means, even of physical ones, the most wise author of nature is able to employ, to bring the resurrection to pass; since 'tis a part of the imperfection of inferior natures, to have but an imperfect apprehension of the powers of one that is incomparably superior to them. And even among ourselves, a child, who is endow'd with a reasonable soul, cannot conceive how a geometrician should measure inaccessible heights and distances; much less, how a cosmographer can determine the whole compass of the earth and sea; or an astronomer shew how far 'tis from hence to the moon; and tell many years before-hand, what day and hour, and to what degree, she will be eclipsed. And, indeed, in the *Indies*, not only children, but rational men, could not perceive how 'twas possible for the *Europeans* to converse with one another, by the help of a piece of paper, at a hundred miles distance; and in a moment produce thunder and lightning, and kill men a great way off, as they saw done by guns; and much less, how they should foretel an eclipse of the moon, as *Columbus* did, to his great advantage: which things made the *Indians*, even the chiefest of them, look upon the *Spaniards* as persons of more than human nature. Now, among those who have a true notion of a Deity, a Being both omnipotent and omniscient, that he can do all, and more than all, that is possible to be perform'd by any way of disposing of matter and motion, is a truth that will be readily acknowledg'd; since he was able at first to produce the world, and contrive some part of the universal matter of it, into the bodies of the first man and woman. And that his power extends to the re-union of a soul and body, which have been separated by death, we may learn from the experiments God has been pleased to give of it, both in the old testament and the new; especially in the raising *Lazarus* and *Christ* again to life: of the latter of which, particularly, we have proofs strong enough to satisfy any unprejudiced person, who desires but competent arguments to convince him.

Since then a human body is not so confined to a determinate bulk, but that the same soul being united to a portion of duly-organiz'd matter, is said to constitute the same man, notwithstanding the vast differences of big-

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neſs that there may be, at ſeveral times, between the portions of matter whereto the human ſoul is united: ſince a conſiderable part of the human body conſiſts of bones, which are bodies of a very determinate nature, and not apt to be deſtroy'd by the operation either of earth or fire: ſince of the leſs ſtable, and eſpecially the fluid parts of a human body, there is a far greater expence made, by inſenſible tranſpiration, than even philoſophers would imagine: ſince the ſmall particles of a reſolved body may retain their own nature, under various alterations and diſguizes, of which 'tis poſſible they may be afterwards ſtript: ſince, without making a human body ceaſe to be the ſame, it may be repair'd and augmented by the adaptation of fitly-diſpoſed matter to that which pre-exiſted in it: ſince theſe things are ſo, why ſhould it be impoſſible, that a moſt intelligent agent, whoſe omnipotence extends to all that is not truly contradictory to the nature of things, or to his own, ſhould be able ſo to order and watch the particles of a human body, that of thoſe remaining in the bones, of thoſe that plentifully fly away by inſenſible tranſpiration, and of thoſe that are otherwiſe diſpoſed of upon their reſolution, a competent number may be preſerved or retrieved; ſo that ſtripping them of their diſguizes, or extricating them from other parts of matter, to which they may happen to be conjoin'd, he may re-unite them betwixt themſelves, and, if need be, with particles of matter fit to be intervoven with them; and thereby reſtore or reproduce a body, which being united with the former ſoul, may, in a ſenſe agreeable to the expreſſions of ſcripture, re-compoſe the ſame man, whoſe ſoul and body were formerly disjoin'd by death.

5*thly*, Hitherto we have taken the doctrine of the reſurrection in a more ſtrict and literal ſenſe, becauſe I would ſhew, that even according to that, the difficulties of anſwering what is mention'd againſt the poſſibility of it, are not inſuperable; tho' it would much facilitate the defence and explanation of ſo abſtruſe a thing, to allow, that as the human ſoul is the form of man, ſo that whatever duly-organiz'd portion of matter is thereto united, it therewith conſtitutes the ſame man; the import of the reſurrection is fulfill'd in this, that after death there ſhall be another ſtate, wherein the ſoul ſhall no longer perſevere in its ſeparate condition, but be again united, not to an ethereal, or the like fluid matter, but to ſuch a ſubſtance, as may, with tolerable propriety of ſpeech, be call'd a human body.

They who aſſent to this account of the poſſibility of the reſurrection of the ſame bodies, will, I preſume, be much more eaſily induced to admit the poſſibility of the qualifications the chriſtian religion aſcribes to the glorified bodies of the raiſed ſaints. For, ſuppoſing the truth of the ſcripture, we may obſerve, that the power of God has already extended itſelf to the performance of ſuch things, as import as much as we need infer; ſometimes by ſuſpending the natural actions of bodies upon one another, and ſometimes by endowing human and other bodies with preternatural qualities. And indeed lightneſs, or rather agility, indifference to gravity and levity, incorruption, tranſparency, opacity, figure, colour, *&c.* being but mechanical affections of matter; it cannot ſeem incredible, that the moſt free and powerful

PHYSICS. powerful author of thoſe laws of nature, according to which, all the phenomena of qualities are regulated, may, as he thinks fit, introduce, eſtabliſh, or change them, in any aſſign'd portion of matter, and conſequently in that whereof a human body conſiſts. Thus tho' iron be a body above eight times ſpecifically heavier than water; yet in the caſe of *Eliſha*'s helve, its native gravity was render'd ineffectual, and it aſcended from the bottom to the top of the water: and the gravitation of St. *Peter*'s body was ſuſpended, whilſt his maſter enabled him to walk on the ſea. Thus the operation of the moſt active body in nature, flame, was ſuſpended in *Nebuchadnezar*'s fiery furnace, whilſt *Daniel*'s three companions walk'd unhurt therein. And thus the body of our Saviour, after his reſurrection, tho' it retain'd the very impreſſions that the nails of the croſs had made in his hands and feet, and the wound of the ſpear in his ſide, as the hiſtory of the goſpel aſſures us; was endow'd with far nobler qualities, than before its death. And, as the apoſtle tells us, that this great change of ſchematiſm in the ſaints bodies will be effected by the irreſiſtible power of Chriſt; we ſhall not ſcruple to allow of ſuch an effect from ſuch an agent, if we conſider how much the bare ſlight mechanical alteration of the texture of a body, may change its ſenſible qualities for the better. Thus, without any viſible addition, I have ſeveral times changed dark and opake lead, into finely-colour'd tranſparent glaſs, of a leſs ſpecific gravity; and thick fetid ſmoke, into a bright and ſcentleſs flame.

THE

Christian Virtuoso.

THE Proposition I shall here endeavour to establish, is, that a man may be a virtuoso, or experimental philosopher, without forfeiting his christianity. *Experimental philosophy leads to religion, in general,*

And first, 'tis certain, that a due course of experimental philosophy, greatly conduces to settle in the mind, a firm persuasion of the existence, and several of the chief attributes of God; which persuasion is, in the order of things, the first principle of that natural religion, we allow as the very foundation of reveal'd religion, in general.

That the consideration of the vastness, beauty, and regular motions of the heavenly bodies, the excellent structure of animals and plants, besides a multitude of other phenomena of nature, and the subserviency of most of these to man, may justly induce him, as a rational creature, to conclude, this vast, beautiful, orderly, and admirable system of things, which we call the world, was framed by an author supremely powerful, wise, and good, can scarce be deny'd by an intelligent and unprejudiced person. And this is strongly confirm'd by experience, which witnesseth, that in almost all ages and countries, the generality of philosophers, and contemplative men, were persuaded of the existence of a Deity, from the consideration of the phenomena of the universe; whose fabric, and conduct, they rationally concluded, could not be justly ascribed, either to chance, or to any other cause than a divine Being. *By discovering the existence of God.*

But, tho' God hath manifested himself, even to such as consider things but superficially, by stamping upon several of the more obvious parts of his works, such conspicuous impressions of his attributes, that a moderate degree of understanding, and attention, may suffice to make men acknowledge his being; yet this assent is greatly inferior to the belief which the same objects are fitted to produce in him, who with care and skill considers them. For, the works of God are so worthy of their author, that, besides the impresses of his wisdom and goodness, left, as it were, upon their surfaces; there are a great many more curious and excellent tokens, and effects of divine artifice, in the hidden and innermost recesses of them: and these are not to be discover'd by the slight glances of the

lazy,

PHYSICS. lazy, and the ignorant; but require, the most attentive and prying inspection of curious, and well qualify'd minds. And, sometimes, in one creature there may be great numbers of admirable things, that escape a vulgar eye, which yet are clearly discernible by that of a true naturalist; who brings with him, besides a more than common curiosity, and attention, a competent knowledge of anatomy, optics, cosmography, mechanics, and chymistry. In short, God has couch'd so many things in his visible works, that the clearer light a man uses, the more he may discover of their unobvious beauty, and exactness; and the more fully, and distinctly discern those qualities that lie more concealed. And the more wonderful things he discovers in the works of nature, the more corroborating proofs he meets with to establish, and enforce the argument drawn from the universe, and its parts, to demonstrate, that there is a God: a proposition of so vast a weight, and importance, that it ought to endear every thing to us, that is able to confirm it, and afford us new motives to acknowledge, and adore, the divine author of things.

Now, with regard to this matter, we may confidently say, that experimental philosophy has a great advantage of the scholastic. For, in the *Peripatetic* schools, where things are ascribed to certain substantial forms, and real qualities; the accounts of nature's works may be easily given in a few words, general enough to be applicable on almost all occasions. But these uninstructive terms neither oblige, nor conduct a man to deeper searches into the structure of things, their manner of being produced, and of operating upon one another; and, consequently, are very insufficient to disclose the exquisite wisdom which the omniscient maker has expressed in the peculiar fabrics of bodies, and the well regulated motions of them, or of their constituent parts. From the discernment of which things, nevertheless, it is, that there arises, by way of result, in the philosophic mind of an intelligent contemplator, a strong conviction of the being of a divine artificer, and a just acknowledgment of his admirable wisdom. To be told, that an eye is the organ of sight, and that this is perform'd by that faculty of the mind, which, from its function, is call'd visive, will give a man but a sorry account of the instrument, and manner of vision itself, or of the knowledge of the artificer who form'd the eye. And he who can take up with this easy theory of vision, will not think it necessary to bestow pains to dissect the eyes of animals, or study the writings of mathematicians, to understand the doctrine of it; and, accordingly, will have but mean thoughts of the contrivance of the organ, and the skill of the artificer, in comparison of the ideas that will be suggested of both to him, who, being thoroughly skill'd in anatomy, and optics, by their help takes asunder the several coats, humours, and muscles, of which that exquisite dioptrical instrument consists: and, having separately consider'd the figure, size, consistence, texture, transparency, or opacity, situation, and connexion of each of them, and how they are all adjusted in the whole eye, shall discover, by the laws of optics, how admirably this little organ is fitted to receive the incident rays of light, and dis-

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dispose them in the best manner possible, to give a lively representation of the numberless objects of sight.

'Tis easy to say, in general terms, that the world is wisely framed; but, I doubt, it often happens, that men confess the creatures are wisely made, rather because they, upon other grounds, believe God to be a wise agent, than because so slight an account as the school-philosophy gives of particular creatures, convinces them of any divine wisdom in the creator. And tho' I am willing to grant, that some impressions of God's wisdom are so conspicuous, that even, as was said before, a superficial philosopher may thence infer, that the author of such works must be a wise agent; yet, how wise an agent he has in those works expressed himself to be, none but an experimental philosopher can well discern. And 'tis not by a slight survey, but by a diligent, and skilful scrutiny, of the works of God, that a man must be, by a rational, and affective conviction, engaged to acknowledge, that the author of nature "is wonderful in counsel, and "excellent in working."

The immortality of the soul.

2. After the existence of the Deity, the next grand principle of natural religion is, the immortality of the rational soul; the genuine consequence whereof is, the belief, and expectation, of a future, and everlasting state. For this important truth, many arguments may be alledged, to persuade a sober, and well-disposed man to embrace it: but, to convince a learned adversary, the strongest argument that the light of nature supplies us with, seems to be that afforded by real philosophy. For this teacheth us to form true, and distinct notions of the body, and the mind; and thereby manifests so great a difference in their essential attributes, that shews the same thing cannot be both. This it makes out more distinctly, by enumerating several faculties, and functions, of the rational soul; such as, to understand, and that so as to form conceptions of abstract things, of universals, of immaterial spirits, and even of that infinitely perfect one, God himself: and also, to conceive, and demonstrate, that there are incommensurable lines, and surd numbers; to form chains of reasoning, and draw both cogent, and concatenated inferences about these things; to express intellectual notions, *pro re natâ*, by words, or instituted signs, to other men; to exercise free-will about many things; and to make reflections on its own acts, both of understanding, and will. For these, and the like prerogatives, that are peculiar to the human mind, and superior to any thing that belongs to the outward senses, or to the imagination itself, manifest, that the rational soul is a Being of an higher order than corporeal; and, consequently, that the seat of these spiritual faculties, and the source of these operations, is a substance, that, being in its own nature distinct from the body, is not naturally subject to perish with it.

And, with regard to this truth, our virtuoso hath an advantage of a mere school-philosopher. For, being acquainted with the true and real causes of putrefaction, and other physical kinds of corruption; and thereby discerning, that the things which destroy bodies, are the avolation, or other recess, of some necessary parts, and such a depraving transposition

PHYSICS. of the component portions of matter, as is altogether unſuitable to the ſtructure, and mechanical modification, eſſential to a body of that ſpecies, or kind, whereto it belongs; he plainly perceives, that theſe cauſes of deſtruction can have no place in the rational ſoul; which being an immaterial ſpirit, and, conſequently, a ſubſtance not really diviſible, can have no parts expell'd, or tranſpoſed; and ſo being exempted from the phyſical cauſes of corruption, that deſtroy bodies, it ought to laſt always. And, being a rational creature, endow'd with internal principles of acting, as appears in free-will, it ought to live for ever, unleſs it pleaſe God to annihilate it; which we have no reaſon to ſuppoſe he will do. On the other hand, the modern *Peripatetics* maintain ſubſtantial forms, by ſome of them, ſtiled *Semi-ſubſtantiæ*; to which, in apes, elephants, and others, that paſs for ingenious animals, they aſcribe ſome ſuch faculties, and functions, as ſeem to differ only in degree from thoſe of the rational ſoul; but, how innocent ſoever their intentions may be, their doctrine tends greatly to weaken the chief phyſical way of proof, from whence the immortality of man's mind is juſtly inferr'd. For ſince, according to the *Peripatetics*, ſubſtantial forms are, as they ſpeak, educed out of the power, or potentiality, of the matter; and ſo depend upon it, not only as to action, but as to being, that they cannot at all ſubſiſt without it; but when the particular body, as an herb, a ſtone, or a bird, is deſtroy'd, they periſh with it; I think they give great advantage to atheiſts, and cavillers, to oppoſe the immortality of the mind.

For if to an ape, or other brute animal, there belongs a Being more noble than matter, that can actuate, and inform it, and make itſelf the architect of its own manſion, tho' ſo admirable as that of an ape, or an elephant; if this Being can, in the body it hath framed, perform all the functions of a vegetable ſoul; and, beſides thoſe, ſee, hear, taſt, ſmell, imagine, infer, remember, love, hate, fear, hope, expect, *&c.* and yet be a mortal thing, and periſh with the body; 'twill not be difficult for thoſe enemies of religion, who are willing to think the ſoul mortal, to fancy, that human minds are only a more noble, but not leſs mortal kind of ſubſtantial forms: as, amongſt ſenſitive ſouls themſelves, which they acknowledge to be equally mortal, there is a great diſparity in degree; that of a monkey, for inſtance, being very far ſuperior to that of an oyſter.

And the belief of a divine providence. 3. A third chief principle of natural religion, and, conſequently, of reveal'd religion, which ſuppoſes the former as its foundation, is a belief of the divine providence. And, in this grand article, as well as in the two foregoing, a man may be much confirm'd by experimental philoſophy; both as it affords him poſitive inducements to acknowledge the article, and as it ſhews the great improbability of the two principal grounds, on one or other of which is founded the denial of God's providence.

A virtuoſo, who, by manifold and curious experiments, ſearches deep into the nature of things, has great, and peculiar advantages, to diſcover, and obſerve, the excellent fabric of the world, as 'tis an immenſe aggregate of the ſeveral creatures that compoſe it; and to take notice in its

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particular parts, especially those that are animated, of such exquisite contrivances, and such admirable co-ordinations, and subordinations, in reference to each other, as lie hid from those who are not both attentive, and skilful. When our virtuoso contemplates the vastness, scarce conceivable swiftness, and yet constant regularity, of the various motions of the sun, moon, and other celestial lights; when he considers how the magnetism of the earth makes its poles constantly point the same way, notwithstanding its various motions; how, by daily turning about its own centre in four and twenty hours, it receives as much light, and benefit from the sun, and all the glorious constellations of the firmament, as if they, with all the vast heavenly region they belong to, mov'd about it in the same time; how, by its situation among them, it enjoys the regular vicissitudes of day and night, summer and winter, *&c.* how the several parts of the sublunary world are mutually subservient to one another, and most of them serviceable to man; how excellently the bodies of animals are contrived; what various, and suitable provision is made for different animals; how admirable, and astonishing a process is gone through in the formation of the fœtus; how various animals are endow'd with strange instincts, whose effects, sometimes, seem much to surpass those of reason itself: when, I say, a philosopher duly reflects on these things, and many others of the like import, he will think it highly rational to infer from them these three conclusions.

First, that a machine immense, beautiful, well contrived, in a word, so admirable as the world, cannot have been the effect of mere chance, or the tumultuous justlings, and fortuitous concourse of atoms; but must have been produced by a cause, exceedingly powerful, wise and beneficent.

Secondly, that this most powerful author, and contriver of the world, hath not abandon'd a work so worthy of him, but still maintains, and preserves it; so regulating the stupendously swift motions of the great globes, and other vast masses of the mundane matter, that they do not, by any great irregularity, disorder the grand system of the universe, and reduce it to a chaos, or confused state of things.

Thirdly, that as it is not above the ability of the divine author, though a single Being, to preserve, and govern all his visible works, how great and numerous soever; so he thinks it not below his dignity, and majesty, to extend his care, and beneficence to particular bodies, and even to the meanest creatures; providing, not only for the nourishment, but for the propagation of spiders, and ants themselves. And, indeed, since the truth of this assertion, that God governs the world he has made, would appear by the constancy, and regularity, and astonishingly rapid motions of the vast celestial bodies, and by the long trains of as admirable artifices employ'd in the propagation of various sorts of animals; I see not why it should be deny'd, that God's providence may reach to his particular works here below, especially to the noblest of them, man; since most of those who deny this, as derogatory to God's majesty, and happiness, acknow-

PHYSICS. ledge, that, at the first formation of things, the great author of them must not only have extended his care to the grand system of the universe in general; but allow'd it to descend so low, as to contrive all the minute, and various parts, not only of greater, and more perfect animals, as elephants, whales, and men; but such small, and abject ones, as flies, ants, mites, *&c.* which being manifestly propagated by eggs, laid by the female, cannot reasonably be thought the offspring of putrefaction. Whence I gather, as from matter of fact, that to be concern'd for the welfare, even of particular animals, as it is agreeable to God's wisdom, and exuberant beneficence; so it is not truly derogatory to his adorable greatness, and majesty.

And, since man is the noblest of God's visible works; since very many of them seem made for his use; since, even as an animal, he is wonderfully made, and curiously, or artificially wrought; and since God has both given him a rational mind, and endow'd it with an intellect, whereby he can contemplate the works of nature, and by them acquire a conviction of the existence, and several attributes, of their supremely perfect author; and, lastly, since God hath planted notions, and principles in the mind of man, fit to make him sensible, that he ought to adore his maker, as the most perfect of Beings, the supreme Lord, and governor of the world; natural reason dictates to him, that he ought to express the sentiments he has for this divine Being, by a veneration of his excellencies; by gratitude for his benefits; by humiliation, in view of his greatness, and majesty; by an awe of his justice; by reliance on his power, and goodness; and, in short, by those several acts of natural religion, that reason shews to be suitable, and, therefore, due to those several divine attributes of his, which it has led us to acknowledge.

And here I shall add, that, from the *Cartesian* principles, a double argument may be drawn for divine providence.

For, first, according to the *Cartesians*, local motion, which is the grand principle of all action among things corporeal, is adventitious to matter; and was, originally, produced in it, and is still every moment continu'd, and preserv'd, immediately by God: whence it may be inferr'd, that he concurs to the actions of each particular physical agent; and, consequently, that his providence reaches to all, and every one of them.

And, secondly, the same *Cartesians* believe the rational soul to be an immaterial substance, really distinct, and separable from the body. Whence I infer, that the divine providence extends to every particular man; since, whenever an embryo, or little human body form'd in the womb, is, by being duly organized, fitted to receive a rational mind, God is pleas'd to create one, and unite it to that body. In which transaction, there seems to me a necessity of a direct, and particular intervention of the divine power; since I understand not, by what physical charm, or spell, an immaterial substance can be allured into this or that particular embryo, of many that are, at the same time, fitted to receive a human soul; nor by what merely mechanical ties, or bond, an immaterial substance can be durably join'd, and united, with a corporeal one, in which it finds no parts, that it has organs to take hold of, and to which it can furniſh no parts to be faſten'd upon by them. No better can I conceive, how a mere body can produce pain, pleaſure, *&c.* by its own mere action, or endeavour to act on an immaterial ſpirit. No will the force of all this reaſoning be eluded, by ſaying, with ſome deiſts, that, after the firſt formation of the univerſe, all things are brought to paſs by the ſettled laws of nature. For tho' this be confidently, and, not without colour, pretended; yet, I confeſs, it does not ſatisfy me. For, not to mention the inſuperable difficulty there is, to give an account of the firſt formation of things, which many deiſts will not aſcribe to God; nor that the laws of motion, without which the preſent ſtate, and courſe of the world, could never be maintain'd, did not neceſſarily ſpring from the nature of matter, but depended upon the will of the divine author of things; I look upon a law, as a moral, not a phyſical cauſe; as being, indeed, but a notional thing, according to which, an intelligent and free agent is bound to regulate its actions. But inanimate bodies are utterly incapable of underſtanding what a law is, or what it enjoins, or when they act conformably, or unconformably to it; and, therefore, the actions of inanimate bodies, which cannot incite, or moderate their own actions, are produced by real power, not by laws; tho' the agents, if intelligent, may regulate the exertions of their power by ſettled rules.

4. I have taken notice of two other accounts, upon which the experimental knowledge of God's works may, in a well-diſpoſed mind, conduce to eſtabliſh the belief of his providence.

And, firſt, when our virtuoſo ſees with how many, and how various, and how admirable ſtructures, inſtincts, and other contrivances, the wiſe artificer hath furniſhed even brutes, and plants to acquire, and aſſimilate their food; to defend, or otherwiſe ſecure themſelves from hoſtile things; to maintain their lives, and propagate their ſpecies; it will very much conduce to perſuade him, that ſo wiſe an agent, who has at command ſo many differing, and excellent methods, and inſtruments, to accompliſh what he deſigns; and, often, actually employs them for the preſervation, and welfare of beaſts, and even of plants, can never want means to compaſs his moſt wiſe, and juſt ends, with relation to mankind; being able, by ways that we ſhould never dream of, to execute his purpoſes, and fulfil his promiſes.

Secondly, when we duly conſider the very different ends to which many of God's particular works, eſpecially thoſe that are animated, ſeem deſign'd, with reſpect both to their own welfare, and the utility of man; and how exquiſitely the great creator has been pleaſed to ſupply them with means admirably fitted to attain theſe reſpective ends; we cannot but think it highly probable, that ſo wiſe, and ſo bountiful a Being, has never left his nobleſt viſible creature, man, unfurniſhed with means to procure his own welfare, and obtain his true end, if he be not wanting to himſelf. And, ſince man is endow'd with reaſon, which may convince

PHYSICS. him, what neither a plant, nor a brute animal is capable of knowing, that God is both his maker and continual benefactor; since his reason likewise teacheth him, that upon both those accounts, besides others, God may justly expect and require worship and obedience from him; since also the same rational faculty may persuade him, that it well becomes the majesty and wisdom of God, as the sovereign governour of the world, to give a law to man, who is a rational creature, capable of understanding and obeying it, and thereby glorifying the author of it; since, finding in his own mind a principle, which tells him, he owes a veneration, and other suitable sentiments, to the divine author of his being, and his continual and munificent benefactor; since, on these accounts, his conscience will convince him of his obligation to all the essential duties of natural religion; and since, lastly, his reason may assure him, that his soul is immortal, and is therefore capable and desirous of being everlastingly happy, after it has left the body; he must in reason be strongly inclined to wish for a supernatural discovery of what God would have him believe and do. And therefore, if, being thus prepared, he shall be very credibly inform'd, that God hath actually been pleased to discover, by supernatural means, what kind of worship and obedience, which by reason alone he could but guess at, will be most acceptable to him; and to encourage man to both these, by express promises of that felicity, which man, without them, can but faintly hope for; he would be ready, then, thankfully to acknowledge, that this way of proceeding becomes the transcendent goodness of God, without derogating from his majesty and wisdom. And by these and the like reflections, a philosopher, who takes notice of the wonderful providence, that God descends to exercise for the welfare of inferior and irrational creatures, will have an advantage above men not vers'd in the works and course of nature, in believing, upon the historical and other proofs which christianity offers, that God has actually vouchsafed to man, his noblest, and only rational visible creature, an explicite and positive law, enforced by severe penalties denounced against the stubborn transgressors, and promising to the sincere observers of it, rewards suitable to his own greatness and goodness. And thus the consideration of God's providence, in the conduct of things corporeal, may convey a well-disposed mind from natural to reveal'd religion.

Experimental philosophy draws the mind from sensual things.

5. Another thing, that disposes an experimental philosopher to embrace religion, is, that his genius and course of studies accustom him to value and delight in abstract truths; by which term, I here mean such truths, as do not at all, or very little, gratify mens ambition, sensuality, or other inferior passions and appetites. For, as the generality of those who have an aversion to religion, are led to it by a contempt of all truths, that do not gratify their passions or interests; so, he who is addicted to experimental knowledge, is accustom'd both to pursue, esteem, and relish many truths, that do not delight his senses, or gratify his passions, or interests; but only entertain his understanding with that manly and rational satisfaction, which is naturally afforded by the attainment of clear and noble truths,

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its genuine objects and delight. And tho' the discoveries made by the help of physical or mechanical experiments, are not, for the most part, allied to revealed religion; yet, besides that some of them manifestly conduce to establish or illustrate natural religion, we may argue *à fortiori*, that he who is accustomed to prize truths of an inferior kind, because they are truths, will be much more disposed to value divine truths, which are of a vastly higher and nobler order, and of an inestimable and eternal advantage. PHYSICS.

6. But farther, both the temper of mind, that qualifies a man for a virtuoso, and the way of philosophizing he chiefly employs, greatly conduce to give him a sufficient, well-grounded, and duly-limited docility; which is a great disposition to the entertainment of reveal'd religion. In the vulgar and superficial philosophy, wherein a man is allow'd to think that he has perform'd his part well, when he has ascribed things to a substantial form, or to nature, or to some real quality, whether manifest or occult, without proving that there are such causes, or intelligibly declaring, how they produce the phenomena, or effects refer'd to them; 'tis easy for a man to have a great opinion of his own knowledge, and be puff'd up by it. But a virtuoso, who cannot satisfy himself, nor dares pretend to satisfy others, till he can, by hypotheses that may be understood and prov'd, declare intelligibly the manner of the operation of the causes he assigns, will often find it so difficult a task to effect all this, that he must easily discern, he needs further information, and therefore ought to seek for it where 'tis the most likely to be had. Besides, the litigious philosophy of the schools seldom furnishes its disciples with better than dialectical or probable arguments, which are not proper fully to satisfy the person who employs them, or to leave his adversary without an answer, as probable as the objection: upon which account, men who have more wit than a sincere love for truth, will be able to dispute speciously enough, as long as they please. And as such slippery arguments are not able to convince even the person who uses them, if he be a man of judgment; so, if he deals with a witty adversary, they will leave him able to elude any arguments of the like nature, with which he shall be press'd. And in effect, we see, that in the *Aristotelian* philosophy there are several establish'd questions; such as, Whether the elements retain their distinct natures in a mix'd body? Whether the celestial orbs are moved by intelligences? *&c.* which have been disputed from age to age, and are like to continue questions for many more, if that philosophy shall last so long. But a virtuoso, who in his reasonings attends to the principles of mathematics, and sound philosophy, and to the clear testimonies of sense, or well verify'd experiments, acquires a habit of discerning the cogency of an argument, or way of probation; and easily perceives, that dialectical subtilties, and school-tricks, cannot shift off its force; but he finds more satisfaction in embracing a demonstrated truth, than in the vain glory of subtilly disputing against it. *Gives it a docility.*

7. Another thing that may, by means of experiments, dispose a studious searcher of truth for reveal'd religion, is, that his inquiries, and course of studies, make him both willing and fit to search out and discover *And a fitness for searching into deep truths.*

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PHYSICS. deep and unobvious truths. I have, with trouble, obferv'd, that the greater part of the libertines among us, being men of *Pilate*'s humour, (who, when he had fcornfully ask'd, what is truth ? would not ftay for an anfwer) with great difdain, decline the ftudy of all truths that require a ferious and fettled application of mind. Thefe men are, for the moft part, a fort of fuperficial and defultory wits, who go no further than the outfide of things, without penetrating into the receffes of them ; and, being eafily tir'd with contemplating one, pafs quickly to another; the confideration whereof, they, with the fame lightnefs, forfake. And, upon this account, among others, it is, that fuch men, though often much applauded by others, becaufe the moft are but fuperficial, as well as they, almoft as feldom make good philofophers, as good chriftians. For tho' all the found arguments, that may be brought to evince the truth of natural and reveal'd religion, be not abftrufe ; yet fome of the principal, efpecially thofe that prove the exiftence and fpecial providence of God, and the immortality of the foul, are, if not of a metaphyfical, yet, at leaft, of a philofophical nature ; and will fcarce be clearly underftood, and duly relifh'd, but by a perfon capable of, and accuftom'd to attentive and prolong'd fpeculations. Now, a man addicted to profecute difcoveries of truths, not only by ferious meditation, but by intricate and laborious experiments, will not eafily be deterr'd from effectually purfuing his end, by the toils or difficulties that attend the clearing of thofe notions, and matters of fact, whereon folid arguments for natural, or reveal'd religion, are founded ; how remote foever thofe truths may be from vulgar apprehenfions. In fhort, a fuperficial wit, fuch as is frequently found in libertines, and often helps to make them fuch, may be compared to an ordinary fwimmer, who can reach but fuch things as float upon the water; whilft an experimental philofopher, like a skilful diver, not only obtains thofe things that lie upon the furface of the fea, but makes his way to the very bottom of it; and thence fetches up pearls, corals, and other precious things, that, in fuch depths, lie conceal'd from other men's fight and reach.

Experimental philofophy leads to the chriftian religion, in particular.

We have feen, then, that experimental philofophy is, in its own nature, favourable to religion, in general: it alfo greatly conduces to fhew the truth of the chriftian religion, in particular.

This excellent religion is recommended to minds rightly difpofed, by a great number of prerogatives, whereof I fhall mention a few.

1. And firft, the three grand arguments, that jointly evince the truth of the chriftian religion in general, are, in my opinion, the excellency of the doctrine, which makes it worthy to have proceeded from God ; the teftimony of the divine miracles, wrought to recommend it ; and the great effects, produced in the world by it. Two of thefe three arguments are bottom'd upon matters of fact, and, confequently, are likely to be the moft prevalent upon thofe who have a great veneration for experience, and are duly difpofed to frame fuch pious reflections, as it warrants and leads to.

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Now, an experimental philosophér, who is master of others experience, as well as of his own, and duly qualified to reflect upon both, will find strong motives to the belief of christianity, in the two last of these arguments of its truth. We must here observe, that the word experience may admit of several senses, whereof one is far more comprehensive than another; and likewise, of several divisions, and distributions. For, besides its more restrained acceptation, it is sometimes set in contra-distinction to reason, so as to comprehend, not only those phenomena that nature or art exhibits to our outward senses, but the things we perceive to pass within ourselves, and all those ways of information, whereby we attain any knowledge that we do not owe to abstracted reason. So that, without stretching the word to its utmost extent, and to which it has been enlarg'd, it may be look'd upon as a very comprehensive term, and fit to be branched into parts. I shall, therefore, divide it into personal, historical, and supernatural, or theological. PHYSICS. *Different kind of experience.*

I call that personal experience, which a man acquires immediately by himself, and which accrues to him by his own sensations, or the exercise of his faculties, without the intervention of any external testimony. 'Tis by this experience we know, that the sun is bright; fire, hot; snow, cold, and white; that, upon the want of aliment, we feel hunger; that we hope for future good; that we love what we judge good, and hate what we think evil; and discern that there is a great difference between a triangle, and a circle, and can distinguish them by it. *Personal.*

By historical experience, I mean that, which tho' it were personal in some other man, is but by his relation, or testimony, whether immediately, or mediately conveyed to us. 'Tis by this we know, that there were such men as *Julius Cæsar*, and *William* the conqueror; and that *Joseph* knew *Pharaoh* had a dream, which the *Egyptian* wise-men could not expound. *Historical.*

Theological experience is that, by which we know what (supposing a divine revelation) God is pleased to relate, or declare concerning himself, his attributes, his actions, his will, or his purposes; whether immediately, as he sometimes did to *Job* and *Moses*, and constantly to our Saviour; or by the intervention of angels, prophets, apostles, or inspired persons; as he did to the *Israelites*, and the primitive christian church; and still does to us, by those written testimonies we call the scriptures. *And theological, or supernatural.*

By personal experience, we know that there are stars in the heavens; by historical experience, we know that there was a new star seen by *Tycho*, and other astronomers, in *Cassiopeia*, in the year 1572; and, by theological experience, we know, that the stars were made on the fourth day of the creation.

I do not, therefore, here take experience in the strictest sense of all, but in a greater latitude, for the knowledge we have of any matter of fact; which, without owing it to our reasoning faculty, we either acquire by the immediate testimony of our own senses, and other faculties, or, it accrues to us by the communicated testimony of others. And, even in common acceptation, the word experience is not always meant of that which

PHYSICS. is immediate, but is often taken in a latitude. As when we ſay, experience teaches us, who, perhaps, were never out of *England*, that the torrid zone is inhabited; and perſuades learned men, who had opportunity to make nice aſtronomical obſervations, that ſtars may be generated, and periſh; or, at leaſt, begin to appear, and then diſappear in the celeſtial region of the world. On this kind of hiſtorical experience, conſiſting of the perſonal obſervations of *Hippocrates*, *Galen*, and other phyſicians, tranſmitted to us, a great part of the practice of phyſic is founded. And the moſt rational phyſicians take, as matters of fact, not only what other phyſicians have left upon record, but many preſent things, which themſelves can know but by the relation of their patients; as, that a man has a particular antipathy to ſuch a thing, which the doctor, perhaps, judges fit for him to uſe; or, that a woman with child, longs for this, or that determinate thing. And, phyſicians reduce theſe, and the like matters of fact, to experience, as to one of the two columns of phyſic, diſtinguiſhed from reaſon.

Since, then, learned men, as well as cuſtom, confine not the application of the word experience, to that which is perſonal, but employ it in a far greater latitude; I ſee not, why that, which I call theological experience, may not be admitted; ſince the revelations that God makes concerning what he has done, or purpoſes to do, are but teſtimonies of things, moſt of them matters of fact, and all of them ſuch, as, (ſo far as they are merely revelations) cannot be known by reaſoning, but by teſtimony; whoſe being divine, and relating to theological ſubjects, does not alter its nature, tho' it give it a peculiar and ſuper-eminent authority.

To apply this diſtribution of experience, to the matters of fact, that recommend the credibility of the chriſtian religion, I ſhall offer my thoughts in two diſtinct propoſitions.

PROP. I.

We ought to believe ſeveral things upon the information of experience, mediate and immediate, which, without that information, we ſhould judge unfit to be credited; or, antecedently to it, actually judged contrary to reaſon.

This propoſition may be underſtood, either of perſons, or of things, and will hold true in both.

And, firſt, as to perſons; if our own obſervation of what occurs among mankind, does not ſatisfy us, that we are oblig'd, after ſufficient tryal, frequently to alter the opinions, which, upon probable reaſons, we had before entertain'd, of the fidelity, prudence, juſtice, chaſtity, *&c.* of this or that perſon; we need only turn to the records of hiſtory, or appeal to the tribunals of judges. For, in both we find but too many inſtances and proofs from matters of fact, that perſons look'd on, even by intelligent men, as honeſt, virtuous, and holy, have prov'd falſe, perfidious, diſloyal, unjuſt, ſacrilegious, perjur'd, *&c.* And, in the courts of juſtice, we find a great deal of time employ'd to detect not only civil tranſgreſſions;

as thefts, cheats, forgery, falſe-witneſs, adultery, and the like, perpetrated by thoſe, who, before they were throughly ſifted, paſs'd for honeſt; but, even ſins againſt nature; as, the murders of parents by their children, and children by their parents, *&c.* Whence 'tis plain, that we ought, upon the teſtimony of experience, to change the opinions we thought we had rationally taken up of perſons. I now proceed to make good the propoſition, in the ſenſe I chiefly intended, which is, as it relates to things. PHYSICS.

If experience did not inform and certify us, who would believe, that a little heap of light, black grains of matter, ſhould be able to over-turn ſtone-walls, blow up whole caſtles and rocks themſelves, and do thoſe other ſtupendous things, that we ſee actually perform'd by gun-powder, made uſe of in ordinance, and in mines? Who would think, that two or three grains of opium, ſhould ſo ſtupify a large human body, as to force a ſleep; and often, even without that, ſuſpend the ſharpeſt torments in the cholic, gout, and other the moſt painful diſeaſes, in patients of quite different ages, ſexes, and conſtitutions; in whom alſo the diſeaſes are produc'd by differing, or even by contrary cauſes? Who would believe, that the poiſon adhering to the tooth of a mad dog, tho', perhaps, ſo little, as to be ſcarce diſcernible by ſenſe, ſhould be able, after the ſlight hurt is quite healed, to continue in the warm, perſpirable body of the bitten perſon, not only for ſome days, or months, but, ſometimes, for very many years? And, after having lurk'd all that while, without giving any trouble to the patient, that it ſhould, on a ſudden, pervert the whole œconomy of his body, and produce a madneſs like that of the dog which bit him, diſcovering itſelf by that ſtrange and fatal ſymptom, an *Hydrophobia?*

But, beſides a multitude of inſtances that may be given of truths, which, were it not for experience, we ſhould refuſe to believe; becauſe the ſmall ſtrength of ſuch agents, ſeems altogether diſproportionate to the effects aſcribed to them; many other inſtances might be alledg'd, wherein we aſſent to experience, even when its informations ſeem contrary to reaſon, and that which, perhaps, we did actually, and, without ſcruple, take to be true.

Since gravity is the principle, that determines falling bodies to move towards the center of the earth; it ſeems very rational to believe, with the generality of philoſophers, that, in proportion as one body is more heavy than another, ſo it ſhall fall to the ground faſter than the other. Whence it has been inferr'd, that of two homogeneous bodies, whereof one, for example, weighs ten pounds, and the other but one pound; the former, being let fall from the ſame height, and at the ſame time with the latter, will reach the ground ten times ſooner.

But, notwithſtanding this plauſible reaſoning, experience ſhews us, (and I have purpoſely try'd it) that bodies of very unequal weight, let fall together, will reach the ground at the ſame time; or ſo near it, that 'tis not eaſy to perceive any difference in the velocity of their deſcent, from a moderate height.

PHYSICS. 'Tis generally taken for granted, by naturalists, as well as others, that strong and loud sounds, as they are heard much farther off, than fainter; so, if the sonorous bodies be equally distant from the ear, the very strong sound will arrive much sooner at it, than the other; yet, by the experiments of the moderns, about the velocity of sounds, (in making many of which, I have endeavour'd at accuracy) it appears, that weaker sounds are (at least, as to sense) transmitted through the air as swiftly as the stronger. And, indeed, 'tis often observ'd, that when cannons and muskets are discharg'd together, the reports of both arrrive together at the ear.

It seems irrational to conceive, that a smaller and weaker load-stone, should draw away a piece of steel from a larger and stronger; yet, my experience (and that of others) proves, that, in some cases, this paradox is a truth.

It has generally, by philosophers, as well as other men, been look'd upon as manifest, and agreeable to reason, that cold condenses water, more or less, according to its degree; and, consequently, that ice is water reduced into a less bulk. But, 'tis plain, by experiments carefully made, that water is, by glaciation, rather expanded; or, at least, that ice takes up more room, than the water did before it was congeal'd. And of this sort of instances, it were easy to add a vast number.

And, to extend the force of our arguments, to that experience which is not immediate, or personally our own, but communicated by others; provided it be competently attested, and duly convey'd to us; there will need but a little reflection on what is judg'd reasonable, and freely practis'd, by philosophers themselves. For, how many conclusions have the modern naturalists admitted, tho' abstract reasoning never led men to make them, and even while plausible arguments, and the notions and axioms of the most generally receiv'd philosophy, were contrary to them? Thus, that in the heavens there should be generations and corruptions, was not only unobserv'd before the time of *Aristotle*, but is contradicted by his arguments; yet I, with many others, have seen great spots (perhaps bigger than *Europe*) generated and dissipated on or near the surface of the sun: and several of the modern philosophers and astronomers, having never beheld any of these, must take the phenomena upon the credit of those who have observ'd them. And much more must they do so, who, in spite of the vulgar philosophy, which made all comets sublunary, believe they are celestial bodies. For, that they appear above the concave of the moon's orb, we credit upon the affirmation of those who observ'd them; which very few have done themselves. And the height of the famous comet, or disappearing star, in *Cassiopeia*, in the year 1572, whereon so much stress is laid by our philosophers and mathematicians, is admitted and urged, chiefly from the opinion they have, not only of *Tycho*'s veracity, but his skill in observing the motions and phenomena of that celestial light; and particularly its having no parallax.

In ſhort, the great archictect of experimental hiſtory, Sir *Francis Bacon*, when he divides it but into three parts, aſſigns the ſecond of them to what he calls preter-generations; ſuch as monſters, prodigies, and other things; which being, as to us, but caſualties, all thoſe that happen'd in other times and places than we have lived in, we muſt take upon the credit of others. And yet theſe mediate experiments, by ſuggeſting new inſtances of nature's power, and uncommon ways of working; and by overthrowing, or limiting, received rules and traditions; afford us a conſiderable and inſtructive part of natural hiſtory, without which, it would not be either ſo ſound, or ſo compleat. PHYSICS.

PROP. II.

We ought to have a great and particular regard to thoſe things that are recommended to our belief, by what we have reduced to real, tho' ſupernatural experience.

For, 1. 'Tis manifeſt, that the moſt rational men ſcruple not to believe, upon competent teſtimony, many things, whoſe truth did no way appear to them, by conſidering the nature of the things themſelves; nay, tho' what is thus believ'd upon teſtimony, be ſo ſtrange, and, ſetting aſide that teſtimony, would ſeem ſo irrational, that antecedently to it, the things, at laſt, admitted as truths, were actually rejected as errors, or judg'd altogether unfit to be believ'd. And the points wherein experience overrules that, which, before it happen'd, was judg'd to be moſt agreeable to reaſon, concern things merely natural, or civil, whereof human reaſon is held to be a proper judge; whereas, many of the points recommended by ſupernatural experience, concern things of a ſuperior order; many of which are not to be adequately eſtimated by the ſame rules with things merely corporeal, or civil; and ſome of which, as the eſſence and manner of exiſtence, and ſome peculiar attributes, of the infinite God, involve or require ſuch a knowledge of what is infinite, as vaſtly exceeds the reach of our limited underſtanding.

But this is not all: for, 2. As 'tis, with juſtice, generally granted, that the better qualified a witneſs is, in the capacity of a witneſs, the ſtronger aſſent his teſtimony deſerves; ſo we ought, of all the things that can be recommended to us by teſtimony, to receive thoſe with the higheſt degree of aſſent, that are taught us by God, thro' the means of thoſe perſons who appear to have been commiſſioned by him, to declare his mind to men. For the two grand requiſites of a witneſs, being the knowledge he has of the things he delivers, and his faithfulneſs in truly delivering what he knows; all human teſtimony muſt, on theſe accounts, be inferior to divine teſtimony: ſince the latter is warranted both by the veracity of God, and by his boundleſs knowledge; which makes it as impoſſible he ſhould be deceiv'd himſelf, as the other does, that he ſhould deceive us. And, becauſe that, for the delivery of divine teſtimony, it has often pleas'd God, who is a moſt free, as well as a moſt wiſe agent, to make uſe of unpromiſing

PHYSICS. mising persons as his instruments; I shall not altogether overlook this circumstance, that an experimental philosopher so often increases his knowledge of natural things, by what he learns from the observations and practices, even of mean, and, perhaps, of illiterate persons, because they are conversant with the works of nature; that he is not only willing to admit, but often curious to seek for informations from them: and therefore he is not likely to shew much reluctance in receiving the doctrines of revealed religion, such as christianity, if the teachers of it were honest men, and had opportunity to know the truth of the things they deliver; tho' they were of some mean profession.

And, indeed, such a person as our virtuoso, will, with great willingness, exercise himself in perusing, with attention, and much regard, the writings of the apostles, evangelists, and ancient prophets; notwithstanding any meanness of their first condition, or of their secular employments. And here, he will not only readily suffer himself to be instructed in the grand and general articles of religion, which, because of their necessity, or very great usefulness, are to be met with in many places, and in variety of expressions, by honest and duly-disposed readers; but he will, instead of disdaining such tutors, both expect, and carefully strive, to improve his knowledge of divine things in general, even by those hints, and incidental passages, that a careless or ordinary reader would overlook, or not expect any thing from. For, as the fertility of the scriptures is not usually enough discern'd by vulgar readers, when the sacred writers transiently touch upon a great many subjects, that they do not expresly handle; so, the docility we have ascribed to our virtuoso, will make him repose a great deal of trust in the testimony of inspired persons, such as Christ and his apostles, about things of all sorts, whether usually taken notice of, or not, that relate to objects of a supernatural order; especially if among these, God himself, and his purposes, be comprized: since several of those things are not knowable without revelation, and others are best known by it. And to be allow'd to ground a belief about such things, on the relations and other testimonies of those that were "eye-witnesses and ministers" of the things they speak of, will, by our virtuoso, be justly reputed such an advantage, in order to the knowledge of things divine; as the consulting with navigators and travellers to *America*, is, to a person curious to learn the state of that new world. For an ordinary sea-man or traveller, who had the opportunity, with *Columbus*, to sail along the several coasts of it, and pass up and down thorough the country, was able at his return to inform men of a hundred things, that they would never have learn'd by *Aristotle*'s philosophy, or *Ptolemy*'s geography; and might not only acquaint them with many particulars, agreeable to the opinions which their receiv'd physics and cosmography suggested, but also rectify several erroneous presumptions and mistakes, which, till then, they thought very agreeable to the dictates of those sciences, and to reason. And as one, who had a candid and knowing friend, intimate with *Columbus*, might better rely on his informations about many particulars of the natural history of those parts, than on those of a hundred school-

school-philosophers, who knew only what they learned from *Aristotle*, *Pliny*, *Ælian*, and the like ancient naturalists; much more may we rely on the accounts given us of divine things, by the apostles, and constant attendants of him who lay in the "bosom of God his father," and commission'd them to declare to the world the "whole counsel of God," as far as 'twas necessary for man to know.

Fuller trials are allow'd, among ingenious men, to rectify the informations of the more imperfect ones; and therefore I shall add, that, tho' the innate notions and sentiments, which nature gives us of the attributes and mind of God, be highly to be prized; yet the informations that theological experience affords of those abstruse things, is far more excellent and compleat.

I have dwelt the longer upon the miracles that may be pleaded to recommend the christian religion, because I thought, that an argument grounded on them is little less than absolutely necessary, to prove that any religion men believe to be supernaturally reveal'd, really proceeds from God.

For, tho' the excellency of the christian doctrine, and other concurrent motives, may justly persuade me, that 'tis worthy and likely to be given by God; yet that, in fact, this doctrine comes from him by way of supernatural revelation, I can scarce be sufficiently ascertain'd, but by the miracles wrought by Christ and his disciples, to evince, that the doctrine they preach'd, as commission'd by God to do so, was indeed his; being, as such, own'd by him. But these miracles having been wrought in the first ages of the church; we can have no knowledge of them by our own senses, or immediate observation, but must take them upon the credit of historical experience, which is afforded us by the duly transmitted testimony of those, who were themselves eye-witnesses of the things they relate. And since we scruple not to believe such strange prodigies, as celestial comets, vanishing and re-appearing stars, islands founded by subterraneal fires in the sea, the darkness of the sun for many months together, earthquakes reaching above a thousand miles in length, and the like amazing irregularities of nature, upon the credit of human histories; I see not, why that historical experience should not more be trusted, which has many peculiar and concurrent circumstances to confirm it; and particularly the death that most of the first promulgators chearfully suffer'd to attest the truth of it; and the success and spreading of the doctrine authorized by those miracles, and receiv'd chiefly upon their account. To which things, some perhaps would add, that 'tis less incredible, that the author of nature should, for most weighty purposes, make stupendous alterations of the course of nature; than that nature herself, for no such end, should, by such prodigies, as were lately mention'd, throw herself, as it were, out of her own course.

Miracles being so necessary to the establishment of reveal'd religion in general, it may be look'd upon as a farther disposition in our virtuoso to receive the christian religion, that the philosophy he cultivates, much conduces to enable him to judge aright of those strange things, that are by many proposed as miracles, and believed to be so.

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For, first, the knowledge he has of the various, and wonderful operations of some natural things; especially when they are skilfully improv'd, and dextrously apply'd by art, particularly, in mathematics, mechanics, and chymistry, will qualify him to distinguish between things that are only strange and surprizing, and those that are truly miraculous: so that he will not mistake the effects of natural magic for those of a divine power. And, by this means, he will be able to discover the subtile cheats, and collusions of impostors; whereby not only the multitude of all religions, especially the heathen, but even learned men of most others, for want of an insight into real philosophy, have formerly been, or are at this day, deluded, and drawn into idolatrous, superstitious, or otherwise erroneous tenets, or practices.

On the other side, the knowledge our virtuoso may have of what cannot be justly expected, or pretended, from the mechanical powers of matter, will enable him to discern, that many things are not producible by them, without the intervention of an intelligent superior power; on which account, he will frankly acknowledge, and heartily believe several effects to be truly miraculous, that may be plausibly enough ascribed to other causes in the vulgar philosophy; where men are taught to attribute stupendous and unaccountable effects to sympathy, antipathy, *Fuga vacui*, substantial forms, and, especially, to a certain Being, presumed to be almost infinitely potent and wise, which they call nature: for this is represented as a kind of goddess, whose power may be little less than boundless: thus, I remember, *Galen* himself compares it to that of God; and saith, that he could not do such a thing, because nature could not; and censures *Moses* for speaking as if he were of another mind.

I know it may be objected, that those who are so well acquainted with the mysteries of nature, and her various, and strange ways of working, as our virtuoso is supposed to be, may, by that knowledge, be strongly tempted to think, that those surprizing things, other men call miracles, are but effects of her power; the extent of which is not easily discern'd by ordinary men, nor safely defined by philosophers themselves. To this I shall only here reply, that to make it reasonable to judge a particular performance supernatural, it is not at all necessary that it surpass the whole power of nature, that is, of physical agents; provided it surpass the power of that cause, or that complex of causes, from which the effect must in reason, if it be purely natural, or physical, be supposed to have proceeded. For instance, that a fisherman or two should speak other languages than their own, does not at all exceed the power of nature, if they employ'd a competent time in learning them: but, that a great number of fishermen, and other illiterate persons, should, all on a sudden, become linguists, and, in an hour's time, be able to speak intelligibly to a great number and variety of nations, in their respective languages, as the new testament relates, that the apostles, and their companions did on the day of pentecost; this gift of tongues, I say, was an ability, which, in those

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circumstances of place, time, and persons, wherein 'twas exercised, may justly be concluded to have been supernatural, or miraculous.

To proceed, 'tis a notorious matter of fact, that, in less than half an age, the christian religion was spread over a great part of the then known world; so that, in a few years after it began to be preach'd, the apostle of the *Gentiles* could tell the *Romans*, with joy, "that their faith was spoken "of throughout the whole world." And, in the second century, *Tertullian*, and other famous writers, shew, that the gospel had already numerous proselytes in a great number of different kingdoms, and provinces. This wonderful quick progress of the christian religion, being ascertain'd to our virtuoso, from a thing whereby he is so much sway'd, as experience; it greatly disposes him to believe the truth of so prevalent a religion. For, if he considers the persons who first promulgated it; they were but half a score of illiterate fishermen, a few tent-makers, and other tradesmen. If he considers the means employ'd to propagate this doctrine, he finds, that they had neither arms, nor external power, to compel men to receive it; nor riches, honours, or preferments, to bribe or allure them to it; nor were they men of philosophical subtilty, to intrap, or entangle the minds of their auditors. Nor did they make use of the pompous ornaments of rhetoric, and glosses of oratory, to inveigle, or entice men; but treated of the most sublime and abstruse matters, in a most plain and unaffected style; as became lovers and teachers of truth. If he considers the nature of the doctrine, that, in a little time, obtain'd so many proselytes, he will find, that, instead of being suited to the natural apprehensions, or the receiv'd opinions of men; and, instead of gratifying their corrupt affections, or complying with so much as their most innocent interests; it prescribed such mortifications, and such great strictness of life, and high degrees of virtue, as no legislator had ever dar'd to impose upon his subjects, nor any philosopher on his disciples. And this doctrine was proposed in such a way, and was accompany'd with predictions of such hardships and persecutions, that should, in those times, be the portion of its sincere professors; as if the law-giver had design'd rather to fright men from his doctrine, than allure them to it: since they could not believe what he said, and foretold, to be true, without believing, that they should be made great sufferers by that belief. If our virtuoso considers the opposition made to the progress of the gospel, he will find cause to wonder, that it could ever be surmounted. For the heathens, which made by far the greatest part of the world, were deeply engaged in polytheism, idolatry, magical rites, and superstitions, and almost all kind of crimes; and some of these were shameless debaucheries, which oftentimes made a part of their worship. And the *Jews*, by the corrupt leaven of the pharisees, the impious errors of the sadduces, and the general mistakes of the nation about the person, office, and kindgom of the Messias; and by their dotage upon their vain traditions, and numerous superstitions, grounded upon them, were, on these and other accounts, highly indisposed, as well as the *Romans*, the two nations to be converted, to be made proselytes:

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One circumstance there is of the propagation of the gospel, which, tho' it may seem more extrinsecal than those hitherto mention'd, is yet too considerable to be here omitted; I mean, that the quick spreading, and success of the christian doctrine in the world, was foretold both by the prophets of the old testament, and the author and promulgators of the new. For, it being notorious, that many errors and superstitions, have, with too much celerity, been spread far and wide in the world; either by reputed mere accidents, that were very favourable to them, or by the industry and artifices of men; it ought to be no small satisfaction to equitable judges, that the sudden progress, and notable effects of the christian religion, were foretold by the ancient prophets, and by the Messias and his apostles. For, by these accomplish'd predictions, it may appear, that the wonderful success of the gospel was not an effect of chance, but was, long before, determin'd by divine providence, to be accomplish'd in a wonderful manner, by his peculiar assistance.

But true prophecies of unlikely events, fulfill'd by unlikely means, are supernatural things; and, as such, may properly be reckon'd among miracles. We might add, that these have a peculiar advantage above most other miracles, on the account of their duration: ſince the manifeſt proofs of the predictions continue ſtill, and are as viſible as the extent of the chriſtian religion; and ſome of them are ſtill more and more accompliſh'd, by the converſions made of multitudes of infidels, in ſeveral vaſt regions of *America*. So that if we may call ſome tranſient miracles, ſuch as the turning water into wine, at a marriage-feaſt in *Galilee*; and the darkening of the ſun when the moon was in the full, at the crucifixion of Chriſt; accompliſh'd predictions may be ſtyled permanent ones; and their difference may appear by the differing ſtates of the *Moſaic* manna: for, tho both that which fell daily (except on the ſabbath) in the wilderneſs, and that which was laid up in a pot before the teſtimony, were ſupernatural productions; yet, tho' a portion of the former out-laſted not two or three days; that kept in the pot, was preſerv'd for many ages, and continu'd to be (as it was foretold it ſhould) a viſible miracle.

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Beſides, the preachers of the chriſtian religion, both pretended and appeal'd to miracles, as proofs of the truth of their doctrine: and if we conſider the great diſadvantages they lay under, and the powerful oppoſition of all ſorts, that they met with and ſurmounted; it cannot reaſonably be thought, that ſuch unlikely men ſhould ſo ſucceſsfully preach ſo uninviting a doctrine, unleſs it were confirm'd by conſpicuous miracles. Or, at leaſt, if ſo uneaſy and perſecuted a religion, was propagated without miracles, that propagation itſelf may juſtly paſs for a miracle; and be no leſs fit than another, to confirm the religion ſo admirably propagated.

Thus, I think, I have ſhewn, that a virtuoſo has ſome helps, which other men, generally ſpeaking, want, to make him judiciouſly approve the arguments for the truth of the chriſtian religion, grounded on the miracles wrought in its favour, and the wonderful ſucceſs of it in the world. But, becauſe a reveal'd religion, how true ſoever, can ſcarce be proved but by moral demonſtrations; and becauſe it is not, therefore, always ſufficient, that the arguments be good in their kind; but there are ſome qualifications requir'd in the minds of thoſe who are to be convinc'd by them; I ſhall now add, that experimental philoſophy alſo diſpoſes the mind of its cultivator to receive due impreſſions from ſuch proofs, as miracles, as well as other topics, afford the chriſtian religion.

Another thing, then, that qualifies an experimental philoſopher, for the reception of reveal'd religion, is, the habit of endeavouring to give clear explanations of the phenomena of nature, and to diſcover the weakneſs of thoſe ſolutions which ſuperficial wits uſually make and acquieſce in, inſenſibly works in him a great and ingenuous modeſty of mind. And, on account of this intellectual, as well as moral virtue, he will not only be very inclinable, both to deſire and admit further information, about things which he perceives to be dark, or abſtruſe; but he will be very unapt to take, for the adequate ſtandard of truth, a thing ſo imperfectly inform'd, and narrowly limited, as his mere, or abſtracted reaſon. And tho' a vulgar philoſopher, who allows himſelf to refer the obſcureſt things in na-

PHYSICS. ture, to ſubſtantial forms, real qualities, ſympathy, antipathy, &c. which need not, and, perhaps, cannot be clearly underſtood; and thence preſumes that he underſtands every thing; and that thoſe things muſt be falſe, or impoſſible, which agree not with his philoſophy; yet a ſober and experienc'd naturaliſt, who knows what difficulties remain unſurmounted, in the conception and explanations even of things corporeal, will not, by a lazy, or arrogant preſumption, that his knowledge about things ſupernatural, is already ſufficient, be induced to reject, or to neglect any information that may increaſe it.

And this frame of mind is a very happy one for a ſtudent in reveal'd religion; where cautiouſneſs is not more neceſſary for the avoiding of errors, than docility is advantageous, in learning of truth: ſince the knowledge and goodneſs of the divine teacher, is ſuch, that a ſcholar, to improve his intellect, needs but bring a mind fitted to receive the genuine informations, that are moſt liberally offer'd, and will never deceive him, who employs a due care not to miſtake the meaning of them.

A well-cultivated converſation with the works of God, brings a man to diſcover, from time to time, ſo many things to be feaſible, or true, which, whilſt he argued but upon the ſhallow grounds of uninform'd reaſon, he judg'd falſe, or unpracticable; that, by degrees, he acquires a habit of receiving ſome ſorts of opinions, and eſpecially thoſe that ſeem unfavourable to religion, with a diſpoſition to reform or diſcard them, upon further information. And this, as he is reſolv'd to ſubmit to, in caſe he meets with it, ſo he is diſpos'd to receive it, by having often found himſelf obliged, upon ſubſequent information, to mend, or lay aſide his former opinions; tho' very agreeable to the beſt light he had to judge by, when he entertain'd them. Thus, tho' it ſeems a viſible truth, that the diſc of *Venus*, is, in all reſpects to the ſun, totally luminous; yet, when the teleſcope diſcovers her to have her full and her wane, like the moon, he will believe this further obſervation, againſt the firſt, made with his naked eyes.

And, indeed, I have ſometimes doubted, whether to be vers'd in mathematics, and other demonſtrative parts of philoſophy, brings a greater advantage to the mind, by accuſtoming and aſſiſting it, ſtrictly to examine things propoſed for truths, and ſtrongly to evince the truths a man knows, to others; than by fitting him to diſcern the force of a good argument, and ſubmit willingly to truths clearly proved; how little ſoever he may have expected to find ſuch concluſions true. 'Twill not be difficult to apply theſe reflections to our preſent purpoſe; ſince there are ſeveral paſſages in the ſcripture that ſufficiently declare, both that multitudes perſiſt in a criminal infidelity, out of a fond conceit of their own knowledge, and a readineſs to be ſway'd rather by ſtrong prejudices, than by the ſtrongeſt arguments that would remove them; and, that docility is a very happy diſpoſition to the entertainment of reveal'd religion. And this qualification will be the more eaſily found in our virtuoſo; becauſe, tho' the things, about which he has long been ſenſible of his ignorance, or deſires further inſtruction, are

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within the ſphere of nature, and the juriſdiction of philoſophy; many of the things which reveal'd religion declares, are ſo ſublime and abſtruſe, that they may well be look'd upon as of an higher order than merely phyſical ones; and, therefore, cannot be ſatisfactorily reach'd by the mere light of nature. 'Tis true, our philoſopher will examine more ſtrictly, than ordinary men, the proofs brought for this, or that propoſed revelation; but that is no diſadvantage to a ſupernatural religion, ſuch as the chriſtian, if it be true: and the real truth about religion itſelf, does not require credulity, but only docility. If a piece of coin, that paſſes for true gold, be offer'd to an ordinary man, and to a skilful refiner, the latter, will, indeed, examine it more ſtrictly, and not acquieſce in the ſtamp, the colour, the ſound, and other obvious marks, that may ſatisfy a ſhop-keeper, or a merchant; yet, when he has try'd it by the ſeverer ways of examining, ſuch as the touch-ſtone, the cupel, *Aqua fortis*, &c. and finds it to hold good in thoſe proofs, he will readily and frankly acknowledge, that 'tis true gold, and be more thoroughly convinc'd of it, than the other perſon; whoſe want of skill will make him ſtill apt to retain a diſtruſt, and render him, indeed, more eaſy to be perſuaded, but more difficult to be fully ſatisfy'd. Thus, tho' our virtuoſo will examine, with more ſtrictneſs and skill, than ordinary men are able, miracles, prophecies, or other proofs, ſaid to be ſupernatural, that are alledg'd to evince a reveal'd religion; yet, if the certain, and genuine characters of truth appear in them, he will be more thoroughly convinc'd of it, than a leſs skilful man, whoſe want of good criteria, and ſound judgment, incline him to be diffident, and to be ſtill afraid of having been impoſed on.

I expect, it will be here objected, that I degrade the human underſtanding, by aſcribing ſo much to experience, natural or ſupernatural, that it has left nothing for reaſon to do, but ſervilely to obey.

This objection, indeed, is plauſible; yet the anſwer to it will not be very difficult, if the matter itſelf be duly conſider'd, and reaſon be brought to act, not as an intereſted party, but an unbyaſs'd judge.

For we have already ſhewn, that rational philoſophers ſcruple not to alter, or renounce, the opinions which ſpecious reaſons had ſuggeſted to them, when once they either find thoſe opinions contradicted by experience, or meet with other opinions more conformable to it. And *Ariſtotle* himſelf, tho' he be accuſed of having wreſted phyſics to logical, and metaphyſical fancies, confeſſes, not only that in the ſcience of nature, reaſon ought to comport with the phenomena, and the phenomena with reaſon; but that to adhere to plauſible reaſonings, and neglect ſenſible obſervations, is a weakneſs or diſeaſe of the mind. And whether the underſtanding be, as *Ariſtotle* taught, like blank paper; and receives no knowledge, but what has been convey'd to it through the ſenſes; or whether the notions are congenite with the underſtanding, or ſo eaſily, and early, acquired by it, as to appear innate; they are but very few, in compariſon of thoſe that are requiſite to judge aright about any one thing, that occurs either

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PHYSICS. in natural philoſophy, or theology. For, in the divine nature, power, wiſdom, and other attributes, there is an exuberance that has produced numberleſs contrivances, laws, and other things, which exceedingly ſurpaſs both the number and variety that the dim and limited intellect of man could reach to, by framing, and compounding ideas, without the aſſiſtance of the patterns afforded by the works and declarations of God.

On account of the ſame prerogative of the divine knowledge, it muſt frequently happen, that the notions and opinions men take up of the works and mind of God, upon the mere ſuggeſtions of abſtract reaſon; will not only be almoſt conſtantly very deficient, but often very erroneous. Of this we ſee evident proofs in many opinions of the old philoſophers; who, tho' men of ſtrong natural parts, were miſ-led, by what they miſtook for reaſon, to maintain ſuch things about the works, and the author of nature, as we, who, by the favour of experience and revelation, ſtand in a much clearer light, know to be falſe, and often juſtly think utterly extravagant.

Abſtract reaſon is a narrow thing, and reaches but to a very ſmall ſhare of the knowable truths, whether human or divine; which may be obtain'd by the help of further experience, and ſupernatural revelation. This reaſon, furniſh'd with no other notices than it can ſupply itſelf with, is ſo ſhallow and deceitful, that he who ſeeks for knowledge only within himſelf, ſhall be ſure to be quite ignorant of far the greateſt part of things; and will ſcarce eſcape being miſtaken about a large part of thoſe he thinks he knows.

I am far from intending to deny reaſon any of its juſt prerogatives; experience itſelf is but an aſſiſtant to reaſon, and ſupplies informations to the underſtanding; which ſtill remains the judge, and has the power, or right, to examine, and make uſe of the teſtimonies that are preſented to it. The outward ſenſes are but the inſtruments of the ſoul, which hears by the intervention of the ear, and in reſpect of which, the eye itſelf is but a more immediate optical tube; and the ſenſe does but perceive objects, not judge of them. Nor do the more wary among the philoſophers truſt their eye, to teach them the nature of the viſible object; but only employ it to perceive the phenomena it exhibits, and the changes that happen to itſelf by the action of it. 'Tis confeſs'd too, that the ſenſes may deceive us, if the requiſites of ſenſation be wanting; as, when a ſquare tower appears round at a diſtance, and a ſtrait ſtick, half in the water, appears crooked: 'tis, therefore, the part of reaſon, not ſenſe, to judge whether none of the requiſites of ſenſation be wanting; which, by the way, often requires, not only reaſon, but philoſophy: and then, alſo, 'tis the part of reaſon to judge what concluſions may, and what cannot, be ſafely grounded on the informations of the ſenſes, and the teſtimony of experience. So that, when we ſay, experience corrects reaſon, 'tis an improper way of ſpeaking; ſince 'tis reaſon itſelf, that, upon the information of experience, corrects the judgment it had made before.

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To illuſtrate the uſe of reaſon, let us ſuppoſe an able judge coming to hear, and decide cauſes, in a ſtrange country: 'tis plain, that the general notions he brings with him, and the dictates of juſtice and equity, can give him but a very ſhort and imperfect knowledge of many things, that are requiſite to frame a right judgment about the caſes which will firſt be brought before him; and, till he has heard the witneſſes, he may be very apt to fall into prejudiced opinions of things: but, when an authentic and ſufficient teſtimony has clear'd matters to him, he then pronounces, according to the light of reaſon he is maſter of; to which the witneſſes did but give information; tho' that ſubſequent information may have obliged him to lay aſide ſome prejudices he had entertain'd before he receiv'd it. And, what is ſaid of natural experience, with regard to the underſtanding, may, with due alteration, be apply'd to ſupernatural revelation: for here, alſo, the underſtanding is to examine, whether the teſtimony be indeed divine; and whether a divine teſtimony ought to be believ'd, in what it clearly teaches. I do not, therefore, degrade reaſon from the dignity that belongs to it, of perceiving, and judging; tho' it be obliged, by its own dictates, to take in all the aſſiſtance it can from experience, whether natural, or ſupernatural; and by the fuller accounts of things it receives from thoſe informations, to rectify, if need be, its former leſs mature judgment.

In ſhort, thoſe who cry up abſtract reaſon, as if it were ſelf-ſufficient, exalt it in words; but we who addreſs reaſon to phyſical and theological experience, and direct it how to conſult both, and take its informations from thence, exalt it in effect: and reaſon is much leſs uſefully ſerv'd by the former ſort of men, than by the latter; ſince, whilſt thoſe do but flatter it, theſe take the right way to improve it.

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High Veneration

Man's INTELLECT owes to

GOD.

'TIS with indignation and wonder, that I hear many men, and ſome of them divines too, who, little conſidering what God is, and what themſelves are, talk of him and his attributes, as freely, and as unpremeditately, as they would of a geometrical figure, or a mechanical engine. And, even the leſs preſumptuous frequently diſcourſe, as if the nature and perfections of that unparallel'd Being, were objects their intellects could graſp; and ſcruple not to dogmatize about thoſe abſtruſe ſubjects, as freely as about other things, that are confeſſedly within the reach of human reaſon.

Yet God may have ſeveral attributes, and perfections, utterly unknown to us; and of thoſe attributes whereof we have ſome knowledge, there are effects and properties, whoſe ſublimity, or abſtruſeneſs, ſurpaſſing our comprehenſion, makes the divine cauſe, or author of them, deſerve our higheſt wonder and veneration.

As there are two chief ways to arrive at the knowledge of God's attributes; the contemplation of his works, and the ſtudy of his word; it may be doubted whether either, or both of theſe, will ſuffice to acquaint us with all his perfections.

For tho' philoſophers have rationally deduced the power, wiſdom, and goodneſs of God, from thoſe marks of them which he hath ſtampt upon many of his viſible works; yet, ſince the divine attributes which the creatures point at, are thoſe whereof themſelves have ſome participation, or reſemblance; and, ſince the fertility of the divine nature is ſuch, that its ex-

excellencies may be participated, or repreſented many ways; how can we be ſure, but ſo perfect, and exuberant a Being, has excellencies not expreſſed in the viſible world, or any of its known parts? Physics.

Some of thoſe divine attributes we know, being relative to the creatures, could ſcarce be diſcover'd by ſuch imperfect intellects as ours, but from conſidering ſome things actually done by God. Thus, ſuppoſing, before the foundations of the viſible world were laid, the angels had no more knowledge than men; they could ſcarce imagine in God a power of creating matter, and of producing local motion in it: much leſs could they have known how the rational ſoul, and human body act upon one another. Whence it ſeems probable, that if God hath made other ſyſtems than that wherein we live (and who can aſſure us, he hath not?) he may have diſplayed in ſome of the creatures that compoſe them, ſeveral attributes that we have not diſcover'd by the help of thoſe works of his wherewith we are acquainted.

God may have ſeveral attributes and perfections unknown to us.

I readily grant, that the revelations God hath vouchſafed us, have clearly taught us ſeveral things concerning their adorable author, which the mere light of nature either would not have ſhewn, or but very dimly diſcover'd. Yet the ſcripture itſelf informs us, that in this life "we know but in part, and ſee things but darkly;" and that we are ſo far from being able "to find out God to perfection," that even the ways of his providence are to us unſearchable. Hence God may have attributes unknown to us.

But it may be alledged, that, beſides the two ways mentioned of attaining the knowledge of God's attributes; there may be a third preferable to both the others, by conſidering the idea of a Being ſupremely, or infinitely perfect; in which idea, all poſſible perfections may be ſaid to be contained; ſo that no new one can be added to it. And, indeed, I readily grant, that this idea is the moſt genuine I am able to frame of the Deity; yet there may be ſeveral attributes, which, tho' in a general way contain'd in this idea, are not, in particular, diſcovered to us thereby. 'Tis true, that when, by whatever means, any divine perfection comes to our knowledge, we may well conclude, 'tis, in a ſenſe, comprized in the comprehenſive notion we have of a Being abſolutely perfect; tho', 'tis poſſible, that perfection would never have come to our knowledge by the bare contemplation of that general idea, but was ſuggeſted by particulars; whence ſuch diſcoveries are not ſo much derived from, as refer'd to the notion we are ſpeaking of.

Theſe conſiderations perſuade me, that God may have, not only various attributes, but various excellencies and perfections, unknown to us. And, farther, many of the attributes we know he hath, we have but an imperfect knowledge of, eſpecially if compared with his; for he poſſeſſes them in a manner peculiar to himſelf; and far tranſcending that wherein men poſſeſs ſome faint reſemblances of them.

The power and wiſdom of God are diſplay'd both in his corporeal, and his incorporeal creatures.

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Effects of the divine power.

Among the manifold effects of the divine power, I ſhall mention only two; which, tho' often very manifeſt, are not very attentively reflected on; the immenſe quantity of corporeal ſubſtance that the divine power provided for the framing of the univerſe; and the great force of the local motion imparted to it, and regulated in it.

The vaſt magnitude of the whole material world.

The vaſtneſs of that huge maſs of matter, whereof the corporeal world conſiſts, cannot but appear ſtupendous to thoſe who ſkilfully conſider it. That part of the univerſe, which has been already diſcover'd by human eyes, aſſiſted with dioptrical glaſſes, is almoſt inconceivably vaſt. The fix'd ſtars of the firſt magnitude, that to vulgar eyes look but like ſhining ſpangles, are by aſtronomers affirm'd to exceed, each of them, above a hundred times the whole globe of the earth in magnitude: and as little as theſe ſtars ſeem to our naked eyes, they appear much leſs thro' our teleſcopes; which taking off the falſe glittering that makes them look as they are painted, ſhew them little other than phyſical points of light. And the ſun, which is granted to be ſome millions of miles nearer to us than the other fix'd ſtars; tho' it ſeem at this leſs diſtance not to be half a foot broad; is by the generality of mathematicians allow'd to be eight or ten thouſand times as big as the terraqueous globe. And it plainly appears by the parallaxes and other proofs, that the globe of earth and water we inhabit, tho' it be divided into ſo many great empires, and kingdoms, and ſeas; and tho', according to the received opinion, it be 5400 *German* leagues in circumference, and conſequently its ſolid content 10,882,080,000 cubic miles; yet this globe is ſo far from being, for its bulk, a conſiderable part of the univerſe, that 'tis in compariſon thereof but a phyſical point. Nay, thoſe far greater globes, the ſun and fix'd ſtars, and all the ſolid maſſes of the world, if reduced into one, would, perhaps, bear a leſs proportion to the fluid part of the univerſe, than a nut to the ocean. And as an excellent modern aſtronomer obſerves, all the ſtars, crowded into one body, would, at a fit diſtance, appear no bigger than a ſtar of the firſt magnitude. After all, I have hitherto ſpoke but of that part of the corporeal univerſe already ſeen by us: I muſt therefore add, that as vaſt as this is, yet all that the eye, even when powerfully aſſiſted by teleſcopes, hath diſcovered, is far from repreſenting the world of ſo great an extent, as more perfect glaſſes will do hereafter. And even then the viſible part of the world will be far from reaching to the bounds of the univerſe *.

From

* "The ſyſtem of the world is now" ſays Dr. *Halley* "underſtood to occupy the whole abyſs of ſpace, and conſequently to be actually infinite; and ſmaller fix'd ſtars being ſtill diſcoverable, as more perfect teleſcopes are employ'd, ſeems to confirm this doctrine. Were the whole ſyſtem finite, it would, tho' ever ſo extended, ſtill occupy no part of the *Infinitum* of ſpace, which neceſſarily and evidently exiſts; whence the whole would be ſurrounded with an infinite *Inane*; and the ſuperficial ſtars would gravitate towards thoſe near the centre, and with an accelerated motion run into them, and in proceſs of time unite into one. But if the whole be

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The prodigious quantity of motion given thereto, and maintain'd therein.

From the vaſt extent of the univerſe, I proceed to conſider the ſtupendous quantity of motion, that the divine power has given the parts of it, and continually maintains in it. Of this we may make an eſtimate, by conſidering with what velocity ſome of the greater bodies are moved, and how large a part of the remaining bodies of the univerſe, is alſo endow'd with motion.

The leaſt velocity I ſhall mention, is that afforded by the *Copernican* hypotheſis; according to which, the earth revolves from weſt to eaſt, about its own axis, in four and twenty hours; yet this terraqueous globe, which we think ſo great, that we commonly call it the world, moves at ſuch a rate, that, as the learned *Gaſſendus* computes, a point ſituated in the equator of it, moves about twelve hundred feet in a ſecond minute: ſo that a bullet ſhot out of a canon, ſcarce flies ſo ſwift.

But this velocity is ſcarce comparable to that of the fix'd ſtars; if we ſuppoſe them to move, in four and twenty hours, about the earth. For ſuppoſing the diſtance aſſign'd by *Tycho* between us and the firmament to be ſeven thouſand diameters of the earth; a fix'd ſtar in the equator, moves, as *Mullerius* calculates, *3,153,333* miles in an hour, and conſequently 52,555 in a minute, and 875 in a ſecond; that is, three thouſand times faſter than a canon-bullet ſhot into the air. 'Tis true, that according to the *Ptolemaic* hypotheſis, a fix'd ſtar in the equator moves, at moſt, but three ſemidiameters of the earth in a ſecond; but, according to *Ricciolus*, this velocity is fifty times greater than in the *Ptolemaic* hypotheſis; and threeſcore and ten times greater than in the *Tychonian*. For, according to him, ſuch a fix'd ſtar as we ſpeak of, moves *629,128* *Engliſh* miles in a ſecond.

But farther, that portion of the univerſe, which, tho' put into motion, is commonly ſuppoſed to be at reſt, is ſo great, that, perhaps, the quantity of motion diſtributed among ſeemingly quieſcent bodies, may exceed the quantity of motion which the firſt mover has communicated to the fix'd ſtars; tho' we ſuppoſe them whirl'd about the earth with that ſtupendous ſwiftneſs, the

" infinite, all the parts of it would be " nearly *in æquilibrio*; whence each fix'd " ſtar, being attracted by contrary powers, " would keep its place, or move till from " ſuch an equilibrium it comes to reſt. " 'Tis no more abſurd, that the fix'd ſtars " ſhould exceed any given number, than " that duration ſhould be eternal, becauſe " no number of days or years can com- " plete it. 'Tis ſaid, I know, that if the " number of the fix'd ſtars were more " than finite, the whole ſurface of their " apparent ſphere would be luminous. " But if we ſuppoſe all the fix'd ſtars to " be as far from one another, as the near- " eſt of them is from the ſun; that is, if " we may ſuppoſe the ſun to be one of " them; at a greater diſtance, their disks " and light will be diminiſh'd, and the " ſpace to contain them increaſed, in the " proportion of the ſquares: ſo that in each " ſpherical ſurface the number of ſtars it " might contain, will be as the biquadrate " of their diſtances. If then, as it needs " muſt, the diſtances be immenſely great, " it will follow, that as the light of the " fix'd ſtars diminiſhes, the intervals be- " tween them decreaſe in a leſs propor- " tion; the one being as the diſtances, " the other as the ſquares thereof, reci- " procally. And the more remote ſtars " are ſo ſmall, that they muſt vaniſh, even " in the moſt exquiſite teleſcopes; as a " ſmall teleſcopic ſtar is inviſible to the " naked eye." *Philoſ. Tranſ.* N° 364. p. 22.

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Physics. ſyſtems of *Ptolemy* and *Tycho* ſuppoſe. For the fix'd ſtars, planets, or all the mundane globes, whether lucid or opake, together bear but a ſmall proportion to the interſtellar part of the univerſe. And tho' I ſhould allow all theſe globes to be ſolid, yet it muſt be confeſs'd, that each of them ſwims in a fluid of much greater extent than itſelf. So that the fluid portion of the univerſe will, in bulk, almoſt incomparably exceed the ſolid. And if we conſider the nature of a fluid body, we ſhall find it conſiſt in having its minute parts perpetually and variouſly moved; ſo that tho' the whole body of a liquor ſeems to be at reſt, yet its component parts are in a perpetual motion; continually ſhifting places amongſt themſelves.

And becauſe the quantity of motion ſhared by the corpuſcles that compoſe fluids, is not uſually reflected on by philoſophers; I ſhall add, that we may gueſs how great and vehement a motion there may be in the parts of fluid bodies, perhaps, when the aggregates of thoſe particles appear to be at reſt, by obſerving them when their ordinary motions happen to be diſturb'd, or to be extraordinarily excited by fit conjunctures of circumſtances. Thus we ſee the ſtrange force and effects of boiſterous winds and whirl-winds, which are but ſtreams and whirl-pools of the inviſible air, whoſe ſingly inſenſible parts are by accidental cauſes determined to have their motion made either in a ſtrait line, or, as it were, about a common centre. Thus when a mine of gun-powder is ſprung, the flame, or ſome ſubtile ethereal ſubſtance, that is always at hand in the air, tho' both are fluids, and the powder perhaps be kindled but by one ſpark of fire, inſtantly exerts a moſt rapid and furious motion.

The velocity of theſe diſcharged flames may appear from that which gun-powder impreſſes on a bullet, ſhot out of a well-charg'd gun; and this *Merſennus*, after ſeveral trials made to meaſure it, defines to be about 450 feet in a ſecond. If, then, we admit the probable opinion of the *Carteſians*, that the earth and planets are turn'd about their own axes by themotion of the reſpective ethereal vortices in which they ſwim; we ſhall eaſily grant, that the motion of the celeſtial matter, which moves, for inſtance, upon the remote confines of the earth's vortex, is by a vaſt exceſs more rapid than that of the ſurface of the earth. But if we chuſe the *Tychonic* hypotheſis, which makes the firmament, with all the vaſt globes of light that adorn it, to move about their common centre in four and twenty hours, the motions of the celeſtial matter muſt be allow'd a far greater, and indeed a ſcarce imaginable rapidity.

Hence we may have more enlarg'd conceptions of the power and wiſdom of the great Creator, who has both put ſo wonderful a quantity of motion into the univerſal matter, maintains it therein, and is able, not only to ſet bounds to the raging ſea, but ſo to curb and moderate thoſe ſtupendouſly rapid motions of the mundane globes and intercurrent fluids, that neither the unwieldineſs of their bulk, nor the celerity of their motions, have made them fly out, for many ages. And if the firmament itſelf, whoſe motion, in the vulgar hypotheſis, is by much the moſt rapid in the world, fail of exactly completing its revolution in four and twenty hours; that retardation is ſo regulated, that ſince *Hipparchus*'s time, who lived above 2000 years ago, the firſt ſtar in *Aries*, which was then near the beginning of it, is not yet come to the laſt degree of that ſign.

The wiſdom of God differently expreſs'd. The wiſdom of God is expreſs'd in two different manners. Sometimes it is ſo manifeſtly diſplay'd in familiar objects, that even ſuperficial ſpectators may take notice of it. But there are many other things, wherein the "treaſures of wiſdom and knowledge" may be ſaid to lie deep.

In the various contrivances of animal bodies. The contrivance of every animal, and eſpecially of a human body, is ſo curious and exquiſite, that 'tis almoſt impoſſible for any one, who has not ſeen a diſſection well made, and anatomically conſider'd, to conceive how much excellent workmanſhip is diſplay'd in that admirable engine. Nay, the meaneſt living creatures of God's making, are far more wiſely contrived, than the moſt excellent pieces of workmanſhip that human heads and hands can boaſt. No watch or clock is any way comparable, for exquiſiteneſs of mechaniſm, to the body of an aſs or a frog.

But God's wiſdom is recommended, as well by the variety, and conſequently the number of the kinds of living creatures, as by the fabric of each of them in particular. The skill of human artiſts is, for the moſt part, limited to one, or to a few ſorts of contrivances; whilſt the great author of nature has not only created four principal ſorts of living engines, beaſts, birds, fiſhes, and reptiles; which differ exceedingly from one another, as the ſeveral regions, or ſtages, where they were to act their parts, required they ſhould; but under each of theſe comprehenſive genus's are compriz'd many ſubordinate ſpecies of animals, which differ exceedingly from others of the ſame kind, according to the exigency of their particular natures; as the hog and the hare, the parrot and the batt, the whale and the ſtar-fiſh, &c.

And what greatly enhances the excellent contrivances to be met with in theſe automata, is the ſymmetry of all the various parts whereof each of them conſiſts. For tho' an animal, conſider'd in his ſtate of intireneſs, is juſtly look'd upon as one engine; yet, really, this whole machine is a complex thing, made up of ſeveral parts, which, conſider'd ſeparately, may paſs, each of them, for a ſubordinate engine, excellently fitted for ſome particular uſe. It ought, therefore, highly to recommend the wiſdom of the great "former of all things," that he has ſo framed each particular part of animals, as not to let the skill beſtow'd on that, render the other leſs perfect: which manifeſts, that this great artiſt had the whole fabric under his eye at once; and did, at one view, behold all that was beſt to be done, in order to compleat the whole animal, and all its parts, at once: whilſt many excellent artificers, who can make a ſingle engine very compleat, may not be able to make it a commodious part of an aggregate of engines. Thus, tho' ſeveral can make pendulum-clocks go very regularly on ſhore, they cannot yet fit them to meaſure time, exactly, in a ſhip, at ſea.

But how much more wonderful than the ſtructure of the grown body, muſt be the contrivance of the *Semen animatum*; ſince all the future parts, the functions, and many actions of the animal to be produced, muſt be

PHYSICS. durably delineated, and couch'd in a little portion of matter, that ſeems homogeneous, and is, unqueſtionably, fluid? And what much increaſes the wonder, is, that one of theſe latent impreſſions, or powers, namely, the prolific, is to lie dormant, perhaps, above thirty or forty years, and then to be able to produce many more ſuch engines, as is the animal itſelf.

In the mutual uſefulneſs of his productions to each other.

Another way whereby God manifeſts his wiſdom in his corporeal creatures, is their mutual uſefulneſs to one another, in a relation either of dependence, or co-ordination. This ſerviceableneſs may be conſider'd, either as the parts of the animal have relation to one another, and to the whole body they make up; or, as intire, and diſtinct bodies, which have a regard to, or dependence on each other. To the firſt ſort of utility belong the offices of the parts of the body, which ſome of them exerciſe for the good of the whole; as, the ſtomach for concocting the aliment; the brain for ſupplying animal ſpirits; the kidneys to ſeparate the ſuperfluous ſerum of the blood. There are, alſo, many other particular parts that have a great ſubſerviency to one another. And ſeveral conſents of parts, and utilities, that accrue from one to the other, are farther diſcovered by diſeaſes; which, primarily affecting one part, or member of the body, diſcover that another has a dependance on it, or a particular relation to it. To the ſecond kind of utility belong thoſe parts that diſcriminate the ſexes of animals; which have ſuch a relation one to another in the male, and the female, that 'tis obvious they were made in order to propagate the ſpecies.

It were endleſs to obſerve, the wiſdom and goodneſs manifeſt in the works of creation; the ſituation of the ſun, its motion in the ecliptic, trade-winds, ſtated rains, *&c.* with regard to the welfare of men, and other animals. Thus, too, we ſee, that, according to the uſual courſe of nature, lambs, kids, *&c.* are brought into the world at the ſpring of the year, when tender graſs, and other nutritive plants, are provided for their food. The like may be obſerved in the production of ſilk-worms, whoſe eggs are hatch'd when mulberry-trees begin to bud; whereon theſe inſects are to feed; the aliment being tender whilſt the worms themſelves are ſo; and growing more ſtrong and ſubſtantial, as the inſects increaſe in vigour and bulk.

And in forming and governing other ſyſtems beſides the ſolar.

I have hitherto taken notice only of the productions of God's power and wiſdom obſerved in the viſible world. But, perhaps, as the ſun, the neareſt fixed ſtar to us, has a whole ſyſtem of planets that move about him; ſo ſome of the other fixed ſtars may be centres of other ſyſtems of celeſtial globes: ſince we ſee, that even ſome planets, determined by aſtronomers to be much inferior in bigneſs to the fixed ſtars, have other globes, that do, as it were, depend on them, and move about them; the earth has the moon for its attendant; *Saturn* is not unattended; and *Jupiter* has no leſs than four ſatellites. But none of theſe ſecondary planets, that move about *Saturn* and *Jupiter*, are viſible to the naked eye; and were, therefore, unknown to the aſtronomers who liv'd before the invention of teleſcopes. Now, in caſe there are other mundane ſyſtems, beſides this viſible one of ours; I think it may be, probably, ſuppoſed, that

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God has given peculiar, and admirable inſtances of his inexhauſted wiſdom in the contrivance and government of ſyſtems, that, for ought we know, may be framed and managed in a manner quite different from what is obſerved in that part of the univerſe known to us. Physics.

However, we need not fly to imaginary ultra-mundane ſpaces, to be convinced, that the effects of the power and wiſdom of God are worthy of their cauſes, and not adequately underſtood by us; if, with ſufficient attention, we conſider that innumerable multitude, and unſpeakable variety of bodies, that make up this vaſt univerſe. For there being among theſe, a ſtupendous number, that may be juſtly look'd upon as ſo many diſtinct engines, and many of them very complex; to know that all theſe, with the reſt of the mundane matter, are every moment ſuſtain'd, guided, and govern'd, according to their reſpective natures, and with an exact regard to the general laws of the univerſe; to know that there is a Being, who, every where, manages all things, without either aberration, or intermiſſion; is a thing, that if we attentively reflect on, ought to produce in us great wonder, and adoration.

The *Epicureans* of old did, with ſome colour of reaſon, urge againſt the belief of a divine providence, that 'tis incredible the gods ſhould be ſufficient for ſuch differing and diſtracting employments, as, according to the exigencies of nature, to make the ſun to ſhine in one place; the rain to ſhower down in another; the winds to blow in a third; the lightning to flaſh in a fourth; the thunder-bolts to fall in a fifth; and other bodies to act and ſuffer, according to their reſpective natures. We, therefore, who, upon good grounds, believe God really does what they thought impoſſible, are much wanting in our duty, if we do not admire an all-pervading wiſdom, that reaches to the utmoſt extent of the univerſe; and, with eaſe, actually performs what theſe philoſophers profeſs'd they could not ſo much as conceive.

Still greater inſtances of power and wiſdom, in the formation and government of immaterial Beings.

We have ſeen God's wiſdom and power in his corporeal works; but ſome of the divine perfections could not be ſo well expreſs'd, or copied, upon corporeal creatures, as upon the rational and immaterial ſoul of man, and other intellectual Beings: as the picture of a plain ſimple thing is not capable of receiving, or containing, ſo much of an excellent painter's skill, as he could exhibit in a piece wherein the paſſions of the mind, and the laws of optics, &c. may be fully expreſs'd. And it may well be preſumed, that if we were as familiarly acquainted with God's incorporeal creatures, as with his viſible ones; we ſhould perceive, that as ſpirits are incomparably more noble than bodies; ſo the divine wiſdom employ'd in the government and conduct of them, is more glorious than that which we juſtly admire in the conduct of his corporeal works. And, indeed, let a portion of matter be ever ſo fine, and ever ſo well contrived, it will not be more than an engine deſtitute of underſtanding and will; and whoſe excellency, as well as its diſtinction from other bodies, even the groſſeſt, and moſt imperfect, can conſiſt but in mechanical properties; which neither excite themſelves into motion, nor regulate and ſtop the mo-

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Physics. tion they once are in: whereas, true ſpirits, or immaterial ſubſtances, have, by God's appointment, belonging to their nature, underſtanding, will, and an internal principle, both of acting, and of arbitrarily ceaſing from action. And tho' God, as the ſole creator of all ſubſtances, has, and may exerciſe, an abſolute dominion over all his creatures, as well immaterial as corporeal; yet, ſince he thinks fit to govern ſpirits according to the nature he has given them; to create ſuch intelligent, free, and powerful Beings, as good and bad angels; and to govern them on ſuch terms, as effectually to make them inſtruments of his glory, which multitudes of them ſubtily and obſtinately oppoſe; requires a wiſdom and providence tranſcending any that can be diſplay'd in the formation and management of merely corporeal Beings. For inanimate engines may be contrived, to act as we pleaſe; whilſt angels, and human ſouls, are endow'd with a freedom of acting, in moſt caſes, as they pleaſe themſelves. 'Tis far eaſier for a watch-maker to regulate the motions of a watch, than the affections and actions of his ſon.

Angels, whether good or bad, are very intelligent and active Beings; and each of them is endow'd with an intellect capable of numberleſs notions, and degrees, or variations of knowledge; with a will capable of no leſs numerous acts; of having various influences upon the underſtanding, and of being variouſly affected by the dictates of it. So that each particular angel, being ſucceſſively capable of ſo many differing moral ſtates, may be look'd upon, as, in a manner, a diſtinct ſpecies of the intellectual kind. And, the government of one dæmon, may be as difficult a work, and, conſequently, may as much declare the wiſdom and power of God, as the government of a whole ſpecies of inanimate bodies, whoſe nature determines them to a ſtrict conformity to thoſe primordial laws of motion, once ſettled by the great creator; and, from which, they have no wills of their own to make them ſwerve.

The ſcripture tells us, that, in the œconomy of man's ſalvation, there is ſo much of the "manifold wiſdom of God" expreſs'd, that the angels themſelves deſire to pry into the myſteries of it. When our ſaviour, having told his apoſtles, that the day and hour of his future coming was not then known to any, ſubjoins, no not to the angels of heaven, he ſufficiently intimates them to be endow'd with excellent knowledge, ſuperior to that of men: which, perhaps, may be one of the reaſons why the ſcripture ſtyles them "angels of light." It alſo teaches us, that the good angels are vaſtly numerous; and that, as they are of differing orders, God aſſigns them very differing, and important employments, both in heaven, and on earth; and, ſometimes, ſuch as oblige them, in diſcharge of their reſpective truſts, to endeavour the carrying on of interfering deſigns. The ſame ſcripture, by ſpeaking of the devil, and his angels, and of the "great dragon, that drew down with his tail the third part of the ſtars from heaven to earth;" and, by mentioning a whole legion of devils that poſſeſſed a ſingle man, *&c.* gives us ground to conclude, that there is a political government in the kingdom of darkneſs; that the monarch of it is exceeding powerful,

whence

whence he is ftyled the prince of this world; and fome of his officers have the titles of principalities, powers, rulers of the darknefs of this world, &c. that the fubjects of it are exceeding numerous; that they are defperate enemies to God and men; that they are very falfe and crafty; and that their malice is active, reftlefs, and great. Thefe things being taught us in the fcripture itfelf, we may rationally fuppofe, that if we were quick-fighted enough to difcern the methods of the divine wifdom in the government of the angelical, and of the diabolical worlds; we fhould be ravifh'd into admiration how fuch intelligent, free, powerful, and immortal agents, fhould, without violence offer'd to their natures, be made, in various manners, to confpire to fulfil the laws, or, at leaft, accomplifh the ends of that great theocracy, which not only reaches to all kinds of bodies, but comprizes the whole creation, or the great aggregate of all the creatures of God. And, indeed, to make the voluntary, and, perhaps, the moft crafty actions of evil men, and of evil fpirits, fubfervient to his wife, and juft ends, no lefs recommends the wifdom of God, than it would the skill of a pilot, to contrive and fteer a fhip, fo as to fail to the defigned port, not only with a fide-wind, but with one that was quite contrary, and tempeftuous.

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At that great decretory day, when the whole off-fpring of *Adam* fhall, by the loud voice and trumpet of the arch-angel, be call'd together, from the remoteft ages and moft diftant climates in the world; when the fal'n angels, and all the human actors that ever lived, fhall appear upon the ftage at once; "when the dead fhall be raifed, and the books fhall be open'd:" then the wifdom of God will fhine in its meridian luftre, and full fplendor; and not only the occurrences which relate to the lives and actions of particular perfons, or of private families, and other lefs focieties of men, will be there found not to have been overlook'd by the divine providence; but the fates of kingdoms and commonwealths, and the revolutions of nations and of empires, will appear to have been order'd and over-ruled by an incomparable wifdom. And thofe great politicians, who thought to out-wit providence, by their refined fubtilties, fhall find themfelves "taken in their own craftinefs;" fhall have their deepeft "counfels turn'd into foolifhnefs;" and not be able to keep the amazed world from difcovering, that whilft they thought they moft craftily purfu'd their own ends, they really accomplifh'd thofe of God. And the fubtile hypocrites, who thought to make pretended religion the inftrument of their fecular defigns, fhall find thofe defigns defeated, and made truly fubfervient to that advancement of religion, which they, in reality, never aim'd at.

To employ, and keep in order, a very complicated engine, tho' all the parts of it be inanimate, and deftitute of purpofes and ends of their own, is juftly counted a piece of skill. And this task is more difficult, and, confequently, recommends the conduct of the artift, in proportion to the intricacy of the ftructure, and the number of pieces whereof the engine confifts. How aftonifhing, then, will appear that wifdom and providence, which is able to guide, and over-rule many thoufand millions of engines, endow'd with wills, fo as to make them all be found, in the final iffue of things, ſubſervient to purpoſes worthy of divine providence, holineſs, juſtice, goodneſs?

In ſhort, when all the actors ſhall appear at once upon the ſtage; when all diſguizes ſhall be ſtript off, all intrigues diſcovered, all hearts and deſigns laid open; then to find, that this whole amazing opera, that has been acting upon the face of the earth, from the beginning to the end of time, has been ſo contrived, and carried on, by the great author of the world, and of men, that their innumerably various actions, and croſs-deſigns, are brought to conſpire to the accompliſhment of a plot worthy of God; will appear an effect of ſo vaſt, and ſo all-pervading a wiſdom, that human intellects will with admiration confeſs, nothing but a divine omniſcience could compaſs.

Great inſtances of wiſdom in the redemption of man.

In the redemption of mankind, more of the divine attributes, than are commonly taken notice of, have their diſtinct agencies; and their co-operation is ſo admirably directed by the divine wiſdom, that an apoſtle might very juſtly call it the "great myſtery of godlineſs."

But many divines have largely treated of this ſubject; tho' I doubt whether moſt of them have not been more happy in avoiding errors about it, than ſucceſsful in unveiling the myſteries couch'd in it. There are, in the great work of man's redemption, ſome characters and footſteps of the divine wiſdom ſo conſpicuous and refulgent, that a believer, of mean parts, may eaſily diſcern them. But there are alſo, in this ſublime and comprehenſive work, ſome "depths of God," and ſo much of "the wiſdom of God in a myſtery," that I cannot think it eaſy to have a mental eye, ſo enlighten'd, and ſo piercing, as to treat largely and worthily of ſo vaſt and abſtruſe a ſubject. And, indeed, a man muſt know much of the nature of ſpirits in general, and even of the father of them, God himſelf; of the intellect, will, &c. of the ſoul of man; of the ſtate of *Adam* in paradiſe, and of the influence of his fall upon his poſterity; of the natural, or arbitrary vindictive juſtice of God; of the grounds, and ends of God's inflicting puniſhments; of the admirable, and unparallel'd perſon of Chriſt; of thoſe qualifications, and offices, that are required to fit him for being a redeemer; of the nature of covenants, and the conditions of thoſe which God vouchſafed to make with man; of the divine decrees, with regard to man's final ſtate; of the ſecret, and powerful operations of grace upon the mind, and the manner by which the Spirit of God works upon the ſouls of men, which he converts, and brings, by ſanctification to glory; in ſhort, there are ſo many points, moſt of them of difficult ſpeculation, that are fit to be diſcuſs'd by him, who would ſolidly, and fully treat of the world's redemption by Chriſt; that, when I reflect on them, I am ready to exclaim with St. *Paul*, "who is ſufficient for theſe things?" And, I am ſo far from wondering, that the generality of divines, and other writers on this ſubject, have not fully diſplay'd the wiſdom which God has expreſs'd in this great work, that to have been able to accompliſh it in ſo admirable a way, as God has actually contrived, and made choice of; is one of the chief reaſons of my admiration of the wiſdom itſelf. And, I am perſuaded, that

that for God to reconcile his inflexible juſtice, his exuberant mercy, and all thoſe other attributes that ſeem'd to claſh, inevitably, about the deſign'd ſalvation of men, and make them co-operate to it; is a ſtupendous manifeſtation of wiſdom: there being no propoſition in *Diophantus*, or *Apollonius*, in algebra, or in geometry, near ſo difficult to be ſolv'd, or that requires a greater number of proportions and congruities, to be at once attended to, and made ſubſervient to the ſame ends; as that great problem, propounded by God's infinite goodneſs, to his divine wiſdom; the redemption of loſt and perverſe mankind, upon the terms declared in the goſpel: which are admirably fitted to promote, at once, God's glory, and man's felicity.

The immenſe difference between the creator and his creatures.

We have here, and that very imperfectly, only ſpoke to two of God's attributes, his wiſdom, and his power: tho' there are many others wherewith we are acquainted; and, perhaps, ſtill more whereof we are ignorant. Now, the natural and genuine reſult of all theſe divine perfections, muſt be a moſt glorious majeſty, that requires the moſt lowly and proſtrate veneration of all his intelligent works. And, accordingly, the angels, of all his mere creatures the moſt excellent and knowing, are repreſented in the ſcripture, as aſſiduouſly employing themſelves in obeying and ſerving, in praiſing and adoring the divine majeſty, with the utmoſt lowlineſs and ſubmiſſion.

This profound reſpect of the angels, is not to be wonder'd at; ſince, where eſteem ſprings not from ignorance, but knowledge; the greater the ability and opportunities are of having the knowledge clear and heighten'd, the greater veneration muſt be produced in an intelligent Being for the object admired; whoſe perfections are here ſuch as even an angelical intellect cannot fully reach: for, as a line, by being ever ſo much extended in length, cannot grow a ſurface; ſo neither can created perfections be, by any ideas, magnify'd into divine ones. And, indeed, ſpeaking in general, the creatures are but ſhadowy, and arbitrary pictures of the great creator; of many of whoſe perfections, tho' they have ſome marks; yet they are ſuch, as rather give the intellect riſe and occaſion to take notice of, and contemplate the divine originals, than afford it true images of them. The awful reverence paid to the ſupreme Being by thoſe excellent ſpirits, who "are greater in power and might than we," ought to admoniſh us of the ecſtatic reſpect we owe him; and teach us, that whenever we ſpeak either to God, or of him, we ought to be inwardly affected with the unmeaſurable diſtance there is between a moſt perfect and omnipotent creator, and a mere impotent creature.

The ſuperiority of the divine knowledge to that of man.

The diſtance betwixt the infinite creator and the creatures, which are but the limited and arbitrary productions of his power and will, is ſo vaſt, that all the divine attributes, or perfections, by unmeaſurable intervals tranſcend thoſe faint reſemblances of them, that he has been pleaſed to impreſs, either upon other creatures, or upon men. God's nature is ſo peculiar and excellent, that there are qualities, which, tho' high virtues in men, cannot belong to God, or be aſcribed to him without derogation. Nay, there are ſome virtues that belong to man himſelf only in his mortal ſtate.

 But,

Physics. But, whatever excellencies there be, that are ſimply and abſolutely ſuch, and ſo may, without diſparagement to his matchleſs nature, be aſcribed to God, we may be ſure that he poſſeſſes them; ſince he is the original author of all the degrees, or reſemblances, we men have of any of them. The pſalmiſt's reaſoning is good. "He that planted the ear, ſhall he not "hear? He that formed the eye, ſhall not he ſee? He that teacheth "man knowledge, ſhall not he know?" Since all the perfections communicated to, or to be found in the creatures, being emanations of the divine excellencies, belong as much to God, as, in a bright day, all the luminous rays found in the air, belong to the ſun. The vaſt difference, then, between the perfections of the great creator, and thoſe that are analogous to them in the creatures, reaches to all the perfections to be found in both: but the human underſtanding, as it values itſelf upon nothing more than wiſdom and knowledge; ſo there is nothing that it eſteems, and reverences, more in other Beings, and is leſs willing to acknowledge itſelf ſurpaſs'd in, than theſe. Now, 'tis certain, that God knows innumerable things, with which we are altogether unacquainted: he cannot but know all the creatures he has made, whether viſible or inviſible, corporeal or immaterial; and what he has enabled them to do. Nay, ſince he cannot but know the extent of his own infinite power, he cannot but know numberleſs things, as poſſible, that he has not yet made, or, perhaps, ever will make. He, alſo, knows thoſe things whereof we men have ſome knowledge, in a manner, or degree, peculiar to himſelf. As, what we know but in part, he knows fully; what we know but dimly, he knows clearly; and what we know but by fallible mediums, he knows moſt certainly.

But the great prerogative of God's knowledge, is, that he perfectly knows himſelf; that knowledge being not only too wonderful for man, but beyond the reach of an angelical intellect: ſince, fully to comprehend the infinite nature of God, no leſs than an infinite underſtanding is requiſite. And for the works of God, even in thoſe that are purely corporeal, our knowledge is incomparably inferior to his. For tho' ſome modern philoſophers have made ingenious attempts to explain the nature of things corporeal; yet their explanations generally ſuppoſe the preſent fabric of the world, and the laws of motion ſettled in it. But God knows, particularly, both why, and how the univerſal matter was firſt contrived into this admirable univerſe, rather than a world of any other of the numberleſs conſtructions he could have given it; and both why thoſe laws of motion, rather than others, were eſtabliſhed; and how ſenſeleſs matter, to whoſe nature motion does not at all belong, comes to be put into motion, and qualify'd to transfer it, according to determinate rules, which itſelf cannot underſtand. But when we come to conſider the particular, and more elaborate works of nature; ſuch as the ſeeds, or eggs, of living creatures, *&c.* the ingenuous confeſs, and the confident betray their ignorance. 'Tis likely, that we men know ourſelves better than what is without us: yet how ignorant we are at home, if the endleſs diſputes of *Ariſtotle*, and his commentators, about the human ſoul, and of phyſicians and anatomiſts,

anatomists, about the mechanism of the human body, were not sufficient to manifest, 'twere easy to shew, by the very conditions of the union of the soul and body: which, being settled, at first, by God's arbitrary institution; and having nothing in all nature parallel to them; the manner and terms of that strange union, is a riddle to philosophers; but must needs be clearly known to him who alone instituted it, and preserves it. There are several advantages of the divine knowledge, above that of man. For, we can perceive, and sufficiently attend but to few things at once; but God's knowledge reaches, at once, to all that he can know; his penetrating eyes pierce quite thorough the whole creation at one glance; and "there is no "creature that is not manifest in his sight." He always sees incomparably more objects at one view, than the sun himself, endued with sight, could do. For God beholds, at once, all that every one of his creatures, in the vast universe, either does or thinks. The knowledge of God is, also, not a progressive, or discursive thing, like that acquir'd by our reasoning; but an intuitive knowledge. Men, by reason of the limitedness and imperfections of their understandings, are obliged to make the notice they have of one thing, a step and help to acquire that of another, less known: but God, whose knowledge, as well as his other attributes, is infinitely perfect, knows every thing in itself; and, all things being equally known to him, he can, by looking into himself, there see every thing that is knowable, most distinctly, yet all at once. But, further, God knows the most secret thoughts and intentions of men; whence he is call'd the "searcher of all hearts;" nay, he knows mens "thoughts afar off." And, by the way, how imperfectly must mere philosophers know God, since they know him but by his works; and know his works themselves but very imperfectly? Another conspicuous prerogative of the divine knowledge, is the prescience of future contingencies, that depend upon the determinations and actions of free agents. For we men are so far from being able to stretch our knowledge to the discovery of such events, that the greatest scholars in vain have try'd to discover how God himself can foreknow them; and, therefore, too many, even among christians, deny that he can; tho' by several accomplish'd predictions, recorded in scripture, it manifestly appears, that he does.

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The obligations men are under to venerate, and contemplate God.

When I consider the transcendent excellency, and the numerous prerogatives of the Deity, I cannot without wonder and concern observe, that rational men, professing christianity, should wilfully neglect to acquire, or reflect on, those notices that are apt to increase their knowledge of God, and consequently their veneration for him. To aspire to a farther knowledge of God, that we may the better adore him, is a great part both of man's duty and his happiness. God, who has put into men an innate desire of knowledge, and a faculty to distinguish the degrees of excellency in different notices; and to relish those most, that best deserve it; and has made it his duty to search and inquire after God, and to love him above all things; would not have done this, if he had not known that those who make a right use of their faculties, must find him to be the noblest object

of

PHYSICS. of the underſtanding; and that which moſt merits their wonder and veneration. And, indeed, what can be more ſuitable to a rational creature, than to employ reaſon to contemplate that divine Being, who is both the author of its reaſon, and the nobleſt object, about which it can poſſibly be employ'd? The knowledge of ſome dead language, or ſome old ruſty medal, or the opinions and cuſtoms of ſome nations or ſects, that did not, perhaps, reaſon or live any better than we do now, are thought worthy of curioſity, and even of the laborious induſtry of learned men; and the ſtudy of things merely corporeal, gains men the honourable title of philoſophers. But whatever theſe objects of inquiry be, in themſelves, 'tis certain the greateſt diſcoveries we can make of them, are but trifles, in compariſon of the excellency of the knowledge of God; which as much ſurpaſſes that of his works, as he himſelf does them. And 'tis the prerogative of his nature, to be infinitely above all that he has made; whether we contemplate the works of nature, or thoſe of art; the former whereof are under another name, his more immediate works; and the others, the effects of one of his works; and, by conſequence, originally his. And tho' it be moſt true, that God has been pleaſed to ſtamp on the corporeal world ſuch impreſſes of his power, wiſdom, and goodneſs, as have juſtly exacted the admiration even of philoſophers; yet the great author of the world is, himſelf, incomparably ſuperior to all his workmanſhip: ſo that, tho' he could have made, and always will be able to make, creatures more perfect than thoſe he has made, by infinite degrees; yet the prerogative of his nature will keep him, neceſſarily, ſuperior to the moſt excellent creatures he can make; ſince the very condition of a creature hinders it from being ſelf-exiſtent and independent. 'Tis therefore, methinks, a ſad thing, that we men ſhould grudge to ſpend now and then a few hours in the contemplation and internal worſhip of that moſt glorious and perfect Being, who continually employs the devotion of angels themſelves.

I know 'tis ſuppoſed a dangerous thing, to be inquiſitive about the nature of God. But the ſecret things of God, which are to be left to himſelf, ſeem to be his unrevealed purpoſes and decrees, and his moſt abſtruſe eſſence or ſubſtance; the ſcrutiny whereof, I readily acknowledge not to belong to us. And I think there is a great difference between contemplating God out of a bold curioſity, merely to know ſomewhat that is not common of him; and doing it out of an humble deſire, by a farther knowledge, to heighten our reverence and devotion towards him. 'Tis an effect of arrogance to endeavour, or ſo much as hope, to comprehend the divine perfections, ſo as to leave nothing in them unknown; but to aſpire to know them farther, that they may proportionably appear more admirable and lovely in our eyes, is not only an excuſable, but a laudable curioſity. The ſcripture, in one place, exhorts us " to grow" not only " in grace," but " in the knowledge of Chriſt;" and in another, " to add to our virtue knowledge." And that we may aſpire to great degrees of knowledge, even as to thoſe ſupernatural objects we cannot adequately know, appears from St. *Paul*, who prays that his *Epheſians*, and all true chriſtians, may be able

to

to comprehend what is "the breadth, and length, and depth, and height, and to know the love of Chriſt, which, ſays he, paſſeth knowledge." Suppoſing it then lawful to contemplate God, not with deſign to pry into his decrees and purpoſes, nor to dogmatize in controverted points about his nature and attributes; but to excite in ourſelves the ſentiments which his indiſputable perfections are, by a more attentive view, qualified to produce; I take the devout contemplation of God, beſides other great advantages that it brings the mind, to be one of the moſt delightful exerciſes the ſoul is capable of, on this ſide heaven. 'Tis generally acknowledg'd, that admiration is one of the moſt pleaſing affections of the mind; which ſometimes, when the object deſerves it, is ſo poſſeſs'd thereby, as to forget all other things, or leave them unregarded. Now, the pleaſure that admiration gives, being uſually proportionate to the uncommon nature and endearing circumſtances of the thing admired; how can any admiration afford ſuch a contentment, as that which has God himſelf for its object? The wonder produced in us by an humble and attentive contemplation of God, has two principal advantages, above the admiration we have for any of his works, or of our own. For, firſt, when we admire corporeal things, how noble and precious ſoever, the contentment that accompanies our wonder, is allay'd by a kind of ſecret reproach, grounded on that very wonder; ſince it argues a great imperfection in our underſtandings, to be unacquainted with things that are but creatures, as well as we; and, what is worſe, of a nature much inferior to ours: whilſt 'tis no diſparagement, for a human intellect to be poſſeſs'd with wonder, tho' heighten'd to amazement, or aſtoniſhment, by the contemplation of that moſt glorious and infinitely perfect Being, who muſt neceſſarily exceed the adequate comprehenſion of any created intellect. But there is a farther and much greater advantage of the admiration of God, above that of other things; for other objects having a bounded nature, and commonly but one thing to deſerve our wonder, our admiration of them is ſeldom laſting; and after a little familiarity, firſt languiſhes, and then ceaſes: but God is an object ſo very ſingular, whoſe perfections are ſo immenſe, that no diligence of conſidering him, can make him ceaſe to be admirable; and the more we know of him, the more reaſon we find to admire him. So that there may here be a perpetual viciſſitude of our happy acquirements of farther degrees of knowledge, and our eager deſires of new ones. God is ſo fertile an object, that we need not fear our admiration of him ſhould expire, for want of variety to keep it up. To the wonderful excellence of God, may be juſtly applied what *Ariſtotle* lays down as a definition of *Infinite*; *viz.* "'tis that, of which how much ſoever one takes, there ſtill remains more to be taken." If the intellect ſhould for ever make a farther progreſs in the knowledge of the wonders of the divine nature, attributes, and diſpenſations; yet it may ſtill make diſcoveries of freſh things worthy to be admired: as in an infinite ſeries, or row of aſcending numbers, tho' you may ſtill advance higher and higher; yet all that you can do by that progreſs, is to go farther and farther from the firſt term of the progreſſion, without ever reaching, or ſo much as approaching to an infinite number.

PHYSICS.

Numerous other arguments might be brought, to ſhew the immenſe inferiority of man's intellect to God.

The manner wherein this is to be done.

I think, then, that it becomes us to uſe an awful circumſpection ; not only when we make philoſophical inquiries about God, that is, when we preſume to diſcourſe of him ; but when we ſolemnly deſign to praiſe him ; for, 'tis one thing to ſay true things, and another to ſay things worthy of God. Our ideas of him, may be the beſt we are able to frame, and yet may far better expreſs the greatneſs of our veneration for him, than the immenſity of his perfection ; and, even thoſe notions that may be worthy of the moſt intelligent of men, will fall extremely ſhort of being worthy of the incomprehenſible God. The brighteſt and leaſt unlike idea we can frame of God, is infinitely more inferior, with regard to him, than a parhelion is with regard to the ſun. He has not, in my opinion, the trueſt veneration for God, who can ſet out his excellencies, and prerogatives, in the moſt high and pompous expreſſions ; but he who, willingly, has a deep and real ſenſe of the unmeaſurable inferiority of himſelf, and his beſt ideas, to the unbounded and unparallel'd perfections of his maker. And, as even our hymns and praiſes of the ſupreme Being, deſerve our bluſhes, and need his pardon ; what confuſion will, one day, cover the faces of thoſe, who not only ſpeak ſlightly and careleſly, but often, contemptuouſly, of that ſupreme, and infinitely perfect Being, to whom they owe thoſe very faculties which they ſo ungratefully, and impiouſly miſimploy? Indeed, ſuch tranſcendent excellencies, as are the divine, might juſtly diſcourage us from offering ſo much as to celebrate them, if infinite goodneſs were not one of them. I ſhall not, therefore, allow myſelf the preſumption of pretending to make a panegyric of God ; but, content my ſelf with an humble adoration of thoſe perfections, whereof my utmoſt praiſes would rather expreſs my own weakneſs, than their excellence ; ſince, of this ineffable object the higheſt things that can be expreſſed in words, muſt fall ſhort ; for words cannot expreſs him

STATICS.

THE

PREFACE.

STATICS is that part of universal mechanics, which considers the gravity of bodies, in all sorts of mediums; tho' 'tis, sometimes, taken for the same as particular mechanics, or the doctrine of motion in bodies, which depends upon their gravity. But, hydrostatics, in general, regards the weight of bodies in all manner of fluids; and, therefore, differs but little from statics, unless when bodies are weighed in vacuo; *or, where there is no sensible resistance. There is, also, a more limited signification of the word, hydrostatics; which restrains it, as its derivation implies, to the weighing of bodies in water; and 'tis hydrostatics in this sense, wherewith we are here principally concern'd: a doctrine which, before Mr.* Boyle *undertook to improve it, consisted chiefly in theory and speculation; having only been treated by mere mathematicians: but, that noble philosopher soon reduced it to practice, and applied it to useful purposes. Some of its propositions, formerly demonstrated in a mathematical manner, he proved by the direct, obvious way of experiment; that is, by the proper way, or hydrostatically. He, also, made many new discoveries in this part of knowledge, which are highly useful in life, and teach us to examine the goodness of drugs, of metals, minerals, and other bodies, both solid and fluid.*

The first of the following pieces appears under the title of paradoxes; and, truly, the propositions it contains, were paradoxes, even to some mathematicians, at the time wherein they were published. Nor is this strange, since, as the learned Wolfius *observes, many persons taking gravity for a permanent power in matter, which must remain unalter'd, so long as it continues its state; and supposing fluids, whilst confin'd, and* *at rest, wholly unable to act upon bodies; no reason appears to them, why they should, as it were, take away a part of the gravity of the bodies immersed in them; and, much less, why they should, sometimes, throw them upwards with a great force. This was remarkably the case with that great scholar, Dr.* More; *who, to solve such an appearance, would fain have introduced a new, and an immaterial principle into physics. And this gave birth to another of the ensuing pieces; which, tho' it also relates to some experiments, that appear under the head of pneumatics, we thought fit to range intire, under this of statics; because, in strictness, 'tis wholly hydrostatical; taking that term in its larger sense; because we would not break in upon the order of the pneumatical pieces; and, lastly, because hydrostatics ought, always, to precede pneumatics; since the latter cannot be understood without a knowledge of the former.*

Hydro-

Hydroſtatical Paradoxes,

Proved and Illuſtrated by

EXPERIMENTS.

POSTULATA, & LEMMATA.

1. SUppoſe a tube, open at both ends, held, with one of them, perpendicularly, under water; the lower orifice may be conceiv'd to terminate in a plain parallel to the horizon, or the upper ſurface of the fluid.

2. All aſſignable equal portions of this ſurface, will be equally preſſed by the water perpendicularly incumbent thereon.

For the fluid being here ſuppoſed homogeneous, as to its gravity, and to ſtand, at the ſame height, upon all the parts of the imaginary plain; no equal part can poſſibly be more preſſed than another, in the ſame ſurface.

3. If any part of this imaginary plain, be preſſed with a greater weight, than another, the former will be either diſplaced, or depreſſed.

Thus, whilſt an heavy body ſinks in water, that part of the imaginary plain, contiguous to the lower part of the body, being preſſed by a greater weight than the other portion of the ſame ſurface, muſt needs give way, ſucceſſively, till the heavy body arrives at the bottom.

4. On the other hand, if any part of the imaginary ſurface be leſs preſſed upon than the remainder, it will, by the weight on the remainder, be impell'd upwards, till the preſſure there be equal to that upon the other parts of the ſame ſurface.

If this be doubted, the following experiment will prove it. If a cylindrical glaſs tube, open at both ends, be ſteadily held in a perpendicular poſture, with one of them immerſed, two or three inches below the ſurface of a proper quantity of water, in a glaſs veſſel; the ſurface of the water within-ſide the tube, will be nearly level with the ſurface that is without-ſide the ſame; becauſe the water, in both caſes, has a free communication.

STATICS. If, now, a convenient quantity of oil, be gently pour'd upon the water that is external to the tube, that within the tube, which we shall call internal, will gradually rise, and continue to do so proportionably; because the imaginary plain, cutting the immersed orifice of the tube, is every where pressed by an additional weight, except in the orifice itself: which pressure must necessarily be increas'd, as more oil is pour'd upon the external water; whilst a circle of our imaginary plain, equal to the lower annulus of the tube, is, by the sides thereof, guarded from the immediate weight of the oil; whence the external water, being more pressed than the internal, is, consequently, forced up thro' the tube, where there is the least resistance, till the cylinder of water within the tube, gravitates upon the subjacent part of the imaginary plain, equally with the sum of the water and oil, upon every other equal portion of the same surface.

5. The air is a ponderous body.

This has been shewn by several experiments; some whereof, indeed, are excepted to; but the following is unexceptionable, and was often repeated.

Having obtain'd a thin, and large glass bubble, with a slender stem, and gradually rarified the air it contain'd, as much as I conveniently could, by the flame of the lamp, whereat the bubble was blown; the stem, whilst the bubble remain'd exceeding hot, being nimbly put into the flame, thereby became hermetically sealed, in an instant. This glass we permitted to cool leisurely, and afterwards weigh'd it in a very exact balance; then carefully breaking off the sealed end, and preserving the fragments, we weigh'd the glass again, and found it considerably heavier than before; which could be owing to nothing but the natural air, that rushed into the bubble, upon breaking off its stem, whilst it contain'd air greatly expanded. For, the noise hereof, in entring, may be plainly heard by an attentive ear; and, as the rarifaction is here great, so the bubbles will sometimes break, by the bare force of the external air; and, lastly, if the sealed end of the glass be broke off, under water, that fluid will, by the pressure of the atmosphere, be forced into the bubble, like an artificial fountain, till it be about three quarters full: whence the weight of the air appears to be very considerable; since the fourth part of what the bubble would contain, remains therein not taken notice of by the balance. In one repetition of this experiment, I found the air fresh admitted into the bubble, weighed near $\frac{3}{4}$ of a grain, and its full content of water *906* grains; whence, if the re-admitted air only filled $\frac{3}{4}$ of the bubble, the whole air it contain'd, may be reasonably supposed at a grain. And thus the water weigh'd about *900* times as much as the air of the bubble; which, all things consider'd, comes near enough to some other trials, whereby we determin'd the specific gravity of air, to be to that of water, as 1 to 1000.

PARA-

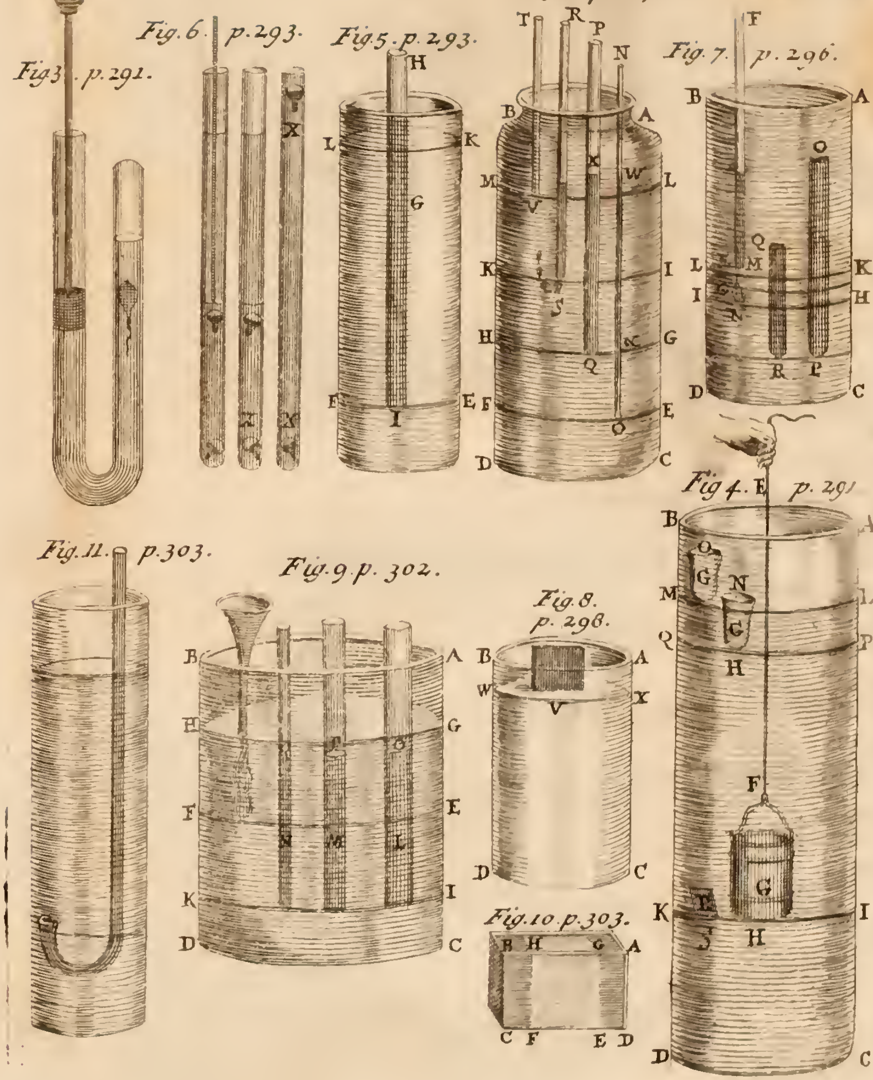
Fig.3. p.291.
Fig.6. p.293.
Fig.5. p.293.
Fig.2. p.287.
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Fig.11. p.303.
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Fig.10. p.303.

PARADOX I.

In all fluids, the upper parts gravitate on the lower.

Suppoſe one end of a ſmall, cylindrical, open glaſs tube, plunged into oil of turpentine, and that liquor to be raiſed, by ſuction, to a convenient height therein; when the lips being removed, and the upper orifice of the tube nimbly ſtopp'd with the finger, to prevent the fluid from falling back, imagine it thus placed perpendicularly in the glaſs A B C D *Fig.* 2. almoſt fill'd with water, ſo that the ſurface of the oil may ſtand ſomewhat higher than that of the water. This done, and the finger removed from the upper orifice of the tube, the oil will not fall out at the lower, but remain ſuſpended near its former altitude. But oil of turpentine, being an heavy fluid, has a tendency downwards; and as the lower orifice of the tube is open, it muſt neceſſarily fall out thereat, did not the preſſure of the water underneath ſuſtain it. No contrariety in the nature of theſe two liquors, can be ſuppoſed the cauſe of this phenomenon; for, if the finger be removed, before the pipe is ſufficiently immerſed, the oil will ſubſide till it becomes a balance to the water; but the reaſon of it is manifeſtly this. Suppoſe the plain wherein the extremity Q, of the pipe P Q, reſts, to be G H; if that part thereof whereon the oil reſts at Q, be equally preſſed by the incumbent cylinder of oil Q X, with the other parts of the ſame ſurface G H, by the water incumbent upon them; the part Q, can neither be depreſſed by the cylinder of oil Q X, nor raiſed by an equal preſſure of water upon the other parts of the plain G H. That this equilibrium, betwixt the oil and water, is juſtly aſſign'd for the cauſe of the phenomenon, appears farther, 1. From what is already obſerved, *viz.* that if the cylinder of oil reach much higher than the ſurface of the water, the oil will deſcend: the reaſon whereof is, that the plain G H, being more charged at Q, than in any other part, it is there unable to make reſiſtance; whence it muſt neceſſarily be thruſt out of its place, by the deſcending oil. 2. Becauſe the oil will continue ſubſiding only till its ſurface becomes almoſt level with that of the water; when the part Q, is preſſed by the oil, equally with the other parts of the plain G H, by the water incumbent on them. 3. If, while the oil and water are balanced, the pipe be gently raiſed from Q to S, the oil will preponderate, and conſequently fall out in drops, which by the ſuperior ſpecific gravity of the water, will be buoy'd up, and ſo float on the ſurface thereof; and as the pipe is gradually elevated towards the ſurface L M, more and more of the oil will drop out: but if the tube be ſtop'd any where in its aſcent, at S, for inſtance, the oil will ceaſe to flow out: and, as at the plain I K, the preſſure of the water, by reaſon of its diminiſh'd depth L I, upon the other parts of the ſurface, is not near ſo great, as upon the plain G H; ſo the remaining cylinder of oil incumbent on S, is, by reaſon of its proportionable efflux, unable to preſs that part more forcibly than the others of the ſame plain I K, are preſs'd by the incumbent water. And, if the lower orifice be raiſed almoſt to V, or near the upper ſurface of the

water

STATICS. water L M, scarce any oil, for the same reason, will remain in the pipe T V. 4. But if, when the end of the tube rests in the plain G H, where the oil is a balance for the water, it be let down to O, the external water will cause the oil to ascend in the tube, the height of E G; so that the pipe will contain, besides a cylinder of oil α W, a shorter one of water α O; for, now a new plain E F, cuts the lower orifice of the tube, whilst that part of it at O, being, by the incumbent oil, pressed less than the others, by the incumbent water, the oil will be buoy'd up, till the water O α, and the cylinder of oil α W, taken together, gravitate as much upon O, as the rest of the incumbent water does upon the other equal parts of the same plain E F. Since, then, the oil, in this case, may be kept suspended at any point, under water, as at Q, being there in an equilibrium with the external fluid; since, when elevated, as from Q to S, it falls by its own gravity; and, since the deeper it is plunged, the greater weight and pressure are required in the cylinder of oil, to balance the pressure of the water; it follows, that the parts of the water incumbent on the plain G H, press that surface more than the plain I K, is pressed by the parts of the water contiguous to it; and consequently, that all the parts of the water which lie under the upper surface, are pressed by those which stand directly over them; as the upper parts of the oil press'd down the lower, whilst the pipe was drawn up from Q to S; so that the oil flow'd out at the orifice S: for since the lower parts of a liquor press proportionably to the height of the fluid, they must necessarily derive their force from the incumbent parts, which, consequently, gravitate upon them.

A glass bubble, about the bigness of a pullet's egg, was purposely blown at the flame of a lamp, with a long stem turn'd up at the end, that it might be the more conveniently broken off. This bubble being well heated, to rarify the air, and thereby drive out a large part of it, was presently sealed at the end; and, by the help of the figure of the stem, was, by a convenient piece of lead, sunk under water, the weight and glass being tied by a string to one scale of a good balance, in the other whereof was placed a counterpoise to the bubble, as it hung freely in the midst of the water. Then, with a pair of forceps, I carefully broke off the sealed end of the bubble, under water, so that no air appear'd to emerge, or escape thro' the water; but the liquor, by the weight of the atmosphere, sprung into the empty part of the bubble, and about half fill'd the whole cavity; whereupon the bubble immediately subsided, and made the scale, whereto 'twas fasten'd, require 4 drams, and 38 grains, to bring it horizontal. Then taking out the bubble, with the water it contain'd, we, by means of the flame of a candle, warily applied, drove out the water, which, otherwise, is not easily excluded at a very narrow stem, into a glass counterpois'd before-hand, and found it to weigh about 4 drams, and 30 grains, besides a little that remain'd in the bubble, and a small matter that might have been rarify'd into vapour; which, added to the piece of glass, broken off under water, and lost there, might very well amount to 7 or 8 grains. By which it appears, not only, that water has some weight, or gravitates in water,

water, but that it weighs very near, or altogether as much in water, as in the air. We repeated the experiment with another ſealed bubble as large as a great hen-egg, and with the like ſucceſs.

COROLLARY.

From hence it appears, why the ſurface X, of the oil in the pipe PQ, riſes a little above the level LM, of the external water. The ſlenderneſs of the pipe may, indeed, contribute hereto; but there would, otherwiſe, be an inequality. For, ſince oil of turpentine is ſpecifically, that is, bulk for bulk, lighter than water; and ſince an equilibrium between them is here conſtantly obſerv'd; the height of the oil incumbent upon Q, muſt, of neceſſity, be greater than that of the water incumbent on the other parts of the ſame plain GH, to make the preſſure on both ſides equal. And were the difference between the ſpecific gravities of theſe two liquors greater, the difference in the heights of their ſurfaces X and LM, would be greater alſo; as may be try'd, by ſubſtituting oil of tartar *per deliquium*, a heavy ſaline liquor, for water.

SCHOLIA.

1. What we have here ſaid, or ſhall hereafter ſay, of the properties of water, is applicable to all ponderous fluids, unleſs where cauſe of exception appears in particular caſes.

2. 'Twas before hinted, that in very ſmall glaſs pipes, many fluids (I except quick-ſilver) will appear with their ſurfaces higher than that of the liquor wherein they are immerſed; and that, as far as I have obſerv'd, in proportion to the ſmallneſs of the bore: but this being of ſmall conſequence in our preſent experiments, we ſhall take no farther notice thereof.

3. Notwithſtanding this inconvenience of ſmall tubes, we chuſe to make uſe thereof; becauſe thoſe of a wider bore are apt, upon any ſmall inequalities of preſſure, to ſuffer the oil and water to paſs by each other in their cavity, and ſo diſturb the experiment.

4. Common oil and water, or any two liquors that will not mix, may ſerve in moſt of our experiments. I made choice of oil of turpentine, becauſe 'tis light, thin, clear, colourleſs, eaſily obtainable, not apt to ſpot apparel, or greatly to adhere to porous bodies; and its offenſive ſcent is eaſily corrected by oil of rhodium, &c.

5. Oil of turpentine will corrode copper, whence by digeſting it upon the crude filings of that metal, a deep green liquor is obtainable; which uſed inſtead of the colourleſs oil, will render the diſtinction of the fluids more conſpicuous.

6. For the ſame purpoſe, I frequently employ a decoction of brazil, logwood, or the common red ink; which muſt be made very ſtrong and deep, otherwiſe their colour in a ſlender pipe will be ſcarce diſcernible.

7. As to the ſhape of the glaſs-veſſel, we need not be ſolicitous, tho' that of a wide-mouth'd jar be for ſome uſes the moſt convenient. The depth hereof, and the length of the tubes muſt be determin'd by the experiments to be made.

STATICS. 8. It ought to be no diſcouragement, if the oil of turpentine ſuck'd up, do not remain in the glaſs, upon the firſt or ſecond attempt; 'tis beſt to draw up more than is neceſſary, and afterwards to let in the air gradually between the orifice of the tube and the finger; which will ſuffer the liquor to deſcend to the height required.

9. Where a conſiderable difference is required between the two fluids made uſe of, oil of tartar *per deliquium*, as we ſaid, tinged with cochineal, may ſupply the place of water, and highly rectified ſpirit of wine that of oil of turpentine: for theſe two liquors, tho' they readily unite with water, will not eaſily mix with each other. Or a very ſtrong filtred ſolution of ſea-ſalt, may be ſubſtituted for water, occaſionally. I have ſometimes added oil of turpentine to ſpirit of wine and oil of tartar, and thereby obtain'd three liquors of different gravities, which will not eaſily mix; by plunging a pipe, with water in the bottom of it, below the ſurface of the loweſt of theſe liquors, and raiſing and depreſſing it in a proper manner, the experiment may be agreeably varied; but ſtill continue explicable by the principles we deliver.

10. This propoſition has been thought liable to the following objections. 1. If the upper parts of water gravitate upon the lower, the latter muſt be more denſe than the former. 2. Divers are ſenſible of no compreſſure under water. 3. Tender plants grow at the bottom of the ſea. 4. An immerſed heavy body is ſupported and drawn out with greater eaſe, than if no water were incumbent thereon; for it weighs leſs in water, than out of it. 5. A bucket of water is lighter when immerſed than before; and weighs leſs in water, than, when empty, out of it; therefore, the water in the bucket does not gravitate, either on the bucket, or the water below it: whence the ſchools have determin'd, that fluids never gravitate in their proper places. 6. Horſe-hairs preſerve any place aſſign'd them in water; and do not ſink by the incumbent weight thereof. 7. Were the propoſition true, all the lower parts of water would be in perpetual motion, and conſtantly expel the upper.

To anſwer each of theſe objections in their order. 1. If the component parts of water be perfectly hard and ſolid, the upper may preſs upon the lower without condenſing them; as would be the caſe in a heap of diamond duſt. And, poſſibly, the parts of water will not ſenſibly yield to ſo ſmall a weight as that of the incumbent fluid, at leaſt in thoſe heights whereat experiments are uſually made; for, in fact, water is not compreſſible by an ordinary force: nor would any abſurdity follow from our doctrine, were the lower parts of the ſea condenſed by the upper. 2. That divers are inſenſible of any compreſſure under water, proceeds from the ſurprizingly ſtrong reſiſtance of an animal body to an uniform force, at once applied to all the external parts thereof; but that, if a proportionable part of this preſſure was ſuſtain'd by one limb, and not by all the reſt at the ſame time, 'twould be greatly pain'd thereby, appears ſenſibly from covering the mouth of an open cylindrical veſſel, with the palm of the hand, whilſt applied like a receiver, to be exhauſted by the air-pump: for as ſoon as the included

included air is withdrawn from under your hand, it will be vehemently preſs'd from without, againſt the edge of the veſſel; as if ſome weight were laid upon the back of your hand, and forced it into the hollow of the cylinder. But that ſo vaſt a weight of water, as muſt compreſs a human body at the depth of near 100 feet below the ſurface of the ſea, (where, a diver told me, he had remain'd without pain) will not ſenſibly affect it, may appear from the following experiment. To a ſmall, cylindrical, glaſs ſyphon, ſealed at one end, we exactly fitted a rammer, by means of ſoft leather, ſo that neither air nor water could paſs between it and the ſides of the glaſs; then pouring in a proper quantity of water, and leaving ſome inches height of air above it, we let a young tadpole, the body of which animal is exceeding ſoft and tender, ſwim about in the glaſs; when a man forcing the rammer down the tube with his utmoſt ſtrength, the bulk of the tender creature ſeem'd ſomewhat diminiſh'd by the preſſure; however, it continued to move freely about in the water, and often aſcended to the top thereof; tho' the inſtrument ſtood perpendicular to the horizon: nor could we perceive that the animal ſuffer'd the leaſt injury hereby. This experiment we frequently repeated, with tadpoles of different ages; whence it may fairly be concluded, that the texture of animals is ſo ſtrong, that tho' water actually preſs on water, the body of a diver, at a great depth therein, ought not to be ſenſible of its weight: for the air being here compreſs'd into an eighth part of its natural dimenſions; the tadpole ſuſtain'd a preſſure of water equal to that of a cylinder of the ſame near 300 feet high. 3. That plants ſhoot up under water, is owing to the ſubjacent preſſure of the ſame; the force whereof, we ſee, is able, when they are detatch'd from their roots, to throw them up to the ſurface, notwithſtanding the direct weight of the incumbent fluid. 4. Tho' the water incumbent on a heavy body actually tends to depreſs it, yet that tendency may not cauſe it to ſink deeper; by reaſon of an equal or greater preſſure upon all the other parts of the ſame plain, wherein its bottom reſts; in which caſe, the hand that ſuſtains it will not feel the weight of the incumbent fluid, but be able to raiſe the body more eaſily here, than in the air: yet if the incumbent water be removed, as in the proof of our eleventh paradox, a great preſſure will appear to have been there ſuſtain'd by the body. 5. To paſs over that ſenſeleſs expreſſion of fluids not gravitating in their proper places, I thus ſolve the phenomenon of the immerſed bucket. Suppoſe A B C D the well, E F the rope whereby the bucket is ſuſpended under water, with its bottom reſting on the plain I K; if the bucket wholly conſiſt of wood ſpecifically lighter than water, it muſt be ſo far from weighing upon the hand at E, that its upper parts muſt needs riſe above the top of the water: but if this veſſel be a compoſition of wood and iron, it may be ſpecifically heavier than water; in which caſe, the hand that draws it out, will feel only that weight, whereby it exceeds an equal bulk of the fluid; the water in the cavity G, and ſo much as reſts thereon, being balanced by the preſſure of the other water above the plain I K: whence the bottom of the bucket is preſſed as forcibly upwards, as the

STATICS.

Fig. 3.

Fig. 4.

STATICS. weight of the incumbent water, in the bucket and above it, tends to depreſs it; ſo that the hand has no more to lift, than the exceſs of weight of the wood and iron of the bucket, above that of an equal bulk of water. And farther to ſhew the abſurdity of that ſcholaſtic doctrine, of water's not weighing in its own element, drawn from this experiment of the bucket, I perform'd the ſame thing, in water, with a ſmall box fill'd with butter, and ſome pieces of lead, to add to its weight; and I hope, it will not be pretended, that the butter and lead did not gravitate here, becauſe they were in their own element *. 6. That horſe-hairs, ſuppoſing them of the ſame ſpecific gravity with water, ſtand ſuſpended at any depth in that fluid, is eaſily accounted for upon our principles. Suppoſe the body R, to be of the ſame ſpecific gravity with water, it cannot preſs that part of the plain S, wherein it reſts, either more or leſs than an equal bulk of water; and conſequently, tho' the whole column that ſtands above it, directly preſſes thereon, that does not ſink it; being balanced by the weight of the other water above the plain IK; whence the body will neither be depreſs'd nor rais'd. And as this muſt be the caſe at what depth ſoever the body be immers'd, it muſt neceſſarily keep in the place aſſign'd it. And I once procured glaſs-bubbles ſo nicely poiſed, as to exhibit this phenomenon with a ſurprizing exactneſs. 7. If the laſt objection ſuppoſes the lower parts of water to be of any conſiderable bulk, 'tis already anſwer'd, when we ſhew'd that the body R, would retain its place aſſign'd, any where in the water. And unleſs we knew the particular magnitude and figure of the component particles of water, 'tis to no purpoſe to diſpute this point. I am, however, ſo far from thinking it abſurd, that the lower parts of water ſhould be in conſtant motion, that I take this to be the very property that conſtitutes a fluid; which, tho' not viſible, is eaſily diſcoverable by its effects. Thus, when ſalt is thrown into water, the parts near the bottom ſoon raiſe ſaline corpuſcles to the top, where they afterwards ſhew themſelves in ſmall, floating, cubical grains.

Fig. 4.

PARA-

* This matter cannot be ſet clearer than in the words of Sir *Iſaac Newton*; who obſerves, that bodies immerſed in fluids, have two kinds of gravity; the one true and abſolute, the other apparent, vulgar and relative. "Abſolute gravity (ſays "he) is the whole force wherewith a "body tends downwards; but the relative "and vulgar is that exceſs of gravity, "whereby a body tends downwards, more "than the fluid which ſurrounds it. By "the former kind, the parts of fluids, and "of all bodies, gravitate in their proper "places; and, by their joint weights, "compoſe the weight of the whole. For "every whole has weight, as is evident "in veſſels fill'd with liquids; and the "weight of the whole being equal to the "weights of all the parts, muſt of neceſ-"ſity be compoſed of them. But bodies, "by the latter kind of gravity, do not "gravitate in their own places; that is, "do not, when compared with one ano-"ther, pregravitate; but mutually hin-"dering each other's endeavour to de-"ſcend, they remain in their places, as "if they had no weight. Bodies in the air "which do not pregravitate, are thought "by the vulgar not to be heavy; but thoſe "which pregravitate, they judge to be "heavy, ſo far as the air does not ſupport "them: ſo that the weight of bodies among "the

PARADOX II.

A lighter fluid will gravitate upon a heavier.

If you ſuck up fair water in a ſlender glaſs pipe, to the height of three or four inches, nimbly ſtop the upper orifice with your finger, plunge the lower into a glaſs of oil of turpentine, till the ſurface of the oil in the veſſel riſe ſomewhat higher than that of the water in the tube, and then, laſtly, remove your finger; the water will not fall out, but continue ſuſpended by the preſſure of the oil; as appears evident from the foregoing propoſition, whereof this is only the converſe. And if either more oil be poured into the veſſel, or the tube be immerſed deeper therein, the water will be buoy'd up towards the top of the pipe; that is to ſay, a heavier fluid will be raiſed by a lighter. Since then, by the preceding propoſition, the liquor riſes in the pipe by the gravity of the external fluid, it follows, that a lighter liquor will gravitate upon a heavier.

COROLLARY.

As the ſurface of the oil in the tube, was always, in the former experiment, higher than that of the external water, ſo, here, the ſurface of the water in the pipe, will conſtantly be lower than that of the oil; becauſe in the plain E F, the cylinder of water I G, contain'd in the pipe I H, will, upon account of its greater gravity, preſs as much upon the part I, as the oil K I, I L, which is ſpecifically lighter than it, does upon the other parts of the ſame plain E F. *Fig. 5.*

SCHOLIA.

1. This ſecond paradox may be farther confirm'd from the common phenomena of glaſs bubbles, the explication whereof is as follows. The bubble X, conſiſting of glaſs, which is ſpecifically heavier than air, which is lighter than water, and of water it ſelf; whilſt the whole compoſition is lighter than an equal bulk of water, it will float; but if it grows heavier than ſo much water, it muſt, according to the laws of hydroſtatics, ſink. When, therefore, there happens any conſiderable preſſure upon the water wherein the bubble is generally immerſed; ſince glaſs is a ſolid body, and *Fig. 6.*

" the vulgar, is only the exceſs of their " real weight above that of the air. And, " therefore, they call thoſe things light, " which being leſs heavy than the air, " and yielding to its greater gravity, " mount upwards. But theſe bodies are " only comparatively light, not really ſo; " for they will deſcend *in vacuo.* Thus " bodies, which by reaſon of their greater " or leſs gravity, deſcend or aſcend in " water, are but comparatively and apparently heavy or light; and their comparative and apparent levity, is the exceſs or defect, whereby their real gravity either exceeds or falls ſhort of the " gravity of water. But whatever bodies neither deſcend by pregravitating, " nor aſcend by yielding to one that pregravitates; tho' they ſtill, by their real " weights, increaſe the weight of the " whole; yet comparatively, and in a " popular ſenſe, they do not weigh in " water." *Newton. Princip.* p. 264.

water

STATICS. water incompressable, but the air included in the bubble, elastic, and easily compressed; this will shrink, and so possessing less space than before, the contiguous water will succeed in its place; which being a body that is a thousand times specifically heavier than air, the bubble thereby becomes heavier than an equal bulk of water, and consequently sinks: but, upon the removal of this pressure, the pent-up air will, by its own spring, free it self from the intruding water; whence the composition of the bubble growing lighter than an equal bulk of water, it will, from the bottom, immediately emerge to the top.

2. Into a long glass pipe, sealed at one end, and open at the other, pour common water, till about half a yard thereof remains unfill'd. In the next place poise a glass bubble, with a slender neck, so that a small addition of weight will sink it; and place it on the surface of the water in the tube; then continue pouring oil of turpentine gently thereon, till it rises to a sufficient height; when the bubble will descend to the bottom, and there continue as long as the oil remains at that height above the water. The reason whereof is, that the oil, tho' lighter than water, gravitates upon that fluid below it, and thereby forces a part thereof into the open orifice in the neck of the bubble, which thence becomes heavier than an equal bulk of water, and consequently descends to the bottom; and the cause of this submersion, the pressure of the oil, continuing, it must necessarily remain there. In confirmation hereof, it may be added, that if the cylinder of oil be diminish'd, or taken away, the bubble will again ascend to the top of the water; because the water being relieved from the pressure of the oil, the air, by virtue of its own elasticity, recovers its former dimensions, and thereby renders the bubble as light as before.

3. The first paradox may, also, be confirmed by the following experiment. Having provided a tube and bubble, as just mention'd, pour water into the tube, to the height of 6 inches, then cast in the bubble, and keep it constantly plung'd below the surface, whilst more water is slowly pour'd into the tube, to a considerable height; when, the bubble, which before attempted to emerge, will, by the additional weight of the incumbent water, be depressed to the bottom of the tube, and there continue, whilst the same weight of water remains.

4. Tho' the weight of the incumbent water alone, here causes the bubble to sink, yet, if it were not kept immersed, a still greater weight of water would not depress it; because, being left to itself, it would rise with the fluid, and never permit the same to come sufficiently above it for that purpose. And that the weight of the incumbent water really sinks the bubble, and keeps it at the bottom, appears from taking away a sufficient part thereof; for, when that is done, the bubble will presently, and of itself, begin to float up towards the surface, from whence it may easily be made to subside, by pouring back the same water that was taken out.

5. The reason why oil of tartar *per deliquium*, was not here made use of, instead of fair water, is, that in such small pipes, as those employ'd about the first experiment, I found that liquor so ponderous, as to flow down

down into the oil of turpentine, whilst the oil passed upwards by it, along the other side of the tube: whence I am surprized, that M. *Paschal* should say, that if a tube above 14 feet long, with one orifice placed 14 feet under water, be full of quick-silver, it will not all run out thereat, tho' the top be exposed to the air, but stop at the height of one foot; because the moment it acquires in falling, must, probably cause the whole to come out. My own trials with quick-silver, and small tubes, with this more favourable circumstance of oil of turpentine, and oil of tartar, tempt me to suspect, that M. *Paschal* never made the experiment, at least, not in a tube so large as his scheme supposes. Experiments, that are only true in speculation, should be proposed as such, for they may often fail in practice. STATICS.

6. This second paradox, I am sensible, contradicts the current opinion of the schools, and even of many modern mathematicians, and writers in hydrostatics; but whether the cause of gravity be the trusion of any superior substance; or the magnetic attraction of the earth, *&c.* there is, in every heavy body, a constant tendency towards the centre of the earth, that cannot be destroy'd by the interposition of another heavier body, tho' its nearer approach towards the centre, may be hindred thereby; the latter, from its greater gravity, obtaining the lower place. But then, the lighter body tending downwards, must needs press upon the heavier that lies in its way; and jointly therewith, press upon whatsoever body supports them both, with a weight made up of their united gravities. Some men of learning are kept from acknowledging this truth, by finding that a lighter fluid, surrounded by a heavier, does not descend, but emerge to the top; whence they consider the former, not as a heavy body, but a light one. 'Tis true, in respect of the heavier liquor, the less heavy may be called light; yet it still retains its own absolute gravity, or tendency downwards, as strongly as before; tho' by a contrary and more powerful tendency of the contiguous liquor upwards, such its endeavour does not appear. Thus whilst a piece of light wood emerges from under water, it retains its gravity in its ascent, being only buoy'd up by the water, whose specific gravity is the greater; so that the sum of the water, and the ascending wood, weighs more than the water alone would do; and therefore, when this wood floats, with some part above the surface of the water, a lighter body actually gravitates upon a heavier. Thus, also, were a man weigh'd in a balance, with a stone in his hand, which he lifts above his head; tho' that may seem light, in respect of his body, yet it loses nothing of its natural weight, neither during its ascent, nor afterwards: for the hand that raises it up, will constantly perceive its tendency downwards, and the stone; and the man together, will weigh no less in the scale, than if he did not support it, but both were weigh'd apart, and their respective weights afterwards added together. And thus, lastly, if a glass of water, with a large quantity of common salt, in powder, be balanced in the scale, whilst the salt gradually dissolves and becomes one body with the water; yet the weight here will not be diminish'd as the salt comes to be supported by the fluid; but both together will gravitate, as at first, on the scale that supports them.

PARADOX III.

If a body be wholly, or in part, immersed below the surface of water, its lower part will be pressed upwards by the water contiguous to it, from beneath.

This appears from the first experiment, ; for, where-ever the lowest part of the body be, in either case, supposed, all the parts of the plain, wherein it rests, are pressed upwards. And if this force under the body be greater than that wherewith it tends downwards, 'twill buoy it up ; but if the body be specifically heavier than water, the tendency of the contiguous fluid from beneath upwards, proves only ineffectual to raise or support it, but is by no means destroy'd ; the heavy body being constantly resisted and retarded by the water, as it would be in a pair of scales, were it hung at one end, and an equal bulk of water at the other end of the beam.

Fig. 5. The same truth is farther manifest from the third experiment ; for, if the tube, wherein the water is suspended, be plunged deeper into the oil ; or, if more oil be poured into the vessel ; the water in the tube, will rise the higher ; which could not happen, unless the oil, tho' specifically lighter, pressed against the lower surface of the water more forcibly than the water tends, by its gravity, downwards. And even when the two fluids balance each other, the oil continually presses upwards against the lower surface of the water ; for herein consists its constant resistance to the perpetual endeavour of the water to descend. And since the same phenomenon happens, whether the water be suspended in oil, or *vice versa*, the proposition holds equally true, let the body be specifically heavier, or lighter than the fluid wherein it is immersed.

SCHOLIA.

1. That water makes a resistance to bodies which sink therein, is thus demonstrated. Suppose the pipe E F, contains oil of cloves, or any other Fig 7. oil, that is specifically heavier than water ; that the oil in the tube, and the external water are in equilibrium ; and lastly, the pipe to be slowly raised towards the top of the vessel : 'tis evident, that drops of oil will, during the tube's ascent, fall from the bottom thereof to that of the glass ; but much more slowly than if they fell in the same manner, thro' air.

2. To compute the quantity of pressure against the lower parts of a drop, suppose it to be G, touching the plain H I ; we must observe, that if the drop of oil were not there, its place would be supplied by an equal bulk of water ; which being of the same specific gravity with the rest of the fluid in the vessel, the surface H I, would be every where equally pressed ; and, consequently no part thereof be displaced. But the drop of oil being heavier than an equal bulk of water, that part of the surface whereon it rests, is more pressed than any other equal part of the same ; and therefore, will give way to the descending drop. And as this must be the case in all the other surfaces as well as in H I, the drop will conti-

nue

nue falling, till it arrives at the bottom of the glafs. And if the drop G, were not fpecifically heavier than water, but barely equal thereto, the contiguous part of the furface H I, would be no more preffed than the other equal parts thereof, and, confequently, it would be neither depreffed nor raifed ; but the drop G, continue in the fame place : whence we conclude, with *Archimedes*, and others, that a body immerfed in a fluid, of the fame fpecific gravity with itfelf, will reft at any affignable depth therein. Since, then, if the drop G, be of the fame fpecific gravity with water, it will neither fink, nor emerge ; if it be heavier, 'twill only fall by the weight whereby it exceeds an equal bulk of water ; for this, alone, is the caufe of its defcent, the other being wholly refifted by the water ; and confequently, it lofes in the water the weight of an equal bulk of water, weighed in the air. STATICS.

3. This grand theorem is thus eafily confirm'd by experiment. Hang a piece of lead, tied to an horfe-hair, from one fcale of an exact balance, counterpoife it, and fuffer the lead to fink down, and hang freely, in a veffel of water; and you will find the former counterpoife much too heavy for the lead ; part of it, therefore, being taken away, till the balance be reduced to an equilibrium, 'tis eafily feen what proportion of weight is loft in the water. If, then, another piece of lead be weigh'd in air, where it equals, fuppofe, 12 ounces, and afterwards be hung to the fcale by an horfe-hair, as the former, and weigh'd in water, you may conclude that 11 ounces, (fuppofing lead to be to water, as 12 to 1) put in the oppofite fcale, will now reduce the beam, how uneven foever it might hang in the air, to an horizontal pofition.

COROLLARIES.

1. From this preffure of water, againft the lower furface of bodies immerfed therein, may be deduced the caufe why light fubftances emerge to the top thereof ; which has, hitherto, remain'd an inexplicable phenomenon. Suppofing, then, a pipe almoft filled with oil of turpentine ; if, as in our firft experiment, the lower orifice be plunged under water, tho' not near fo deep as the oil is high, and the upper end be gradually unftopp'd, the oil will fall, in drops, from the bottom of the tube, and prefently emerge to the furface of the water; notwithftanding they are preffed as well downwards, as upwards : the reafon whereof is, that the upward tendency of the water, contiguous to the lower furface of the oil, is ftronger than the tendency of the oil, and the incumbent water downwards. Thus, when the drop G, touches the two parallel plains H I, K L, 'tis evident, that Fig. 7. the lower part of the drop G, is more preffed by the water, than the upper part M, becaufe upon the whole furface K L, there is only an uniform preffure of the water, A K B L, whilft upon all the parts of the furface H I, there lies a greater quantity of Water, A H B I, except upon the part N, where the oil G, being lighter than an equal bulk of water, and expofed to a greater preffure from beneath, than its own gravity, with that of the incumbent water, can refift ; it muft neceffarily give way, and

STATICS. be impelled upwards. And, as the ſame reaſoning holds, for all the points in its aſcent, it muſt, conſequently, be driven, gradually, to the ſurface, and there float. Or, in other words; when any body, ſpecifically lighter than water, is immerſed in that fluid, it ſuſtains the preſſure of two columns of water, one againſt its upper, and the other againſt its lower part: and, as the height of theſe columns muſt be computed from the top of the water; the lower part of the immerſed body is preſſed upon by a column longer than that which preſſes upon the upper, by the thickneſs of the body, which, therefore, will be preſſed more upwards than downwards. And the greater difference there is in the ſpecific gravities of the water, and the body made uſe of, the ſwifter, *cæteris paribus*, will be the aſcent; becauſe of the greater preſſure upon all the other parts of our imaginaty plain, than upon that which is contiguous to the bottom of the aſcending body.

2. Hence, alſo, we may ſolve that hydroſtatical problem; why if two cylindrical pieces of wood, one of them twice the length of the other, be plunged to an equal depth under water, and thence ſuffer'd to emerge, at the ſame time, the longer gains the top before the ſhorter: for, ſuppoſe O P, to be one of theſe bodies, and two feet long, Q R, the other, and one foot, and that they both reſt upon the ſame plain, parallel to the upper ſurface of the water, and three feet diſtant therefrom; there will be a lateral preſſure of the incumbent water upon the lower ends of each of theſe bodies, as great as that upon the equal parts of the plain whereon they reſt; but upon the upper end of the ſhorter body Q R, there will preſs a column of water two feet high, whilſt the taller P O, ſuſtains only a pillar of one foot; ſo that both being ſpecifically lighter than water, they will be impell'd upwards; but the column conſiſting of one foot of wood, and two feet of water, will, by its gravity, reſiſt this impulſe more than that which is made up of two feet of wood, and but one of water; whence the reaſon of their unequal velocities is evident. *Fig. 7.*

3. From hence, alſo, we may, probably, deduce the reaſon why, in diſtilling aromatic oils, with water, the whitiſh liquor, which frequently comes over, long continues turbid; for this chiefly proceeds from the exceeding minuteneſs of the drops, whereof it conſiſts, allowing the height of the water, preſſing upon the upper part thereof, to be almoſt as great as that which preſſes againſt the lower, ſo that the drops are raiſed but very ſlowly, tho' they will, at length, emerge; eſpecially when, by their frequent occurſions, they have form'd drops of a larger ſize.

4. From this propoſition, it, likewiſe, follows, that all bodies floating upon a fluid, ſink till the irimmerſed part be exactly equal, in bulk, to ſuch a quantity of the fluid, as equals the whole weight of each body. For inſtance, if V, be a cubic yard of wood, 6 pounds in weight; ſince 'tis heavier than air, it will ſink in water, ſuppoſe to the plain X W, where it muſt neceſſarily reſt; for all the other equal parts of that plain X W, being preſſed upon by columns of water, equal in height to X A, if the whole weight of the cube be greater than that of as much water as equals the *Fig. 8.*

immerſed

immerſed part thereof, it muſt ſink lower, becauſe the ſubjacent part at V, would then be more charged than the reſt. On the contrary, if the cube were lighter than the water, whoſe place the immerſed part poſſeſſes, it muſt by the greater preſſure of the water upon the other parts of the plain XW, than upon that contiguous to the wood at V, be impell'd upwards, till the preſſure of the whole cube upon the part that ſuſtains it, be equal to that of the water upon the reſt of the plain; and, conſequently, be the ſame with the water, whoſe place the immerſed part poſſeſſes: the lightneſs of that part, in reſpect of ſo much water, being compenſated by the weight of what remains unimmerſed above the level of the water. Thus, when a piece of wood is thrown into water, tho' the moment it acquires in falling, carries it thro' many imaginary plains below its proper ſtation; yet the ſuperior preſſure to which each of thoſe plains is expoſed in all other parts, but in that contiguous to the bottom of the wood, preſently impels it up again, till at length it reſts in the place we have aſſign'd it.

SCHOLIUM.

This ingenious and uſeful propoſition, relating to floating bodies, is mathematically demonſtrated by *Archimedes* and his commentators; and may be farther manifeſted to the eye, by the following experiment. Having poured a convenient quantity of water into a large deep glaſs, and placed another deeper, that was properly ſhaped for ſwimming, within it; we furniſh'd the latter with balaſt, a deck, and other parts of a ſhip, till it ſunk down to ſome particular marks we had placed on the outſide. Then, obſerving how high the water reach'd in the larger veſſel, we carefully placed two or three marks in a level with the horizontal ſurface of the water; and taking out the floating glaſs, we wiped the outſide dry, and weigh'd it, with all its rigging, in an exact pair of ſcales; and afterwards pour'd an equal weight of water into the large glaſs, which we found reach'd up to the marks whereto our counterfeit ſhip had raiſed it. This experiment we ſeveral times repeated in veſſels of different ſizes, ſhapes, and lading. From whence it appears, that the floating veſſel, with all it contained and ſupported, was equal in weight to the water, whoſe bulk equall'd the immerſed part of the veſſel, had it been cut off from the other, by a plain continu'd from the horizontal ſurface of the water. This theorem, with what we have brought to confirm, or illuſtrate it, may be apply'd, *mutatis mutandis*, to a ſhip with all her rigging and lading; for 'tis univerſal, that the weight of a floating body equals that of the water, whoſe ſpace the immerſed part poſſeſſes. And hence the learned *Stevinus* ſomewhere ſays, that a whole ſhip, with whatever belongs to it, preſſes no more upon the bottom it floats over, than a bulk of water equal to the immerſed part of its hull.

PARADOX IV.

A competent preſſure of an external fluid, is, alone, ſufficient to raiſe the water in pumps.

The truth of this propoſition may appear from the preceding experiments; however, we ſhall give it a farther illuſtration and proof, as follows.

Suck deeply tinged water into a ſlender glaſs-pipe, to the height of an inch; and nimbly ſtopping the upper orifice, immerſe the lower end into a glaſs half fill'd with the ſame tinged liquor, till the ſurface of that in the pipe be an inch below the top of the external fluid; next, pour oil of turpentine thereon, till it ſwim three or four inches above the water; when eaſing your finger, gently, from the upper orifice of the pipe, to admit a communication between the internal and external air, the liquor in the tube will be impell'd up almoſt as high as the ſurface of the external oil. Now, it cannot here be pretended, that the aſcent of the liquor, in the pipe, is owing to nature's abhorrence of a *Vacuum*; ſince, as the pipe is full of air, and its orifice open, there's no danger of a *Vacuum*, tho' the water did not riſe; the air and water remaining contiguous as before. The true reaſon, then, of this aſcent, is, that, upon all the other parts of the imaginary plain cutting the immerſed end of the tube, there is a preſſure of water and oil ſwimming thereon, equal to four or five inches height of water; whilſt, upon that part where the liquor contain'd in the pipe reſts, there is only the preſſure of one inch of water; whence the parts near the immerſed orifice, muſt be neceſſarily thruſt out of their places by thoſe of the water that are more preſſed, till ſo much liquor be forced up into the pipe, as makes the preſſure on that part of the imaginary plain equal to that of every other ſuch part; when, the water will riſe no farther, but, by reaſon of the equilibrium, reſt a little below the ſurface of the oil; the ſpecific gravity of this being leſs than that of water. In like manner, it may happen in pumps; for as the oil of turpentine, tho' lighter than water, by reſting upon the ſurface of the external fluid, forces up the liquor, within the pipe, far above the external water; ſo the air, tho' lighter than oil of turpentine, reaching to the height of many miles, and reſting upon the water in a well, will preſs that water up the cylindrical cavity of a pump, much higher, if all impediments were removed, than the ſurrounding water. Now the obſtacle here is either the ſucker, or the preſſure of the external air; the whole uſe of the ſucker is, therefore, to free the water in the pipe from any impediment to its aſcent; as, in our experiment, the ſides of the tube ſufficiently defend the water therein from any preſſure of the external oil that might obſtruct its riſing. Laſtly, as the liquor in our pipe was forced up, till the cylinder raiſed, became a balance to the preſſure of the water and oil on the outſide; ſo in pumps, the water riſes but to a certain height, *viz.* thirty-three, or thirty-four feet, beyond which it cannot be impell'd; becauſe at that ſtandard the preſſure of the water in the pump upon that part of the imaginary plain wherein its lower

lower orifice lies, is the ſame with that the other equal parts thereof ſuſtain, from ſo much of the external water and atmoſphere as reſts thereon. And this may ſuffice to ſhew, that nothing more than a competent weight of an external fluid, ſuch as the air, is requiſite to raiſe the water in pumps.

SCHOLIUM.

For a farther confirmation of this propoſition, and, at the ſame time, to ſhew that a ſmall quantity of quick-ſilver may be ſuſpended in an open tube; I ſhall here offer another experiment. Having ſuck'd into the lower end of an open glaſs-tube, the diameter of whoſe bore was above the ſixth of an inch, near half an inch height of quick-ſilver, and nimbly ſtopped the upper end with my finger; I plunged the mercury into a deep glaſs of oil of turpentine, being careful not to open the upper orifice, till the quick-ſilver was ſunk eighteen or twenty times its own height below the ſurface of the oil; after which, it remain'd ſuſpended, while the tube continued unſtopped: and as if I raiſed it towards the upper ſurface of the oil, it would drop out; ſo if I depreſſed it deeper into that fluid, it would be impelled higher up into the pipe: and, by this means, ſo ponderous a body as mercury, was made ſometimes to riſe, and ſometimes to fall, but ſtill to continue ſupported by a liquor ſpecifically lighter than common ſpirit of wine. This experiment, however, may eaſily miſcarry, unleſs great care be uſed; and, after all, the oil will ſoon inſinuate itſelf between the ſides of the pipe, and ſo ſhort a cylinder of mercury; for which reaſon, I here prefer water to oil of turpentine.

COROLLARY.

Since a cylinder of mercury, about thirty inches high, is equal in weight to one of water, about the height of thirty-three, or thirty-four feet; 'tis evident, that the preſſure of the external air, which can raiſe and ſuſpend that height of water, will do the ſame to the proportionable one of mercury, in the *Torricellian* experiment.

PARADOX V.

The preſſure of an external fluid will keep an heterogeneous liquor ſuſpended at the ſame height, in tubes of very different bores.

Pour a ſufficient quantity of deeply tinged water into a clear, deep, wide-mouth'd glaſs, and cloſe it with a broad, thin cork, wherein various round holes, of different magnitudes, have been burnt; into each of which you may thruſt an open glaſs-tube of a correſponding ſize, ſo that they may ſtand parallel to one another, and perpendicular to the ſurface of the fluid, wherein they are immerſed. Beſides theſe holes, another aperture muſt be made in the cork, to receive the ſmall end of a glaſs-funnel, by means whereof, oil may be conveyed into the veſſel, when ſtopped; and in the ſlender part of this funnel, 'tis proper to place ſome cotton, to break the force of the deſcending oil, which might otherwiſe fruſtrate the experiment.

STATICS. riment. Things being thus prepared, pour oil of turpentine thro' the funnel, till the tinged water be preſſed up into all the pipes, and riſe to a conſiderable height therein, above the lower ſurface of the oil; when, you'll plainly perceive the liquor to ſtand at the ſame level, both in the greater and ſmaller tubes, while in each it is plainly ſuſtain'd at that height above the other water, by the preſſure of the external oil; which being ſpecifically lighter than water, its ſurface will remain ſomewhat higher on the outſide of the tubes, than the tinged water they contain. And if, by means of a ſyphon, apply'd in the place of the funnel, the oil be gradually drawn off from the water; as the depth and preſſure of the ſame decreaſes, the liquor in all the pipes, both great and ſmall, will gradually ſubſide.
Fig. 9. For, ſuppoſe E F, the ſurface of the water within-ſide of the tubes, as well as without them, before any oil ſwims upon it; the lightneſs of that fluid, compared to water, will prevent it, when pour'd thereon, from getting into the cavity of the pipes L, M, N: and, therefore, riſing on the outſide thereof, it muſt, by its gravity, preſs the water, and impel it up into them. And if the upper ſurface of the oil reach to G H, and that of the water deſcend to I K; by means of the quantity of oil made uſe of, the liquor in the pipes ought to have its ſeveral upper ſurfaces in the ſame level with each other, notwithſtanding the great inequality of their bores; ſince that part of the plain I K, whereon reſts the circular orifice of the largeſt tube L, is no more preſſed by the incumbent water, than any other equal part of the ſame, is by the oil it ſuſtains; the oil having only an additional height on the plain I K, to balance the preſſure of the liquor in the pipe. And tho' the tube L were twice its preſent bigneſs, 'twould but preſs the ſubjacent plain I K, equally with the oil on the other equal parts. Nor ought this preſſure of the external oil to raiſe the water in the ſlender pipe N, above the ſurface Q, in the ſame level with O; for were the liquor higher in the ſmall pipe, it muſt preſs that part of the plain I K, whereon it reſts with a force ſuperior to that of the oil upon the other parts of I K, which is greater than the oil could reſiſt; whence, conſequently, the liquor in the ſlender pipe, muſt ſubſide, till its ſurface fall below that of the external oil; for, till then, the different fluids cannot reſt in equilibrium. In a word, it matters not how wide the cylinder of water be, which the oil is to ſupport, provided it riſe no higher than the difference in the ſpecific gravities of the two fluids will admit of; for in this caſe, the preſſure of water on the ſuſtaining part of the plain, will be equal to the preſſure of the external oil on all the other parts thereof; and, conſequently, the two liquors muſt remain perfectly balanced.

SCHOLIA.

1. It may here be obſerv'd, that there's no neceſſity the glaſs-tubes L, M, N, ſhould be of the ſame length; ſince, as their lower orifices are open, the liquor will riſe to the ſame height within them, tho' they lie not in the ſame plain below the water.

2. We

2. We all along ſuppoſe, and particularly in the laſt experiment, that the ſmalleſt glaſs tubes employ'd are of a moderate ſize, and not exceeding narrow; or, in caſe they are, that allowance be made for the property of water's riſing in them to a greater height, than can be attributed to the balance either of water or oil, that keeps it there ſuſpended. STATICS.

PARADOX VI.

The direct preſſure ſuſtain'd by a body, placed any where under water, with its upper ſurface parallel to the horizon, is that of a column of water, whoſe baſe is the horizontal ſuperficies of the body, and height the perpendicular depth of the water; and if the water preſſing upon a body, be contained in open tubes, its preſſure is to be eſtimated by a column of the ſame, the baſe whereof equals the lower orifice of the pipe, and height a perpendicular from thence to the top of the water: and this, tho' the pipes ſtand obliquely, be irregularly ſhaped, or wider in ſome parts than the ſaid orifice.

The former part of our propoſition we might, with *Stevinus*, make more general, and thus demonſtrate. Suppoſe A B C D, a ſolid rectangular figure of water, whoſe baſe D C, is parallel to the horizon, and height G E, a perpendicular let fall from the upper to the lower ſurface of the water; if that part of the baſe E F, be charged with more water than G E F H, the overplus muſt come from the adjacent water; ſuppoſe it then, if poſſible, to come from A G E D, and H B C F: then, for the ſame reaſon, the baſe D E, will have more water incumbent thereon than A G E D; and the like may be ſaid of F C, whence the whole baſe D C will ſuſtain a greater weight than that of the whole water A B C D, which is abſurd. By the like reaſoning, we prove that the baſe D C, ſuſtains no leſs a weight than that of the water G H F E; ſince, then, it ſuſtains neither a greater weight nor a leſs, than the column of water G H F E, it muſt, exactly ſuſtain the ſame. *Fig.* 10.

To this demonſtration, we ſhall annex ſenſible experiments, to confirm and illuſtrate each part of our propoſition. Firſt then, ſuck oil of turpentine into a ſlender, glaſs ſyphon, till it fill the ſhorter leg, and riſe two or three inches high in the longer; then nimbly ſtop the upper orifice with your finger, and plunge the lower part of the ſyphon into a deep glaſs of water, till the ſurface of the oil in the longer leg riſes but little higher than that of the external water, and upon removing your finger, the ſurface of the oil will either intirely, or almoſt, preſerve its former ſtation; when if the ſyphon be immerſed deeper, the oil in the ſhorter leg will be depreſs'd; but upon gently raiſing the ſyphon towards the top of the water, it will exceed its former ſtation, gradually flow out in drops, and emerge to the ſurface. Now, ſince the water, at firſt, kept the oil in the longer leg, ſuſpended no higher, than it would have been by a cylinder of water, equal in baſis to the orifice of the ſhorter leg, and reaching, in length, from thence to the top of the water, as may be eaſily ſeen in a ſyphon with legs ſufficiently long; and ſince, when by raiſing the ſyphon, *Fig.* 11.

the

STATICS. the height of the incumbent water was diminiſh'd, the oil over-balanced the water, and run out; it may be fairly concluded, that tho' much of the water in the veſſel, lay higher than the immerſed orifice of the ſyphon, yet no more gravitated thereon, than what ſtood directly over it; that is, ſuch a column as our paradox requires.

COROLLARY.

It follows from hence, that water may preſs as regularly upon an immerſed body, when not reſtrained by pipes, as when included in them; we may, therefore, properly conceive a particular column of water, tho' it has no actual bounds, in an unlimited quantity of the ſame fluid.

The firſt part of our paradox holds equally true, whether the incumbent water be free or confined in veſſels of any irregular ſhape. Thus, tho' the ſhorter leg of the ſyphon be faſhion'd into a funnel, and filled with water, the oil in the other leg will reſiſt the preſſure thereof, ſo that the ſurface of the oil in the longer leg will riſe but little above that of the water in the funnel. For farther confirmation hereof, we try'd the experiment in a *Fig.* 12. ſyphon; in one leg whereof, a glaſs ſphere was made to communicate with the upper parts of the ſame. Into the uniform leg of this ſyphon, we poured a proper quantity of oil of turpentine, and fill'd the lower and globular part of the other, with water; which proved inſufficient to ſuſtain the oil at a greater height, than if this leg alſo had been uniform: all the water in the ſpherical cavity, which fell not directly over the lower orifice thereof, being ſupported by the ſides of the ſame. And when the irregular leg was fill'd with oil, and the other with water, the former fluid would not ſuſtain the latter to an equal height, notwithſtanding its exceſs of quantity contain'd in the concave ſphere. To clear up this matter ſtill farther, we poured mercury into the ſyphon ABCD, till it almoſt reach'd the bottom of the globular part, in the ſhorter leg, and to an equal height in the longer; then letting a proper quantity of water run into the longer leg, it impell'd the mercury in the ſhorter leg, till it more than half filled the cavity of the ſpherical part E, the tube AB, not proving ſufficiently long to allow of its being quite fill'd; when, we obſerv'd the ſurface of the quick-ſilver HG, to lie as high as the different ſpecific gravities of the two fluids required: ſo that notwithſtanding the great weight of the mercury contain'd in the concave ſphere E, no more preſſed upon the ſubjacent ſlender part of the leg EC, than ſtood directly over its lower orifice: whence the water in the leg AB, appear'd preſs'd only as much as if the leg CD, had been uniform, and without the ſpherical cavity E. And thus, if the ſphere had been made larger, a ſmall quantity of water in the leg AB, would have ſomewhat raiſed a much greater weight of mercury than its own.

Fig. 13. The ſecond part of our preſent propoſition is proved thus. We took three open, glaſs pipes, of the irregular figures in the ſcheme, and plunged them in a glaſs veſſel of water, the two crooked ones ſtanding very oblique to the upper ſurface thereof, and the higher ends of them all coming

thro'

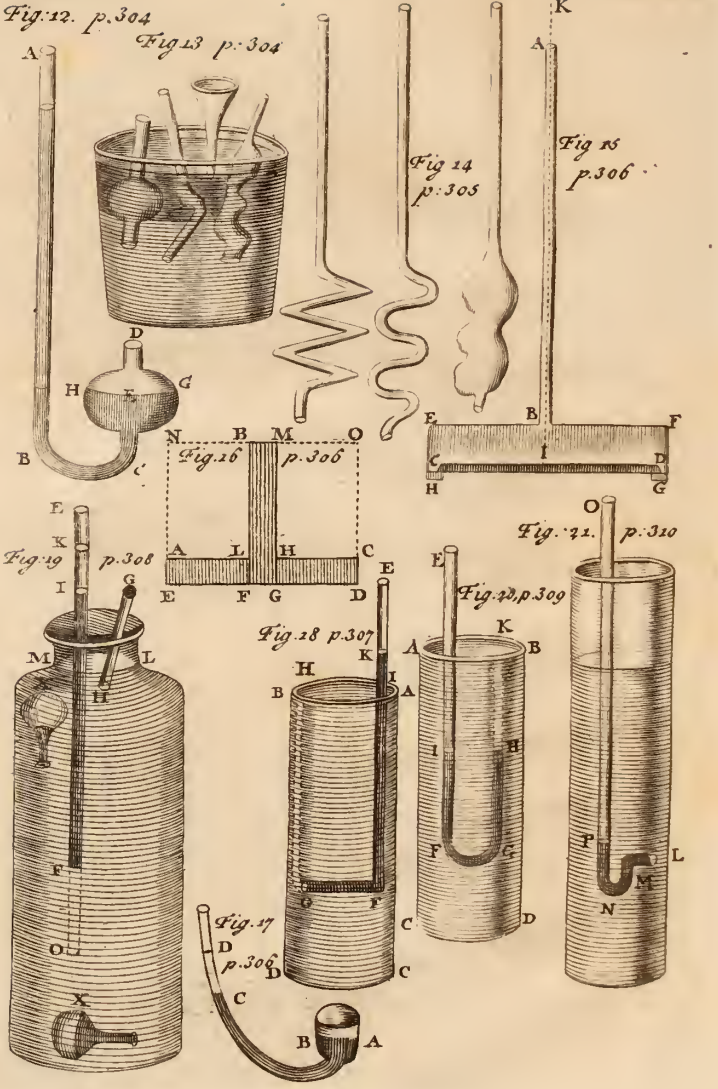
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Fig.20, p.309
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thro' particular ſtrait holes made in a cork fitted to the veſſel's mouth, whilſt the water aſcended to a certain height, thro' the lower orifice of every pipe; then pouring a conſiderable quantity of oil of turpentine thro' a funnel, into the veſſel, the water was, thereby, impelled to the ſame height, of two or three inches, into the ſeveral tubes: whence 'tis apparent, that notwithſtanding the irregularity of ſhape in the pipes, and the different wideneſs in each of their bores, the preſſure of the contain'd water upon their lower orifices did not exceed that of five cylinders of the ſame, with each of thoſe orifices for baſes, and the perpendicular depth of the water and oil therefrom for their height. For had each of the pipes contain'd only ſuch a cylinder, its upper ſurface would have ſtood at the ſame height; and if it did not depreſs the ſubjacent fluid as much as the external force tended to raiſe it, that exceſs of preſſure muſt have impelled it higher. Since then the water roſe to the ſame height in the ſeveral pipes, tho' two of them, being greatly inclined, contain'd much more water than if they had ſtood erect; by a like way of reaſoning, we may conclude, that the imaginary plain cutting their immerſed ends, ſuſtain'd no greater preſſure than that of a ſhorter, erect cylinder of water. For in all caſes where the pipe is either inclined, or wider in one part than in another, the weight of the liquor it contains, is not wholly ſupported by the body contiguous to the lower orifice, but in ſome meaſure by the ſides of the pipe. Thus, when oil of turpentine, in a ſlender tube, balances the preſſure of the external water, if the tube be barely inclined to the ſides of the containing glaſs, a conſiderable quantity of water will get up into the pipe; becauſe the oil no longer reſting wholly upon the water, but partly upon the tube, its preſſure on the water is diminiſhed, while that, continuing its force, impels up the oil, and intrudes it into the pipe; which being reſtored to a perpendicular poſition, the oil will again depreſs, and drive the water out of its cavity. We farther cauſed three pipes to be blown differently from the former, and filled their winding and irregular parts, as alſo their uniform ſtems, to a proper height, with oil of turpentine; we then plung'd them to a due depth under water, and there unſtopping their upper ends, the ſurface of the oil they contain'd appear'd at the ſame height above the water, it would have done had the tubes been ſtrait; as we found by a cylindrical one fill'd and immerſed as the reſt; tho' the quantity of oil in one of theſe pipes were, perhaps, thrice as great as that in the ſtrait one. Hence we may fairly conclude, that the preſſure of fluids contain'd in pipes, muſt be computed by their perpendicular height, how wide, long, or irregular ſoever they are.

STATICS.

Fig. 14.

SCHOLIUM.

The learned *Stevinus* has a corollary from the preceding propoſition, to this purpoſe. If thro' the upper ſurface of a concave cylinder, an open, cylindrical tube be erected perpendicularly, and the whole united cavity of both fill'd with water, the bottom circle will ſuſtain a preſſure equal to that of a cylinder of water, whoſe baſis is that of the veſſel, and height that of

STATICS. the tube therefrom. The truth of this paradox we attempted to try in the following manner. Having provided a latton veſſel A B E H G F, furniſh'd with a falſe wooden bottom C D, which was cover'd with a fine bladder, and oiled about the lower edges, to facilitate its riſing from the rim of wood H G, that lay every where contiguous to the inſide of the metal, and kept the water from paſſing between; to the middle of this looſe bottom we alſo faſten'd a ſtrong ſtring, which came out at the orifice A. Thro' this orifice we then pour'd in water, which preſſing the falſe bottom C D, help'd to tighten the veſſel, and keep its parts cloſe. When the whole cavity was fill'd, we faſten'd the upper end of the ſtring A, to the beam of a balance, and gradually placed weights in the oppoſite ſcale, till they elevated the falſe bottom C D, from the rim H G, and conſequently lifted the incumbent water, which ſoon ran down between them. Now, we had before-hand found what weight ſufficed to raiſe the bottom C D, alone; deducting that, therefore, from the weight in the ſcale, and comparing the remainder with the weight of as much water, as the ſhallow cylinder B E C H G D F, would, alone, without taking notice of that in the pipe A B, contain; we found the preſſure upon C D, ſo vaſtly greater than could be attributed to the whole quantity of water made uſe of, had it been contain'd in an uniform cylinder of the ſame baſis with our inſtrument, that we thought it ſome ſmall confirmation of *Stevinus*'s doctrine; tho' the paradox itſelf be greatly queſtion'd by ſome men of learning *.

Fig. 15.

* 'Tis now an eſtabliſh'd propoſition, that fluids preſs not according to their quantity, but perpendicular altitude; and may be demonſtrated thus.

Fig. 16. Let A B C D F E, be a veſſel full of water. Now the column B F, being heavier than the column H G, 'tis plain that if the veſſel were open at H, the column G H, would aſcend till it balanced the column B F: but ſince it cannot aſcend at H, the water there muſt be preſs'd back by the obſtacle, with a force equal to the weight of B L; for action and re-action are equal; and all preſſure here being reciprocal, the water at G, will preſs againſt the bottom of the veſſel with the ſame force. Now, as the weight of the column G H, is added to the former, the force of the water's preſſure at G, will be the ſame, as if the column G H were of equal height with the column F B, that is, as if it reach'd to M. And the ſame reaſoning is applicable to all the other columns in the veſſel; conſequently, the bottom of it E D, will ſuſtain the ſame preſſure, as if the veſſel were fill'd up to N O.

This ſurprizing property in fluids ſeems applicable to conſiderable purpoſes; as it ſhews us how an exceeding ſmall quantity of matter may be made to ſupply the place of one infinitely larger.

Thus, for inſtance, if the tube A B, of the inverted ſyphon A B C D, were cloſe ſhut, and the ſmall pipe C D, fill'd with water to D, that little quantity C D, would exert ſuch a force upon the larger, A B C, that if the cover of the great orifice were perforated, the water would be made to fly out thereat, as violently, as if part of the little tube, C D, contain'd as much water, as the great one A B. *Fig.* 17.

Again, if two cylindrical emboli were fitted to the tubes A B, and C D; the weights laid upon them would be balanced, provided they were proportionable to the capacity of the tubes; that is, ſuppoſe the capacity of the tube A B, was four times greater than that of C D, one pound laid upon the little embolus would balance the force of four pounds upon the greater. And theſe experiments might be varied infinite ways. See *Clark. Annot. in Rohault.* p. 41.

PARADOX VII.

A body, immersed in a fluid, sustains a lateral pressure therefrom; which increases with the depth whereto 'tis plunged.

Bend a small glass tube at right angles, into a longer and a shorter part; *Fig.* 18
then, by suction, draw up oil of turpentine enough to fill the shorter, and to rise two or three inches high in the longer; nimbly stop the orifice, and immerse the lower end of the tube under water; so that the longer leg, E F, may stand perpendicularly to the surface A B, and the surface of the oil I K, but a little above the same A B; when, if the tube be unstopp'd at E, the oil will continue as it was, and, consequently, there's a lateral pressure of the water against the oil contiguous to G; for, nothing else could hinder the perpendicular pressure of the oil in the longer leg, from forcing it out; as will farther appear, by gently raising the tube, in the same posture, towards the top of the water; for, as the lower leg ascends, the oil will be driven out thereat, by the pressure in the other. And that the lateral pressure of the water against the lower orifice is, before the tube be raised, nearly the same with the perpendicular pressure of a cylinder of water, reaching from the orifice G, to the top of the water; appears from the surface of the oil in the longer leg, always continuing a little above that of the water; as would happen, were a pipe of an equal bore, continued from the orifice G, to the top of the water at H. But, farther, if the syphon be plunged deeper into the water; the oil, by the lateral pressure thereof, will be gradually driven quite out of the shorter leg into the longer; and if you immerse it still deeper, a cylinder of water will rise in the same longer leg, and sustain that of the oil, which is now no longer able to balance the lateral pressure of the water at so great a depth; whence we find, that water has also a lateral pressure against water, which increases proportionably to the depth; for the external fluid, could not, otherwise, impel that of the shorter leg into the longer, which it does by greatly exceeding the resistance of the whole cylinder of oil therein. But if the tube be now gently raised again, and the lateral pressure of the water against the immersed end thereby diminished, the oil will force the water first out of the longer leg, and then out of the shorter, till, at length, it leaves the orifice G, and emerges in drops to the surface. Again, when the oil in the tube is a balance for the external water; if its shorter leg be kept parallel to the surface thereof, and moving backwards and forwards any way at the same depth therein; the oil in the longer leg will appear at the same height; from whence 'tis plain, a pillar of water, with a basis equal to the side of the body immersed, may be any where supported in the containing vessel; and that tho' this imaginary column, as G H, be not included in any firm surface, its lower parts have, by means of the incumbent fluid, a lateral pressure, tending outwards against its imaginary sides; and lastly, that this pressure increases in proportion to the height of the column of water above them. To conclude, if instead of

STATICS. its lower orifice L, till it reach'd to P, in the longer leg, in a level with L; and, ſtopping the upper end O, with my finger, I plunged the tube to a convenient depth under water; when, removing my finger, the external fluid firſt drove away the oil in L M, then depreſſed it from M to N; and laſtly, wholly impelled it into the longer leg N P O; ſo that the oil was, here, evidently preſſed not only laterally, but alſo downwards, and upwards.

PARADOX IX.

Notwithſtanding the doctrine of poſitive levity, an oil, lighter than water, may be kept immerſed in that fluid.

Since the ſurface of ſtagnant water is, phyſically ſpeaking, horizontal; that part which preſſes againſt the lower ſuperficies of an immerſed body, muſt be deeper than that which preſſes upon the upper: if, therefore, the water incumbent on the upper part of a body, can be made to ſtand at a greater height than the reſt of the fluid wherein 'tis immerſed, ſo as to balance what preſſes againſt the lower; oil may be ſuſpended betwixt two parcels of water. To reduce this theory to practice, I ſuck'd water into a ſtrait, ſlender glaſs pipe T P, to the height of an inch, and there held it ſuſpended, by ſtopping the upper orifice with my finger; then plunging the lower end of the tube, two inches below the ſurface of ſome oil of turpentine, deeply tinged with copper, whereby the oil, driving up the water, aſcended to the height of an inch in the lower end of the pipe; when, I again cloſed its upper end, to keep both liquors ſuſpended. In the next place, I plunged the tube into a glaſs of water, three or four inches below its ſurface; ſo that the water, upon the removal of my finger, was preſſed into the lower end of the pipe P, to Q, impelling up the oil Q R, to R, and the incumbent water to T S; at which ſtations, the three ſeveral parts of oil and water together, preſs upon P, as much as the external water upon the other parts of the ſame plain G H; yet the oil Q R, aſcends not, becauſe the water R S, is kept from deſcending below it, by the ſides of the tube; whilſt its upper ſurface T S, ſtands higher than A D, that of the reſt of the water: by which means, the incumbent fluid may be made to preſs upon the upper ſurface R, of the cylinder of oil, as forcibly as the water preſſes againſt Q, the lower ſurface of the ſame; whence the oil will remain ſuſpended.

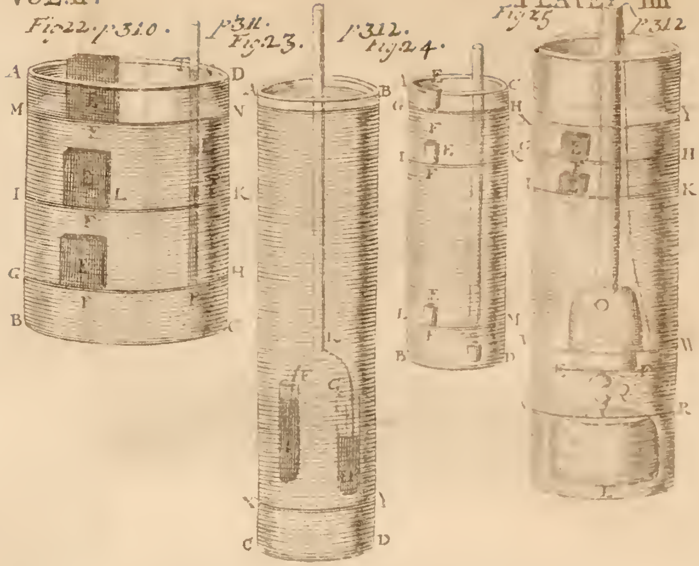
Fig. 22.

PARADOX X.

The aſcent and flux of water in ſyphons, are explicable, without ſuppoſing a Fuga vacui.

Fig. 23. Provide a cylindrical glaſs A B C D, both wide and deep, as alſo, a ſyphon F K G, with unequal legs, communicating with a long cylindrical pipe K E; to each leg of this ſyphon, fix a cylindrical glaſs I, and H, filled with water, as in the ſcheme, that the water may run from the higher glaſs

VOL. II. PLATE IIII



glass I, into the lower H; then, while the pipe E K, is held still, pour oil of turpentine, gently, into the cylinder A B C D, till it rise above the top of the syphon F K G; when, the oil pressing on the water, will raise it into the legs of the syphon, and force it out of the vessel I, into H: in which case, 'tis manifest, the water runs thro' the syphon, whilst the air has free admission thereinto, at the orifice E; so that nature's abhorrence of a vacuum, cannot be the cause of this flux. A just solution of the phenomenon may be had from considering the pressure of the oil, and the situation of the vessels; for, that fluid ascending higher than K, presses upon the surface of the external water, in the glasses I, and H, wherein the legs of the syphon are plunged, and forces it up into them; but as the excess of water in the glass I, proves an over-balance for the greater depth of oil in the glass H, the water is obliged to run from the shorter leg into the longer. Thus it happens in syphons; when once the water is brought to run thro' them, the air gravitates upon the surface of the external fluid, wherein their legs are plunged, and not upon that which they contain; and, therefore, tho' the incumbent air, stands a little higher over the fluid in the lower vessel, than in the upper, yet the water in the longer leg, greatly over-balancing that in the shorter, it must needs flow out at the latter. And the pressure of the external air being able to raise water to a much greater height than that of the shorter leg, the efflux will continue till some other cause put a stop thereto. But if the legs of a syphon exceed 35 feet in perpendicular altitude, the water will not flow thro' them; because the pressure of the external air is unable to raise water to that height. So, likewise, were an hole made in the top of a syphon, whilst water was flowing thro' it, the flux would presently cease; for the air, then pressing, as well within as without the syphon, would return the water in each leg to its proper vessel. To confirm this particular, I procured two syphons, the one of tin, the other of glass, each of which had in the upper part of its bend, a small perforation, which I could stop with my finger, or open at pleasure; wherein I frequently perform'd the experiment. And having once carefully stopp'd one of the above-mention'd holes, with oiled paper and cement, and found the water to run freely thro' the syphon; yet a puncture made in the paper, by the point of a needle, admitted air sufficient to make the water in the legs fall back, before I could possibly stop the needle quick enough into the hole to prevent it.

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PARADOX XI.

The most ponderous solid body we know, immersed in water to a depth exceeding that of twenty times its own thickness, will float, if it be there fenced from the direct pressure of the incumbent fluid.

Let a deep glass vessel A B C D, be almost fill'd with water; wherein if a small cylinder of brass E F, be immersed, till its upper surface is just cover'd, and then let go; it must, by reason of its greater specific gravity, Fig. 24.

necessa-

STATICS. necessarily fall to the bottom. But, were it placed on the plain I K, which lies at above the depth of nine times the body's thickness below the upper surface of the fluid; I assert that it would not descend, unless it were pressed downwards by the weight of the incumbent fluid. For brass being nearly nine times specifically heavier than water; the body E F, alone, would press upon the part F, as much as a cylinder of water, of an equal basis, but about nine times the height of the brass; and, consequently, the part F, will be no more depressed, than any other part of the plain I K. But since water gravitates on water, and also upon bodies heavier *in specie* than itself, if immersed therein; the brass-cylinder E F, wherever it rests in the fluid, must be forced downwards, both by its own weight, and that of the incumbent water. But were the incumbent water prevented from gravitating thereon, it follows, that the body would, at that depth, be kept suspended. And, in case the body rested, in like manner, upon the plain L M; the much greater pressure of the water on the other parts of the same surface, would impel it upwards, with a force proportionable to the difference of their respective weights.

And having found, by exact tryals, that the purest gold, the most ponderous body we know, is not quite twenty-times so heavy as an equal bulk of water; if this metal, also, were immersed into a greater depth than that of twenty times its own thickness, and there fenced from the pressure of the incumbent water; it must necessarily float. To confirm this

Fig. 25. by an experiment. Suppose the brass-body E F, to be the cover of a brass-valve, and the valve itself strongly cemented to an open glass-pipe O P; when, the body E F, being the lower part of the instrument, and every way detached therefrom, would fall by its own weight, if unsupported. Then to the button Q, fasten a thread, which, by being pulled upwards, may cause E F, to close the orifice of the valve: the valve being now plunged under water, to the depth of a foot, the cement, and the sides of the pipe O P, will prevent the fluid from pressing upon the upper part of the body E F; and, consequently, that part of the plain V W, contiguous to its surface, will be press'd upon only by the weight of the body E F, but in its other parts by that of the incumbent water; so that, letting go the string which held the body E F, close to the instrument, that body will remain suspended by the bare pressure of the water beneath it. And that this alone is the cause of the phenomenon, appears by gently raising the instrument towards the top of the water; for when it has ascended near the surface, suppose between the plains I K, and X Y, the body E F, will fall down, and the water rise in the pipe O P, to a level with that in the containing vessel. But if the valve be at first plunged much deeper under water, before the string be relaxed, for instance, to the plain S R, the valve E F, will require a considerable weight, as L, besides its own, to disjoin it: but if the weight prove insufficient for the purpose, it may soon be render'd effectual, by raising the instrument, far less than before, towards the top of the water.

COROLLARY.

This experiment renders it unneceſſary to fly to a *Fuga vacui* for a reaſon why two flat poliſh'd marbles, when they adhere, require a conſiderable force to disjoin them; they being preſſed together by air of an immenſe height, after a like manner with our valve, and the inſtrument, here, by water; which requires the great weight L, to over-balance it, ſo as to force them aſunder.

HYDROSTATICS

Applied to

ORES,

And to the

MATERIA MEDICA.

SECT. I.

Fossils, their medicinal virtue, whence?

AS the virtues of gems proceed, chiefly, from the metalline and mineral substances, that, whilst their original matter remain'd either fluid or soft, are incorporated therewith, and afterwards hardned into stones; so it seems very probable, that several boles, clays, earths, and especially minerals, *&c.* may be endow'd with considerable medicinal virtues, and, perhaps, greater than those of the finer gems; because herein are often found, more of the metallic and mineral parts, which, whilst *in solutis principiis*, might very plentifully insinuate themselves into the more open bodies, and there sit looser than in diamonds, rubies, sapphires, *&c.* which *Aqua fortis*, itself, is unable to penetrate and dissolve.

A new way of examining them, hydrostatically.

I, therefore, thought it might be of use to physicians, mineralists, and miners, to advance a new way of examining fossils; for tho' the method itself pretends not, directly, to discover more than one quality of the body it examines; yet that quality, being its specific gravity, is so considerable, that it may lead one farther than might, at first, be imagined.

Its foundation.

Reflecting that rock-crystal is of the most pure, and homogeneous kind of stones; I pitched upon this for the standard, whereby to adjust and ascertain the purity and simplicity of other mineral substances.

By

By carefully weighing several pieces of native crystal, clear and colourless, first in air, and then in water, we found its proportion to clear water, of the same bulk, as $2\frac{1}{2}$ to 1, nearly. Thus, if a hollow, metalline cube be exactly fill'd by one ounce of water; and afterwards, again, as exactly fill'd by a cubical piece of rock-crystal, that bulk of the stone will weigh about two ounces and a half. Some of my trials, indeed, made with tender balances, represented the proportion of these two bodies, with a very inconsiderable variation; but 'tis probable, that different pieces of crystal, tho' of equal bulk, may not have precisely the same weight: however, the difference is so small, that it may here be safely neglected.

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And to prevent any scruple about the origin and nature of crystal, I shall add, that I procured some strong isicles, that had been fasten'd to vaults, *&c.* as bodies that would be acknowledg'd true stones, and yet to have once been in a liquid form: and having hydrostatically examin'd these concretions, their specific gravity appear'd to be little differing from that of crystal.

To apply this fundamental observation to the present occasion; when I would, with probability, discover whether the mere stony matter of a mineral body were mix'd with some adventitious substance of a metalline nature, or of some other mineral more ponderous than crystal; I carefully weigh'd it, as we said, in air, and then in water: when, if its proportion to water of the same bulk, exceeded that of $2\frac{1}{2}$ to 1, or 5 to 2, I conclude it probable, that the concrete had in it a portion of foreign matter specifically heavier than crystal, proportionably to the excess of the weight of the solid body above that of an equal bulk of water. To illustrate this procedure by example.

Examples to illustrate the doctrine.

The magnet, upon account of its great hardness, is usually reckon'd among stones; but having observ'd a particular kind thereof, to be apparently more ponderous than common stone, of equal bulk, we weigh'd them in air and water; and found the specific gravity of some of them so far to exceed that of crystal, or marble, that we concluded they contain'd a considerable quantity of metalline matter, which, by collateral experiments appear'd to be of a ferruginous nature.

Emeri is commonly reputed a mere stone; but finding its weight in water considerably to exceed that of an equal bulk of crystal; for it was to that liquor nearly as 4 to 1; I conjectured, that it contain'd a metalline substance; as afterwards, by proper trials, I found it did.

By the weight, also, of *Lapis Hæmatites* on my hand, I conjectured, it largely partook of a metalline ingredient; and presently I discover'd that iron or steel was contain'd therein. And, in general, I have frequently suspected different bodies to be of a metalline or mineral nature, and was seldom mistaken in my conjecture; tho' they have commonly been thought to contain no such substance. The same thing I, likewise, found true, even in granats and *American* talc, from which I have extracted metalline parts.

Preliminary observations, with regard to fossils.

But we must here set down some preliminary observations. And first, if a proposed fossil be much lighter than an equal bulk of crystal, it is

STATICS. almoſt certain it cannot be a metalline ore; and this negative proof is here uſually ſtronger than the affirmative kind. Thus, for inſtance, when I find that jet, tho' a foſſil, has a far leſs ſpecific gravity than cryſtal, I conclude it to be no metalline body. The like determination I make as to foſſil-amber, *Sulphur vivum*, common ſulphur, *Engliſh* and *Venetian* talc, and ſome other firm concretions, which are dug out of the earth: among which, I alſo rank black-lead. For having found its weight to that of water as 1.86 to 1, I afterwards diſcover'd it to be not lead-ore, but a particular kind of mineral, and near allied to a ſort of talc that I have met with.

2. We muſt diſtinguiſh between the ſeveral uſes to be made of foſſils by men of different profeſſions. So that if a foſſil be found very little heavier *in ſpecie* than cryſtal or marble, it may, poſſibly, have a metalline or mineral portion, which, however ſmall in quantity, will perhaps entitle it to the eſteem of a jeweller, phyſician, or chymiſt. But if this exceſs of its ſpecific gravity be inconſiderable, the foſſil itſelf will prove of little price to a mineraliſt; for if it be poor in metalline ſubſtance, it may chance to be hardly worth the working.

3. But tho' the great ponderoſity of a foſſil generally proceeds from a proper metalline ſubſtance embody'd with the other parts thereof, which ſhews it not to be a mere ſtone; yet this, alone, is not a ſure ſign that the mineral part is properly metalline: and, therefore, where any doubt ariſes, we ſhould have recourſe to collateral ſigns. For, beſides metalline ores, properly ſo call'd, there are other foſſils, which, tho' of affinity to metals, may be diſtinguiſh'd from true metalline ores; ſuch, for inſtance, are antimony, biſmuth, *Lapis Calaminaris*, and marcaſites. But there will not, perhaps, occur many caſes, wherein we need to have recourſe to collateral ſigns, to ſhew whether the mineral part of a foſſil be, in a ſtrict ſenſe, of a metalline nature, or not. For ſuch ſemi-metals are moſt commonly found either in veins, maſſes, or great lumps of their reſpective kinds; and eaſily diſcover to what ſpecies they belong. I have, indeed, receiv'd a lump of matter from *Devonſhire*, wherein I found ſome antimony mix'd with lead, which was the predominant body. But ſuch mixtures occur ſo ſeldom in *England*, that our way of eſtimating ponderous foſſils, will, nevertheleſs, be uſeful on moſt occaſions.

4. Two different eſtimates may be made of the ſpecific gravity of ores; one, when the body propoſed is weigh'd in its natural ſtate, that is, as it comes out of the earth, accompanied with the ſpar, or other heterogeneous matter, that firmly adheres to it; and the other, after it has been beaten ſmall, and ſeparated from heterogeneous ſubſtances by the help of water, after being skilfully agitated, wherein there is a remarkable difference in weight between theſe and the genuine parts of the ore; which being thus ſever'd from the reſt, are call'd waſhed. 'Tis ſometimes, alſo, very proper to prepare the ore by roaſting it once or more, or by keeping it for ſeveral hours in a competently ſtrong fire; as is uſually practiſed in preparing copper-ore; eſpecially if it be ſtubborn. I purpoſely mention theſe

two

two ſtates, wherein the weight of an ore may be taken, becauſe I have obſerv'd, that, in ſeveral caſes, 'tis of great importance to diſtinguiſh them carefully. For ſeveral ores, which, in their natural ſtate, have too little ſpecific gravity, to make them judg'd worth the charge of being wrought, may yet, after they are prepar'd by water and fire, afford a metalline portion, ſo ponderous, as to allow one to ſuſpect they contain either ſilver or gold. I remember, that a piece of lead-ore, brought from *Ireland*, ſeem'd to me ſo light in the lump, that I thought it deſerv'd not to be wrought for lead; yet, afterward, upon trial, it appear'd ſo well ſtor'd with particles of ſilver, that I encourag'd the owner of the mine to work it.

To diſtinguiſh marcaſites from metalline ores.

5. There is one kind of minerals that I have obſerv'd to impoſe upon men ſo frequently, that I ſhall take a particular notice of it. For, not to mention examples from travels, and voyages, I have myſelf met with numbers who had great expectations from marcaſites. And I have receiv'd not only from places near home, but even from the *Indies*, foſſils, whereof my opinion was deſir'd; which I found to be only marcaſites. Many of theſe foſſils have two qualities, that make them fit to delude the vulgar; for, they ſhew a multitude of ſhining ſtreaks, reſembling gold; or have, at the ſame time, a weight equal to that of true metalline ores. Marcaſites, then, being thus adapted to impoſe upon the unwary, I have had much ado to undeceive ſome perſons, as to the pleaſing confidence they had entertain'd, that theſe promiſing foſſils were lumps of rich gold, or ſilver-ore. Wherefore, ſince their weight, the criterion of minerals, is one of the two things that delude ſo many, I ſhall give a few inſtances of the ſpecific gravity of marcaſites, to make it appear, that ſome of them are, bulk for bulk, far more ponderous than true metalline ores. And, indeed, the great ponderoſity of foſſils, has ſeveral times occaſion'd me to determine, before trial, that they were marcaſites. But, to prevent being impos'd on, by this, or the like ſpecious ſubſtance, it is but placing it in a ſtrong fire, and blowing, now and then, with a pair of bellows; for, by this means, the ſulphur wherewith marcaſites abound, will take fire, and burn with a flame for the moſt part blue, like that of common ſulphur: ſo that once, by diſtillation, in a cloſe veſſel, I obtain'd four ounces of good brimſtone, from three pounds of theſe ſtones. And, if when this foſſil ceaſes to flame, and ſmoke, it be taken out of the fire, and ſuffer'd to cool, it will be depriv'd of all its gaudy appearance, and turn'd to a brittle, blackiſh ſubſtance, very different from that of a proper metalline ore. Marcaſites, however, may be look'd upon as a kind of metalline bodies; for, I have found ſeveral of them to contain, not only particles of copper, but alſo of iron, or ſteel; for, after calcination, applying the pulveriz'd remains, to a vigorous load-ſtone, great multitudes of chalybeate corpuſcles quickly adhered thereto. And, I remember, in a catalogue of the foſſils of *Miſnia*, publiſhed by *Kentmannus*, under the head of *Pyrites*, he places ſeveral marcaſites; ſome whereof contain'd copper, others ſilver, others gold, and others, again, both ſilver and gold. Having preſented a very fine marca-

STATICS. ſite to the overſeer of one of the emperor's beſt mines, he preſently examin'd it, by a peculiar way, with hopes to find in it ſome gold or ſilver; but, inſtead thereof, obtain'd a parcel of running mercury, which he preſented me. Notwithſtanding the caution here given, I do not deny, but that 'tis poſſible, for a skilful artiſt, to make a profitable uſe of marcaſites, either by fixing the volatile gold, or ſilver, to be found in ſome of them; by graduating ſilver by their means, &c. But that for which I much more value them, is, what I deſire ſhould be well remark'd, that I am perſuaded, if they were ſimply, and, of themſelves, but dextrouſly handled, they may afford very noble and uncommon medicines, particularly excellent in continual fevers; tho' their uſual operation be almoſt inſenſible, unleſs by their good effects.

Directions to find the beſt flux-powders, for foſſils.

6. I muſt not here omit, that tho' many, who make trials of ores, value their own flux-powders, or ſuch as are cry'd up by others; yet they commonly ſeem to expect nothing from thoſe they prefer, more than that they ſhould beſt facilitate the fuſion of the ore; as that which being once done, the metalline part will ſeparate by its own weight, or, as it were, ſpontaneouſly. But yet, having purpoſely examin'd the matter more nicely, and compared the quantities of metal obtain'd from two portions, of equal weight, of the ſame ore, we found thoſe proportions conſiderably differ'd; tho' that which yielded leaſt metal, was flux'd down with a coſtly, and well-adapted powder. And, I doubt not, but from other metalline ores, a greater quantity of pure metal is obtainable, by ſome flux-powders, that are but little employ'd, or known; than by others, that are much more famous and common. Thus two equal portions of the ſame lead-ore, clear of ſpar; being, the one reduced with a due weight of nitre and tartar fulminated together, afforded me a much leſs proportion of malleable lead, than the other, by means of half, or a quarter the quantity of filings of iron. And, to inſtance in a much more precious mineral than lead-ore, I try'd the like with ſome ounces of good native cinnabar, finely pulveriz'd; by adding to one half a fix'd alkali of tartar, and to the other, a different flux-powder, we obtain'd from the former, twice as much mercury, as we did from the latter, tho' diſtill'd with a fix'd alkali even of a mineral nature.

Directions relating to the management of the hydroſtatical balance.

7. Before we proceed farther, 'tis neceſſary the reader be acquainted with the method of weighing heavy bodies in water; which being liable to ſeveral contingencies, 'tis proper in this place to take notice of the more conſiderable ones.

Fig. 26.

But, firſt, to give a general notion of the thing, I deſire the figure of the hydroſtatical balance itſelf, with all its apparatus, may be view'd; wherein A A, is the beam of the balance, B B, the diſhes, C C, the frame whereon the beam is ſuſpended; with D, its ſliding ſocket; E, the arm; with F, a pulley; over which, and G, another pulley paſſes H, a line faſten'd to I, a moveable weight; by which the beam is elevated and depreſs'd. K, is a hair wherewith to ſuſpend L, the body to be weigh'd in the water of M, the glaſs ciſtern. N, is the bucket for liquors, O, the box of grains, P, the

the forceps, wherewith to manage them, Q, the pile of weights, R, the handle of the balance, and S S S S, the table. STATICS.

The ſolid body to be examin'd, ſhould be ty'd about with an horſe-hair, of a competent length, and faſten'd at its other end, to one of the ſcales, of an exact and tender balance; that the body, having been carefully weigh'd in the air, may be immerſed in a proper veſſel, almoſt full of fair water, ſo as to hang freely therein, and be on every ſide encompaſs'd with the fluid. This done, put weights into the oppoſite ſcale, to balance the body hanging in the water; when the beams of the ſcale will lie horizontal, and the weights employ'd give that of the body in water. Deducting, therefore, this weight from the weight of the ſame body in the air; by the remainder, that is, the difference of the two, divide the whole weight of the given body in air; and the quotient will ſhew the proportion in ſpecific gravity, between the ſolid, and an equal bulk of water. To illuſtrate this by an example, we put a fine piece of white marble, which ſeems the moſt pure of any common opake ſtone, into an exact balance, and took its weight in the air 1169 grains; then a horſe-hair being ty'd about it, and the other end of the ſame hair, faſten'd to one of the ſcales; under which, at a convenient diſtance, was placed a deep glaſs, almoſt full of fair water; we ſuffer'd the ſtone to hang freely beneath the ſurface of the fluid; and, in the oppoſite ſcale, placed weights enow to bring it to an equilibrium with the other. Theſe weights amounted to 738 grains, which gave us the weight of the marble in water; which being ſubſtracted from the weight of the ſame ſtone in air, there remain'd 431 grains for the weight of the equal bulk of water: by this remainder, we divided the weight of the marble in air, *viz.* 1169, and found the quotient to be 2.7 nearly, for the ſpecific gravity of white marble to water.

'Tis here manifeſt, from the nature of the thing, that the body propos'd to be weigh'd, muſt be heavy enough to ſink in water; otherwiſe its weight in the fluid cannot be ſignificantly deducted from its weight in air; but if there be occaſion to weigh in water, a body lighter *in ſpecie* than water, it may be done, tho' with difficulty, by joining to it another body, that is able to ſink it.

8. A horſe-hair is made choice of for hydroſtatical operations, becauſe 'tis thought to be of the ſame ſpecific gravity with water; and tho' that be not ſtrictly true, yet a horſe-hair is fitter to be employ'd in theſe trials, than any other ſtring I know of; and its weight uſually differs ſo little from that of water, that the difference may be ſafely neglected: but if the ſolid propoſed, be too heavy to be ſuſtain'd by a ſingle hair, ſeveral of them may be twiſted together.

9. I ſometimes had occaſion to examine bodies hydroſtatically, which, by reaſon of their roundneſs, or other inconvenient figure, could not well be faſten'd to a hair; in which caſe I uſed hairs interwoven into a kind of a ſmall hoop-net, the meaſhes whereof would not ſuffer the body to ſlip thro' them.

10. And

STATICS. 10. And here let it be noted, once for all, that whenever any hydroſtatical trial is made, by means of a horſe-hair, there muſt be put into the oppoſite ſcale, as much of the ſame hair as ſeems equal to what ſuſtains the body, and appears above the ſurface of the water; for that fluid only takes off the weight of as much of the hair as is immers'd in it: ſo that its unimmers'd part adds to the weight of the pendulous ſolid; and, therefore, ought to be compenſated by an equal weight in the other ſcale.

11. When I reſerv'd a balance for hydroſtatical trials, I found it convenient, on ſeveral occaſions, to take off one of the ſcales, with the ſtrings belonging to it, and ſubſtitute, in its ſtead, a piece of metal, of a conical, or other convenient figure, and equal in weight to the oppoſite ſcale; as alſo, at the ſame end of the ſtring, to faſten one end of the horſe-hair that tied the body to be weigh'd in water. And ſometimes, likewiſe, when I did not take off one of the ſcales, I cauſed it to be perforated in the middle, without leſſening its weight; that ſo the body to be immers'd, might hang perpendicularly from the centre of the ſcale.

12. Care muſt be taken, that the body to be examin'd, hang freely in the water, ſo that no part of it, any where, touch the bottom, or ſides of the veſſel, or reach above the upper ſurface of the water it contains; for, when any of theſe circumſtances are neglected, the true weight of the ſolid is ſomewhat alter'd: and if any part of the body, or horſe-hair, 'tis tied with, appears above the water, it adds to the weight thereof. And, as nothing but the water is to touch the hanging body, ſo no part of the fluid muſt touch the ſcale from whence it hangs. I have ſeveral times obſerved, that immers'd bodies have been concluded to weigh more in water than they really do, from a neglect in obſerving, that if the ſtring be too ſhort, or the veſſel too full, the vibrating motion of the balance will, at one time or other, carry down the ſcale to which the ſuſpended body is tied, ſo as to make ſome part of it touch the ſurface of the water; a few drops whereof will readily ſtick to it; which, becauſe they adhere to its under part, may lie conceal'd from a heedleſs eye, ſenſibly add to the weight of the ſcale, and make the body be thought heavier than it naturally is: an error very prejudicial, when exact experiments are required.

13. But the moſt uſual cauſe of miſtakes, in hydroſtatical trials, proceeds from hence, that the given ſolid, and the ſtring whereto 'tis faſtned, carry down with them particles of air; which, perhaps too, may extricate others that lay conceal'd in the pores of the liquor, in the form of bubbles. Theſe aerial particles faſten themſelves to the little aſperities they meet with, on the ſurface of the immers'd body, and like ſo many little bladders of air, endeavour to buoy up the body they adhere to; and, therefore in proportion to their number and bulk, leſſen the due weight of the immers'd body, in water. Great care, therefore, muſt be had, in nice experiments, to ſhake the ſtring, and warily knock the body againſt the ſides of the glaſs, that the adhering bubbles may thereby be diſplac'd, and emerge to the top of the water. And, on ſome occaſions, this caution ſhould be uſed more than once in the ſame experiment; becauſe I have often obſerv'd,

ſerv'd, that after the immerſed body has been freed from the firſt bubbles that appear'd about it, others ſucceeded in their ſtead, before the experiment was finiſh'd. STATICS.

I have been the more circumſtantial in explaining the way of weighing bodies in water, becauſe experience hath taught me, that the practice of it is not ſo eaſy as might, at firſt ſight, be expected.

Having obtain'd the weight of the given body, firſt in the air, and then in water, according to this method; it will not be difficult to diſcover, practically, the proportion in weight between the ſolid and the fluid. The foundation of our practice, is this theorem of *Archimedes.* " A body heavier than water, weighs leſs in that, than it does in air, by the weight of an equal bulk of water." Whence we deduce a rule for our preſent purpoſe. For if the weight of the propoſed body, whilſt it is every way ſurrounded with water, be ſubſtracted from that of the ſame body in air, the remainder gives the aerial weight of a quantity of water equal in magnitude to the ſolid; ſo that having the weight both of the ſolid and fluid, divide the greater by the leſs, and the quotient compared to an unit, will be the antecedent of the proportion ſought between the ſolid and the water. And this rule holds equally, *mutatis mutandis*, for other liquors, as well as water.

To apply this doctrine firſt to ores.

The hydroſtatical balance applied to ores, and firſt to gold-ore.

Many learned men have been of opinion, that, properly ſpeaking, there were no ſuch things as gold-mines; and I confeſs myſelf to have been long kept in ſuſpence as to this particular: for, notwithſtanding all the inquiry I could make after mines, wherein gold was the predominant metal, I could not find a man who would ſay he had ſeen any. At length, I met with ſome ore, preſented to King *Charles* II. which I judged to be genuine; and alſo received from an unknown hand in the *Eaſt-Indies*, another piece, in the clefts whereof, and a little beyond them, there appear'd ſome lumps, wherein, by their colour and other ſigns, it ſeem'd manifeſt that gold was the predominant metal. But the largeſt piece, and that which was beſt furniſh'd with metalline parts, weighing about an ounce and a quarter, contain'd ſo great a proportion of ſpar, that its ſpecific gravity to water, was but as 2.21 to 1. Its metalline portion, however, ſeem'd to be all gold; for there appear'd no ſign of any other metal therein, nor, in ſome leſſer pieces that I receiv'd along with it. Its ſpar, or ſtony matter, wherein the true ore is immediately lodg'd, did not reſemble the ſpar of lead-ore, or that of any other of our *Engliſh* metals; but ſeem'd, at firſt view, to be a kind of white marble, with a daſh of yellow. And, upon trial, I found it differ'd ſtill more from the ſpar of lead-ore, which, with us, is uſually white, and almoſt ſemi-diaphanous. For our ſpar of lead-ore is often ſo ſoft, that it may eaſily be cut with a knife; but the ſparry portion of this gold-ore was a ſolid ſtone; and ſo hard, that when ſtruck againſt a piece of ſteel, it would yield ſparks of fire. And, farther, the ſpar of lead-ore will eaſily, and ſuddenly, calcine to a kind of lime; but our golden-ſpar, tho' kept for ſome hours red-hot in a crucible, did not appear to be at all cal-

STATICS. cined. The ſpar of lead-ore will, alſo, diſſolve in ſome acid menſtrua, and even in diſtill'd vinegar: yet I could not find that this golden ſpar, when kept for ſeveral hours in ſpirit of ſalt, *Aqua fortis*, or *Aqua regia*, was manifeſtly wrought upon by any of them.

A piece of ſpar, that ſeem'd ſcarce to contain any gold at all, being hydroſtatically examin'd, we found its ſpecific gravity to water, as 2.65 to 1.

Theſe obſervations, imperfect as they are, may, perhaps, be ſerviceable to ſuch as have never ſeen true gold-ore; and, in particular, to thoſe who ſearch after gold-mines in *Jamaica:* where, as general *Venables*, the perſon who conquer'd it for the *Engliſh*, inform'd me, at his return from thence, the *Spaniſh* governor of the iſland, whilſt his priſoner, confeſs'd there was mineral-gold; tho' the *Spaniards*, for want of workmen, could not dig deep enough in queſt of it.

By what I have ſaid of the true gold-ore, I would not have any one diſcouraged from ſeeking for gold in other metals; becauſe, I know, it may ſometimes be found blended with predominant minerals, as appears from the copper-mines of *Cremnitz*, whence a conſiderable quantity of gold is yearly obtain'd. And I have ſeen an *Engliſh* tin-ore, wherein there lay, in little cells, a number of ſmall leaves or chips of gold; and tho' the tin-men, being unable to ſeparate them to profit, uſually melted both the metals together, and ſold the maſs for mere tin; yet the owner of the mine aſſured me, that one of his work-men advantagiouſly employ'd his own children to pick the gold out of the ore, when skilfully broken. There is, alſo, a place in *Scotland*, where, over a lead-mine, near the ſurface of the ground, they often find large grains of native gold free from ſpar; ſome of which I thought worthy to be preſented to that curious examiner of ores, Prince *Rupert*; and I ſtill have a piece of native metal by me, that came from the ſame place, in weight above forty grains, and wherein gold is the predominant metal.

I have found a grain of natural *Scotch* gold, without any adhering ſpar, to weigh three drams, twenty-one grains; another grain of the ſame, that had a little ſpar ſticking to it, weigh'd three drams, three grains; ſo that, allowing for the heterogeneous ſubſtance, it weigh'd about three drams. And, laſtly, a grain of *Scotch* gold weigh'd, in air, 43 grains; in water, $39\frac{1}{7}$ grains; whence its proportion to water is as $12\frac{2}{7}$ to 1.

It often happens, that, among the leſſer grains of gold, properly call'd ſand-gold, there are pieces, ſingly big enough to be ty'd about with a horſe-hair, and weigh'd in water; to which, therefore, our hydroſtatical way of examining ores may be uſefully apply'd. For, ſince pure gold is to water of the ſame bulk, as 18 or 19 to 1; it will readily appear, whether the fragment propoſed be perfectly pure, or not.

We frequently receive from the maritime parts of *Africa*, ſmall fragments of gold, in the form of ſand or gravel, which ſeem to have been waſh'd away from hidden veins by the violence of the waters; to eſtimate the genuineneſs and degrees of purity whereof, our hydroſtatical method of of examination may be of singular service. For the specific gravity of perfectly refined gold to water, being known; 'tis, thence, easy to examine its degrees of fineness. And, when once the true specific gravity of a parcel of sand-gold is known, together with its degree of fineness, gain'd by collateral trials; this specific gravity may be used as a standard, whereby to try the fineness of other parcels of the like native gold: whence the fraud of the *Negroes* may, in this case, be prevented; who often clandestinely mix with the right sand-gold, filings of copper, or brass; whose specific gravity is not half so great as that of fine gold.

Aqua fortis, also, would immediately discover this fraud, which will not work upon gold; but corrodes brass, and thereby gains a colour betwixt blue and green; tho' if much silver be naturally mix'd with the gold, this proof, by *Aqua fortis*, will require skill: and, therefore, good spirit of urine may be substituted in its stead, occasionally; for this will readily work upon filings of copper or brass in the gold, and gain from them a fine blue colour. And, to hasten the operation of this liquor on filings of brass, or copper; 'tis but spreading them thin, upon a piece of white paper, and moistening them throughly therewith, that the air may promote its action; for, by this means, I have often produced, in a few minutes time, a pleasant blue colour upon the paper. Spirit of hart's-horn, or other volatile alkalies, or even stale rank urine, will serve in time of need, and yield the same phenomenon.

But if a solid substance be desired for this purpose, common sal-armoniac may be used, by making a strong solution thereof in water, and applying it as any of the former; for this also, will soon gain a greenish colour, or a blue, from brass or copper, and without affecting the gold.

All minerals should be carefully examined, and, if ponderous, hydrostatically.

I have observ'd such a variety of appearances and disguizes in metalline bodies, and other minerals, that I would advise the searchers after mines to have their eyes always open, and ready to take notice of any unknown, or uncommon fossil; and not neglect to weigh it in their hands; and if it seem to exceed the weight of crystal, or marble, to examine it hydrostatically. For there are in *England*, and several other countries, useful fossils, usually overlook'd by the unskilful. I have found, in this kingdom, eagle-stones even upon the high-ways; and some other minerals that were not suspected to be of *English* growth. And, I remember, that passing by a potter's work-house, and viewing the ground attentively, I made a discovery of manganese, a mineral very proper for glazing and colouring of earthen vessels, which the potter, afterwards, gladly used for that purpose. The same person shew'd me a place that contain'd great store of a fossil substance, unknown in *England*, that ran very far under ground, like a vein of metalline ore; by some easy trials I found it abounded with vitriolate salt, much more than any marcasite I had examin'd in the form of stones: whence I concluded, it might more profitably be employ'd to make vitriol, than the marcasites used for that purpose in the works at *Deptford*, or elsewhere, in *England*.

I remember, also, that a mineral of an odd appearance, being sent me, as unknown to the miners who dug it up; I found it to be the ore of bismuth, tho' the vein that afforded it was very small. But the chief reason for which I give this particular caution, is, that an application of our general remark upon the specific gravity of fossils, may be extended to a new and considerable use; for we need not confine ourselves to examine only those fossils, whereof we can obtain pieces sufficiently large to be singly weigh'd in water; because, not to mention the minerals that may be found useful to the physician, the drugster, or the mineralist; the ores of metals may be often found disguiz'd in the form of earth, or mud, that is easily dry'd; which fossils, tho' unfit to be immediately suspended by a horse-hair, may conveniently be examin'd by means of a glass jar-bucket, of a known specific gravity: for, this vessel being almost fill'd therewith, and that matter, carefully counterpois'd in air, and made thoroughly wet with water, and the whole warily let down in the same fluid, and there kept suspended by a horse-hair, tied to a tender balance, the difference between the weight of the mineral, and vessel in air and water, will be obtain'd; the weight, therefore, of the vessel, in water, being substracted from that difference, will give the weight of the fossil in water, and, consequently, its specific gravity in water.

How to examine earths, or soft substances, hydrostatically.

And to shew that this method wants not proper subjects, whereto it may be apply'd; we are told, that one of the best sorts of *Swedish* iron is often found in the form of a red mud, at the bottom of lakes, or other stagnant water: and I have observ'd *English* okers to be richer in iron, than some ores of that metal. And, an experienc'd writer, upon the gold and silver mines of *America*, takes notice, that gold, itself, is often found disguized in a reddish earth. *Vannochio*, also, a famous *Italian* mineralist, tells us, that a reddish sort of earth, sometimes, contains the richest metals. I, myself, have observ'd some finely figur'd crystals, to grow in a red earth. And, lastly, a traveller presented me with a certain earth, which he affirm'd to come from the diamond-mines; and this, also, I found to be red.

Colour'd sands, and gravel.

But a mineralist may make a more advantagious use of our hydrostatical bucket, by employing it in weighing colour'd sands and gravel. And, to shew how apt we are to over-look sands, for want of trying them by weight; I have, sometimes, seen a sort, slighted as worthless, which being wash'd, and view'd thro' a microscope, appear'd like a heap of small granats, and, perhaps, were really so. But what is more extraordinary, having observ'd, that the black sand, usually employ'd to dry fresh writing, seem'd, manifestly, heavier than the common; I examin'd it by the hydrostatical bucket, which gave us its specific gravity to water, as about 4.6 to 1; and, by melting it down, with two or three parts of antimony, and casting it into an iron cone, I found it of an irony nature; and, by applying it to a load-stone, to be far richer in metal, than any of our *English* iron-ores; for, at least, seven parts in eight, would easily be taken up by the magnet. But such observations as these, are not my chief motives to recommend the examination of sands and gravel to the mineralist.

'Tis

'Tis well known, that *European* traders yearly bring large quantities of gold from the coaſt of *Guinea*, which is waſh'd, or pick'd out of the ſand. And, even in *Europe*, there are rivers whoſe ſand is inrich'd by grains of gold. For this, the *Tagus*, that runs by *Lisbon*, and *Pactolus*, were famous among the ancients. An induſtrious chymiſt aſſur'd me, he got gold, with profit, from the ſand found on the banks of the *Rhine*: and there is a little river in *Savoy*, on the banks whereof, after a land-flood, I ſaw poor people employ'd in ſeeking for grains of gold. Some trial, alſo, that I cauſed purpoſely to be made, confirm'd me in a perſuaſion, that the ſands, of many places, if ſkilfully treated, by chymiſtry, may afford much more gold than is got from them in form of grains. For, beſides that there may be many particles of gold, ſo very minute, and cloſely fix'd to grains of ſand, as not to be taken notice of by the eye, or prove ſeparable by waſhing or picking; there may, as I conceive, be many ſmall portions of that metal incorporated with the body of the ſand; which a ſkilful artiſt, by the help of proper additions, might ſeparate to good advantage; eſpecially, if with litharge, or minium, the ſand be firſt reduced to a glaſs; and then care be taken to get the volatile gold, by giving it a pure body, fit to retain, and fix it; ſuch, for example, is fine ſilver; out of which, I remember, we ſeparated, by quartation, from a crucible full of vitrified ſand, and two or three fluxing materials, of ſmall price, ſixteen grains of pure gold.

Volatile gold is, I know, by many learned men, look'd upon as a fictitious thing; yet I have, by the help of an addition, inconſiderable as to bulk, and more ſo, as to weight, without a naked fire, and in a glaſs retort, ſublimed crude gold; ſometimes in the form of a yellow ſalt; and ſometimes, when the operation ſucceeded better, in that of thin cryſtals, prettily ſhaped, gloſſy, and as red as rubies: this by the bye. It may, perhaps, be more uſeful to ſearchers after rich foſſils, to obſerve, that when they meet with ſands, earths, mineral fragments, &c. whoſe ſpecific gravity but little exceeds that of cryſtal,; and yet, by the place wherein they are found, or by other tokens, give hopes of their containing particles of gold, they ſhould not haſtily reject them. For, having ſometimes diſcover'd corpuſcles of iron, and ſteel, in a vaſt variety of foſſils, and diſguizes, I ſuſpect that ſome golden particles may lie conceal'd in ſeveral bodies, which are thought to contain no metal; and ſtill more in metalline ores.

Ores in general; and that of lead in particular.

But thoſe who would apply hydroſtatics to ores in general, ſhould endeavour to procure ſpecimens of different mines, eſpecially if they be found in the ſame country; and either by trial, or ſtrict inquiry, inform themſelves what proportion of the reſpective metal they contain. For theſe portions of ores, and minerals, being carefully weigh'd in air and water, and their ſpecific gravities thereby known, may ſerve for a kind of ſtandard, wherewith to compare, hydroſtatically, the metalline portions contain'd in other parcels of ore, of the ſame ſpecies. Thus, for inſtance, all our *Engliſh* lead-ores, worth taking notice of, may be divided into three

STATICS. three kinds; and, in each of theſe, a latitude allow'd for greater or leſs degrees of goodneſs. Of the firſt ſort are thoſe that, in the ordinary ways of melting, hold, ſome of them, from thirty to forty pounds of lead in an hundred weight; and others, to forty-five pounds; which are often ſlighted, as ſcarce worth the working. The ſecond ſort may reach from forty-five to ſixty pounds in a hundred; but the moſt uſual proportion, I have found, in my trials, hath been about half the weight of the ore, in clean and malleable lead. Theſe are thought indifferently good, and worth the working; but others, comprized in this ſecond ſort, hold about fifty-five, and ſome near ſixty pounds; and theſe we look'd upon as pretty rich. As for the third ſort, it may conſiſt of thoſe that yield from ſixty to eighty pounds in the hundred; and ſuch ores are juſtly reputed very rich, eſpecially if they come near up to eighty pounds. I confeſs, I never met with any that reach'd ſo far; but was aſſur'd by the maſter of his majeſty's mint, that he had found ſome ſuch. Theſe look as if they were all metal; and I have obſerv'd ſome lumps thereof to be compos'd of ſeveral large cubes, very firmly adhering to one another.

As to the ores, preſented me from different countries, I have inſerted their ſpecific gravities, in the table annexed to this treatiſe; ſuppoſing it may be of uſe, in making a conjecture, with other concurrent circumſtances, whether a mine may be advantagiouſly wrought; but, concurrent circumſtances are, by no means, to be neglected.

SECT. II.

The hydroſtatical balance applied to the Materia medica; *and firſt, to the* Lapis Hæmatites.

WE come now to the ſecond part of our deſign; that is, to apply the hydroſtatical balance to the *Materia medica*. And firſt, there is a deeply red, and opake hard mineral, call'd, in the ſhops, by the name of *Lapis Hæmatites*, which, tho' little uſed by our *Engliſh* phyſicians, is, in ſeveral places abroad, highly eſteem'd, and, in my opinion, not without cauſe; particularly for the ſomniferous quality to be obſerv'd in ſome of its preparations. Of the *Engliſh* ſort of this foſſil, we weigh'd, in a tender balance, a piece that chanc'd to amount to three ounces, two drams $\frac{1}{4}$, firſt in the air, and then in water; and found its proportion to that liquor, as 4.15 to 1; that is, almoſt twice as heavy as a mere ſtone of equal bulk. So great a weight, confirm'd me in the conjecture I had made, that this ſtone contain'd a large proportion of metalline ſubſtance. I therefore ſublimed it, when finely powder'd, and diligently mix'd it with a double weight of ſal-armoniac, and found it, as I expected, very aſtringent upon the tongue, like ſeveral preparations of iron; and, for a farther proof, having put leſs than a grain of it into a ſpoonful, or two, of a ſtrong infuſion of galls, it immediately produced a black and inky mixture.

To the Lapis Lazuli.

Lapis Lazuli is ſometimes uſed by *European*, but more frequently by the *Arabian*, and other eaſtern phyſicians; and, particularly, as an emetic; which faculty I thought it poſſeſs'd, upon account of ſome metalline ingredient; and, accordingly, having hydroſtatically examin'd a piece, that was judg'd moderately rich, we found the proportion thereof to an equal bulk

bulk of water, as 3 to 1; which argues, that notwithſtanding its briskneſs in operation, it contain'd a much ſmaller proportion of metalline ſubſtance, than *Lapis Hæmatites*, or ſeveral leſs violent minerals.

The magnet. The load-ſtone is applicable to medicinal uſes, and may have ſenſible operations upon the human body; for which reaſon we examin'd it hydroſtatically, and found the weight of a lump thereof, that I judg'd to be either *Engliſh*, or *Norwegian*, was, in proportion to water of the ſame magnitude, as 4.93 to 1.

Calamine. *Lapis Calaminaris*, is frequently uſed in phyſic, eſpecially by chymiſts, to abſorb acidities; for which purpoſe I prefer it to ſeveral more famous drugs; but though 'tis uſually employ'd only as an external remedy, yet ſome uncommon chymical preparations of it, make me think it deſerves to be further examin'd. A famous empiric candidly aſſured me, that the medicine he with great ſucceſs made uſe of, againſt fluxes, was nothing but pure, and well-ground *Lapis Calaminaris*; hence I readily conjectur'd, that it participates of a metalline nature: which may alſo be argued from its operation upon copper, which it turns into braſs: wherefore, weighing a piece of this foſſil, we found it to water, as 4.69 to 1.

Let it be here obſerv'd, that tho' when a hard foſſil is found to be much heavier than cryſtal, of the ſame bulk, 'tis very probable, the ſolid concrete contains a conſiderable portion of ſome metalline, or ponderous mineral body; whence its good or bad qualities, with regard to the animal œconomy, may, probably, be deduced; yet 'tis very poſſible, for a foſſil to be endow'd with medicinal virtues, or noxious qualities, on account of its extraneous matter, tho' its ſpecific gravity but little exceeds that of cryſtal, or the difference be inconſiderable; for a very ſmall proportion of adventitious metalline, or mineral ſubſtance, if it be of a violent nature, may, in ſome caſes, diffuſe it ſelf thro' the reſt of the maſs, and impregnate it with active qualities; as we ſhall ſee hereafter.

Red coral. But further, this method of examination, may aſſiſt us to gueſs, with probability, whether a mineral body be of a ſtony nature, or not. Thus coral, for inſtance, is, by ſome, thought to be a plant; by others, a *Lythodendron*; but, by the greater number, a precious ſtone; in which difference of opinions, its ſpecific gravity may prove of conſiderable ſervice. We, therefore, weigh'd a piece of choice, and well-colour'd red coral, firſt in air, and then in water, and found its proportion, to an equal bulk of the latter, to be as 2.68 to 1: whence their opinion, who take it for a ſtone, ſeems moſt probable; ſince its ſpecific gravity exceeds that of cryſtal.

Pearl. Pearls, beeauſe of their hardneſs, are often thought of a ſtony nature. A monſtrous one being preſented me, that was taken out of an oyſter, its ſhape irregular, its magnitude extraordinary, and its aerial weight amounting to 206 grains; I weigh'd it in water, and found it to be thereto as 2.51 to 1: ſo that its ſpecific gravity nearly equall'd that of cryſtal.

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Calculi humani.

Calculi humani are frequently supposed to be true and genuine stones; and, indeed, the great hardness of several of 'em, may not improperly entitle them to that appellation: however, I should rather call them animal stones, than simply stones; which, by our method, may be easily distinguish'd from those, and other the like hard concretions, found in the bodies of animals. For, having a considerable number of these animal stones; I found, not only by chymical analysis, that they belong'd not to the mineral kingdom; but, by an hydrostatical examination of several of them, I found they differ'd greatly in specific gravity from true fossil stones; one of them, weighing above six drams and a half, was found to be in proportion to an equal bulk of water, as 1.76 to 1; and another, that weigh'd four drams and above a half in the air, being also weigh'd in water, appear'd to be thereto as 1.69 to 1.

I mention these stones as belonging to the *Materia medica*, tho' they are look'd upon rather as morbific substances; because a famous physician, who practised long in the *East-Indies*, and had better opportunity than any *European* had before him, to try the virtue of bezoar, either equals or prefers them to oriental bezoar.

And bezoar.

We made the like experiment upon bezoar-stones; the first of which weighing in the air three drams, and odd grains, was found to be in proportion to water of the same bulk, as 1.47 to 1. Another, weighing somewhat less than three drams, prov'd to an equal bulk of water, as 1.53 to 1. A third, being the nucleus of a larger stone, weigh'd in the air two drams and fifty-one grains; and its proportion to water of the same magnitude, was found as 1.55 to 1. In which several instances, we may observe, that these animal stones, not amounting to twice the weight of an equal bulk of water, have a less specific gravity, by above a fifth part, than a true fossil stone.

The hydrostatical balance will distinguish between bodies of the same denomination.

Again, we may by hydrostatics be assisted to discover the resemblance or difference between bodies of the same denomination. Thus, for instance, we have found a notable difference between the specific weights of several load-stones dug up in different countries or mines: and if a greater number and variety of experiments of this kind were made, we might possibly find, that, *cæteris paribus*, the magnets of one country or mine, are considerably heavier than those of another: for I usually observe, that the *English* and *Norwegian* load-stones are heavier *in specie* than those which are said to come from *Italy*. And this difference of weight between fossils of the same kind, when considerable, may help us to distinguish between the stones of the same lowest species, that are peculiar to different countries or mines. But in case the unequal weight proceed, as it often does, from an adventitious matter insinuated into the more genuine part of the fossil, whilst that remain'd fluid or soft; it may enable us to conjecture at its greater or less purity; which, on many occasions, may be of no small use to a physician, a jeweller, or a naturalist.

Between genuine stones and counterfeit.

Another considerable advantage of our hydrostatical method, is, that it helps us to discern genuine stones from counterfeit, which too often pass for

for true, to the great prejudice of phyſicians and their patients, and the great loſs of lapidaries and their cuſtomers: for as there are few qualities of bodies more eſſential than their ponderoſity; ſo there is ſcarce any wherein impoſtors find more difficulty to make a notable alteration, than in this, without being diſcover'd. In ſeveral caſes, indeed, 'tis not very difficult to alter the ſpecific weight of a particular body, yet it may, perhaps, be impracticable to make a conſiderable change in that quality, unleſs by ſuch additions or operations, as will cauſe a ſenſible alteration in ſome other qualities too, and thereby ſubject the fallacy to a diſcovery. And this will prove more particularly difficult to vulgar cheats, or adulterators of gems, and other valuable minerals; becauſe the ſmall knowledge they have of the number and variety of natural and artificial productions, confines them within narrow bounds, to accompliſh their fraudulent deſigns. And whilſt they are intent upon counterfeiting only the more obvious qualities of things, and, perhaps, upon eluding the known and popular trials; they will, probably, neglect the ſpecific gravity. By this means, a pearl, for inſtance, is diſcoverable to be counterfeit, without the leaſt prejudice to it. And I remember, ſome factitious corals, which I made, to ſhew what might be done in that kind, were, notwithſtanding their fine colour, ſhape, and gloſs, eaſily diſcoverable by their ſpecific gravity manifeſtly exceeding that of natural corals.

And, ſometimes, I have made paſtes, or factitious gems, with red lead; which, tho' tranſparent, and finely colour'd; yet by reaſon of their weight, were very liable to be detected by hydroſtatical trials. I have ſeen a fair adulterate bezoar-ſtone ſo reſembling the genuine, that a great price was ſet upon it; but being brought to me for my opinion, I made no doubt of its being counterfeit, from its appearing as heavy, as a mineral ſtone of the ſame bulk.

And ſhew the genuineneſs and purity of bodies.

Another uſe to be made of our hydroſtatical method of examining ſolids, is, on many occaſions, to aſſiſt us in making eſtimates of the genuineneſs, or the degree of purity, of ſeveral bodies, that may be uſefully employ'd in phyſic; provided they are heavy enough to ſink in water: for when we have once found the ſpecific gravity of a concrete, which we know to be good and genuine; the degree of its ponderoſity may ſerve for a kind of ſtandard, whereby to judge of other portions of the ſame denomination.

A great difference ſhould, doubtleſs, be made between the eſtimate of ſome ſtones, vulgarly called gems, according as they ſerve the purpoſes of jewellers and goldſmiths, of phyſicians or chymiſts: for the tradeſman, who uſually aims wholly at the beauty and luſtre of a ſtone, may juſtly eſteem thoſe, *cæteris paribus*, the beſt, which are *in ſpecie* lighteſt; becauſe ſuch are generally more uniform as to ſenſe, and more tranſparent; and alſo receive their colour from pigments of finer parts. But, on the contrary, thoſe who ſeek for medicinal virtues in gems, may juſtly value ſuch the higheſt, as are moſt ponderous; becauſe they are more plentifully ſtock'd with metalline or mineral ſubſtances: whence the greateſt part of their virtue is, in all probability, derived. And this difference in the

 ſpecific

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And further to manifeſt, that a little metalline matter may ſuffice to give a tincture, and thereby impart a virtue to a vitreous body, and even to gems; I ſhall add another experiment. I had long conjectured, that there was in granats, eſpecially in ſome of a deep colour, abundance of chalybeate corpuſcles, more than ſufficient for the granat itſelf: upon this ſuppoſition, I took a *German* granat, which I had kept by me for a rarity, on account of its bigneſs and deep colour. Of this, carefully reduced to very fine powder, we exactly mix'd eight grains with an ounce of finely pulveriz'd cryſtalline glaſs, and afterwards kept the mixture for two hours in a very hot furnace; by which means, we obtain'd a pretty uniform maſs, ting'd of a green colour, like what prepared iron or ſteel gives to pure glaſs.

Thus much may ſerve in ſome meaſure to ſhew the uſe of the hydroſtatical method of examining drugs; upon a ſuppoſition that they are ſolid, of a ſufficient bulk, and gravity able to ſink them in water. But many ſimples, and other ponderable ſubſtances, that may, upon good grounds, be refer'd to the *Materia medica*, want one or more of theſe conditions: I ſhall, therefore, offer the expedients I make uſe of in ſuch caſes, to bring them to this hydroſtatical trial. Now a body may either be liquid, and conſequently not immediately applicable to a ſlender ſtring; in the form of a powder, or ſmall fragments, and impoſſible, or difficult at leaſt, to faſten each of them to a hair, and ſuſpend it as a body of a greater bulk; or laſtly, diſſoluble in water, and conſequently unfit to be weigh'd therein.

This method of examination applied to fluids heavier than water, and unapt to mix therewith; for inſtance, to mercury.

Suppoſe, then, the liquor we would examine, be ſpecifically heavier than water, and unapt to mix therewith; I take ſuch a bucket, or wide-mouth'd glaſs, as we formerly mention'd, capable of containing an ounce or two of common water, and whoſe weight in air is about three or four drams. This glaſs we weigh very carefully, once for all, firſt in air, and then in water; and thereby find a weight equivalent to it in water: which being put into the oppoſite ſcale of the balance, whilſt the veſſel hangs under the ſurface

of

of the water, may be consider'd as having no weight at all; and, consequently, the additional weight of this bucket, may be alone taken for that of the contain'd body itself in water: so that, for instance, the bucket thus makes a mass of quick-silver, tho' fluid, as fit for weighing as if it were coagulated into a solid. And in this manner we proceed to weigh it hydrostatically, as if it were a solid.

But, for the more ready performance hereof, 'tis convenient to have in readiness a couple of weights, the one equal to the weight of the glass-bucket in the air, and the other to the weight of the same in water; for these immediately afford a counterpoise to the vessel in either medium. And, thus, we put a small glass-jar, capable of containing half an ounce of water, into one scale of a tender balance, and furnish'd the opposite with a counterpoise thereto; into this little vessel we pour'd 480 grains of *Spanish* mercury, and suffer'd the bucket, with this mercury in it, to hang by an horse-hair, from one of the scales, in a deep glass-vessel of water: there was now in the opposite scale a counterpoise to the weight of the glass in the water; so that the weights that were added, gave us the weight of the quick-silver only; the weight of the glass being already allow'd for. By this means we found the weight of the quick-silver, in water, to be 446 grains; whereby the greater number being divided, the quotient is 14, and about $\frac{1}{10}$: so that the mercury appear'd to be in gravity, to water of the same bulk as 14.11 to 1: tho', in former trials, I scarce found common quick-silver to weigh full fourteen times, and sometimes scarce thirteen and a half so much as an equal bulk of water. Whether the weight of the present mercury proceeded from its participating of gold more than others; which, from another experiment, seem'd not improbable; I will not here determine.

However, upon abundance of tryals of this kind, I did not find that all running mercuries, tho' they appear'd unadulterated, were precisely of the same weight. Nay, I observ'd, that even distill'd mercuries, if once combined with metalline bodies, and particularly if drawn from fine gold, differ'd more from the common sort, than these from each other: and, also, between common mercuries distill'd, we found a notable difference. And, by this method, tho' not always with the same ease, we may examine the specific gravity of other heavy fluids that are indisposed to mix with water; such as are, for instance, the chymical oils of cinnamon, cloves, guaiacum, *&c.* But what made me the more circumstantial in delivering the foregoing experiment, was, that this practical direction for weighing one liquor in another, will, hereafter, appear applicable to very useful purposes.

To powders and fragments of bodies.

The way of hydrostatically examining the powders and fragments of sinking bodies, hath been already touch'd upon; and differs little from the former: only it deserves to be again repeated, that great caution must be here used to moisten the powder with the same water wherein 'tis to be weigh'd, before the experiment is began; that the liquor may have time to insinuate itself between the dry corpuscles, and thence expel the air that

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If this way of examining bodies be carefully practiſed by a skilful hand, and with a tender balance, it may prove of conſiderable uſe to phyſicians, druggiſts, apothecaries, lapidaries and goldſmiths. Thus, for example, the fragments of the five precious ſtones, that are made ingredients of *Confectio Hyacinthi*, may each ſort of them, a-part, be properly examin'd by their weight in water, when the true ſpecific gravity of a parcel of the fineſt is once known. And, by the way, tho' granats are reckoned among the five precious medicinal ſtones, and, in ſome diſpenſatories, are prefer'd as the beſt; I have found a great difference, in point of ponderoſity, between *European* and *American* granats, compar'd with others, which I myſelf took out of an odd *American* mineral; whence, 'tis natural to conclude, that their virtues might be very different, if not as to kind, yet at leaſt as to degree. And not only factitious pearls, that often paſs for genuine, may often, by this expedient be diſcover'd, eſpecially if mercury enter their compoſition; but we may, probably, by the ſame method, diſtinguiſh the natural pearls of ſeveral kinds, and different countries; whereof I have ſeen, and poſſeſs a far greater variety than would eaſily be credited. But, becauſe it more concerns phyſicians, and their patients, to make an eſtimate of ſeed-pearls, than of the larger ſort, that are ſeldom uſed but for ornament; I ſhall here obſerve, that, when furniſh'd with very fine oriental ſeed-pearls, which ſeem'd proper for a ſtandard wherewith to compare others, we found them to water, of the ſame bulk, as $2\frac{3}{4}$ to 1.

To bodies that will diſſolve or mix with water. But what courſe muſt we take, when the body to be hydroſtatically examin'd, will diſſolve, or eaſily mix with water? Why, tho' the propoſed body cannot be immediately weigh'd in water; yet we may ſubſtitute another liquor, that will not diſſolve it, and thereby find its ſpecific gravity to that medium; and then, by comparing the difference of thoſe two liquors, in point of gravity, we ſhall diſcover the body's weight in water; ſuppoſing it could have been kept undiſſolv'd therein. Conſidering, then, that, except quick-ſilver, the viſible fluids we can command, are either of an aqueous, or oily nature; and that moſt bodies, whereof we can make ſolutions in liquors of the former kind, will not diſſolve in thoſe of the latter; we preſume, that the moſt ſaline bodies, ſuch as alum, vitriol, ſal-gem, borax, ſublimate, *&c.* may be commodiouſly weigh'd in oily fluids, as oil of turpentine, for inſtance, or rather the ſpirit of turpentine, which I, in this caſe, prefer to any other fluid.

And here, to avoid the trouble of calculation, we may, often, eaſily find the ſpecific gravity of bodies to others of the ſame, or a different ſpecies; and ſo make a probable gueſs as to their genuineneſs, by being provided with one piece of the body, of known purity, to ſerve as a ſtandard.

dard. Thus, for example, if the goodneſs of ſublimate be ſuſpected, take an ounce of ſome that is known to be fine, carefully balance it in oil of turpentine, and note its weight therein; then, an ounce of the ſuſpected ſublimate being weigh'd as the former, if it prove of the ſame gravity with the ſtandard, it may be ſuppoſed good; but if it weighs leſs, 'tis a ſign it wants a due proportion of mercury. The ſame method is applicable to *Mercurius dulcis*, and many other bodies, wholly, or partially diſſoluble in water; as alum, which is often ſophiſticated with ſome baſer ſalt; to *Roman* vitriol, which is, ſometimes, either counterfeited, or adulterated, by roch-alum and a tincture of copper: and, according as the weight of the body in oil recedes more or leſs from that of the ſtandard; ſo the adulteration may be probably concluded leſs, or greater.

Another way of finding the ſpecific gravity of fluids.

I might, alſo, obſerve another uſeful way of eſtimating the gravity of ſolids and fluids, by taking a heavy ſolid, and carefully obſerving, once for all, its weight in the air; then weighing the ſame, firſt in one of the liquors to be examin'd, and afterwards in others: whence, the difference between the ſolid, and each of the fluids being obſerv'd, 'tis eaſy to find the ſpecific gravity of each, and to aſſign the proportions betwixt them.

But if only one ſolid be employ'd, 'tis neceſſary ſuch an one be choſen; as hath a much greater ſpecific gravity than is barely neceſſary to ſink it in water; becauſe ſome liquors are much heavier than that: nay, it ought to be heavy enough to ſink in all fluids, except quick-ſilver, if 'tis deſign'd for experiments in general. But 'tis not very eaſy to pitch upon ſuch a ſingle ſolid as hath all the qualities requiſite to our purpoſes: for, it ought not to loſe any of its weight by the inſenſible avolation of effluvia, yet be of a conſiderable ſpecific gravity, and not too large, or intenſely heavy, leſt it overload a tender balance, or require too much liquor to ſurround it; its texture ſhould be proof againſt ſharp and piercing menſtrua; it ſhould, alſo, be of a make not liable to be broken or ſpoil'd; and, laſtly, of a natural, uniform, and procurable ſubſtance.

Brimſtone, hard wax, ivory, and white marble, have been, when properly ſhaped, ſeverally made uſe of for this purpoſe; yet they all wanted one or other of the deſirable qualifications mention'd. I, therefore, frequently employ'd three other bodies, as more proper. The firſt was a piece of amber, between three and four drams in weight, of a high yellow colour, very tranſparent, and of an uniform texture, and convenient figure. This we apply'd to examine the lighter ſort of liquors, ſuch as water, wine, brandy, *&c.* but 'tis not proper for the more ponderous liquors, ſince 'twill float at the top thereof.

The ſecond is a globular glaſs, which I caus'd to be blown at a lamp, and to be hermetically ſeal'd at the neck, that was purpoſely made very ſhort, after there had been lodg'd therein juſt as much quick-ſilver as we gueſs'd would ſerve to ſink it in any fluid, except mercury. This, by reaſon of its great bulk, in proportion to its weight, was fit to diſcover minute differences in the gravity of the liquors 'twas weigh'd in, and could

STATICS. could not be corroded by ſharp menſtrua; and, therefore, on ſeveral occaſions, I prefer'd this inſtrument to either of the other.

But this being too liable to accidents, for experiments that are to be imparted, and recorded for poſterity; I made uſe of rock-cryſtal, which, on account of its numerous good qualities, affords a fit meaſure, whereto other bodies may be compared. Of this pure concrete, we employ'd an almoſt compleat globe, weighing in the air two ounces, thirty-three grai s; wherein were two ſmall holes near one another, that might eaſily be ſtopped up with hard wax, after an horſe-hair had been run thro' them; by means whereof it was eaſily faſten'd to the ſcale, and made to hang in the water. The bulk of this ſphere made it the more fit to diſcover ſmall differences between liquors, in point of gravity. But, becauſe we may have often occaſion to know the weight of fluids, which, by reaſon of their ſcarcity, we can command only in ſmall quantities; as chymical oils, tinctures, eſſences, *&c.* we provide for ſuch liquors an hexagonal priſm of natural cryſtal, with a kind of pyramid at one end; which, by reaſon of its oblong figure, might be commodiouſly weigh'd in a ſlender cylindrical glaſs, that required but a ſmall quantity of liquor to ſurround a body ſo conveniently ſhaped, and that weigh'd in the air but half an ounce, and ſixteen grains. And, to render the obſervations made with theſe two bodies upon medicinal, and other liquors, the more uſeful; let it be taken notice of, once for all, that the ball of cryſtal was to an equal bulk of water, as 2.57 to 1, or thereabouts; and the priſmatical oblong piece, as 2.*66* to 1, to the ſame.

I have been the more particular upon the way of trying the gravity of ſeveral liquors with one ſolid, becauſe a double application may be made of it on ſeveral occaſions, ſerviceable to chymiſts, phyſicians, apothecaries, and others.

The ſeveral uſes thereof. And, firſt, a piece of clear amber, or ſome ſuch convenient body, that is not too little, nor, *in ſpecie,* too heavy, may ſerve the chymiſt, apothecary, *&c.* to make probable gueſſes at the degree of ſpirituoſity, or tenuity, to be found in many liquors belonging to the vegetable, or animal kingdom; and this with far leſs error, than by thoſe ſigns whereon the common conjectures are grounded. For, a ſtandard-liquor being provided, 'twill be eaſy, by obſerving the different weights of amber in ſeveral fluids, to judge of the fineneſs of any of them in its kind; for, *cæteris paribus,* that is the thinneſt, or abounds moſt in ſpirituous parts, where the ſolid weighs more than in another; as, for inſtance, the amber we employ'd, that in water weigh'd *6* $\frac{3}{4}$ grains, weigh'd in common red *French* wine 8 $\frac{1}{2}$ grains; in common brandy, of a pretty good ſort, 17 $\frac{1}{8}$ grains; and in ſpirit of wine, highly rectify'd, 34 $\frac{1}{8}$ grains. The ſame way may be employ'd to judge of the ſtrength of ſpirit of vinegar, *&c.* But here it muſt paſs for a general rule, that, probably, as in liquors the ſolid weighs more or leſs, according as the liquor 'tis weigh'd in, is more or leſs ſpirituous; ſo, on the contrary, in acid ſpirits and liquors, the leſs the ſolid weighs, the ſtronger the fluid muſt be reputed; that greater decreaſe

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of weight proceeding uſually from the greater proportion it contains of fixed ſalts.

'Tis another advantage of this method, that as the common way of trying the goodneſs of ſpirit of wine, brandy, *&c.* by ſetting fire to a ſpoonful of them, to ſee how much is inflammable, and what portion of phlegm will be left behind; whereby, in tract of time, a conſiderable loſs is ſuſtain'd; the proper proof may be made in our way, without any ſuch expence.

By theſe obſervations, alſo, eſtimates may be made of undiſtill'd liquors of the ſame kind; as, ſeveral ſorts of beer, ale, cyder, *&c.* and the ſame ſolid may be employ'd to compare liquors of different kinds with each other.

But if the liquors to be examin'd are very ponderous; amber will not be a proper ſolid in this caſe: for I have found, by trial, not only that it ſwims or floats in ſeveral fluids made by ſolution of ſalts, whether in the moiſt air, or with water; ſuch as oil of tartar *per deliquium*, a ſolution of the ſalt of pot-aſhes, *&c.* but that ſome diſtill'd liquors would not ſuffer it to ſink to the bottom of them; as I found in oil of vitriol, ſpirit of nitre, and even good ſpirit of ſalt.

There is ſtill another uſe to be made of our hydroſtatical ſolid, which may frequently be as conſiderable, in general, by aſſiſting in proportioning the ſtrength of menſtrua; as the former is to diſcover the ſtrength of particular liquors already prepared. For there are many experiments that do not ſucceed ſo well, unleſs the liquor be of a determinate degree of ſtrength. Thus, I have found, that if *Aqua fortis*, whoſe ſtrength is reckoned the beſt quality it can have, were rectify'd too high, it would not diſſolve ſilver; but require to be weakned, by an addition of water; and that it would not near ſo well diſſolve the raſpings of crude lead, when only moderately ſtrong, and fit to diſſolve ſilver, as when conſiderably diluted with water. And in making extracts from many vegetable ſubſtances, chymiſts themſelves may fall into a miſtake, by affecting to employ their moſt rectify'd ſpirit of wine, as the beſt menſtruum for their purpoſe; for the medicinal virtue of many ſuch bodies does not wholly reſide in what they call their ſulphur, or roſin, which indeed is beſt diſſolved by highly rectify'd ſpirit of wine; but in a more gummy, and, perhaps, mucilaginous ſubſtance, for whoſe extraction a moderately low ſpirit is more proper. Thus we ſee that gum-arabic, gum-tragacanth, *&c.* are indiſpoſed to diſſolve with the beſt rectify'd ſpirit of wine, but readily mix with more aqueous liquors. And ſome bodies, tho' diſſoluble in both kinds of menſtrua; yet open leſs eaſily to ſtrong ſpirit of wine, than wateriſh fluids; as may be obſerv'd, particularly, in myrrh.

And thus much may ſerve to ſhew how uſeful it will be, on ſeveral occaſions, to regard the degree of ſtrength of the menſtruum, or liquor, employ'd in any curious experiment; ſo that when it ſhould be repeated to the ſame purpoſe only, we may be able to bring the fluid we make uſe of, to the ſame degree of ſtrength with that before employ'd, which produced

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Still other methods for the same purpose. Besides the method just mention'd, there is another, which we have often found of service, in comparing the weight of different liquors of the same magnitude. This is done by successively filling a vial, furnish'd with a long, slender cylindrical stem, to a certain standing mark, made near the top, with the several liquors, to be, by weight, compared together. But this not being hydrostatical, I shall no longer insist upon it here. To proceed, therefore, there is, likewise, another way to discover whether two or more liquors proposed, differ in specific gravity; and to make some estimate of their difference: *viz.* by a hollow cylinder of brass, or other metal, made somewhat heavy at the bottom, that it may swim upright; which will sink more or less, in several liquors, as they are lighter or heavier than one another. But *Mersennus*, who proposes this way, confesses it to be very difficult to make sure observations thereby; to which I add, that the metal may be corroded, or otherwise affected by urinous menstrua.

What *Mersennus* said of this instrument, is applicable to a different one, made of two glass bubbles, and a very slender stem, which is hermetically sealed; with a ballast of quick-silver, in the lowest, to keep it steady, when partially immers'd in liquors; where like the metalline cylinder, it sinks deeper in the lighter than in the heavier, according to their differences in gravity. But tho' I have often employ'd this instrument, and found it serviceable, when I made use of several of different sizes, according to the various liquors I was to examine; yet it is, in many cases, inferior to the balance.

Mersennus, also, proposes another hydrostatical manner of weighing liquors in water, thus. Take a glass vial, to which, being first weigh'd in air, and then in water, you are exactly to fit a stopple of wax, or cork; this done, fill the vial with the liquor you would examine, so that no air be left between that and the stopple: the vessel, thus fill'd, is to be weigh'd in water; and substracting from its weight there, that of the glass in water, and also that of the stopple, the remainder will give the weight of the liquor in water. This may, perhaps, be serviceable, on some occasions, yet, I fear, it is troublesome, in practice; for, ordinary vials, capable of containing a competent quantity of liquor, are usually too heavy to be applied to tender balances; and common stopples will be subject to various inconveniencies: for which reason, I prefer to it a method I formerly thought of, and which I have, sometimes, put in practice, by chusing a small round vial; and, instead of ordinary stopples, fitting it with one of glass, carefully ground to the neck of it: for, by this means, the inconveniencies of a stopple, lighter than water, were avoided; nor would that alter its specific gravity, either by absorbing, or evaporating; nor would it be penetrated

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by the most subtle, or corroded by the most fretting spirits: to which may be added, (because, in some cases, it is considerable) that a glass stopple will not communicate any tincture, or extraneous quality to the liquor; which cannot be said of cork, or wax, in regard of some subtile, and very corrosive liquors. This vial, then, together with its stopple, being carefully weigh'd, first in air, and then in water, to settle the gravity of the whole instrument; we fill'd it exactly with the liquor to be examin'd, and proceeded as if we were to weigh quick-silver, in the manner already described: the weight of the given liquor in water being thus obtain'd, its proportion, to an equal bulk thereof, may be easily discover'd. This way of examining liquors has its use; and I the rather, sometimes, made choice thereof, because 'tis applicable to all kinds of fluids, whether heavier *in specie* than water, or lighter.

Laying aside the stopple, the round ball may serve, on several occasions, instead of the hydrostatical bucket, formerly mentioned; for the weighing of quick-silver, and heavy powders; especially coarse ones. But, if the instrument be fitly shaped, and not too heavy, a greater conveniency than this attends it: for when you have liquor enough to surround the bottle, it may be commodiously substituted for the hydrostatical bubble, with quick-silver; its exact stopple supplying the place of an hermetical seal: and, besides, 'tis far less subject to break than a bubble. That I made most use of, which weigh'd about 709 grains, being well stopp'd, with only air in it, would sink, by its own weight, in brandy, wine, water, *&c.* And, if it were to be employ'd in liquors much more ponderous than water, as *Aqua fortis*, oil of tartar *per deliquium*, *&c.* 'twas easy to fit it for them, also, by putting into it a quantity of quick-silver, of a determinate weight, before it be stopp'd; which balast, when the operation is over, may be easily taken out, and kept a-part for the like uses.

But, notwithstanding all this, since glasses of a proper size, shape, and weight, fit for tender balances, and furnish'd with exact glass stopples, are very difficult to procure; and since the way itself is subject to some inconveniencies, it seems, in general, that this way of finding the weight of liquors in water, is inferior to the more simple ones, before recommended.

The use and advantages of weighing one fluid in another.

Having now laid down the method of weighing one liquor in another, 'tis fit we subjoin some application of it.

Among other advantages of hydrostatics, to a sagacious physician, 'tis none of the least, that it gives him the specific gravities of various liquors; which may not only help to distinguish the genuine and good, from the adulterate and decay'd, but serve to other purposes also. Instances hereof are afforded by the juices of herbs, and fruits; a determinate quantity of which, being first weigh'd in the hydrostatical bucket; and some oil of turpentine poured on it, we sink it warily in the same liquor, whose specific gravity to refined silver, or clear rock crystal, is before-hand carefully registred: for thus substituting this oil for common water, we may discover the specific gravity of liquors that would mix with water. And,

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STATICS. by this means, we may find, not only the difference in weight, between the juices of plants of differing kinds, but, on ſome occaſions, obſerve how far the keeping of a juice, for ſome time, or the fermentation, or putrefaction thereof, will alter its ſpecific gravity. Other liquids, alſo, that are uſed in phyſic, but not ponderable in water, may, by this way, be examin'd; as honey, vinegar, verjuice, *&c.* And, by the ſame method, likewiſe, the ſpecific gravity of the juices of fruits, may be diſcover'd, and compared together. And hence a perſon of curioſity will, probably, be enabled to take notice of the differences produced in their ſpecific gravities, thro' their ſeveral ſucceſſive ſtates, at different times. Thus the juice of ripe grapes alters when newly preſs'd; again, when it begins to ferment; when it becomes new wine; when it has attain'd its full maturity, and perfection; and, laſtly, when it degenerates into prick'd wine, and abſolutely changes into vinegar, or *Vappa*.

All waters nearly of the ſame weight. It may be here proper to obviate an objection, that will, probably, be made againſt the method hitherto deliver'd, of finding a proportion in weight, betwixt a ſinking body, and water of the ſame bulk. Naturaliſts might alledge, that, by this method, we cannot diſcover the proportion between a ſolid body, and water in general; but only betwixt the propoſed body, and the particular water 'tis weigh'd in; becauſe there may be a great diſparity between liquors that are call'd common water. Thus ſome travellers tell us, that the water of the river *Ganges* is, by a fifth part, lighter than ours.

To this plauſible objection, I anſwer, firſt, that having had opportunity to examine the weight of various waters, ſome of them taken up in places very diſtant from one another, I found the difference between their ſpecific gravities to be exceeding ſmall; perhaps, not above the thouſandth part of the weight of either: nor did I find any conſiderable difference, between the weight of ſeveral waters of differing kinds; as between ſpring-water, river-water, rain-water, and ſnow-water: tho' this laſt was ſomewhat lighter than any of the reſt. And having received water from the river *Ganges* itſelf, I found it very little, if at all, lighter than ſome of our common waters.

And, ſecondly, 'tis not neceſſary, that the proportion obtainable by our method, ſhould be mathematically exact: for, in experiments, where we are to deal with groſs matter, and employ material inſtruments, a phyſical accuracy will ſuffice.

To diſcover the magnitudes of bodies, hydroſtatically. There is an uſe of hydroſtatics, which tho' it do not directly tend to the examination of the *Materia medica*, may yet be ſerviceable, both to the phyſician and the naturaliſt, in delivering deſcriptions thereof; and thereby indirectly conduce to the knowledge of drugs; and help to diſtinguiſh between genuine and adulterate ſimples. 'Tis known, that the writers upon the *Materia medica*, uſually ſet down the magnitude of the bodies they deſcribe, by very uncertain gueſſes; and thoſe who aſſign them determinate meaſures, either do it by ſaying, ſuch a fruit, for example, is an inch, or two inches long, and half an inch, or a whole one, broad: but, according to this way of describing bodies, there may, by reason of the great variety of figures they are capable of, be a very great difference in the bulk of bodies, to each of which, the same length and breadth are applicable.

I caused several cubes to be carefully made, by skilful artificers, of different sizes, and materials, whose sides were each of them an exact inch, or a precise number of inches. These cubes, being carefully weigh'd in exact balances, first in air, and then in common water, we concluded, from the result of our several trials, that, without considerable error, a cubical inch of water might be supposed to weigh 256 grains.

Suppose, then, for example, that a solid heavier *in specie* than water, having been weigh'd first in the air, be found in water to lose 256 grains of its weight; the dimensions of this solid, if it were of a cubical shape, will equal a cubic inch; so that if the given body be suppos'd a metal easily fusible, as tin, or lead, melted and pour'd into a hollow, metalline, cubic inch, and suffer'd to cool, it would exactly fill it. For a sinking solid weighs less in water than in air, by the weight of an equal bulk of water. And, as a cubical inch of water weighs 256 grains, it follows, that when the decrease of a body's weight in water, is 256 grains, the solid content of that body is a cubical inch; since a body of water, weighing 256 grains, is equal in magnitude, as well to the solid, as to a cubic inch of water.

And here it may prevent a scruple, to observe, that to make bodies equal in magnitude, it is not necessary that they should be of the same weight, or matter; as is evident in bullets of copper, tin, and gold, cast in the same mould. For, tho' they be all equal in bulk, yet the bullet of copper will be much heavier than that of tin; and the bullet of pure gold, more than twice as heavy as that of copper. Whensoever, therefore, a solid, ponderous enough to sink in water, loses therein 256 grains of the weight it had in air; the magnitude, or bulk of that body, is equal to a cubical inch; of whatever matter it consists, or, of what shape soever it be. And, in case the solid proposed, loses of its weight in the water, less than 256 grains; its bulk will be proportionably less than a cubical inch. Thus every 32 grains that the solid loses of its weight in the water, answers to an eighth of an inch in the bulk of the body: so that if the decrease be 128 grains, the solid will be half a cubic inch; and if but 64 grains, a quarter of a cubic inch: on the other side, if the decrease of the given body, exceed the standard, 256 grains, twice, thrice, *&c.* the body will be equal to two, three, *&c.* cubical inches.

To gain the solidity of a body, hydrostatically, tho' lighter than water.

'Tis easy, from the doctrine deliver'd, to discover, hydrostatically, the solid contents of a body, heavier *in specie* than water: but to measure, by the help of water, the solidity of a body lighter *in specie* than that fluid, is a work of more difficulty. In order to it, we may consider there are two sorts of bodies, which will not, naturally, sink in water; some, being of a closer texture, resisting it; and others abounding with pores, which dispose them to imbibe the water.

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Now the weight of a body of the firſt kind, may be gather'd from that of the water equal in magnitude to the immerſed part of the body, when it floats freely thereon. Thus, if a parallelopipid, or a cylinder of wood twelve inches long, placed upon water, ſhould reſt there when a twelfth part of it lies beneath the ſurface of the fluid; the weight of the water, equal in bulk to that immerſed twelfth part, would be equal in weight to the whole body of the wood. But becauſe the bodies, whoſe bulk phyſicians and chymiſts may have occaſion to examine, will very ſeldom happen to have regular figures; 'tis proper to add another method, more ſuitable to the preſent deſign. To meaſure then the ſolid contents of bodies ſpecifically lighter than water, but irregularly ſhaped*; the body muſt, firſt, be weigh'd in air. 2. A plate of lead, capable of ſinking this body in water, and of ſome round ſum in weight, ſhould be provided. 3. This plate being weigh'd in water, and its weight therein ſubſtracted from its weight in air, will give the weight of as much water, as is equal in bulk to the immerſed lead; or the ſpecific weight of the lead in water. 4. The plate of lead, and the lighter body, muſt be tied together with horſe-hair, and the weight of the aggregate noted. 5. This aggregate muſt be weigh'd in water, and its weight therein ſubſtracted from that it had in air; and the difference will give the ſpecific weight of the ſaid aggregate in water. 6. From this difference ſubſtract the ſpecific weight of the plate in water, and the remainder will give the weight of the lighter body in the ſame fluid. Then, laſtly, that weight of the light body in water being divided by 256 grains, will give its ſolid content.

To clear and confirm this method by an example; we took a conveniently ſhaped piece of oak, that weigh'd in air 193 $\frac{1}{2}$ grains; to this we tied, with an horſe-hair, a plate of lead, weighing juſt 240 grains. But, before we tied them together, the lead was weigh'd in water, where it loſt of its former weight 20 grains; which, being deducted out of the 240 grains, left a difference of 20 grains, for the ſpecific weight of the lead in the water. Then the aggregate of the wood and lead was weigh'd, firſt in the air, and found to be 433 $\frac{1}{2}$ grains, and next in water, where it amounted but to 162 grains; which being ſubſtracted from the aggregate of the ſame bodies in air, the difference was 271 $\frac{1}{2}$ grains; from which, the other difference of 20 grains of the leaden-plate in water being deducted, there remain'd 251 $\frac{1}{2}$ grains, for the weight of a bulk of water equal to that of the given piece of wood. Now, if this number of grains had amounted to 256, we might have concluded the ſolidity of the body

* There is a very eaſy method of obtaining the content, or cubic meaſure, of any ſolid, tho' ever ſo irregularly figured. For, ſince ſuch a body, immerſed in a fluid wherein it will not diſſolve, cauſes that fluid to riſe in direct proportion to the bulk immerſed; if the fluid be contain'd in a cylindrical or priſmatic veſſel, and the additional height given it by the body be marked on the veſſel; this will enable us to meaſure a part of the whole cylinder or priſm, equal in bulk to the irregular body. And thus may the ſolid content of ſtatues, carv'd or emboſs'd work, &c. be known.

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to be a cubic inch; ſince 256 grains of water, which we formerly found equal to a cubic inch of water, was alſo now found equal to the bulk of the given piece of wood. And having cauſed the wood I employ'd to be formed into a cubic inch; the difference of its weight in water from 256 grains, may, probably, be imputed to ſome little imperfection in the figure, or other like circumſtance. Firſt, then, the cube of oak in air, weigh'd 193 $\frac{1}{2}$ grains. 2. The lead in air, 240 grains. 3. The lead in water, 220 grains; which, ſubſtracted from its weight in air, leaves for its ſpecific weight in water, 20 grains. 4. The aggregate of the two in air, 433 $\frac{1}{2}$ grains. 5. The weight of both together in water, 162 grains; which, ſubſtracted from the ſum of their weight in air, gives the difference of the two ſeveral aggregates, 271 $\frac{1}{2}$ grains. 6. The difference between the weight of the lead in air, and in water, 20 grains; which, ſubſtracted from the difference of the weights of the aggregates in air, and in water, gives for the weight of the propoſed cube 251 $\frac{1}{2}$ grains.

This way of meaſuring bodies is appropriated to ſuch of them as will not readily diſſolve in water. But, becauſe there are many other ſolids, as ſalt, alum, vitriol, ſugar, *&c.* whoſe magnitudes it may often be proper to know, and compare; I ſhall add, that the ſame method is applicable, alſo, to ſolids diſſoluble in water, if, inſtead of that fluid, be ſubſtituted oil of turpentine, whoſe proportion of ſpecific gravity to water, is otherwiſe known. To diſcover this, I employ'd the hollow cubic inch of braſs, made uſe of to find the weight of a cubic inch of water; and found, that, when carefully filled, it contained 221 $\frac{1}{3}$ grains of this oil: by which number, the difference of the weight of a ſolid in the air, and in that oil, being divided, the quotient will give the ſolid contents of the body to be examined.

Let us now conſider how ſuch bodies as, by their poroſity, are ſubject to imbibe too much of the liquor, while the experiment is in hand.

Merſennus's expedient, in this caſe, is to cover the body to be weigh'd in water, with wax, pitch, or ſome other glue, of a known ſpecific weight in water. But I prefer bees-wax for this purpoſe; and proceed with it in the following manner. The ſolid which is lighter than water, having been firſt weigh'd in air, over-lay it carefully with thin bees-wax; then take, alſo, in the air, the weight of the wax employ'd; and faſten to the body thus coated, a plate of lead, or tin, heavy enough to ſink it; and obſerve the weight of the aggregate in water. This done, ſubſtract the weight of as much water as is equal in bulk to the wax, and proceed as is before taught.

But this method helps us only to the weight of the propoſed body in water; to diſcover its ſolid content, we muſt divide the weight of the ſolid in water, by 256 grains.

And, by the way, I have, ſometimes, made uſe of another expedient to hinder ſmall ſolids, whether lighter or heavier *in ſpecie* than water, from imbibing the liquor wherein they were weigh'd: for having firſt found the weight of a cubic inch of quick-ſilver; we placed the body to be meaſured, STATICS. sured, in a vessel, whose solid contents were known before-hand; when, the space that remain'd unpossessed by the firm body being fill'd with quick-silver, 'twas easy to know, by the difference in weight of that quick-silver, from the weight of the quick-silver requisite to fill the whole vessel, how much thereof was equal to the surrounded body. And, by this means, and the knowledge before gain'd of the weight of a cubical inch of mercury, the solid content of the body proposed, was easily obtain'd.

What accuracy is to be expected in hydrostatical experiments.

Before I put an end to this discourse, 'tis proper to shew what credit may be given to the estimates of the weights and proportions of bodies obtain'd by hydrostatical trials; because mathematicians, either not knowing, or not applying our observation about the specific gravity of rock-crystal, and the nature of oil of turpentine, have given us very different accounts of the proportions of metals, and a very few other familiar bodies, which are all they seem'd to have examin'd by this method.

And, indeed, I should not be surprized to find, that the experiments of the same person, made at distant times, and under different circumstances, disagreed; for some difference there may be betwixt the waters employ'd in this case; especially if the air be at one time intensely hot, and at another exceeding cold. The difference, also, of degrees in goodness of the balances used in nice experiments, is not altogether inconsiderable. But a greater hindrance to the accuracy of hydrostatical experiments, is the difficulty of finding an exact uniformity in weights of the same denomination, which are vulgarly supposed to be exactly equal. I have myself found it so difficult in practice to procure, and keep weights as exact as I desired, that I have left off the hopes of it; for the very air may, in time, cause an alteration in them. And tho' the accurate *Ghetaldus*'s tables of the weight of metals, and some other bodies, in respect to one another, are look'd upon as the most authentic that have been publish'd; and are, accordingly, the most made use of; yet 'tis certain, the weights he employ'd are not divided as ours are. For tho', according to him, as well as with us, the ounce consists of four and twenty scruples; yet the scruple, which with us is divided but into twenty grains, he divides into twenty-four. But tho' hydrostatical experiments are not, always, either singly accurate, or exactly agreeable to one another; they prove accurate enough to be very useful in practice, and more exact than any other method, hitherto employ'd, of determining the proportions of bodies, in point of weight and bulk, and of measuring their solid contents; but, especially, such little ones as are necessary to be examined in the *Materia medica*. And this is a corollary from the whole of what we have deliver'd.

And, indeed, as little as my skill is in hydrostatics, I would not be debarr'd from the use of it, for a very valuable consideration; for it has already done me acceptable service, and on a vast variety of occasions; especially in the examination of metals and mineral bodies, and of several chymical productions. I have often been able, by its means, to undeceive artists in their persuasion of possessing *Luna fixa*, and other valuable commodities; and to make a judgment, as to the genuineness

or

or falsity, and as to the degrees of worth or strength of many rich and poor metalline mixtures, and other bodies, both solid and fluid; whose fair appearances might, otherwise, have greatly deceived me. STATICS.

And, to shew some curious persons how far hydrostatics might be serviceable to as accurate mensurations as need be expected in physical experiments; I desired a virtuoso to mix tin and lead in a certain proportion, unknown to me; and melt them into one mass; which I carefully weigh'd in water, and also examined it algebraically; and from hence assign'd the respective quantities of each; which agreed, within little more than a grain, with those he had committed to paper, before he mixed them. And this small difference, probably, proceeded from some scarce avoidable inaccuracies in melting and managing the given bodies.

To conclude; 'tis not to be expected, that the specific gravities of the bodies mention'd in the following table, shall, all of them, be found, in future tryals, precisely the same as we there exhibit them. For, besides that experiments are made by persons of different qualifications, with different degrees of care, and with different instruments; * the varieties may proceed from a difference in the texture and compactness that may be found in several bodies of the same kind. For, neither nature, nor art, give to all the productions of the same name, a mathematical preciseness, either in gravity, or other qualities.

* Dr. *Jurin* recommends it, as a necessary caution, to all those who shall attempt to weigh dry, porous solids in water, for philosophical purposes, that, by some proper means, they first extricate the air out of all the small pores and cavities of them; whereby the water may have free liberty to enter thereat: otherwise, the air contain'd therein, by keeping the water out, will render the solid lighter in water than it really is. The best way of avoiding this inconvenience, the Doctor tells us, is to set the vessel of water, wherein the solid is immersed, under the receiver of an air-pump, and extract the air out of the body; which will succeed the better if the water be first heated over the fire: or, in defect of an air-pump, he directs us to let the solid, if its texture will safely permit, continue for some time in boiling-water, over the fire. To a neglect of this observation, the Doctor thinks, among other things, is owing that difference found in the accounts of the specific gravity of the *Calculi humani*. And when the air is thus extracted out of wood, as oak, fir, &c. and the roots, stalks, leaves, and seeds of vegetables, they prove specifically heavier than water. *Philos. Trans.* N°. 369. p. 223.

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A TABLE of the specific Gravities of Bodies compared with Water. *

	Weigh'd in Air.	In Water.		Proportion.	
AGate	251	156	Grains.	2.64	to 1
A piece of alum-stone	280¼	152¼		2.18	
Amber	306	12		1.4	
Antimony, good, and supposed *Hungarian*	391	295		4.7	
Bezoar stone	187	61		1.48	
A piece of the same	56½	22		1.64	
A fine oriental one	172	60		1.53	
Another	237	61		1.34	
Calculus humanus	2570	1080		1.72	
Another	302	97		1.47	
Calx of lead	138½	123		8.94	
Cinnabar, native	197	171		7.57	
Cinnabar, native, and very sparkling	226	194		7.6	
Cinnabar common	802	702		8.1	
Cinnabar of antimony	197	169		7.3	
Coco-shell	331	85		1.34	
Copper-ore	1436	1090		4.15	
Copper-ore, rich	413	314		4.17	
Copper-stone	65½	49½		4.09	
Coral, white	336	204		2.54	
Another piece, fine	139	85		2.57	
Coral, red	129¼	80¼		2.63	
Cornelian	148	103		3.29	
Crabs eyes, native	77½	36½		1.89	
Crabs eyes, artificial	90½	54		2.48	
Crystal	256	140		2.21	
Gold-ore, not rich, but brought from the *East-Indies*	1100	682		2.63	
Another lump of the same	1151	717		2.61	
Granat, *Bohemian*				4.16	
Granati minera	217	147		3.1	
Ivory	173½	83		1.11	
Lapis Calaminaris	477	380		4.72	
—— *Hæmatites*, English	1574	1156		3.16	
—— *Judaicus*	261½	164		2.69	
—— *Lazuli*, one piece	385	256		2.78	
—— *Manati*	452	293		2.16	
A fragment of the same	218½	123		2.22	

Another

	Weigh'd in Air.	In Water.		Proportion.	
Another	345	197	Grains.	2.33	to 1
Another from *Jamaica*	2011	1127		2.27	
Lead-ore	686	590		7.14	
Another piece from *Cumberland*, rich	1872	1586½		.54	
Manganese, a piece	321	230		3.53	
Marcasite	814	631		4.65	
Another from *Stalbridge*	243	189		4.5	
Another more shining than ordinary	287	227		4.78	
Mineral, *Cornish*, like a shining marcasite	145	129		9.6	
Ore of silver, choice from *Saxony*	458	366		4.22	
Another piece	1120	960		7.	
Osteocolla	195	108		2.24	
Rhinoceros's horn	8563	4260		1.99	
Rock-crystal	256	140		2.20	
Slat, *Irish*	779	467		2.49	
Sulphur vivum	371	185		2	
German, very fine	306	152		1.98	
Talc, a piece like *Lapis Amianthus*	596	334		2.28	
Talc *Venetian*	082	508		2.73	
Talc of *Jamaica*	1857	1238		3	
Tin-glass	468	419		9.55	
Tin-ore, *New-English*	812	613		4.8	
Tin-ore, black, rich	1293	984		4.18	
Another piece, choice	2893	2314		5	
Tutty, a single piece	104	83		5	
Vitriol, *Engl.* a very fine piece	1093	512		1.88	
Vitrum Antimonii	357½	282½		4.76	
Unicorn's horn, a piece	407	195		1.91	

* Later trials have furnish'd us with the following table of the specific gravities of solids and fluids.

A TABLE of the specific Gravities of several solid and fluid Bodies.

Fine gold	19,640
Standard-gold	18,888
Quick-silver	14,000
Lead	11,325
Fine Silver	11,091
Standard-silver	10,535
Bismuth	9,700
Copper	9,000
Cast brass	8,000
Steel, the same piece, soft	7,738
Steel, the same piece, hard	7,704
Steel, the same piece, spring temper	7,809
Iron	7,645
Tin	7,320
Glass of antimony	5,280
A pseudo topaz	4,270

STATICS.

A diamond	3,400	Serum of human blood	1,190
Clear cryſtal glaſs	3,150	Pitch	1,150
Iſland cryſtal	2,720	Spirit of ſalt	1,130
Fine marble	2,700	Spirit of urine	1,120
Rock-cryſtal	2,650	Human blood	1,040
Common green glaſs	2,620	Amber	1,040
Stone of a mean gravity	2,500	Milk	1,030
Sal gemmæ	2,143	Urine	1,030
Brick	2,000	Dry box-wood	1,030
Nitre,	1,900	Sea-water	1,030
Alabaſter	1,875	Common water	1,000
Dry ivory	1,825	Camphire	0,996
Brimſtone	1,800	Bees-wax	0,955
Dantzick vitriol	1,715	Linſeed oil	0,932
Alum	1,714	Dry oak	0,925
Borax	1,714	Oil olive	0,913
Calculus humanus	1,700	Spirit of turpentine	0,874
Oil of vitriol	1,700	Rectified ſpirit of wine	0,866
Oil of tartar	1,550	Dry aſh	0,800
Bezoar	1,500	Dry maple	0,755
Honey	1,450	Dry elm	0,600
Gum arabic	1,375	Dry fir	0,550
Spirit of nitre	1,315	Cork	0,240
Aqua fortis	1,300	Air	0,001$\frac{3}{4}$

AN

A N

Hydroſtatical Diſcourſe,

By way of

Anſwer to the Objections of Dr. MORE, *and others, againſt ſome Explanations of particular Experiments:*

With farther Conſiderations thereon.

SECT. I.

FInding no cauſe to alter my judgment, as to the ſolutions I have given, of ſome of my experiments, attack'd by Dr. *More*; I here deſign to vindicate them from his objections.

And, firſt, the Doctor having made a deſcription of my pneumatic engine, pretends, that the aſcent of the ſucker, after it is depreſs'd, and clogg'd with a weight, is not mechanically accounted for, by the gravity and preſſure of the atmoſphere; becauſe, ſays he, "if this ſolution were truly "mechanical, he muſt have aſſign'd the true mechanical cauſe of the gravity "of all the parts, and of the whole atmoſphere." *Mechanical ſolutions of phenomena, what?*

To this I anſwer, that in delivering my experiments about the effects of the air, I did not intend to write a whole ſyſtem, or ſo much as the elements of natural philoſophy; but having ſufficiently prov'd, that the air we live in, is not deſtitute of weight, and has an elaſtic power; I endeavour'd, by thoſe two principles, to explain the phenomena exhibited in our engine; without recourſe to a *Fuga vacui*, an *Anima mundi*, or any ſuch unphiloſophical principle. And ſince ſuch kinds of explanations have, of late, generally been call'd, mechanical, as being grounded upon the laws of mechanics, I thought, as I permit, ſo I might be allow'd, the uſe of that term; and to entitle my explanations, mechanical, in the uſual ſenſe of that expreſſion. I am not obliged to treat of the cauſe of gravity in general, ſince many propoſitions of *Archimedes*, *Stevinus*, and others, who have

STATICS. written of ſtatics, are confeſs'd to be mathematically, or mechanically demonſtrated; tho' thoſe authors do not aſſign the true cauſe of gravity, but take it for granted, as a thing univerſally acknowledg'd. And, if in each ſcale, of an ordinary balance, a pound weight, for inſtance, be put; he who ſhall ſay, that the ſcales hang in equilibrium, becauſe the equal weights balance one another; and, in caſe an ounce be added to one of the ſcales, and not to the oppoſite, he who ſhall ſay, that the former is depreſs'd, becauſe urg'd by a greater weight than the other, will be thought to have given a mechanical explanation of the equilibrium of the ſcales, and their loſing it; tho' he cannot give a true cauſe why either of thoſe ſcales tends towards the center of the earth. Since, then, to aſſign the true cauſe of gravity, is not required, even in ſtatics, tho' one of the principal, and moſt known parts of mechanics; why may not other propoſitions, and accounts, that ſuppoſe gravity in the air, and prove it too, be look'd on as mechanical?

The Doctor, however, is pleas'd to grant me almoſt as much as I need deſire, as to the truth of the hypotheſis whereon my explanations are founded. The principal thing which I ſuppoſe, in my hydroſtatical writings, is, that in water, tho' ſtagnant, the upper parts actually gravitate upon the lower; or preſs upon them, even when they do not ſenſibly depreſs them. This hypotheſis, the Doctor allows, agreeable to the principles of the mechanical philoſophy: and, accordingly, having ſhew'd, that in a ſuſpended tube of water, the whole liquor gravitates upon the bottom of it; and, conſequently, that all the parts thereof do ſo, the upper upon the lower, "provided" ſays he" "there be no immaterial principle in nature."

Now, I as freely, as the Doctor himſelf, aſſert an incorporeal Being, that made and governs the world. All that I have endeavour'd at, in explaining what happens among inanimate bodies, is to ſhew, that ſuppoſing the world to have been at firſt made, and to be continually preſerv'd by God's divine power and wiſdom; and ſuppoſing his general concourſe to the maintenance of the laws he has eſtabliſh'd in it; the phenomena I conſider, may be ſolv'd mechanically; that is, by the mechanical properties of matter; without recourſe to nature's abhorrence of a vacuum, to ſubſtantial forms, or to other incorporeal creatures. And, therefore, if I have ſhewn, that the phenomena, I attempted to ſolve, are explicable by the motion, magnitude, gravity, ſhape, and other mechanical affections of the ſmall parts of liquors; I have done what I pretended: which was not to prove, that no angel, or other immaterial creature, could interpoſe in theſe caſes; for, concerning ſuch agents, all that I need ſay, is, that, in the caſes propoſed, we want not their aſſiſtance; and, therefore, have no occaſion to fly to it, in ſolving our phenomena.

That the upper parts of fluids gravitate upon the lower.

But the Doctor, it ſeems, would have the gravitation of the elements, in their proper places, ſuſpended by an incorporeal principle; and hereto he is led by this experiment, which, he ſays, is moſt manifeſtly repugnant to our hypotheſis. He conceives, then, that in a bucket of water, with a perfectly cylindrical cavity, whoe di ameteri s of *62* parts, there is forcibly

kept

kept at the bottom, by means of a ſtick, a round piece of wood, whoſe diameter amounts but to 61 of thoſe parts; and that as ſoon as ever the ſtick is remov'd, the piece of wood will emerge to the top, and float: "which," ſays he "is impoſſible, if all the parts of the water, did not only "jointly preſs the bottom of the veſſel, but each preſs'd the other directly "downwards."

STATICS.

But, firſt, ſince according to his computation, the area of the interval between the ſides of the veſſel, and the edges of the round board, is 123 of ſuch parts, whereof the area of the board amounts to 3721; 'tis evident, that there muſt be room enough for the water to paſs between the ſides of the veſſel, and the edges of the board; which is ſuppoſed, on all hands, to be of ſome wood lighter *in ſpecie* than water, ſince elſe it would not riſe, upon with-drawing the ſtick.

Secondly, this round board is not ſuppos'd to be made exactly fit to the bottom of the veſſel, and, conſequently, the water may get in between them; for which reaſon, 'tis neceſſary to keep the piece of wood forcibly down with a ſtick; which were, otherwiſe, needleſs; and, conſequently, this interpos'd water, will communicate with the upper, along the ſides of the veſſel; which latter may, according to the laws of hydroſtatics, by means of that interpoſed, exerciſe its preſſure upwards, againſt the lower ſurface of the wooden plate.

Thirdly, the Doctor ſuppoſes an imaginary plane of water to be parallel to the bottom of the veſſel, and to paſs along the bottom of the board; ſo that of the water that lies between this plane, and the bottom of the veſſel, one part is cover'd by the piece of wood; and the other, between the edges of that, and the ſides of the bucket, is cover'd with the incumbent water only.

Now, 'tis manifeſt, that in water, thoſe parts which are moſt preſs'd, will thruſt out of their place, thoſe that are leſs preſs'd. 'Tis alſo evident, that the part of the imaginary plane, cover'd by the round piece of wood, muſt be preſs'd by a leſs weight than the other part of the ſame plane; becauſe the wood being, bulk for bulk, lighter than water, the aggregate of the wood and water, incumbent on the cover'd part of the ſame plane, muſt be lighter *in ſpecie*, than the water alone, that is incumbent on the uncover'd part of the ſame plane; and, conſequently, this uncover'd part being more preſs'd than the other part of the plane, the heavier muſt diſplace the lighter; which it cannot do, but by thruſting up the board; as it does when the external force that kept it down, is removed. And this greater preſſure againſt the bottom, than againſt the top of bodies immers'd in water, ſpecifically heavier than themſelves, is a true reaſon of their emerſion.

'Tis true, that according to the Doctor's computation, if the ſolid cylinder, conſiſting of the wooden plate, and all the water directly incumbent on it, were put into an ordinary balance; it would, there greatly outweigh the hollow cylinder of water alone, that reſts upon the uncover'd part of the imaginary plane. And this is what ſeems to have deceiv'd the

Doctor:

STATICS. Doctor. But there are many hydroſtatical caſes, wherein the phenomenon depends not ſo much upon the abſolute weight of the compared bodies, as upon their reſpective and ſpecific gravity; on account whereof, a ſmall pebble, for inſtance, that weighs not a quarter of an ounce, will readily ſink to the bottom of a river, on whoſe ſurface a log of a hundred pound weight will float. 'Tis a rule in hydroſtatics, that when two portions of water, or any other homogeneous liquor, preſs againſt each other; the prevalency will go, not according to the abſolute weight, but the perpendicular height of thoſe portions. And, accordingly, we find, that if a ſlender pipe of glaſs, being fill'd with water, have its lower orifice unſtopp'd at the bottom of a veſſel of water, which contains much more of that liquor than the pipe; yet if the water in the tube were, for inſtance, two feet high, and that in the veſſel but one; the water in the pipe will readily ſubſide, till it comes almoſt to a level with the external water; tho' it cannot do ſo, without raiſing the whole maſs of water ſtagnant in the veſſel.

Demonſtrated by experiments. We took an open-mouth'd glaſs, or jar, three inches and a half in diameter, and ſomewhat leſs in depth, its cavity being cylindrical; into this, having put ſome water to cover the protuberance that is left at the bottom of ſuch glaſſes, we took a convenient quantity of bees-wax, and having juſt melted it, we pour'd it cautiouſly into the glaſs, warm'd before-hand to prevent its cracking, till it reach'd to a convenient height. This veſſel, and the contain'd liquors, we ſet aſide to cool, that the wax might ſhrink from the glaſs, and, conſequently, have a little interval every where between the concave ſuperficies of the veſſel, and convex of the hardned wax; when, carefully pouring ſome water between the glaſs and the wax, ſo that it fill'd all the interval left between thoſe two bodies, both at the bottom and the ſides, the wax was made preſently to float; being viſibly rais'd up from the bottom, and its upper part appearing a little above the level of the water, as it ought, according to the true principles of hydroſtatics. For water, being ſomewhat heavier, *in ſpecie*, than wax, and that which was poured into the bottom, and ſtagnated there, being preſs'd by the collateral water, every way interpoſed between the concave part of the glaſs, and convex of the wax; (ſo that this collateral liquor anſwer'd what I call a hollow cylinder of water, in the Doctor's experiment) that part of the ſtagnant water which was preſs'd upon by the wax, being leſs preſs'd than the other part of the ſame ſtagnant water was by the water incumbent on it; this latter muſt diſplace the former; which it could not do, but by raiſing up the wax that reſted upon it: yet this collateral water was ſo far from being heavier than the wax impell'd up by its preſſure, that both the collateral and the ſtagnant water together, being weigh'd, amounted to little above a fourth of the weight of the wax; which happen'd by reaſon of the narrowneſs of the veſſel: but if it had been wider, the experiment, I doubt not, would have ſucceeded, tho' the wax had out-weigh'd the collateral water ten times more than in our experiment it did. But, that the ſolid body exceeded almoſt four times the weight, not only of the collateral, but the ſtagnant liquor

quor too, ſufficiently overthrows the Doctor's reaſoning: the fallacy whereof will further appear from hence, that tho' we gradually pour'd in water, as long as the veſſel would contain any; the cylinder of wax was, indeed, lifted higher and higher from the bottom of the glaſs, but did not appear rais'd more than at the firſt, above the upper ſurface of the water; which argues, that it was not at all the quantity of the lower water that continually increaſed; but the preſſure of the collateral water, which continued ſtill at the ſame height, with reſpect to the wax, that caus'd the elevation of the body.

And, to manifeſt yet more clearly the Doctor's miſtake, I deviſed the following experiment. We took a round plate of lead, about the thickneſs of a ſhilling; and having ſtuck it faſt to the bottom of the cylinder of wax, we ſucceſſively placed upon the upper part of the wax, ſeveral grain-weights, till the wax ſubſided to the bottom: by this means, the glaſs being, at firſt, almoſt fill'd with water, there ſwam about an inch of that liquor above the upper ſurface of the wax. And, laſtly, we took off, by degrees, the grain-weights that we had added, till we ſaw the wax, notwithſtanding the adhering lead, riſe, by degrees, to the top of the water; above which, ſome part of it was, viſibly, extant.

From this experiment I argue, that, according to the Doctor's ſuppoſition, here was incumbent on the wax, a cylinder of an inch in height, and of the ſame diameter, or breadth, with the round ſurface of the wax; but, upon the removing part of the water that lay at the bottom, when the wax began to riſe, there was incumbent no greater a weight than that of the collateral water; and as much of the upper and ſtagnant, as was directly incumbent upon that collateral water. But now, according to the Doctor's reaſoning, this cylinder of water incumbent on the wax, being an inch deep, and above three inches broad, it muſt preſs the wax with a far greater weight than that which the lateral, and hollow cylinder of this ſtagnant water, could exert upon the reſt of the collateral water; yet the height of this aggregate of collateral water, being the ſame with that of the wax, and the water ſwimming upon it; the difference of the preſſure was ſo ſmall, that, barely taking off a weight of four or five grains, the wax would, notwithſtanding the preſſure of the water incumbent on it, be impell'd up, and made to float; and, by the like weight put on again, it would be made to ſink; and, by another removal of ſuch a weight, it would, tho' ſlowly, re-aſcend. And this phenomenon depends ſo much upon a mechanical balance of preſſure, that even four grains would not have been neceſſary to make the wax riſe or ſink, were it not for ſome little accidental impediments, that are eaſily met with in ſuch narrow glaſſes; for, otherwiſe, in a larger veſſel, we have made the ſame lump of wax readily ſink, or float, by putting on, or taking off, a ſingle grain, or leſs.

Hence it appears, that, for the regulation of hydroſtatical phenomena, nature has her balance too, as well as art; and that, in the balance of nature, the ſtatical laws are nicely obſerv'd.

And,

Statics. And, ſtill further to demonſtrate, that, in ſtagnant water, the upper parts gravitate upon the lower; we took a very ſlender pipe of glaſs, whoſe cavity was narrower than that of an ordinary gooſe-quill, that heterogeneous liquors might not be able to get by one another in it. This pipe, near one end, was bent upwards, like a ſyphon, that it might have a ſhort leg parallel to the longer; into this crooked pipe we put a little oil, and then held it perpendicularly in a deep, wide-mouth'd glaſs, fill'd with water, and a lump of wax, of the bigneſs and ſhape of that before-mention'd; ſo that the preſſure of the incumbent water upon the open orifice of the ſhorter leg, might impel the oil into the longer, above the ſurface of the water in the veſſel. The pipe being thus held upright, 'twas eaſy to take notice, by a mark fixed on the outſide, to what height the oil reach'd in it.

Now, if we conceive a horizontal plane, parallel to the bottom of the veſſel, to paſs by the baſis of the floating wax; 'tis evident, that, of this imaginary plane, the part on which the wax reſts, is as ſtrongly preſs'd by the weight of the wax, as the lateral part of the ſame plane is by the weight of the water incumbent on it; and, conſequently, that part of this plane, which is placed directly over the orifice of the ſhorter leg of the pipe, is no more preſs'd, than any equal portion of that part of the ſame plane cover'd by the wax. This body, being taken out of the water, the liquor ſubſided a great way in the veſſel; and ſo did, proportionably, the oil in the longer leg of the pipe. And, laſtly, having weigh'd out as much water as we found the wax to amount to; this liquor was, inſtead of the wax, pour'd into that which remain'd in the glaſs: whereupon, the oil in the longer leg of the pipe, was again impell'd up to the former mark, to which the wax had rais'd it. Whence we may gather, that the water newly put in, tho', in the air, it weigh'd no more than the wax did, yet preſs'd the water that lay beneath the fore-ſaid imaginary plane; and, conſequently, that which was directly over the ſhorter leg of the pipe, as much as the wax before had done. And, ſince we have already prov'd, that the wax conſiderably preſs'd that plane; it cannot be deny'd, that the water did, in like manner, preſs that plane: and, conſequently, that water may gravitate in water, as well as a ſolid body, ſuch as is wax.

But, to rectify that plauſible miſtake, which has long deluded both philoſophers and mathematicians, who think a body does not actually gravitate, when it does not deſcend; we have ſeen, that the immerſed wax, and the braſs-grains which lie on it, actually preſs, or gravitate, upon the ſubjacent water, and bottom of the veſſel, on which 'tis incumbent; and, conſequently, its preſſure, not being ſurmounted by that of the collateral water, which is unable to raiſe it, muſt be as great as that of this collateral water. Therefore, when, upon the removal of a ſingle grain, the wax, with its incumbent weight, is made to aſcend, and that but very ſlowly; 'tis evident, that 'twas ſo far from not gravitating before, becauſe it did not actually deſcend, that it retain'd its gravity even whilſt it aſcended; as may appear, not only by the ſlowneſs of its motion upwards, proceeding from

from its being in nature's ſcale very little leſs heavy than it need be, to balance the preſſure of the collateral water; but by this alſo, that if but a ſingle grain be laid on, when it begins to riſe, its aſcent will be check'd, and hinder'd; which could not be done by the addition of ſo inconſiderable a weight, if the wax, and the adhering metal, did not, even during their aſcent, retain their former gravity; tho' that were fruſtrated as to the act of deſcending, or ſo much as keeping their ſtation, by the prevailing preſſure of the collateral water. So that, ſince the wax, and adhering metal amounted to conſiderably above four thouſand grains; it did, in the balance of nature, weigh, whilſt aſcending, not ſo much as a four thouſandth part leſs than it did, whilſt it was actually deſcending. STATICS.

Upon the whole, then, Dr. *More*, had he throughly conſider'd the matter, needed not have concluded the account of his experiment, as he does, by ſaying, "this is ſo evident a demonſtration againſt the gravity of the "parts of water downwards; that unleſs it prove true, I ſhall never aſ-"ſent to any reaſoning of my own, for the future, nor to that of any other "man whomſoever." But, I hope, he will conſider, as well as I, that a man may be very happy in other parts of learning, who has had the misfortune to miſtake in hydroſtatics; a ſcience which very few ſcholars have been at all vers'd in.

Water made to ſupport a body of a much greater ſpecific gravity than itſelf.

As for the laſt experiment in my hydroſtatical paradoxes, the Doctor ſeems to ſuſpect the matter of fact; or, ſuppoſing it true, accounts for it from his hylarchic principle, and the interpoſition of the valve of the inſtrument; but what conſiderable intereſt the imaginary retraction of the valve, or the air itſelf, can have in this phenomenon, I confeſs I do not diſcern: for, I think, the experiment would ſucceed, when try'd *in vacuo*, tho' all the atmoſpherical air were annihilated.

However, to avoid all miſtakes, and diſputes, that may ariſe upon account of the valve employ'd in our experiment; a quantity of quick-ſilver, being, by ſuction, rais'd into a very ſlender glaſs-pipe, whoſe upper orifice was ſtopp'd with the finger, to keep the mercury from falling out; we thruſt the open end of the pipe, with the mercury in it, into a deep glaſs of water, till the little cylinder of mercury had, beneath the ſurface of the water, attain'd to a depth, at leaſt fourteen times as great as the height of the mercurial cylinder; when, the finger being removed from the upper orifice, the glaſs-pipe will be open at both ends, and nothing can hinder the quick-ſilver from falling to the bottom, but the reſiſtance of the cylinder of water that is under it; which cylinder can reſiſt but by virtue of the weight, or preſſure, of the ſtagnant water above it, tho' but collaterally placed: yet this water being, by the pipe, whoſe upper part is higher than the ſurface of that, and acceſſible only to the air, kept from preſſing againſt the mercury any where but at the bottom of the pipe; and being about a fourteenth part of the weight of an equal bulk of mercury; it is able, at that depth, to make the ſubjacent water preſs upward againſt the mercury, which is but a fourteenth part as high as the water is deep, with a force equal to that of the gravity, wherewith the mercury tends

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downwards. And, to manifest that this phenomenon depends merely upon the equilibrium of the two liquors; if you gently raise the lower end of the pipe towards the surface of the water, this liquor, being not then able to exercise such a pressure as it could at a greater depth, the mercury preponderating, will fall out to the bottom of the tube. But if, when the quick-silver is at the first depth, instead of raising the pipe, you thrust it down farther under the water; the pressure of that liquor against the mercury, increasing with its depth, will not only sustain the mercury, but impel it up in the pipe to a considerable distance from the lower orifice, and keep it about the same distance from the surface of the water, which is laterally above it. And this experiment may not only serve for the purpose for which I here alledge it; but, also, if duly consider'd, and apply'd, very much illustrate and confirm the explanation, formerly given, of the seemingly spontaneous ascent of the clogg'd sucker in our exhausted air-pump.

The case of divers, with regard to the pressure of the water they sustain, at great depths.

The last argument which the Doctor urges against the gravitation of water, in its proper place, as they speak, is deduced from what happens to divers, who, in the midst of the sea, tho' salt-water be much heavier than fresh, do not find themselves compressed by the vast load of the incumbent water.

But if observations about diving were made by philosophers, and mathematicians, or, at least, by intelligent men; we should, I do not doubt, have accounts of it, very different from the current reports. A learned physician, of my acquaintance, upon diving leisurely, perceived a constriction of his thorax, by the action of the surrounding sea-water.

A *Spanish* prelate, who liv'd long in *America*, speaking of the *Indians*, employ'd by their inhuman masters in fishing for pearls, tells us, "it is "impossible that men should live for any long season under the water, "without taking breath; so that they die commonly thro' vomiting blood, "and of the bloody flux." And a general of the *English* in the *East-Indies*, sent on an embassy to the emperor of *Japan*, speaks thus of some female-divers he met with in his voyage. "All along the coast, and so up to *Ozaca*," says he, "we found women-divers, who lived, with their houshold and family, in "boats upon the water. These women," he says, "would catch fish by di-"ving, which they missed by net and line, and that in eight fathoms depth. "Their eyes, by continual diving, grow as red as blood; whereby a "diving woman is distinguished from all others." However, the reason why so little damage is sustain'd by diving, seems, in my opinion, owing to the uniform pressure of the surrounding fluid, and the robust texture of a human body *.

The

* 'Tis remarked by Sir *Isaac Newton*, that since fluids, in pressing bodies immersed in them, do not change the external figures thereof, they will not change the situation of the internal parts among one another; and, therefore, that if sensation proceeds from the motion of the parts of animals; fluids will not damage the animals immersed therein, nor excite any sensation in them, farther than as their bodies

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Vaſt preſſures of a fluid ſuſtain'd by weak and tender bodies.

The ſwims of ſmaller fiſh appear very unable to reſiſt compreſſure, being much more thin and delicate to the eye, than a piece of fine *Venice* paper; yet having cauſed one of theſe bladders, above an inch in length, and proportionably wide, to be taken out of a roach, and anointed with oil, to keep it ſupple, and preſerve it from being pierced, or ſoften'd, by the water; and having, by a weight of lead, faſten'd to the neck of it, let it down to the bottom of a cylindrical tube of water, ſealed at one end, and made large, and about fifty-ſix inches long; we could not perceive that, by the weight of all the incumbent water, it was manifeſtly compreſs'd, or that it diſcover'd the leaſt wrinkle, or other depreſſion of that very thin membrane, tho' fill'd but with air. And this trial was made more than once with the ſame ſucceſs. Yet that this proceeded rather from the ſtrength of the bladder, that was able to reſiſt the weight of a taller pillar of water, than from the levity of water in the upper part of the tube on that in the lower, we ſhew'd, by preſently letting down a mercurial gage, by a ſtring, to the bottom of a tube: for the weight of the incumbent water forced up ſome of the mercury out of the open leg of the ſyphon into the ſeal'd one; and, conſequently, compreſs'd the air included there: and the uncompreſſed air, being three inches and $\frac{5}{8}$ in length, we judged it, at the bottom of the tube, to be about $\frac{5}{8}$ impell'd up by the intruſion of the mercury. And, to ſatisfy myſelf, and others, that if the incumbent water had been heavy enough, it would, viſibly, have affected the bladder, in ſpite of any *Principium hylarchicum*; we ſunk it in a cryſtal glaſs, that had a long cylindrical neck, and was exactly fitted with a ſtopple; then a competent quantity of air being left above the water, the ſtopple was warily, and by degrees, thruſt down; and ſo, leſſening the capacity of the glaſs, compreſſed the air that was next it, and, by the intervention of that, the water under it. And tho' there did not, upon a ſlight compreſſion of the outward air, appear any ſenſible effect upon the bladder that was at the bottom of the water; yet, upon a farther intruſion of the ſtopple, the preſſure being increaſed, the immerſed bladder diſcover'd two conſiderably deep wrinkles, that preſently diſappear'd upon drawing up the ſtopple; which, being thruſt in again, the depreſſions were again to be ſeen on the ſwim. And, having convey'd a mercurial gage into the ſame glaſs, we eſtimated, by the condenſation of the air in the ſeal'd leg of it, that the bladder had been expoſed to a preſſure equal to that of a column of about forty feet of water.

This may leſſen our wonder, that bodies, of ſo firm a texture as thoſe of luſty men, ſupport the preſſure of the water at ſuch depths as divers uſually remain; ſince we ſee what reſiſtance can be made by ſo exceeding thin

bodies may be condenſed by compreſſure. *Newton. Princip.* p. 264, 265. And *Borelli*, having ſhewn, that a quantity of ſand, contain'd in a very hard or rigid veſſel, cannot poſſibly be divided, or entred, by a wedge; and that water, in like manner contained in a bladder, every way equally compreſſed, can neither be contracted, bent, or otherwiſe diſtorted; hence deduces the reaſon why divers are not ſenſible of the preſſure of the water. *De motib. nat. à gravitate factis.* Prop. 29. 34.

STATICS. thin and delicate a membrane, diſtended only with the air, in compariſon of the ſtrong membranes and fibres of a man, fill'd, beſides air, with more firm parts. 'Tis alſo conſiderable, what great weights may be ſuſtain'd in the air, by ſuch fibres or tendons, and by other fibres interwoven into membranes, in compariſon of what an ordinary man would expect. And, not only upon account of the ſtable parts of a human body, but of the ſpirits too, it may reſiſt very violent preſſures of a fluid, without any manifeſt contuſion, or diſlocation of parts, or even ſenſe of pain; as appears from the great effects which guſts of wind have upon trees, houſes, &c. tho' a man will withſtand the impetuoſity of ſuch a ſtrong wind, and walk directly againſt it, by virtue of the vigour of his muſcles and ſpirits, without being thrown down, or bruiſed, by ſo violent a current of air, and without ſo much as complaining of pain; and this, tho' the wind that beats againſt him, acts as a ſtream, and does not uniformly compreſs him, but invade only the fore-part of his body. Thus, alſo, in the lifting up of heavy weights by luſty men, we may ſee the ſlender tendons of the hand loaded with an hundred and fifty pounds, or more, without having their fibres ſo far compreſſed, or ſtretched, as to make the perſon complain of pain. A human body, therefore, is an engine of a much firmer ſtructure, than mere ſcholars uſually take notice of. And, I doubt, whether, if the ſtructure of a man were not conſiderably firm, he could, eſpecially in a deep ſea, ſupport the preſſure of the water, tho' not immediately apply'd, without pain. For, having, ſeveral times, convers'd with a man who got his living by diving for ſhip-wreck'd goods; he aſſured me, that, when he ſtay'd at a conſiderable depth, as ten or twelve fathoms, under the ſurface of the ſea, he felt a great pain in both his ears, which often put him to ſhifts to leſſen it; and this, by his manner of deſcribing it, I concluded, aroſe from the incompetent reſiſtance of the air, which he found, by manifeſt tokens, to be greatly compreſſed by the upper water. This relation, from ſuch a perſon, not only confirms our explanation, but likewiſe warrants us to doubt whether the common reports that are made concerning divers, ought to be rely'd on, without further examination.

We took a common fleſh-fly, of a middle ſize, and having put it into the ſhorter leg of a bent glaſs, which we cauſed to be hermetically ſealed, we added as much mercury as fill'd that leg, and a part of the other; leaving little more than an inch of air between the quick-ſilver, and the ſeal'd end, that there might be room both for the fly, and the condenſation of the air; and then, with a little rammer, fitted for the purpoſe, we caus'd the mercury, in the open leg, to be thruſt againſt that in the ſeal'd one; which neceſſarily crowded the air, near the fly, into leſs room; ſo that it ſeem'd condens'd into about a third part of the ſpace which it poſſeſs'd before, and which it regain'd, when the rammer was withdrawn: and tho' this were done more than once, yet the fly appear'd not ſenſibly hurt; and I perceiv'd her, whilſt ſhe was pent up, to move her legs, and to rub them one againſt the other, as 'tis uſual with that ſort of inſects in the free air.

Another

Another experiment, to the ſame purpoſe, we made with water; tho' this wet the wings of the fly, and ſoon after, by miſchance, drown'd it: but we had, firſt, an opportunity to compreſs the air into a third, if not a fourth part, of its former dimenſions; yet the fly continued to move ſeveral of her parts, and, eſpecially her legs, very vigorouſly; as if nothing troubled her, but being, as it were, glued to the inſide of the glaſs by her wetted wings. And this, I hope, will keep the reſiſtance of divers, to the ſurrounding water, from ſeeming incredible; ſince ſuch flies were able to reſiſt, and, for ought appear'd, without harm, or pain, the preſſure of the crowded particles of the air: tho' we gueſs'd it to have been as much compreſs'd by the force of the rammer, as it would have been by a cylinder of water, of between 50 and 60 feet high. STATICS.

Hence, too, we may be aſſiſted to conceive, how great a difference there is, whether the ſame preſſure be exerciſed by a ſolid, or by a fluid body. For, according to our eſtimate, the preſſure againſt the body of the fly was as great, as if a ſlender pillar of marble, having the fly for its baſe, and 18, or 20 feet in height, had reſted upon the little animal.

We alſo took ſome ordinary black flies, of a middle ſize; and having placed one of them, with the head upwards, and left ſome diſtance betwixt her and the ſeal'd end of the glaſs tube, we pour'd in quick-ſilver very ſlowly, and cautiouſly, leſt the force of ſo heavy a body, acquired by its velocity in the fall, ſhou'd, more than the mere weight of the fluid, oppreſs her: at length we got in as much mercury as the tube would receive; and then, holding it upright, we watch'd whether the fly would make any motions; and finding that ſhe did manifeſtly ſtir, notwithſtanding the incumbent mercury, we meaſur'd the height of the mercurial pillar, reaching from the middle of her body, to the top of the fluid, and found it to be about eight inches; and the quick-ſilver being pour'd out, the fly appear'd very lively, and vigorous.

We repeated the experiment with one of the beſt flies we could take, when their ſeaſon was almoſt over, and of the ſame ſize with the former; when, ordering the matter ſo, that the mercury incumbent on her, appear'd to be of a greater height than that of the tube before employ'd, we ſaw her move one, or other of her legs, ſeveral times, tho' the tube was held upright; and therefore, meaſuring the height of the mercury above her, we found it to amount to above ſixteen inches; then freeing her from this preſſure, we obſerv'd, that ſhe immediately found her legs again, and moved, up and down, briſkly: tho, when ſhe was aftewards oppreſs'd, with 23, or 24 inches of the ſame quick-ſilver, ſhe gave no ſigns of life. But having got another fly, of about the ſame bigneſs; tho', when ſhe was at the bottom of the quick-ſilver, ſhe ſeem'd ſo compreſs'd as not to have any motion, yet, upon being taken out of the glaſs, ſhe preſently diſplay'd her wings; tho' the pillar of mercury, that preſs'd upon her, amounted to above 27 inches.

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Another fly, that ſeem'd but about half ſo big, as one of thoſe hitherto mention'd, being well placed, with ſome mercury under her, in a glaſs pipe, held upright, ſuſtain'd a mercurial pillar of 25 inches; and tho' ſhe was not obſerv'd to move, under ſo great a weight, yet, when once that was taken off, ſhe appear'd unhurt; and, probably, would have eſcaped under a much greater weight, if the tube, which was too large, had not, already, employ'd all the ſtock of mercury we then had at hand. Since, then, ſo ſmall an animal, as a fly, may ſurvive ſo great a preſſure, and remain able to move ſuch long and ſlender bodies, as her legs, when preſs'd againſt by above 16 inches of mercury; and, conſequently, by a weight, equivalent to a pillar of water, of more than 18 feet and a half; which being 590 times her own length, and many times more her height, it appears, that a diver, ſix foot tall, to have as many times his height of water above him, as our fly might have had, and yet continue to have moved under it, muſt dive to near an hundred fathoms; which is a depth vaſtly greater than, from what I could learn by enquiry, the divers, either for coral, or pearl, deſcend to.

Thus, then, we have accounted for a phenomenon, which Dr. *More* thinks inſoluble, without his hylarchic principle. There would, indeed, be much more weight in what he objects, if our aſſertion, of the gravitation of water in water, were, like the *Principium hylarchicum*, a mere hypotheſis, advanced without any poſitive proof: but our doctrine is directly proved, by particular experiments; to elude the force whereof, ſo ingenious a perſon, is obliged to call in a principle that is not phyſical.

An actual preſſure at the bottom of the ſea.

But, whatever power he is pleas'd to ſuppoſe at the bottom of the ſea, to ſuſpend the preſſure of the incumbent water, I think, that ſuppoſition muſt give place to experience; which ſhews, there really is a great preſſure exercis'd by the water at the bottom of the ſea. A gentleman, who has been often preſident of the royal ſociety, aſſured me, that a friend of his, having let down a pewter bottle into a deep ſea, with weight enough to ſink it, that he might try whether any ſweet water would ſtrain into it; found, when he pull'd it up again, the ſides of it very much compreſs'd, and, as 'twere, ſqueez'd inwards by the water. An acquaintance of mine, trying to cool his liquor, when he ſailed thro' the torrid zone, by letting the containing veſſels to a great depth into the ſea, was, at firſt, amaz'd to find the corks, with which the ſtrong ſtone bottles had been well ſtopp'd before, ſo forcibly, and ſo far thruſt in, that they could ſcarce have been ſo violently beaten in with a hammer. And, an ingenious perſon, who practiſes phyſic in the *Indies*, told me, he try'd, in a very deep part of the ſea, whether any freſh water would ſtrain into ſtone bottles, thro' a thick cork, ſtrongly ſtopp'd in; and having let it down with a convenient weight to 100 fathoms, was much diſappointed, when he drew it up, by finding that the preſſure of the water, at ſo vaſt a depth, had quite thruſt down the cork into the cavity of the bottle: an effect which he ſcarce could have expected from the ſtroke of a mallet.

And,

And, to ſhew, *ad oculum*, that water may gradually, as it grows deeper, preſs againſt the ſtopple of a bottle, tho' the veſſel be inverted; we took a glaſs vial, furniſhed with a cylindrical neck, and its cavity large, in proportion; into this we put as much quick-ſilver as would, in the neck, make a ſhort mercurial pillar, of between half an inch, and an inch; then a piece of very fine bladder, dipped in oil, was ſo tyed over the orifice of the glaſs, that no mercury could fall down, or get out, nor water get in at the orifice; and yet the bladder, by reaſon of its great limberneſs, might be eaſily thruſt up towards the cavity of the vial, or depreſs'd by the weight of the mercury. This little inſtrument, firſt furniſh'd with a weight of lead, to ſink it, being inverted, the mercury deſcended into the neck, and cloſed the orifice as exactly as a ſtopple; but, with its lower part, depreſs'd the bladder beneath the horizontal plane, that might be conceiv'd to paſs by the orifice: then the glaſs, being, for a while, kept in the water, and, by a ſtring, let further down into the ſame glaſs veſſel, fill'd to about two feet in height; the preſſure of the liquor againſt the orifice of the vial, by degrees drove up the bladder, and the mercurial ſtopple into the cavity of the neck; as was manifeſt by the aſcent of the quick-ſilver: and when the inſtrument was leiſurely drawn up again, the weight of this mercury made it ſubſide, and plump up the bladder, as before. STATICS.

Meeting, caſually, with an ingenious mechanic, who deviſed proper accommodations, and a boat, wherewith he could continue, for a great while, at a conſiderable depth under water; he aſſured me, that when he was about four or five yards deep in the river *Thames*, his breaſt and abdomen were ſo compreſſed, that, there being hardly room enough left for the free motion of his lungs, he could ſcarce fetch his breath; and was obliged to be drawn quickly up: and that, to remedy this, he cauſed a kind of armour for the cheſt and back, to be made of copper; and tho' the metal defended him from any miſchief in thoſe parts, yet, in others, where only leather was interpoſed, when he came to the depth of about ſix fathoms, he found a great preſſure upon his legs and arms, and all the other parts, againſt which the water was able to thruſt the leathern ſuit inwards: and this preſſure he found pretty equal; ſo as to receive no great inconvenience from it; being able to continue under water, tho' not at any great depth, for about two hours. He farther declared, from his own experience, that the ambient water endeavour'd to preſs him, and his diving ſuit, every way inwards. I have, alſo, been aſſured, that a profeſs'd diver, when he deſcended in his bell, to very great depths, has often had the blood ſqueez'd out at his noſe and eyes.

Upon the whole, it appears, that water actually preſſes againſt bodies under it, whether ſpecifically lighter, or heavier than it ſelf.

SECT.

SECT. II.

Water actually weigh'd in water, by common scales.

TO my particular method of actually weighing water in water, it has been objected, by a late writer of hydrostatics, that there is a mistake in it. I shall here, therefore, endeavour to clear this matter, and set my thoughts about it in a fuller light.

My opinion is, that water, as it is a heavy fluid, always retains its gravitation, and power of pressing; (by which, I mean, a tendency downwards, whatever be the cause of that gravity) whether it have a body under it, either specifically heavier, or lighter than it self, or one equi-ponderant to it. For I see not what should destroy, or abolish this gravity, tho' many things may hinder some effects of it. And, therefore, I suppose that water retains its gravity, not only in air, but in water too, and in heavier liquors; and, consequently, by virtue of this, that liquor presses upon them. But, if a surrounding fluid have, upon account of its specific gravity, an equal, or a stronger tendency downwards, than water, it will, by virtue of that, be able to impel up this liquor, or to keep it from actually descending; so that a portion of water, supposed to be included in a vessel of the same specific weight with water, will, placed in a greater quantity of the same water, neither rise nor fall, tho' it retains its gravity there; only this gravity is kept from making it actually descend by the contrary action of the other water, whose specific gravity is supposed equal: as when a just balance is loaded with a pound weight, in each of the scales, tho' neither of the weights actually descend, being hinder'd by its counterpoise; yet each retains its whole weight, and, with it, presses the scale it rests on: so that our included portion of water, really presses the subjacent water, tho' it does not actually depress it. Nor do I think that the only way of judging whether a body gravitates, is to observe, whether it actually descends; since, in many cases, its gravity may be proved, by the resistance it makes to heavy bodies, which would otherwise raise it; as appears by equal weights in a balance. And, for want of this distinction, I have known even learned men, treating of hydrostatics, mistake the question.

Now, the adversaries I had to deal with, both in print, and in discourse, deny'd, that in standing water, the upper press'd, or gravitated upon the lower parts. And tho' they could not but grant, that the whole weight of the water gravitated upon the bottom of the vessel; yet they would have the parts of it to do so *actione communi*, as they speak; and fancied I know not what power of nature, to keep the homogeneous portions of water, as well as other elements, from pressing one another, whilst it is in its proper place. Against this opinion, it was alledg'd, (besides other things, which I found many, otherwise good scholars, were not fitted to understand) that if a glass vial, or bottle, well stopp'd, were deeply immersed under water, it would strongly tend upwards; but if it were dextrously unstopp'd, when 'twas thus immers'd, so that the water cou'd get in; allowing for the weight of the glass itself; 'twould, by the water that crowds

crowds in, and thrusts out the air, be made strongly to tend downwards, and continue sunk. But this not satisfying, because 'twas pretended, that the reason of the empty bottle's emerging, when stop'd, was the positive levity of the air that fill'd it; and that the sinking of it, when unstop'd, was from the recess of the same air, which, by the intruding water, was driven, with large bubbles, out of the bottle: I thought this evasion might be obviated, by contriving an experiment, wherein the water should be plentifully, and suddenly admitted into the glass, and yet no air expell'd out of it; so that, if then the glass, which was sustain'd before, should fall to the bottom, with a gravitation amounting to a considerable weight, in respect of its capacity, the sinking of it could not be ascribed, as before, to the recess of the air, endow'd, as they suppose, with positive levity; but to the weight of the water admitted, which, when thus weigh'd, would be environ'd with water of the same kind: and to shew, that this water might have a considerable weight, notwithstanding the place it was in, I employ'd a pair of scales, after the manner recited in the experiment, page 288, Vol. II.

However, therefore, my expressions disagree with those of my adversary, the distance of our opinions is not so wide, as it seems at first sight: for, he allows, as well as I, that the superior parts of water do, by their gravity, press the inferior; but this he would not have amount to so much as to mean, that water weighs, or gravitates in water. But, if he thinks, that, in my experiment, I meant to propose a method of making water descend in water, and weigh it in that liquor, with a pair of scales, just as I would a piece of lead, or a portion of mercury; which are bodies much heavier *in specie*, than water; either he mistakes my intention, or I did not sufficiently declare it. What I design'd to shew, and, I think, have shewn, was, that by the help of an ordinary balance, it may be made appear, that water, admitted into the glass bubble, I employ'd, caus'd it to weigh much heavier than it did before that liquor enter'd into it; and that this new weight, manifested by the balance, was not due, as my adversary supposes, to a recess of the air.

To this experiment, our author objects the following. "Take a piece "of wood, lighter *in specie* than water, and add weight to it, by degrees, "till it become of the same weight with water; fasten it, with a string, to a "balance, and weigh it in water, and you will find the whole weight sup-"ported by the water." But this is so far from overthrowing my opinion, that it agrees very well with it. For, suppose the weight, added to the light wood, be lead, it cannot be said, that the metal loses its native ponderosity, whilst it rests in the water; and the reason why it descends not, is, that it, and the wood whereto it is joined, are hinder'd by the counterpoise of the collateral water, which, by its pressure, would raise the surface of the water whereon the floating body rests, if it were not hinder'd by the weight of these incumbent solids: and this resistance of theirs, to the endeavour of the water upwards, being exercised only upon account of their gravity, shews, that they do, in my sense, gravitate, tho' not pregravitate.

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The relative levity of bodies under water.

'Tis obvious, that if wood, wax, or other bodies, lighter *in ſpecie* than water, be detain'd under it, they will, upon removal of the force, emerge to the top. And this they do ſo readily, and, as it ſeems, ſpontaneouſly, that the generality of philoſophers, both ancient and modern, aſcribe it to an internal principle, which they call poſitive levity.

But this principle was not, always, ſo univerſally receiv'd among philoſophers, as in later ages; *Democritus*, and ſeveral of the antients, admitting no abſolute, but only a relative levity: which opinion, ſome of the moderns have attempted to revive.

The experiment uſually urged, to prove the poſitive levity of wood in water, ſeems, to me, too ſlightly made, to be acquieſced in. 'Tis true, indeed, if a flat board, for inſtance a trencher, have its broad ſurface kept by a man's hand upon the horizontal bottom of a bucket, full of water; when the hand that detain'd it, is removed, the trencher will commonly ſoon aſcend to the ſurface of the water. But, I do not perceive, that a deciſive experiment, of this kind, is eaſy to be made with ſuch materials: for, the wood, whereof both the trencher, and the bottom of the bucket conſiſt, are ſuppoſed to be lighter, *in ſpecie*, than water; and, conſequently, they muſt be of a porous, and lax texture; whilſt the more ſolid woods, as *Lignum vitæ*, brazil, &c. whoſe texture is cloſer, ſink in water. If, therefore, there be not much care uſed, to bring the ſurfaces of the trencher, and the bottom of the bucket, to a true flatneſs, and ſmoothneſs, the experiment will not be accurately made; and, perhaps, tho' it be mentally, yet it is ſcarce practically poſſible to bring ſuch porous bodies, as the lighter woods, to ſuch a contact as is neceſſary in this caſe. And, were that actually done, I ſhould not expect, that the trencher would aſcend. For, in my opinion, the cauſe why, in ordinary inſtances, wood, wax, and other bodies, ſpecifically lighter than water, being let go at the bottom of a veſſel, full of that liquor, emerge to the top, is chiefly, that there is no ſuch exquiſite congruity, and contact, between the lower ſuperficies of the wood, and the upper of the bottom of the veſſel; but that the lateral parts of the water, being impell'd by the parts of the ſame liquor incumbent on them, are made to inſinuate, and get between the lower parts of the wood, and the bottom of the veſſel, and ſo lift, or thruſt upwards, the wood, which, bulk for bulk, is leſs heavy than the water that extrudes it.

But, as the whole of the argument of thoſe I diſpute with, conſiſts in a ſuppoſition, that becauſe the trencher is placed on the bottom of the veſſel, no water can come between to buoy it up; whence they conclude, it muſt aſcend by an internal, and poſitive principle of levity; I made the experiment after another, and, if I miſtake not, a better manner.

Shewn by experiments, which overthrow the doctrine of poſitive levity.

We took, then, two round, cylindrical plates of black marble, which had thoſe ſuperficies that were clapp'd together, ground very flat, and carefully poliſh'd, that they might touch in as many parts as poſſible; and that the upper being taken up, the other might ſtick to it, and aſcend with it:

it: and the better to keep out the water, the internal surfaces were, before they were put together, lightly oil'd; which did not hinder them from most easily sliding along one another, as long as the contiguous surfaces were kept horizontal: then a blown bladder, of a moderate size, was fasten'd to the upper marble, and both of them were let down to the bottom of some stagnant water; where, by the help of an easy contrivance, the lower marble was kept level to the horizon. And now the patrons of positive levity would have concluded, that the bladder, being a body much lighter than wood; and being in an unnatural place, beneath the surface of the water, should, of its own accord, and with impetuosity, emerge: but I expected a contrary event, because the bladder, being ty'd to the upper marble, so that both of them might, in our case, be consider'd as one body; the water could not impel them up, since the close contact of the surfaces of the two marbles, kept the water from insinuating between them; and, consequently, from getting beneath the upper marble, and pressing against the lower superficies of it. And, to shew that this was the reason why the bladder did not emerge, I caused part of the oil'd surface of the upper marble to be slid off from that of the lower; which, by reason of the smoothness and slipperiness of the surfaces, was very easy to do. But the contact still continuing in a greater part of the surface than was requisite, more and more of the upper marble was gradually slipt from the lower; till, at length, when the marbles touch'd but in one half of their surfaces, the endeavour of the water to force up the bladder full of air, being stronger than the resistance which the contact but of part of the surfaces of the stones, was able to make, they were suddenly disjoin'd; and the bladder, by the extruding water, impetuously shot up above the top of the water.

With the same marbles, we made several other experiments of this kind, most commonly letting them down both together; but once or twice, at least, by placing the upper marble, under water, upon the lower, already fix'd to the bottom of the vessel.

That 'twas not the weight of the upper marble, nor want of lightness, whether positive or relative, of the air included in the bladder, that kept it from ascending, is manifest by the impetuous ascent of it upon disjoining the marbles; and by this, that the bladder would lift up, from the lower parts of the water, not only the upper stone, when it touched not the other, but a weight of seven or eight pounds hanging at it.

We, also, took a bladder, in great part freed from its air; and tying the neck of it very close, that none of the remaining air might get out, we fasten'd a considerable weight to it; and, by the help of this, sunk the bladder to the bottom of a wide-mouth'd glass, full of water, that the surface of the liquor might be considerably higher than the upper part of the bladder. This wide-mouth'd glass we included in a great receiver, which was carefully cemented on to the engine. The principal design of the experiment was to shew, that tho' the air included in the bladder, was very far from being able, by its absolute levity, to lift up so great a weight

 as

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But if, inſtead of hanging ſo great a weight at the neck of the bladder, we faſten'd only a moderate piece of lead, ſuch as would only ſerve to ſink the bladder, and keep it at the bottom of the water, ſo that the aggregate of the bladder, air, and metal, was but a little heavier than an equal bulk of water; then, upon the firſt ſtroke of the pump, which could withdraw but a ſmall part of the air in the receiver, that in the bladder ſuddenly expanding itſelf, the whole would immediately be impetuouſly extruded by the water; tho', after ſome reciprocations, it floated in its due poſition; but, upon the return of a little outward air, it would immediately ſubſide.

It may be objected, that the aſcent of the weight was not effected by the preſſure of the water; becauſe rarity and levity, being ſimilar qualities, the great rarifaction of the air might, proportionably, increaſe the levity of it; and, conſequently, enable it to perform much greater things than it could do before. I ſhall anſwer this objection by the following experiment.

About the neck of a conveniently ſhaped vial, able to hold a few ounces of water, I cauſed to be carefully ty'd the neck of a ſmall bladder, whence the air had been well expreſs'd; ſo that the bladder, being very limber of itſelf, and, probably, made more ſo, as well as more impervious to the air and water, by the fine oil we had caus'd it to be rubb'd with, lay upon the orifice of the vial, like a ſkin clapp'd together, with many folds and wrinkles. This done, we let down the vial into a veſſel full of water; and the vial being poiſed for that purpoſe, ſunk perpendicularly in the liquor, till its neck was partly above, and partly beneath the ſurface of the water: then, covering the external glaſs with a large receiver, we cauſed the air to be pumped out; and as the preſſure of that was gradually withdrawn, the air in the floating vial expanded itſelf into the bladder, and unfolded the wrinkles of it, till, at length, it became full blown, without altering the erect poſture of the glaſs it reſted on. But this

this great expanſion, being made above the water, and, conſequently, in a medium, not heavier than the included air, gave that highly rarify'd air no ſuch increaſe of levity, as ſhew'd that it made ſo much as the neck of the glaſs riſe higher in the water, than it did before. Nor did we obſerve, that the return of the air into the receiver, by reducing the air in the bladder to its former unrarify'd ſtate, made the glaſs ſink deeper than before. But when the experiment was try'd with the ſame glaſs and bladder, at the bottom of the water; upon pumping out the air, the bladder, being dilated under water, was, after a while, carried up to the top, and took up with it about eight or ten ounces, that, to clog it, had been faſten'd to the bottom of the vial.

But, to examine whether a portion of air, by being rarify'd, or expanded, acquires a new and proportionable degree of poſitive levity; we took a bladder, of a moderate ſize, which was very fine and limber, that it might be the lighter, and more eaſily diſtended. The greateſt part of the air, being ſqueezed out of the bladder, the neck of it was tied up very cloſe, that no air might get in or out of it. This limber bladder was hung at one of the ſcales of a balance, whoſe beam had been purpoſely made more than ordinarily ſhort, that the inſtrument might be ſuſpended, and continue capable of playing in the cavity of a great receiver, into which we convey'd it; having, firſt, carefully counterpois'd the bladder with a metalline weight, in the oppoſite ſcale. This done, the air was pumped out, and as that was withdrawn, the bladder was gradually expanded by the ſpring of the internal air; till, at length, when the receiver was well exhauſted, it appear'd to be quite full. But, notwithſtanding this great dilatation of the included air, it did not appear, by the depreſſion of the oppoſite ſcale, to be grown manifeſtly lighter than it was at firſt. And the bladder ſeem'd, alſo, to retain the ſame weight, after it had, by the air that was let into the receiver, been compreſſed into its former flaccid ſtate.

To determine whether a fiſh riſes or ſinks in water, by contracting, or expanding itſelf; take a bolt-head, with a wide neck, almoſt fill it with water, and put therein a large live fiſh; then draw out the neck of the glaſs exceeding ſlender, and fill that alſo with water. Obſerve the motion of the fiſh; and if, upon his ſinking, the water, at the ſlender top, ſubſides; we may infer, he contracts; and if, upon his riſing, the water be raiſed, we may conclude, he dilates himſelf.

The preſſure of the air's ſpring on bodies under water.

I employ two ſorts of experiments, to ſhew, that a ſmall quantity of incloſed air, may, by its preſſure, have a conſiderable effect upon bodies under water, notwithſtanding the interpoſition of that liquor. For this preſſure may be manifeſted by what it directly, and poſitively, performs upon bodies cover'd with water; and, alſo, by the phenomena that regularly enſue upon the removal of the incloſed air, or the weakning of its ſpring.

Manifeſted by experiments.

We took a ſquare glaſs-vial, capable of holding twelve ounces of water; the neck of this we luted on carefully, and ſtrongly, over the orifice

STATICS. of the small pipe, at which the air passes in our engine, out of the receiver into the pump: then, covering this glass with a great receiver, we luted it strongly to the engine, and, at the top, pour'd in as much water as sufficed to surround the internal receiver, if I may so call it, and cover it to a pretty height. This done, we exactly closed, with a turn-key, the hole in the great receiver, at which the water had been pour'd in, that no air might pass that way. And, lastly, we began to pump out the air contain'd in the internal receiver; that the air, which, by the above-mention'd pipe, had communication with the external air, might no longer, by its pressure, assist the glass to resist the pressure which the incumbent, and inclosed air, by virtue of its spring, constantly exercises upon the subjacent water, and, by its intervention, upon the sides and bottom of the internal receiver. At the first exsuction, which could not be supposed to have well emptied the internal glass, this vessel was, by the pressure of the superior air upon the surrounding water, broken into numerous pieces. The same experiment, with a like success, was repeated with a stronger internal glass.

Having squeez'd out of a moderately sized bladder, the greatest part of its air, we tied the neck of it very close; and then, fastening to it a competent weight, we placed it at the bottom of the tallest, and largest glass we could cover with our great receiver; that so, tho' the incumbent air were pump'd out, none of the water might escape with it, but still retain the same height above the bladder. Having, then, poured upon the bladder as much water as would swim high above the upper part of it; we cover'd this glass of water with a great receiver, which, being carefully cemented on to the engine, the pump was set on work; and as the air which, by its spring, press'd upon the surface of the included water, was, by degrees, pumped out; so the air, imprison'd in the bladder, gradually expanded, at the bottom of the water, as if no such liquor had interposed between them, otherwise than by its weight; upon whose account it must be allow'd a little to hinder the expansion of the bladder, in proportion to the height it has above it. Thus the immersed bladder was, at length, full blown, by the dilatation of the air inclosed in it; and, by its intumescence, made a considerable part of the water run over the sides of the glass that before contain'd it all. And when access was given again to the external air, the internal being compressed, the bladder was presently reduced to its flaccid state.

We took a small fine bladder, whose neck was strongly ty'd up, when it was about half full of air; this we put into a short brass-cylinder, the lower of whose bases was closed with a brass-plate, and the other left open; this open orifice we afterwards stopp'd, but not exactly, with a cylindrical plug, that was somewhat less than it, and was, by a rim at the top, hinder'd from reaching too deep into the cavity of the cylinder, that it might not damage the bladder which lay beneath it: upon the plug we placed a conical weight of lead; and this pile of several things, being so placed upon our engine, that we could cover it with a great receiver, we carefully

fully cemented on, ſuch a veſſel, and at the top of it pour'd in ſo much water as would ſerve to fill the vacant part of the braſs-cylinder, and the cavity of the engine to ſuch a height, that it cover'd all the leaden weight, which was ſeveral inches high, except a rim faſtened to the top of it, for the conveniency of removing it. This being done, the pump was work'd; and, long before we had exhauſted the air of the receiver, that which was incloſed in the lank bladder had, by degrees, diſplay'd ſo vigorous a ſpring, that it raiſed up the weight, which lay upon it, to a conſiderable height, and kept it there, till the air was let in from without, to aſſiſt its depreſſure by the lead, which amounted to no leſs than about twenty-eight pounds.

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We took a cylindrical copper-veſſel, of a conſiderable height; into this, being firſt almoſt fill'd with water, we put a ſquare glaſs-vial, capable of containing nine or ten ounces of water, and exactly ſtopp'd with a cork, and a cloſe cement; this vial, by a competent weight, was detained at the bottom of the water, from whoſe upper ſurface it was conſiderably diſtant; then the copper-veſſel, being placed upon the engine, and included in a great receiver, well cemented on; the air was, by degrees, pumped out: but, before it was quite exhauſted, the glaſs, at the bottom of the water, was, by the ſpring of the air included in it, burſt into many pieces, not without great noiſe, and a kind of ſmoke, or miſt, that appeared above the ſurface of the water: and another glaſs, of the ſame ſort, was broke, after the ſame manner, in another veſſel.

Now, the conſequence that naturally reſults from the three laſt experiments, is, that, ſince barely upon withdrawing the preſſure of the included air, the air reſiding in the immerſed bodies, did, by virtue of its ſpring, expand itſelf ſo forcibly, and perform ſuch conſiderable things; the air above the water muſt have exerciſed a very ſtrong preſſure upon the ſurface of it; for, it muſt have been, at leaſt, equivalent to that force of the immerſed air, the exerciſe whereof it was able totally to hinder.

And, from hence, it may be eaſily deduced, that the weight of the atmoſphere acts upon bodies under water; tho' the interpoſed liquor is vaſtly heavier, *in ſpecie*, than air: for, we have juſt now proved the preſſure of incloſed air, which conſiſts in its ſpring, upon bodies under water. And, 'tis manifeſt, that the ſtrength of the ſpring of this lower air, we make our trials with, is cauſed by the weight of the upper, which bends and compreſſes thoſe little ſpringy particles whereof our air conſiſts; ſo that the weight of the atmoſphere, being equivalent to the ſpring of the lower air, muſt preſs upon the ſurface of the ſubjacent water, with a force equal to the ſpring of that part of it which is contiguous to the water.

Fluids preſs in a different manner from ſolids.

It has proved a great impediment to the reception of the doctrine of the weight and preſſure of the atmoſphere, that, if it could, really, exerciſe ſo great a force as we aſcribe thereto, it would, unavoidably, oppreſs all the bodies expoſed to it: ſo that neither men, nor other animals, would be able to move under ſo great a load, or withſtand its force.

This,

STATICS.

Whence, the weight of the atmosphere is not prejudicial to the bodies of animals.

This I grant to be a plausible objection, but the force of it will be taken away by the following considerations put together.

And first, the power of pressing, that we ascribe to the air, is not a thing deduced from doubtful suppositions, or bare hypotheses, but from real and sensible experiments. And, therefore, since we have clear and positive proofs of the pressure of the air; tho' we could not explain how men and other animals are not destroy'd by it; yet we ought rather to acknowledge our ignorance in a doubtful problem, than deny what experience manifests to be true. Thus the attractive virtue, and other powers of the loadstone, are freely acknowledg'd, even by those who confess themselves unable to explain them; tho', if experience did not satisfy us of them, they were liable to many more considerable objections, than any urg'd against the pressure of the air.

But those who make this objection against the great pressure of the air, seem not aware, that we were conceiv'd, and born in places exposed to the pressure of the atmosphere: and, therefore, how great soever that pressure appears to be, it ought not to crush us now: since, when we were but embryos, or newly born, we were able to resist it; and not only live, but grow to all dimensions, in spite of it.

If there were any place about the moon, or other celestial globes, which some suppose to be inhabited, that had no atmosphere, or equivalent fluid, about it, and where men, yet, could be generated a-new; and one of these men should be transported thence, and set down upon our earth, there might be made an experiment proper to decide our controversy. In the mean time, I doubt, since nature is not observ'd to make things superfluously strong, such a human body, not being design'd to resist any weight or pressure of air, would be of so tender and compressible a make, that it would easily be crush'd inwards, by our atmospherical pressure. And tho' we cannot give an instance of this kind, yet we make trials somewhat like it, in our pneumatical engine. For when we place water in our receiver, and pump out the air that was above it, there will be generated a multitude of bubbles; some of which, when the air is carefully withdrawn, swell to a strange, and scarce credible bulk. These bubbles being generated where the air cannot press upon them, such dimensions are so natural to them, that if the receiver be stanch, and no other unfriendly accidents intervene, they would probably be durable; since I have known the spring of highly-dilated air continue for many months: and a bladder would, for no less time, continue blown and fill'd, in our vacuum, by a little air left in it, when the external was withdrawn from it. Yet the large bubbles, when once the outward air is suffer'd to come in upon them, are thereby so violently compress'd, that in a trice they shrink into dimensions too small to keep them visible. And if I could have succeeded in my attempt of producing living bodies in our vacuum, I suppose the success would have confirm'd our doctrine.

You will tell me, that so great a weight and pressure, as I assign to the atmosphere, must needs make a man feel pain; and if not dislocate some

of

of the parts, at leaſt preſs the whole body inward. But, 1. being accuſtom'd to this preſſure from our very birth, and even before it, we are hinder'd from taking notice of it; thoſe preſſures only being ſenſible to us, that are made ſo by ſome additional cauſe, which, by making a new impreſſion, excites us to obſerve it. Thus, we are not ſenſible of the weight of the clothes we wear; and ſo a healthy man is not ſenſible of the heat in his heart, becauſe 'tis conſtant there, and the ſentient parts of the heart have been uſed to it, tho' that heat has been often very conſiderable; but when, in live diſſections, a man puts his finger into the heart of an animal, which probably has a fainter, or at leaſt no ſtronger degree of heat, than a human heart, he will feel in his fingers, accuſtom'd to the air, a manifeſt degree of heat. 2. It has been proved, that a cubic inch of air, for inſtance, has a ſpring ſufficient to reſiſt the weight of the whole atmoſphere, as far as it is expoſed thereto; otherwiſe it would be more compreſs'd than in fact it is. 3. A very little portion of air, tho' it much ſooner loſes its ſpring by expanſion than a greater, yet will reſiſt further compreſſion, as much as a greater. 4. The pores of the parts of animals, whether fluid or conſiſtent, as in their blood, gall, urine, heart, liver, *&c.* there are included a multitude of aerial corpuſcles; as appears from the numerous bubbles afforded by ſuch liquors, and the ſwelling or expanſion of the conſiſtent parts in our exhauſted receiver. 5. To this we may add, that beſides the bones, whoſe ſolidity is not queſtion'd, a much greater part of the human body than is uſually imagined, really conſiſts of membranes and fibres, and the coalitions and contextures of them; and that theſe ſubſtances are, by the moſt wiſe author of things, made o ſa much cloſer and ſtronger texture, than thoſe, who have not try'd, will be apt to think. Laſtly, there is a far greater difference than men commonly ſuſpect, between the effects of the preſſures made upon bodies by ſolid weights, and thoſe they ſuffer from ſurrounding fluids.

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Hence 'tis not neceſſary that the preſſure of the atmoſphere, tho' as great as we ſuppoſe it, ſhould oppreſs and cruſh the bodies of men, who live under it; for the ſolidity of the bones, and the ſtrong texture of the membranes and fibres, with the ſpring of the aerial particles, (which abound in the ſofter as well as in the fluid parts of bodies) that is equivalent to the preſſure of as much of the atmoſphere, as can exerciſe its preſſure againſt them; make the frame of a human body ſo firm, that it may well reſiſt the preſſure of the outward air, without having any part violently diſlocated; whilſt this external preſſure is exerciſed only by the air, which being but a ſurrounding fluid, preſſes equally on every ſide. And becauſe our bodies have been produced in the atmoſphere, and from our very birth expoſed, without intermiſſion, to the preſſure of it; our having been continually accuſtom'd to this preſſure, and the firmneſs of our ſtructure, keep us from being ſenſible of the weight or preſſure.

Multitudes of men have had occaſion to paſs over high mountains, and I myſelf have been upon the *Alps* and *Apennines*; yet I never met with, nor heard of any one, who took notice of the difference, as to

STATICS. to the weight of air ſuſtain'd; or who complain'd, that, when come down to the foot of the mountain, they felt any greater compreſſure from the air, than at the top. Yet, numerous experiments witneſs, that on more elevated parts of the earth, which have a leſs height of the atmoſphere incumbent on them; the weight and preſſure of the air is not ſo great as below. And, on very high mountains, 'tis not unlikely that this difference may be very conſiderable; ſince, when the *Torricellian* experiment was made near *Clermont* in *France*, upon the *Puy de Domme*, which is none of the higheſt mountains in the world, they found the difference of the mercury at the top and bottom, to amount to about three inches; and, conſequently, if the trial had been made with water inſtead of quick-ſilver, the difference would have been about three feet and a half, perpendicular height. And, on much higher mountains, the difference of the height of the mercurial cylinder at the top and bottom, would be much greater. But, at the bottom of ſome very deep well, or mineral groove, which may, without improbability, be ſuppoſed placed at, or near the foot of one of theſe mountains; if we conceive the barometer to be let down, the variation would be much more conſiderable; yet we find not that the diggers in the deepeſt mines, of mountainous countries, are ſenſible of being depreſſed, or compreſſed, by any unuſual weight. But, to build only on matter of fact; it appears by the preceding obſervations, that when a man was at the bottom of the hill, he had an additional weight of air preſſing upon his head, above what he ſuſtain'd at the top, equal to the height of an imaginary veſſel of water, having his head for its baſis, and three feet and a half high; which is ſo conſiderable a weight, as could not but have been, of itſelf, very uneaſy to ſupport. And the ſame muſt be, proportionably, applied to the other parts of his body. Whence, we may infer, that ſuch a weight was not felt by the man it compreſſed, becauſe the preſſure was exerciſed after the uniform manner of fluid bodies.

And this may ſuffice to ſhew, that there is no neceſſity the compreſſure of the atmoſphere ſhould make it impoſſible to live in it. But, to clear this matter ſtill farther, we made the following experiments.

Experiments to illuſtrate and confirm this doctrine.

Into a ſhort hollow cylinder of braſs, we put a fine bladder, ty'd ſo cloſe at the neck, that none of the air, whereof it was about half full, would, probably, get out. This we did, to the end that the hen's egg we were to bed in it, might lie ſoft, and have its ſides almoſt cover'd with the limber flaccid bladder, and contained air; then we cover'd the remaining part of the egg with another bladder, that nothing hard might come to bear immediately upon the ſhell; when, putting a wooden plug into the cylinder, and a weight upon the plug, tho' ſlowly and warily, leſt the quick deſcent of the weight ſhould make the plug break the egg; we, in the laſt place, cover'd the cylinder with a large receiver, and the air being drawn out, that air which was ty'd up in the bladder, by degrees, expanded itſelf ſo ſtrongly, as to lift up the plug and the incumbent weight, to a conſiderable height, and keep it there, till the external air was re-admitted.

Now,

Now, ſince the air, in ſuch caſes, expands itſelf, vigorouſly, every way, it muſt preſs againſt the egg with the ſame force that it did, proportionably, againſt the bottom of the plug; and that force was more than ſufficient to lift up the weight, which, together with the plug, amounted to about thirty pounds: yet the egg, being taken out, appear'd perfectly whole, and no way harmed. But the ſame egg was afterwards cruſh'd to pieces, by laying, warily, ſmall weights, one after another, upon it, till they amounted to about four pound weight. STATICS.

We took a glaſs-bubble, about an inch and a half in diameter, blown at the flame of a lamp, that it might be far more eaſy to break than the thinneſt vial; this bubble we included between bladders, as we did the egg in the former experiment; and then, having warily put the plug into the cylinder, ſo as to preſs upon the bladder that ſurrounded the glaſs, we leiſurely put the weights upon the plug, till they, together with it, amounted to thirty pounds, or more; which being removed, the plug was removed; and the glaſs-bubble, tho' it were, perhaps, as thin as fine white paper, was taken out whole.

But, leſt the great reſiſtance of ſo thin a glaſs, which yet was not hermetically ſeal'd, ſhould be aſcribed to its ſpherical figure; we employ'd, inſtead of it, the ſhell of an egg, whence, by a hole made at one end, the yolk and white had been taken out. This empty and imperfectly cloſed ſhell, we treated as the glaſs-bubble, in the laſt experiment; and, notwithſtanding the great leaden-weight that reſted upon the plug, it eſcaped without the leaſt crack.

And, to ſhew that what we obſerv'd about the nature of the compreſſure of fluid bodies, will hold as well in water as air; we put an hen's egg into a limber bladder, almoſt full of water; and tying the neck very ſtrait, that nothing might get in or out, we ſo placed the bladder in the braſs-cylinder, that the egg might not be immediately touch'd by any thing that was hard; then putting the plug into the cylinder, we warily and leiſurely heaped upon it convenient weights of lead, till they amounted to about ſeventy-five pounds: notwithſtanding all which, the egg was taken out ſound, and uncrack'd; and, probably, might have ſupported a much greater preſſure.

Nor were eggs the only bodies we endeavour'd to cruſh after this manner: the trial having been, alſo, made upon a piece of butter, of a very irregular ſhape, it came out unalter'd.

AN

Essay-Instrument,

Wherewith to examine if COIN be adulterate or counterfeit.

An easy method of determining the specific gravities of fluids and solids.

HAving, formerly, used a little instrument, consisting of a glass-bubble, with a long slender stem, to compare the specific gravities of several fluids, by its degrees of sinking in them; and, coming to consider its uses more attentively, I found it, also, proper to discover the specific gravities of several solids. For, every solid, heavier than water, loses in water as much of the weight it had in air, as a bulk of water, equal to the immersed solid, would weigh in the air. And, consequently, as gold is by far the most ponderous of metals, a piece of it, and one of equal weight of any other metal, being produced, the gold must be the least in bulk. If both of them, therefore, be weigh'd in water, the gold must lose less of its former weight than the other metal; because both grow lighter herein by the weight of a bulk of water, equal to each respectively. Hence, I concluded, that my floating instrument would be made to sink deeper by an ounce, for instance, of gold hanging at it under water, than by an ounce of brass.

This instrument may be made of glass, copper, silver, or almost any other solid body, that is fit to float on water, with a piece of coin hanging at it, and of a texture close enough to keep out the water. It consists of three parts, the ball, the stem, and the place for the coin. The ball BCDE, if of metal, consists of two thin concave plates, solder'd together in the middle; and at the most distant parts from the juncture, there ought to be left two opposite holes, one in each plate, for the two other parts of the instrument. The ball should not be exactly round, but of any fit shape, to keep the instrument erect and steady in the water; and it must contain air enough to prevent the whole instrument, when loaded,

Fig. 27.

from

from sinking below the top of the stem. The stem A B, must be solder'd on to the ball, at the upper hole thereof. It may be either hollow, or solid; but it ought to be made very slender, and considerably long. In the lower hole of the ball, is solder'd the undermost part of the instrument, which I call the stirrup G. The screw F, is a very short piece of brass, with a broad slit in it, capable of receiving the edge of the coin; which, with a turn or two of a slight small side-screw, may be kept fast in. The stirrup G, is made of wire, which, a little below the bottom of the ball, is bent round, so as to stand horizontally, that the coin may be laid upon it. 'Twould be convenient to have the under stem and screw made separate, that the ball of the instrument, being sufficiently large, there may be room for two or three flat round pieces of metal K, with each of them a hole in the middle, fitted to the size of the stem, to serve as balast; being placed near the lower part of the ball, when the screw may be thrust on to support them.

Fig. 28.

Fig 29.

To adjust this instrument for the examination of a guinea, you must, by the help of the stirrup, or screw, hang a guinea, known to be genuine, at the bottom of it; and having stop'd the orifice of the stem, if hollow, that no water may get in, immerse the instrument gently, and perpendicularly in a vessel of fair water, almost to the top of the stem; then letting it alone, if, being settled, it continues in the same station, and posture, the work is done. If it emerge, add a little weight to it, either at the bottom, or the top, that the guinea may depress it as low as is required; but if it sink quite under the water, lighten it, by filing off from the balast-plates, above-mentioned, or otherwise. Then set a mark H, where the surface of the water touches the stem; and taking out the instrument, substitute in the place of the guinea, a little round plate of brass, of the same weight, or a grain or two heavier, in the air; and putting the instrument into water, as before, let it settle, and make another mark I, at the intersection of the stem, and the horizontal surface of the water.

Applied to the examination of coin.

Fig. 27.

But there may happen a case, wherein the practical application of our instrument will he unsecure. For, if a falsifier of money have the skill, by washing, or otherwise, to take off much of the substance of the coin, without altering or impairing either the figure, or the stamp; the coin will not depress the instrument to the usual mark, whence the guinea may be judg'd counterfeit, tho' it be only too light. However, it presently shews, that the proposed guinea is abused; and, if the want of weight appear, by the instrument, to be very great, 'tis a strong presumption, that it is rather wash'd, than counterfeit. But the balance will presently resolve the doubt: for, if the suspected coin has its due weight in the air, the great lightness of it in water, seems to proceed from its not being of the requisite fineness; and, if it want much of its due weight in the air, 'tis probably wash'd, *&c.* rather than adulterated.

It may be very convenient, for those who have frequent occasions to examine various sorts of coin, to have a different instrument adjusted to each. But if the ball be made large, and with a stem long and slender enough, we may

STATICS. may ſoon, by changing the balaſt-plates occaſionally, fit the ſame inſtrument to examine coins of different metals, and weights. Thus one of theſe, made of copper, ſerves me for guineas, crown-pieces, and half-crowns; and may eaſily be made to ſerve for ſeveral foreign coins.

And other mix'd metals. After the ſame manner, may pewter be compared and examined. For, having once obſerved, how far the inſtrument is ſunk, by a determinate weight of it, which is known to be good in its kind; and to contain its due proportion of lead and tin; if you load the inſtrument with an equal weight of any other maſs of pewter, and it ſink deeper, the proportion of lead, very probably, exceeds in the mixture; tho' 'tis poſſible to debaſe pewter with mineral ſubſtances, whoſe ſpecific gravities are not well known: but to do it with lead, ſeems to be the moſt gainful way.

This inſtrument, may, alſo, aſſiſt us to make a near eſtimate of the fineneſs of gold, and its different alloys with ſilver, or other determinate metals: for, being fitted to ſink to the top of the pipe, with a certain weight of the pureſt gold, as ſuppoſe an ounce, make a mixture containing a known proportion of the metal wherewith you alloy the gold, ſuppoſe, of nineteen, or fifteen parts of gold, and one of ſilver; and letting the inſtrument ſettle in the water, mark the place where the ſurface of the fluid cuts the ſtem. Then putting in another mixture, wherein the ſilver bears a greater proportion to the gold, as, ſuppoſe the former be an eighteenth or a fourteenth part of the latter, you may obſerve how much leſs than before, this depreſſes the inſtrument: and ſo proceed to as many degrees of alloy, as can be diſtinguiſhed on the ſtem; always obſerving, that the weight of each maſs, in the air, be exactly the ſame with that of the pure gold; which we ſuppoſe an ounce.

By the ſame method may be examined the different alloys of pure ſilver, with copper, or other metal, ſpecifically lighter than ſilver; and, with a ſlight variation, 'twill not be difficult to eſtimate in what degree, ſeveral coins, whether ſilver, or gold, are debaſed, by the known ignobler metal mixed in the piece propoſed; and this with greater exactneſs, than by the touch-ſtone, or much more troubleſome and chargeable methods. And the ſame is applicable to other mixtures of metals.

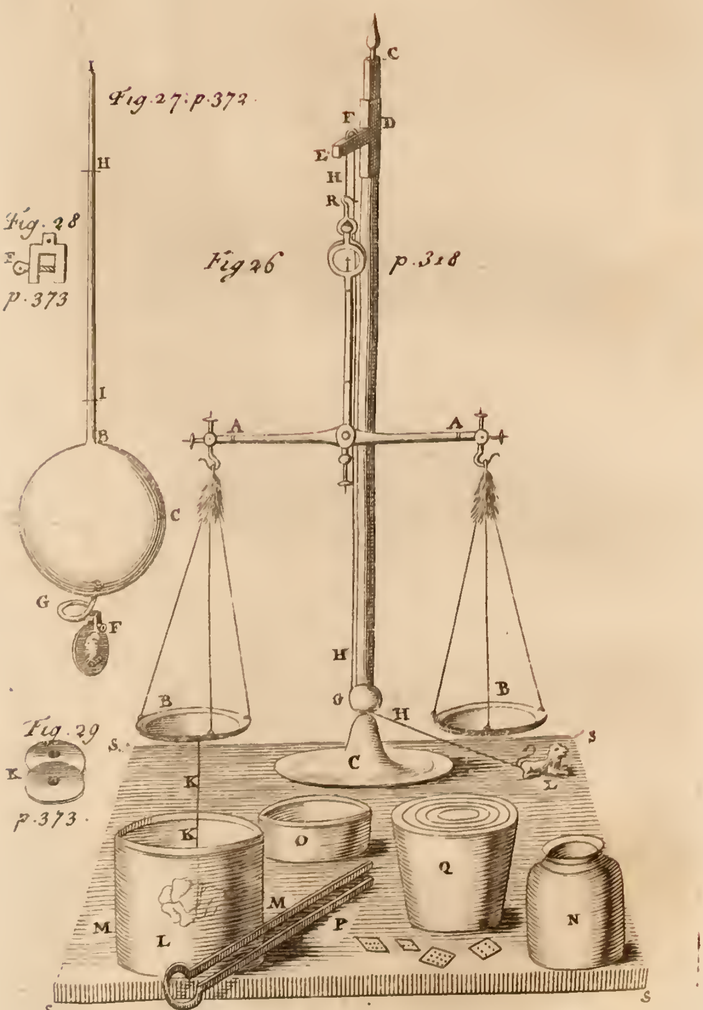

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Plate V.

A

Statical Hygroſcope.

SECT. I.

HAVING had occaſion, amongſt other ſubjects relating to the air, to conſider its moiſture, and dryneſs, I eaſily diſcern'd, they had no ſmall influence upon ſeveral bodies; and, among the reſt, upon thoſe of men; as the air we breathe, either paſſes from one of theſe fluid qualities to the other; or from one degree to another, in the ſame quality. *Various bodies propoſed, to diſcover the dryneſs and moiſture of the air.*

I, therefore, immediately conſider'd of a proper way, to diſcover the changes of the air, as to moiſture and dryneſs; with their ſeveral degrees. For this purpoſe it ſeem'd, to me, that, if a ſtatical hygroſcope could be contrived, it would be very convenient; and fit, both to determine the degrees of the moiſture, or dryneſs of the air; and to tranſmit the obſervations made of them to others. And conſidering, further, that among bodies, otherwiſe well qualified for ſuch a purpoſe, that was likelieſt to give the moſt ſenſible informations of the changes of the air, which, in reſpect of its bulk, had the largeſt ſurface expoſed thereto; I pitch'd upon a fine ſpunge, as a thing which is portable, not eaſily diſſipated, but, by its readineſs to imbibe water, ſeem'd likely to attract the aqueous particles it might meet with in the air, and which, by its great poroſity, has much more of ſuperficies, with regard to its bulk, than any body, not otherwiſe leſs fit for the purpoſe.

But I here propoſe, not the beſt hygroſcope that might be contriv'd, only one that is ſimple, cheap, contained and ſet up in a little room, eaſy to be made and tranſported, ſtatical, commodious, and applicable to various uſes.

In conſidering, what bodies were the fitteſt to be employ'd for ſtatical hygroſcopes, I made tryal of more than one, that ſeem'd not the leaſt promiſing. I know, that common, or ſea-ſalt, will greatly relent in moiſt air; and ſalt of tartar much more: but then, theſe ſalts will not eaſily part with the aqueous corpuſcles they have once imbibed; and are, in other reſpects, leſs convenient than a ſpunge. I made tryal, alſo, with lute-

ſtrings,

STATICS. ſtrings, choſen very ſlender, that they might have the greater ſurface, in reſpect of their bulk; and theſe I found very well to imbibe the moiſture of the air: but, afterwards, they did not anſwer my expectation. I, alſo, cauſed a cup to be turn'd out of light wood, which, that it might leſs burden a tender balance, had, inſtead of a foot, a little button, whereto a hair might be tied, to ſuſpend it; and this cup, being exceeding thin, that it might have much ſurface expoſed to the air, proved ſo good a hygroſcope, that I made ſeveral obſervations with it, for a conſiderable time. It agreed, alſo, with many trials, made on other occaſions, that white ſheeps leather, ſuch as is uſed for plaiſters, would be very convenient for my purpoſe. And, indeed, I found, by ſeveral obſervations, that if this leather were a ſubſtance as little liable to corruption as a ſpunge, it would be a fitter matter, than any other I had employ'd, for hygroſcopes.

A ſpunge made choice of. But, upon the whole, I found no body ſo convenient for my purpoſe, as a ſpunge: to try the poroſity whereof, we weigh'd out a dram of the fine ſort; and having ſuffer'd it to ſoak up what water it could, we held it in the air, till it dropp'd ſo very ſlowly, that we counted a hundred between the falling of two ſucceeding drops; then putting it into the balance wherein it had been weigh'd before, we found, that as its dimenſions were increaſed to the eye, ſo its weight increaſed upon the ſcale; amounting now, to above two ounces and two drams; ſo that one dram of ſpunge, tho' it ſeem'd not altogether ſo fine as the portion we had choſen for our hygroſcope, imbibed and retained ſeventeen times its weight of water.

And turn'd into a hygroſcope. Now a ſpunge is eaſily turn'd into a hygroſcope; for, having weigh'd it when the air is of a moderate temperature, it only requires to be put into the ſcale of a good balance, ſuſpended on a fix'd and ſtable ſupporter. For the ſpunge, being carefully counterpoiſed, at firſt, with a metalline weight, which alters not, ſenſibly, with the changes of the air, it will, by its riſing or falling, ſhew how much the neighbouring air is grown dryer or moiſter, in the place where the inſtrument is kept. The weight of the ſpunge may be greater, or leſs, according to the ſize and goodneſs of the balance, and the accuracy required in the diſcovery. For my part, tho' I have, with very tender ſcales, long employed but half a dram of ſpunge, I found it anſwer my expectation: and tho', when I uſed a bulk ſeveral times as great, in a ſtronger, but proportionably leſs accurate balance, I had ill ſucceſs; yet, after trials with different quantities of it, I preferr'd that of a dram; as not being ſo heavy as to overload the finer ſort of goldſmith's ſcales, and yet ſufficient to diſcover changes conſiderably minute; turning, diſcernably, with a ſixteenth or twentieth part, and, manifeſtly, with half a quarter of a grain.

Experiments and obſervations made with it. With ſuch hygroſcopes as theſe (wherein the balance ought to be ſtill kept ſuſpended and charged) I made ſeveral trials, ſometimes in the ſpring, ſometimes in the autumn, and ſometimes, alſo, in the ſummer and winter. Having once kept a dram of ſpunge ſuſpended, during a

whole

whole spring, and a great part of the preceding winter, and subsequent summer, I was well rewarded by the observations it afforded me. STATICS.

Now, the use of a hygroscope is either general, or particular.

The general use is, to estimate the changes of the air, as to moisture and dryness, by such ways of measuring them, as are easy to be known, provided, and communicated. 'Tis obvious that our statical hygroscope has the advantage of the common sorts, in these several respects. *The use of hygroscopes.*

There is, however, one great imperfection, which all that I have been acquainted with, are liable to; the want of a standard of the dryness and moisture of the air; by relation to which, hygrometers should at first be adjusted, and afterwards compared with one another: tho' this inconvenience may possibly be remedied. In the mean time, even our slight instrument will, in many respects, prove very serviceable.

And first, it will, on many occasions, be useful to shew, both that the air is usually less moist at one part of the artificial day, than at any other; and at what particular time of the day, or night, it most commonly is so. I remember, when the weather was at a stand, I usually observed, that the spunge had manifestly gain'd in the night, tho' it were kept in a bed-chamber, and grew lighter again between the morning and noon. But this observation, which was made towards the end of winter, would not hold, in case frosty nights, or some other powerful cause intervened. 'Twere proper, also, to observe, whether there be not a correspondence betwixt the hygrometer, and barometer,; and, if there be, in what kind of weather, or constitution of air, it is most, or least to be discerned. And this enquiry seems the more dubious, because the same changes of the atmosphere may, upon differing accounts, have either the like, or quite contrary operations upon these two instruments. For, in summer, when the atmosphere is usually heavier, the hygroscope is usually lighter: some strong winds, as with us the north-west, may make both the atmosphere, and barometer lighter; whereas, southerly winds, especially if accompanied with rain, often make the atmosphere lighter, and the spunge heavier. On the other side, I observe, that easterly winds, especially when they begin to blow in winter; tho', by reason of their dryness, they make the hygroscope lighter; yet, at least here, at the west-end of *London*, they commonly shew the air, by the barometer, to be heavier. It were, likewise, fit to be observed, particularly by those who live on the sea-coast, whether the daily ebbing, or flowing of the sea, do not sensibly alter the weight of the hygroscope. 'Twould, also, be very convenient, to take notice, at what times of the day or night, *cæteris paribus*, the air is most damp and most dry; and not only in several parts of the same day, but in several days of the same month; especially on those, wherein the full or new moon happens. And this seems a more hopeful way of discovering, whether the full moon diffuses a moisture in the air, than those vulgar traditions of the plumpness of oysters, shell-fish, the brains of some animals, the marrow in their bones, &c. It may also be noted, whether monthly spring-tides, especially when *The first use of the statical hygroscope, to shew the different variations of weather in the same month, day, and hour.*

STATICS. they happen near the middle of *March* or *September*, have any sensible effect, upon our instrument.

A second use, to shew how much one year, and season, is dryer and moister than another.

The hygroscopes made of an oaten beard, after some months, begin to dry up and shrink; so that their sense of the varying degrees of the moisture of the air, is not so sudden as before; and the informations they give of the degrees of it, especially towards the outmost bounds of their power, to shew the air's alterations, become more and more irregular. But the durableness, and other convenient qualifications of our spunge, making its capacity of doing service more lasting, may the better help us to compare the greatest moisture and dryness, both of the same season, and of the seasons of one year, with the correspondent ones of another. And, if the weight of the spunge, at a proper time, when the temperature of the air is neither considerably moist, nor considerably dry, be taken for a standard; a person may, by computing for how many days, at such an hour, and how much, at that hour, it was heavier or lighter than the standard; and, also, by comparing the result of such an account, in one year, with the result of the like account in another, be assisted to make a more particular, and near estimate of the differing temperature of the air, as to moisture and dryness, in one year than in another; and in any correspondent season, or month, assigned in each of the two years proposed. And, how far the comparison, or continuance of such observations, both in the same place, and also in different countries, and climates, may be useful to physicians, in reference to those diseases, where the moisture and dryness of the air have much interest; and the husbandman, to foresee what seasons will prove friendly, or unkind, to particular soils and vegetables; it must be the work of time to teach us: in the interim we have no reason to despair, that the uses to be made of such observations, may prove considerable. And if, by the result of many observations, men may be enabled to foresee the temperature of a year, or even of a season, it may advantage other professions of men, who receive much profit, or prejudice, by the great dryness, or excessive moisture of the seasons. Not to mention those who cultivate hops, saffron, and other plants, that are tender, and bear a great price; such a foresight, as we are speaking of, may be of great use to shepherds, who, in several parts of *England*, often sustain great losses, by the rot of sheep; which usually happens through excess of moisture, in certain months of the year. And, in order to lay foundations, whereon to build predictions, it may not be amiss to register the number, magnitude, and duration, of the considerable spots, that may at this, or that time of the year, happen to appear, or be dissipated, on, or near the sun; or, to take notice of any extraordinary absence of them; and to observe whether their apparition, or dissipation, produce any changes in the hygroscope. This, indeed, I should not venture to propose, but that eminent astronomers have casually observed great dryness to attend the extraordinary absence, or fewness of the solar spots. And such persons as are astrologically given, may extend their curiosity, in the use of this instrument, to observe, whether eclipses of the sun and moon, and the great conjunctions of the superior planets, have any remarkable effect upon it. I

The third use, to discover and compare the changes of the temperature of the air, made by winds, frosty, snowy, and other weather.

I have found, by removing the ſpunge into the wind, that it ſoon receiv'd a very conſiderable alteration, in point of weight; as it, alſo, did, when remov'd out of a room, into a garden, where the ſun ſhone: for, tho' the ſeaſon were not warm, it being then the month of *January*, yet, in three quarters of an hour, the ſpunge loſt a 24th part of its weight. We may, alſo, in ſome caſes, uſefully ſubſtitute a broad piece of ſheep's-leather, by diſplaying it to the wind. For this having, by reaſon of its thinneſs, in proportion to its breadth, a very large ſuperficies immediately expoſed to the wind, we found it to be conſiderably alter'd thereby; ſo that half an ounce of it, well-prepared, being kept, for an hour, in a place where the ſun-beams might not beat upon it, did, in a ſtrong wind, vary an eighteenth part of its original weight. But tho' I think it very poſſible to make ſuch obſervations of the temperature of particular winds, as will frequently prove ſo true as to be uſeful, at leaſt, to thoſe who live in the places where they are made; yet I am of opinion, that, to be able to ſettle general rules, to determine, with any certainty, the quality of winds, according to the points of the compaſs, from whence they blow, great warineſs will be required: and he who has not ſome competent skill in phyſics, and coſmography, muſt be very ſubject to miſtake in forming his rules.

For, winds that blow from the ſame quarter, are not, in ſome countries, of the ſame quality they are in moſt others; but participate much of the nature of the region over which they blow, in their paſſage to us. At *Archangel* they obſerve, that tho' a northerly wind, almoſt every where elſe, without the tropics, produces froſt in winter, it is there attended with a thaw; probably becauſe this wind comes over the ſea, which lies north from that place: and on the contrary, a ſoutherly wind, blowing over a thouſand, or twelve hundred miles, of frozen land, rather increaſes the froſt, than brings a thaw. The northern winds, which are elſewhere drying, are ſaid in *Egypt* to be moiſt. And we are told, that the northern wind in *Egypt*, is the moiſteſt. Monſieur *de Serres*, ſpeaking of the changes of the air, with regard to huſbandry, in ſeveral parts of *France*, informs us, that about *Tholouze*, the ſouth-wind dries the ground, and the north gives rains. On the contrary, from *Narbonne* to *Lyons*, all over *Provence* and *Dauphiné*, the latter cauſes dryneſs, and the other brings moiſture.

But, farther, the vehemence, or the faintneſs of the wind, though blowing over the ſame country, may much diverſify its operation on the hygroſcope; and the ſame wind, which, when it blows but moderately, appears moiſt by the hygroſcope, may, when vehement, or impetuous, make the inſtrument grow lighter; diſcuſſing and driving away more vapours, by the agitation of parts it makes in the ſpunge, than is balanced by thoſe aqueous vapours that are brought along with it. And froſty weather, as we found often, made the hygroſcope grow lighter, even at night; but ſnowy weather, which laſted not long, added ſomething to the weight of the ſpunge: and it has been obſerv'd, that miſts and foggy weather add a weight to it.

STATICS.

Nay, 'tis probable, that a transient cloud, in fair weather, may be manifestly observable by our instrument. For, the 9th of *September*, being for the most part, a very fair sun-shiny day, and tho', about ten a clock in the morning, the sun shone brightly, the spunge began to preponderate; which unexpected phenomenon made the person, in whose chamber the instrument was, look out at the window, where he discover'd a cloud that darken'd the sun; but, after a while, this being past, the balance return'd to an equilibrium. And, I have more than once, or twice, observed, especially in summer, that when the air grew heavier, the hygroscope either continued at a stand, or, perhaps, grew lighter; as if, when such cases happen, the effluvia that get into the air, either from the terrestrial, or some other mundane globe, were not fit, like vapours, to enter and lodge in the pores of the spunge; and so were corpuscles of another nature; with which, when we find by the baroscope, that the air is plentifully stock'd, it may be worth while to observe, whether any, and what kind of meteor, will commonly be signified, and produced.

A fourth use, to compare the temperature of different houses, and different rooms in the same house.

As it is of great use, both in respect of mens health, especially if they be of a tender, or sickly constitution; and for the conveniency of the keeping flesh, several sorts of wares, &c. that are subject to be damaged by moist air; so this may be readily and manifestly derived from our instrument. For, by removing it into several houses, or into several parts of the same house, and letting it stand, for a competent time, in each, we have often observed a notable difference; of which I shall here give two or three instances.

Having, upon the 10th of *October*, taken a piece of fine spunge out of a cabinet, and clip'd it till it weigh'd just half a dram, in a nice pair of scales, and a warm room; we afterwards removed them into a neighbouring room, destitute of a chimney. This statical hygroscope, was, *October* 12. at night, remov'd into the former room, and the spunge found to have gain'd three grains, and an eighth; and, consequently, more than a tenth part of its first weight: but, being suffer'd to stand in this warm room; in less than twelve hours, it lost a grain, and about one eighth of its former weight; though the time it stood in this room were, for the most part, night, and the weather rainy.

At another time, we took a piece of very fine spunge, which had formerly weigh'd just a dram; but having been, for many months, kept in a very warm room, it was grown much lighter: for, removing it into an upper chamber, in a neighbouring house, and weighing it in tender scales, in the evening 'twas found to want four grains, and three quarters, of a dram; and tho' there was a fire in the room, and the scales stood not far from it, yet, in a short time (the day being foggy and rainy) the spunge visibly depress'd its scale three eighths, and, the next morning, was found to want but one grain and a half, of a dram; so that it had gain'd about three grains and a quarter: and the following evening, being the second of *January*, it weigh'd one dram, a grain, and almost a half. So that in about one natural day, the spunge had acquired six grains from the moisture of the air; that

that is, a tenth part of its first weight; and a greater proportion to the weight it had the day before. The third of *January*, the weather being yet moist, the weight exceeded two grains: but about three or four of the clock, in the afternoon, it began to lose of that great weight, which diminish'd more by the next morning; the weather having chang'd that night, and become frosty.

A dram of spunge, that had, for several weeks, been kept in a dry room, was (*January* 10.) carried out into a room, where a fire is not usually kept; the weather being extraordinary foggy: the next morning it was brought into the former room; and though, now the weather was clear, it appear'd to have gain'd about eleven grains in weight; yet it presently lost two grains, by standing in this room.

The fifth use, to observe, in a chamber, the effects of the presence, or absence of a fire.

In small chambers, I have often observed a moderate fire to alter the weight of the instrument, placed at a distance from it, after it had been well kindled for a very little while; but, in wet weather, if the fire were not seasonably recruited, with fresh fewel, the decay of it would soon begin to be discernable by the instrument.

The sixth use, to keep a chamber in any assign'd degree of dryness.

Supposing the alteration of weight in our spunge, to depend, only, upon the degree of the moisture of the air; if a convenient part of the room be chosen for the hygroscope, and it be kept constantly there, 'tis easy, from time to time, to perceive when 'twill be requisite to increase, or moderate the fire, so as to keep the spunge at that weight it was of, when the temperature of the air of the chamber, as to dryness and moisture, was such as is desired. I have, sometimes, observed, with pleasure, how manifest and great a change, in the weight of our spunge, would be made, when the room was wash'd, and, for a considerable time after; tho' a large fire was kept in it, to hasten its drying.

Besides these several uses of our hygroscope, there may, possibly, be many others; and, perhaps, by a little alteration, and improvement, it might be made capable of shewing some difference betwixt steams of differing natures; as those of water, spirit of wine, chymical oils; or new kinds of substances in the air, wherein I suspect there may sometimes be dispersed great plenty of unknown corpuscles. For, I have, more than once, observed, the hygroscope not to be affected with the alteration of weather, answerably to what the manifest constitutions, or variations of it, seem'd plainly to require. Whether unobserv'd particles perform'd this, by making the other steams, in point of figure, or size, unfit to enter the minute pores of the spunge; or by dissipating, or otherwise procuring the avolation of more of the watery particles, than they could countervail, I shall not now examine. And, perhaps too, by associating this instrument with the thermoscope, baroscope, &c. it might be so improved, as to help us to foresee many considerable things, that either are, themselves, changes of the air, or consequences thereof: for instance, sickly and healthful constitutions of the air; barren, or plentiful seasons, in particular places or countries; strong hurricanes, earth-quakes, inundations, and their ill effects; and especially those accidents that depend much upon the surcharge of the air, with

other

STATICS. other exhalations, and moist vapours, which before-hand operate sensibly upon our instrument; and therefore may be discernable by it, long before they arrive at that height, which makes them formidable meteors. And, if it were but the foretelling approaching rain; this very thing may, on many occasions, prove very serviceable, and recommend our instrument, which often receives much earlier impressions from the steams that float in the air, than our senses do; so that I have been able to foretel a shower of rain, especially in dry weather, for a considerable time before it fell.

And here I must advertise, with regard to our instrument, as well as the barometer, that if a theory, or hypothesis, in itself rational, be found agreeable to what happens the most usually in observation; it ought not to be rejected, or laid aside; tho', sometimes, particular instances may call it in question. For, 'tis very possible, that the theory, or hypothesis, may be as good as a wise man would require, about so mutable a subject as the weather. And the cause assign'd by the hypothesis, may, really, act suitably to what that requires, tho' a contrary effect ensue, by reason of this cause being accidentally over-ruled, by some more powerful agent, which for that time invades the air. Thus we know, that tides do, in general, correspond with the motions of the moon; and generally ebb and flow at such times, and in such measures, as the theory, grounded on that correspondence, requires: yet seamen find, that in this, or that particular harbour, or mouth of a river, fierce contrary winds, great land-floods, and other casual causes, sometimes, both very much disturb the regular course of the tides, and either increase, or lessen them.

SECT. II.

Instances of the power of the moisture in the air, at all seasons.

STill farther, to recommend the use of hygroscopes, I shall here shew, that the quality they acquaint us with, may be very considerable, not only upon mens healths, but upon subjects far less tender, and less curiously contriv'd, than human bodies.

Upon animal substances.

That the moisture of the air may have no small influence, and, usually, a bad one upon mens healths, tho' experience did not so often teach it, I should directly infer, from its operation upon the dry and firm parts of animals; and even in those cases, where, for want of time, or other impediments, it cannot produce any sensible degree of putrefaction.

The skins of animals may easily be invaded by the moist particles of the air, because of the great number of their pores; which may be concluded from their hairiness, their sweat, or both. And I lately observed, that sheep's-leather imbibes the moisture of the air, and increases in weight upon it, as plentifully as almost any body I have expos'd to it.

But to shew, that much closer membranes, and which nature made to be impervious to such a liquor as urine, may be affected by the vapours of the air; I shall add, that having purposely taken pieces of fine, well-blown bladders, and, as far as appear'd, of a very close texture, and counterpois'd them in a good balance, I found they would considerably increase their

their weight in moist, and lose it again in dry weather; so that I might have employ'd the most membranous part of a bladder, to make a statical hygroscope. STATICS.

And tho' bones are the firmest and most solid parts of animals; and, as it were, the pillars by which their fabric is sustain'd; yet, even these may be pierc'd, and sensibly affected, by the moisture of the air. For, having caused the skeleton of a human body, to be so set together, that by the help only of slender wires, artificially order'd, the natural motions which the muscles make of the bones, might be well imitated; I observed, that tho' in dry, and fair weather, the flexures of the limbs might be readily made; yet, in very moist weather, the joints were not easily bent; as if the parts were grown stiff and rigid: which seem'd to proceed from the moist particles of the air, which having plentifully insinuated themselves, at the pores, into the bones, had every way distended them, and thereby made the parts bear harder against one another, at the junctures, or articulations.

But, farther, the moisture of the air may exercise a considerable force, *Upon vegetables.* even upon inanimate, and inorganical bodies. Even well-season'd wood, will suffer a kind of divulsion of a multitude of its parts, and be manifestly distended by aqueous corpuscles getting into its pores. Having procur'd a piece of sound and season'd wood, of about an inch in diameter, to be turn'd exactly cylindrical, and also a ring of brass, or ivory, to fit it; so that it might, with ease, be put on, and taken off: we set the piece of wood in fair water, and left it to soak there for several hours; at the end of which, it was visibly swell'd: so that the ring which was adjusted to it before, was manifestly too little to go again upon it; or, with its orifice, to cover the whole basis of the distended cylinder; which, afterwards, being dry'd in the air, shrunk into a capacity fit to enter the ring again. And, in this experiment, I took notice, that the great distension of the wood, was not produced all at once, or soon after it was put into the water, but it swell'd by degrees, and lay soaking there for several hours, before it arriv'd at its utmost dimensions; the aqueous corpuscles requiring, it seems, so much time to insinuate themselves, sufficiently, into the wood: which argues, that the internal parts were, likewise, affected, tho' when these came to swell, they had a great thickness of wood about them, to hinder their dilatation.

And to manifest, that even in summer, there are aqueous vapours floating in the air; within some days of *Midsummer*, and in clear sun-shiny weather, we took a pint glass-bottle, and having put into it a convenient quantity of water, placed it, and four ounces of beaten sal-armoniac in one scale of a good balance, and a counterpoise in the other; and then, putting the salt into the water, I observ'd, that tho' for a while the equilibrium remain'd, yet when the frigorific mixture had sufficiently cool'd the outside of the bottle, the roving vapours of the air, that chanc'd to pass along the surface of the vessel, were, by the contact of that cold body, arrested, and turn'd into a kind of dew, which, from time to time, made the scale, that held the glass, preponderate more and more: and, at length, the drops grow-

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STATICS. ing larger, ran, in ſmall rivulets, down the ſides of the glaſs. So that in leſs than an hour, the condens'd ſteams amounted to near a dram, which weight was, afterwards, much increas'd within about two hours more.

And that the multitude, figures, and motions of theſe particles, in the form of vapour, may enable them to diſplay no ſmall force in their operations on ſome bodies, appears from the breaking of the ſtrings of muſical inſtruments, firſt brought to a ſtrong tenſion, in rainy weather. For the vapours, which then wander through the air, inſinuating themſelves into theſe ſtrings, diſtend and ſwell them, and thereby endeavour to ſhorten them, ſo forcibly, that they often break with a ſmart noiſe and great violence: and if we conſider how great a weight ſome of the bigger ſtrings of baſe-viols, that have been obſerved to break in moiſt weather, require to ſtretch them till they break; this effect of the moiſt air will appear to require a very great force.

And even upon metals and minerals.

Beſides, an ancient muſician, and a great organ-maker, inform'd me, that the wire-ſtrings of muſical inſtruments will, for the moſt part, be out of tune in wet weather; the ſtrings then, generally, affording their notes ſharper than uſual; and that, upon great changes of weather, ſeveral organs would (after they had been long tuned) grow out of tune, the wooden pipes ſwell, and the metalline ones be untuned.

But if bodies be of ſuch a conſtitution, as to aſſiſt the operation of the moiſt air, the penetrancy and efficacy of it may be ſtill more conſiderable. For, there are ſome kinds of thoſe marcaſites that yield vitriol, which, whilſt they lie under ground, or are cover'd with the ſea-water, on ſhores, retain a ſtony hardneſs, and are often taken for mere ſtones; yet credible perſons have, caſually, obſerv'd, that theſe, being expoſed to the air, would, in tract of time, be ſo penetrated by the moiſt particles of it, as to ſwell and burſt. That this will happen to ſuch kind of ſtones, by the help of rain, experience has perſuaded me; and that it may, alſo, happen even to very hard and ſtone-like marcaſites, when they are merely expoſed to the air, I am apt to think, from ſome tryals of my own. For, from ſhining marcaſites, tho' kept but in my chamber-window, I have had vitriolate efflareſcencies, which ſeem'd to be produced by the action of the piercing moiſture of the air upon the mineral. And I remember, that very hard and heavy lumps of a marcaſitical ſubſtance, which ſeem'd to be ſtony, were ſo diſpoſed to be wrought on by the air, that tho' kept in my own chamber, or other cover'd places, yet in a ſhort time were ſo penetrated by the moiſt corpuſcles of the air, that they burſt into many pieces, which fell from one another; and ſeveral of the divided portions might, eaſily, be crumbled betwixt one's fingers. And of ſome of theſe I have obſerv'd, that a vitriolate ſubſtance was produced more plentifully in their innermoſt parts, than on or near their outſide. So that, conſidering how great an external force would have been requiſite to make ſuch a comminution of minerals thus ſolid and hard, 'tis obvious, that the air's moiſture is capable, when it meets with fitly-diſpoſed bodies, to exerciſe a far greater force than is uſually imagin'd.

And

STATICS.

The cord-hygrometer.

And thinking it probable, that ropes themſelves would conſiderably imbibe and part with the moiſture of the air, and that ſo as to ſhrink in rainy weather, tho' clogg'd with a weight faſten'd at the lower end; I firſt caus'd a rope, that was about twenty yards in length, but of no great thickneſs, to have one of its ends faſten'd to an immoveable body, at a convenient height from the ground, and a pully to be ſo faſten'd to another ſtable body, at the diſtance of eighteen or twenty yards from the firſt, that the rope, reſting upon the pully, lay almoſt horizontally. But to the end of that part of the rope, which, from the pully, reach'd within two or three feet of the ground, was faſten'd, by a ring, a leaden weight of fifty pound. To this was, alſo, faſten'd a light index, placed horizontally, whoſe end moved along an erected board, which by tranſverſe lines was divided into inches and parts of inches, reaching a conſiderable way upwards and downwards, that the index might, within thoſe bounds, have room to play, according to the alterations of the weather.

It being now ſummer, we placed this apparatus in a garden, tho' partly under a pent-houſe, that the rope might be more expoſed to the air, than it would have been within doors; and in two or three days, the weight had brought the rope to the utmoſt ſtretch that was able to give it: after which, it began manifeſtly to ſhrink and lengthen, according to the weather. So that once looking, over-night, upon the ſuſpended weight, and marking how low it reach'd upon the divided board; when I came again the next morning, a great part of the night having been rainy, I found the cord ſo ſhrunk, that the weight was rais'd above five inches: yet the day growing dry and windy, and ſometimes warm, the weight had, at night, ſtretched the rope more than the moiſture had contracted it before.

Afterwards having procured a far greater weight, I ſubſtituted it in the place of the other; and having ſuffer'd it to ſtretch the rope as far as it could, I regiſter'd ſome obſervations about it; two whereof I ſhall here ſet down.

June 4. At half an hour after nine at night, I look'd upon the hundred weight that hung at the bottom of the rope, the weather being then fair; and a mark being put at that part of the erected board, where the bottom of the weight touch'd, I perceiv'd the sky a while after to be cloudy and overcaſt, but without rain: wherefore going to view the weight again, I found it in an hour and a quarter to have riſen a quarter of an inch, or more.

June 6. Yeſterday the weight was obſerv'd to reſt at the eleventh inch of the erected board. This morning about eight, I found it riſen about half a quarter of an inch above the eighth; the morning being cloudy, tho' the ground very dry and duſty. The weather being overcaſt, within leſs than an hour afterwards, (ſome ſcatter'd drops of rain then beginning to fall) I found the weight riſen about half an inch above the eighth mark *.

Hence

* Dr. *Jurin*, to gain materials for an exact meteorological hiſtory, deſires the curious of all parts to keep a diary of the weather, and to ſend in their accounts of it, annually, to the ſecretaries of the Royal Society; where, being compared with thoſe made

STATICS. Hence the force of the air's moisture is not small, since it could raise an hundred weight, tho' the chord measured, in diameter, but about the third part of an inch.

made by the order of that illustrious body, the result may be publish'd in their Transactions. As such a work, for its great usefulness, is highly desirable, we shall here give the particulars, which the Doctor desires should be principally inserted in such a journal.

And, 1st, he requests, that the heights of the barometer, thermometer, and the state of the hygroscope, be observ'd, at least once every day; from what point of the compass the wind blows, and with what force; the appearance of the sky, and the quantity of rain or snow, that fell since the preceding observation was made.

2. That in case of a storm, its beginning, increase, height, decline, and cessation, be accurately remark'd, with the precise times wherein each happen'd, and the heights of the barometer corresponding to those times.

3. That the common barometer, artificially fill'd, be made choice of, the tube being $\frac{1}{4}$ or $\frac{1}{3}$ inch in diameter; and the vessel containing the stagnant mercury, 8 or 10 times wider than the tube.

4. If a portable barometer be employ'd, the Doctor recommends those of that excellent artist Mr. *Hauksbee*, in *Crane-Court*, *Fleetstreet*; who also (says the Doctor) makes most exquisite thermometers.

5. If any other thermometer be used, the Doctor desires that its situation, structure, scale, and the name of the person who made it, may be set down in the diary; and that this instrument be placed in a room that looks north, where a fire is seldom or never made.

6. That they may be the better compared together, the Doctor farther desires, that the first column may exhibit the day and hour of the observation, old stile; that the second may shew the height of the mercury in the barometer, in inches of an *English* foot, and their decimal parts; that the third column may contain the degree and decimal parts of a degree of the spirit in the thermometer; that to the fourth be assign'd the wind, with its degrees of force, which may be express'd by the figures 1, 2, 3, 4, and 0 for a perfect calm. That the fifth may contain the appearance of the heavens, and a short history of the weather. That the last may give the height of the rain or snow when dissolv'd, which fell since the preceding observation, in *English* inches, and their decimal parts.

The quantity of rain, this very learned gentleman tells us, may be easily computed by means of a funnel two or three feet wide, set in a close narrow-mouth'd vessel to receive the water falling in an open place, and a cylindrical measure divided into inches and decimal parts. But the diameter of the cylindrical measure should be 10 times less than that of the funnel; whence an inch height of water in the measure, will appear to have fallen to the height of $\frac{1}{100}$ inch upon the funnel, and so for other decimal parts of an inch. At the end of every month and year, the Doctor desires may be set down the mean menstrual and annual height of the barometer and thermometer, as likewise the sum of all the heights of the rain that has fallen each month or year. See *Philos. Trans.* N° 379. p. 427.

A

A specimen of this Diary take as follows.

Dies & Hora 1723. Nov. St. V.	Barom. alt. dig. dec.	Therm. alt. gr. dec.	Vent.	Tempestas.	Pluvia. dig. dec.
1. 8 *a. m.*	29.75	49 . 6	S.W. 1	Cœlum nubibus obduct.	0.035
4 *p. m.*	29.56	47 . 3	S.W. 2	Imbres interrupti. Sol per vices intercurrens	0.043
2. 7 ½ *a. m.*	29.24	48 . 5	S. 1	Pluvia fere perpetua	0.725
3. 9 *a. m.*	29.95	49 . 7	N. 1	Cœlum sudum	0.032
5 *p. m.*	30. 4	49 . 2	N. 1	Cœlum sudum	0.000
4. 7 *a. m.*	29. 9	47 . 0	S.W. 1	Nubes sparsæ	0.000
10	29. 7	46 . 2	S.W. 2	Imbres intercurrentes	0.103
12	29. 4	45 . 0	S. 3	Cœlum nubibus undique fere tectum	0.050
3 *p. m.*	28. 8	46 . 0	S. 4	Nubes sparsæ	0.000
5	28. 6	47 . 2	S.W. 4	Eadem Cœli facies	0.000
7	28. 9	48 . 0	S.W. 2	Pluit	0.000
9	28. 9	48 . 2	0	Pluvia fere perpetua	0.305
5. 7 *a. m.*	29. 7	53 . 4	N. E. 1	Sudum. Gelu.	0.250

Fire and Flame

Weigh'd in a

BALANCE.

SECT. I.

Experiments to ſhew that flame may incorporate with ſolid bodies, ſo as to increaſe their weight and bulk.

1. A Piece of copper-plate, not near ſo thick as a half-crown, weighing two drams and twenty-five grains, being placed, horizontally, in a crucible, whoſe bottom had a little hole in it, for the fumes to get out at; ſo that it could not be remov'd from its poſition, nor be eaſily made to drop down, and loſe its level, tho' the crucible were turn'd upſide down; about an ounce and half of common ſulphur, was put into a taller and broader crucible, and that wherein the copper ſtuck, inverted into the orifice of it, that ſo the ſulphur being kindled, the flame might reach the plate, and have ſome vent beyond it, at the hole above-mention'd. The brimſtone burn'd about two hours, in which time it ſeem'd all to have been reſolv'd into flame; no flowers of ſulphur appearing to have ſublim'd into the inſide of the upper crucible. And tho' the copper-plate were at a conſiderable diſtance from the fired ſulphur, yet the flame ſeem'd to have actually penetrated it, and to have made it viſibly ſwell, and grow thicker; which appear'd to be done by a real acceſſion of ſubſtance; ſince, after we had wiped off ſome little adhering *ſordes*, and, with them, ſeveral particles of copper, that ſtuck cloſe to them, the plate was found to weigh near thirty-two grains more than at firſt; and, conſequently, had increas'd its former weight, by above a fifth part.

2. Having, by refining one ounce of ſterling ſilver, with ſalt-peter, reduced it to ſeven drams, or ſomewhat leſs; we took a piece of it, that weigh'd fifty-eight grains, and ordering it, as the copper-plate, in the former experiment. After the flame of above one ounce and a quarter of ſulphur had, for about an hour and half, beat upon it, the ſilver plate ſeem'd,

to

to the eye, ſomewhat ſwell'd; and the lower ſurface of it, next the flame, was brought to a great ſmoothneſs; the weight being increas'd to one dram, five grains, and three quarters; which acquired weight, falling ſo ſhort of that gain'd by the copper, the difference may, perhaps, be attributed to the cloſeneſs, and compactneſs of the ſilver, argued by its being heavier, *in ſpecie*, than copper; or to the greater diſpoſition of the pores of copper, to be wrought on by the fiery menſtruum*.

Nor is it ſtrange, that, the particles of flame, which are either of a ſaline, or a piercing terreſtrial nature, being wedg'd into the pores, or being brought to adhere, very faſt, to the little parts of the bodies, expoſed to their action, the addition of ſo many little particles, that want not gravity, ſhould, becauſe of their multitude, be conſiderable upon a balance; tho' a few of them would have no viſible effect.

3. Into a very ſhallow crucible, we put one ounce of copper-plates, and ſet it in a cupelling furnace, where 'twas kept for two hours; and then being taken out, we weigh'd the copper, which had not been melted, (having firſt blown off all the aſhes) and found it had gain'd thirty grains. *That fire may do the ſame.*

4. We took an ounce of copper filings, and putting them upon a very ſhallow crucible, and under a muffler, we kept them there for about three hours; and afterwards taking them off, we found them of a very dark colour; not melted, but caked together, in one lump; and increas'd in weight, the aſhes and duſt being blown off, no leſs than 49 grains: part of which increaſe, above that obtain'd by the copper plates, may, probably, be due to the longer time that, in this experiment, the filings were kept in the fire.

5. We weigh'd out one ounce of ſmall lumps of hartſhorn, that had been burn'd till they appear'd white; and having put them into a crucible, and kept them in a cupelling furnace, for two hours, we found, when they were taken out, that they had loſt ſix or ſeven grains of their former weight; perhaps, becauſe the internal parts of ſome of them, might not be exquiſitely calcined, but retain'd ſome oleaginous, or other volatile ſubſtance; or becauſe, having omitted to burn them well, before they were weigh'd, they might, ſince their firſt calcination, have imbibed ſome moiſt particles of the air: for having kept them awhile in the ſcale, wherein they were weigh'd, they did, within two or three hours, make it preponderate. At the ſame time with this hartſhorn, we, alſo, put in one ounce of well-heated brick, and kept that, likewiſe, in the furnace, for above two hours; at the end of which,

* M. *Muſchenbroeck*, counterpoizing a priſm of iron, that weigh'd three pound, in ſcales which would turn with $\frac{1}{20}$ grain, afterwards heated the priſm red hot; when examining it again, by the ſame balance, he ſtill found it exactly three pound: whence he juſtly concludes, that it had gain'd in weight from the fire. For, the experiment being made in the open air, and the metal being expanded by heat, 'tis plain, that the ſpecific gravity of the heated metal, was decreas'd by the expanſion, ſo as to be then lighter, with reſpect to the air. *De materiâ ſubtili*, p. 8, 9.

weigh-

STATICS. weighing it, whilſt it continued hot, we did not find it to have either ſenſibly got or loſt; but, ſome time after, it ſeem'd, upon the balance, to have imbibed a little moiſture from the air.

6. Upon a good cupel, we put an ounce of *Engliſh* tin, of the better ſort; and having placed it in the furnace, under a muffler, tho' it preſently melted, yet it did not forſake its place, but remain upon the concave ſurface of the cupel, till, at the end of about two hours, it appear'd to have been well-calcin'd; and then being taken out, and weigh'd by itſelf, the ounce of metal was found to have gain'd no leſs than one dram.

7. An ounce of lead was put upon a cupel, made of calcined hartſhorn, and placed under a muffler, after the cupel was firſt made hot, and then weigh'd. This lead did not enter the cupel, but was turn'd into a kind of litharge on the top of it, and broke the cupel, whereby ſome part of the latter was loſt in the furnace; yet the reſt, together with the litharge, weigh'd ſeven grains more than the lead and heated cupel, when they were put in.

8. We took a cupel about two ounces in weight, made of ten parts of bone-aſhes, and one of charcoal-aſhes, mix'd together with ale. This was, by it ſelf, put in a cupelling furnace, under a muffler; and when 'twas throughly and highly heated, we weigh'd it in that ſtate. This done, 'twas immediately again plac'd under the muffler, and kept there for about two hours; at the end of which time, 'twas taken out, red-hot, and preſently put into the ſame balance, as before; where, having blown off the adhering aſhes, I found that, tho' when 'twas firſt taken from under the muffler, we had but two ounces, and two grains, yet now, the weight being put into the oppoſite ſcale, it gain'd, very near 21 grains. And here note, that 'twas not without ſome cauſe, that I weigh'd the cupel red-hot: for I had a ſuſpicion, that notwithſtanding the dryneſs of the bone, it might receive ſome little alteration of weight, by imbibing the moiſt particles wandering in the air; and accordingly, leaving the cupel counterpois'd, to cool in the balance, in a ſhort time, it began ſenſibly to preponderate; and ſuffering it to continue there, for nine or ten hours, I found it about three grains heavier than before.

9. Steel being a metal that, as experience inform'd me, will, very eaſily, be wrought on by fluids of a ſaline nature, 'twas reaſonable to expect, that flame would have a greater operation on it, eſpecially if it were, before-hand, reduced to ſmall parts, than on any of the bodies hitherto deſcribed. And, accordingly, four drams of the filings of ſteel, being kept two hours on a cupel, under a muffler, acquired one dram, ſix grains and a quarter, increaſe of weight.

10. A piece of refined ſilver, being put upon a cupel under a muffler, and kept there for an hour and a half, was taken out, and weigh'd again; and as before it weigh'd three drams, thirty-two grains, and a quarter, it now weigh'd in the ſame ſcales, three drams, thirty-four grains, and a half.

We

We repeated the experiment with half an ounce of the filings of ſilver, well refin'd with lead, in our own laboratory, and kept them about three hours upon the cupel; when taking them out, we found them of a leſs pleaſant colour, than before, and melted into a lump, which weigh'd four drams, ſix grains.

11. We took a dram of the filings of zink, or ſpelter, and, having put it upon a cupel, under a muffler, we kept it there, in a cupelling fire, for about three hours; then taking it off, we found it to be caked into a brittle dark-colour'd lump, which look'd as if the filings had been calcined. This being weigh'd in the ſame ſcales, gain'd full ſix grains.

12. Among our various trials, upon common metals, we thought fit to make one or two upon a metal brought us from the *Eaſt-Indies*, and there called *Tutenag*. Two drams of the filings of tutenag, being put upon a cupel, and kept under the muffler for about two hours, the filings were not melted into a lump of metal; but look'd as if powder'd ceruſe and minium had been mix'd together; ſome of the parts appearing diſtinctly white, and others red; the calx being put into the balance, appear'd to have gain'd 28 $\frac{1}{4}$ grains. Another time, the experiment being repeated, with the like circumſtances, we found that two drams of filed tutenag, gave almoſt the like increaſe of weight. So that this *Indian* metal, ſeems to have acquir'd more in the fire, in proportion to its weight, than any we have hitherto made trial of.

13. To confirm, by a clear experiment, that in all cupellations, the lead then employ'd, does not, together with the baſer metals, to be purg'd off from the ſilver, or gold, fly away in ſmoke; we took two ounces of good lead, and one dram of filings of copper; and having caus'd a cupel to be ignited, and ſuddenly taken out of the furnace, and weigh'd, whilſt 'twas very hot, we preſently put it back, together with the two metals laid on it, into the furnace: where, having been kept for about two hours, it was taken out again, and found to have nothing on the ſurface of it, worth weighing diſtinctly, in the ſcales, wherein the cupel, with what was ſunk into it, amounted to four ounces, three drams, and eleven grains; which wanted but nine grains of the whole weight of the cupel, and the two metals, when they were, all three together, committed to the fire. So that tho' we make a liberal allowance for the increaſe of weight, that may, with any probability, be ſuppoſed to have been attain'd by the cupel, and what was put upon it, yet it will eaſily be granted, that much the greater part of the metals, was not driven off in fumes, but enter'd into the ſubſtance of the cupel.

After having ſhewn, that either flame, or the analogous effluvia of the fire, will be incorporated with metals, and minerals, expoſed naked to its action, I thought proper to try whether this flame, or igneous fluid, were ſubtile enough to exerciſe any ſuch operation upon the like bodies, ſhelter'd from its immediate contact, by being included in cloſe veſſels.

STATICS.

Even tho' the bodies be not immediately exposed to it.

14. We took an ounce of new filings of ſteel, and having included them betwixt two luted crucibles, kept them, for two hours, in a ſtrong fire, and ſuffer'd them to continue there till the fire went out; the crucibles being unluted, the filings appeared hard caked together, had acquired a dark colour, ſomewhat between black and blue, and were increaſed five grains in weight. At another time, an ounce of filings of ſteel, being put between two crucibles, in the ſame manner, after they had been kept about an hour and half in the fire, were taken out, and found to have gain'd ſix grains in weight.

15. We put two ounces of copper-plates into a new crucible, over which a leſſer was whelm'd, and luted the junctures; after the ſame manner, two ounces of tin were included betwixt crucibles; and alſo, two ounces of lead: and all theſe, being put into the cupelling furnace, were kept in a ſtrong fire, for about an hour and a half; and then being taken out, the copper-plates, tho' they ſtuck together, were not quite melted; and ſeem'd, ſome of them, to have acquired ſcales, like copper put into a naked fire; and the two ounces had gain'd eight grains in weight. The lead had broke thro' the bottom of the crucible, and thereby hinder'd the deſign'd obſervation. The tin acquired ſix grains in weight, and was, in part, brought to a pure white calx; but much more of it was melted into a lump, of a fine yellow colour, almoſt like gold, but deeper.

16. We took one ounce of very thin copper-plates, and putting them betwixt two luted crucibles, we kept them in the cupelling furnace for about three hours; and then disjoining the veſſels, we found the metal cover'd with a dark and brittle ſubſtance; which, when ſcaled off, diſcloſed a finely colour'd metal, that, together with theſe burnt ſcales, amounted to twenty-one grains above the weight firſt put in.

17. We took a piece of fine block-tin, in weight half a pound; this we put into a choice glaſs-retort, and kept it for two days in a ſand furnace; which gave heat enough to keep the metal in fuſion, without cracking the glaſs: then taking out the mixture, we carefully weigh'd it in the ſame ſcales, and found the ſuperficies a little alter'd, as if it were diſpoſed to calcination; and the weight to be increas'd about two grains.

And alſo after they have been calcined.

18. We farther try'd how much ſome bodies would increaſe in weight, by the fire, when they had already been expoſed to the vehemence of it. One ounce of calx of tin, made *per ſe*, being put in a new cupel, and kept under the muffler for about two hours, was taken out hot, and put into the ſcales, where the powder appear'd to have gain'd in weight, one dram, thirty-five grains, by the operation of the fire; which made it alſo look much whiter than it did before.

19. Out of a parcel of filings of ſteel, that had been before expoſed to the fire, and had conſiderably increas'd its weight thereby; we took an ounce, and having expoſed them at the ſame time with the calx of tin, and for the ſame time kept them in the fire, we took them out at the two hours end, and found the weight to be increaſed, two drams, twenty-two grains. The filings were very hard caked together, and the lump being broken, look'd almoſt like iron.

20. An

20. An ounce and four drams of copper, which had endured a violent fire for an hour and half, being included betwixt two crucibles, and exposed to a ſtrong fire for two hours, and ſuffer'd to continue there till the fire went out, appear'd, thereby, to have gain'd ten grains in weight, and to have, upon the ſuperficial parts of the plates, ſeveral dark-colour'd flakes; ſome of which ſtuck to the metal; but more, upon handling it, fell off.

21. We took eight ounces of block-tin, which, being cut into bits, was put into a good round vial, with a long neck; and then warily held over quick-coals, without touching them, till it was melted; after which, it was kept almoſt continually ſhaking, to promote the calcination, for near an hour; the metal being all the while in fuſion, and the glaſs kept at ſome diſtance from the coals. For the greateſt part of this time, the orifice of the vial was cover'd with a cap of paper, to keep the air, and ſteams of the coals, from getting into the neck: the vial, being afterwards remov'd from the fire, was broken, and the metalline lump had a little darkiſh calx, here and there, upon the upper ſurface; but much more beneath, where it had been contiguous to the bottom of the glaſs. Then putting all this, (carefully freed from little fragments of broken glaſs) into the ſame balance, with the ſame counterpoiſe I had uſed before, I found an increaſe of weight, amounting to eighteen grains, which the tin had acquired by this operation.

22. We then ſeparated the calx; and having melted the metal in a crucible, that, by pouring it out, it might be reduced into thin plates, capable of being cut in pieces, and put into ſuch another vial as the former, we weigh'd it again, together with the reſerv'd calx, but found, that notwithſtanding all our care, we had loſt three grains of the eighteen gain'd before. This done, we put the metal into another vial; which having a ſtopple of paper, to keep out ſmoke and air, was held at ſome diſtance from the coals, only whilſt the tin was melting; being afterwards warily laid upon them, and kept there for two hours; at the end of which, 'twas again taken off, and the metal weigh'd with the ſame counterpoiſe, and balance, as before; and then it amounted to eight ounces, twenty-four grains; and had much more ſeparable calx, than at the firſt time.

Nor did I much wonder, that the weight ſhould be increas'd, in this laſt operation, but nine grains, in two hours; and, in the former twice as many, in half the time; ſince, during the two hours the glaſs was kept in one poſture: but, in the firſt operation, it was, almoſt, perpetually ſhaken, during the time 'twas kept in fuſion; for 'tis obſerv'd, that the agitation of melted minerals, will much promote the effect of the fire upon them, and conduce to their calcination.

23. But, to prevent all ſuſpicion of any increaſe of weight, in the metal, ariſing from ſmoke, or ſaline particles, getting in at the mouth of the veſſel, I made the experiment in glaſſes, hermetically ſeal'd, as follows. Eight ounces of good tin, carefully weigh'd, we hermetically ſeal'd up in

STATICS. a new ſmall retort, with a long neck, by which 'twas held in the hand near a charcoal fire, that kept the metal in fuſion; being now and then ſhaken for almoſt half an hour; in which time, it ſeem'd to have acquir'd, on the ſurface, ſuch a dark colour, as argued a beginning calcination; and it both emitted fumes that play'd up and down, and, alſo, afforded two or three drops of liquor, in the neck of the retort. The glaſs was, at length, laid on quick-coals, where the metal continued above a quarter of an hour longer in fuſion; but, before the time was come, that I intended, to ſuffer it to cool, in order to its removal, it ſuddenly broke, into a great multitude of pieces, and with a noiſe, like the report of a gun. In the neck we found ſome drops of a yellowiſh liquor, which had an odious and peculiar taſte; its ſmell alſo, was very fetid, not unlike the diſtill'd oil of fiſh.

24. Some tin, which had been, before, partly calcined in a glaſs, being melted again in a crucible, we weigh'd out juſt eight ounces, and theſe we put into a bolt-head of white glaſs, with a neck above twenty inches long, which being hermetically ſealed, after the glaſs had been a while kept over the fire leſt it ſhould break by the rarifaction of the air, the metal was kept in fuſion for an hour and a quarter. Being unwilling to venture the glaſs any longer, it was taken from the fire, and when grown cold, the ſeal'd end was broken off; but before I could have the bottom cut out, I obſerv'd, that the upper ſurface of the metal was very darkly colour'd, and very irregularly rough; and the lower part had, between the bottom, and the under ſide of the lump, a pretty deal of looſe dark-colour'd calx, tho' the neighbouring ſurface, and ſome places of the lump it ſelf, look'd, by candle-light, of a golden colour. The lump, and calx together, were weigh'd in the ſame ſcales carefully, when we found the weight to have increas'd above twenty-three grains; tho' all the calx we could eaſily ſeparate, being weigh'd by it ſelf, amounted not to eighty grains.

25. Two ounces of filings of tin, were carefully weigh'd, and put into a little retort, whoſe neck was afterwards drawn ſlender to a very ſmall apex; then the glaſs was placed on kindled coals, which drove out fumes at the ſmall orifice of the neck, for a pretty while. Afterwards, the glaſs, being ſealed up at the apex, was kept in the fire for above two hours; and then being taken off, was broken at the ſame apex: whereupon I heard the external air ruſh in, becauſe, when the retort was ſealed, the air, within it, was highly rarified. Then the body of the glaſs being broken, the tin was taken out, conſiſting of a lump, about which there appear'd ſome grey calx, and ſome very ſmall globules, which ſeem'd to have been filings melted into that form. The whole weigh'd two ounces, and twelve grains. In the neck of the retort, where it was join'd to the body, there appear'd a yellowiſh, and clammy ſubſtance, thinly ſpread, which ſmelt almoſt like the fetid oil of tartar.

26. We took an ounce of the filings of zink, carefully weigh'd; and having, as carefully, put them into a round bolt-glaſs, we caus'd the neck

to

to be drawn out very ſlender, and then order'd it to be ſet upon quick-coals, where, being kept for four hours, there appear'd, for great part of the time, ſmoke to aſcend from the zink, and get out at the unſtopp'd apex. And I obſerv'd, that the upper part of the glaſs was lined with a darkiſh grey ſublimate. The glaſs being dexterouſly cut aſunder, we took out, not only the filings of zink, ſome of which were melted into little globules, but the *flores* too; yet, weighing all theſe in the ſame ſcales we had uſed before, we found above five grains wanting of an ounce.

27. Having carefully weigh'd out four ounces of good lead, cut into little pieces, I put them into a ſmall retort with a long neck, wherein was, afterwards, left only an orifice, not much bigger than a pin's-head; this glaſs was kept over, and upon the coals, for above two hours; and then, ſuppoſing the danger of the glaſs's breaking to be over, we ſeal'd it up at the little orifice, and kept it on the coals for two hours longer. Before we broke the glaſs, I perceiv'd the pieces of lead to have been melted into a lump, whoſe ſurface was dark and rugged, and part of the metal to have been turn'd into a dark-colour'd powder, or calx. All this being taken out of the retort, was weigh'd in the ſame balance, whereon the lead appear'd to have gain'd, by the operation, ſomewhat above thirteen grains.

28. To ſhew that metals are not the only bodies capable of receiving an increaſe of weight from the fire, we ſeal'd up two drams of little bits of good red coral, hermetically, in a thin bubble of glaſs, and kept them warily, at ſeveral times, over and upon kindled coals; and, at length, being taken out, found them of a very dark colour, and to have gain'd, in weight, about 3 $\frac{1}{2}$ grains.

29. And having taken very ſtrong freſh quick-lime, provided on purpoſe for choice experiments, and expoſed it, before the air had time to ſlake it, upon the cupel, to a ſtrong fire, where it was kept for two hours; I found that it had increaſed, in weight, beyond my expectation: for, being ſeaſonably put into the balance, the lumps that weigh'd, when expoſed, but two drams, amounted to two drams and twenty-nine grains. Hence it appears, that, notwithſtanding a body may, for many hours, or even for ſome days, be expoſed to a very violent fire; yet it may be ſtill capable of admitting, and retaining freſh corpuſcles: ſo that, tho' well-made lime be uſually obſerv'd to be much lighter than the ſtones whereof 'tis made; yet the lightneſs does not neceſſarily prove, that, becauſe a burnt lime-ſtone has loſt much of its matter by the fire, it has, therefore, acquired no matter from the fire; but only infers, that it has loſt far more than it has got. And this may give ground to ſuſpect, that, in moſt of the foregoing trials, the acceſs of the fiery particles was greater than the balance diſcover'd; ſince, for ought we know, many of the leſs fix'd particles of the expoſed body, might be driven away, by the vehemence of the heat, and, conſequently, the fiery corpuſcles that faſten'd themſelves to the remaining matter, might be numerous enough, not only to give the additional weight, found by the ſcales; but to make amends for all the fugitive particles that had been expell'd by the fire. And, ſince

STATICS. ſo fix'd a body as quick-lime, is capable of being wrought upon by the fiery effluvia; ſo that they come to be, as 'twere, incorporated with it; perhaps, in other calcin'd or incinerated bodies, the remaining calces or aſhes may retain more than the bare impreſſion of the fire. By this experiment, and thoſe made in ſeal'd retorts, which ſhew, that what is afforded by fire, may, in a corporeal way, invade, adhere, and add weight even to fix'd and ponderous bodies; there is a large field open'd, for the ſpeculative to apply the diſcovery to many phenomena of nature and chymiſtry.

SECT. II.

Glaſs pervious to ponderous parts of flame.

TO obviate ſome ſcruples, that might be entertain'd upon this circumſtance of our experiments, that the glaſſes employ'd about them were not expoſed to the action of mere flame, but held upon charcoal; I attempted to make them ſucceed with a body ſo thin, and diſengaged from groſs matter, as mere flame is allow'd to be.

1. Suppoſing, then, that good common ſulphur, by reaſon of its great inflammability, and the vehemency and penetrancy of its flame, would be a very fit fewel for my purpoſe; I provided a ſmall double veſſel, ſo contriv'd, that the one part ſhould contain as many coals as were neceſſary to keep the ſulphur melted; and the other, which was much ſmaller, and ſhaped like a pan, ſhould contain the brimſtone requiſite for our tryal; and that theſe two ſhould be, with a convenient lute, ſo join'd to one another, that all being cloſed at the top, except the orifice of the little pan, the fire and ſmoke of the coals having their vent another way, no fire ſhould come at the retort to be employ'd, but the flame of the burning brimſtone. Then two ounces of filings of tin being carefully weigh'd out, and put into a glaſs retort, provided for ſuch trials, and made fit to be eaſily ſeal'd up at the neck; the ſulphur, which ought to be of the purer ſort, was kindled, and the glaſs, by degrees, expoſed to it; where that continued near two hours before the metal melted: after which, the retort was there kept on near an hour and a half more, with the metal melted in it; then perceiving a pretty deal of darkiſh calx at the bottom, and partly, too, upon the ſurface of the far greater portion of the metal, which now lay in one lump; the part of the retort which had been ſeal'd, being broken off, we firſt took out the calx, and next the lump, and putting them into the ſcales, they had been formerly weigh'd in, we found them to have acquired four grains and a half. The experiment was afterwards repeated with the ſame quantity of filings of the ſame metal, and at the end of the operation, which in all laſted ſomewhat above three hours, having broken off the ſeal'd neck of the retort, we found that a large proportion of dark-colour'd calx had been produced: and this being weigh'd with the uncalcin'd part of the metal, the two ounces, we firſt put in, appear'd to have acquir'd no leſs than eleven and a half.

Such

Such ſuperſtructures may, poſſibly, in time, be built on this and the like experiments, that I ſhall here obviate even a ſcruple, which appears too ſceptical. But, conſidering that tho' it were very improbable, yet poſſible, that the increaſe of weight acquired by bodies expoſed in glaſs veſſels to the fire, might proceed not from the corpuſcles of fire, but from the particles of the glaſs itſelf, looſen'd by the power of ſo intenſe an heat, and forcibly driven into the incloſed body; I took two glaſſes, the one ſhaped into a little retort, and having weigh'd them, kept them for a conſiderable time upon kindled coals, and then weigh'd them again, I could gather little of certainty from the experiment; (the retort at one time ſeeming to have acquired above half a grain in the fire) only that there was no likelihood at all, ſo conſiderable an increaſe of weight, as we ſeveral times obtain'd in cloſed veſſels, ſhould proceed from the glaſs itſelf, and not from the fire. STATICS.

2. We carefully weigh'd out an ounce of the filings of block-tin, and put them into a glaſs retort, whoſe neck was afterwards drawn out ſlender; we alſo provided ſuch a convenient metalline lamp, that the flame of the ſpirit burnt in it, might not melt or crack the glaſs. This lamp, tho furniſh'd with a cotton wieck, afforded no ſoot; becauſe, as long as it was well ſupplied with liquor, it remain'd unburnt. Theſe things being in readineſs, the retort was warily approach'd to the flame, and the metal thereby in a ſhort time melted. After which, the glaſs being kept expoſed to the ſame flame, for near two hours, the ſeal'd apex of the retort was broken off; when there appear'd to have been produced a conſiderable quantity of calx, that lay looſe about the remaining part of the tin, which, upon its growing cold was harden'd into a lump. This and the calx being taken out of the retort, with care, that no little fragment of glaſs ſhould impoſe upon us, it was weigh'd in the ſame ſcales as formerly, and found to have gain'd $4\frac{1}{2}$ grains, beſides the duſt that ſtuck to the inſide of the retort; of which we reckon'd enough to make about half a grain more: ſo that of ſuch a fine and pure flame as of this totally ardent ſpirit, enough to amount to five grains was arreſted, and, in good meaſure, fix'd, by its operation on the tin.

3. For confirmation of the laſt experiment, wherein we had employ'd the inflammable ſpirit of ſugar, we made the like with highly rectify'd ſpirit of wine; only ſubſtituting an ounce of lead inſtead of one of tin. The event was, that after the metal had been, for above two hours, kept in the flame, the ſeal'd neck of the retort being broken off, the external air ruſh'd in with noiſe; and we found above ſeven ſcruples of lead turn'd into a greyiſh calx; which, together with the reſt of the metal, being weigh'd again, ſix grains appear'd to be gain'd by the operation. Theſe ſeven ſcruples of calx being weigh'd in air and water, I found, that tho' the abſolute gravity of the metal had been increaſed by the particles of flame which ſtuck faſt to it; yet this aggregate of lead, and extinguiſhed flame, had loſt much of its ſpecific gravity: for lead is to water of the ſame bulk, as about $11\frac{1}{2}$ to 1; whilſt this ſubtile calx of lead was to water of the ſame bulk, but as 9 to 1. Now

STATICS.

Corollaries from this discovery.

Now whence can this increaſe of abſolute weight, obſerv'd in the metals expoſed to the mere flame, be deduced, but from ſome ponderous parts of the flame? And how could theſe parts invade thoſe of the metal incloſed in a glaſs, otherwiſe than by paſſing thro' the pores of that glaſs?

But I do not, by theſe experiments, pretend to make out the poroſity of glaſs, any farther than with regard to ſome of the ponderable parts of flame; for, otherwiſe, glaſs is not eaſily penetrable.

That flame may act as a menſtruum, and make coalitions with the bodies it works on.

Upon the whole, it appears, 1ſt, That flame may be a menſtruum, and work on ſome bodies, not only by making a notable comminution, and diſſipation of their parts, but, alſo, by a coalition of its own particles with thoſe of the fretted body; and thereby, permanently, add ſubſtance and weight thereto. Nor is it repugnant to flame's being a menſtruum, that, in our experiment, the lead and tin expoſed to it, were but reduced to powder, and not diſſolved in the form of a liquor, and kept in that ſtate. For, beſides that the interpoſed glaſs hinder'd the igneous particles from getting thro' in plenty enough, 'tis not neceſſary that all menſtrua ſhould be ſuch ſolvents. For whether it be, that the menſtrua we think ſimple, may be compounded of very different parts, whereof one may precipitate what is diſſolved by the other, or for ſome other cauſe; it is certain, that ſome menſtrua corrode metals, and other bodies, without keeping any conſiderable part diſſolved; as may be ſeen, if you put tin to a certain quantity of *Aqua fortis*, which will, in a very ſhort time, reduce it, almoſt totally, to a very white ſubſtance, that, when dry, is a kind of calx. And ſo, with a due proportion of oil of vitriol, abſtracted from quick-ſilver, by a ſtrong fire, we have ſeveral times reduced the main body of the mercury into a white powder, whereof but an inconſiderable part was diſſoluble in water. And ſuch a white calx I have had, by the action of another fretting liquor on a body not metalline. But whether, as it ſeems probable, it be from the vehement agitation of the permeating particles of flame, that violently tear aſunder the metalline corpuſcles; or, from the nature of the igneous menſtruum, which being, as 'twere, ſtrain'd thro' glaſs itſelf, muſt be ſtrangely minute; 'tis worth obſerving how ſmall a proportion, in point of weight, of the additional adhering body, may ſerve to corrode a metal, in compariſon of the quantity of vulgar menſtrua requiſite for that purpoſe. For, tho' we are obliged to employ, to make the ſolution of crude lead, ſeveral times its weight of ſpirit of vinegar, or a large proportion of *Aqua fortis*; 'twas obſerved in our experiment, that tho' the lead was increaſed but ſix grains in weight, yet above a hundred and twenty of it were fretted into powder; ſo that the corroſive body appear'd to be but about the twentieth part of the corroded.

That calces of metals are the magiſteries of them.

2dly, Another conſequence, deducible from our diſcovery of the perviouſneſs of glaſs to flame, may be this, that there is cauſe to queſtion the truth of what is, generally, taken for granted about calcination; and, particularly, of the notion that chymiſts have entertain'd about the calces of metals and minerals. For, it does not appear, by our trials, that any proportion, worth regarding, of moiſt and fugitive parts, was expell'd in the

the calcination; but it appears very plainly, that, by this operation, the metals gain'd more weight than they loſt: ſo that the main body of the metal remain'd entire, and was far from being either elementary earth, or a compound of earth and fix'd ſalt. From which very erroneous hypotheſis, the chymiſts infer the ſweet vitriol of lead, which they call *Saccharum Saturni*, to be but the ſweet ſalt of it, extracted only by the ſpirit of vinegar, which does, indeed, plentifully concur to compoſe it. Whence, I conclude, that the calx of a metal, even made *per ſe*, that is, by fire alone, may be, at leaſt in ſome caſes, not the *Caput mortuum*, or *Terra damnata*, but a magiſtery of it. For, in the ſenſe of the moſt intelligible of the chymical writers, that is properly a magiſtery, wherein the principles are not ſeparated; but the bulk of the body being preſerved, it acquires a new and convenient form, by the addition of the menſtruum, or ſolvent, employ'd about the preparation. Beſides, I have, ſeveral times, from the calx of lead, reduced actual lead. And, having once taken but about a third or fourth part of the calx of lead, produced by the flame of ſpirit of wine, I found, that, without any flux-powder, or other addition, but merely by the application of the flame of highly rectify'd ſpirit of wine, there would, in a ſhort time, be obtain'd a conſiderable proportion of malleable lead; whereof the part I examined, was truly ſo.

The effects of fire upon bodies, in cloſe glaſſes, not wholly owing to the agitation of the glaſs.

3dly, One uſe, among the reſt, we may make of the foregoing diſcovery, regards a controverſy among the corpuſcular philoſophers. For ſome of them think, that when bodies are expoſed in cloſe veſſels to the fire, tho' the igneous corpuſcles do not ſtay with the bodies they invade; yet they, really, get thro' the pores of the interpoſed veſſels, and permeate the included bodies in their paſſage upwards: whilſt others will not allow the igneous corpuſcles, which they take to be but vehemently agitated particles of terreſtrial matter, to penetrate ſuch minute pores as thoſe of glaſs; but ſuppoſe the operation of the fire to be perform'd by the vehement agitation made of the ſmall parts of the glaſs, and by them propagated to the included bodies; whoſe particles, by this violent commotion, are greatly alter'd.

But tho' it be probable, that the briſk agitation communicated by the ſmall parts of the glaſs to thoſe of the body contain'd in it, may contribute much to the effect of the fire; and tho', by the ſmall increaſe of weight we found in our expoſed metals, 'tis very likely that far the greater part of the flame was excluded by the cloſe texture of the glaſs; yet, on the other ſide, 'tis plain, that igneous particles paſs'd thro' the glaſs, and not only divided and agitated the included bodies, but manifeſtly and permanently adhered to them.

Particles extremely volatile may, by aſſociating with others, quite loſe their volatility.

4thly, We receive this further information from our experiments, that bodies very ſpirituous, fugitive, and minute, may, by being aſſociated with proper particles, tho' of quite another nature, ſo change their former qualities, as to be arreſted by a ſolid and ponderous body, to that degree, as not to be driven away from it by a fire intenſe enough to melt and calcine metals. For the foregoing trials ſeem plainly to diſcover, that

STATICS. that even the agitated parts of flame, minute enough to paſs thro' the pores of glaſs itſelf, were, ſome way, entangled among the metalline particles of tin and lead; and thereby brought to be ſo fix'd, as to endure the heat that kept thoſe metals in fuſion, and gradually reduced them to calces: a phenomenon that one would not eaſily look for; eſpecially conſidering how ſimple a texture that of lead, or tin, may be, in compariſon of the more elaborate ſtructures of many other bodies. And this phenomenon, which ſhews us what light and fugitive particles of matter, may permanently, concur to the compoſition of ponderous and fix'd bodies, will, perhaps, afford uſeful hints to the ſpeculative; eſpecially, if this ſtrict combination of a ſpirituous and fugitive ſubſtance, with ſuch as, being groſs and unwieldy, are leſs fit than organized matter, to entangle, or detain them, be apply'd, as it may be, with advantage, to thoſe aggregates of ſpirituous corpuſcles, and organical parts, that make up the bodies of plants and animals. And this hint may ſuggeſt a conſiderable inference to be drawn from the operation of the ſun-beams on appropriated ſubjects; ſuppoſing it to prove like that of flame on tin and lead *.

* The following queries of Sir *Iſaac Newton*, give us the beſt light we have into the nature of fire and flame. "Is not," ſays that great philoſopher, "fire a body, "heated ſo hot, as to emit light copiouſ- "ly? For, what elſe is a red-hot iron, "than fire? And, what elſe is a burning "coal, than red-hot wood? Is not flame "a vapour, fume, or exhalation, heated "red-hot; that is, ſo hot as to ſhine? "For, bodies do not flame, without emit- "ting a copious fume; and this fume burns "in the flame. The *Ignis fatuus* is a vapour, "ſhining without heat: and, is there not "the ſame difference between this vapour, "and flame, as between rotten wood, "ſhining without heat, and burning coals "of fire? In diſtilling hot ſpirits, if the "head of the ſtill be taken off, the vapour, "which aſcends out of the ſtill, will take "fire at the flame of a candle, and turn "into flame; and the flame will run a- "long the vapour, from the candle, to the "ſtill. Some bodies, heated by motion, "or fermentation, if the heat grow in- "tenſe, fume copiouſly; and, if the heat "be great enough, the fumes will ſhine, "and become flame. Metals, in fuſion, "do not flame for want of a copious "fume. Salt-petre fumes copiouſly, and "thereby flames. All flaming bodies, as "oil, tallow, wax, wood, foſſil, coals, pitch, "and ſulphur, by flaming waſte, and vaniſh "into burning ſmoke; which ſmoke, if "the flame be put out, is very thick, and "viſible; and ſometimes ſmells ſtrongly: "but, in the flame, it loſes its ſmell, by "burning: and, according to the nature "of the ſmoke, the flame is of ſeveral co- "lours; as that of ſulphur, blue; that of "copper, open'd with ſublimate, green; "that of tallow, yellow; that of camphire, "white. Smoke paſſing thro' flame, can- "not but grow red-hot; and, red-hot "ſmoke can have no other appearance "than that of flame. When gun-powder "takes fire, it goes away into flaming "ſmoke. For the charcoal and ſulphur, "eaſily take fire, and ſet fire to the ni- "tre; and the ſpirit of the nitre, being "thereby rarified into vapour, ruſhes out, "with exploſion, much after the manner "that the vapour of water ruſhes out of "an æolipile; the ſulphur, alſo, being vo- "latile, is converted into vapour, and "augments the exploſion. And, the acid "vapour of the ſulphur, namely, that "which diſtils under a glaſs-bell, into oil "of ſulphur, entering violently into the "fixed body of the nitre, ſets looſe the "ſpirit of the nitre, and excites a great "fermentation, whereby the heat is far- "ther augmented, and the fixed body of "the nitre is alſo rarified into fume; "and the exploſion is, thereby, made "more vehement, and quick. For, if ſalt "of tartar be mixed with gun-powder "and the mixture be warm'd till it takes "fire,

" fire, the explosion will be more violent, " and quick, than that of gun-powder a- " lone; which cannot proceed from any " other cause, than the action of the va- " pour of the gun-powder upon the salt " of tartar, whereby the salt is rarified. " The explosion of gun-powder arises, " therefore, from the violent action, " whereby all the mixture, being quickly " and vehemently heated, is rarified, and " converted into fume and vapour; which " vapour, by the violence of that action, " becoming so hot as to shine, appears " in the form of flame."

The following query is so beautiful, and opens such a glorious scene, that I should be tempted to add it, tho' it bore a less relation, than it does, to the present subject.

" Do not " continues the same great author, " large bodies conserve their heat " the longest; their parts heating one a- " nother? And, may not a great, dense, " and fixed body, when heated beyond " a certain degree, emit light so copiously, " as, by the emission, and re-action of its " light, and the reflexions, and refractions " of its rays, within its pores, to grow " still hotter, till it comes to a certain pe- " riod of heat; such as is that of the sun? " And, are not the sun, and fixed stars, " great earths, vehemently hot; whose " heat is conserved by the greatness of the " bodies, and the mutual action, and re- " action between them, and the light " which they emit; and whose parts are " kept from fuming away, not only by " their fixity, but, also, by the vast weight " and density of the atmospheres, incum- " bent upon them; and very strongly com- " pressing them, and condensing the va- " pours and exhalations which arise from " them? For, if water be made warm, in " any pellucid vessel emptied of air, that " water, in the vacuum, will bubble and " boil, as vehemently as it would in the " open air, in a vessel set upon the fire, " till it conceives a much greater heat. " For, the weight of the incumbent at- " mosphere, keeps down the vapours, and " hinders the water from boiling, till it " grow much hotter than is requisite to " make it boil *in vacuo*. Also, a mixture " of tin and lead, being put upon a red-hot " iron, *in vacuo*, emits a fume and flame; " but the same mixture, in the open air, " by reason of the incumbent atmosphere, " does not so much as emit any fume, " which can be perceiv'd by sight. In " like manner, the great weight of the " atmosphere, which lies upon the globe " of the sun, may hinder bodies there from " rising up, and going away from the sun, " in the form of vapours, and fumes, un- " less by means of a far greater heat than " that which, on the surface of our earth, " would very easily turn them into va- " pours and fumes. And the same great " weight may condense those vapours, and " exhalations, as soon as they shall, at " any time, begin to ascend from the sun, " and make them presently fall back a- " gain into him; and, by that action, in- " crease his heat, much after the same " manner that, on our earth, the air in- " creases the heat of a culinary fire. And " the same weight may hinder the globe " of the sun from being diminish'd; unless " by the emission of light, and a very " small quantity of vapours and exhala- " tions. *Newton. Optic.* p. 316.--319.

PNEUMATICS.

THE

PREFACE.

PNeumatics is that part of universal philosophy, which considers the properties, the use, and effects of the air; the fluid we breathe, and which is absolutely necessary to maintain the life of animals.

This is a subject, therefore, that cannot but be highly useful to inquire into: and herein Mr. Boyle *appears to have exercised himself more than any philosopher before his time, or since. Indeed, the ancients had no notion of this part of philosophy; and seem to have never dream'd of an engine wherewith to draw the air from vessels, and prevent its return into them.* Otto Gueric, *the famous consul of* Magdeburg, *first, unless we except* Friar Bacon, *hit upon an expedient to do it; and, from a hint of this it was, that Mr.* Boyle, *improving upon the design of the consul, originally contrived his air-pump: a machine, to which we are indebted for the solution of many phenomena in nature; and momentous discoveries in the fluid, which, of all external ones, seems to be the most familiar, and useful to us. A man of an ordinary capacity, possess'd of such an engine as this, would have been perplexed to know what use to make of it; but such a genius as Mr.* Boyle, *could not but apply it to noble and serviceable purposes. In vessels of glass, emptied of air, by its means, he soon included a great variety of proper subjects, one after another; of which he had, before-hand, wrote down a long catalogue. And, as this engine was, at first, imperfect, but afterwards received considerable improvements, at different times, I am induced to let all the experiments, made with it, by Mr.* Boyle, *stand in the order wherein he wrote them, rather than to range them under several heads. For, thus they give the history of the air-pump, and its improvements from time to time: but, if the other method had been taken, the advantage of these experiments, must have been lost in their historical capacity; which would prove a greater inconvenience, than the seeming disorder wherein they now occur: especially considering, that this may easily be remedied, by means of the index.*

Nume-

Numerous are the experiments made by Mr. Boyle, with this engine; but ſtill there may be ſomething left for future philoſophers to do with it, beſides repeating, varying, and confirming his trials. The air-pump, as 'tis now made, has many advantages over even the beſt which was employ'd by Mr. Boyle: it is ſo much more manageable, commodious, and exact, (eſpecially as improved by that excellent mechanic, Mr. Hauksbee) its apparatus is ſo well ſuited for all exigencies of experiments; and, the whole comes on ſuch eaſy terms, in compariſon of what it coſt our generous philoſopher, that, if it were only to be witneſs of the ſurprizing diſcoveries he made with it, 'tis pity any well-wiſher to philoſophy ſhould be without it.

But, were this inſtrument in more general uſe; and, would men of invention ſet themſelves to contrive new experiments for it, who knows what farther light it might afford us; and in producing how many other effects of nature, the air would appear to be concern'd? The late Mr. Hauksbee made ſeveral experiments with it, which, perhaps, Mr. Boyle himſelf never thought of; tho' he evidently contrived a large number; which, for want of a fit opportunity, convenient glaſſes, and other apparatus, he could never ſee the event of. The ſubject, therefore, appears, far from being exhauſted: the air-pump is a fruitful engine, and ſeems fitted to examine almoſt all the productions of nature: and, there are many things, in philoſophy, that cannot be done without it. 'Twas this which lately open'd a new field of knowledge, and greatly aſſiſted Mr. Boyle to introduce, or revive the mechanical philoſophy; this, evidently, help'd to overthrow the doctrine of nature's dread of a Vacuum, by which, the modiſh philoſophy of his time, falſely accounted for abundance of phenomena; to this is owing, the greateſt part of thoſe noble experiments, which appear under the following head of Pneumatics; and, laſtly, to this we owe ſome ſurprizing diſcoveries of Sir Iſaac Newton, and a great ſhare of the preſent ſyſtem of philoſophy.

Phyſico-

Phyſico-mechanical EXPERIMENTS,

To ſhew the

Spring and Effects of the AIR.

SECT. I.

THE air is ſo neceſſary to life, that moſt creatures, which breathe, cannot ſubſiſt, for many minutes, without it; and moſt of the natural bodies we deal with, being, as well as our own, almoſt perpetually contiguous to it, the alterations thereof have a manifeſt ſhare in many obvious effects, and, particularly, in diſtempers: wherefore, a farther inquiry into the nature of this fluid, will, probably, ſhew, that it concurs to exhibit abundance of phenomena, wherein it has, hitherto, ſeemed little concerned. So that, a true account of any new experiment, upon a thing whereof we have ſuch a conſtant and neceſſary uſe, may prove advantagious to human life. *The origin of the air-pump.*

With this view, before ever I was informed that *Otto Gueric*, the ingenious conſul of *Magdeburg*, had practiſed a way, in *Germany*, of emptying glaſs veſſels of the air, I had made experiments on the ſame foundation; but, as that gentleman firſt produced conſiderable effects by this means, I acknowledge the aſſiſtance and encouragement which the report of his performances afforded me.

But, as few inventions happen to be compleat at the firſt, ſo the engine employ'd by the conſul, ſeem'd very defective in its contrivance; whence but little more could be expected from it, than thoſe very few phenomena obſerved by the author, and related by *Schottus*. I, therefore, put Mr. *Hook*, upon contriving an air-pump, more manageable and convenient, that might not, like the *German*-engine, require to be kept under water: and, after ſome unſucceſsful attempts, he fitted me with one, conſiſting of two principal parts; a glaſs veſſel, and a pump to evacuate the air.

The

PNEUMATICS.

Fig. 30.

The air-pump described.

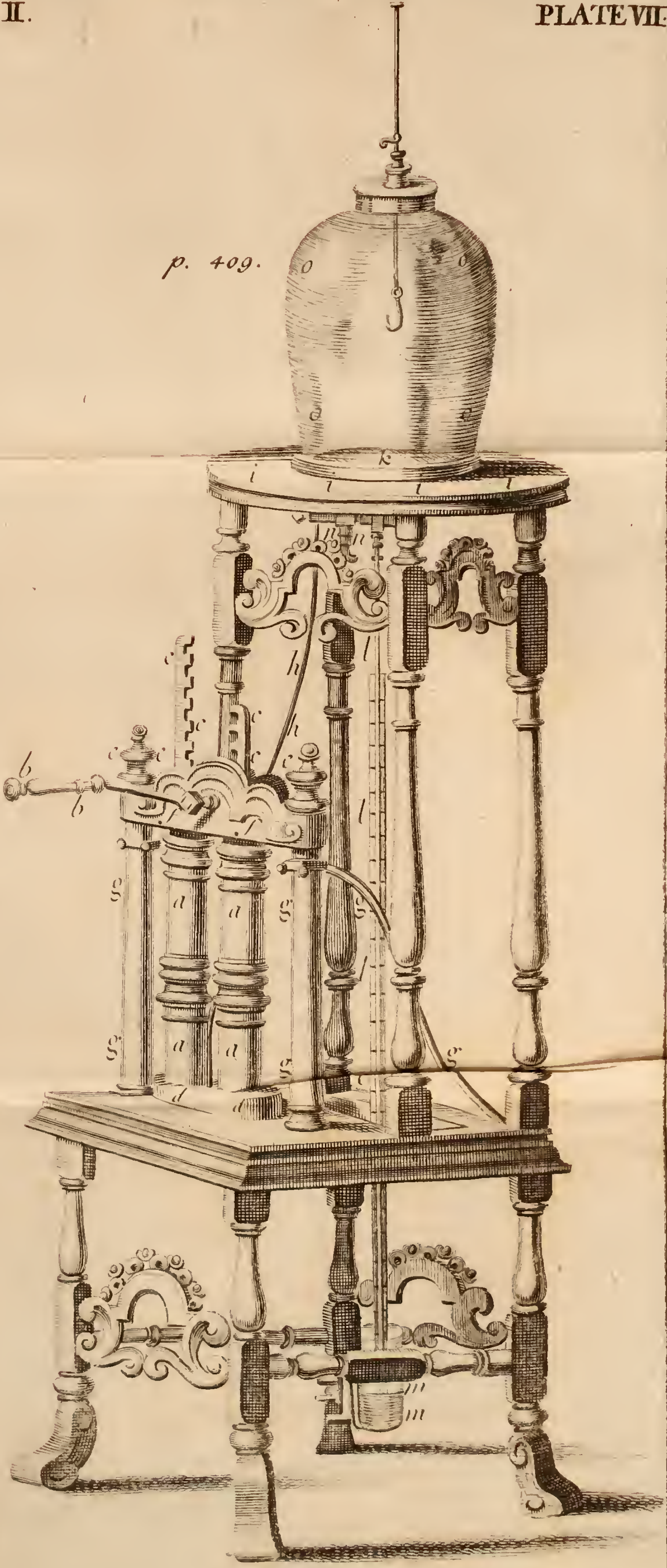
The first is a glass A, with a large mouth, a cover thereto, and a stop-cock fitted to the neck below. This would contain 30 quarts of water. B C, the mouth of it, is about four inches in diameter, and surrounded with a glass lip, almost an inch high, for the cover to rest on; wherein D E, is a brass ring, to cover, and be cemented on to the lip B C. To the internal orifice of this ring, a glass stopple is fitted, to keep out the external air. In the middle of this cover is a hole H I, half an inch in diameter, incircled with a ring, or socket; to which is adapted a brass stopple K, to be turn'd round, without admitting the least air. In the lower-end of this, is a hole 8, to admit a string, 8, 9, 10; which also passes thro' a small brass ring L, fixed to the bottom of the stopple F G, to move what is contain'd in the exhausted vessel, or receiver. That the stop-cock N, in the first figure, might perfectly exclude the air, we fasten'd a thin tin-plate, M T V W, to the shank of the cock X, all along the neck of the receiver, with a cement made of pitch, rosin, and wood-ashes, poured hot into the cavity of the plate; and to prevent the cement from running in at the orifice Z, of the shank X, it was stopt with a cork fix'd to a string, that it might be drawn out at the upper orifice of the receiver; and then the neck of the glass, being made warm, was pressed into the cement, which thus fill'd the interstices betwixt the tin-plate and the receiver, and betwixt the receiver and the shank of the cock.

The lower part of our engine consists of a sucking-pump, supported by a wooden frame, with three legs 111, so contrived, that, for the freer motion of the hand, one side of it may stand perpendicular; and a-cross the middle of the frame we nail'd a piece of board 222, to which the principal part of the pump is fixed. The pump consists of an exact strong concave cylinder of brass, fourteen inches long, its cavity three inches in diameter; to which a sucker, 4455, is adapted, made up of two parts; one of which 44, is less in diameter than the cavity of the cylinder, with a thick piece of tann'd leather nail'd on it, whereby it excludes the air. The other part, a thick iron plate 55, is firmly join'd to the middle of the former, and is a little longer than the cylinder; one edge of it being smooth, and the other indented, to receive the teeth of a small iron-nut $\alpha\beta\gamma$, fixed by two staples to the underside of the board nailed a-cross 22, on which the cylinder stands; and it is turn'd by the handle 7.

The last part of the pump is the valve R, a hole at the top of the cylinder, and taper towards the cavity; to this is fitted a brass-plug, to be taken out as occasion requires. The engine being thus contrived, some oil must be pour'd in at the top of the receiver upon the stop-cock, to fill up the interstices of its parts, and that the key S, may turn with the greater ease. A quantity of oil, also, must be left in the cylinder, to prevent the air from getting betwixt that and the sucker; for the like reasons, some must, likewise, be apply'd to the valve.

And here 'tis proper to observe, that when we used oil, or water, separately, for this purpose, and they have not answered the end, a mixture of

p. 409.



of the two has afterwards proved effectual. And, that the air may not enter betwixt the brass-cover and the ring, 'twill be convenient to lay some diachylon-plaister on their edges with a hot iron. That no air, also, may remain in the upper part of the cylinder, the handle is to be turn'd till the sucker rises to the top; and then, the valve being shut, it is to be drawn down to the bottom; by which means, the air being driven out of the cylinder, and a succession from without prevented, the cavity of the cylinder must be empty of air; so that, when the stop-cock is turn'd to afford a communication betwixt the receiver and the cylinder, part of the air before lodged in the receiver, will be drawn down into the cylinder; which, by turning back the key, is kept from entering the receiver again, and may, by unstopping the valve, and forcing up the sucker, be driven into the open air; and so, by repeated exsuctions out of the receiver, and expulsions out of the cylinder, the vessel may be exhausted as the experiment requires *. PNEUMATICS.

1. Upon

* The air-pump has received great improvements since the time of Mr. *Boyle*, and seems brought to its utmost degree of simplicity, and perfection, by the late, and the present Mr. *Hauksbee*. This instrument, as 'tis now made, by Mr. *Hauksbee*, consists of two brass-clylinders, *a a a a*, twelve inches high, and two their internal diameter. The emboli are raised, and depressed, by turning the winch *bb*, backward and forward. This winch is fasten'd to a spindle, passing thro' a lanthorn, whose pins serve for cogs, laying hold of the teeth of the racks *c c c c*; so that one is depressed, and the other elevated reciprocally. By this means the valves, made of limber bladder, and fix'd on the upper part of each embolus, and at the bottom of the cylinders, mutually exhaust and discharge the same air from the receiver: which becoming nearly empty, the pressure of the external air on the descending embolus is so great, that the power required to raise the other, need but little surmount the friction of the moving parts; whence this pump becomes preferable to all others. The bottoms of the barrels lie in a brass-dish *dd*, its sides two inches high, containing water to keep the leather-collars, on which the cylinders stand, moist; whereby the air is precluded. The cylinders are screw'd hereon by the nuts *e e e e*, which force the frontispiece *ff*, down upon them; thro' which pass the two pillars *g g g g*. Each pillar has an iron belonging to it, passing from them in the form of a swan's neck *g g*; these irons being fastened to the hind part of the frame, to prevent their shaking. Between the two barrels, rises a hollow brass-wire *h h h h*, communicating with each of them, by means of a perforated piece of brass, lying horizontally from one to the other. The upper end of this wire is fasten'd to another piece of perforated brass, screw'd on below the plate *i i i i*, which is ten inches over; having a brass-rim soldered on it, that it may contain water. Between the middle, and the side of this plate, rises a small pipe *k*, about an inch and half high; thro' which, into the hollow wire, passes all the air into the barrels from the receiver. Upon the plate of the pump is always laid a wet leather, for the receivers to stand on. This leather prevents the air's getting into the glasses, whose edges are ground true; and serves for this purpose vastly beyond any cement whatever. Another excellence in this pump, is the gage *l l l l*, a glass-tube about thirty-four inches long, so placed, that it cannot easily be damaged, or prove inconvenient. Its lower orifice is immersed in a glass of quick-silver *m m*; on the surface whereof is a perforated piece of cork for the tube to pass thro'. On this cork is placed a board of box-wood, about an inch in breadth, and grooved in the middle, to receive the tube, which is looped on thereto, that it may rise and fall as the

Fig. 31.

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Some phenomena of the engine solved.

1. Upon drawing down the sucker of our engine, whilst the valve is shut, the cylindrical space deserted by it will be left empty of air; and, therefore, upon turning the key, the air contain'd in the receiver rushes into the cylinder, till, in both vessels, it be brought to an equal dilatation; so that, upon shutting the receiver, turning back the key, opening the valve, and forcing up the sucker again, almost a whole cylinder of air will be driven out after this first exsuction; but, after every succeeding stroke, less air will come out of the receiver into the cylinder: so that, at length, the sucker will rise almost to the top of the cylinder, before the valve need be open'd. And if, when it is so exhausted, the handle of the pump be let go, and the valve be stopp'd, the sucker, by the force of the external air, which is an over-balance to the internal rarify'd air, will be forced to the upper part of the cylinder, and higher, in proportion, as the air is more exhausted *. We observed, also, that, whilst any considerable quantity of air remains in the receiver, a brisk noise is immediately produced, upon turning the key.

The spring and pressure of the air explain'd.

But to render our experiments the more intelligible, we must premise, that the air abounds in elastic particles, which being pressed together by their own weight, constantly endeavour to expand and free themselves from that force; as wool, for example, resists the hand that squeezes it, and contracts its dimensions; but recovers them as the hand opens, and endeavours at it, even whilst that is shut. It may be alledg'd, that tho' the air consists of elastic particles, yet this only accounts for the dilatation of it in pneumatical engines, wherein it hath been compress'd, and its spring violently bent; by an external force; upon the removal whereof, it expands, barely to recover its natural dimensions; whilst, in our experiments, the air appears not to have been compressed, before its spontaneous dilatation. But, we have many experiments to prove, that our atmosphere is a heavy body, and that the upper parts of it press upon the lower. And I found a dry lamb's bladder, containing two thirds of a pint, and compress'd by a pack-thread tied about it, to lose, in a very tender balance, 1 $\frac{1}{8}$ grain of

the mercury ascends or descends in the gage. To the upper part of this tube is cemented a brass-head, that fits into the perforated brass-piece, screw'd on under the plate, and communicating both with the receiver, and the hollow brass-wire *h h h h*. The box board is graduated into inches and quarters, from the surface of the quick-silver to twenty-eight inches high; and thence 'tis divided into tenths. By this means, the degrees of rarifaction may, at all times, be nicely observed in an experiment. The air-cock *n*, which lets in the air, is, likewise, a screw on the same perforated brass, in which the upper parts of the gage, and the hollow wire, are inserted. *o o o o* represents a receiver, standing on the plate of the pump; on whose upper part *p p*, thro' a box of leather-collars, passes a slip of wire, to take up, let fall, or suspend any thing in the receiver, without admitting the air.

* The original air in the receiver, is always to the remainder, as the sum of the capacity of the vessel of the pump, raised to the power, whose exponent is equal to the number of the strokes of the sucker, to the capacity of the vessel raised to the same power. See this demonstrated by M. *Varignon*. *Memoir. de l'Academ.* A. 1705. p. 397.

its

its former weight, by the recefs of the air, upon pricking it. Suppofing, therefore, that the air is not deftitute of weight, 'tis eafy to conceive, that the part of the atmofphere wherein we live, is greatly comprefs'd by thofe directly over it, to the top of the atmofphere. And tho' the height of this atmofphere, according to *Kepler*, fcarce exceeds eight miles, yet later aftronomers extend it fix or feven miles farther. The learned *Ricciolo* makes it reach fifty miles high. So that a column of air, feveral miles in height, preffing upon fome elaftic particles of the fame fluid here below, may eafily bend their little fprings, and keep them bent; as if fleeces of wool, were piled to a vaft height upon one another, the hairs of the loweft locks would, by the weight of all the incumbent parts, be ftrongly comprefs'd. Hence it is, that, upon taking off the preffure of the incumbent air, from any parcel of the lower atmofphere, the particles of the latter poffefs more fpace than before. If it be farther objected againft this condenfation of the inferior air, that we find this fluid readily yields to the motion of flies, feathers, &c. we may reply, that as when a man fqueezes wool in his hand, he feels it make a continual refiftance; fo each parcel of the air, about the earth, conftantly endeavours to thruft away fuch contiguous bodies as keep it bent, and hinder the expanfion of its parts; which will fly out towards that part, where they find the leaft refiftance. And, fince the corpufcles whereof the air confifts, tho' of a fpringy nature, are fo very fmall, as to compofe a fluid body, 'tis eafy to conceive, that here, as in other fluids, the component parts are in perpetual motion, whereby they become apt to yield to, or be difplaced by other bodies; and that the fame corpufcles are fo varioufly mov'd, that, if fome attempt to force a body one way, others, whofe motion hath an oppofite determination, as ftrongly prefs it the contrary way; whence it moves not out of its place; the preffure, on all fides, being equal. For if, by the help of our engine, the air be drawn only from one fide of a body, he, who thinks to move that body, as eafily as before, will, upon trial, find himfelf miftaken.

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2. Thus, when our receiver is tolerably exhaufted, the brafs ftopple in the cover, is fo difficult to lift, that there feems to be fome great weight faften'd to the bottom of it: for, the internal air being, now, very much dilated, its fpring muft be greatly weakned; and, confequently, it can but faintly prefs againft the lower-end of the ftopple, whilft the fpring of the external air keeps it down, with its full natural force. And, as the air is gradually admitted into the receiver, the weight is manifeftly felt to decreafe; till, at length, the receiver being again filled with air, the ftopple may be eafily lifted.

It may feem furprizing, that we fpeak of the air fhut up in our receiver, as of the preffure of the atmofphere; tho' the glafs manifeftly keeps the incumbent pillar of air from preffing upon that within the veffel. But, let us confider, that if a fleece of wool, by preffure, be thus directly reduced into a narrow compafs, and convey'd into a clofe box, tho' the former force ceafes to bend its numerous fpringy parts, yet they continue

PNEUMATICS.

as ſtrongly bent as before; becauſe we ſuppoſe the including box reſiſts their expanſion, as much as the force that crowded them in. Thus the air, being ſhut up in our glaſs when its parts are bent by the whole weight of the incumbent atmoſphere, though that weight can no longer preſs upon it; yet the corpuſcles of the internal air, continue as forcibly bent, as before they were included. If it be ſaid, that the continual endeavour it has to expand itſelf, ought then to break the glaſs, we muſt obſerve, that the expanſive force of the internal air, is balanc'd by preſſure of the external, which preſerves the glaſs intire; as, by the ſame means, thin large bubbles, made with ſoapy water, will, for ſome time, continue whole in the open air.

3. And though, by help of the handle, which is a lever, the ſucker may eaſily be drawn down to the bottom of the cylinder; yet, without ſuch a mechanic power, the ſame effect could not be produced, but by a force able to ſurmount the preſſure of the atmoſphere: as in the *Torricellian* experiment, if the column of mercury be too high, it will ſubſide, till its weight be a balance to the preſſure of the air. Hence we need not wonder, that tho' the ſucker move eaſily in the cylinder, by means of the handle, yet, if that be taken off, it will require a conſiderable force to raiſe or depreſs it. Nor will it ſeem ſtrange, that if, when the valve, and ſtop-cock are exactly cloſed, the ſucker be drawn down, and then the handle let looſe, that the ſucker, as of itſelf, re-aſcends to the top of the cylinder; ſince the ſpring of the external air, finds nothing to reſiſt its preſſure upon the bottom of the ſucker. And, for the ſame reaſon, when the receiver is almoſt emptied, tho', the ſucker being drawn down, the paſſage from the receiver to the cylinder be open'd, and then ſtop'd again, the ſucker will, upon the letting go the handle, be forcibly carried up, almoſt to the top of the cylinder; becauſe the air within the cylinder, being equally dilated and weakned with that of the glaſs, is unable to reſiſt the preſſure of the external air, till it be crowded into ſo little ſpace, that both their forces are in equilibrium. So that, in this caſe, the ſucker is drawn down with little leſs difficulty, than if, the cylinder being deſtitute of air, the ſtop-cock were exactly ſhut. It muſt alſo be obſerv'd, that when the ſucker hath been impell'd to the top of the cylinder, and the valve is ſo carefully ſtop'd, that no air remains in the cylinder, above the ſucker; if, then, the ſucker be drawn to the lower part of the cylinder, no greater difficulty is found to depreſs the ſucker, when nearer the bottom of the cylinder, than when it is much farther from it. Whence it appears, that the preſſure of the external air, is not increas'd upon the acceſſion of the air driven out; which, to make itſelf room, forceth the contiguous air to a violent ſub-ingreſſion of its parts, as ſome ſuppoſe; for otherwiſe the ſucker would be more reſiſted by the external air as it comes lower; more of the diſplaced air being thruſt into it, to compreſs it.

Bladders dilated by the ſpring of the air.

4. We took a large lamb's bladder, well dry'd, and very limber, and leaving in it about half the air it would contain, we ſtrongly tied the neck of

of it; then conveying it into the receiver, the pump was work'd; and after two or three ſtrokes, the impriſon'd air began to ſwell in the bladder, and continued to do ſo, as the receiver was farther exhauſted, till, at length, the bladder appear'd perfectly turgid. Then, by degrees, allowing the external air to return into the receiver, the diſtended bladder ſhrunk proportionably, grew flaccid, and, at laſt, appear'd as full of wrinkles as before. PNEUMATICS.

And to try whether the actual elaſticity of the fibres of the bladder, had any ſhare in this effect, we let down to the former, two ſmaller bladders, of the ſame kind; the one not tied up at the neck, that the air it contain'd might paſs into the receiver; the other, with its ſides ſtretch'd out, and preſs'd together, that it might hold the leſs air, and then ſtrongly tied up at the neck; and, whilſt the firſt, upon working the pump, appear'd, every way diſtended to its full dimenſions, neither of the others were remarkably ſwell'd; and that whoſe neck was left looſe, ſeem'd very little leſs wrinkled than when firſt put in.

We made, likewiſe, a ſtrong ligature about the middle of a long bladder, emptied of its air in part, but left open at the neck; and, upon exhauſting the receiver, obſerv'd no ſuch ſwelling betwixt the ligature, and the neck, as betwixt the ligature and the bottom of the bladder, where air was included.

5. We hung a dry bladder, well tied, and blown moderately full, in the receiver, by a ſtring faſten'd to the inſide of the cover; and, upon exhauſting the glaſs, the included air firſt diſtended the bladder, and then burſt it, as if it had been forcibly torn aſunder. *And burſt by the ſame.*

This experiment was repeated with the like ſucceſs; and the bladder burſting, long before the receiver was fully exhauſted, gave a great report.

But it was often, in vain, that we try'd to burſt bladders, after this manner, becauſe they were commonly grown dry, before they came to our hands; whence, if we tied them very hard, they were apt to fret, and ſo become unſerviceable; and, if tied but moderately hard, their ſtiffneſs kept them from being cloſed ſo exactly, that the air ſhould not get out into the receiver. We found, alſo, that a bladder moderately filled with air, and ſtrongly tied, being held for a while, near the fire, grew exceeding turgid; and, afterwards, being brought nearer to the fire, ſuddenly burſt, with ſo loud and vehement a noiſe, as made us almoſt deaf for ſome time after *.

6. Having

* M. *Amontons* ſhews, that the ſame degree of heat, how ſmall ſoever, may perpetually increaſe the force of the air's ſpring, provided that air be continually preſs'd by a weight ſtill greater and greater; and that any parcel of air, how ſmall ſoever, may perpetually increaſe the force of its ſpring, by a ſmall degree of heat; provided this air be more and more preſſed continually. The ſame gentleman, alſo, found by experience, that the heat of boiling water, which he ſhews to be the great-

PNEUMATICS.

6. Having thus found, that the air hath an elastic power, we were desirous to know how far a parcel of that fluid might be dilated by its own spring.

The dilatation of air by its spring, measured.

We thoroughly wetted a limber lamb's bladder, in water, that the sides of it being squeezed together, no air might be left in its folds, and strongly tied the neck of it about that of a small glass, capable of holding five drams of water; the bladder being first so squeez'd, that the air it contain'd was wholly forced into the glass, without being compress'd there; then the pump being set on work, the air, in the vial, soon began to dilate, produc'd a small tumor in the neck, and gradually came further into the bladder; elevating the sides, and displayng the folds, till, at length, it seem'd blown up to its full extent; when the external air, being permitted to return into the receiver, the air that had fill'd the bladder, was thereby reduced into its former narrow receptacle, and the bladder became flaccid and wrinkled, as before. Then taking out the bladder, and glass, we fill'd them both with water, thro' a hole made in the top of the bladder; and found the weight of it to be five ounces, five drams, and a half. So that the air, at its utmost expansion, possess'd above nine times the space it did when first put into the receiver.

But to measure the expansive force of the air more accurately, we took a cylindrical pipe of glass, its bore about a quarter of an inch in diameter, its length about seven inches, and left it open at one end; but the other, where it was hermetically sealed, had a small glass bubble, to receive the air, whose dilatation was to be measur'd. Along the side of this tube we pasted a slip of parchment, divided into twenty-six equal parts, marked with black lines, to measure both the included air, and its expansion. Afterwards we almost fill'd the tube with water; when, stopping the open end, and inverting it, the air was permitted to ascend to the bubble; and, as the ascent was very slow, it gave us the opportunity to mark how much more, or less than one of those divisions, this air took up. Thus, after a trial, or two, we convey'd to the top of the glass, a bubble of air, apparently equal to one of those divisions; then the open end of the tube being put into a small vial, whose bottom was cover'd with water, we included both glasses in a small slender receiver, and caused the pump to be work'd. The event was, that, at the first exsuction of the air, there seem'd not any expansion of the bubble, comparable to what appear'd at the second; and, after a very few strokes, the bubble, reaching as low as the surface of the subjacent water, gave us cause to think, that it would have expanded much farther, had there been room. We, therefore, took out the little tube, and found that, besides the twenty-six divisions, the glass bubble, and some part of the pipe, to which the parch-

greatest that liquor is capable of, tho' ever so long detain'd upon a vehement fire, increases the spring of the air as much as about $\frac{1}{3}$ of the weight of the atmosphere, shewn by the barometer, in spring, or autumn: and upon this foundation, he ingeniously attempts to establish an uniformity in thermometers. See *Memoirs de l'Academ.* A. 1702. p. 204. A. 1703. p. 61, &c.

ment

ment did not reach, amounted to six divisions more. Whence it appears, that the air possess'd one and thirty times more space than before; and yet seem'd capable of a far greater expansion. Wherefore, after the same manner, we let in another bubble, that seem'd but half as big as the former, and found that, upon exhausting the receiver, it did not only fill up the whole tube, but, in part, broke thro' the water in the vial; and thereby manifested itself to have possess'd above sixty times its former space. Pneumatics.

Finding, then, that our tube was still too short, we took a slender conical one, thirty inches long, hermetically seal'd at the slender end, and almost fill'd it with water; and conveying a bubble of air to the top of it, we put the open end in a vial, as before: then the cover, by means of a small hole made in it, for the glass-pipe to come out at, was cemented to the receiver; and the pump being set on work, the air manifestly appear'd extended below the surface of the water; and some bubbles were seen to come out at the bottom of the pipe, and break thro' the water. This done, we left off pumping, and observ'd, that at unperceiv'd leaks of the receiver, the air got in so fast, that it very quickly impell'd up the water to the top of the tube; excepting a little space, whereinto that bubble was driven, which had before possess'd the whole tube. This air, at the slender end, appear'd to be a cylinder of $\frac{1}{7}$ inch in length; but when the pipe was taken out, and inverted, it seem'd, at the other end, less in bulk than a pea. Then, with a small pair of scales, weighing the tube and water, we found they amounted to one ounce thirty grains and a half; and filling the tube with water, and weighing again the pipe and water, we found the weight increas'd only by one grain. Lastly, pouring out the water, and carefully freeing the pipe from it, we weigh'd the glass alone, and found it wanted two drams and thirty-two grains of its former weight. So that the bubble of air possessing the space but of one grain weight of water, it appear'd that this air, by its own spring, was rarified to one hundred fifty-two times its former dimensions; tho' it had been compress'd only by the ordinary weight of the contiguous air. The experiment, indeed, was made in a moist night, and in a room with a large fire; which did, perhaps, somewhat rarify the bubble of air.

It hath seem'd almost incredible, what *Mersennus* relates, that the air, by the violence of heat, may be dilated so as to take up seventy times its natural space: we, therefore, once more, convey'd into the tube a bubble of the same bigness with the former; and prosecuting the experiment as before, we observ'd, that the air did manifestly stretch itself so, as to appear, several times, far below the surface of the water in the vial; and that, too, with a surface very convex toward the bottom of the pipe. Nay, the pump being ply'd a little longer, the air reach'd to that place, where the tube rested upon the bottom of the vial, and seem'd to hit against and rebound from it. Whence 'tis probable, if the experiment could be so made that the expansion of the air might not be resisted, it would yet enlarge its bounds, and perhaps stretch itself to more than two hundred times

Pneumatics. times its former bulk. And this may render many phenomena of our engine credible; since, of that part of the atmosphere wherein we live, what we call the free air, and presume to be uncompress'd, is crowded into so very small a portion of the space, it would, if unresisted, possess.

The strength of glass, and the advantages of figure in sustaining a pressure.

7. To discover the strength of glass, and what interest the figure of a body may have in resisting a pressure, we made the following experiments.

A round glass bubble, capable of containing five ounces of water, being purposely blown very thin, and with a slender neck, we moderately emptied the receiver, and nimbly applied the neck of the bubble to the orifice of the bottom of it; and after turning the key of the stop-cock, we made a free passage for the air to come out of the bubble into the receiver; which it did with great celerity; leaving the bubble as empty as the receiver itself. We then let in the external air, which now press'd only on the outside of the exhausted bubble, being prevented from getting within it; nevertheless, it continued as intire as before; the roundness of its figure enabling it, tho' almost as thin as paper, to resist a pressure equal to that of the whole incumbent atmosphere. And repeating the experiment, we found again, that the pressure of the air, thrusting all the parts inwards, made them, by reason of their arched figure, so support one another, that the glass would not break.

Fig. 32. 8. We took a glass alembic, containing between two and three pints; the rostrum C, being hermetically seal'd; and at the top of it was a hole, wherein we cemented one of the shanks of a stop-cock; so that the glass being inverted, the wide orifice stood uppermost; and to this was cemented a cover of lead: the other shank of the stop-cock was also, with cement, fasten'd into the upper part of the pump, which beginning to be work'd, the remaining air became by much too weak to balance the pressure of the external air, when the glass was, with a great noise, crack'd almost half round, along that part of it where it began to bend inwards; as in the line A B; and upon attempting to evacuate more of the air, the crack appear'd to run further, tho' the glass, where it was broken, seem'd above twenty times as thick as the bubble employ'd in the preceding experiment. Hence it may seem strange, that taking another glass bubble, alike in all respects, for ought appear'd, to that just mention'd, sealing it up hermetically, and suspending it in the receiver, the exsuction of the surrounding air did not enable the internal air to break or crack it: and this prov'd the case, tho' the experiment were tried several times, with bubbles of different sizes. But, perhaps, the heat of the lamp, wherewith such glasses are hermetically seal'd, might rarify the contain'd air, and weaken its spring.

Fig. 33. 9. Into the neck of a common four-ounce vial, we put a slender pipe of glass, and carefully fasten'd it, with a mixture of pitch and rosin, to the neck thereof. This vial, containing water that reach'd considerably higher than the lower end of the pipe, was put into a small receiver, in such manner, that the glass pipe, passing thro' a hole in the leaden cover of the receiver, was

was principally without the veſſel; which being exactly cloſed, we work'd the pump: but at the very firſt ſtroke, and before the ſucker was drawn to the bottom of the cylinder, there flew out of the vial, a large piece of glaſs, with a ſurprizing violence and noiſe, ſo as to crack the receiver in many places. PNEUMATICS.

For farther ſatisfaction, we repeated the experiment in a round glaſs, that would contain ſix ounces of water; which we put into a ſmall receiver, ſo that the bottom of it reſted upon the lower part of the receiver, and the neck came out thro' the leaden cover of the ſame. This vial we included in a bladder, before it was put in, and the receiver being clos'd, ſo that the outward air could not enter but by breaking thro' the vial, into whoſe cavity it had free acceſs by the mouth, the ſucker was nimbly drawn down; upon which, the external air immediately preſs'd forcibly, as well upon the leaden cover as the vial; and the cover happening to be in one place a little narrower, than the edge of the receiver, it was depreſs'd, and thruſt into it ſo violently, that getting a little within the lip of the glaſs, it thruſt out the ſide, where it was depreſs'd, ſo as to ſplit the receiver. And having fitted a wider cover to the ſame receiver, and clos'd both that, and the crack with cement, we proſecuted the experiment in the former manner with this ſucceſs; that, upon ſuddenly depreſſing the ſucker, the external air burſt the vial into above a hundred pieces, many of them exceeding ſmall, and with ſuch violence, that we found a wide rent, and many holes made in the bladder.

And to ſhew, that theſe phenomena were the effects of a limited force, and not of ſuch an abhorrence of a vacuum, as muſt, upon occaſion, exerciſe a boundleſs power, we try'd ſeveral thicker glaſſes, and found that the experiment would not ſucceed; for the glaſſes were taken out, as entire as they were put in.

And here, by the way, we may obſerve, that every ſmall crack will not render a roundiſh receiver uſeleſs in our experiments, becauſe, upon evacuation of the internal air, the external on all ſides preſſing the glaſs towards the center, thruſts the edges of the crack cloſer together.

And, in caſe of conſiderable flaws, we ſucceſsfully apply a plaiſter, made of quick-lime, finely powder'd, and nimbly ground, with a proper quantity of the ſcrapings of cheeſe, and fair water, enough to bring the mixture to a ſoft paſte; which, when the ingredients are exquiſitely incorporated, will have a ſtrong, and fetid ſcent; and then, it muſt be immediately ſpread upon a linen cloth, and applied, leſt it begin to harden.

10. We let down, into our receiver, a tallow-candle of a moderate ſize, and ſuſpending it, ſo that the flame appeared in the middle of the veſſel, we preſently clos'd it up, and upon pumping found, that within little more, than half a minute after, the flame went out. *The flame of tallow and of wax in vacuo.*

At another time, the flame laſted about two minutes, tho' upon the firſt exſuction it ſeem'd to contract itſelf in all its dimenſions, and after two or three exſuctions, it appear'd exceeding blue, and gradually

PNEUMATICS. receded from the tallow, till at length it ſeem'd to poſſeſs only the very top of the wiek, and there it vaniſh'd.

The ſame candle, being lighted again, was ſhut into the receiver, to try how it would burn there, without exhauſting the air; and we found that it laſted much longer than formerly; and before it went out, it receded from the tallow, towards the top of the wiek, tho' not near ſo much, as in the former experiment.

We took notice, that when the air was not drawn out, a conſiderable part of the wiek remain'd kindled upon the extinction of the flame, which emitted a ſmoke, that ſwiftly aſcended directly upwards, in a ſlender and uninterrupted cylinder, till it came to the top, from whence it return'd, by the ſides, to the lower part of the veſſel; but when the flame went out, upon the exſuction of the air, we once perceiv'd it not to be follow'd by any ſmoke at all. And at another time, the upper part of the wiek, remaining kindled after the extinction of the flame, a ſlender ſteam aſcended, but a very little way, and after ſome uncertain motions, for the greateſt part, ſoon fell downwards.

Joining together ſix ſlender tapers of white wax, as one candle, and having lighted all the wieks, we let them down into the receiver, and made what haſt we could to cloſe it up with cement. But, tho' in the mean while, we left open the valve of the cylinder, the hole of the ſtop-cock, and that in the cover of the receiver, that ſome air might get in to cheriſh the flame, and that the ſmoke might have a vent; yet the air ſufficed not for ſo great a flame, till the cover could be perfectly luted on; ſo that before we were ready to employ the pump, the flame was extinguiſh'd. Wherefore, we took but one of the tapers, and having lighted it, cloſ'd it up in the receiver, to try how long a ſmall flame, with a proportionable ſmoke, would continue in ſuch a quantity of air; but we found, upon two ſeveral trials, that from our beginning to pump, the flame went out in about a minute. It appear'd, indeed, that the ſwinging of the wire, whereby, the candles hung, haſtned the extinction of the flame, which ſeem'd, by the motion of the pump, to be thrown, ſometimes on one ſide of the wiek, and ſometimes on the other. But, once refraining to pump, after a very few exſuctions, the flame laſted not much longer. And laſtly, cloſing up the ſame lighted taper, to diſcover how long it would laſt, without drawing out the air; we found, that it burnt vividly for a while; but afterwards, began to diminiſh gradually in all its dimenſions, tho' the flam edid not, as before, retire itſelf by little and little towards the top, but towards the bottom of the wiek, ſo that the upper part of it, manifeſtly appear'd for ſome time, above the top of the flame; which, having laſted about five minutes, was ſucceeded by a ſtream of ſmoke, that aſcended in a ſtrait line.

Kindled char-coal. Fig. 34. 11. A ſpiral wire, fill'd to the height of about five inches, with wood-coals throughly kindled, being let down into the receiver, and the pump ſet to work; we obſerv'd, that upon the very firſt exſuction of the coals, the fire grew dim, and tho' the agitation of the veſſel made them ſwing; yet, when we could no longer diſcern a redneſs in any of them,

we

we found that, from the beginning of the pumping, that is, about two minutes after the coals had been put in, glowing, to the total disappearing of the fire, there had pass'd three minutes. *Pneumatics.*

We then, presently, took them out, and found there had remain'd some little parcels of fire, rather cover'd, than totally extinguish'd; for, in the open air, the coals began to re-kindle, in several places. Wherefore, having, by swinging them about in the wire, throughly lighted them a second time, we let them down again into the receiver; and closing it, waited till the fire seem'd totally extinct, without working the pump, and found that from the time the vessel was closed, till no fire at all could be perceiv'd, there had elapsed four minutes.

Lastly, having taken out the wire, and put other coals into it, we, in the same room where the engine stood, let it hang quietly by a string, in the open air; and found that the fire began to go out first at the top, and outsides of the coals; but inwards, and near the bottom, it continu'd visible for above half an hour; a great part of the coals, especially the lowermost, being reduced to ashes before the fire was extinguish'd.

A piece of iron, of the bigness of a middle-sized charcoal, being, also, made red-hot throughout, we suspended it in the exhausted receiver; but could not observe any manifest change upon the exsuction of the air. The iron, indeed, began to lose its fiery redness at the top; but that seem'd owing to the upper-end's being somewhat more slender, than the lower; and the redness, tho' it were in the day-time, continued visible about four minutes; and then before it quite disappear'd, we let in the air, but no change ensued. Yet some little remainders of wax, that stuck to the wire, and were turn'd into fumes by the heat of the iron, afforded a more diffusive smoke when the air was drawn out, than afterwards; tho' allowance were made for the decreas'd heat of the metal. And lastly, notwithstanding a considerable extraction of the air, and the inconsiderable dissipation of the parts of the iron, the sides of the receiver were very sensibly hot, and retain'd a warmth for some time after the iron was taken out, *Red-hot iron.*

12. We suspended a piece of well-lighted match, in our receiver, with the lighted end downwards, when the fumes of it, almost, immediately fill'd, and darken'd the receiver. Wherefore, lest the vessel should be endanger'd, the pump was nimbly ply'd, and a great deal of air and smoke, mix'd together, drawn out; whereby the receiver growing more clear, we could discern the fire in the match, to burn, by degrees, more languidly; and, after no long time, it ceas'd to be discernible either by its light, or smoke. And tho' we continued pumping for a while longer, yet, upon admission of the external air, the fire, that seem'd to have been long extinguish'd, presently reviv'd, and began again to shine, and dissipate the adjacent fewel into smoke, as before. *Lighted match.*

13. We, afterwards, let down into the receiver, together with a piece of lighted match, a large bladder, well tied at the neck, and containing only about a pint of air, tho' capable of containing ten times as much.

 This

PNEUMATICS. This was design'd to try, whether the ſmoke of the match, repleniſhing the receiver, would hinder the dilatation of the internal air, upon the extraction of the external; and to diſcover whether the extinction of the fire in the match, proceeded from want of air, or, barely, from the preſſure of its own fumes.

The event was, that, at the beginning of our pumping, the match appeared well lighted, tho' it had almoſt fill'd the receiver with ſmoke; but, by degrees, it burnt more dimly; tho', by nimbly drawing out the air, and ſmoke, the veſſel became leſs opake: ſo that the longer we pump'd, the leſs air, and ſmoke, came out of the cylinder, upon opening the valve; yet the fire in the match, went out but ſlowly. And when, afterwards, we had darken'd the room, and, in vain, attempted to diſcover any ſpark of fire, we ſtill continued pumping; and, at laſt, letting in the air, the fire quickly revived, yielded light, and plenty of ſmoke. Then we fell to pumping a-freſh, and continued it till long after the match went out again; ſo that in leſs than half a quarter of an hour, the fire was extinguiſhed, beyond the poſſibility of a recovery by re-admitting the air. If the cylinder were emptied, when the receiver was full of ſmoke, immediately upon turning of the ſtop-cock, the receiver would appear manifeſtly darkned, to an eye viewing the light thro' it; and this darkneſs was leſs, as the receiver contain'd leſs ſmoke: it was alſo inſtantaneous, and ſeem'd to proceed from a ſudden change of place and ſituation, in the exhalations, upon the vent afforded them, and the air they were mix'd with, out of the receiver into the cylinder. We alſo obſerv'd a kind of a halo, for a conſiderable time, about the fire, that ſeem'd to be produced by the ſurrounding exhalations. And, when the fumes ſeem'd moſt to repleniſh the receiver, they did not, ſenſibly, hinder the air, included in the bladder, from dilating itſelf, after the ſame manner it would otherwiſe have done: ſo that, before the the match was quite extinct, the bladder appear'd diſtended to ſix or ſeven times its former dimenſions.

We, alſo, took a ſmall receiver, capable of containing about a pound and a half of water, and, in the midſt of it, ſuſpended a lighted match; but tho' within a minute, from putting in the match, we had cemented on the cover, yet, before we began to pump, the ſmoke had ſo fill'd the receiver, as, apparently, to choke the fire. And finding it thus impoſſible to cloſe up the veſſel, and pump out the fumes ſoon enough to prevent the extinction of the fire, we uſed this expedient: as ſoon as we had pump'd once or twice, we ſuddenly turn'd the key, and thereby gave acceſs to the excluded air, which ruſhing violently in, drove away the aſhes, fill'd the glaſs with freſh air, and re-kindled the fire; and having, by this means, obtain'd a lighted match in the receiver, without ſpending time, to cloſe it up, we exhauſted the receiver, and found the match then quickly ceas'd to ſmoke.

And gun-powder fired in vacuo.

14. We took a piſtol, and having firmly ty'd it to a ſtick, almoſt as long as the cavity of the receiver, we primed it with dry gun-powder; then cocking it, we faſten'd the trigger to one end of a ſtring, whoſe other end

was

was faſten'd to the key in the cover of our receiver. This done, we convey'd the whole apparatus into the veſſel, which being cloſed up, and emptied after the uſual manner, we turn'd the key in the cover, and thereby ſhortning the ſtring, pull'd the trigger, and obſerv'd, that the force of the ſpring of the lock, was not ſenſibly abated by the abſence of the air; for, the cock falling with its uſual violence, ſtruck as many, and as conſpicuous ſparks of fire, as, for ought we could perceive, it did in the open air. Upon often repeating this experiment, we could not perceive, but that the ſparks of fire moved upwards, downwards, and ſide-ways, as when out of the receiver.

Pneumatics.

We, likewiſe, ſubſtituted a piece of ſteel for the flint, when, the piſtol being cock'd, and convey'd into the receiver, we pull'd the trigger, after the air was drawn out; and tho' the place were purpoſely darkned, there appear'd not, upon the colliſion of the two ſteels, the leaſt ſpark of fire. We have, indeed, found, that, by the dextrous colliſion of two harden'd pieces of ſteel, many ſparks may be ſtruck out; but that was done with ſuch a vehement percuſſion of their edges, as could not well be procured in our receiver.

But moſt of our attempts, to fire the gun-powder in the pan of the piſtol, fail'd, becauſe we were obliged to let it hang, almoſt perpendicularly, in the receiver; whereby the powder was ſhook out, before the ſparks could reach it. Once, however, the experiment ſucceeded; and the kindled powder ſeem'd to make a more expanded flame, than it would have done in the open air, and mounted upwards: upon the extinction of the flame, the receiver appear'd darkned with ſmoke, which ſeem'd to move freely up and down, and, upon letting in the air, began to circulate much faſter than before.

15. We convey'd into a ſmall receiver, a piece of combuſtible, dry, black matter; and carefully cloſing the veſſel, we brought it to a window, at which the ſun ſhone in very freely; then, drawing out the air, we, with a burning-glaſs, threw the ſun's rays upon the combuſtible matter, which began immediately to ſend out a ſmoke that darkned the receiver; but, notwithſtanding all our care, the external air got in, and fruſtrated the experiment.

An attempt to kindle a combuſtible body, by the ſun's rays in vacuo.

We, therefore, lodg'd this combuſtible matter in the cavity of our largeſt receiver, ſo that it was almoſt contiguous to the ſide next the ſun: we then endeavour'd to kindle it, but found, that by reaſon of the thickneſs of the glaſs, the ſun-beams, thrown in by the burning-glaſs, were, in their paſſage, ſo diſlocated, and ſcatter'd, that we could not, poſſibly, unite enow of them, to make the matter yield a ſenſi bſmoke.

16. We convey'd into the receiver, a little pedeſtal of wood, in the midſt of which was, perpendicularly erected, a ſlender iron, upon the ſharp point whereof, an excited needle of ſteel, of about five inches long, was ſo placed, that, hanging in equilibrium, it could move freely every way. Then the air being pump'd out, we employ'd a load-ſtone, moderately vigorous, to the outſide of the glaſs, and found that it attracted, or repell'd

An excited needle in vacuo affected by the magnet.

PNEUMATICS. repell'd the ends of the needle, without any remarkable difference from what the ſame load-ſtone would have done, had none of the air been drawn away from about the needle; which, when the load-ſtone was remov'd, reſted, after ſome tremulous vibrations, in a poſition north and ſouth.

The Torricellian *experiment in* vacuo.

17. A ſlender, and very exact cylinder of glaſs, near three feet in length; its bore, a quarter of an inch in diameter; being hermetically ſealed, at one end, was, at the other, filled with quick-ſilver; care being taken, that as few bubbles as poſſible, ſhould be left in the mercury. Then the tube, being ſtop'd with the finger, and inverted, was open'd into a long, ſlender, cylindrical box, half fill'd with quick-ſilver; when that in the tube ſubſiding, and a piece of paper being paſted level to its upper ſurface, the box and tube were, by ſtrings, carefully let down into the receiver; and the cover, by means of this hole, ſlipt along as much of the tube, as reach'd above the top of the receiver: the interval left betwixt the ſides of the hole, and thoſe of the tube, being exquiſitely fill'd up with melted diachylon; and the round chink, betwixt the cover and the receiver, likewiſe, very carefully clos'd; upon which cloſure, there appear'd no change in the height of the mercurial cylinder: whence the air ſeems to bear upon the mercury, rather by virtue of its ſpring, than of its weight; ſince its weight could not be ſuppos'd to amount to above two or three ounces; which is inconſiderable, in compariſon of ſuch a cylinder of mercury as it would ſuſtain. Now the ſucker was drawn down, and immediately, upon the evacuation of a cylinder of air, out of the receiver, the quick-ſilver in the tube ſubſided; and notice being carefully taken of the place where it ſtop'd, we work'd the pump again, and mark'd how low the quick-ſilver fell at the ſecond exſuction: but, continuing thus, we were ſoon hinder'd from accurately marking the ſtages in its deſcent, becauſe it preſently ſunk below the top of the receiver: ſo that we could, from hence, only mark it by the eye. And continuing pumping, for about a quarter of an hour, we could not bring the quick-ſilver, in the tube, totally to ſubſide. Then we let in ſome air; upon which, the mercury began to re-aſcend in the tube, and continued mounting, till having return'd the key, it immediately reſted at the height it had then attain'd. And ſo, by turning, and returning the key, we did, ſeveral times, impel it upwards, and check its aſcent; till, at length, admitting as much of the external air, as would come in, the quick-ſilver was impell'd up, almoſt, to its firſt height; which it could not fully regain, becauſe ſome little particles of air were lodg'd among thoſe of the quick-ſilver, and roſe in bubbles to the top of the tube.

It is remarkable, that having, two or three times, try'd this experiment, in a ſmall veſſel; upon the very firſt cylinder of air that was drawn out of the receiver, the mercury fell, in the tube, 18 inches and a half; and, at another time, 19 inches and a half.

We, likewiſe, made the experiment in a tube leſs than two feet in length; and, when there was ſo much air drawn out of the receiver, that the remaining part could not counter-balance the mercurial cylinder, it fell

above

above a span at the first stroke; and the external air being let in, impell'd it up again, almost to the top of the tube: so little matters it, how heavy or light the cylinder of quick-silver be, provided its gravity overpower the pressure of as much external air, as bears upon the surface of that mercury into which it is to fall. PNEUMATICS.

Lastly, we observ'd, that if more air were impell'd up, by the pump, into the receiver, after the quick-silver had regain'd its usual standard in the tube, it would ascend still higher; and immediately, upon letting out that air, fall again to the height it rested at before.

But, in order to fill the *Torricellian* tube with exactness, the edges of the open end should be made even, and turned inwards, that so the orifice, not much exceeding a quarter of an inch in diameter, may be the more easily, and exactly stop'd by the finger; between which, and the quick-silver, that there may be no air intercepted, it is requisite that the tube be perfectly full, that the finger, pressing upon the protuberant mercury, may rather throw some out, than not find enough to keep out the air exactly. It is, also, an useful way, not quite to fill the tube, but to leave, near the top, about a quarter of an inch empty: for, if you then stop the open end, and invert the tube, that quarter of an inch of air, will ascend in a great bubble to the top; and, in its passage, lick up all the little bubbles, and unite them with itself, into one great one. So that, if by re-inverting the tube, you let that bubble return to the open end of it, you will have a much closer mercurial cylinder than before; and need add but a very little quick-silver more, to fill up the tube exactly. And, lastly, as for such less, and invisible parcels of air, which cannot be thus gather'd up, you may endeavour, before you invert the tube, to free the quick-silver from them, by shaking the glass, and gently knocking on the outside of it, after every little parcel of quick-silver pour'd in; and afterwards, forcing the bubbles to disclose themselves, and break, by applying a hot-iron near the top of the glass; which will raise the bubbles so powerfully, as to make the mercury appear to boil. I remember, that by carefully filling a short tube, tho' not quite free from air, we have made the mercurial cylinder reach to thirty inches, and above an eighth; which is mention'd, because we have found, by experience, that in short tubes, a little air is more prejudicial to the experiment, than in long ones.

18. We fill'd a glass tube, about three feet long, with mercury; and having inverted it into a vessel of other quick-silver, that in the tube, fell down to its usual height; leaving some little particles of air in the space it had deserted: for, by the application of hot bodies, to the upper part of the tube, the quick-silver would be a little depress'd. Lastly, having put both the tube, and the vessel whereon it rested, into a convenient wooden frame, we placed them together in a window of my chamber. *Odd phenomena of the mercurial barometer.*

And during several weeks, that the tube continu'd there, I observ'd, that the quick-silver did, sometimes faintly imitate the liquor of a thermometer, PNEUMATICS. meter; ſubſiding a little in warm, and riſing a little in cold weather; which we aſcrib'd to the greater, or leſſer preſſure of that little air, which remain'd at the top of the tube, expanded, or condens'd by the heat, or cold of the ambient air. But, the quick-ſilver often roſe, and fell in the tube very conſiderably, after a manner, quite contrary to that of weather-glaſſes, where air is at the top; for ſometimes, I obſerv'd it, in very cold weather, to ſink much lower, than at other times, when the air was comparatively warmer. And ſometimes, the quick-ſilver would, for ſeveral days together, reſt almoſt at the ſame height; and at others, it would in the compaſs of the ſame day conſiderably vary its altitude; tho' there appear'd no change, either in the air abroad, or in the temper of that within my room, nor in any thing elſe, to which ſuch a change could reaſonably be imputed; eſpecially conſidering, that the ſpace wherein the mercury continued unſettled for five weeks, amounted to full two inches; deſcending in that time about $\frac{2}{10}$ of an inch from the place where it firſt ſettled, and aſcending the other inch, and $\frac{7}{10}$: and when we took the tube out of the frame, after it had ſtaid there part of *November*, and *December*, a large fire being then in the room, we found the mercurial cylinder to be above the upper ſurface of the ſtagnant mercury 29 $\frac{3}{4}$ inches *.

Such

* That the quick-ſilver in the barometer ſhould ſtand lower, when the air is thick and moiſt, than when it is dry, and clear, ſeems to overthrow the theory of the air's gravitation. Indeed, to diſcover the cauſes of all the minute variations in the air, is a very difficult task. The winds have a great ſhare herein, with the vapours, exhalations, and expirations of the earth; perhaps alſo, the changes, which happen in the adjacent regions; the flux and reflux cauſed by the moon in the air, no leſs than in the ſea, and many other particulars, are not unconcern'd. Now, the air is heavier, than the vapours it ſuſtains; its particles being more groſs, and ariſing from denſer bodies, than the particles of vapours. But, winds may change this weight of the air, in any particular region; either by bringing, and keeping up more air over it, as may eaſily happen, when two contrary winds blow; or by ſweeping it away, and affording room for the ſubjacent air to expand itſelf; as may be the caſe, when two oppoſite winds meet, or, when only one blows exceeding ſtrong. Thus, 'tis fact, that violent guſts make the mercury in the barometer greatly to ſink of a ſudden. The cold nitrous particles of the air, or the air itſelf, being condenſed by cold in the north, and blown to another quarter, may, not only condenſe the atmoſphere, but make it heavier. Moreover, heavy dry exhalations will increaſe the weight of the air, (as ſalts and metals diſſolv'd in proper menſtrua, increaſe the ſpecific gravity of them;) and perhaps, at the ſame time, add to its elaſticity. Again, the air, by theſe, or the like cauſes, being rendred heavier, is the more able to ſuſtain the vapours; which therefore coming to be intimately mix'd therewith, and floating every where uniformly therein, render it fair and clear. But, when from contrary cauſes it becomes lighter, 'tis rendred unable to ſuſtain the vapours, which always oppreſs it; ſo that being, as it were, precipitated together, they form clouds, and running into drops, fall, by their increaſed gravity, to the earth. Hence we ſee, what cauſes render the air heavier, and more able to ſuſtain the quick-ſilver in the barometer, namely, ſuch as make the air clear and dry: but the cauſes, which render the air light and unfit to ſuſtain the mercury, produce rain. When therefore, the air is lighteſt, and the mercury in the barometer loweſt, the clouds appear very low, and in very ſwift motion; and the air having clear'd itſelf of its clouds by rain, becomes very bright and tranſ-

Such an inequality in the rise, and fall of the mercury will, I fear, render it difficult to determine by the barometer, whether the moon be the cause of the tides, especially, till the reason of this odd phenomenon be certainly known; which seems principally to depend upon considerable alterations in the air, in point of rarity and density. PNEUMATICS.

19. We took a tube of glass, about four feet in length, hermetically seal'd at one end, fill'd it with common water, and inverted the open end, beneath the surface of a vessel of water. Then this vessel, with the tube in it, being let down into the receiver, the pump was set on work; when, till the receiver was moderately exhausted, the tube continu'd quite full of water; it being requisite, that a great part of the air contain'd in the receiver should be drawn out, to bring the remaining to an equilibrium, with so short a cylinder of water. But, when once the water began to fall in the tube, each exsuction of air made it descend a little lower; tho' nothing near so much, nor so unequally, as the quick-silver did. The lowest, we were able to draw down the water, was, to about a foot above the surface of that in the vessel. And, when the water was drawn down thus low, we found, that by letting in the outward air, it might be immediately impell'd up again, to the higher parts of the tube. *A like experiment made with water.*

Upon making this experiment in a small receiver, we observ'd, that at the first exsuction of the air, the water usually subsided several inches; and at the second, sometimes near two feet; whereupon letting in the external air, the water was impell'd up, with a very great velocity.

20. That the air hath a considerable elastic power, we have abundantly proved: but, whether water participates, in any measure, thereof, seems hitherto, to have been scarce consider'd. *Whether water be elastic?*

Into a large glass bubble, with a long neck, we pour'd common water, till it reach'd about a span above the bubble; and a piece of paper being pasted thereon, we put it, unstopp'd, into the receiver; when, the pump

transparent, so as to afford an excellent prospect of remote objects. But, when it is heavy, and the quick-silver stands high in the barometer, the heavens appear fair, but somewhat thick, by reason of the vapours, every where equally dispersed therein, and is less fit to afford a good view of objects at a distance. And if any clouds are seen, they be very high, and move slow. When the air is at the heaviest, thick clouds sometimes cover the earth, consisting probably of such exhalations, as the air, at that time, is unable to sustain; and which, cannot float therein, when 'tis light. In our climate, the barometer stands highest, when the weather is coldest, and when the east, or north-east winds blow; because, at that time, two winds blow together, from opposite parts; for in the *Atlantic* ocean, at the degree of latitude answering to ours, the wind, almost continually blows west; and when the north-wind blows, an air condens'd by cold is brought to us. Farther, in the most northern regions, the height of the barometer varies more, than in the southern; the winds being there more strong, changeable, and contrary to one another, on a small tract of land; whereby, at one time, they heap up, and condense the air, and at another, sweep it away, and rarify it. Lastly, the barometer varies least between the tropics, because the wind is there almost always gentle, and blows the same way. See *Clark. Annotat. in Rohault.* & *Philos. Trans.* No. 181. 292.

PNEUMATICS. was work'd, after the uſual manner, and a conſiderable part of the air in the receiver drawn out, before we diſcern'd any expanſion of the water; but continuing to pump, the water manifeſtly began to aſcend in the ſtem of the glaſs, and ſeveral bubbles, from the lower parts of the veſſel, made their way thro' the liquor to the top of it, and there broke into the receiver. After the water once appear'd to ſwell, at each time the air was let out from the receiver into the pump, the water in the neck of the glaſs, ſuddenly roſe, about the breadth of a barley-corn, and ſo by degrees attain'd to a conſiderable height, above the mark. And at length, the external air, being ſuddenly re-admitted, the water immediately ſubſided, and deſerted all the additional ſpace, it had gain'd in the glaſs.

21. We convey'd into the receiver a new glaſs-vial, capable of holding about ſix or ſeven ounces of water; into which we had before-hand put only two or three ſpoonfuls of that fluid, and ſtopp'd it cloſe with a fit cork. The receiver being emptied, there appear'd no change in the incloſed water; the air, impriſon'd with it, not having the force to blow out the ſtopple. Wherefore, we again put in the vial, leſs firmly cloſed than before; but when the air was pumped out of the receiver, that within the vial quickly found little paſſages to get out at: for when the vial was put in the time before, the water remain'd all the while perfectly free from bubbles; but now the bottom of the glaſs appear'd all cover'd with them, which, upon the return of the excluded air, preſently ſhrunk up.

Hence it ſeem'd deducible, that, whilſt the vial continu'd well ſtopp'd, the included water ſuſtain'd, from the air ſhut up with it, a preſſure equal to that of the atmoſphere; ſince, till the air could get out of the glaſs, there appear'd no bubbles in the water, notwithſtanding the want of preſſure in the ambient body.

But, further, we cauſed a convenient quantity of water to be hermetically ſeal'd up in a glaſs-egg, whoſe long neck was faſten'd to one end of a ſtring, the other end whereof was ty'd to the cover of our receiver; then the egg being convey'd into the receiver, and that being evacuated, we, by turning the braſs-ſtopple, ſo ſhorten'd the ſtring, as to break the glaſs; whereby liberty being given to the air impriſon'd in the egg, to paſs into the receiver, its ſudden receſs made ſo many bubbles appear immediately, and aſcend ſo ſwiftly in the water, that their motion look'd like that of a violent ſhower of rain; except that the bubbles did not, like the drops of rain, tend downwards, but upwards; as happens in the diſſolution of ſeed-pearl, in ſome very acid menſtruum, wherein, if a large quantity of the pearls be caſt whole, they will, at firſt, be carry'd in ſwarms from the bottom to the top of the liquor. And, without ſealing up the glaſs, this experiment may be try'd in a ſmall receiver: for the air may here be drawn out ſo ſoon, that the bubbles, lurking in the water, will, immediately, diſplay themſelves, and aſcend in throngs. So that, having made the experiment, in ſuch a receiver, with red wine, inſtead of water, the wine appear'd all cover'd with a large vaniſhing white froth.

22. To

22. To discover whether the expansion of the water really proceeded from an elastic power in the parts of that fluid; we fill'd a glass-vial, with a pound and some ounces of water, and then put into it a glass-pipe, open at both ends, and several inches in length, so as to reach a little below the neck; then we carefully cemented it thereto, that no air might come into the vial, nor any water get out of it, but thro' the pipe; and the pipe, being warily fill'd about half way with water, and a mark being pasted over-against the upper surface thereof, the whole was, by strings, let down into the receiver: when, pumping out the air, the water in the pipe began to rise, while some little bubbles discover'd themselves on its sides; and, soon after, the water still swelling, there appear'd, at the bottom of the pipe, a bubble, about the bigness of a small pea; which, ascending thro' the tube to the top of the water, staid there a while, and then broke. But the pump being nimbly ply'd, the expansion of the water so increased, that, quickly getting up to the top of the pipe, some drops of it began to run down along the outside of it; which obliged us to forbear pumping a while, and let it subside, as it did, within less than two inches of the bottom of the pipe. Then the pump being again set on work, the bubbles began to ascend from the bottom of the pipe; of which we reckon'd about sixty large ones, that ascended one after another. And, at length, letting in the external air, the water, in the pipe, instantly fell down almost to the bottom of it. PNEUMATICS.

When the greater part of the air had been pump'd out of the receiver, the bubbles ascended so very slowly in the pipe, that their progress was scarce discernible; their magnitude not permitting them sufficiently to expand themselves in the cavity of the glass, without pressing against the sides of it. And, what seems strange, these bubbles were commonly much larger than those which rose before them; some of them being equal in bulk to four or five peas.

And tho', in ordinary bubbles, the air, together with the thin film of water that invests it, commonly swells above the surface of the water, and constitutes hemispherical bodies; the little parcels of air, that came up after the receiver was tolerably emptied, did not make protuberant bubbles; but such, whose upper surface was either level with, or beneath that of the water: so that, the upper surface being usually somewhat convex, the less protuberant parts had a quantity of water above them.

We farther observ'd, that, in the bubbles which first appear'd, the ascending air made its way upwards, by dividing the water thro' which it pass'd; in those that rose at the latter end of the experiment, the ascending parcels of air, having now little more than the weight of the incumbent water to surmount, were able to expand themselves, so as to fill that part of the pipe which they pervaded, and, by pressing every way against the sides of it, to raise what water they found above them, without letting any considerable quantity glide down along the sides of the glass: so that, sometimes, we could see a bubble thrust on before it a whole cylinder of water, perhaps an inch high, and carry it up to the top of

PNEUMATICS. of the pipe; tho', upon letting in the external air, these bubbles suddenly vanish'd.

Hence it appears, that the air, and other bodies under water, may be press'd upon as well by the atmosphere, as by the weight of the incumbent water. Hence, likewise, it cannot from the preceding experiment be safely concluded, that water uncompressed, has an elastic power; since the intumescence, produced in that experiment, may be ascribed to the numerous little bubbles produced in water, freed from the pressure of the atmosphere. And hence, lastly, it seems probable, that, in the interstices of water, there lie conceal'd many parcels either of air, or something analogous thereto; tho' so very small, that they have not been hitherto suspected to lurk there.

23. It may, indeed, be conjectur'd, that these bubbles proceed not so much from any air in the water, as from the more subtile parts of the water itself.

We, therefore, repeated our former experiment, in a three-foot tube, fill'd with water, and in a small receiver; and found, that, upon the subsiding of the fluid, so many bubbles, visibly broke into the upper part of the tube, that, having afterwards let in the external air, the water was not thereby impell'd to the top, within more than half an inch. Then we, again, drew the air out of the receiver, and found, that, by reason of the body which possess'd the top of the tube, we were able, not only to make the water fall to a level, with the surface of that in the vessel; but also a great way beneath it. Now, since this could not well be ascribed to the bare subsiding of the water by its own weight, the water seems to have been depressed by the air. And, indeed, the surface of the water, in the tube, was much more concave than usual. And, by the way, when the water, in the pipe, was sunk almost as low as the water without; we observ'd, that, by the bare application of the hand, moderately warm, to the deserted part of the tube, the remaining water would be, suddenly, considerably depress'd. And having, for a while, held a kindled coal to the outside of the tube; the air was, by the heat, so far expanded, that it quickly drove the water to the bottom of the tube, which rested several inches below the surface of the ambient water. Hence it appears, that the air, when expanded to between ninety, and a hundred times its natural dimensions, will, yet, readily admit of a much farther rarifaction, by heat.

But, to proceed; in case our bubbles were produced by air, lurking in the water; that air being got together at the top of the tube, I imagin'd, if the receiver were again exhausted, bubbles would not rise, as before: and, accordingly, the air being again pumped out, the water, in the tube, descended; but, for a great while, we scarce saw one bubble appear; only when the receiver had been very much exhausted, and the water fallen very low, we discover'd, near the bottom of the tube, some little ones, which seem'd to consist of such parcels of air, as had not, by reason

son of their smallness, got up to the top of the water, with the more bulky and vigorous sort. And having, by letting in the air, forced up the water into the tube, we could not perceive that it ascended near the top, tho' the engine remain'd unemploy'd for two or three nights together. Having, also, try'd a like experiment with quick-silver, instead of water, in a tube about a foot and a half long; upon drawing down the quick-silver as low as possible, and letting in the external air, we found, that some lurking particles of air were got up to the top of the tube, and hinder'd the quick-silver from rising to that height again. And, tho' the mercury were, by this means, brought to appear as a very close cylinder; yet the air, in the receiver, being again evacuated, I could perceive several little bubbles fasten'd to the inside of the tube, near the bottom. And, having purposely watched one or two of the principal, I observ'd, that tho' they grew gradually bigger, as the surface of the mercurial cylinder fell nearer to them; so that, at length, they swell'd to a considerable bulk; yet, upon letting in the air, they did not break, but presently shrunk up, till they became invisible.

Hence, it seems highly probable, that, even in the closest, and most ponderous liquors, and, therefore, much rather in water, there may lurk undiscernible parcels of air, capable, upon the removal of the pressure of the atmosphere, and that of the liquor wherein it lurks, to produce conspicuous bubbles.

From these several particulars, it seems plain, that the bubbles we have been treating of, were produced by such a substance, as may be properly enough call'd air; tho' we do not, positively, determine, whether air be a primogenial body, that cannot be generated, or turn'd into water, or any other body. This seems an important question, and might greatly conduce to explain the nature of the air.

Whether air may be generated, or transmuted.

Many naturalists esteem the air to be ingenerable, and incorruptible; and plausible reasons may be drawn, to countenance this opinion, from the permanency required in the corporeal principles of other bodies. *Schottus* tells us, that, in the *Musæum Kircherianum*, there is a glass, near half full of ordinary spring-water, which, having been hermetically seal'd up by the famous *Clavius*, is, to this day, preserv'd not only clear and pure, but without, in the least, turning into air, tho' it has stood for fifty years.

Nor doth it appear, in those glasses which are hermetically seal'd for chymical uses, that the included air, during its long imprisonment, notwithstanding the alteration it receives from various degrees of heat, discernibly alters its nature; whilst we plainly perceive, in digestions and distillations, that, tho' water may be rarify'd into vapours; yet it is not, really, changed into air, but only divided by heat, and diffused into very minute parts; which, meeting together, presently return to such water as they constituted before. And even spirit of wine, and other subtile and fugitive spirits, tho' they readily fly into the air, and mingle with it, do yet, in the glasses of chymists, easily resume the form of liquors. And so

PNEUMATICS. volatile salts, tho' they will readily disperse themselves in the air, and play up and down the capacity of a receiver; yet, after a while, fasten themselves to the inside thereof, in the form of salts.

And the experiment made in our engine, with a piece of match, seems to shew, that even those light and subtile fumes, into which the fire itself shatters dry bodies, have no such spring as that of the air; since they were unable to hinder the expansion of the air, included in a bladder they surrounded. *Josephus Acosta*, indeed, tells us, that he saw, in the *West-Indies*, some grates of iron so rusted and consumed by the air, that the metal crumbled between the fingers, was like parch'd straw. *Varenius*, also, tells us, that, in the islands call'd *Azores*, the air is so sharp, as, in a short time, to fret not only iron-plates, but the very tiles upon the roofs of houses, and reduce them to dust. But it may be said, that these authors ascribe such effects, chiefly, to the winds; and that the corrosion of the iron may proceed not from the air itself, or any of its genuine parts; but from some saline corpuscles dispersed thro' it, and driven, by the winds, against the bodies it is presumed to fret.

But, to try whether water could be turn'd into air, we fill'd an æolipile therewith; and placing it upon kindled coals, when the heat forc'd out a vehement stream of aqueous vapours, we ty'd an empty bladder about the neck of it; and finding the æolipile, after a while, to blow up the bladder, we carefully ty'd it again, that the included substance might not get away. Then slipping it off from the æolipile, we convey'd it into our exhausted receiver, and found, that the included substance expanded to a much greater bulk than before. And, having again taken out the bladder, we suffer'd it to remain ty'd up till the next morning, when it appear'd little less tumid: but, upon repeating the experiment, I found it very difficult to make it so accurately, as to shew, that water may be rarify'd into true air.

On the other hand, we found, by experience, that water, rarify'd into vapour, may, for a while, resemble the elastic power of the air. For, if you fill a convenient æolipile with water, and lay it upon quick-coals, you may, after a while, observe so great a pressure of some of the parts, contain'd in it, upon others; that the water will, sometimes, be thrown up into the air, above three or four feet high. And, if you then take the æolipile, almost red-hot, from the fire, you may perceive, that the water will, for a considerable time, be spouted out in a violent stream. And, if there remains but little water in the æolipile, when 'tis thus taken from the fire; immersing the neck of it into cold water, you will find, that, after it begins to draw some of it in, there will be generated, from time to time, many large bubbles in that water wherein the neck was plunged. These bubbles seem manifestly to proceed from hence, that, for a while, the heat, in the æolipile, continues strong enough to rarify part of the water that is suck'd in, and expel it, in the form of vapours, thro' that incumbent on the pipe. If, also, when the æolipile is almost full of water, you hold a fire-brand in that stream of vapours which issues out of of the narrow mouth thereof, it will be very ſtrongly blown with a conſiderable noiſe. And it has been obſerved, that, by placing the brand almoſt at the mouth of the æolipile, the wind appear'd more vehement, than if it were held ſome inches from it. Pneumatics.

The elaſtic power of this ſtream, indeed, ſeems manifeſtly owing to the heat that expands, and agitates the aqueous particles thereof; and ſuch rapid winds ſeem to be but water broke into little parts, and put in motion; ſince, by holding a ſolid, ſmooth, and cloſe body againſt it, the vapours condenſing thereon, will preſently cover that body with water.

But *Kircher* relates a remarkable experiment, which ſeems to ſhew, that water is convertible into air. He tells us, that he made an hydraulic organ, which was ſupplied with wind after the following manner. " There " was built a little chamber A H, five feet high, and three broad, with " two tranſverſe partitions C D, and E F, perforated like a ſieve; under theſe " ran a pipe G, which carried the water that, by a ſtop-cock, was let out at " H: the water, therefore, ruſhing in violently at G, excited a very great " wind within; which bringing too much moiſture along with it, the " partitions were contrived to purge it therefrom, that it might be convey'd more pure thro' the pipe A: but to render the air ſtill more pure, " we made a ſpiral tube of lead Q R, and inſerted it into the veſſel S: " by which means the air arrvied at the organ, thro' the orifice Z, as dry " as if it had come out of an oven". *Fig.* 35.

Now, if the wind that blows the organ here, doth not, upon the ceſſation of its unuſual agitation, gradually relapſe into water, I ſhould ſtrongly ſuſpect, that 'tis poſſible for water to be eaſily turn'd into air; for it can ſcarce ſeem probable, that ſo little air, as is commonly contain'd in water, ſhould be able, in ſo ſmall a quantity of water, as ſeems here employ'd, to make ſo violent a wind as our author ſpeaks of. I, therefore, ſuſpect that the wind,in this caſe, may be produced by ſmall particles of the water it ſelf, forcibly expell'd out of the chamber into the organ. And tho' no heat intervenes, perhaps, motion alone, if vehement, may ſuffice to break water into very minute parts, and make them aſcend upwards, if they cannot, otherwiſe, more eaſily , continue their agitation. For, I remember, that betwixt *Lyons* and *Geneva*, where the *Rhone* is ſuddenly ſtraitned by two rocks, exceedingly near each other, that rapid ſtream, daſhing, with great impetuoſity, againſt them, breaks part of its water into ſuch minute corpuſcles, and gives it ſuch a motion, that a miſt, as it were, may be obſerv'd at a conſiderable diſtance, ariſing from the place, and aſcending high into the air. But, it ſeems odd, that aqueous vapours ſhould, like a dry wind, paſs thro' ſuch a long winding pipe of lead, as that deſcribed by our author; ſince we ſee, in the heads of ſtills, and in the necks of æolipiles, ſuch vapours are preſently, even by a very little cold, condenſed into water.

We took a clear glaſs bubble, capable of containing three ounces of water, with a long and wide cylindrical neck ; this we fill'd w th oil of vitriol, and fair water, of each almoſt a like quantity; and caſting in ſix ſmall

Pneumatics. ſmall iron nails, we ſtop'd the mouth of the glaſs, which was now full of liquor, with a piece of diapalma, and ſpeedily inverting the bubble, we put the neck of it into a ſmall wide-mouth'd glaſs, with more of the ſame liquor in it; and as ſoon as the neck had reach'd the bottom of the liquor, there appear'd, at the upper-part of the vial, a bubble, about the bigneſs of a pea, which ſeem'd rather to conſiſt of new ſmall bubbles, produc'd by the action of the diſſolving liquor upon the iron, than any parcel of the external air, that might be ſuſpected to have got in upon the inverſion of the glaſs; eſpecially ſince we allow'd time to thoſe little particles of air, which were carried down with the nails, to fly up again: and, ſoon after, we perceiv'd the bubbles, produced by the action of the menſtruum upon the metal, aſcending in ſwarms to the former; and breaking into it, they ſoon exceedingly increas'd it, and, by degrees, depreſs'd the water, till, at length, the ſubſtance contain'd in theſe bubbles, poſſeſs'd the whole cavity of the vial, and moſt of its neck too; reaching much lower therein, than the ſurface of the ambient liquor, wherewith the open-mouth'd glaſs was, by this means, almoſt repleniſhed. We ſuffer'd both the vial, and the open-mouth'd glaſs, to remain as they were, in a window, for three or four days and nights together; but often looking upon them, during that time, as well as at the expiration of it, the whole cavity of the glaſs bubble, and moſt of its neck, ſeem'd to be poſſeſs'd by air; ſince, by its ſpring, it was able, for ſo long, to hinder the expell'd liquor that ſurrounded it, from regaining its former place. And juſt before we took the vial out of the other glaſs, upon the application of a warm hand to the convex part of the bubble, the impriſon'd ſubſtance readily dilated itſelf, like air, and broke thro' the liquor in ſeveral ſucceeding bubbles.

Having alſo, at another time, made the like experiment, with a ſmall vial, and nails diſſolv'd in *Aqua fortis*, we found it ſucceeded as the foregoing. And here we obſerv'd, that the ſteams newly generated, did not only poſſeſs almoſt the whole cavity of the glaſs, but ſeveral times, of themſelves, broke away in large bubbles, thro' the ambient liquor into the open air: whence theſe experiments ſeem'd, manifeſtly, to prove, that, in general, air may be generated *de novo.*

And if, according to the mechanical hypotheſis, the difference of bodies proceeds but from the various magnitudes, figures, motions, and textures of the ſmall parts they conſiſt of; there appears no reaſon why the minute parts of water, and other bodies, may not be ſo agitated, or connected, as to deſerve the name of air.

24. We choſe a glaſs-egg, half an inch in diameter at the top, and an inch at the bottom; and filling it with common water, to the height of about a foot and a half, ſo that the upper part remain'd empty, we encloſed it in the receiver; and, upon pumping, obſerved bubbles at the bottom and ſides of the glaſs; and, increaſing as the air was drawn away, they, from time to time, plentifully aſcended to the top of the water, where they quickly broke: but the wideneſs of the glaſs allowing them free paſ-

passage thro' the water, they did not, as in the former experiments, seem to make it swell; and, upon the return of the external air, the water appear'd to have lost of its first extent, by the avolation of the air interspersed. PNEUMATICS.

We put about two ounces of rain-water, carefully distill'd, into a round glass-bubble, with a very small neck, which was thereby fill'd half way to the top, and then convey'd it into the receiver; and, tho' we drew out more air than ordinary, there appear'd not the least intumescence of the water, nor any ascending bubbles. But suspecting that either the small quantity of the water, or the figure of the vessel, might affect the experiment, we took the former glass egg, and another, not much different from it, and fill'd the first, with distill'd rain-water, to the old mark, and, into the latter, put a long cylinder of solid glass, to straiten the cavity of the neck; and then pouring some distill'd water into that, also, till it reach'd near the top, they were both let down into the receiver: but here the air was so far exhausted, before there appear'd any bubble in either of the glasses, that the difference betwixt this, and common water, was very manifest. But, at length, when the air was almost quite drawn out, the bubbles began to disclose themselves, and to increase, as the pressure of the air, in the receiver, decreas'd. But, in the first egg, the bubbles were very small, and never able to swell the water above the mark; in the other, whose neck was straitned, great numbers of large ones, fasten'd themselves to the lower-end of the solid piece of glass, and gather'd to such a degree, between it and the sides of the neck, that the water swell'd a finger's breadth above the mark; tho', upon admitting the external air, it relaps'd to the former mark, or rather fell below it: upon which, all the bubbles presently disappear'd in the former vessel; whilst several remain'd fasten'd to the lower-part of the glass cylinder, and continued there for above an hour after, but contracted in their dimensions.

And having suffer'd these glasses to remain above twenty-four hours in the receiver, we, afterwards, repeated the experiment; but tho' the receiver was carefully exhausted, yet we scarce saw a bubble in either of the glasses; yet the water rose the breadth of a barley-corn in the neck of that glass wherein the solid cylinder had been placed; the liquor, in the other, not being sensibly swell'd. And, lastly, upon letting in the air, the water in the straitned neck, soon subsided to the mark, above which it had stretched.

25. We took a glass egg, with a long neck, of about $\frac{1}{7}$ inch in diameter, and pouring in sallad-oil till it reach'd above half-way to the top, we inclosd it in the receiver, together with some common water in a similar vessel. The pump being set on work, there began to appear bubbles in the oil, much sooner than in the water; and afterwards they, also, ascended more plentifully in the former, than in the latter; and when the receiver was well exhausted, the bubbles rose almost as numerous as ever: so that none of the various liquors, we have try'd, seem'd to abound more with aerial particles, than this oil. And here 'twas remarkable, that, between the time it *The air contain'd in oil.*

it was put into the receiver, and that before we could work the pump, it ſubſided about half an inch below the mark it at firſt reach'd to.

Oil of turpentine. Common oil of turpentine, being put into a ſmall glaſs bubble with a ſlender neck, ſo as to fill it about two inches from the top, preſented us, upon evacuating the receiver, with numerous bubbles; moſt of which, riſing from the bottom, expanded themſelves exceedingly in their aſcent, and made the liquor, in the neck, to ſwell ſo much by degrees, that at length, it ſeveral times ran over at the top: whereby we were hinder'd from diſcerning, upon letting in the air, how much the ſinking of the oil, below the firſt mark, was due to the receſs of the bubbles.

Oil of tartar. Having fill'd a glaſs egg with a very ſtrong ſolution of ſalt of tartar in fair water; tho' this, except quick-ſilver, is reckon'd the heavieſt of liquors; we try'd, whether it would afford any bubbles; and putting it into the receiver, along with other liquors, we found that they yielded many bubbles, long before any appear'd in that: and upon proſecuting the experiment, it ſeem'd, of all the liquors whereof we made trial, this afforded the feweſt, and ſmalleſt bubbles.

Spirit of vinegar, red wine, and milk. Spirit of vinegar, examined after the ſame manner, exhibited a moderate quantity of bubbles. In red wine, we found nothing very remarkable: for tho' upon the exſuction of the air, the bubbles aſcended in it, as it were in ſhoals, and ſhifted places, among themſelves, in their aſcent; yet the intumeſcence of the whole bulk of the liquor, was ſcarce ſenſible; the bubbles moſt commonly breaking very ſoon after their arrival at the top; where during their ſtay, they compoſed a kind of ſhallow froth, which, alone, appear'd higher, in the neck of the glaſs, than the wine, when it was firſt let down. Milk convey'd into our receiver, preſented us with nothing conſiderable, except that the bubbles, not eaſily breaking at the top, and thruſting up one another, made the intumeſcence appear much greater, than that of common water.

Eggs. We likewiſe convey'd hens eggs into the receiver, but after the exſuction of the air, took them out whole again.

Spirit of urine, and of wine. We put ſome ſpirit of urine into a glaſs egg, fill'd another glaſs, to about two thirds of its neck, with rectified ſpirit of wine, and a third with common water, till it reach'd to the middle of the neck, and then pour'd to it of the ſame ſpirit of wine, till it reach'd about an inch higher. Theſe glaſſes, having marks ſet on them, over againſt the tops of the contain'd liquors, were put into the receiver, and that beginning to be evacuated, bubbles began to appear in all three. The mixture of ſpirit of wine and water, diſcloſed numerous bubbles, eſpecially towards the top, and the ſpirit of urine appear'd to ſwell near an inch and a half above the mark, and yielded plenty of bubbles, which made a kind of froth at the upper part of it; and above that, there appear'd eight or ten great bubbles, one higher than another, each of them conſtituting, as it were, a cylinder of about half an inch high, and as broad as the internal cavity of the neck; ſo that all the upper part of the neck ſeem'd to be divided into equal parts, by tranſ-

verſe

verſe partitions, conſiſting of the coats of the bubbles, whoſe edges appear'd like ſo many rings, ſuſpended one above another. PNEUMATICS.

In the ſpirit of wine, there aroſe a great multitude of bubbles, all the while the experiment was in hand, which aſcended with a great velocity, and being arriv'd at the top, made no ſtay there; yet, notwithſtanding the great fluidity and volatility of the liquor, before they broke they lifted up the upper ſurface of it, and for a moment or two, form'd thereof, a thin film, which appear'd protuberant, above the reſt of the ſuperficies, like a ſmall hemiſphere: theſe alſo aſcended in ſtrait lines, whilſt thoſe produced at the lower part of the veſſel, containing the mixture of the water and ſpirit of wine, aſcended with a wavering motion, deſcribing an indented line. Laſtly, it was obſervable in the ſpirit of wine, as alſo in the oil of turpentine, lately mention'd, that not only the bubbles ſeem'd to riſe from determinate places, at the bottom of the glaſs; but that, in their aſcent, they kept an almoſt equal diſtance from each other, and ſucceeded in a certain order, whence they ſeem'd part of ſmall bracelets, conſiſting of equally ſmall ſeparate beads; the lower end of each bracelet being, as it were, faſten'd to a point, at the bottom of the glaſs.

The air being ſparingly let into the receiver, the great bubbles incumbent upon one another, in the glaſs that contain'd the ſpirit of urine, were by regular degrees leſſen'd, till at length, they wholly ſubſided. Notwithſtanding the receſs of ſo many bubbles as broke on the top of the ſpirit of urine, during all the time of the experiment, yet it ſcarcely appear'd, at all ſunk below the mark. Nor did the mixture of ſpirit of wine and water conſiderably ſubſide. But the ſpirit of wine, not only viſibly expanded itſelf in the neck of the veſſel, that contain'd it, whilſt the bubbles broke at the top of it, almoſt as ſoon as they arrived there; but upon the re-admiſſion of the external air, it retain'd its new expanſion. And, tho' we let it alone, for near an hour together, yet when we took it out, it ſtill ſwell'd between a quarter and half an inch above the mark. Repeating the experiment with freſh ſpirit of wine, it ſwell'd in the neck as formerly; and leaving it all night in the receiver, and allowing free acceſs to the external air at the ſtop-cock, I found it, the next day, ſtill expanded, as before; only it ſeem'd a little lower; which decreaſe, perhaps, proceeded from the avolation of ſome of the fugitive parts of the liquor. And for farther ſatisfaction, having taken out the glaſs, and conſider'd it in the open air at a window; I could not find, that there was any remaining bubble to occaſion the continuance of this ſtrange expanſion.

26. We took two very ſmall vials, of the ſize and ſhape expreſs'd in *Fig.* 36. and into one of them, put ſo much of a certain ponderous mercurial mixture, that, the mouth being ſtop'd with a little ſoft wax, the glaſs would but juſt ſink in water: this we let fall to the bottom of a wide-mouth'd cryſtal jar, fill'd with about half a pint of common water; and into the ſame veſſel, we ſunk the other glaſs, unſtop'd, with as much water in it as was more than ſufficient to make it ſubſide. Both theſe ſunk with their mouths downwards; the former being about three quarters full of air, *The gravity of air expanded under water.*

 and

and the latter containing in it a bubble of air as big as half a pea ; then the wide-mouth'd glaſs was let down into the receiver, and the engine being work'd, the bubbles began to appear in the water, as in the former experiments ; but continuing long to ply the pump, that little glaſs, whoſe mouth was open'd, came to the top of the water; being, as it were, buoy'd up by a great number of bubbles, that had faſten'd themſelves to the ſides of it ; and ſwimming thus, with the mouth downward, we could eaſily perceive, that the internal air above-mention'd, had much dilated itſelf, and thereby ſeem'd to have contributed to the emerging of the glaſs, which remain'd floating, notwithſtanding the breaking, and vaniſhing of moſt of the contiguous bubbles. And perſiſting in pumping, we obſerv'd, that at each time the key was turn'd, the air, in the little glaſs, manifeſtly expanded itſelf, and thruſt out the water ; generally retaining a very protuberant ſurface, where it was contiguous to the remaining water. And when, after ſeveral exſuctions of the air in the receiver, that in the vial ſo dilated itſelf, as to expel almoſt all the water, it turn'd up its mouth towards the ſurface of the water in the jar, and there deliver'd a large bubble, and then relaps'd into its former floating poſture.

This experiment taught us, that it was a work of more time and labour, than we imagin'd, to exhauſt our receiver as much as it may be exhauſted ; for tho' before the ſmal viall emerged, we thought the receiver conſiderably emptied, becauſe there ſeem'd to come but very little air at each exſuction, out of the cylinder ; yet, afterwards, the air included in the vial, manifeſtly dilated itſelf upon each ſtroke, ſo long, that for nine times it turn'd its mouth upwards, and diſcharg'd a bubble about the bigneſs of a pea. But that vial which had the weight in it, roſe not at all : then leiſurely letting in the air, that within the vial ſhrinking into a very narrow compaſs, the glaſs fell down to the bottom of the jar.

But being deſirous to try once more, whether the little glaſs with the weight in it, might not alſo be rais'd ; after we had ſuffer'd the engine to remain clos'd, as it was, for five or ſix hours, the pump was again ply'd ſo vigorouſly, that not only about the upper-part of the jar, there appear'd a large number of ſmall bubbles ; but afterwards, there came from the bottom of the jar, ſome as large as ſmall peas, which, the pump being ſtill kept going, follow'd one another, to the number of forty, coming from the ſtop'd vial ; whoſe mouth, it ſeems, had not been ſhut ſo cloſely, but the included air found a paſſage betwixt the wax and the glaſs. After this, the unſtop'd glaſs began to float again ; the air ſhut up in it, being ſo dilated as to expel a large part of the water, but not ſo much as to break quite thro'. And, at length, the heavier of the two vials began to riſe, but immediately ſubſided again : which ſeem'd owing to the air within it, whoſe bulk and ſpring being weakned by the receſs of the forty bubbles, it was no longer able to break thro' the incumbent water ; but forming a bubble, at the mouth of the glaſs, buoy'd it up towards the top, and there getting away, left it to ſink again ; till the preſ-

ſure

ſure of the air in the receiver, being farther taken off, the air, in the vial, was permitted to expand itſelf farther, and create another bubble, by which it was again, for a while, carry'd up. And tho', after having empty'd the receiver as far as we well could, we ceaſed from pumping; yet the veſſel, continuing more ſtanch than uſual, this aſcent, and fall of the vial were repeated to the ninth time; the included air, by reaſon of the ſmallneſs of the vent at which it muſt paſs out, being not able to get away, otherwiſe than by ſmall degrees, and, conſequently, in ſeveral ſuch parcels as were able to conſtitute bubbles, each of them big enough to raiſe the vial, and keep it ſuſpended, till the bubble flew off. Hence it may appear, that a body, lighter than an equal bulk of water, will float in that fluid, when the preſſure of the atmoſphere is, in very great meaſure, taken off from the liquor, and the body: tho' it were worth inquiring, what it is, that ſo plentifully concurs to fill the bubbles made, in our experiment, by the air ſo much expanded. PNEUMATICS.

In this experiment, as in the former, the external air being let in, ſoon precipitated the floating veſſel. And the water which, in the heavier vial, ſucceeded in the room of thoſe forty, or more, great bubbles of air, which, at ſeveral times, got out of it; was of a very inconſiderable bulk.

27. It having been obſerv'd, that pendulums vibrate more ſlowly, and that their motion ſooner ceaſes in a thicker, than in a thinner medium; we thought proper to try if a pendulum would move faſter, or vibrate longer, in our exhauſted receiver, than out of it. We, therefore, took two round poliſhed ſteel-pendulums, of equal bigneſs, each of them weighing twenty drams, bating ſo many grains. One of theſe we ſuſpended in the cavity of the receiver, by a very ſlender ſtring, about ſeven inches and a half in length, from the cover of the receiver whereto it was faſten'd. Then we made the pendulum ſwing, and, counting the returns of the other that hung in the open air, by a ſtring of about the ſame length, we ſhorten'd and lengthen'd this, till it appear'd to keep the ſame pace with that in the receiver. Then, having carefully drawn away the air, we again made the pendulum in the receiver, vibrate; and, giving the other ſuch a motion, as caus'd it to deſcribe an arch, apparently equal to that of the included pendulum, we counted the recurſions of both; and we reckon'd two and twenty vibrations of the included pendulum, whilſt but twenty were obſerv'd of the other. And at another time, alſo, the former was found to have made twenty-one returns, whilſt the other made but twenty. Yet this experiment ſeem'd to teach us little, except that the difference betwixt the motion of ſuch a pendulum, in common air, and in a medium exceedingly rarify'd, is ſcarce ſenſible in veſſels no bigger than our receiver; eſpecially, ſince we could not ſuppoſe that to be altogether free from air. We obſerved, alſo, that, when the receiver was full of air, the included pendulum continu'd its recurſions about fifteen minutes, before it left off ſwinging; and that, after the exſuction of the air, the vibration of theſame pendulum appear'd not to laſt ſenſibly longer. *A pendulum made to ſwing in vacuo.*

28. That

PNEUMATICS.

A watch and a bell in the exhausted receiver.

28. That the air is the medium whereby ſounds are convey'd to the ear, was a current opinion, till ſome pretended, that if a bell, with a ſteel clapper, be faſten'd to the inſide of a tube, upon making the experiment *de vacuo*, with it, the bell remaining ſuſpended in the deſerted ſpace, at the upper end of the tube; if a vigorous load-ſtone be apply'd on the outſide of the glaſs, it will attract the clapper; which, upon the removal of the load-ſtone, falling back, will ſtrike againſt the bell, and thereby produce a very audible ſound: whence, ſeveral have concluded, not the air, but ſome more ſubtile body, to be the medium of ſounds. But ſuſpending a watch, freed from its caſe, in the cavity of our receiver, by a packthread; and then, cloſing up the veſſel with melted plaiſter; we liſten'd near the ſides of it, and plainly heard the balance beat, and obſerv'd, that the noiſe ſeem'd to come directly in a ſtreight line, from the watch to the ear. We found, alſo, a manifeſt difference in the noiſe, by holding our ears near the ſides of the receiver, and near the cover of it; which ſeem'd to proceed from the difference between the glaſs, the cover, and the cement, thro' which the ſound was propagated. But, upon working the pump, the ſound grew gradually fainter; ſo that, when the receiver was emptied as much as uſual, we could not, by applying our ears to the very ſides of it, hear any noiſe from within; tho' we could eaſily perceive, that, by the motion of the hand which mark'd the ſeconds, and by that of the balance, the watch neither ſtood ſtill, nor ſeem'd irregular. And, to ſatisfy ourſelves farther, that it was the abſence of the air about the watch, that hinder'd us from hearing it, we let in the external air at the ſtop-cock; and then, tho' we turn'd the key, and ſtopp'd the valve, yet we could plainly hear the noiſe made by the balance; tho' we held our ears, ſometimes, at the diſtance of two feet from the outſide of the receiver. And this experiment, being repeated, ſucceeded after the like manner: which ſeems to prove, that the air is, at leaſt, the principal medium of ſounds. And, by the way, it is very well worth noting, that, in a veſſel ſo exactly cloſed as our receiver, ſo weak a pulſation as that of the balance of a watch, ſhould propagate a motion to the ear, in a ſtreight line, notwithſtanding the interpoſition of glaſs, ſo thick as that of our receiver. We, afterwards, took a bell of about two inches in diameter at the bottom, which was ſupported, in the midſt of the cavity of the receiver, by a bent ſtick, preſſing with its two ends againſt the oppoſite parts of the inſide of the veſſel; which, being cloſed up, we obſerved the bell to ſound more dead than in the open air. And yet, when we had empty'd the receiver, we could not diſcern any conſiderable change in the loudneſs of the ſound: whereby it ſeem'd, that, tho' the air be the principal medium of ſound; yet, either a more ſubtile matter may be, alſo, a medium of it; or elſe thatan ambient body, that contains but few particles of air, is ſufficient for that purpoſe. Whence, perhaps, in the above-mention'd experiment, made with the bell and the load-ſtone, there might, in the deſerted part of the tube, remain air enough to produce a ſound.

But

But as, in making the experiment of firing gun-powder with a piſtol in our evacuated receiver, the noiſe made by the flint, ſtriking againſt the ſteel, was exceeding languid, in compariſon of what it would have been in the open air: ſo, on ſeveral other occaſions, it appear'd, that the ſounds produced there, if they were not loſt, ſeem'd to arrive at the ear very much weakned. PNEUMATICS.

29. We have a liquor which, tho' moſt of its ingredients be metals, and all of them ponderous, is yet of ſuch a nature, that, whilſt the vial wherein it is kept, remains ſtopp'd, appears tranſparent, as, alſo, the upper part of the glaſs, to which the liquor reacheth not; but as ſoon as ever the ſtopple is taken out, and full acceſs given to the external air; both the under part of the cork, and the liquor itſelf, preſently ſend upwards, and diffuſe a fume, as thick and white as if a quantity of alabaſter-duſt were thrown up into the air. And this ſmoking of the liquor laſts, till the vial be ſtopp'd again; and then the aſcent of the fumes ſuddenly ceaſes. *A fuming liquor in the receiver.*

To a vial of this fuming liquor, we faſten'd a weight of lead; and, having ty'd to the ſtopple one end of a ſtring, whilſt the other was made faſt to the cover of the receiver, the liquor was carefully cloſed up; and, the air being diligently pump'd out, we unſtopp'd the vial. And tho, immediately upon drawing out the cork, there appear'd ſome white fumes, which ſeem'd to proceed from the air being impriſon'd in the vial, and diffuſing itſelf ſuddenly into the receiver; yet we afterwards obſerv'd, that the fumes did not mount, and diſperſe themſelves, as they uſed to do in the open air; but, aſcending to the lip of the vial, they ſtopp'd there, and ran down along the outſide, and thence along an inclining piece of lead, on which the vial reſted, like a little ſtream, that quitted not the vial, till it was come to the bottom of it, and there forſook it, like a ſtream of water of the ſame bigneſs. Then, letting in ſome of the external air, the ſtream run a-freſh, tho 'not altogether ſo large: and, after the receiver was fill'd with air, I found, to my ſurprize, that, tho' the ſtream diſappear'd, yet no white fumes aroſe, either from the cork, or out of the vial; no, not when the cover was removed from the receiver: tho', after a while, there aſcended white fumes from the receiver. But, having immediately taken out the vial into the open air, it emitted white exhalations, as before; and having, preſently after, unſtopp'd it in an open window, we found both it, and the cork, immediately yielded a much more plentiful ſmoke; tho' it were now ſeveral years ſince this parcel of liquor was prepared.

30. Into one of our ſmall receivers, we convey'd a piece of well-lighted match; and, letting it remain there, till it had fill'd the receiver with ſmoke, we took it out, and immediately cloſed the receiver again, that the ſmoke might not get away. Then ſtaying, to let theſe fumes leiſurely ſubſide, we found, that, after ſome time, they ſettled themſelves in the lower half of the receiver, in a darkiſh body; leaving the upper half tranſparent, and, as to ſight, full only of clear air. And, inclining the veſſel that contain'd this ſmoke, ſometimes to one ſide, and ſometimes to the other; we obſerv'd the fume to keep its ſurface almoſt horizontal, as wa- *Smoke in vacuo.*

PNEUMATICS. ter, or any other liquor, would have done in the like caſe. And if, by a quicker rocking of the engine, the ſmoke were more ſwiftly ſhaken, it would, like water, either vibrate from one ſide to the other of the glaſs; or elſe have its ſurface manifeſtly curl'd, like waves, and preſerve itſelf, in an entire and diſtinct body, from the incumbent air; and, being permitted to reſt a while, would ſoon recover its former ſmooth and level ſuperficies. If, alſo, the key were turn'd, and the valve unſtopp'd, ſo that there was a free paſſage open'd betwixt the external air, and the cavity of the receiver; then would ſome of this ſmoke fall down, as it were, in a ſtream, into the ſubjacent cylinder; and a proportionate quantity of the outward air, would, manifeſtly, aſcend thro' it, into the incumbent air; after the ſame manner, as, when a vial, with a long neck, fill'd with red wine, being inverted into a glaſs of fair water, the water and wine, by degrees, mix, as it were, in little curl'd ſtreams with each other; the one falling down, and the other aſcending in its place. And if, when the ſuperficies of our ſmoke lay ſmooth, and horizontal, a hot iron were held near the outſide of the receiver; the adjacent part of the included fumes, being rarify'd by the heat, would readily aſcend in a large pillar of ſmoke, to the very top of the receiver; yet, without ſeeming to loſe its diſtinct ſurface, or to be confounded with the air, below which, upon the receſs of the adventitious heat, it would again ſubſide.

Since, then, there is ſo vaſt an inequality in the denſity and weight of liquors; we may conſider the atmoſphere as a peculiar kind of thin fluid, much lighter than ſpirit of wine. And as waves appear'd upon the ſurface of our agitated ſmoke; ſome ſuch thing may, poſſibly, happen on the ſuperficies of the atmoſphere: as may be conjectur'd from thoſe ſtrange inequalities that often appear, eſpecially when the air abounds with exhalations and vapours, upon the limb of the ſun in its riſing and ſetting. And if this phenomenon be owing to the refraction, which the ſun's rays ſuffer in our air; 'tis eaſy to ſuppoſe the ſurface of the atmoſphere to be often, as we ſaid, exceedingly curl'd, or wav'd. And, certainly, it is ſurprizing to ſee how, thro' a good teleſcope, there will not only appear inequalities in the edge of the ſun, which often ſeems to be indented; but thoſe inequalities vaniſh in one place, and preſently appear in another, and ſeem perfectly to move, like waves ſucceeding and deſtroying one another: only their motion frequently ſeems to be quickeſt; as if, in that vaſt ſea, they were carry'd on by a current, or a tide. And this, alſo, appears to the eye, when a large, and well defined image of the ſun, is, by the teleſcope, caſt upon white paper.

The coheſion of poliſh'd marbles in vacuo.

31. It hath been thought ſtrange, that, the perfectly poliſh'd ſurfaces of two flat pieces of marble being apply'd to each other, they ſhould ſtick ſo faſt together, that the lower may be raiſed, by taking hold of the upper. But, as this ſeems owing to the unequal preſſure of the air upon the undermoſt ſtone, the lower ſuperficies of that being freely expoſed thereto, and preſs'd upon by it, whilſt the upper ſurface is defended therefrom; which, conſequently, preſſing the lower ſtone againſt the upper, hinders it

it from falling; we therefore conjectur'd, that two marbles, being exactly ground to one another, and together suspended in our evacuated receiver, the lower stone would fall from the upper: but we could not procure marbles to be ground so true, as to sustain one another in the air for above a minute or two, which is a much shorter time, than is required to empty our receiver. We did, indeed, try to make our marbles stick close together, by moistning their surfaces with rectified spirit of wine; but to little purpose; for having convey'd into the receiver two black square marbles, the one with its side two inches and a third, and somewhat more than half an inch in thickness, the other of equal surface, but not above half so thick, fastned together by means of that spirit; and having suspended the thicker by a string from the cover, we found not, that the exsuction of the air would separate them, tho' a weight of four ounces were fasten'd to the lower, to facilitate its falling. PNEUMATICS.

I would gladly have the experiment try'd with marbles, so well polish'd, as to need no liquor to make them cohere, and in a vessel, out of which, the air may be more perfectly drawn, than it was out of ours. But, tho' we will not determine, whether the spirit of wine contributed to the strong cohesion of these stones, otherwise than by keeping the subtilest parts of the air from getting in between them; yet it seem'd, that the reason, why the lower marble fell not, was, probably, because of the pressure of the air remaining in the receiver; which, as we formerly noted, being able to sustain a cylinder of water, of above a foot in height, may be supposed capable of keeping so broad a marble from descending. And, tho' this may seem a strange proof of the strength of the spring of the air, even when rarified; yet it will scarce appear incredible to him, who hath observ'd, how exceeding strong a cohesion may be made, betwixt broad bodies, only by immediate contact. A notable instance of this, is given us by the learned *Zucchius*, who tells us, that " a young fellow, bragging " of his strength, some body set him to pull at the ring in the middle of a " brass-plate, that lay upon a polish'd marble, whereto, it was exactly " ground: this he thought a trivial matter; but after his utmost endeavour, " found it impossible to separate them by direct pulling; which made him " imagine they were fastned together, by means of some vehement strong " glew, till he saw the plate, afterwards lifted by another, who, first slipt it " along the marble ".

33. Our receiver being exquisitely clos'd, and the air, in a good measure, drawn out, we remov'd it from the pump, and to the lower branch of the stop-cock, speedily apply'd a tapering valve of brass, made fit to go with its narrower end into the cavity of the branch, and to fill the orifice of that cavity with its broader part. And, that the air might not get in at the little intervals, between the convex surface of the stopple, and the internal edge of the branch, they were stop'd with diachylon. And, to the door of the valve, there was, at a button of brass, fasten'd a broad scale, wherein weights were to be put. This done, the key of the stop-cock was turn'd, and the external air beating like a forcible stream upon the valve to get in *An erect pressure exercised by the atmosphere.* *Fig.* 37.

 there,

PNEUMATICS. there, it ſuddenly ſhut the valve, and kept it ſo cloſe, that we had time to caſt in ſeveral weights, one after another, into the ſcale, till at length, the weight overpowering the preſſure of the atmoſphere, drew down the valve by the ſtrings that ty'd the ſcale to it, and gave liberty to the outward air to ruſh into the receiver. Tho', another time, when the valve had but little weight hanging to it, being, by accident, drawn down beneath its former place, it was, by the impetuous current of the external air, ſuddenly impell'd up into it again, and kept there. But, in the former experiment, tho' the receiver were not well exhauſted; tho' it leak'd, whilſt the reſt of the experiment was in hand; and tho' the valve, whereon the cylinder of the atmoſphere could preſs, were not above an inch and a half in diameter; yet the whole weight, ſupported by the air, amounted to about ten pounds, of ſixteen ounces each: ſo that, had the experiment been made with favourable circumſtances, the air endeavouring to preſs in, at the orifice of the ſtop-cock, would very probably have kept a much greater weight from falling out of it.

The preſſure of the atmoſphere computed.

33. But our pump, alone, may afford us a nobler inſtance of the force of the air; ſo that, by means of this part of our engine, we may conjecture at the ſtrength of the atmoſphere, computed as a weight. For, firſt, the ſucker, brought to move eaſily up and down the cylinder, being impell'd to the top of it, and the receiver taken off from the pump, that the upper orifice of the cylinder remaining open, the air may freely ſucceed the ſucker, and, therefore, readily yield to its motion downwards; and there being faſten'd to one of the iron teeth of the ſucker, ſuch a weight, as may juſt ſuffice to draw it to the bottom of the cylinder; we may hence find the weight neceſſary to draw down the ſucker: and when the atmoſphere makes the ordinary reſiſtance againſt its deſcent, the ſucker being again forc'd to the top of the cylinder, whoſe upper orifice muſt now be exactly clos'd; we may eaſily, by hanging a ſcale to the above-mention'd iron, that makes part of the ſucker, caſt in known weights, till the ſucker be drawn down; then, to theſe weights in the ſcale, that of the ſcale itſelf being added, the ſum will give us the weight of a column of air, equal in diameter to the ſucker, or to the cavity of the cylinder, and, in length, to the height of the atmoſphere.

According to this method, we attempted to meaſure the preſſure of the atmoſphere, but found it more difficult, than we expected, to perform it accurately; for tho', by the help of the handle, the ſucker mov'd up and down with great facility; yet, when it came to be mov'd by a dead weight, we found, that the little inequalities, and, perhaps, the unequal preſſure of the leather againſt the cavity of the cylinder, now and then ſtop'd the deſcent or aſcent of the ſucker; tho' a very little external help, would eaſily ſurmount that impediment. We found then, that a weight of twenty-eight pounds, being faſten'd to one of the teeth of the ſucker, drew it down cloſe, when the upper orifice of the cylinder was left open; but, by the help of oil, and water, and the frequent working of the ſucker with the handle, its motion in the cylinder had been before purpoſely facilitated.

cilitated. Then the upper orifice of the cylinder was very carefully ſtop'd; the valve being likewiſe ſhut, with its ſtopple well oiled, after the ſucker had been again impell'd up to the top of the cylinder. To the former weight we now added a hundred and twelve pounds, which forcing down the ſucker, though but leiſurely, we took off the 28 pound weight, and hung on, inſtead of it, fourteen pound; but found that, with the reſt, unable to carry down the ſucker. And to ſatisfy ourſelves it was the reſiſtance of the ambient air, that hinder'd the deſcent of ſo great a weight; after we had try'd, that upon unſtopping the valve, and thereby opening an acceſs to the external air, the ſucker would be immediately drawn down, having forcibly depreſs'd the ſucker, to the bottom of the cylinder, and then faſten'd weights to the iron, the preſſure of the external air, finding little reſiſtance, in the cavity of the cylinder, preſently began to impel the ſucker, with the weights that clogg'd it, towards the upper part of the cylinder, till ſome ſuch accidental impediment, as we formerly mention'd, check'd its courſe; and when that was remov'd, it would continue its aſcent to the top. And tho', poſſibly, there might remain ſome particles of air in the cylinder, after the ſucker was drawn down; yet the preſſure of a cylinder of the atmoſphere, ſomewhat leſs than three inches in diameter, uncompreſs'd, not only ſuſtain'd, but drove up a weight of a hundred and odd pounds: for, beſides the weight of the whole ſucker itſelf, which amounts to ſome pounds, the weights annex'd to it, made up a hundred and five pounds; yet all this falls ſhort of the weight juſt ſaid to be ſuſpended, by the reſiſtance of the air, in the cavity of the cylinder. This experiment was made in the winter, the weather neither froſty nor rainy, about the change of the moon; and at a place whoſe latitude is about 51 degrees and a half: for, perhaps, the force, or preſſure of the air, may vary, according to the ſeaſons of the year, the temperature of the weather, the elevation of the pole, or the phaſes of the moon; any of them ſeeming able to alter either the height, or conſiſtence of the atmoſphere. And therefore, it would not be amiſs, if this experiment were try'd carefully, at ſeveral times and places, with variety of circumſtances. It might, alſo, be try'd with cylinders of ſeveral diameters, exquiſitely fitted with ſuckers; that we might know what propoption ſeveral pillars of the atmoſphere, bear to the weight they are able to ſuſtain, or lift up; and conſequently, whether the increaſe, or decreaſe of the reſiſtance of the ambient air, can be reduced to any regular proportion, to the diameter of the ſuckers. Theſe, and other experiments, which may be made with this cylinder, might, moſt of them, be more exactly try'd by the *Torricellian* tube; if glaſs could be blown, and drawn perfectly cylindrical.

The nature of ſuction.

Here we may obſerve, that as many other phenomena of our engine, ſo eſpecially the two laſt experiments, ſeem to ſhew the nature, or cauſe of ſuction. It's true, indeed, in ſucking, we commonly uſe ſome manifeſt endeavour, by a peculiar motion of our mouths, cheſts, &c. yet it appears not how the upper-part of the emptied cylinder, that remains at reſt all the while, or any part of it, endeavours to draw the depreſs'd

 ſucker,

PNEUMATICS. ſucker, and the annex'd weights to it; tho' ſuch as behold the aſcent of the ſucker, without conſidering the cauſe of it, readily conclude it to be rais'd by ſome ſecret thing, that powerfully ſucks or attracts it. Whence it ſeems not abſolutely neceſſary to ſuction, that there be in the body, which is ſaid to ſuck, an endeavour, or motion in order thereto; but rather that ſuction may be reduced to truſion, and its effects aſcribed to a preſſure of the neighbouring air, upon the bodies contiguous to that which is ſaid to attract them. To object here, that ſome particles of air, remaining in the emptied cylinder, attracted this weight, to obviate a vacuum, is to no purpoſe; unleſs it can clearly be made out, by what grappling inſtruments the external air could take hold of the ſucker; how ſo little of it obtain'd the force to raiſe ſo great a weight; and why, upon letting a little more air into one of our evacuated veſſels, the attraction is much weakned. For that ſtill there remain'd in the exhauſted cylinder many little empty ſpaces, may appear by the great violence wherewith the air ruſheth in, if it be permitted to enter. In the next place, theſe experiments may teach us, what to judge of the vulgar axiom, That nature utterly abhors a vacuum; ſo that no human power is able to make one in the univerſe. For, if by a vacuum we underſtand a place perfectly free from all corporeal ſubſtance, it may be plauſibly maintain'd, that there is no ſuch thing in the world. But the generality of the pleniſts take not the word in ſo ſtrict a ſenſe. For when they alledge, that by ſucking water thro' a long pipe, the liquor, contrary to its nature, aſcends into the mouth, only to fill up that ſpace, made by the dilatation of the breaſt and lungs, which would, otherwiſe, in part, be empty; and when they tell us, that the reaſon why in a gardener's watering-pot, conically ſhaped, and filled with water, none falls thro' the numerous holes at the bottom, whilſt the orifice at the top, is clos'd; muſt be, that if, in caſe the water ſhould deſcend, the air being unable to ſucceed it, there would be left a vacuum at the upper part of the veſſel, they ſeem to mean by a vacuum, any ſpace here below, that is not fill'd with a viſible body, or, at leaſt, with air, tho' it be not quite deſtitute of all bodies whatſoever.

And vacuity.

Taking then, a vacuum in this vulgar and obvious ſenſe, the common opinion about it ſeems liable to ſeveral exceptions, whereof ſome of the chief are ſuggeſted by our engine.

It ſeems unintelligible, how hatred, or averſion, which is a paſſion of the ſoul, can either for a vacuum, or any other object, be ſuppoſed in water, or any inanimate body, which cannot be preſum'd to know when a vacuum would enſue, if they did not attempt to prevent it; nor to act contrary to what is moſt conducive to their own particular preſervation, for the good of the univerſe. The meaning, therefore, of this metaphorical expreſſion ſeems to be, that by the wiſe author of nature, the univerſe, and the parts of it, are ſo contriv'd, that it is as hard to make a vacuum in it, as if they ſtudiouſly conſpired to prevent it.

But our experiments teach, that this ſuppoſed averſion of nature to a vacuum, is merely accidental, or conſequent upon the weight, fluidity, or fluxi-

fluxility of the bodies here below; and, perhaps, principally of the spring of the air, whose constant endeavour to expand every way, makes it either rush, or compel the interposed bodies, into all spaces where it finds no greater resistance than it can surmount; and shew, that the power, exercised by nature, to avoid, or replenish a vacuum, is limited, and may be determined even to pounds and ounces *. PNEUMATICS.

And the experiment we are now upon, affords us a notable proof of the unheeded strength of the pressure sustain'd by the free air, which we presume to be uncompress'd: for hence we see, that even in our climate, and without any other compression than what is natural, or ordinary, it bears so strongly upon contiguous bodies, that a cylinder of it, not exceeding three inches in diameter, is able to raise, and carry up a weight, amounting to between sixteen and seventeen hundred ounces. In more northern countries, the air may be much thicker, and able to support a greater weight; since the *Hollanders*, who were forced to winter in *Nova Zembla*, found the air there so condens'd, that they could not make their clock go, by a very great addition to the weights that used to move it.

34. We took a dry bladder, strongly ty'd at the neck, and about half filled with air, and fastening it to one part of a very exact balance, we put a metalline counterpoise into the opposite scale; and so the two weights being brought to an equilibrium, the balance was convey'd into the receiver, and suspended from the cover of it: when we observ'd, that presently after laying on the cover, the bladder appear'd to preponderate; whereupon the scales being taken out, and reduced very near to an equilibrium, yet so, that a little advantage remain'd on that side to which the metalline weight belong'd; they were again let down into the receiver, which was presently closed. Soon after this, before the pump was work'd, the bladder seem'd again a little to preponderate; and the air in the glass beginning to be drawn out, the bladder expanded itself, and greatly raised the opposite weight, by drawing down the scale to which it was fasten'd, especially when the air had swell'd it to its full extent. This done, we, very leisurely let in the external air, and observ'd that, upon the flagging of the bladder, the scale whereto it was fasten'd, not only, by degrees, return'd to an equilibrium with the other; but, at length, was a little outweigh'd by it; tho' the bladder, after a while, began again to preponderate, and, by degrees, to sink lower for several hours: wherefore, leaving the vessel closed up all night, we, next morning, found the bladder fallen

Bodies of different specific gravities, lose their equilibrium in vacuo.

* "All the parts of space," says Sir *Isaac Newton*, "are not equally full; for if they "were, the specific gravity of the fluid, "which would fill the region of the air, "could not, by reason of the exceeding "great density of its matter, give way to "the specific gravity of quick-silver, gold, "or any body how dense soever; whence "neither gold, nor any other body, could "descend in the air. For no bodies can "descend in a fluid, unless they be "specifically heavier than it. But, if a "quantity of matter may, by rarifaction, be diminish'd in a given space, "why may it not diminish *in infinitum*? "If all the solid particles of bodies, "are of the same density, that is, have "their *vires inertiæ* as their magnitudes, "and cannot be rarified, without leaving "pores, there must be a vacuum". *Newton. Princip.* p. 368.

yet

Pneumatics. yet lower: as if the very ſubſtance of it, had imbibed ſome of the moiſture wherewith the air then abounded; as the ſtrings of muſical inſtruments, are known to ſwell ſo much in rainy weather, as to break. This conjecture is the more to be regarded, becauſe having a little warm'd the bladder, we found it lighter than the oppoſite weight. And, without removing the ſcales, or the cover of the receiver, we again caus'd the air to be drawn out; the weather continuing very moiſt; but found not any manifeſt alteration in the balance.

But to make the experiment with a body, leſs apt to be alter'd by the temperature of the air, than a bladder, we brought the ſcales again to an equilibrium with two weights, the one lead, and the other cork. And, having exhauſted the receiver, obſerv'd, that both upon the exſuction, and after the return of the air, the cork manifeſtly preponderated: and much more, a while after the air had been let in again, than whilſt it was kept out. Wherefore, for the cork, we ſubſtituted a piece of charcoal, as leſs likely to imbibe any moiſture from the air; but the event proved much the ſame; ſo that this experiment ſeems very liable to caſualties.

The aſcent of liquors in ſiphons, and filtres, whence.

35. The true cauſe of the aſcent of liquors, in ſiphons and filtres, remaining unknown; we were deſirous to try whether the preſſure of the air might reaſonably be ſuppoſed to have any conſiderable ſhare in it. But, becauſe we could not ſo far evacuate our receiver, but the remaining air would impel the water to a greater height than is uſual in filtrations; inſtead of a liſt of cotton, or the like filtre, we made uſe of a ſiphon of glaſs, conſiſting of three pieces, two ſtrait, and the third crooked, to join them together; whoſe junctures were carefully cloſed, that no air might find entrance at them: one of the legs of this ſiphon was ſomewhat longer than the other, and pervious at the bottom of it, only by a hole almoſt as ſlender as a hair, that the water might drop very gently out of it. The ſhorter leg of the ſiphon was quite open at the end, and of the ſame diameter with the reſt of the pipe; that is, about a fourth of an inch. The whole ſiphon was deſign'd to be about a foot and a half long, that the remaining air, when the veſſel was exhauſted, might not impel the water to the top of it: then the ſiphon, being inverted, was fill'd with water, and the ſhorter leg let down, two or three inches, into a glaſs-veſſel; whilſt the upper part remain'd faſten'd to the inſide of the cover of the receiver. *Fig. 38.*

And, till a conſiderable quantity of the air had been evacuated, the water dropp'd freely out at the lower end of the lower leg of the ſiphon; as if the experiment had been made in the free air: but, afterwards, the bubbles began to appear in the water; and, aſcending to the top of the ſiphon, run into one, which was gradually augmented by the riſing of other bubbles, that, from time to time, broke into it, but much more by its own dilatation, which increaſed, proportionably, as the receiver was evacuated ſo that, at length, the water, in the ſhorter leg, was reduced, by the extraction of the ambient air, and the expanſion of the great bubble, at the upper part of the ſiphon, to the height only of a foot; whence, the

courſe

courſe of the water, in the ſiphon, was interrupted, and that which remain'd in the longer leg of it, continued ſuſpended there, without dropping any longer. But, upon turning the ſtop-cock, the external air got into the ſiphon, by the little hole at which the water formerly dropp'd out: and, traverſing all the incumbent cylinder of water, in the form of bubbles, join'd itſelf with that air which before poſſeſſed the top of the ſiphon. PNEUMATICS.

To prevent the inconveniences ariſing from theſe bubbles, two glaſs-pipes, like the former, were ſo placed, as to terminate together in the midſt of the belly of a glaſs-vial, into whoſe neck they were cemented; and then both the vial, and the pipes, being filled with water, the ſiphon was placed with its ſhorter leg in the glaſs of water, as before; and the experiment being proſecuted after the ſame manner, much more air was now drawn out before the bubbles cauſed any diſturbance; becauſe there was room enough in the vial for them to ſtretch, without depreſſing the water below the ends of the pipes; and during this time, the water continued to drop out of the lower leg of the ſiphon. But, at length, the receiver being very much emptied, the water ceaſed to run thro' the ſiphon; the upper ends of the pipes beginning to appear above the remaining water in the vial, the dilated air wherein, ſeem'd likewiſe to preſs down the water in the pipes, and fill the upper part of them. *Fig.* 39.

Hence, the experiment being interrupted, we let in the air again, which, according to its various proportions of preſſure, to that of the air in the vial, and the pipes, exhibited a pleaſing variety of phenomena. And upon the whole, there ſeem'd little cauſe to doubt, if the bubbles had not diſturb'd the experiment, that the courſe of water, thro' ſiphons, would have appear'd to depend upon the preſſure of the air.

An eminent mathematician lately told me, ſome *French* gentlemen had obſerv'd, that, if one end of a ſlender open pipe of glaſs, be dipp'd in water, the liquor will aſcend to ſome height in the pipe, tho' held perpendicular to the plain of the water; and, ſoon after, brought me two or three ſmall pipes of glaſs, which gave me the opportunity of trying it: tho' I had often before, in the long and ſlender tubes of ſome weather-glaſſes, made after a peculiar manner, taken notice of the like aſcent of liquors; but, preſuming it to be caſual, I made little reflection upon it. But, after this trial, ſuppoſing that tho' the water, in theſe pipes, roſe not above a quarter of an inch; yet, if the tubes were ſlender enough, it might aſcend to a much greater height; I cauſed ſeveral of them to be dextrouſly drawn at the flame of a lamp, in one of which, that was almoſt incredibly ſlender, we found, the water aſcended five inches, tho' the pipe were held erect: but, if it were inclined, the water would fill a greater part thereof. We alſo found, that, when the inſide of the pipe was wetted before-hand, the water would riſe much better than otherwiſe. And ſome of theſe ſlender pipes, being bent, like ſiphons, we immers'd the ſhorter leg of one in a glaſs of fair water; and found, that the water, riſing to the top of the ſiphon, of itſelf, ran down the longer leg, and continued *Their aſcent in capillary tubes.*

run-

PNEUMATICS. running, like an ordinary ſiphon. The cauſe of this aſcent of the water, appears very difficult to diſcover *. We try'd, indeed, by conveying a very ſlender pipe, and a ſmall veſſel of water, into our engine, whether the exſuction of the ambient air would aſſiſt us herein; but, tho' we employ'd red wine, inſtead of water, yet we could ſcarce certainly perceive, thorough ſo much glaſs as was interpoſed betwixt our eyes and the liquor, what happen'd in a pipe ſo ſlender, that the redneſs of the wine was ſcarce viſible in it. But, as far as we could diſcern, there happen'd no great alteration to the liquor; which ſeem'd the leſs ſtrange, becauſe the ſpring of that air, which might depreſs the water in the pipe, was equally debilitated with that which remain'd to preſs upon the ſurface of the water in the little glaſs. Wherefore, in favour of that conjecture, which aſcribes this phenomenon to the greater preſſure upon the water by the air, without the pipe, than by that within, it was ſhewn, that, in caſe the little glaſs-veſſel of water were ſo cloſed, that the air might, by the mouth, be ſuck'd out of it, the water would immediately ſubſide in the ſmall pipe. Hence, we might infer, that it aſcended before, by the preſſure of the incumbent air; only it may be objected, that this, perhaps, would not happen, were the upper end of the pipe in a vacuum; as alſo, that, 'tis very probable, the water may ſubſide, not becauſe the preſſure of the internal air is taken off by ſuction, but becauſe the ſpring of the external air impels the water in its way to the cavity, deſerted by the other air; and would as well impel the ſame water upwards, as make it ſubſide, were it not for the accidental poſture of the glaſſes. 'Twere here, likewiſe, proper to inquire, why the ſurface of water, in pipes, ſhould be concave; and, on the contrary, that of quick-ſilver, convex; and why, if the end of a ſlender pipe be dipp'd in the latter, the ſurface of that fluid will be lower within the pipe, than without.

A parcel of air weigh'd. 36. We cauſed a glaſs-bubble to be blown at the flame of a lamp, about the ſize of a ſmall hen-egg, and of an oval form; only, at one end, there

* This phenomenon, the ſuſpenſion of water in capillary tubes, is, with great ſagacity, accounted for by Dr. *Jurin*; who proves it owing to the attraction of the upper periphery, or ſection, of the concave ſuperficies of the tube; that is, a ſmall ſurface, or annulus, whoſe baſe is that periphery, and height the diſtance, whereto the attractive power of the glaſs extends. For the gravity of the water that enters the orifice of the tube, upon its immerſion, being immediately taken off, by the attraction of the annulus, wherewith its upper ſurface is in contact, the water muſt neceſſarily riſe higher, by reaſon of the preſſure of the ſtagnant fluid, and the attraction of the periphery immediately above that whereto the upper ſurface of the water is already contiguous. The conſideration of this phenomenon, and the experiments made with relation to it, both in water and quick-ſilver; thoſe made with the latter, proving exactly the reverſe of the former; led the Doctor to clear the whole matter, by ſhewing, that the particles of water attract each other; that the particles of quick-ſilver attract each other; that water is attracted by glaſs; that quick-ſilver is attracted by glaſs; that the particles of water are more ſtrongly attracted by glaſs, than by one another; and, laſtly, that the particles of quick-ſilver are more ſtrongly attracted by each other, than by glaſs. See all theſe proved in the *Philoſ. Tranſ.* N° 355. p. 739.

was

was drawn out an exceeding ſlender pipe, that the bubble might be ſeal'd up, with as little rarifaction, of the air included in the great cavity, as poſſible. This glaſs, being ſeal'd, was faſten'd to one of the ſcales of an exact balance; and, being counterpois'd with a weight of lead, was convey'd into the receiver, and cloſed up in it. The beam appearing to continue horizontal, the pump was ſet on work; and, after two or three exſuctions, the balance inclined to that ſide on which the bubble hung; which, as the air was farther drawn out, preponderated more manifeſtly: at length, the air being gradually let in again, the ſcales, by degrees, return'd to an equilibrium. Then we took them out, and caſting into that ſcale, to which the lead belong'd, three fourths of a grain, we again placed them in the receiver; which, being cloſed and exhauſted as before, as the air was drawn out, ſo the glaſs-bubble came nearer to an equilibrium with the other weight, till the beam ſtood horizontal: which, by another trial, we could not bring it to do, when one fourth of a grain more was added to the ſcale whereto the lead belong'd. Tho', without doubt, if we could have perfectly evacuated the receiver, the air included in the bubble, would have weigh'd above a grain; tho' it were ſomewhat rarify'd by the flame wherewith the bubble was ſeal'd. And, upon the return of the excluded air, the lead, and the weight caſt into the ſame ſcale, did again very much preponderate.

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We, likewiſe, convey'd into the receiver, the ſame bubble, open'd at the end of the ſlender pipe above-mention'd; but, having drawn out the air as uſual, we found not, as before, the bubble to out-weigh the oppoſite lead: ſo that by the help of our engine, we can weigh the air, as we weigh other bodies, in its natural or ordinary conſiſtence, without condenſing it. Nay, having convey'd a lamb's bladder, half full of air, into the receiver, we obſerv'd, that tho' upon working the pump the impriſon'd air expanded, till it ſeem'd ready to burſt the bladder; yet this rarified air, manifeſtly depreſſed the ſcale whereto it was annexed.

Whether glaſs be pervious to air.

And, having once caus'd the pump to be obſtinately ply'd, in repeating the former experiment, the impriſon'd air broke the containing glaſs-bubble, and threw the greateſt part of it againſt the ſide of the receiver, whereby 'twas ſhatter'd into a multitude of pieces. Hence we may diſcern, of how cloſe a texture glaſs is, ſince ſo very thin a film of it, as this bubble was, prov'd ſo impervious to the air, that it could not get away thro' the pores, but was forced to break the glaſs in pieces, to free itſelf; and this, notwithſtanding the time, and advantage it had, to force thro' the pores. This I mention, that our experiments may receive no prejudice from one I happen'd to make long ſince; which might be drawn to countenance their opinion, who would perſuade us that glaſs is pervious to air, properly ſo call'd: for, in diſtilling a certain ſubſtance, greatly abounding with ſubtile ſpirits, and a volatile ſalt, in a ſtrong earthen veſſel, of an unuſual ſhape, to which was luted a large receiver of green glaſs; the fire was, by accident, ſo exceſſively increas'd, that we found the ſpirituous and ſaline corpuſcles, thrown over ſo hot, and in ſuch plenty, into the re-

PNEUMATICS. ceiver, as to render it all opake, and likely to fly in pieces. We ventur'd, however, to approach it, and obferv'd, on the outfide thereof, at a great diftance from the juncture, there was fettled a round, whitifh fpot, or two, which, at firft, we thought might be fome ftain upon the glafs; but after finding it, in feveral qualities, like the oil and falt of the concrete diftill'd, we fufpected, that the moft fubtile, and fugitive parts of the impetuous fteams, had penetrated the fubftance of the glafs, and, by the cold of the ambient air, were condenfed on the furface of it. And, indeed, upon examining the whole matter, a number of us unanimoufly concluded, that the fubtile parts of the diftill'd matter, being violently agitated by the exceffive heat, had pafs'd through the pores of the glafs made wide by the fame heat. But this having never happen'd, more than once, in any of the diftillations we have either made, or feen, it is much more reafonable to fuppofe, that the perviousnefs of our receiver, to a body much more fubtile than air, proceeded from the loofer texture of that particular parcel of metal, the receiver was made of; for all glafs is not equally compact, and folid; and from the prodigious heat, which, together with the vehement agitation of the fubtile fpirits, open'd the pores of the glafs; than to imagine, that fuch a fubftance as air, fhould be able to permeate the body of glafs, contrary to the teftimony of a thoufand chymical and mechanical experiments; and, of many made in our engine.

The penetrating power of air, compared with that of water.

And, the following experiment feems to teach, that tho' air, when fufficiently comprefs'd, may, perhaps, get entrance into fmaller cavities, than water; yet, unlefs the air be forc'd in, it will not pafs them, whilft they may admit of water. I took a glafs fiphon, the lower end of whofe longeft leg was drawn fo flender, that the orifice, at which the water was to fall out, would hardly admit a very fmall pin. This fiphon being inverted, we fo order'd it, that a little bubble of air was intercepted in the flendereft part, betwixt the little orifice, juft mention'd, and the incumbent water; whence the air, being not to be forced thro' fo narrow a paffage, by fo light a cylinder of water, as refted upon it, hinder'd the farther efflux of the water, as long as we let it ftay in that narrow place: but when, by blowing a little at the wider end of the fiphon, that fmall parcel of air was forc'd out, with fome water; the remaining water that before continued fufpended, began freely to drop down again, as before. And a glafs pipe, either in the form of a fiphon, or otherwife, half an inch in diameter, but at one end fo flender, as to terminate in an orifice almoft as fmall as a horfe-hair, be fill'd with water, it will drop down freely thorough the flender extremity. But if the pipe be inverted, the air will not eafily get in at the fmall hole, thro' which the water pafs'd. For, in the fharp end of the pipe, fome inches of water will remain fufpended; which, probably, would not happen, if the air could get in to fucceed it; fince, if the orifice were a little wider, the water would immediately fubfide. And tho' when the pipe is many inches long, a great part of the water will run down at the wider orifice; yet that feems to happen for ſome other reaſon, than becauſe the air ſucceeds it at the upper and narrow one; ſince all the ſlender part of the pipe, and, perhaps, ſome inches more, will continue full of water. PNEUMATICS.

And, tho' we have formerly ſhewn, that the aerial corpuſcles cannot paſs thro' the pores of a lamb's bladder; * yet, particles of water will; as may eaſily be try'd, by very cloſely tying a little alkaline ſalt in a fine bladder, and dipping its lower end in water: for, if it be held there for a competent time, there will ſtrain thro' the pores of the bladder, water enough to diſſolve the ſalt into a liquor.

But, to return to our bubble; we endeavour'd to meaſure its capacity by filling it with water, to find how much water anſwer'd, in weight, to $\frac{1}{4}$ of a grain of air; but all the diligence we uſed to preſerve ſo brittle a veſſel, could not prevent its breaking, before we had gain'd our point.

But, there occurs a problem, upon occaſion of the ſlow breaking of the glaſs bubble in our evacuated receiver. For, it might ſeem ſtrange, ſince the air, as we have ſeen, expands itſelf by its own internal ſpring, twice as much as *Merſennus* was able to rarify it by a red-hot æolipile; that yet, the ſpring of the air was ſcarce able to break a very thin glaſs bubble; and utterly unable to break one ſomewhat thicker, within whoſe cavity it was impriſon'd; whereas, air pen'd up, and agitated, is able to perform effects ſo much more conſiderable, that the learned jeſuit *Cabeus* tells us, he ſaw a vaſt ſtrong marble pillar quite broken off in the middle, by the heat proceeding from wood, which happen'd to be burnt juſt by it; which ſo rarified ſome air or ſpirituous matter ſhut up in the cavities of the marble, that it burſt thro' the ſolid body of the ſtone by the force of expanſion. But, probably, the reaſon why the included air did not break the ſeal'd bubbles, in our exhauſted receiver, was, that being ſomewhat rarified by the flame employ'd to ſeal the glaſs, its ſpring upon the receſs of the heat grew weaker than before. Yet, this will not, alone, ſerve the turn, becauſe, much ſmaller glaſs bubbles, exactly cloſ'd, will by the included air be made to fly in pieces.

The proportion of the weight of air to that of water.

We took an æolipile of copper, weighing ſix ounces, five drams, and forty-eight grains; and being made hot, we remov'd it from the fire, and immediately ſtop'd it with hard wax, that no air might get in at its orifice. Then the æolipile, being ſuffer'd leiſurely to cool, 'twas again weigh'd, together with the wax, and found to be ſix ounces, ſix drams, and thirty-nine grains. Laſtly, the wax being perforated, without taking any of it out of the ſcale, the external air was ſuffer'd to ruſh in; and then the æo-

* M. *Homberg* is of opinion, that water enters ſuch narrow pores of animal ſubſtances, as will not admit the air, only becauſe it moiſtens and diſſolves the glutinous matter of the fine fibres of the membranes, and alſo renders them more pliable and diſtractile; which are things, that the air, for want of a wetting property, cannot do. As a proof of this doctrine, he fill'd a bladder with air, and compreſs'd it with a ſtone, and found no air to come out; but, placing the bladder, thus compreſs'd, in water, that air eaſily eſcaped. *Hiſt. de l'Acad.* A. 1700. p. 17.

PNEUMATICS. lipile, and wax, being again weigh'd, amounted to ſix ounces, ſix drams, and fifty grains. So that the æolipile, freed as far as our fire could free it, from its air, weigh'd leſs than when repleniſh'd with air, full eleven grains; that is, the air containable within the cavity of the æolipile, amounted to eleven grains, and ſomewhat more. And, by the way, if there be no miſtake in the obſervation of *Merſennus*, it may ſeem ſtrange that it ſhould ſo much differ from two or three of ours; in none of which we could rarify the air in an æolipile, though made red-hot, almoſt all over, and immediately plung'd into cold water, to half that degree which he mentions, *viz.* ſeventy times its natural extent; unleſs the æolipile, he employ'd, was able to ſuſtain a more vehement heat than ours *.

This way of weighing the air, by the help of an æolipile, ſeems ſomewhat more exact, than that which *Merſennus* uſed, becauſe we weigh'd not the æolipile till it was cold; whereas he weigh'd it red-hot, whereby it is ſubject to loſe of its weight in cooling: for, copper heated red-hot, throws off, in the cooling, little thin ſcales in ſuch plenty, that, having purpoſely watch'd a copper æolipile, during its refrigeration, we have ſeen the place round about it, almoſt cover'd with them every way. Perhaps, too, the æolipile, in cooling, may not receive ſome little increaſe of weight, either from the vapid, or ſaline ſteams that float in the air. We employ'd, to weigh our æolipile, both when fill'd with air, and when repleniſh'd with water, a pair of ſcales that would turn with the fourth part of a grain. As to the proportion of weight betwixt air and water, ſome learned men have attempted to ſettle it, by ways ſo inaccurate, that they ſeem to have been much miſtaken. *Ricciolus* having purpoſely endeavour'd to diſcover this proportion, by means of a thin bladder, eſtimates the weight of the air, to that of the water, as about 1 to 10,000; and, indeed, having once weigh'd a large bladder, full of air, and found it to contain 14 grains; the ſame bladder, afterwards fill'd with water, contain'd near 14 pounds; whence the proportion of air to water, ſeem'd, almoſt, as a grain to a pound, that is, as 1 to above 7600. On the other hand, *Galileo* makes the air to water, as 1 to 400. But our way of weighing the air by an æolipile, ſeems, by much, the more exact. And, according to our obſervations, the water it contain'd, amounting to 21 ounces and a half; and as much air as was requiſite to fill it, weighing eleven grains; the proportion in gravity of air, to water of the ſame bulk, will be as 1 to 938. And tho' we could not fill the æolipile with water, very exactly; yet, as we neither could perfectly drive the air out of it

* It may be pretended, that 'tis not the air, but ſome vapour, or exhalation, contain'd in it, that here weighs upon the balance. To obviate this objection, M. *Muſchenbroeck* contrived the following experiment. 'Tis a known thing in chymiſtry, that dry alkaline ſalts attract, and abſorb the moiſture of the air, and thereby run, *per deliquium*, as 'tis called. That philoſopher, therefore, having exhauſted a proper veſſel of its air, fitted another veſſel, wherein was lodg'd a large quantity of very dry ſalt of tartar, reduced to fine powder, and made hot, to the neck of the former; ſo that the external air muſt paſs ſlowly thro' this ſalt, before it could poſſibly get into the exhauſted veſſel; whereby the air that entred, was ſtrained, and perfectly freed from any moiſture that might have been lodged therein. The veſſel being thus fill'd with pure air, and put into the ſcale, was found to weigh as much as when fill'd with unpurged air. *De Mater. ſubtil.* p. 7.

by

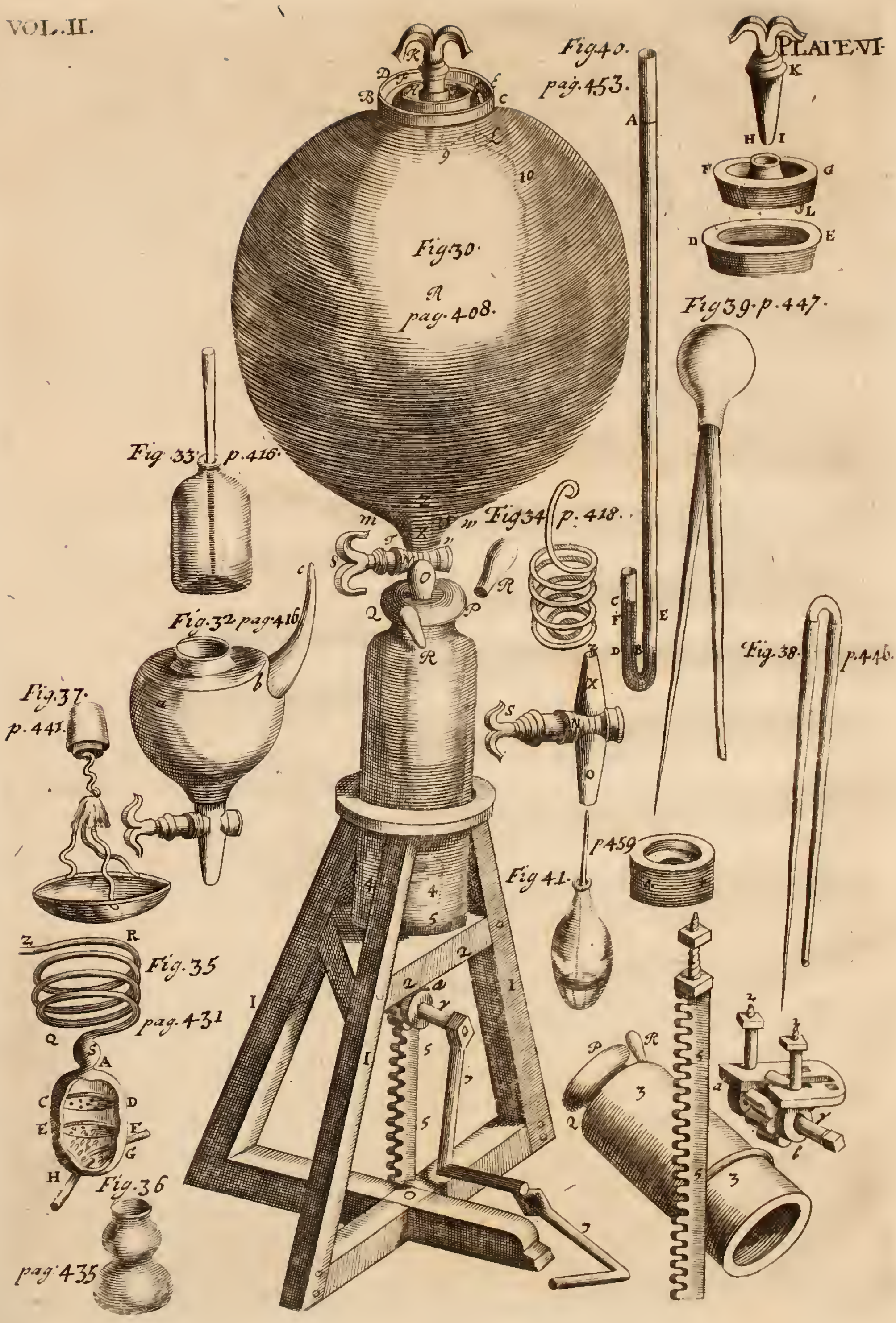
Fig. 30.
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Fig. 40.
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Fig. 39. p. 447.
Fig. 33. p. 416.
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by heat, we think the proportion may hold good: however, in a round ſum we may ſay, water is near 1000 times heavier than air. And accordingly, having, at another time, put ſome water in the æolipile, before we ſet it on the fire, that the vapours of the rarified liquor might the better drive out the air, we found, upon trial carefully made, that when the æolipile was refrigerated, and the included vapours, by the cold, turn'd again into water; the air being let in, increas'd the weight of the æolipile, eleven grains, as before; tho' there were already in it, twelve drams, and 32 grains of water, which remain'd of that we had put into it, to drive out the air. *Merſennus*, indeed, tells us, that, by his account, air is in weight to water, as 1 to 1356; and adds, that we may, without any danger, ſuppoſe, the gravity of water to that of air of a like bulk, as 1300 to 1; and, conſequently, that a quantity of air, to a quantity of water, equiponderant thereto, is as 1300 to 1. But why we ſhould relinquiſh our own carefully repeated trials, I ſee not; yet I am unwilling to reject thoſe of ſo accurate and uſeful a writer; and therefore ſuſpect, that the difference in our obſervations, proceeds from the different conſiſtence of the air at *London*, and at *Paris*: for, our air being more cold and moiſt than theirs, may be ſuppoſed, alſo, to be a fourth, or a fifth part heavier. Perhaps it may be of moment, too, that our obſervations were made in the midſt of winter, whilſt his might be made in ſome warmer part of the year.

The height of the atmoſphere computed.

It might be expected, that we ſhould, from theſe and other obſervations, decide the controverſy about the height of the atmoſphere; but, tho' it ſeems eaſy to ſhew that many famous writers have been miſtaken in aſſigning this height; yet, 'tis very difficult, preciſely to define its extent.

Now, we have, already made it appear, that at leaſt about *London*, the proportion of ſpecific gravity betwixt water and air, is, as 1000 to 1. And, to determine the difference in weight betwixt water and quickſilver; we took a glaſs-pipe, in the form of an inverted ſiphon, and pouring into it a quantity of quick-ſilver, we held it ſo, that the ſuperficies of the liquor, both in the longer and ſhorter leg, lay in the horizontal line E F; then pouring water into the longer leg of the ſiphon, till that was almoſt fill'd, we obſerv'd the ſurface of the quick-ſilver in that leg to be, by the weight of the water, depreſs'd from E, to B, and in the ſhorter leg, to be as much impell'd upwards from F, to C. And having, before-hand, made marks, as well at the point B, as at the oppoſite point D, we meaſur'd both the diſtance D C, to have the height of the cylinder of quick-ſilver, which was rais'd above the point D, by the weight of the water, and the diſtance B A, which gave us the height of the cylinder of water. So that the diſtance D C, being $2\frac{13}{52}$ inches, and the height of the water $30\frac{45}{54}$ inches, the proportion appear'd to be as 121 to 1665, or as 1 to $\frac{92}{121}$. *Fig.* 40.

We alſo meaſured the proportion betwixt quick-ſilver and water by the help of a balance, which would turn with the hundredth part of a grain. But, becauſe an over-ſight is uſually committed in weighing quick-ſilver, and water; eſpecially, if the orifice of the containing veſſel be wide; ſince the

the ſurface of water in veſſels, will be concave, but that of quick-ſilver conſiderably convex; to avoid this inconvenience, we made uſe of a glaſs-bubble, blown very thin, that it might not be too heavy for the balance, and terminating in a very ſlender neck, wherein the concavity or convexity of a liquor could not be conſiderable. This glaſs weighing $23\frac{1}{2}$ grains, we almoſt fill'd it with quick-ſilver, and faſtning a mark over againſt the middle of the protuberant ſuperficies, we found that the quick-ſilver alone, weigh'd $299\frac{7}{12}$ grains; then the quick-ſilver being pour'd out, and the ſame glaſs fill'd to the ſame height with common water, we found it to weigh $21\frac{7}{8}$ grains: whereby, it appear'd, that the weight of water to quick-ſilver, is, as 1 to $13\frac{19}{28}$; tho' the illuſtrious *Verulam*, merely for want of exact inſtruments, makes the proportion betwixt them greater than of 1 to 17. And, by the way, ſince quick-ſilver, and well rectified ſpirit of wine are accounted, one the heavieſt, and the other the lighteſt of fluids; with the ſame glaſs, and ſcales, we found, the difference betwixt them, to be, as 1 and $16\frac{641}{1084}$; whence, the difference betwixt highly rectified ſpirit of wine, and common water, is, as betwixt 1 and $1\frac{44}{71}$. But to avoid fractions, let us ſuppoſe quick-ſilver is fourteen times as heavy as water. We have then given us, the proportion of air to water, and of water to quick-ſilver; from whence, it is eaſy to find the proportion betwixt air and quick-ſilver; if we ſuppoſe the atmoſphere to be uniformly of ſuch a conſiſtence as here below. For, ſince our engine hath ſufficiently manifeſted, that 'tis the equilibrium with the external air, that, in the *Torricellian* experiment, ſuſtains the quick-ſilver; and, ſince by our accurate experiment, formerly mention'd, a cylinder of mercury, able to balance a cylinder of the whole atmoſphere, amounted to about thirty inches; and, ſince, conſequently, we may aſſume the proportion of quick-ſilver to air, to be as 14000 to 1; it will follow, that a cylinder of air, able to maintain an equilibrium with a mercurial cylinder of two feet and a half in height, muſt amount to 35000 *Engliſh* feet, and conſequently to ſeven miles.

But we cannot ſafely conclude, that the air is every where of the ſame conſiſtence we found it, near the ſurface of the earth; not only becauſe, as *Seneca* ſays, "the air is more groſs, the nearer it comes to the earth, as the feces fall to the bottom in water"; but becauſe the ſpringy texture of the aerial corpuſcles makes them capable of a very great compreſſure, which the weight of the incumbent part of the atmoſphere exerts upon the undermoſt, near the ſurface of the earth. And as we have ſeen, that air, much rarify'd without heat, may eaſily admit a farther rarifaction with it; and that, even without being expanded thereby, it is capable of being rarify'd to above a hundred and fifty times the extent it uſually poſſeſſeth here below; perhaps the atmoſphere may riſe to the height of ſome hundred miles: nay, exhalations may aſcend much higher, if there was no miſtake in that ſtrange obſervation made at *Toulouſe*, in a clear night in *Auguſt*, by the diligent *Emanuel Magnan*; who, as *Ricciolus* tells us, "ſaw, "from eleven a-clock at night, till twelve, while the moon was under the

"horizon,

" horizon, a little lucid cloud, near the meridian, and almoſt in the zenith, which could be illumin'd by nothing but the ſun; and, therefore, " muſt have been higher than the whole ſhadow of the earth. And," ſays *Ricciolus*, " the like phenomenon was obſerv'd by the great mathematician " *Riccius*."

Various obſervations, made at the feet, tops, and interjacent parts of high mountains, might, perhaps, aſſiſt us to make an eſtimate, in what proportion the higher air is thicker than the lower; and to gueſs at the different conſiſtence, as to laxity and compactneſs, of the air, at ſeveral diſtances from us. And, if the difficulties about the refractions of the celeſtial luminaries were ſatisfactorily determined; that might, alſo, conduce to aſſign proper limits to the atmoſphere. But, at preſent, we dare not pronounce any thing, peremptorily, concerning the height of it.

Odd phenomena of light produced in the receiver of the air-pump.

37. We have often obſerv'd, that, when the ſucker of our pump was drawn down, immediately upon turning the key, there appear'd a kind of light in the receiver, almoſt like a faint flaſh of lightning, in the day-time; and almoſt as ſuddenly did it appear, and vaniſh. When we firſt took notice of this phenomenon, the day was clear, the hour about ten in the morning, and the only window in the room faced the north; and we found that, by interpoſing any opake body between the receiver, and the window, tho' the reſt of the room were ſufficiently enlighten'd, yet the flaſhes did not appear as before. As ſoon as night was come, we made the room very dark, and plying the pump, as in the morning, could not find, upon turning the key, the leaſt glimmering of light. Whence we inferr'd, that the flaſh, appearing in the receiver, did not proceed from any new light, generated there; but from ſome reflections of the light of the ſun, or other luminous bodies, placed without: tho', whence the reflection ſhould happen, was hard to ſay.

Wherefore, the next morning we went about to repeat the experiment; but tho' we could, as well as formerly, exhauſt the receiver; tho' the place wherein we made the trial, was the very ſame; and tho' other circumſtances correſponded; yet we could not diſcover the leaſt appearance of light all that day, no more than on ſeveral others: nor can we, to this very time, be ſure, a day before-hand, that theſe flaſhes will appear in our great receiver. Nay, having once found the engine diſpoſed to exhibit this phenomenon, we ſent notice of it to Dr. *Wallis*, who was then very near at hand, and made haſte to ſatisfy his curioſity; yet, by that time he arrived, the appearance was ceaſed: and having long, in vain, endeavour'd to exhibit it again, we were, after all, unexpectedly preſented with a few flaſhes.

And this contingency, whereto our experiment is liable, being ſuch, that, in all conſtitutions of the weather, times of the day, *&c.* it will ſometimes anſwer, and ſometimes diſappoint our expectations; we are much diſcouraged from framing an hypotheſis to ſolve it; tho' it might be attempted from conſidering the following phenomena. (1.) The appearance may as well be exhibited by candle-light, as by day-light, and in

what-

whatever poſition the candle be held to the receiver, provided the rays of light be not hinder'd from falling upon the veſſel. (2.) The flaſh appears immediately upon turning the key, to let the air out of the receiver into the empty'd cylinder; ſo that, I remember not, that the flaſh appear'd, when at any time, in our great receiver, the ſtop-cock was open'd, before the cylinder was exhauſted. (3.) When, inſtead of the great receiver, we made uſe of a ſmall glaſs, not containing above a pound and a half of water; the phenomenon might be exhibited, tho' the ſtop-cock remain'd open, provided the ſucker was drawn nimbly down. (4.) When we began to empty the veſſel, the appearances of light were much more conſpicuous, than towards the latter end, when little air, at a time, could paſs out of the receiver. (5.) When the ſucker had lately been well oil'd, and, inſtead of the great receiver, the ſmaller veſſel, above-mention'd, was emptied; upon opening the ſtop-cock, as the air deſcended out of the glaſs into the emptied cylinder, there aſcended out of the cylinder into the veſſel, a certain ſteam, which ſeem'd to conſiſt of very little bubbles, or other minute corpuſcles, thrown up from the oil, rarify'd by the attrition it ſuffer'd in the cylinder. For, at the ſame time that theſe ſteams aſcended into the glaſs, ſome of the ſame kind manifeſtly iſſued out, like a little pillar of ſmoke, at the orifice of the valve, when that was occaſionally open'd. And theſe ſteams, frequently preſenting themſelves to our view, we found, by expoſing the glaſs to a clear light, that they play'd up and down in it; and, by their whitiſhneſs, in ſome meaſure reſembled the appearance of light. (6.) For, when the flaſh was great, the receiver, at the very inſtant, loſt of its tranſparency, by appearing full of ſome kind of whitiſh ſubſtance; and, for a ſhort time after, the ſides of the glaſs continu'd opake, and ſeem'd to be darken'd, as if ſome whitiſh ſteam adhered to the inſide of it.

But he who would fairly account for the phenomenon, whereof theſe are not all the circumſtances, muſt ſhew from whence the apparent whiteneſs proceeds; and why that whiteneſs ſometimes appears, and ſometimes not. Now, had our phenomenon been conſtant, and uniform, we ſhould ſuſpect it to have been produced after the following manner; for tho' what we ſaw in our receiver, ſeem'd to be a kind of light, yet it was, indeed, but a whiteneſs, which render'd the inſide of the glaſs opake.

Now our common air abounds with particles, able to reflect the rays of light, as appears from that vulgar obſervation, the motes in the air, when the ſun-beams ſhooting into any ſhady place, diſcover them, tho', otherwiſe, the eye cannot diſtinguiſh them from the air. And, I particularly remember, that being at ſome diſtance from *London*, at a time when numerous bonfires happen'd to be made there; tho' we could not ſee the fires themſelves, yet we could plainly perceive the air all enlightned near the city: which argued, that the rays, ſhot upwards from the fires, met, in the air, with corpuſcles opake enough to reflect them to our eyes.

White

White may be produc'd, when the continuity of a tranſparent body happens to be interrupted by a great number of ſurfaces, which, like ſo many little looking-glaſſes, confuſedly repreſent a multitude of ſmall and ſeemingly contiguous images of the lucid body. For, water, or the whites of eggs, beaten to a froth, loſe their tranſparency, and appear white. And, having, out of one of our ſmall receivers, carefully drawn out the air, and left a very little hole, by which the water was to get in, we obſerv'd that the neck, being held under water, and the little hole open'd, the water that ruſh'd in, was ſo broken, and acquired ſuch a multitude of new ſurfaces, that the receiver ſeem'd to be full rather of milk, than water. And farther, by heating a lump of cryſtal, and quenching it in fair water, it will be diſcontinued by ſuch a multitude of cracks, which create new ſurfaces within it, that tho' it will not fall aſunder, yet it loſes its tranſparency, and appears white.

Hence we might imagine, that upon the ruſhing of the air out of the receiver, into the empty'd cylinder, the air in the receiver, being ſuddenly, and vehemently expanded the texture of it was as ſuddenly alter'd; and the parts made ſo to ſhift places, and, perhaps, ſome of them, to change poſtures, as during their new and vehement motion, and their varied ſituation, to diſturb the uſual continuity, and, thereby, the tranſparency of the air; which ceaſing to be a tranſparent body, muſt eaſily degenerate into white.

Several things there are which make this conjecture ſeem the more probable; as, firſt, the whiteneſs always appear'd greater, whilſt there was much air in the receiver, than when the air was in great part drawn out. Secondly, having exhauſted the receiver, and applied to the hole in the ſtop-cock, a large bubble of clear glaſs; ſo that we could, at pleaſure, let the air paſs out, at the ſmall glaſs, into the great one, and eaſily fill the ſmall one with air again; we obſerv'd, that upon opening the communication betwixt the two glaſſes, the air, in the ſmaller, finding ſo much room in the greater, to receive it, flew out with ſuch force, that the ſmall vial ſeem'd to be full of milk: and this experiment we repeated ſeveral times. And, thirdly, having provided a ſmall receiver, with its upper orifice ſo narrow, that I could ſtop with my thumb, I obſerv'd, that when, upon the exſuction of the air, the capacity of the glaſs appear'd white; if, by a ſudden removal of my thumb, I let in the outward air, that whiteneſs would immediately vaniſh. It may, indeed, be objected, that when water turns from tranſparent to white, the air intervenes, which converts it into bubbles. To this I reply, there are two very volatile liquors, which being gently put together, are as clear as rock-water, and yet will inſtantly, without the help of air to turn them into bubbles, ſo alter the diſpoſition of their inſenſible parts, as to become a white conſiſtent body. And this happens not as in the precipitation of benjamin, and ſome other reſinous ſubſtances; which being diſſolv'd in ſpirit of wine, may, by the affuſion of fair water, be turn'd into a milky ſubſtance: for this whiteneſs belongs not to the whole liquor, but to the corpuſ-

PNEUMATICS. cles of the diſſolved gum, which, after a while ſubſiding, leave the liquor tranſparent, themſelves only remaining white. But, in our caſe, 'tis from the varied texture of the whole tranſparent fluid; and not from any particular part, that this whiteneſs reſults: for the body is white throughout, and will long continue ſo; and yet may, in proceſs of time, without any addition, be totally reduced into a tranſparent body, as before.

Another conjecture, we grounded upon this obſervation: having convey'd ſome ſmoke into our receiver, placed againſt a window, we obſerv'd, that, upon the exſuction of the air, the corpuſcles floating in it, manifeſtly enough made the receiver ſeem more opake, at the very inſtant the air ruſhed out. For, conſidering that the whiteneſs, whoſe cauſe we enquire after, did but ſometimes appear, it ſeem'd not impoſſible, that, at ſuch times, the air in the receiver, might abound with particles capable of reflecting the light, in the manner requiſite to exhibit a white colour, by being put into a certain unuſual motion; as the new motion of their former fumes, made the inſide of the receiver appear darker than before; and as our ſmoking liquor, formerly mention'd, whoſe parts, tho' they ſeem'd tranſparent, whilſt they compos'd a fluid; yet when the ſame corpuſcles, upon unſtopping the glaſs, were put into a new motion, and diſpos'd after a new manner, they render'd that part of the air opake, wherein they mov'd, and exhibited a greater whiteneſs than ſometimes appears in our receiver.

But as to the reaſon why our phenomenon appears not conſtantly, I remember not that we ever made the experiment in a ſmall veſſel, without finding the expected whiteneſs. But it remains to be explain'd, why in our great receiver, the phenomenon ſhould ſometimes be ſeen, and often not. All I have to ſay on this head is, that the air about us, and much more that within the receiver, may be much alter'd by ſuch caſes, as few are aware of. The learned *Joſephus Acoſta* tells us, that "in *America* there "are winds which naturally trouble the water of the ſea, making it green "and black, and others as clear as cryſtal." And, tho' we convey'd into the receiver, the ſcales and the pendulums, formerly mention'd, clear and bright; yet, after the veſſel had been emptied, and the air let in again, the luſtre of both appear'd tarniſh'd by a beginning ruſt. And, laſtly, having, with pure ſpirit of wine, drawn a tranſparent tincture out of a certain concrete, commonly reckon'd among minerals, we put it into a cryſtal-vial, carefully ſtop'd it, and lock'd it up in a preſs; and this liquor, being a chymical rarity, and of a pleaſing golden colour, we had often occaſion to view it; and took notice that once it ſeem'd to be very thick: whereupon, we imagin'd it poſſible, that ſome of the mineral corpuſcles were then precipitating. But finding, after ſome days, that tho' no precipitation had been made, and that the liquor, retaining its former vivid colour, was grown clear again, as before; we lock'd it up again in the ſame preſs, and reſolv'd to obſerve whether the like changes would again appear in our tincture; and, in caſe they ſhould, whether they might be aſcribed to the alterations of the weather. But tho' during the greateſt part of a winter,

winter, and a ſpring, we obſerv'd the liquor would often grow turbid; and, after a while, clear again; yet we could not find, that it depended upon any manifeſt changes in the air; which would be often dark and cloudy, when the tincture was clear and tranſparent; as, in clear weather, the liquor would, ſometimes, appear troubled, and more opake. PNEUMATICS.

38. Into a glaſs vial, open at the top, we put a mixture of ſnow, and common ſalt; and, in the midſt of this mixture, ſet a cylindrical glaſs, cloſely ſtopp'd at the lower-end, and open at the upper, where we fill'd it with common water; then let them all down into the receiver; and the pump being ſet on work, the ſnow began to melt faſter than we expected. However, by that time the receiver had been conſiderably exhauſted, which it was in leſs than a quarter of an hour, we perceiv'd the water, near the bottom of the glaſs cylinder, to freeze; and the ice, by a little longer ſtay, ſeem'd to increaſe, and to riſe ſomewhat higher than the ſurrounding ſurface of the liquor whereinto, almoſt all the ſnow and ſalt were diſſolved. The glaſs being taken out, it appear'd that the ice was as thick as the inſide of the veſſel it fill'd; tho', into that, I could put my thumb. The upper ſurface of the ice was very concave, and, held againſt the light, appear'd not deſtitute of bubbles; tho' they were fewer than if the water had been frozen in the open air. The like experiment we made, alſo, in one of our ſmall receivers, with like ſucceſs. *Water made to freeze* in vacuo.

But, whence proceeds that ſtrange force, we may ſometimes obſerve in frozen water, to break the bodies that impriſon it, tho' hard and ſolid? A ſtone-cutter, lately complain'd to me, that, ſometimes, thro' the negligence of his ſervants, the rain being ſuffer'd to ſoak into marble, the violent froſts coming on, would burſt the ſtones. And, another tradeſman complain'd, that, even implements made of bell-metal, being careleſsly expos'd to the wet, have been broken and ſpoil'd by the water, which, having enter'd at the little cavities of the metal, was there, afterwards, froze, and expanded into ice. And *Cabeus* tells us, that he ſaw a huge veſſel of exceeding hard marble ſplit aſunder, by congeal'd water. I know it will be ſaid, to ſolve this problem, that congelation doth not reduce water into leſs ſpace, than it before poſſeſs'd, but, rather makes it take up more. But, tho' we grant, that water ſwells in freezing; yet how cold, which, in weather-glaſſes, manifeſtly condenſeth air, ſhould expand either the water or the intercepted air, ſo forcibly as to perform what we have here related, remains to be diſcover'd.

39. We took an oval glaſs, clear, and pretty ſtrong, with a ſhort neck at the obtuſer end, thro' which we thruſt, almoſt to the bottom, a pipe of glaſs, and cloſely cemented it to the neck: the upper part of the pipe was drawn, in ſome parts, more ſlender than a crow's quill, that the changes of the air in the glaſs-egg, might be the more conſpicuous; then we convey'd into the glaſs, five or ſix ſpoonfuls of water, part of which, by blowing air into the egg, was rais'd into the ſlender part of the pipe; ſo that the water was interpos'd between the external air, and that included in the egg. This weather-glaſs, was ſo placed, and cloſ'd up *A water-thermometer* in vacuo. *Fig. 42.*

PNEUMATICS. up in the cavity of a ſmall receiver, that only the ſlender part of the pipe, to the height of four or five inches, paſſing thro' a hole in the cover, remain'd expos'd to the open air.

In evacuating the receiver, the water, in the pipe, deſcended about a quarter of an inch; and this upon two or three repeated trials; which ſeem'd to argue, that there was no heat produced in the receiver, upon the exſuction of the air: for even a little heat would, probably, have been diſcover'd by that weather-glaſs; ſince, by the bare application of my hand to the outſide of the receiver, the warmth, after ſome time, having been propagated thro' both the glaſſes, and the interval betwixt them, to the impriſon'd air, ſo rarify'd it, that, by preſſing upon the ſubjacent water, it impell'd that in the pipe much higher than it had fallen downwards, upon the exſuction of the air.

Yet we do not hence conclude, that in the cavity of the receiver the cold was greater after the extraction of the air, than before.

If it be demanded, what then could cauſe the water to ſubſide; we anſwer, that, probably, it was the ſtretching of the glaſs-egg, which, upon the exſuction of the ambient air, was unable to reſiſt, as formerly, the preſſure of the included air, and of the atmoſphere, which, by the intervention of the water, preſs'd upon its concave ſurface. This ſeems probable, as well from the experiment about breaking a glaſs, by the force of the atmoſphere, as becauſe, when by drawing the air out of the receiver, the water, in the pipe, was ſubſided, upon the re-admiſſion of the external air, to preſs againſt the convex ſurface of the egg, the water was preſently re-impell'd to its former height: for, if a glaſs-egg be blown exceeding thin, and afterwards broken, you may, by degrees, conſiderably bend ſome narrow parts of it; and upon the removal of what kept it bent, it will readily recover its former ſtate. From our experiment, then, it appears either that there ſucceeds no body in the room of the air drawn out of the receiver; or, that every ſubſtance is not ſubtile enough, readily to paſs the pores of glaſs, tho' always ſufficiently agitated to produce heat, wherever it is found in plenty. So that if we admit no vacuum, this experiment requires us to allow a great diſparity, either as to bulk, or agitation, or both, betwixt ſome parts of the ætherial ſubſtance, and thoſe which, here below, produce heat and fire.

We try'd, alſo, what operation the extraction of the air would have upon camphire; which conſiſts of ſuch volatile parts, that they will exhale without any greater agitation, than that of the open air. But we found not, that even this looſe body, was ſenſibly alter'd thereby.

Inſects in vacuo. 40. We convey'd a large fleſh-fly into a ſmall receiver; and, at another time, ſhut into a great receiver, a humming-bee, that appear'd ſtrong and lively; we alſo procur'd a white butter-fly, and incloſ'd it in a ſmall receiver; where, though at firſt, he flutter'd about, yet, preſently, upon the exſuction of the air, he fell down, as in a ſwoon; retaining no other motion, than ſome little trembling of the wings. The fly, after ſome exſuctions of the air, drop'd down from the ſide of the glaſs, whereon ſhe

was

was walking: but, that the experiment of the bee might be more inſtructive, we convey'd in with her a bundle of flowers, which remain'd ſuſpended by a ſtring, near the upper-part of the receiver; and having provoked the bee, we excited her to fly up and down the veſſel, till, at length, ſhe lighted upon the flowers, when we preſently began to draw out the air, and obſerv'd, that tho', for ſome time, ſhe ſeem'd to take no notice of it, yet, within a while after, ſhe fell down from the flowers, without making any uſe of her wings.

41. To ſatisfy ourſelves, in ſome meaſure, why reſpiration is ſo neceſſary to the animals, that nature hath furniſh'd with lungs, we took a lark, one of whoſe wings had been broken by a ſhot; but, notwithſtanding this hurt, the bird was very lively; and put her into the receiver, wherein ſhe, ſeveral times, ſprung up to a conſiderable height. The veſſel being carefully cloſed, the pump was diligently ply'd, and the bird, for a while, appear'd lively enough; but, upon a greater exſuction of the air, ſhe began manifeſtly to droop, and appear ſick; and, very ſoon after, was taken with as violent, and irregular convulſions, as are obſerv'd in poultry, when their heads are wrung off, and died; (tho' when theſe convulſions appear'd, we let in the air,) with her breaſt upward, her head downward, and her neck awry; and this within ten minutes, part of which time had been employ'd in cementing the cover to the receiver. Soon after we put a lively hen-ſparrow, which was not at all hurt, into the receiver; and proſecuting the experiment, as with the former, ſhe appear'd to be dead within ſeven minutes; one of which was employ'd in cementing on the cover: but, upon ſuddenly turning the key, the freſh air, flowing in, began ſlowly to revive her; ſo that, after ſome pantings, ſhe open'd her eyes, and regain'd her feet, and, in about a quarter of an hour after, attempted to eſcape at the top of the glaſs, which had been unſtop'd to let in the air upon her: but the receiver being cloſed the ſecond time, ſhe died, violently convuls'd, within five minutes from the firſt ſtroke of the pump.

Birds and mice in the exhauſted receiver.

Then we put in a mouſe, newly caught, and, whilſt he was leaping up very high in the receiver, we faſten'd the cover to it; expecting, that an animal, uſed to live with very little freſh air, would endure the want of it better than the birds; but tho', for a while after the pump was ſet on work, he continu'd leaping up, as before; yet 'twas not long e'er he began to appear ſick, giddy, and to ſtagger; after which, he fell down as dead, but without ſuch violent convulſions as the birds had: when, haſtily letting in ſome freſh air upon him, he recover'd his ſenſes, and his feet, but ſeem'd to continue weak and ſick; at length, growing able to skip, as formerly, the pump was ply'd again, for eight minutes; about the middle of which ſpace, a very little air, by miſchance, got in at the ſtop-cock; and, about two minutes after that, the mouſe, ſeveral times, leap'd up lively; tho', in two minutes more, he fell down quite dead; yet with convulſions far milder than thoſe wherewith the birds expired. This alacrity, ſo little before his death, and his not dying ſooner

PNEUMATICS. than at the end of the eighth minute, ſeem'd owing to the air that paſs'd into the receiver: for, the firſt time, the convulſions ſeiz'd him, in ſix minutes after the pump began to be work'd. Theſe experiments ſeem'd the more ſtrange, becauſe, during a great part of thoſe few minutes, the engine could but conſiderably rarify the air, and that too by degrees; and, at the end thereof, there remain'd in the receiver, a large quantity: for, as we formerly ſaid, we could not draw down water in a tube, within much leſs than a foot of the bottom. And, by the exſuction of the air, and interſperſed vacuities, there was left in the receiver, a ſpace ſome hundreds of times exceeding the magnitude of the animal, to receive the fuliginous ſteams, from which, expiration diſcharges the lungs, and which, in the other caſes, may be ſuſpected, for want of room to ſtifle thoſe animals that are cloſely pent up in too narrow receptacles.

Having cauſed theſe three creatures to be open'd, I could diſcover little of what we ſought for, and might, poſſibly, have found in larger animals: for tho' the lungs of the birds appear'd very red, and, as it were, inflamed; yet that colour is uſual in the lungs of ſuch winged animals: but in almoſt all the deſtructive experiments, made in our engine, the animals appear'd to die with violent convulſive motions. From whence, whether phyſicians can deduce any thing towards the diſcovery of the nature of convulſive diſtempers, I leave to them to conſider.

And, to obviate objections, and remove ſcruples, about the fuliginous ſteams of pent up animals, which are ſuppoſed to kill them; we ſhut up another mouſe, as cloſe as poſſible, in the receiver, where it liv'd about three quarters of an hour; and might, probably, have done ſo, much longer, had not a perſon of quality deſired to ſee whether the mouſe could be kill'd by the exſuction of the ambient air. Upon this, we open'd, for a while, an intercourſe betwixt the air in the receiver, and that without, whereby the mouſe might be refreſhed, tho' without uncementing the cover at the top; to avoid the objection that, perhaps, the veſſel was more cloſely ſtopp'd for the exſuction of the air than before.

The event was, that, after the mouſe had liv'd ten minutes, the pump being a little out of order, he died with convulſive motions; wherein he made two or three bounds into the air, before he fell down dead.

I, alſo, cauſed a mouſe, that was very hungry, to be ſhut up all night into a well-cloſed receiver, with a bed of paper for him to reſt on; and caus'd the engine to be placed by the fire-ſide, to keep him from being deſtroy'd by the immoderate cold of a froſty night; and, the next morning, I found he had devour'd a large part of the cheeſe that had been put up with him. And, having thus kept him alive full twelve hours, we, by ſucking out part of the air, brought him to droop, and to appear ſwell'd; but, by letting it in again, we ſoon reduced him to his former livelineſs.

The nature of reſpiration conſider'd.

It may be here expected, I ſhould attempt to clear the nature of reſpiration; but I pretend to go no farther in it, than our engine leads me.

'Tis alledged by thoſe who would have the lungs rather paſſive than active, in reſpiration, that as the lungs, being deſtitute of muſcles and fibres,

fibres, are unfit to dilate themſelves; ſo, without the motion of the thorax, they would not be fill'd with air: ſince, as Dr. *Highmore* hath well obſerv'd, if a live dog have a great wound made in his cheſt, the lobes of the lungs, on that ſide of the mediaſtinum, will collapſe, and lie ſtill; whilſt the thorax, and the lobes on the other ſide of the mediaſtinum, continue their former motion. And if, at once, the muſcles of the cheſt be on both ſides diſſected; upon the ingreſs of the air, the whole lungs, tho' untouch'd, will remain without motion, at leaſt as to any expanſion, or contraction of their ſubſtance.

And *Bartholine* affirms, that if the diaphragm be wounded, the lungs will fall together, and reſpiration ceaſe; which appears to be true, provided the wound be large. And, indeed, the diaphragm ſeems the principal inſtrument of ordinary reſpiration; tho' the intercoſtal muſcles, and, perhaps, ſome others, may be allow'd eminently to concur in extraordinary caſes. But it is not yet decided, what conveys air into the lungs; for 'tis demanded, what ſhould bring the air into the lungs, if they do not attract it? To this queſtion, ſome of the beſt modern philoſophers anſwer, that, by the dilatation of the cheſt, the contiguous air is thruſt away; and that, preſſing upon the next air to it, and ſo onwards, the propulſion is continu'd, till the air be drawn into the lungs, and ſo dilates them. It is, again, objected by *Bartholine*, that, according to this doctrine, a man could not fetch his breath from a great veſſel, with a ſlender neck, full of air; becauſe, when his mouth covers the orifice of the neck, the dilatation of his thorax could not propel the air of the veſſel into his lungs; being ſeparated by the incloſing veſſel, from the ambient air: and yet, it will be ſaid, experience witneſſeth, that out of ſuch a veſſel a man may ſuck air. But this difficulty our engine can eaſily ſolve; ſince many of the preceding experiments ſhew, that, in this caſe, there needs no propulſion of the air, by the ſwelling thorax, or abdomen, into the lungs: ſince, upon the bare dilatation of the thorax, the ſpring of that internal air, which poſſeſſes as much of the cavity of the cheſt, as the lungs fill not, being much weaken'd, the external and contiguous air, muſt neceſſarily preſs thro' the open wind-pipe into the lungs, as finding there the leaſt reſiſtance.

And hence, by the way, we are aſſiſted to judge of that famous controverſy, among naturaliſts and phyſicians, ever ſince the time of *Galen*; ſome maintaining, that the cheſt, with the contain'd lungs, reſembles a pair of bellows, which are, therefore, fill'd, becauſe dilated: and others pleading, that the compariſon ſhould be made with a bladder, which is, therefore, dilated, becauſe it is fill'd. For, as to the thorax, it ſeems evidently, like a pair of bellows, to be partly fill'd with air, becauſe it was dilated; but as for the lungs, which want fibres to diſtend them, they may fitly be compared to a bladder; ſince they are dilated, by being fill'd with that air which ruſheth into them, upon the dilatation of the cheſt, in the cavity whereof, it finds leſs reſiſtance to its ſpring, than elſewhere. And this calls to mind that ſtrange obſervation of *Nicholaus Fontanus*, a phyſician at *Amſterdam*, who declares, that, in a boy of the ſame city, four years old, there

PNEUMATICS. there was found, inſtead of lungs, a certain membranous bladder, which, being fill'd with air, and furniſh'd with little veins, had its origin from the wind-pipe. This being ſuppoſed true, I leave it to be conſider'd, how well it will agree with moſt of the opinions, as to reſpiration.

And thus may the grand objection of *Bartholine*, and others, be anſwer'd; but I leave anatomiſts to conſider what is to be ſaid to ſome obſervations, that ſeem to contradict thoſe anatomical experiments above-mention'd: ſuch was, particularly, that in *Sennertus*, of a melancholy ſtudent, who, having ſtabb'd himſelf, and pierc'd the diaphragm in the tendinous part, lived ſeven months after the wound was made; but, dying at length, it appear'd ſo great, being, perhaps, dilated by his ſtraining to vomit, that the whole ſtomach was found to have got by it, into the left ſide of the thorax. And ſuch, alſo, was the accident which happen'd to a nobleman whom I have ſeen, and who is yet alive; in whoſe cheſt, there hath, for theſe many years, remain'd a hole ſo great, that the motion of his heart may be perceiv'd thro' it. An ingenious conjecture hath been made, at the cauſe of the ſudden death of animals in the exhauſted receiver, which ſuppoſes it to be, not the want of air that deſtroys them, but the preſſure of that in the cavity of the cheſt; as if the ſpring thereof, being no longer balanced by the ambient air, thereby becomes ſo ſtrong, as to keep the thorax forcibly diſtended, hinder its wonted contraction, and ſo compreſs the lungs and their veſſels, as to obſtruct the circulation of the blood. But *Wallæus* relates, that he often obſerv'd, in the diſſection of live bodies, the membrane which inveſts the lungs, had pores in it, as big as the larger ſort of peas: which agrees with the obſervations of chirurgeons and phyſicians, that matter, collected in the thorax, hath penetrated into the lungs, and been diſcharged by coughing. And moſt of the animals, kill'd in our engine, were birds; whoſe lungs, Dr. *Harvey* has obſerv'd, very manifeſtly to open, at their extremities, into the abdomen: and, by ſuch perforations, we may well ſuppoſe the paſſage free, betwixt the external air, and that in the abdomen. Beſides, to ſhew that the animals, which expired in our glaſſes, need not be ſuppoſed to have been kill'd by the want of air; we foreſee another argument, which ought not to be conceal'd. The poſſibility of a vacuum is, frequently, deny'd; and the ſpaces void of air, and other groſſer bodies are, all of them, ſuppoſed exactly repleniſhed with a certain ethereal matter, ſo thin and ſubtile, that it can freely penetrate the pores of the moſt compact, and cloſe bodies, even glaſs itſelf. Hence it may be ſaid, that the animals, included in our receiver, died not ſo much for want of air, as becauſe the air pumped out, was neceſſarily ſucceeded by an ethereal ſubſtance; which, conſiſting of parts vehemently agitated, and ſo very ſmall, as, without reſiſtance, to paſs in and out, thro' the pores of glaſs; a conſiderable quantity of this reſtleſs matter, meeting together in the receiver, may be quickly able, by the exceſſive heat of it, to deſtroy a little animal, or, at leaſt, make the air too hot to be fit for reſpiration.

But

But we have already anſwer'd this objection, by the late experiments; which ſhew no heat to be generated in our exhauſted receiver. PNEUMATICS.

It might, alſo, ſeem probable, that, upon the ſudden removal of the wonted preſſure of the ambient air, the warm blood of our animals was ſo vehemently expanded, as to diſturb the circulation, and ſo diſorder the whole œconomy of the body; did ſuch animals, alone, as are of a hot conſtitution, loſe their lives in the exhauſted engine. But as to the uſe of air, in reſpiration, 'tis known to ſerve in the production and modulation of the voice; the expulſion of excrements, by coughing; the conveying in of odours, by inſpiration, *&c.* which are rather convenient for the well-being of an animal, than neceſſary to life. *Hippocrates* ſays, of the air in animals endow'd with lungs, that "tis the cauſe both of life, and diſeaſes; "that 'tis ſo neceſſary, a man cannot live part of a day without it; and "that reſpiration, alone, is the action which can never be ſuſpended." But, as to the reaſon why the inſpiration, and expiration of air, are ſo very neceſſary to life, both naturaliſts, and phyſicians, differ ſo widely, that it will be very difficult, either to reconcile their opinions, or determine their controverſies.

Many ſuppoſe the chief uſe of reſpiration, is to cool and temper that heat in the heart and blood, which would, otherwiſe, be immoderate. They, alſo, ſuppoſe, that the air is neceſſary, by its coldneſs, to condenſe the blood that paſſeth out of the right ventricle of the heart into the lungs; whereby it may gain ſuch a conſiſtence, as is requiſite to make it fit fewel for the vital flame in the left ventricle of the heart. And, indeed, fiſh, and other cold creatures, whoſe hearts have but one cavity, are unprovided of lungs. But, tho', poſſibly, the air inſpired, may, ſometimes, be of uſe in refrigerating the heart; yet, it may be objected, that ſeveral cold creatures, as, particularly frogs, ſtand in need of reſpiration; which ſeems unneceſſary for refrigeration to them, who are deſtitute of any ſenſible heat, and live in the cold water; that even decrepid old men, whoſe natural heat is very languid, and almoſt extinguiſhed, have, yet, a neceſſity of frequent reſpiration; that a temperate air, is fitteſt for the generality of breathing creatures; and as an air too hot, ſo alſo, an air too cold, may be inconvenient for them; that in ſome diſeaſes, the natural heat is ſo weaken'd, that were the uſe of reſpiration to cool, it would be more hurtful, than beneficial, *&c.* Theſe, and other objections, might be oppos'd, and preſs'd, againſt the recited opinion; but, we ſhall only add, that it appears not, by our foregoing experiments, in the exhauſted receiver, where animals die ſo ſuddenly, for want of reſpiration, that the ambient body is ſenſibly hotter, than the common air.

Others will have the very ſubſtance of the air to get, by the veſſels of the lungs, to the left ventricle of the heart, not only to temper its heat, but to provide for the generation of ſpirits. And, theſe alledge the authority of the ancients, among whom, *Hippocrates* ſeems, manifeſtly, to favour their opinion; and both *Ariſtotle*, and *Galen*, ſometimes appear inclinable to it. But, it ſeems very difficult to ſhew, how the air is convey'd into the

PNEUMATICS. the left ventricle of the heart; eſpecially, ſince the ſyſtole and diaſtole of the heart, and lungs, are very far from ſynchronal: beſides, the ſpirits appearing to be, but the moſt ſubtile, and unctuous particles of the blood, ſeem of a very different nature, from that of dry incombuſtible corpuſcles of air.

Another opinion of reſpiration makes the genuine uſe of it to be the ventilation of the blood, in its paſſage thro' the lungs; whereby, it is diſburthen'd of thoſe excrementitious ſteams, proceeding, for the moſt part, from the ſuperfluous ſeroſities of the blood, and chyle. But, this hypotheſis may be explain'd two ways. For the neceſſity of air in reſpiration, may be ſuppos'd to proceed from hence; that, as a flame cannot long ſubſiſt in a narrow, and cloſe place, becauſe, the fuliginous ſteams, it continually throws out, cannot be long receiv'd into the ambient body, which, after a while, growing too full of them, to admit any more, ſtifles it; ſo the vital fire in the heart requires an ambient body of a yielding nature, to receive into it the ſuperfluous ſeroſities, and other recrements of the blood; the ſeaſonable expulſion whereof, is requiſite to depurate the maſs, and make it fit, both to circulate, and to maintain the vital heat reſiding in the heart. The other way, is, by ſuppoſing, that the air doth, not only as a receptacle, admit into its interſtices the excrementitious vapours of the blood, when they are expell'd thro' the wind-pipe; but, alſo conveys them out of the lungs; becauſe, the inſpired air, reaching to all the ends of the *Aſpera Arteria*, there aſſociates itſelf with the exhalations of the circulating blood: and, when 'tis exploded, carries them away with itſelf, as winds ſpeedily dry up the ſurfaces of wet bodies.

Now, to the firſt of theſe two ways, our engine affords us this objection; that upon the exſuction of the air, the animals die a great deal ſooner, than if it were left in the veſſel; tho', by that exſuction, the ambient ſpace is left much more free to receive the ſteams, that are either breathed out of the lungs of the animal, or diſcharg'd by inſenſible tranſpiration.

But, if the hypotheſis be taken in the other ſenſe, it ſeems agreeable to that grand obſervation, which the phenomena of our engine, and the relations of travellers ſuggeſt, that there is a certain conſiſtence of air, requiſite to reſpiration; ſo that, if it be too thick, and already over-charg'd with vapours, it will be unfit to unite with, and carry off thoſe of the blood; as water will diſſolve, and aſſociate, but a certain proportion of ſaline corpuſcles; and, if it be too thin, the number or ſize of the aerial particles is too ſmall to receive, and carry off the excrements of the blood in due quantity.

Now, that air too much thicken'd with ſteams, is unfit for reſpiration, appears by what happens in the lead-mines of *Devonſhire*, and, perhaps, of ſome other countries; for, I am credibly inform'd, that damps often riſe here, which ſo thicken the air, as ſuddenly to ſtifle the workmen. And, that this proceeds, not from any arſenical, or poiſonous exhalation contain'd in the damp; but, from too great a condenſation of the air; ſeems

ſeems probable, becauſe it often leiſurely extinguiſhes the flames of their candles, or lamps; and alſo, becauſe in thoſe cellars, where large quantities of new wine are ſet to work, men have been ſuffocated by the ſteams exhaling from the muſt, and too much thickening the air: for this reaſon, in ſome hot countries, thoſe who have occaſion to go into ſuch cellars, carry with them a quantity of well-kindled coals, which they hold near their faces, whereby, the fumes being diſſipated, and the air rarified, the ambient body is reduced to a conſiſtence fit for reſpiration. PNEUMATICS.

And, by way of confirmation hereof, we may add, that in a ſmall receiver, we carefully clos'd a bird, which, tho' for a quarter of an hour, he ſeem'd not much prejudiced, by the cloſeneſs of his priſon, he, afterwards, began to pant vehemently, keep his bill open, and appear very ſick; and, at length, after ſome long, and violent ſtrainings, he caſt up a little matter out of his ſtomach: and this he did ſeveral times, till growing ſo ſick, that he ſtagger'd, and gaſp'd, and was ready to expire. Now, we perceiv'd, that within three quarters of an hour, from the time he was put in, he had ſo thicken'd, and tainted the air, with the ſteams of his body, that it was become altogether unfit for the uſe of reſpiration; which is no wonder, ſince, according to *Sanctorius*, that part of our aliment, which goes off by inſenſible perſpiration, exceeds, in weight, all the viſible, and groſſer excrements, both ſolid, and liquid.

That air too much dilated, is unfit for reſpiration, the ſudden death of animals kill'd in our exhauſted receiver, ſufficiently manifeſts. And, it may well be doubted, whether if a man were rais'd to the very top of the atmoſphere, he would be able to live there many minutes. *Joſephus Acoſta* tells us, that when he himſelf paſs'd the high mountains of *Peru*, to which, he ſays, the *Alps* ſeem'd but as ordinary houſes, compared with high towers; he, and his companions were ſurpriz'd with extreme pangs of ſtraining, and vomiting blood, and with ſo violent a diſtemper, that he concludes, he ſhould undoubtedly have died, but, that this laſted not above three, or four hours, before they came into a more natural temperature of air. Our author adds, that he is, therefore, perſuaded, "the element of the air is there ſo "ſubtile, and delicate, as to be inconſiſtent with the reſpiration of man, "which requires a more groſs, and temperate air."

But, perhaps, the air doth ſomething more, than barely help to carry off what is thrown out of the blood, in its paſſage thro' the lungs, from the right ventricle of the heart to the left. For in phlegmatic conſtitutions, and diſeaſes, the blood will circulate tolerably well, notwithſtanding its being exceſſively ſerous; and in aſthmatical caſes, tho' the lungs be greatly ſtuff'd with viſcid phlegm, yet the patient may live for ſome years: whence it is ſcarce probable, that either the detention of the ſuperfluous ſerum of the blood, for a few moments in the lungs, ſhould be able to kill a perfectly ſound and lively animal; for we commonly found, upon repeated trials, in a ſmall receiver, that, within half a minute, a bird would be ſurpriz'd by mortal convulſions, and, within a minute more, would die, beyond a poſſibility of recovery from the air, tho' never ſo haſtily let in. And, what ſhews it was not the cloſeneſs of the veſſel, but the ſudden exſuction of the air, that killed thoſe creatures ſo ſoon; we once inclos'd a bird in a ſmall receiver, where, for a while, he eat very chearfully ſome ſeeds that we convey'd in with him; and not only liv'd ten minutes, but had, probably, ſurviv'd much longer, tho' he had not been reſcu'd. Another bird being, within half a minute, caſt into violent convulſions, upon the exſuction of the air; we haſtily turn'd the ſtop-cock, to let it in again, whereby the gaſping animal was preſently recover'd. And, at another time, we, at night, ſhut up a bird in one of our ſmall receivers, and obſerv'd, that, for a while, he was ſo inſenſible of the alteration of the air, that he fell aſleep, with his head under his wing; and tho' he afterwards awaked ſick, yet he continued upon his legs, for above forty minutes; and then ſeeming ready to expire, we took him out, and ſoon found him lively. Upon the whole, there appears reaſon to ſuſpect, that there is ſome uſe of the air, which we do not yet thoroughly underſtand, that makes it ſo neceſſary to the life of animals.

Paracelſus, indeed, tells us, that "as the ſtomach concocts the aliment, "and makes part of it uſeful to the body, rejecting the other; ſo the "lungs conſume part of the air, and reject the reſt." Whence, according to him, we may ſuppoſe a little vital quinteſſence in the air, which ſerves to refreſh and reſtore our vital ſpirits; for which purpoſe, the groſſer, and far greater part of the air, being unſerviceable, it is not ſtrange that an animal ſhould inceſſantly require freſh air. This opinion, indeed, is not abſurd; but it requires to be explain'd and prov'd: beſides, ſome objections may be made to it, from what has been already argued againſt the tranſmutation of air, into vital ſpirits. Nor is it probable, that the bare want of the generation of the uſual quantity of vital ſpirits, for leſs than one minute, ſhould be able to kill a lively animal, without the help of any external violence. And, upon this ſuppoſition, *Cornelius Drebell*, is affirm'd, by many credible perſons, to have contrived a veſſel to be row'd under water: for *Drebell* conceiv'd, that it is not the whole body of the air, but a certain ſpirituous part of it, that fits it for reſpiration; which being ſpent, the remaining groſſer body of the air, is unable to cheriſh the vital flame reſiding in the heart. So that, beſides the mechanical contrivance of his boat, he had a chymical liquor, which, by unſtopping the veſſel wherein it was contain'd, the fumes of it would ſpeedily reſtore to the air, foul'd by reſpiration, ſuch a proportion of vital parts, as would make it again fit for that office; and having made it my buſineſs to learn this ſtrange liquor, his relations conſtantly affirm'd, that *Drebell* would never diſcloſe it, but to one perſon, who himſelf told me what it was. I have, therefore, been ſometimes, inclined to ſuppoſe, the air neceſſary to ventilate and cheriſh the vital flame, which ſome imagine to be continually burning in the heart: for that, in our engine, the flame of a lamp will vaniſh almoſt as ſoon after the exſuction of the air, as the life of an animal. We have made a hard body, in the form of a clove, but twice as long, and proportionably thick, of ſuch a compoſition, that if it be kindled at the upper end, it will

moſt

most certainly burn away to the bottom, much better than a match: this we often convey'd, kindled at the upper end, into a small receiver; but still found, that tho' presently, upon the exsuction of the air, it would leave smoking, and seem quite gone out; and again begin to smoke, as soon as the air was let in upon it; yet, if the air were kept out but four or five minutes, the fire would be totally, and irrecoverably extinguish'd. And, conveying a small lamp into a large receiver, with highly rectified spirit of wine, we could not, upon several trials, make the flame last two minutes, after the air was began to be drawn out. This latter opinion, however, has its difficulties: for tho', in the hearts of many animals, the blood be a warm liquor, and, in some, even hot; yet it is hard to conceive either how the air can get thither; or how, in case it could, it should increase the heat: since, however the air may increase the heat of a coal, by blowing off the ashes, and making the active corpuscles penetrate farther into the kindled body, and shatter it the more; yet hot liquors have their heat allay'd, by air blown on them. And, since some naturalists think the heat residing in the heart, to be a true flame, but temperate as the flame of spirit of wine; which will long burn upon fine linen, or paper, without consuming them; I wish they had been more curious to make different trials with that liquor. For the flame of highly rectified spirit of wine, will not only consume paper, and linen; but I have used it in lamps, to distil liquors out of tall cucurbits, and found that it gave, at least, as great a heat, as oil: nay, I have readily melted crude gold, with the bare flame of this spirit.

Dr. *Harvey* demands, "why a fœtus, even out of the womb, if involv'd "in the secundines, may live, for a considerable time, without respiration; "yet, if after having once began to breathe, its respiration be stop'd, it "presently dies?" We pretend not to solve this problem, but made the following experiment with a view to it. We caus'd a bitch to be strangled, that was almost ready to whelp; and presently opening her, found four puppies; one of which we freed from the coats that involv'd him, and from the liquor wherein he swam, and observed, that he quickly open'd his mouth very wide, mov'd his tongue, and exercis'd respiration. Then we open'd both his abdomen, and chest, and cut the diaphragm asunder; notwithstanding which, he seem'd often to endeavour at respiration, and remarkably mov'd the intercostal muscles, part of the diaphragm, the mouth and tongue. But being desirous to try whether the other young ones, that had not yet breath'd at all, would long survive this; we took them out, and having open'd them, found none of them so much alive as to have any perceptible motion in their hearts; whereas the heart of that which had once enjoy'd the benefit of respiration, continued its motion so long, that we observ'd the auricle to contract, after five or six hours; and it continued about two hours longer.

It is much doubted, whether fish breathe under water. That such as are not of the whale kind, have no respiration, as 'tis exercised by beasts, and birds, may be argued from their having no cavity in their hearts, and from their

PNEUMATICS. their want of lungs, whence they are obſerv'd to be mute; unleſs we ſay, that their gills anſwer to lungs. But that air is neceſſary even to the lives of fiſh; and that therefore, 'tis probable, they have ſome obſcure kind of reſpiration, ſeems manifeſt from obſervations, and experiments. Several authors tell us, that fiſh ſoon die in ponds, and glaſſes quite fill'd with water, if the one be ſo frozen over, and the other ſo cloſely ſtop'd, that they cannot enjoy the benefit of the air. And our engine hath taught us, that many little parcels of interſperſed air, lurk in water; and this, perhaps, fiſh may make ſome uſe of.

Removing a large eel, out of a veſſel of water, into our great receiver, we cauſed the air to be evacuated, and obſerv'd, that after ſome motion in the glaſs, ſhe ſeem'd ſomewhat diſcompoſed, and, at length, turn'd up her belly, and afterwards lay altogether moveleſs, as if quite dead; but upon taking her out of the receiver, ſhe ſhew'd herſelf as much alive as before.

But, indeed, a large grey houſe-ſnail, being clos'd up in one of our ſmall receivers, neither fell down from the ſide of the glaſs, upon drawing out the air; nor was ſo much as depriv'd of progreſſive motion thereby: tho', except this, we never put any living creature into our exhauſted receiver, but what gave ſigns of death.

Hippocrates, and ſome learned phyſicians of late, ſuppoſe, that a fœtus reſpires in the womb; but it ſeems very difficult to conceive how air ſhould traverſe the body of the mother, and the teguments of the child: and ſince nature hath, in new-born infants, contrived peculiar temporary veſſels, that the blood may circulate thro' other paſſages, than it does in the ſame individuals, when they come to have the free uſe of their lungs, 'tis improbable that the fœtus in the womb ſhould properly reſpire: but, then, ſince our experiments have manifeſted, that almoſt all kinds of liquors, as well as water, abound with interſperſed corpuſcles of air, it ſeems not altogether abſurd, that when the fœtus is grown big, it may exerciſe ſome obſcure reſpiration; eſpecially ſince children have been heard to cry in the mother's womb. And I know a young lady, whoſe friends, when ſhe once went with child, complain'd to me, that ſhe was ſeveral times much frighted with ſuch cries; which, till I diſabuſed her, ſhe, and her friends, look'd upon as portentous. And 'tis no very unfrequent thing, to hear the chick pip in the egg, before the ſhell is broken. This, however, I only bring as a probable argument, till I can diſcover whether the motion of a rarified ſubſtance, tho' no true air, may not, at the top of the larynx, produce a ſound; ſince the blade of a knife, held in ſeveral poſtures, in the ſtream of the vapours that iſſues out of an æolipile, will afford various and very audible ſounds. I have, alſo, had thoughts of trying to make a large receiver, with little glaſs windows, capable of holding a man, who may obſerve ſeveral things as to reſpiration, &c. and, in caſe of fainting, may, by giving a ſign, be immediately relieved with freſh air. And it ſeems not impoſſible, that ſome men, by uſe, may bring themſelves to ſupport the want of air a pretty while; ſince we ſee that ſeveral will live much longer than others under water. Thoſe who dive for pearls in the

the *West-Indies*, are reported to be able to stay a whole hour under water: and *Cardan* tells us of one *Colanus*, a diver in *Sicily*, who was able to continue there three or four times as long. We have, also, often seen in *England*, a corpulent man, who descends to the bottom of the *Thames*, and thence brings large fish, alive in his hands, out of deep holes; as *Acosta* tells us, he saw in *Peru*, the like manner of fishing practised by the *Indians*.

However, there are but few men, who, even by use, can support, for many minutes, the want of air: a famous diver, of my acquaintance, tells me, that at the depth of 50 or 60 feet under water, he cannot continue above two minutes, without resorting to the air which he carries down with him in an engine. He, also, told me, that by the help of spunges dip'd in oil, and held in his mouth, he could much longer support the want of respiration, under water, than without them: the true cause of which, would, perhaps, if discover'd, hint the nature of respiration in fish. But the necessity of air to the greatest part of animals, unaccustomed to the want of it, may be best judg'd of by the following experiment.

We convey'd a bee, a flesh-fly, and a palmer-worm, into one of our small receivers, and, upon exhausting thereof, observ'd, that the bee and the fly fell down, and lay with their bellies upwards, and that the worm seem'd to be suddenly struck dead; all of them lying without motion, or any other discernible sign of life, in less than one minute; notwithstanding the smallness of the animals, in proportion to the receiver, which, too, was not free from leaks: but we had no sooner re-admitted the air, than all the three insects gave signs of life, and, by degrees, recover'd. When we had again drawn out the air, their motions presently ceased, and they fell down, seemingly dead, as before; continuing moveless, as long as, by pumping, the vessel was kept exhausted. Herein appears the wise conduct, and goodness of the creator, who, by giving the air a spring, hath made it very difficult to exclude a thing so necessary to animals. And here we may suspect, that if insects have no lungs, nor any part answering thereto, the ambient air affects, and relieves them, at the pores of their skin; for, as *Hippocrates* well said, " a living body is every where perspirable." Thus the moister parts of the air readily insinuate themselves into, and recede from the pores of the beards of wild oats, and of other wild plants, which almost continually wreath and untwist themselves, according to the lightest variations in the temperature of the air.

We, particularly, took notice in this experiment, that, when, at any time, upon the re-admission of the air, the bee began to recover, the first sign of life she gave, was a vehement panting, which appear'd near the tail; the like we have observ'd in bees drown'd in water, when they first come to be revived, by a convenient heat; as if the air were, in one case, as proper to set the spirits, and alimental juice in motion, as heat, in the other.

This experiment, also, seems to manifest, that, even living creatures, man always excepted, are a kind of very curious machines. For, here we see animals lively, and perfectly sound, immediately deprived of motion, and

 and all discernible signs of life, and reduced to a condition that differs from death, only, in being not absolutely irrecoverable: and this is perform'd without the least external violence, more than is offer'd to a windmill, when, the wind ceasing to blow on the sails, all the several parts remain moveless, and useless, till a new breeze puts them again into motion.

'Tis known, that bees, and some other insects, will walk, and fly, for a great while after their heads are off, and sometimes one half of the body will, for several hours, walk up and down, when it is sever'd from the other; yet, upon the exsuction of the air in this experiment, not only the progressive motion of the whole body, but the very motions of the limbs immediately cease; as if the air were more necessary to these animals, than their own heads.

But, in these insects, that fluid body, in which life chiefly resides, seems nothing near so dissipable, as in perfect animals. For, the birds convey'd into our small receiver, were, within two minutes, brought past recovery; but, we were unable to kill our insects, by the exsuction of the air: for, tho' as long as the pump was kept working, they continued immoveable, yet, when that rested, the air, which press'd in, at the unperceiv'd leaks, slowly restored them to the free exercise, and functions of life. Without denying, then, that the air may be, sometimes, very useful, by condensing, and cooling the blood, that passeth thro' the lungs; I am of opinion, that the depuration of that animal fluid, is one of the ordinary, and principal uses of respiration.

Whether the action of menstruums depends upon the pressure of the air.

42. Having entertain'd a suspicion, that the action of corrosive liquors in dissolving bodies, may be considerably varied by the gravitation, or pressure of the incumbent air, and the removal of it; I examined my conjecture by the following experiment.

I cast ten whole pieces or sprigs of red coral, into as much spirit of vinegar as reach'd an inch above them; then putting these, together with the menstruum, into a long-neck'd vial, whereof they scarce fill'd a third part, we convey'd that vial into one of our small receivers, and having fasten'd on the cover, we let the liquor remain unmov'd a while. But finding, there only arose, as before, a number of small bubbles, that caused no sensible froth upon the surface of the vinegar; we made two or three exsuctions of the air, upon which there rose, from the coral, such a multitude of bubbles, as made the whole body of the menstruum appear white; and soon after, yielded a froth, equal in magnitude to the rest of the liquor; the menstruum plainly appearing to boil: tho', if we desisted but one minute from pumping, the decrease of the froth, and ebullition, upon the getting in of a little air, at some leak or other, seem'd to argue, that the removal of the pressure of the external air, gave occasion to this effervescence. But, for farther satisfaction, we let in the external air at the stop-cock, when, immediately, the froth vanished; and so many of the bubbles, within the body of the liquor, disappear'd, that it lost its whiteness, and became transparent again; the menstruum, also, working as languidly upon the coral, as before they were put into the receiver: but,

when

when we had again drawn out the air, firſt the whiteneſs re-appear'd, and then the ebullition was renew'd; which, at length, grew ſo great, that, for three or four times ſucceſſively, when the air was let out of the receiver into the emptied cylinder, the froth overflow'd the glaſs, and ran down the ſides of it: and yet, upon re-admitting the excluded air, it grew, immediately, calm and tranſparent; as if its operation upon the coral, had been facilitated by the exſuction of the incumbent air; which, on its receſs, left it eaſier for the more active parts of the liquor to ſhew themſelves, than whilſt the preſſure of the air continued. It may, indeed, be ſuſpected, that thoſe vaſt and numerous bubbles proceeded not from the action of the menſtruum upon the coral, but from the ſudden emerſion of thoſe many little parcels of air, which are diſperſed in liquors; but, having had this ſuſpicion before we made the experiment, we convey'd our diſtill'd vinegar, alone, into the receiver, and kept it a while there, to free it from its bubbles, before ever we put the coral into it. It may be ſuſpected, likewiſe, that the agitation of the liquor, conſequent upon ſhaking the glaſs, by pumping, might occaſion the ebullition; but, upon trial, there appear'd no conſiderable change in the liquor, or its operation, tho' the containing veſſel was ſhaken, if no air were drawn out. The experiment was again made in a ſmall receiver, upon coral groſly powder'd, with a ſucceſs very like the former; only the coral, being now reduced to ſmaller parts, ſo many little lumps of it would, upon the ebullition of the liquor, be carry'd, and buoy'd up, by the emerging bubbles, as ſometimes to darken the vial; tho' they would fall again, upon letting in the air. We muſt not omit, that, when the ſpirit of vinegar was boiling upon the coral, we took out the vial, but could not find that the liquor was ſenſibly hot.

PNEUMATICS.

The ebullition of warm liquors in vacuo.

43. We cauſed water to be long boil'd, that it might be freed from its air; then, almoſt filling a four-ounce glaſs-vial with it, we convey'd that, whilſt the water was yet hot, into a ſmall receiver; and having luted on the cover, the air was drawn out: upon the two firſt exſuctions, there ſcarce appear'd any change in the liquor; nor was there any great alteration made by the third; but at the fourth, and afterwards, the water appear'd to boil in the vial, as if it had ſtood over a very ſtrong fire; for the bubbles were much greater than are uſually found, upon the ebullition of large quantities of water. And this effervеſcence was ſo great, that, the liquor, boiling over the top of the neck, much of it ran down into the receiver, and, ſometimes, continued to boil there. In proſecuting the experiment, we obſerv'd, that, ſometimes, after the firſt ebullition, we were obliged to make ſeveral exſuctions, before the liquor could be brought to boil again: but, at other times, as often as the air was ſuffer'd to paſs from the receiver into the pump, the effervеſcence would begin afreſh; tho' the pump were ply'd for a pretty while together: which ſeem'd to argue, that the boiling of the water proceeded from hence, that, upon withdrawing the preſſure of the incumbent air, either the fiery corpuſcles, or rather the vapours agitated by the heat in the water, were permitted

PNEUMATICS. greatly to expand themselves in the evacuated receiver; and, in their tumultuous dilatation, lifting up the higher part of the water, and, turning it into bubbles, made it appear to boil; for the effervescence was confined to the upper part of the water; the lower remaining quiet, unless the liquor were but shallow. And tho', sometimes, as we said, the ebullition began again, after it had ceased a pretty while; whence it seem'd that some concurrent cause did a little modify the operation of heat; yet, when the water, in the vial, could, by pumping, be brought to boil no more, the same water, being, in the very same vial, convey'd back to the receiver, was quickly brought to boil afresh, with vehemence, and for a considerable time; whilst a new parcel, taken out of the same boil'd water with the former, and put in cold, could not, by pumping, be brought to shew the least effervescence. And hot sallet-oil shew'd no effervescence in our receiver; but the chymical oil of turpentine was presently made to boil up, till it reached four or five times its former height in the vial; and continued boiling, till it was almost but luke-warm. Wine, also, being convey'd in hot, did, at the very first exsuction, begin to boil so vehemently, that, in a short time, while the pump was kept moving, four parts of five boil'd over the vial, tho' it had a long neck. And even of the water itself, near one half would, sometimes, boil over into the receiver, before it became luke-warm. It was, also, remarkable, that once, when the air had been drawn out, the liquor did, upon a single exsuction, boil so long, with prodigiously vast bubbles, that the effervescence lasted almost a quarter of a minute. Hence it appears, that the air, by its stronger, or weaker pressure, may very much modify several operations of that vehement and tumultuous agitation of the small parts of bodies, wherein the nature of heat seems to consist: so that if a heated body were convey'd above the atmosphere, 'tis probable, that the heat would have a different operation, as to the power of dissipating the parts of it, from what it hath here below.

SECT. II.

The air-pump farther improved.

HAving now presented my great engine to the royal society, I was obliged to procure another; wherein, tho' the construction, in general, be the same in both, there were some alterations, and improvements made.

Fig. 42 & 43. The figures represent this engine, as ready for work; and, because the sucker is to be always under water, and the perforation P Q, that is continu'd perpendicularly quite thro' it, and serves, together with the stick R S, for a valve, is to be stopp'd at the bottom of the cylinder, as at N O, when 'tis full of water; 'twas requisite to make the stick R P, two or three feet long. But the chief thing is, that, in the second figure, the pipe A B, whose end B, bends upwards, lies in a groove, purposely made, in the flat wooden board C D E F, on which the receivers are to rest. This square board, I caused to be overlaid with very good cement, on which was applied a strong plate of iron, of the bigness and shape of the board, leaving only a

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PLATE IX.

Fig. 43.

p. 474.

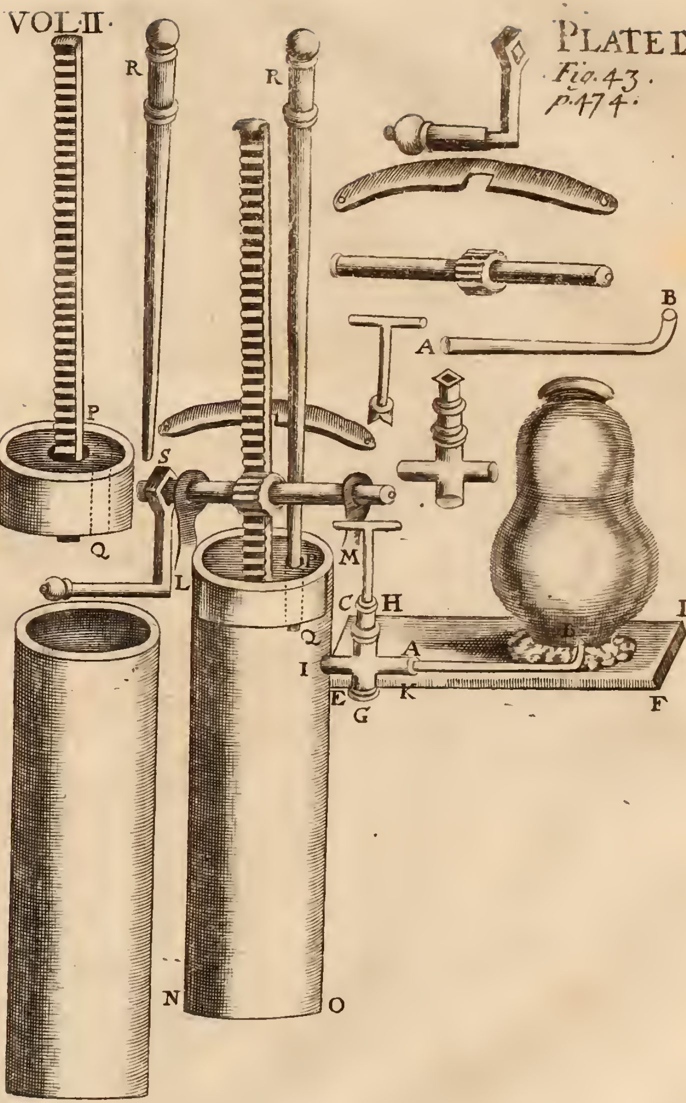
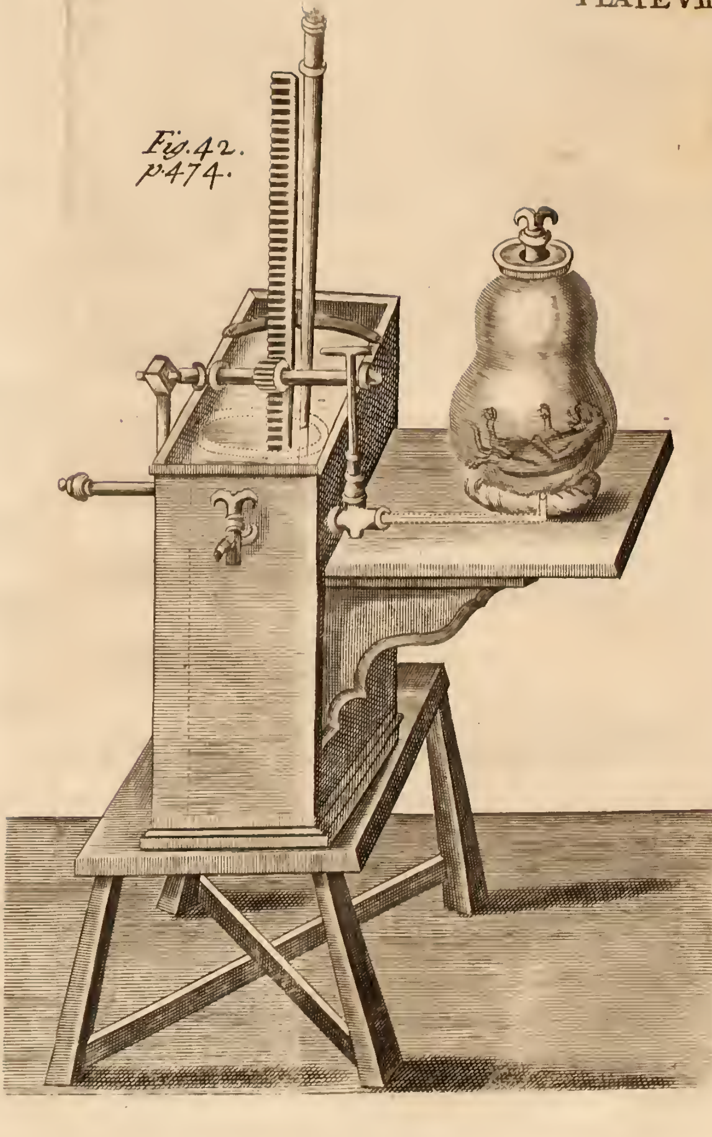


Fig. 42.
p. 474.



ſmall hole, for the erect part of the pipe to come out at; which I added, not only to keep the board from warping, but becauſe the preſſure of the atmoſphere on the ſide of it, when there is none, or very little, on the other, will enable many aerial particles to ſtrain thro' the wood, tho' thick, and moiſten'd with oil. To this iron-plate, we can fit a lip, turning up about it, to prevent the water, that, on ſome occaſions, comes from the receiver, from falling on the floor. And, by the way, tho' the ſtop-cock G H I K, that belongs to the pipe, may be inſerted at I, into the cylinder L M N O, by the help of ſolder; yet we choſe to have the branch I, of the ſtop-cock, made like a ſcrew, which, being once firmly fitted to the barrel, is not apt to be broken off, and may be more eaſily mended, if any thing happen to be out of order; which the engine is moſt liable to, in, or about the pipe. PNEUMATICS.

The ſquare, and hollow wooden part of this engine diſcernible in the firſt figure, is ſo made, not only to contain the cylinder, but as much water as will always keep it quite covered, by which means, the ſucker lying, and playing always under water, is continually kept plump, and turgid; and the water being ready to fill up any little interval, that may happen, between the ſucker, and the inſide of the barrel, farther conduces to keep out the air. But, if great care be not taken in turning the ſtop-cock, the water will be impell'd into the receiver, and prejudice ſeveral experiments, when the included bodies may be ſpoil'd, or impair'd by that liquor.

The flat plate, lately mentioned, has this great conveniency in many experiments, that the receiver needs no ſtop-cock of its own; for ſuch a veſſel being made of an entire piece of glaſs, and laid upon the plate, well cover'd with cement, can better keep out the air, than if there were a ſtop-cock, at which the air too frequently gets in.

A good cement, wherewith to faſten the receivers to the iron-plate, is a thing of great moment in making the following experiments, and we employ different compoſitions for different purpoſes; but, in general, only a mixture of bees-wax and turpentine, made with equal parts for the winter, and three parts of the former to two of the latter, for the ſummer.

Mercury rais'd by the ſpring of a little included air. *Fig. 44.*

1. We took a vial with a ſmall neck, and having fill'd about a fourth part of it with quick-ſilver, we ſo erected, and faſten'd a long and ſlender pipe of glaſs, open at both ends, in the neck thereof, with hard ſealing wax, that the lower end reach'd almoſt to the bottom of the quick-ſilver, and the upper more than a yard above the vial; then, having blown in a little air, we convey'd the whole into a long ſlender receiver: upon evacuating whereof, we found, that the ſpring of the air included in the vial, impell'd the quick-ſilver into the erected pipe, to the height of 27 inches; and ſuffering the external air to return into the receiver, the quick-ſilver ſubſided in the tube, ſometimes almoſt, and ſometimes quite as low as the ſtagnant mercury in the vial.

This experiment we made ſeveral times; and having once blown in ſo much, air, that what was in the cavity of the vial rais'd and kept the quick-ſilver three inches high in the pipe, we found, by emptying the receiver, that the quick-ſilver roſe 30 inches, or more, above that in the vial.

Sometimes it may happen, that the mercury, when taken very ſoon out of the receiver, will not appear to have ſubſided to its firſt ſtation; which is not to be wonder'd at; ſince, in a receiver, containing but little air, the heat of the cement, and of the iron employ'd to melt it quite round the glaſs, may impart a little warmth to the air in the vial, which will afterwards return to its former temper.

'Tis very remarkable, if the receiver be properly ſtopp'd and ſlender enough, that upon the turning of the ſtop-cock to let out the air at the firſt exſuction, the mercury will be impell'd up by the ſpring of that in the vial, ſo as to riſe ſeveral inches above the height it will afterwards reſt at, and make ſeveral vibrations up and down before it comes to ſettle; juſt as the mercury does in the *Torricellian* experiment: and ſuch motions of the mercury will be made for four or five ſubſequent exſuctions; but they grow gradually leſs, as the ſpring of the included air is weaken'd.

At the firſt exſuction, when the ſpring of the included air was yet ſtrong, we found the mercury would be rais'd above half, if not $\frac{2}{3}$ of the whole height, whereto 'twill, at length, aſcend: but the ſubſequent ſtrokes add a leſs proportion of height to the mercurial cylinder, ſucceſſively; becauſe the more mercury is impell'd into the tube, the greater weight preſſes upon the included air; and becauſe the air hereby gains the more room, in the vial, to expand itſelf: whence the ſpring muſt be, proportionably, weakned.

Laſtly, in making of theſe trials, I obſerved the mercury in a good barometer, and found its greateſt height twenty-nine inches, and $\frac{3}{8}$; and ſoon after we had finiſhed, but twenty-nine.

To eſtimate the quantity of air, that had raiſed the quick-ſilver to twenty-ſeven inches; we counterpois'd the vial, employ'd about this experiment, whilſt it was empty; afterwards filled it with water, and found the liquor to weigh five ounces, two drams, and twenty grains; then having pour'd out the water, till it was ſunk to a mark, we made on the outſide of the glaſs, we weigh'd the remaining water, equal in bulk to the quick-ſilver, and found it one ounce, two drams, fourteen grains: ſo that the air, which had rais'd up the mercury, poſſeſs'd, before its expanſion, the ſpace but of four ounces, and a few odd grains in the vial. The bore of the pipe uſed in this experiment, was about $\frac{1}{8}$ inch in diameter.

Much included air raiſes mercury, but to the uſual ſtandard of the barometer.

2. Into a ſtrong glaſs bottle, capable of holding a quart, we put a convenient quantity of quick-ſilver, and erected in it a very long ſlender pipe of glaſs, open at both ends, and reaching with the lower beneath the ſurface of, the ſtagnant mercury; and having well cemented this pipe in the neck of the bottle, we convey'd the whole into a receiver, much larger than the former; and then the engine being work'd, the quick-ſilver was preſently rais'd to a greater height than before; and when it ſtood ſtill, we, by the help of ſome marks made before-hand on the pipe, and a very long and well-divided ruler, carefully meaſured the height of the mercurial

curial cylinder, which we found to be 29 inches and about $\frac{7}{8}$; but deduct- *Pneumatics.* ing half an inch, which was rais'd, before we employ'd the pump, by ſome air, that had been blown into the bottle, to try whether it were ſtanch, there remain'd 29 inches and near $\frac{1}{8}$ for the height of the mercury rais'd by the ſpring of the air ſhut up in the bottle; and then, conſulting the barometer, which ſtood in another part of the houſe, I found the weight of the atmoſphere ſuſtain'd a mercurial cylinder, of about twenty-nine inches, and a half.

We cauſed the pump to be well ply'd, to try whether the quick-ſilver would not riſe higher; but were confirm'd, that the ſpring of the air was inſufficient for that purpoſe.

The ſpring of included air raiſes mercury nearly to an equal height, in unequal tubes.

3. Taking the glaſs-bottle, uſed in the former experiment, and erecting in it, after the manner above-deſcribed, a cylindrical pipe of glaſs, much larger than the other; we proſecuted the experiment, as with the ſlender tube before-mention'd, and found, that, by the ſpring of the air in the bottle, the quick-ſilver was raiſed twenty-eight inches, and one eighth; that is, above an inch ſhort of the mercurial cylinder in the barometer, at the ſame time; a difference no greater than I expected; conſidering the weight of the atmoſphere, remains the ſame, when the mercury is at its full height, in a ſeal'd tube, whether great or ſmall; whilſt the ſpring of our included air muſt needs be weaken'd, the larger the tube, and the higher the mercury is impell'd in it. Whence, 'tis conſiderable, that the ſpring of ſo little air ſhould raiſe the mercury within an inch as high in a wide, as in a ſlender tube; for the diameter of the bore of the former, was double to that of the latter: and the greater mercurial cylinder may be ſuppoſed to have weigh'd near four times as much as the leſs; allowance being made for an inch difference in their heights. But, in caſe theſe had been equal, then the ſolidity of the cylinders would have been as their baſes; that is, as the ſquares of their diameters, or as 1 to 4.

We thought it worth trying, whether, when the included air had raiſed this great cylinder of mercury to the utmoſt height it could, by the ſpring it then had, heat would not force it ſtill higher. And, having cauſed a hot iron, and a ſhovel of kindled coals, to be held near the oppoſite parts of the receiver; we perceived, after a while, that the mercury aſcended one eighth of an inch, or more, above the greateſt height it had reached before; and, cauſing the pump to be ply'd again, to withdraw the air I ſuſpected to have ſtole in; the mercury was quickly raiſed five eighths of an inch, by virtue of the additional force which the included air acquired by the heat.

A fountain made by the ſpring of uncompreſſed air. Fig. 45.

4. We took a glaſs-bottle, furniſhed with a convenient quantity of water, and fitted it with a ſlender glaſs-pipe, about three feet long, open at both ends; which was ſo placed, that the lower orifice reached far beneath the ſurface of the water, and the pipe itſelf paſſed, perpendicularly, upwards, thro' the neck; which, by the pipe, and hard cement, was ſo firmly cloſed, that no water, or air, could get out of the bottle, or external air get into it, but by paſſing thro' the pipe. This inſtrument we

PNEUMATICS. we convey'd into a large receiver, ſhaped like a pear; of which a great part of the obtuſe end, and a ſmall portion of the ſharp one, were cut off by ſections parallel to the horizon. And, becauſe this receiver was not long enough to receive the whole pipe, there was cemented on, to the upper part of it, a ſmaller, of ſuch a length and bigneſs, that the higher end of the pipe might reach to the middle of its cavity; and that the motions of the ſpringing water might have a convenient ſcope, and be the better obſerved.

This double receiver, being cemented on to the engine, a little of the air was, by one ſtroke of the pump, drawn from it; by which, the preſſure of the remaining air being weaken'd, that included in the bottle, having not its ſpring, likewiſe, weaken'd, expanded itſelf; and, conſequently, impell'd up the water, in the ſame bottle, thro' the pipe, ſo as to make it ſtrike briſkly, at firſt, againſt that part of the top of the ſmall receiver, which was juſt over the orifice of the tube. But, after the water was, for a while, thus forced up, in a perpendicular line, it would be impell'd up leſs ſtrongly, and leſs directly, till the air, in the bottle, being as much expanded, as that in the receiver, it quite ceaſed to aſcend, unleſs by pumping a little more air out of the receiver, we renew'd it again. The
Fig. 46. other figure is deſigned to repreſent the difference that would happen, if, inſtead of making this experiment with water, it were made with quick-ſilver.

In making this experiment, 'tis convenient that the upper part of the pipe be very ſlender; whence the water, having but a very ſmall orifice to iſſue out at, may be ſpent but ſlowly, and thereby make the experiment laſt ſo much the longer: or, inſtead of making the upper part of the pipe ſlender; a top, conſiſting of three, or more, very ſlender pipes, with a ſmall hole at the end of each, may be cemented on to it; that one of theſe, pointing directly upwards, and the others to the right hand, and to the left, the water may ſpin out ſeveral ways at once; by which kind of branched pipes, we have, ſometimes, imitated a *Jet d'eau*, and the artificial fountains of gardens, and grotto's.

Hence we infer, that, had we not wanted convenient veſſels, we might, by the preſſure of the air, included in the bottle, have raiſed water fourteen times as high as we did quick-ſilver in the former experiment; ſince, upon weakening the preſſure of the air, but a little, in the double receiver, that within the bottle was able to impel the water, forcibly, and for a conſiderable time, to the top of a pipe a yard long, and higher.

Hence, too, it appears, that, in thoſe hydraulo-pneumatical engines, where water is placed between two parcels of air, the water may be put in motion, as well by the mere dilatation of one of the parcels, as by giving a new force by heat, or compreſſion, to the other. And, whether this mechanical principle of motion may not prove uſeful in engines, we leave to be conſider'd.

But if, when ſome of the air had been pumped out of the receiver, we removed that double veſſel from the bottle, the external air would, by its weight,

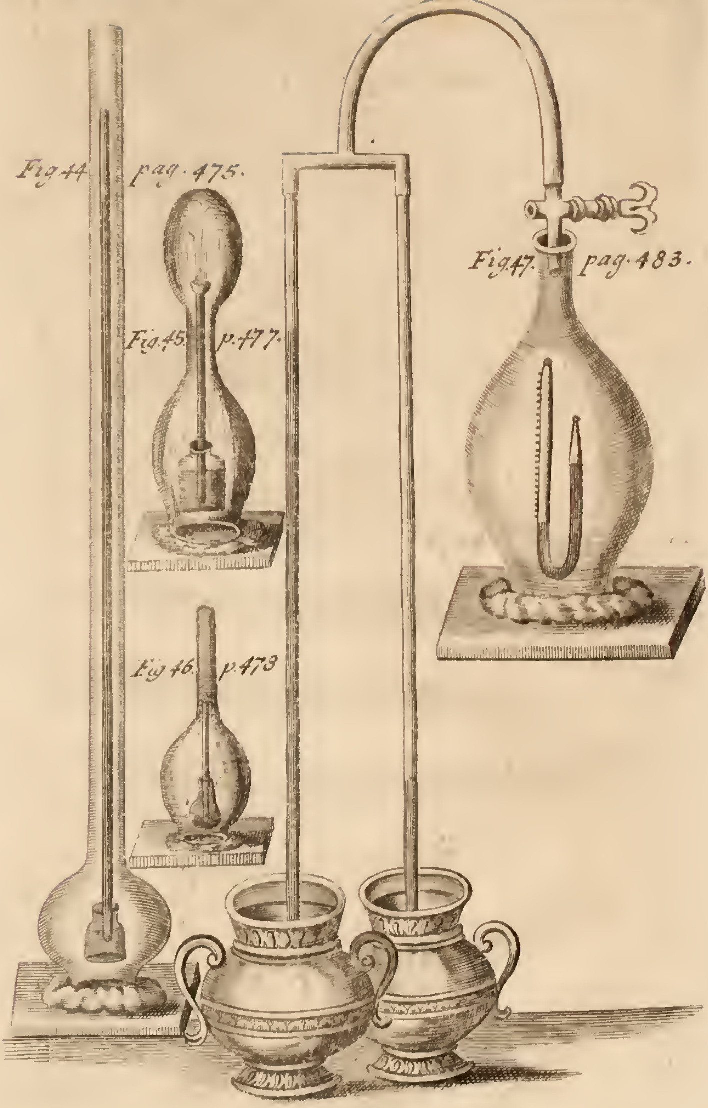


Fig. 44. pag. 475.
Fig. 45. p. 477.
Fig. 46. p. 478
Fig. 47. pag. 483.

weight, ſuddenly depreſs the water in the pipe, till, having driven it to the very bottom, it aſcended in numerous bubbles thro' the water, and joined it ſelf with the air incumbent on that liquor. 'Twas here obſervable, that all the external air, which got into the bottle, did not come in ſuddenly; but, after the firſt irruption, we could perceive, from time to time, new portions of air, leiſurely inſinuate themſelves thro' the pipe into the bottle, and emerge thro' the ſtagnant water in bubbles, that ſucceeded one another very ſlowly; as if the ſpring of the included air, having been once deprived of its natural conſtitution, by its late expanſion, could be but gradually reduced to it, by the weight of the atmoſphere, which was ſtill the ſame; or rather, as if between the ſpring of the included, and the preſſure of the external air, balancing each other, there happen'd ſome ſuch thing as is obſervable in ſcales, of which one is too much depreſs'd; whilſt the motion becomes ſlower, as the weights are nearer to an equilibrium.

But, our principal deſign, in this experiment, was to obſerve, whether the lines made by the water, in its efflux, would retain the ſame figure, notwithſtanding the rarifaction of the air, in the upper part of the receiver; and, for this purpoſe, it is beſt to make the obſervation towards the latter-end of the experiment; becauſe, then, the receiver being moſt exhauſted, the difference, made by the change of the denſity of the medium in which the ſtreams of water move, is likely to be beſt diſcern'd. And this convenience we had, by our way of making the experiment, that we could obſerve the lines, deſcribed by the flowing water, as the projection thereof grew fainter. But, for want of a large upper receiver, we could not be ſatisfied in the nature of the curve; tho' both Dr. *Wallis*, and my ſelf, found it to be, ſometimes, part of a parabola.

Flat glaſſes broke by the weight of the atmoſphere.

5. We provided a braſs ring of a conſiderable thickneſs, in height three inches; and the diameter of its cavity, as well at the upper as the lower orifice, was ſomething more than three inches. To this ring we ſucceſſively faſten'd, with cement, ſeveral round pieces of window-glaſs, and thereby made the ring a kind of receiver, whoſe open orifice we carefully cemented on to the engine; and found, that uſually, at the firſt exſuction, the glaſs plate would be broken inwards, with ſuch violence, as to be ſhatter'd into a great multitude of ſmall fragments; and the irruption of the external air, driving in the glaſs conſtantly, made a loud report, like that of a piſtol.

Without the aſſiſtance of a Fuga vacui.

6. If, inſtead of the braſs ring, above-mention'd, both orifices whereof are equal in breadth, you employ a taller hollow piece of braſs, or latton, ſhaped like a truncate cone; and the two orifices be made very unequal; as if the larger be as wide as that of our braſs ring, and the ſtraiter were leſs than an inch in diameter; and this piece of metal be made uſe of, as that in the preceding experiment the flat glaſs will be eaſily broken when cemented to the wider orifice: but, if the narrower orifice be turn'd upward, the glaſs thereon, if it be of a due ſtrength, tho' no thicker than the former, notwithſtanding the air is withdrawn from beneath it, will

remain

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remain entire: which ſufficiently argues, that nature's abhorrence of a vacuum, is not the cauſe why glaſſes are uſually broken in ſuch experiments, ſince, whether the wider, or narrower orifice be uppermoſt, and cover'd, the capacity of the exhauſted veſſel, will be equal; and therefore nature ought to break the glaſs, in one caſe, as well as the other.

This phenomenon, therefore, is more properly explain'd, by ſaying, that when the wider orifice lies uppermoſt, the glaſs that covers it, muſt ſerve for the baſis of a large column of the atmoſphere, which, by its great weight, may eaſily force thro' the glaſs; whereas, when the ſmaller orifice is uppermoſt, there reſts upon its cover, ſo ſlender a pillar of air, as cannot, by its weight, ſurmount the natural coheſion of the parts of the glaſs.

Blown bladders burſt by the ſpring of the air included in them.

7. We ſeldom fail'd of burſting blown bladders in our exhauſted receiver, by tying their necks very cloſely, and keeping them, for a pretty while, in the glaſs, whilſt the air was exhauſting, and then taking them out again; that the fibres being ſtretch'd, and relax'd, and the capacity diminiſh'd by a new ligature, tho'the air were the ſame,and the membrane being not ſo able to yield, as before; upon the ſecond exhauſtion of the receiver,they would break far more eaſily, than otherwiſe; and ſometimes be oddly lacerated.

A conſiderable weight lifted by the bare ſpring of a little air, included in a bladder.

8. We took a middle-ſized bladder, and having preſs'd out the air, till there remain'd but about a fourth or fifth part, we caus'd the neck to be very ſtrongly ty'd again; and, about the oppoſite part of the bladder, within an inch of the bottom, we ſo ſtrongly tied another ſtring, that it would not be ſlip'd off, by a conſiderable weight hung at it. Then faſtening the neck to the turn-key, we convey'd the bladder, and the weight hanging at it, into a large receiver; when, by plying the pump, the air, within the bladder, being freed from the preſſure of the air without it, manifeſtly ſwell'd by its own ſpring, and thereby greatly ſhortned the bladder that contain'd it, and lifted up the weight, which exceeded 15 pounds.

After this, we took a large bladder, and having let out ſo much air, that it was left lank, we faſten'd the two ends of it to the upper part of the receiver, and hung a weight from the middle of the bladder; then exhauſting the receiver, as before, tho' the bladder, and this new weight, which ſtretch'd it, reach'd ſo low, that, for a while, we could ſcarce ſee whether it hung in the air or no; yet, at length, we perceiv'd the bladder to ſwell, and concluded it had lifted up its clog about an inch; as was confirm'd by the return of the air into the receiver; upon which, the bladder became more wrinkled than before; and the weight, amounting to about 28 pounds, deſcended.

Perhaps this experiment may conduce to explain muſcular motion *.

* Something has, from this hint, been offer'd, with a very ſpecious and plauſible ſhew of reaſon, to account for muſcular motion; but when thoroughly conſider'd, it fails in ſolving the phenomenon. And the laſt beſt writer on this ſubject, the learned Dr. *Pemberton*, after ſhewing the inſufficiency of all other methods, accounts for it, from that ſubtile medium whereby the great Sir *Iſ. Newton* ſolves various other phenomena of nature.

A

9. A large glaſs bubble, hermetically ſeal'd, being put into the receiver, and the air drawn out ſomewhat more than uſual; tho' I had, ſeveral times, obſerved, that ſuch bubbles would not break immediately, upon evacuating the receiver; yet this continued ſo long entire, after we had left off pumping, that preſuming it had been blown too ſtrong, I began to deſpair of ſucceſs in the experiment; when, about four minutes after the pump had been let alone, the bubble ſurpriz'd us with breaking ſo violently, by the ſpring of the included air, that the fragments of it were daſh'd every way againſt the ſides of the receiver, and broke to powder.

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10. We took the braſs-ring, lately mention'd, whereto were fitted ſome plates of window-glaſs, as covers; and, having carefully faſten'd one of them, with cement, to the upper orifice of the ring; and cementing the lower orifice to the engine, ſo that the veſſel, compoſed of metal and glaſs, ſerv'd for a ſmall receiver, we whelm'd another over it that was large and ſtrong; which was alſo faſten'd to the engine, with cement, after the uſual manner. By this contrivance, when the pump was ſet on work, the ſmall included receiver muſt have its air withdrawn, while that, in the larger, could not get out, but by breaking through the glaſs; ſo that the internal air of the ſmall receiver, being evacuated, the glaſs plate, that made part of it, muſt lie expoſed to the preſſure of the ambient air, ſhut up in the other receiver, without having the former aſſiſtance of the air, now withdrawn, to reſiſt the preſſure; wherefore, at the firſt or ſecond exſuction of the air, included in the ſmall receiver, the glaſs plate was, by the preſſure of the incumbent air, contain'd in the larger one, broken into a hundred pieces, which were beaten inwards into the cavity of the ring.

The external force of the ſpring of uncompreſs'd air upon ſolid bodies.

But to ſhew that there needed not the ſpring of ſo great a quantity of included air, to break ſuch glaſſes, we took another roundiſh one, which, tho' wide enough at the orifice, to cover the braſs ring, and the new glaſs plate, that we had cemented on it, was yet ſo low, that it held but a ſixth part of what the large receiver, formerly employ'd, would contain; and having whelm'd this veſſel, which was ſhaped like a tumbler, over the little receiver, and well faſten'd it to the engine with cement, we found, that tho' the external receiver had a great part of its cavity fill'd by that included; yet when this internal one was evacuated, by an exſuction or two, the ſpring of the little air that remain'd, broke the plate into a multitude of fragments.

And becauſe the glaſs plates, hitherto mention'd, ſeem'd not ſo thick, but that the preſſure of the included air might give greater inſtances of its force; inſtead of the ſmall metalline receivers, before employ'd, we took a ſtrong, ſquare bottle of glaſs, able to contain a pint, inverted it, and applied it to the engine, as a receiver; over which we whelm'd, and cemented the large one, formerly mention'd; and ſetting the pump on work, to empty the ſquare bottle, the figure of the veſſel allow'd the preſſure of the air, included in the external receiver, to cruſh it into a great number of pieces.

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We, also, took another glass, of the shape, and about the bigness of the former; and having applied it to the engine, as before, and cover'd it with a receiver, that was a little higher than itself; upon exhausting the air, this was, likewise, broken into many fragments, some of them very thick: tho', probably, the cracks that reach'd thereto, were begun in much weaker parts of the glass.

The bottoms, and the necks of both these square bottles, were entire; by which it seem'd probable, that the vessels had been broken, by the pressure of the air against the sides, which were not only thinner than the other parts, but expos'd a larger superficies to the lateral pressure of the air, than to the perpendicular. We observ'd, in one of these experiments, that the vessel did not break presently, upon the last exsuction of the included air, but a considerable time after.

To confirm that it is the spring of the air, in the external receivers, that breaks the glasses, and to prevent some scruples, we apply'd a plate of glass, like those formerly mention'd, to the brass ring; but, in the cementing of it on, we placed, in the thickness of the cement, a small pipe of glass, about an inch long, whose cavity was not so big as that of a straw, and which, being left open at both ends, might serve for a little channel, for the air to pass thro', from the external receiver, to the internal; over this we whelm'd a small receiver, and then, tho' we work'd the pump much longer than would have been necessary, if the little pipe had not been made use of, we found the internal receiver continue entire; because the air, whose spring should have broken it, having liberty to pass thro' the pipe, and, consequently, to expand itself, into the place deserted by the air pump'd out, thereby weaken'd its spring too much for that purpose.

But, either the pipe must be made bigger, than that lately mention'd, or the exsuction of the air must not be sudden, by the pump; otherwise the plate of glass may be broken, notwithstanding the pipe: because the air contain'd in the external receiver, having a force much greater than is necessary to break such a plate, it may well happen, as I have sometimes found it, that if the air be hastily drawn out of the internal receiver, that which should succeed in its room, cannot get fast enough out of the external receiver, thro' so small a pipe; whilst the air remaining in the same, will yet retain a spring strong enough to break the glass. Thus, sometimes, when at the flame of a lamp, glass-bubbles are blown with slender stems; if they be suddenly remov'd out of the flame, they either break, if cool'd too fast; or are compress'd inwards, if they long retain the softness given them by fusion. For the air in the bubble, being exceedingly rarified, and expanded, whilst the glass is kept in the flame, and coming to cool hastily, when remov'd from thence, loses, upon refrigeration, the spring which the heat had given it; and so, if the external air cannot press in fast enough, thro' the too slender pipe, a sufficient quantity of air will not get in to resist the pressure of the atmosphere; and therefore, if this pressure find the bubble yet soft, it will press it a little inwards, and either

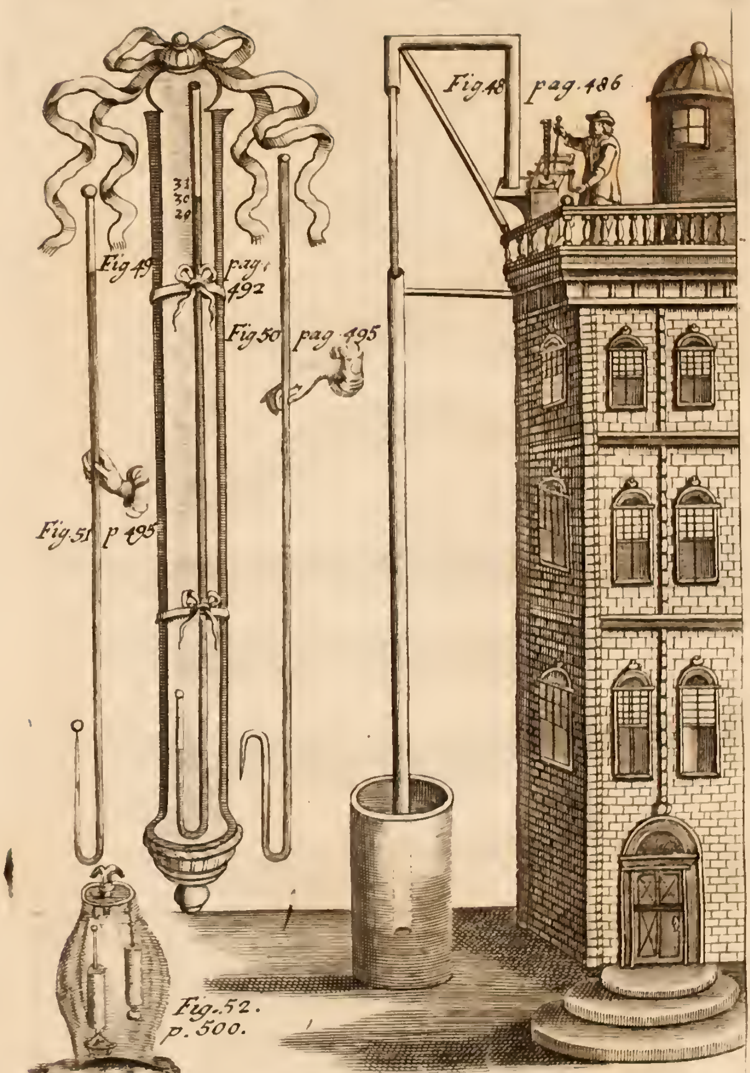
Fig.48 pag.486
Fig.49 pag. 492
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Fig.52.
p.500.

either flatten it, or make a dent, though the orifice of the pipe be left open. PNEUMATICS.

II. We took a [illegible] brass pipe, bent like a siphon, and fitted at the bigger end with a stop-cock, [illegible] &c. and to the slender end of this, we fasten'd, with cement, the upper end of a cylindrical glass pipe, about fifty inches long, open at both ends, and having the lower plung'd into a [illegible] vessel of stagnant quick-silver, whose upper superficies reach'd considerably higher than the immers'd orifice of the glass tube: then, causing the pump to be work'd, the air was, by degrees, drawn out of the siphon, and, conseqently, out of the glass tube that open'd into it; and the stagnant mercury, proportionably impell'd up into the glass tube, till it had attain'd to its due height, which exceeded not thirty inches. And, then, tho' there remain'd in the upper part of the pipe, above twenty inches unfill'd, with quick-silver, we could not, by further pumping, raise it higher.

Mercury rises no higher by suction, than the weight of the atmosphere impels it.

Fig. 47.

Hence it appears, that the fancied power of nature, to prevent a vacuum, has its bounds; and those depending upon the specific gravity of the liquor, to be rais'd by suction. For, substituting, instead of the stagnant mercury, a bason of water; and, instead of the many strokes, in vain employ'd, to raise the quick-silver above the height just mention'd, making scarce one exsuction, which only, in part, emptied the siphon; yet the water, upon opening the stop-cock, was not only impell'd to the very top of the glass tube, but continu'd running, for a considerable time, thro' the siphon, and thence fell upon the plate of the engine: so that it appear'd strange to those, who knew not the reason of it, that the water should run very briskly, of its own accord, out of the leg of a siphon; which, perhaps, was not above a quarter so long as the other. I must not here omit, that tho', sometimes, in the *Torricellian* experiment, I have observ'd the mercury to stand at thirty inches, and, now and then, above it; yet the height of the mercury in our glass tube, appear'd not to reach full twenty-nine inches, and a quarter. But, consulting the barometer, I found the quick-silver at twenty-nine inches, and one eighth; which, probably, would have been the very height of that, rais'd by the engine, had it been freed from bubbles.

Hence we may conclude, that suction will elevate liquors in pumps, no higher than the weight of the atmosphere is able to raise them; since the closeness requisite in the pump of our engine, makes it very unlikely, that a more accurate suction can be effected by an ordinary pump.

Tho' the exhausting siphon, used in this experiment, may be easily conceiv'd by an attentive inspection of the figure; yet, because I frequently employ it in pneumatical experiments, 'tis proper to intimate, once for all, that though the bended pipe itself, may be, on some occasions, more conveniently made of glass, for the sake of transparency; yet, for the most part, we chose to employ pipes of brass, because the others are so very subject to break; that 'tis convenient to make the longer leg of the siphon, a little larger at the bottom, than the rest of the pipe usually needs

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to be, that it may the more commodiouſly admit the ſhank of a ſtop-cock, which is to be very carefully inſerted, with cement; by ſeaſonably turning and returning whereof, the paſſage between the engine and the veſſel to be exhauſted, is to be open'd and ſhut; and, laſtly, that tho' we ſometimes immediately apply the braſs ſiphon to the engine by cementing the external ſhank of the ſtop-cock to the orifice of the little pipe, thro' which, the exſuction of the air is made; yet the bended pipe alone, is ſo apt to be looſen'd by the motion of the engine and the turning of the ſtop-cock, that, for the moſt part, we uſe a ſiphon conſiſting of a braſs-pipe, a ſtop-cock, and a glaſs eight or ten inches high, and of ſome ſuch ſhape as is expreſſed in the figure; for, by this means, tho' the exhauſtion is longer in making, yet it is more ſecurely and uninterruptedly carried on; becauſe of the ſtability, which the breadth of the lower orifice of the glaſs gives to the whole inſtrument. Beſides, not only the ſiphon is thus much lengthen'd, but we may commodiouſly place a gage in the glaſs part of this compounded ſiphon, to ſhew, from time to time, how far the air is drawn out of the veſſel to be exhauſted.

Liquors aſcend to different heights by ſuction, according to their ſpecific gravities.

12. I caus'd to be made and inſerted to the ſhorter leg of the above-mention'd ſiphon a ſhort pipe, which branch'd itſelf equally to the right hand and to the left; ſo that I might exhauſt two glaſs tubes, at the ſame time, and prevent any ſuſpicion, that the engine was not equally applied to both. This additional braſs pipe, being carefully cemented into the ſiphon, to each of its two branches were well faſten'd, with the ſame cement, a cylindrical glaſs of about forty two inches in length; the lower orifice of one of theſe glaſſes being immers'd in a veſſel of ſtagnant mercury, and that of the other in a veſſel of water; when care was taken, that as the tubes were choſen near of a ſize, ſo the ſurfaces of the two different liquors ſhould be near of a height. This being done, we began to pump warily and ſlowly, till the water in one of the pipes was elevated about forty-two inches; and then meaſuring the height of the quick-ſilver in the other pipe above the ſurface of the ſtagnant mercury, we found it to be almoſt three inchees, ſo that the water was about fourteen times as high as the quick-ſilver. And, to proſecute the experiment further, we very warily let in a little air to the exhauſting ſiphon, and ſaw the two fluids proportionably deſcend; till turning the ſtop-cock, when the water was about fourteen inches high, we thereby kept them from ſinking any lower, before we had meaſur'd the height of the quick-ſilver, which we found to be about one inch.

But, we obſerv'd, that the quick-ſilver, for the moſt part, ſeem'd to be a very little higher, than the proportion of one to fourteen required; and accordingly, I had long before, by particular trials, found, that, tho' fourteen and one be the neareſt of ſmall integer numbers, that expreſs the proportion between the ſpecific gravities of mercury and water; yet the former is not quite ſo heavy as this proportion ſuppoſes.

This experiment evidently ſhews that the fluids roſe by the weight of the air, and leaves no pretence of a *Fuga vacui*. It may alſo be made uſeful to eſtimate the different gravities of liquors: for which purpoſe,

pose, I caus'd the afore-mention'd glass pipes, to have their ends plung'd, the one in fresh water, and the other in some impregnated with a large proportion of sea-salt; and found, that when the fresh water was rais'd to about forty-two inches, the saline solution had not fully reach'd to forty.

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But, to make the disparity more evident, I prepar'd an unusual brine, by suffering sea-salt to dissolve in the moist air: and, having apply'd this liquor, and fresh water to the two pipes, and proceeded after the former manner; we found, that when the pure water was elevated to near forty-two inches, the liquor of sea-salt wanted about seven inches and one fourth of that height; and when the water was made to subside to the middle of its pipe, the saline liquor in the other pipe was between three and four inches lower than that. I also took fair water, and a liquor made of the salt of pot-ashes suffer'd to run *per deliquium*, and proceeding as before, found, that when the common water was about forty-two inches high, the solution wanted of thirty inches; and when the water was made to subside to the middle of its tube, the other liquor was between six and seven inches lower.

The heights whereto water and mercury may be rais'd by the spring of the air.

13. We took a strong glass bottle, that would contain above a pint, and having in the bottom of it lodg'd a convenient quantity of mercury, we pour'd on it a greater quantity of water; and providing two slender glass pipes, open at both ends, we so plac'd and fasten'd them close by cement, that the shorter of the pipes had its lower orifice immers'd beneath the surface of the quick-silver, and the longer reach'd not quite so low as that surface, and so was immers'd but in the water. This done, we convey'd the bottle into a proper receiver, and having begun to pump out the air; we took notice to what heights the quick-silver and water were impell'd up in their respective tubes, on which, we had before made marks; and found, that when the quick-silver was impell'd up to two inches, the water was rais'd to about twenty-eight; and when the quick-silver stood at about one inch, the water stood at about fourteen.

And the heights whereto they will subside upon withdrawing it.

14. We convey'd into a fitly shaped receiver two glass pipes very unequal in length; but each of them seal'd at one end: the shorter tube was fill'd with mercury, and inverted into a small glass jar, wherein a sufficient quantity of that fluid had been before lodg'd, the longer pipe was fill'd with common water; and inverted into a larger glass, which likewise contain'd a fit proportion of the same liquor. Then the receiver being closely cemented to the engine, the air was pump'd out for a pretty while before the mercury began to subside; but when it was so far withdrawn, that its pressure could no longer keep up a mercurial cylinder of that height, the quick-silver began to sink; the water in the other tube, tho' three times as long, still retaining its full height. But when the quick-silver was fallen to between three and four inches above the surface of that in the vessel, the water also began to subside; but sooner than according to the laws of staticks it ought to have done: because many aerial particles emerging from

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The greateſt height to which water can be rais'd by attraction, or ſucking-pumps. Fig. 48.

15. Having procured ſeveral tin pipes above an inch in bore, very carefully ſolder'd together, to make one whole tube, about thirty-two feet long; and cas'd it over firſt with cement and then with plaiſter of *Paris*; we very carefully cemented a ſtrong pipe of glaſs, between two and three feet in length to the upper part of it; and to the upper end of this pipe, by means of cement and a ſhort elbow of tin, we very cloſely faſten'd another pipe of the ſame metal, conſiſting of two pieces making a right angle; whereof the upper part was parallel to the horizon, and the other, which lay parallel to the glaſs pipe, reach'd down to the engine that was placed on the flat roof of a houſe thirty feet high from the ground, and was to be cemented to the lower end of this deſcending part of the pipe, whoſe horizontal leg reſted upon a piece of wood nail'd to the rails on the top of the building: the tube, alſo, was kept from ſhaking by a board faſten'd to the ſame rails, with a deep notch for it to be inſerted in.

This apparatus being made, and the whole tube, with a pole to ſuſtain it, erected along the wall, faſten'd there, and the deſcending pipe carefully cemented on to the engine; there was placed under the bottom of the long tube a convenient veſſel, whereinto ſo much water was pour'd, as reach'd far above the orifice of the pipe; and providing, that the veſſel might ſtill be kept competently full, we, at length, rais'd the water to the middle of the glaſs pipe; but not without numerous bubbles, made by the air conceal'd in the pores of the water, which, for a time, kept a kind of foam upon the ſurface of it. And finding the engine, and tube as ſtaunch as could be expected; I thought fit to try what was the utmoſt height, to which, water could be elevated by ſuction: and therefore, tho' the pump ſeem'd to have been ſufficiently ply'd already; yet, for further ſatisfaction, when the water was within a few inches of the top of the glaſs, I caus'd twenty exſuctions more to be ſuddenly made. And, having taken notice where

where the surface rested, we measured the height of the cylinder of water, and found it thirty-three feet, and about six inches; the barometer then standing at twenty-nine inches, and between two and three eighths of an inch. Now, supposing the specific gravity of water, to that of quick-silver, as 1 to 14; the height of the water ought to have been thirty-four feet, and about two inches; that is, about eight inches more than we found it. But, then, I formerly noted, that the proportion betwixt mercury and water, is not altogether so great; and, therefore, in so tall a cylinder as ours was, the difference must be considerable. If, therefore, instead of making an inch of quick-silver, equivalent to fourteen inches of water, we abate a quarter of an inch; which is but a fifty-sixth part of the height of the water; this abatement, being repeated twenty-nine times and one quarter, will amount to seven inches, and above a quarter; which, added to the former height of the water, thirty-three feet, six inches, will make thirty-four feet, and above an inch: so that the difference between the height of the mercury, sustain'd by the weight of the atmosphere in the barometer, and that of the water, rais'd, and sustain'd, by the pressure of the same in the long tube, did not appear to differ more than an inch or two, from the proportion they ought to have, according to their specific gravity: nor could we, by obstinately plying the pump, raise the water higher. PNEUMATICS.

This experiment, being soon repeated, in my absence, by Dr. *Wallis*, Dr. *Wren*, and Dr. *Millington*; they, presently after, assured me, that the greatest height, whereto they could raise the water, was thirty-three feet and a half: and, as it happen'd, within less than an hour before, I had observed the barometer to stand somewhat below twenty-nine inches, and three eighths; when, now, consulting the same instrument again, the mercury appear'd to be risen a little higher. Hence appears the impossibility of making water pass over the highest mountains, by the help of inflected pipes, and suction. For, if the water be to rise above thirty-five, or thirty-six feet, a sucking-pump will not, ordinarily, here in *England*, suffice for that purpose.

16. To try whether the air contributes to the elasticity of bodies, we took a piece of whale-bone, of a convenient length, and, having fasten'd one end of it into a thick heavy trencher, to be placed on the plate of the engine; to the other end we tied a weight, whereby the whale-bone was moderately bent, which reached down to a flat body, placed under it, so that if the spring were but a little weaken'd, the weight must either rest upon, or touch the horizontal plane; or if, on the other side, the spring should grow sensibly stronger, it might be easily perceived, by the distance of the weight, which was so near the plane, that a little increase of it must be visible. These things we convey'd into the receiver, and took care to shake the engine as little as possible, that the weight might not hit against the body which lay under it; or, we be hinder'd from discerning, whether it were depressed by the bare extraction of the air. And, when the air had been well pumped out, I watched attentively, whether any notable change, in the distance of the weight from the plane, would happen upon *An elastic body bent in the exhausted receiver.*

its

Pneumatics. its being let in again; for the weight was then at rest: and the returning air, flowing in much faster than it could before be drawn out, this seem'd the likeliest time to discover, whether the absence of the air had, sensibly, alter'd the spring of the whale-bone. But, tho' the experiment were made more than once, I could only satisfy myself, that the depression, or elevation, of the weight, owing to the mere change of the spring, was not very considerable; for I do not think myself sure, that I perceived any at all: tho', sometimes, when the receiver was well exhausted, the weight seem'd to be a little depress'd; yet this, I thought, might well be ascribed to the absence of the air, not consider'd as a body that had any thing to do directly with the spring, but as a body that had some gravity; whereby it made the medium, wherein the experiment was try'd, contribute to support the weight that bent the spring; which weight, when the air was absent, must have its gravity increased, by as much weight, as a quantity of the exhausted air, equal to it in bulk, amounts to.

To make gages for estimating how far the receiver is exhausted.

17. The air, being invisible, it is not always easy to know, whether it be sufficiently pumped out of the receiver, to be exhausted; we, therefore, thought it very convenient to have some instrument within the receiver, that might serve for a gage, or standard, whereby to judge when it was sufficiently evacuated. The first attempt, made to this purpose, was by means of a bladder, very strongly tied at the neck; after having had only so much air left in its folds, as might fully distend it, when the receiver was very well exhausted. And this way, in some cases, is useful; but, in others, a bladder takes up too much of the receiver, and hinders the objects from being observ'd on all sides.

Another sort of gage we made with quick-silver, pour'd into a very short pipe, which was, afterwards, inverted into a little glass of stagnant quick-silver, as in the *Torricellian* experiment. For this pipe, being but a very few inches long, the mercury in it would not begin to descend, till a very great proportion of air was pumped out of the receiver; because, till then, the spring of the remaining air would be strong enough to sustain so short a cylinder of mercury. And this kind of gage is no bad one. But, because it cannot easily be suspended, and the mercury in it is apt to shake, by the motion of the engine, another was substituted in its place, consisting of a kind of siphon, to the shorter leg whereof belong'd a large glass-bubble.

But none of these gages having the conveniences, that some of our experiments require; I devised another, after the following manner.

Fig. 47. Take a cylindrical pipe of glass, six, eight, ten, or more inches in length, and not so thick as a goose-quill; and, by the flame of a lamp, melt it, but not too near the middle, and make it into a siphon; the legs whereof are to be parallel, and as near to each other, as possible. In one of these legs, usually the longer, leave at the top, either half an inch, or a whole inch, more or less, according to the length of the gage, or the design of the experimenter, of air in its natural state; and fill the rest of the longer leg,

leg, and as great a part of the ſhorter as ſhall be thought proper, with quick-ſilver. This done, there may be marks placed on the outſide of the longer, or ſeal'd leg, whereby to meaſure the expanſion of the air included therein.

This inſtrument, being convey'd into a receiver, and the air very diligently pumped out, notice muſt be taken, to what part of the gage the mercury is depreſſed, that we may know, when the mercury ſhall, afterwards, be driven ſo far, that the receiver, wherein the gage is placed, is well exhauſted. And if it be deſired to know, more accurately, what ſtations of the mercury, in the gage, are anſwerable to the degrees of the rarifaction of the air in the receiver; this may be gain'd, by letting in water, as often as is neceſſary, into a receiver, whoſe entire capacity is firſt meaſured; and in which there might be marks made, to ſhew when the water to be let in, ſhall have fill'd a fourth, a half, *&c.* of the cavity. For if, when the quick-ſilver in the gage, is depreſſed to a certain mark, you let in water, which appears to fill a fourth part of the receiver; you may conclude, that about one fourth of the air was pumped out; or that a fourth of the ſpring of the whole included air was loſt. And if the water either falls conſiderably ſhort of, or exceeds the quantity expected; you may, the next time, let in the water, either after the mercury has a little paſs'd the former mark, or a little before it is arrived at it. And when once you have, this way, obtain'd one long, and accurate gage, you may divide others by the help of this, placed with them in a ſmall receiver: when, the mercury in the former, being depreſſed to any determinate diviſion, obtain'd by obſervation; you may, thence, conclude, how much the air, in the receiver, is rarify'd; and, conſequently, by taking notice of the place where the mercury reſts in the other gages, determine what degree of exhauſtion, in a receiver, is denoted by that ſtation of the mercury.

That leg of the gage which includes the air, may be ſeal'd up, either before the pipe is bent into a ſiphon; or, which is much better, by firſt drawing out that end of it you deſign ſhall be ſeal'd, to a ſhort, and very ſlender thread: then, having made the tube into a ſiphon, pour into the leg, which is to remain open, as much quick-ſilver as you judge convenient, which will riſe to an equal height in the other leg; and, by gently inclining the ſiphon, you may pour the ſuperfluous mercury out of it, if there be any; and when there is an inch, or the proper ſpace, unfill'd with mercury, next the end that is to be cloſed; and the reſt of that leg, and as much of the other as is neceſſary, fill'd with quick-ſilver; you may, by keeping the ſiphon in the ſame poſture, and warily applying the ſlender apex, above-mention'd, to the upper part of the flame of a lamp, blown horizontal, conveniently ſeal it up.

But there are ſome experiments, wherein it is not neceſſary that the receiver ſhould be fully exhauſted; but, rather, that the degrees of the air's rarifaction ſhould be well meaſured. And, in many caſes, we may uſe gages, ſhaped like thoſe hitherto deſcribed, made as long as the receiver will admit, and furniſh'd, inſtead of quick-ſilver, either with tinged ſpirit

 of

Pneumatics. of wine; or else the tincture of red rose-leaves, drawn with common water, and heighten'd with a little spirit of vitriol. For the lightness of these liquors, in comparison of quick-silver, will allow the expansions of the air, included in the gage, to be very manifest; tho', perhaps, a quarter of the air be not pumped out of the receiver.

We may, also, in such cases, and where the receiver is sufficiently large, and not to be quite exhausted, make use of a mercurial gage, differing from the former in this, that the shorter leg need not be above an inch, or half an inch long, before it widens into a bubble, about half an inch, or an inch in diameter; and having, at the upper part, a very short and slender open pipe, whereat the air may get in and out: and here we need not include so much air as, otherwise, would be requisite, at the top of the longer leg; because the mercury, in the shorter, cannot, by reason of the breadth of the bubble, into which the expansion of the air drives it, be considerably raised; whereby the degrees of the included air's rarifaction become very visible.

An easy way to make the pressure of the air sensible to the touch. 18. I caused a hollow strong piece of brass to be made, two or three inches high, opening, at both ends, in orifices circular and parallel, but not equal; which, being cemented, as a small receiver, to the engine; whoever doubted the pressure of the air to be considerable, needed only lay the palm of his hand upon the upper orifice, and press it close thereto: for, upon withdrawing, by a single stroke, the greatest part of the pressure of the internal air, that, before, counter-balanced the external; the hand, being left alone, to support the weight of the atmosphere, would be press'd inwards very forcibly; especially, if, by a second stroke of the pump, the little receiver were farther exhausted: and this pressure continues, till the air be re-admitted into the receiver. If a more sensible conviction be desir'd, tis easy to give it, by turning the larger orifice uppermost, and proceeding, as before; but this ought not much to exceed two inches and a half in diameter, lest the great weight of the air should break, or considerably hurt the hand: as I once much endanger'd my own, thro' mistake of the pumper, who fell to his work, while I held it upon the orifice of a vessel too large in diameter.

Mercury subsiding in the Torricellian tube to a level with the stagnant, by extracting the air. 19. A barometer being included in a receiver, made of a long bolt-head, with the lower part of the ball cut circularly off; upon the first exsuction of the air, the quick-silver, that before stood at twenty-nine inches, would fall, and rest, at nine or ten inches; and, in about three strokes more, it would be brought quite down to the level of the stagnant quick-silver, and somewhat below: but the air, being let into the receiver, the mercury would be impell'd up slow, or fast, as we pleas'd, to the former height of twenty-nine inches.

If the air were suffer'd to go hastily out of the receiver, the mercury would, at the very first stroke, descend, till it reach'd within an inch or two of that in the vessel; tho' it would, presently after a few risings and fallings, settle at the height of nine, or ten inches, till the next stroke brought it down lower.

PNEUMATICS.

And if, when the mercury was re-impell'd up to its due height, instead of rarifying the air, it were a little compress'd; the quick-silver would be easily made to rise an inch, or more, above the former standard of twenty-nine inches.

We, also, took a glass-tube, seal'd at one end, much shorter than the due length, and having fill'd it with mercury, and inverted it into a vessel of stagnant mercury, we placed all in the former receiver; where the mercurial cylinder, for want of the requisite height, remain'd totally suspended; but, upon the first, or second stroke, subsided, and, after two or three more, fell to a level with the stagnant mercury, or a little below it: and, upon the letting in the air, it would be again impell'd to the very top of the tube, bating an aerial bubble, which seem'd to come from the mercury itself; and was so little, as not to be at all discernible, but to a very attentive eye.

In small and large open tubes, when no Fuga vacui *can be pretended, the weight of water raises quick-silver to an equal height.*

20. Into a very large glass-tube, hermetically seal'd at one end, and about two feet and a half in length, we pour'd quick-silver, to the height of three or four fingers; then we took two cylindrical pipes, of very unequal bores, and open at both ends, and plung'd the lower ends of both into the quick-silver; fastening them to the former tube, that they might not be mov'd out of their posture; in which the convex surface of the mercury, in both, seem'd almost to lie in a level; the tube, also, being placed, perpendicularly, in a frame: then, by the help of a funnel, we pour'd water, by degrees, in at the top of the tube; and observ'd, that, as the water gravitated, more and more, upon the stagnant mercury; so the included mercury rose equally, in both the pipes; till the tube, being almost fill'd with water, the mercury appear'd to be impell'd, and sustain'd in both, at the height of about two inches above the surface of the stagnant quick-silver. And, having caus'd about half the water, in the large tube, to be suck'd out at the top; we observ'd the quick-silver, in both the others, to subside uniformly, and to re-ascend alike upon the re-affusion of the water.

We, also, took a very wide tube of glass, a foot long, and pour'd into it a convenient quantity of quick-silver; then we took two pipes, of an equal length, but unequal bores, as before; and these, being fill'd with quick-silver, as in the *Torricellian* experiment, were let down into the tube, and unstopp'd, under the surface of the stagnant mercury: when, that in the pipes, falling to its wonted station, and resting there, we pour'd into the tube about a foot height of water, whereby the quick-silver appear'd equally impell'd above its station, and sustain'd there, in both the pipes; and, upon withdrawing some of the water, it began to subside alike, as to sense, in both: and water, being a second time pour'd down into the tube, the mercury, in both pipes, rose uniformly, as before. By which, and the former experiment, it appears, that a gravitating liquor, as air, or water, may impel, or sustain mercury, at the same height, in tubes of very different capacities; and that liquors balance each other, according to their altitude, and not barely according to their weight. For, PNEUMATICS. in the laſt experiment, the additional cylinder of one inch of mercury, was manifeſtly rais'd, and kept up by the water incumbent on the ſtagnant mercury. And the ſame parcel of water counterpois'd, in the different pipes, two mercurial cylinders, which, though of the ſame altitude, were very unequal in weight.

The height whereat pure mercury, and mercury amalgamated with tin, will ſtand in barometers.

21. Amalgamating mercury with a convenient proportion of pure tin, that the mixture might not be too thick, we therewith fill'd a cylindrical pipe, ſeal'd at one end, and of a fit length; and then inverted it into a little glaſs, furniſh'd with the like mixture. The event was, that the amalgam did not fall down to twenty-nine, but ſtop'd at 31 inches, above the ſurface of the ſtagnant parcel. Hence, it appears, that the height of the liquor, ſuſpended in the *Torricellian* tube, depends ſo much upon its equilibrium, with the external air, that it may be varied as well by a change of gravity in the ſuſpending liquor, as we formerly ſaw it might by an alteration in the atmoſphere.

It might be worth while to try, by comparing the height of the amalgam to what it ought to be by the ſpecific gravities of the mercury, and the tin mix'd in a known proportion, whether theſe metals penetrate each other, in the ſame manner as copper and tin have been obſerv'd to do; when being melted down together, they make a more cloſe and ponderous body than their reſpective weights ſeem'd to require.

To make portable barometers.

Fig. 49.

22. We took a hollow cylinder of glaſs, ſeal'd at one end, and four or five feet in length; and, by the flame of a lamp, bent it after the manner of a ſiphon, one of the legs whereof is three or four times longer than the other; whence the ſhorter leg may ſerve, inſtead of the veſſel, uſually employ'd to contain the ſtagnant mercury. To fill this, take a ſmall glaſs funnel, with a long and ſlender ſhank, ſo that it may reach three or four inches, or farther, into the ſhorter leg of the barometer; and, by the funnel, pour into the ſhorter leg, as much mercury as may reach about two or three inches, in both legs; then ſtopping the orifice with your finger, and ſlowly inclining the tube, the mercury, in the longer leg, will fall to the ſeal'd end, and the air that was there before, paſs by, and give it room. The mercury, in the ſhorter leg, which ought to be held uppermoſt, will, by the ſame inclination of the tube, fall towards the orifice; but being, by the finger, kept from falling out, if you ſlowly erect the glaſs again, and then ſtop it, as before, the mercury will paſs out of the ſhorter leg into the longer, and join with that which was there before: and if all the mercury do not ſo paſs, the orifice is to be ſtop'd again with the finger, and the tube inclin'd as formerly. This done, the tube is to be erected, and, by the help of the funnel, more mercury is to be pour'd in; and the ſame proceſs of ſtopping the orifice, inclining the tube, &c. is to be repeated, till all the mercury, pour'd into the ſhorter leg, be brought to join with that in the longer; and then the open leg is to be furniſh'd with freſh mercury; obſerving that the nearer the longer leg comes to being fill'd, the leſs you muſt raiſe it, from time to time, when you pour mercury into the ſhorter; as alſo, that when the longer leg is quite

quite full of mercury, you need not pour in any more., if the longer much exceed a yard; becauſe, upon erecting the tube, there will ſubſide, from the taller leg into the other, a conſiderable quantity of mercury. And to free it from bubbles, you muſt, once more, ſtop the orifice with the finger, and incline, and re-erect the tube ſeveral times, till you have thereby brought moſt of the ſmaller bubbles into a ſingle large one; then making this paſs leiſurely, two or three times, from one end of the tube, to the other, it will unite all the ſmall bubbles to itſelf: and this may, afterwards, by one inclination more of the tube, be made to paſs into the ſhorter leg, and thence into the free air.

But there is another ſort of funnels, with which, if skilfully uſed, the bended tubes of our portable barometers, may be very expeditiouſly fill'd. For, if the ſlender part of the funnel be bent in an obtuſe angle, and ſo long, that the part which is to go into the ſhorter leg of the ſiphon, may reach to its flexure; you may, by holding the tube ſo, that the ſealed end be ſomewhat lower than the other, and by pouring in mercury at this obtuſe end of the angular funnel, eaſily make it run over the flexure, into the longer leg of the ſiphon; provided you, now and then, as occaſion requires, erect, and ſhake the tube, to help the mercury to get by the air, and expel it.

We accompliſh'd another part of our deſign, by means of a piece of wood, ſomewhat longer than the tube, and conſiderably broader in the lower part, than in the upper, to receive the ſhorter leg of the ſiphon. In ſuch a piece of wood, which was about an inch thick, we caus'd ſuch a channel to be made, that our ſiphon might be placed in it ſo deep, that a flat piece of wood might be laid on it, without touching the glaſs; ſo that this piece of wood may ſerve for a cover to defend the glaſs, to be put on when the inſtrument is to be tranſported; and taken off again, when 'tis to be hung up for obſervation; the channel'd piece of wood ſerving both for part of a caſe, and for an entire frame; which may, for ſome uſes, be a little more commodious, if the cover be join'd to the reſt of the frame, by two or three little hinges, and a haſp, whereby the caſe may be readily open'd and ſhut, at pleaſure.

The third thing we propoſed, is not ſo eaſy as the ſecond; nor have we yet had opportunity to try whether the way we made uſe of, will hold, if the barometer be tranſported into very remote parts; tho', by ſmaller removes, we found reaſon to hope 'twill ſucceed in greater.

The grand difficulty was, to prevent the ſpilling of the mercury; for, the upper part of the tube being deſtitute of air, if the quick-ſilver, by the motion of the inſtrument, be made to vibrate, it will hit ſo violently againſt the top of the glaſs, as to break it. To obviate this inconvenience, we incline the tube, till the mercury be impell'd to the very top of it; when yet there will remain a competent quantity in the ſhorter leg of the glaſs, if that be not too ſhort; then the remaining part of the ſhorter leg, is to be fill'd up either with water, or mercury, and the orifice of it very carefully ſtop'd with cement: by this means, the mercury in the longer leg, having no room to play, cannot ſtrike with violence againſt the top of the glaſs.

When

When the instrument is to be transported, the height of the mercurial cylinder being taken for that place, day, and hour, and compared with that of another good barometer, which is to continue in the same place; as much of the channel, as is unpossess'd by the glass, may be stuffed with cotton, or the like; and some of the same matter may be put between the rest of the frame, and the cover, which ought to be well bound together. And when the instrument is arrived at the place design'd, the water, that is added, may be taken off again, by pieces of spunge, linen, *&c.* but, if instead of water, mercury be employ'd, it ought to be taken out, till you have just the weight that was put in. The chief use of this barometer is, by keeping a diary of the heights of the mercury herein, and comparing them with those in the barometer, that was not remov'd, to discover the agreement, or difference of the weight of the atmosphere, in distant places. The structure of this instrument, also, fits it to be securely let down into wells, or mines; to be drawn up to the top of towers, and other elevated places; and, perhaps, by a convenient addition, such barometers may shew very minute alterations of the atmosphere's pressure.

Whether this barometer, furnish'd, at its upper end, with a ball and socket, and at the lower, with a great weight, may be serviceable at sea, notwithstanding the rolling of a ship, I have not try'd; but it may, at least, be apply'd in flat calms, to shew the weight of the atmosphere, in different climates, upon the sea; which may, perhaps, prove useful to navigators, by enabling them to foretel the end of the calm. Besides, having one of these instruments ready, whenever they come on shore, they can presently take notice of the gravity of the atmosphere, in that place; and this, perhaps, compared with other observations, may, in time, help them to guess where they are, and to foresee some approaching changes of weather.

Mercury in a barometer, will be kept suspended higher at the bottom than at the top of a hill.

23. Two persons, whom I employ'd, found the mercury, in a portable barometer, fall a little, as they ascended a hill; at the top whereof they let the fluid settle, and carefully noted the place whereat it rested, which was one quarter of an inch beneath its former station; tho' the hill was not high, and the air and wind seem'd, to them, much colder at the top, than below. And as they descended, they observed that the mercury rose gradually.

The weight of the air will sustain the mercury in the barometer, tho' it press thereon but at a very small orifice.

24. Take the bent tube, mention'd in the twenty-second experiment, and inclining it, till the greatest part of the mercury pass from the shorter leg into the longer, the upper-end of the shorter leg, may, by the flame of a lamp, be drawn out so slender, that its orifice shall not be above an eighth, or tenth part as big as 'twas before. This being done, and the tube erected again, if the tall cylinder of mercury be of the usual, or former height, as we found it, 'twill appear that the weight of the external air may press as much upon the stagnant mercury, thro' a little hole, as when all the upper superficies of that mercury, was directly exposed to it.

Both an oblique pressure of the atmosphere, and the spring of a little included air, will sustain the mercury in the barometer.

25. If, instead of drawing the shorter leg of our siphon directly upwards, or parallel to the longer, as in the foregoing experiment, you bend off the slender

der part, so that, were it continued, it would make a right angle with the longer leg of the siphon, or an acute one, tending downwards; and when the tube is erected, the mercury rests at its usual station; 'twill appear, that the pressure of the atmosphere, may be exercis'd upon it as well obliquely, when the pipe that conveys it, is either horizontal, or opens downwards. PNEUMATICS. *Fig.* 50.

And, if instead of bending this slender pipe, you seal it up hermetically, the continuance of the mercurial cylinder, at the same height, will shew, that the spring of a very little air, shut up with the pressure of the atmosphere upon it, is able to support as tall a cylinder of mercury, as the weight of that part of the atmosphere, which can come to exercise its pressure against the mercury. *Fig.* 51.

If, when the shorter leg of the barometer is sealed, you move the instrument up and down, the mercury will vibrate, by reason of the yielding spring of the imprison'd air; but, because of the resistance of the spring, the motion will be diversified after an odd manner; which may be easily perceiv'd by the impression it makes upon the hand, but not so easily described. And as, when the shorter leg is drawn out slender enough, after the instrument is furnish'd with quick-silver, 'tis easy to seal it up with the flame of a candle, without the help of any instrument at all; I might here observe, that it may, on some occasions, be convenient to seal up the barometer, before it be transported; and, in some cases, to incline the tube before-hand, till the quick-silver have quite fill'd the longer leg: for by this means, the vibrations of the quick-silver will be less; and 'tis easy, when the instrument is brought to the design'd place, to break off the slender apex of the shorter leg, and so expose, again, the mercury to the pressure of the atmosphere.

Having caus'd a portable barometer to be made, with the shorter leg of a more than ordinary length; I afterwards, caused the upper part of this leg to be drawn out very slender; and lastly, the same to be, about the middle, bent downwards, so that the small orifice of the slender apex, pointed towards the ground; when neither I, nor some others, took notice that the mercury stood lower than in ordinary barometers: whence we concluded, that the atmosphere could press, not only at a very small orifice, but, when the air must, at this little orifice, tend upwards, to press upon the surface of the stagnant mercury.

26. When it appear'd, by a good barometer, that the atmosphere was considerably heavy, I caus'd a glass pipe, hermetically sealed at one end, and in length about two feet and a half, to be fill'd with quick-silver; except a very little part, wherein some drops of water were put, that we might the better discern the bubbles, if any should be left, after the inversion of the tube into an open glass, containing stagnant mercury. Having, by this means, freed the tube from bubbles, we so order'd the matter, that the quick-silver, and the little water about it, exactly fill'd the tube, without leaving any visible interval at the top; and yet the mercurial cylinder was but very little higher than that of our barometer at *To make a barometer useful but at certain times.*

that

PNEUMATICS. that time. Then the pipe was left erected in a quiet place, where the liquors retain'd their former height for several days. A school-philosopher would confidently have attributed this sustentation of so heavy a body, to nature's dread of a vacuum; but either she is not always equally subject to that fear, or some other cause of the phenomenon must be assign'd: for, when, long after, I had observ'd, by the barometer, that the atmosphere was grown much lighter than before, I found the quick-silver, in the short tube, considerably subsided; leaving a cavity at the top, which afterwards grew less, as the atmosphere became heavier.

The ascent of liquors in very slender tubes in vacuo.

27. Some spirit of wine, ting'd with cochineal, being put into the receiver, and the air withdrawn, it bubbled exceedingly for a considerable time. Then, little hollow pipes, of different sizes, were put into it, when the red liquor ascended higher in the more slender, than in the others; but upon extracting the air, there scarce appear'd any sensible difference in the heights of the liquor, nor upon the letting it in again.

Afterwards, two such tubes, of different sizes, being fasten'd together with cement, were let down into the same spirit of wine, when the receiver was well exhausted: notwithstanding which, the liquor ascended in them, for ought we could plainly see, after the ordinary manner; only when the air was let in again, there seem'd to be some little rising, at least in one of the tubes.

In this experiment, tho' there appear'd no bubbles at all in the spirit of wine in the vessel, yet, for a considerable time, there arose bubbles in that part of the liquor which was got into the slender pipes.

A spontaneous ascent of water in a tube filled with a compact body.

28. I took a strait pipe of glass, open at both ends, and of a moderate bore; and having tied a linen rag to one end of it, that the water might have free passage in, and the powder not be able to fall out, we carefully fill'd the cavity with minium; and then having erected the tube, so that the bottom of it rested upon that of a shallow, open-mouth'd glass, containing water enough to rise an inch or two above the bottom of the tube, it insinuated itself, by degrees, into the cavity thereof, as appear'd by a little change of colour in that part of the minium which it reach'd; till the open glass being, from time to time, supplied with fresh liquor, it attain'd to the height of about thirty inches.

Taking, afterwards, another tube, and some minium, carefully prepared, I prosecuted the experiment, so as to make the water rise in the pipe about forty inches above the surface of the stagnant water.

Making the experiment with beaten glass, pieces of spunge, putty, *&c.* I did not find any of them succeed so well as the minium. Ting'd liquors, as ink, tincture of saffron, *&c.* seem'd not to rise near so high as water; as if the dissolved ingredients gradually choaked the pores of the minium.

To have the grains of our powder more minute, and the intervals between them smaller, I chose the best sort of minium, sifted it very fine, and so put it, by little and little, into the tube; that by ramming it, from time to time, it might be made to lie the closer: and this method succeeded

ceeded well. It ſeem'd, by a trial or two, that if the tube were very ſlender, the experiment would not ſucceed. Pneumatics.

It may be worth while to obſerve, in what times the water aſcends to certain heights; for, at the beginning, 'twill aſcend much faſter than afterwards, and ſometimes continue riſing for thirty hours, or longer.

One end, propoſed in this experiment, is, to diſcover a miſtake in the modern explication of filtration; which ſuppoſes, that the parts of the filtre, which touch the water, being ſwell'd, by the ingreſs of it into their pores, are thereby made to liſt up the water, till it touch the higher parts of the filtre; by which means, theſe being alſo wetted and ſwell'd, raiſe the water to the other neighbouring parts of the filtre, till it have reach'd to the top of it, whence its own gravity makes it deſcend: but, in our caſe, we have a filtre made of ſolid, metalline corpuſcles; where 'twill be very hard to ſhew, that any ſuch intumeſcence is produced, as this explanation requires.

Water aſcends ſo few inches, even in very ſlender pipes, that the riſe of the ſap in trees, ſeems hardly accountable for, from the ſame cauſe. In the laſt trial, above-mention'd, I made water to aſcend above three feet and a half: and, if by ſo ſlight an expedient, water may be rais'd as high as is neceſſary for the nutrition of ſome thouſands of plants; for ſuch a number there is, that exceed not three feet and a half, in height; I ask why nature may not have uſed other contrivances, to make liquors aſcend to the tops of the talleſt trees; eſpecially, ſince beſides heat, and ſomething equivalent to valves, *&c.* many other things, perhaps not yet dreamt of, may probably concur to the effect?

As formerly, by bending theſe ſlender pipes, we made ſhort ſiphons, thro' which the water would run, without being at firſt aſſiſted by ſuction; ſo I try'd whether I could, in larger pipes, make much longer ſiphons, by the help of minium. But tho', when the orifices pointed upwards, fine minium were ramm'd into both the legs, and both the orifices cloſed, yet, when they came to be again turn'd downwards, the weight of the minium would make ſome ſuch diſcontinuation, as to hinder the farther progreſs of the water. This impediment, however, I judg'd ſuperable, but had no opportunity to proſecute the experiment.

29. Having in ſhallow, wide-mouth'd glaſſes, expoſed a ſtrong ſolution of common ſea-ſalt, or of vitriol, to the air, which reach'd not, by ſome inches, to the tops of the veſſels; and, having ſuffer'd much of the aqueous part to exhale very ſlowly; the coagulated ſalt, at length, appear'd to have lined the inſide of the glaſſes, and to have aſcended much higher than where the ſurface of the remaining water then reſted; or the part whereto the liquor reach'd, when 'twas firſt pour'd in. And if the experiment were continued long enough, I ſometimes obſerv'd this aſcent of the ſalt, to be of ſome inches; and that the ſalt did not only line the inſide of the glaſs, but getting over the brim of it, cover'd the outſide, alſo, with a ſaline cruſt; ſo that, conſidering what a little liquor remain'd in the glaſs, 'twas ſurprizing how it could poſſibly get thither. Other ſalts, alſo, beſides theſe *The ſpontaneous aſcent of ſalts along the ſides of glaſſes.*

PNEUMATICS. mention'd, will exhibit the ſame phenomenon. The cauſe of this odd effect may be referr'd to that of the aſcent of liquors in pipes.

I obſerv'd in water, and aqueous liquors, that part of the ſurface next the ſides of the glaſs, to be ſenſibly more elevated, than the reſt of the ſuperficies: and if very minute clippings of ſtraw, or other ſmall and light bodies, floating upon the water, approach near enough to the ſides of the glaſs, they will be apt to run up, as 'twere, this aſcent of water, and reſt againſt the ſides of the glaſs.

We may, alſo, obſerve, that ſea-ſalt uſually coagulates at the top of the water, in ſmall and oblong corpuſcles; ſo that, as to theſe, 'tis eaſy to conceive, how numbers of them may faſten themſelves a-round the inſide of the glaſs. And beſides ſea-ſalt, I have found ſeveral others, which, if their ſolutions be ſlowly evaporated, will, whilſt yet there remains a large proportion of liquor, afford ſaline concretions at the top of the water. And the faſtening of ſaline particles to the ſides of the glaſs, may, perhaps, be promoted by a coldneſs, communicated by corpuſcles contiguous to the glaſs; becauſe the glaſs may be ſuppoſed more cold, upon account of its denſity, than water: but by the evaporation of the aqueous parts of the ſolution, the ſurface of the remaining liquor muſt neceſſarily ſubſide; and thoſe ſaline particles that were contiguous to the inſide of the glaſs, and the more elevated part of the water, having no longer liquor enough to keep them diſſolv'd, will be apt to adhere to the ſides of the glaſs; and upon the leaſt farther evaporation of the water, become a little higher than the greater part of the ſuperficies of that liquor: whence, by reaſon of the little inequalities, that will be on the internal ſurface of the adhering corpuſcles of the ſalt; and perhaps, alſo, on the internal ſuperficies of the glaſs; there will be intercepted between the ſalt and the glaſs, little cavities, into which the water, contiguous to the bottom, will aſcend, or be impell'd by the ſame power that raiſes it in ſlender pipes. And when the liquor is thus got to the top of the ſalt, and lies expoſed to the air; the ſaline part may, by the evaporation of the aqueous, be brought to coagulate there; and conſequently, to increaſe the height of the ſaline film, which, by the like means, may, at length, reach to the very top of the glaſs; and thence it may eaſily be brought over to the outſide of the veſſel, where the natural weight of the ſolution will facilitate its progreſs downwards: whence the pellicle of ſalt, together with the contiguous ſurface of the glaſs, may, at length, conſtitute a kind of ſiphon.

Thus I have uſually obſerved the ſaline film to be very eaſily ſeparable from the glaſs in large flakes; which argues, that they did not ſtick cloſe to one another, except in a few places; but had a thin cavity interpoſed between them, thro' which the water might aſcend.

Nor is it repugnant to this explanation, that in caſe the water aſcended, it ſhould diſſolve the ſalt; for the liquor being already upon the point of concretion, it is ſo ſaturated with ſalt, that it can diſſolve no more. Whence we may alſo ſee, why, when the ſaline film reaches to the outſide of the glaſs, the liquor does not run down to the bottom, but coagulates by by the way. And I have suspected, that when the concretion is once begun, the film may be raised, and propagated, not only by the motion of the liquor between the inside of that and the glass; but by the same liquor insinuating itself on the outside of the film, into the small interstices of the saline corpuscles; as ink rises into the slit, and along the sides of the nib of a pen, though nothing but the very point touch the surface of the liquor. And, by this means, the impregnated solution may, as it were, climb up to the top of the saline concretion, and, by coagulating there, add to its height. *Pneumatics.*

30. Having caus'd a cylindrical piece of brass to be very carefully turn'd, of an inch in diameter, three inches in length, and open at both ends; to one of these ends we exactly fitted a flat bottom of the same metal, and fasten'd it very close with little screws on the outside. *To estimate the gravity of cylinders of the atmosphere in known weights.*

This instrument, being balanc'd in an exact pair of scales, was carefully fill'd with pure mercury, which we found to weigh one hundred thirty-seven drams, and forty-five grains; and multiplying that by ten, there will arise, for the weight of a mercurial cylinder of one inch in diameter, and thirty inches in height, about fourteen pound, two ounces, and three drams, troy.

The weight of a mercurial cylinder in an equilibrium with the atmosphere, and of an inch in diameter, being thus settled, we may easily compute the weight of a cylinder of quick-silver of another diameter, and consequently the force of the pressure of an atmospherical column of the same diameter. For, since cylinders of equal heights are to one another, as their bases; and the bases of cylinders to each other as the squares of their diameters; and lastly, since we here suppose mercury a homogeneous body; the mercurial cylinders will be to each other in weight, as they are in bulk: if then, for instance, we would know the weight of a cylinder thirty inches high, whose diameter is two inches, the rule is this: as the square of the diameter of the standard cylinder, whose weight is known, to the square of the diameter of the cylinder propos'd; so is the bulk of the former to the bulk of the latter, and the weight of that to the weight of this. Thus the square of one inch, the diameter of the standard cylinder, being one, and the square of two, the diameter of the cylinder given, being four; the bulk or solid content of this latter cylinder, and consequently its weight, will be four times as great as those of the standard cylinder.

31. We took a small vigorous load-stone, cap'd and fitted with a loose plate of steel, so shaped, that when sustain'd by the stone, we could hang, at a little crook that came out of the midst of it, and pointed downwards, a scale; into which, we put weights; and then, by shaking the load-stone, as much as we guess'd it would be by the motion of the engine, we found the greatest weight, that we presum'd it would support, notwithstanding the agitation whereto 'twould be expos'd, was, besides the iron plate and the scale, six ounces troy: and, if we added half an ounce more, the whole weight appear'd too easy to be shaken off. This done, we hung the load-stone with all the weight it sustain'd, at a button of glass fasten'd to the top of the *The attractive virtue of the load-stone in an exhausted receiver.*

PNEUMATICS. the inside of the receiver, when 'twas first blown; and, tho' in about twelve exsuctions we usually emptied such receivers, as much as was requisite for most experiments; yet, this time, we made above twice that number: when, violently shaking the engine, without thereby shaking off the weight that hung at the load-stone, the iron seem'd to be very nearly as firmly sustain'd by it, as before the air began to be pump'd out; for the extraction of the air, tho' it be not suppos'd to weaken the precise power of the load-stone; yet, it must lessen its power to sustain the steel, because this in so thin a medium must weigh heavier than in the air.

The pressure of the external air being taken off, the sucker of a syringe is easily drawn up, tho' the lower orifice be stopp'd.

32. We took a brass syringe, the barrel about six inches in length, and the diameter about an inch and three eighths; and having, by placing a thin bladder about the sucker, and pouring oil into the barrel, made the instrument stanch, whilst the sucker mov'd without much difficulty; we thrust this to the bottom of the barrel to exclude the air; and having laid aside the slender pipe of the syringe, we carefully stopp'd the orifice to which the pipe, in these instruments, is usually screw'd; then drawing up the sucker, we let it go, to judge, by the violence with which it would be driven back again, whether the syringe were fit for our purpose; and finding it to be so, we fasten'd a ponderous piece of iron to keep it down; and then fixing to the handle of the rammer one end of a string, whose other end was ty'd to the turn-key, we convey'd this syringe and the weight belonging to it, into a receiver; and having pump'd out the air, we began to turn the key, thereby to shorten the string that ty'd the handle of the syringe to it, and found no resistance in drawing up the sucker from the bottom of the cylinder. *Fig.* 52.

And repeating the experiment with the like success, when the receiver being exhausted, we had drawn up the sucker, almost to the top of the barrel by a weak string, we kept the parts of the syringe in that posture, till a passage was open'd to the outward air; upon which, the sucker was so forcibly depress'd, that it broke the string, and was violently driven back to the lower part of the barrel; tho' the string had sustain'd between four and five pound weight, and broke long before all the air, that flow'd in to fill the receiver, had found entrance.

Again, we took the same syringe, and having found it tight enough for our purpose, we carefully clos'd the vent with a cork and cement, and having ty'd a weight of two pound two ounces to the barrel, we suspended the rammer of the syringe, by a string, in a large receiver; and causing the pump to be ply'd, we made eleven or twelve exsuctions, without finding any appearance of change in the syringe: but causing the pumping to be continued, I perceiv'd, within two or three exsuctions more, the cylinder began to be drawn very slowly down, by the weight hanging at it; and likewise try'd, that, just upon a fresh exsuction, the descent would be manifestly accelerated. And, when we had suffer'd the barrel and weight to slide down as far as we thought fit, we let in the external air, which rais'd them both again, much faster than they had subsided.

And,

And, ſubſtituting a far heavier weight for the former, the depreſſion of the barrel of the ſyringe ſucceeded for two or three times, ſucceſſively, much ſooner than before.

Pneumatics.

33. Having cemented up the hole at the bottom of the ſyringe, we ty'd to the barrel a hollow piece of iron, that ſerv'd for a ſcale; into which we put weights, ſucceſſively, to try if, when the ſucker was forcibly drawn up, and held ſteddily, in its higheſt ſtation, the weight, faſten'd to the barrel, which was held down whilſt the ſucker was drawn up, and, afterwards, let go, would be conſiderably rais'd. And, when we perceiv'd, that the addition of half a pound, or a pound, more, would make the weight too great to be ſo rais'd; we forbore to put in that increaſe of weight: and, having ty'd the handle of the rammer to the key, we convey'd the ſyringe, together with its clog, into a receiver; out of which, a convenient quantity of air being pump'd, we were, thereby, eaſily enabled to draw up the ſucker, without the cylinder: after which, having let in the air, ſo that the weight was rais'd a little, I cauſed two pound to be taken out; and then the receiver, being ſomewhat exhauſted, and the air admitted; the clog, which amounted to about ſixteen pound, was ſwiftly rais'd, and, as it were, ſnatch'd up from the middle, to the upper part of the rammer.

A ſyringe cauſing the preſſure of the air to raiſe a conſiderable weight.

Fig. 52.

34. We took a ſmall receiver, ſhaped like a pear, cut off, horizontally, at both ends; we, alſo, took the ſyringe, formerly mention'd, and, having cemented thereto, inſtead of its own braſs-pipe, a ſmall pipe of glaſs, about half a foot in length, we put this ſyringe in at the narrow end of the receiver; to whoſe orifice was, afterwards, carefully cemented the braſs-cap, with the key, whereto we ty'd the handle of the rammer: then, having conveniently placed, upon the engine, a very ſhort thick conical glaſs, containing a ſufficient quantity of quick-ſilver; we ſet the receiver over it, ſo that the lower end of the pipe of the ſyringe reach'd almoſt to the bottom of this glaſs; and, conſequently, was immers'd far beneath the ſurface of the quick-ſilver: when, all things being prepared, the air was pump'd out of the receiver, and, conſequently, out of the little glaſs that held the mercury; the ſucker being warily drawn up; we could not ſee the quick-ſilver aſcend to follow it; but the air, being let ſlowly into the receiver, the mercury was quickly impell'd up to the top of the glaſs-pipe.

The aſcent of liquors in ſyringes owing to the preſſure of the air.

Fig. 53.

And, for farther ſatisfaction, when the experiment was repeated, we plainly obſerv'd, that tho', when the receiver, being not yet exhauſted, the ſucker was drawn up but one inch, the mercury would be rais'd to the upper part of the glaſs-pipe of the ſyringe; yet, after the exhauſting of the receiver, tho' the ſucker was drawn up twice as high, there appear'd no aſcent of the mercury in the pipe.

To confirm this experiment, we caus'd the ſyringe to be ty'd faſt to a ponderous body, that might keep the cylinder unmov'd, when the ſucker ſhould be drawn up; we, alſo, cemented to the vent, or ſcrew, at the bottom of the ſyringe, a pipe of glaſs, about two inches long; and, having placed

PNEUMATICS. placed the heavy body upon a pedestal of a convenient height, that the glass-pipe might be all seen beneath it; and a very low vial, almost fill'd with quick-silver, might be so placed underneath the pipe, that the stagnant mercury reach'd far above the immers'd orifice of the said pipe: when things being thus provided, and the handle of the rammer ty'd to the key, belonging to the brass-cover of the receiver, this vessel was cemented to the engine, and exhausted.

We then look'd upon the glass-pipe, above-mention'd, and, being able to see thro' it, we, by the string, drew up the sucker to a considerable height, but could not perceive the pipe to be fill'd with any succeeding mercury; but, warily letting in some air, we quickly saw the mercury impell'd to the very top of the pipe; and concluded, from the quantity rais'd, that some was, also, driven into the cavity of the cylinder. This experiment, also, we successfully try'd with tinged spirit of wine. Hence it appears, that, if a syringe were made use of above the atmosphere, neither the stopping of the pipe would hinder the easy drawing up of the sucker; nor the drawing up the sucker, tho' the pipe were not stopp'd, raise, by suction, the liquor wherein the pipe was immers'd.

The adhesion of cupping-glasses depends upon the pressure of the air.

Fig. 54.

35. We took a glass, about an inch and a half in diameter, but much longer than an ordinary cupping-glass of that breadth; we, also, provided a receiver, shaped like a pear, and open at both ends, at the sharper whereof, there was a small orifice; but, at the obtuser, a short neck, wide enough to admit the cupping-glass, without touching the sides of it. The smaller orifice of the receiver, being cemented to the engine, I caus'd the cupping-glass to be well fasten'd, with the mouth upwards, to the palm of a person's hand; then caus'd him to put it into the receiver, and lay his hand so upon the orifice, that it might serve for a cover to it, and hinder any air from getting in between them: but, upon the first suck, the cupping-glass fell off; the weight of the atmosphere pressing so strongly upon the person's hand, that he complain'd, he could very hardly take it from the glass, into which it was almost thrust. We repeated the experiment, fastening the cupping-glass more strongly than before; the tumour, occasion'd whereby, was very visible: but now, also, as before, at the very first turning of the stop-cock, to let the air out of the receiver, the cupping-glass fell off.

A great weight rais'd by a cupping-glass without heat.

Fig. 55.

36. We took the brass-ring, formerly mention'd, and cover'd it with a wet bladder, which was so ty'd on, that the bottom of the bladder cover'd the upper orifice of the ring, and lay stretch'd upon it, whilst the neck of the bladder was ty'd with a string, near the middle of the lower orifice of the ring; and, in this lower part of the bladder, we made two or three small holes, for the air to pass in and out at: then, having placed, at the bottom of our capp'd receiver, a thick piece of wood, perforated to receive the neck of the bladder; we placed the cover'd ring upon this piece of wood, so that the upper part of the bladder lay parallel to the horizon; then we suspended, at the key belonging to the cap of our receiver, a blind glass-head, instead of a cupping-glass, which name it may bear; and

Fig 59. pag. 506.

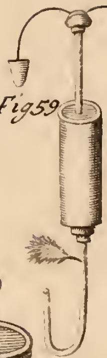


Fig. 60. pag. 507.

Fig. 61. pag 508.

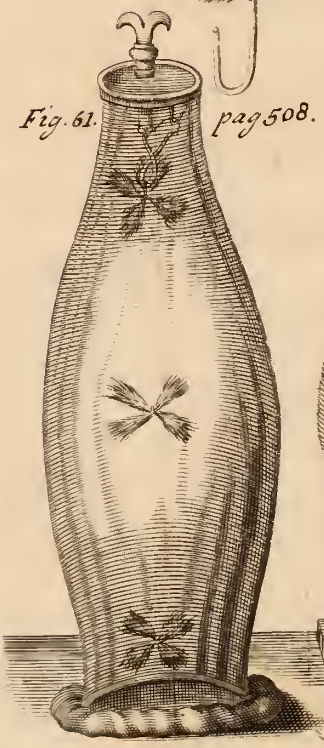
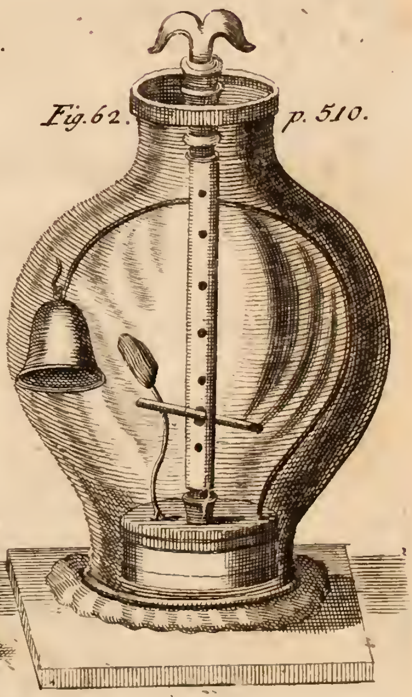


Fig. 62. p. 510.



and to the upper part of this glaſs, we faſten'd a large ring of metal to preſs it againſt the bladder. The receiver being now cemented on to the engine, we, by the help of the key, let down the cupping-glaſs till it almoſt touch'd the level ſuperficies of the bladder; and when the receiver was moderately exhauſted, we let down the cupping-glaſs a little lower, ſo that it reſted upon the bladder, and touch'd it with all the parts of its orifice; whence the cupping-glaſs with its ſubjacent bladder was become an internal receiver wherein the air was conſiderably expanded. Then we warily let the air into the receiver, and thereby the air that ſurrounded the cupping-glaſs or internal receiver, having now a ſtronger preſſure than that in the cupping-glaſs could reſiſt, the bladder on which the cupping-glaſs reſted, was conſiderably thruſt into the cavity of the glaſs, and made to ſtick very cloſe to the orifice of it. PNEUMATICS.

Repeating the experiment, and exhauſting the receiver further than before, we took out the cupping-glaſs and the bladder, which, together with the included braſs-ring was hanging at it; and having ty'd the glaſs to the hook of a ſtatera, and a large ſcale to the neck of the bladder, we put weights, by degrees, into the ſcale, till we thus forced off the bladder from the glaſs; which hapned not till the weight amounted to thirty-five pound. *Fig. 56.*

37. We caus'd a pair of bellows to be made different from ordinary ones, their boards being circular, without handles, and without clack or valve, the noſe but an inch long, to be lengthned, if occaſion required, with a pipe, and about ſix inches in diameter, the leather being limber; ſo that when the bellows were open'd to their full extent, by drawing up the upper baſis at a button purpoſely made in the midſt of it, they reſembled a cylinder ſixteen or eighteen inches high; but there was ſome little, and unperceiv'd leak in them, whereat air had paſſage, when the noſe was accurately ſtopp'd; however, if we drew up the upper baſis from the lower, the external air would, on all ſides, preſs the leather inward, and render the ſhape of the inſtrument very far from cylindrical. Then carefully ſtopping the noſe, after we had brought the baſes to touch each other, and conveying the inſtrument into a large receiver, it quickly appear'd, when the pump was ſet on work, that, at every exſuction, the air in the folds of the leather, and the reſt of the little cavity left between the baſes, made the upper of them manifeſtly riſe; tho' its own weight would ſoon after depreſs it again, either by driving out ſome of the air, where the inſtrument was not ſufficiently tight, or by making it, as it were, ſtrain thro' the leather itſelf: and if the pump were ply'd faſter than ordinary, the upper part of the bellows, would be ſoon rais'd to a conſiderable height; as appear'd more evidently, if we haſtily let in the external air, whereby the baſes would be clapt together, and the upper of them conſiderably depreſs'd; ſo that the imperfection of the bellows render'd the experiment rather more than leſs concluſive: for ſince there was no external force apply'd to open them, if, notwithſtanding ſome of the included air could get out, the ſpring of the internal air was ſtrong enough to open *Bellows, with the noſe ſtopp'd, open of themſelves, when the preſſure of the air is taken off.* *Fig. 5.*

the

PNEUMATICS. the bellows, when the ambient air was withdrawn, much more would the effect have been produced, if the bellows had been perfectly ftanch.

An attempt to examine the hypothefis of æther, as to its exiftence.

38. Since, if there be fuch a thing as a celeftial matter, or æther, it muft compofe far the greateft part of the univerfe known to us; it deferves to be enquir'd, whether we can, by fenfible experiments, difcover its exiftence, or qualities. To this end I thought our pneumatical engine might contribute, if I could manage therein fuch a pair of bellows as I defign'd; for I propofed to faften a convenient weight to the upper bafis, and clog the lower with another, able to keep it horizontal, and immoveable, fo that when, by the help of the turn-key, the upper bafis fhould be rais'd to its full height, the cavity of the bellows might be brought to its full dimenfions. This done, I intended to exhauft the receiver, and, confequently, the bellows, thus open'd; fo that both the receiver, and they, might be carefully freed from air: after which, I purpos'd to let go the upper bafe of the bellows, that being haftily deprefs'd by the incumbent weight, it might fuddenly fall down to the lower; and by thus greatly leffening the cavity, expel thence the matter, if any there were, before contain'd in it; and that, if it could, by this way, be done, at the hole of a flender pipe, fasten'd either near the bottom of the bellows, or in the upper bafis, againft, or over the orifice of which pipe, there might be placed, at a convenient diftance, either a feather, or the fail of a little wind-mill, made of fome other light body, fit to be put into motion by the impulfe of any matter which fhould be forc'd out of the pipe.

Now, if by this means, notwithftanding the abfence of the air, it fhould appear, that a ftream of other matter, able to fet vifible bodies in motion, fhould iffue out at the pipe of the compref's'd bellows, it would alfo appear, that there may be, plentifully, found a much fubtiler body than common air, in places deferted by fuch air; and that it is not fafe to conclude, from the abfence of the air, in our receivers, and the upper part of the *Torricellian* tube, that there is no body, but an abfolute vacuity. But if, on the other fide, there fhould appear no motion at all to be produc'd, fo much as in the feather, it fhould feem, that either the cavity of the bellows was abfolutely empty; or that it would be very difficult to prove, by any fenfible experiment, that it was full. And if, by any other means, it be demonftrable, that it was replenifh'd with æther, we might fuppofe, from our experiment, that 'tis not eafy to make it fenfible by mechanical experiments; and that 'tis really fo fubtile, and yielding a matter, as does not either eafily impel light bodies, or fenfibly refift, like air, the motions of other bodies thro' it; but is able, freely, to pafs the pores of wood, leather, and clofer fubftances, which the air, in its natural ftate, doth not.

Fig. 58.

And, to make the trial more accurate, I caus'd a fmall pair of bellows to be made with a bladder; and that this might remain entire, we glued the two bafes, the one to the bottom, and the other to the oppofite part thereof; fo that the neck came out at a hole, purpofely made for it, into the upper bafis; whence, into the neck it was eafy to fix what pipe we judg'd fit. We had, alfo, thoughts of procuring another pair of tight bellows, made

made with a very little clack in the lower basis; that, by hastily drawing up the other basis, when the receiver and bellows were very carefully exhausted, we might see whether the subtile matter that was expell'd by the upper basis, in its ascent, would, according to the modern doctrine of the circle, made by moving bodies, be impell'd up, or not.

We, likewise, thought of placing the little pipe of the bladder-bellows, beneath the surface of water, exquisitely freed from air, to see whether, upon the depression of them, by the incumbent weight, when the receiver was carefully exhausted, there would be any thing expell'd at the pipe, productive of bubbles in the liquor, wherein its orifice was immers'd.

To bring our conjectures to a trial, we put into a capp'd receiver, the bladder, accommodated as already mentioned, containing between half a pint, and a pint; and to depress the upper basis of these little bellows the more easily, and uniformly, we cover'd the round piece of past-board, that made the upper basis, with a pewter-plate; a hole being made in it for the neck of the bladder: which, upon trial, prov'd not ponderous enough without weight of lead. And to secure the feather above-mention'd, from being blown aside, we made it to move in a perpendicular slit in a piece of past-board, fasten'd to one part of the upper basis; as that whereto we glued the feather, was to another part. Things being thus provided, the pump was work'd; and as the ambient air was, from time to time, withdrawn, that in the bladder expanded itself so as to lift up the metalline weight, and yet, in part, to sally out at the little glass pipe of our bellows; as appear'd by its blowing up the feather, and keeping it suspended, till the spring of the air in the bladder was too far weakned. In the mean time, we did, now and then, by the help of a string fastned to the turn-key, and the upper basis of the bellows, let down the basis a little, to observe how, upon its sinking, the blast, against the feather, would decrease, as the receiver was further exhausted. And when we judg'd it to be sufficiently freed from air, we let down the weight, but could not perceive that, by shutting the bellows, the feather was at all blown up as before; tho' the upper basis were more than usually depress'd. And yet it's somewhat odd, that when, in order to a further trial, the weight was drawn up again; as the upper basis rose from the lower, the sides of the bladder were sensibly press'd, or drawn inwards. The bellows being thus open'd, we let down the upper basis again, but could not perceive that any blast was produced; for tho' the feather, which lay just over, and near the orifice of the little glass pipe, had some motion, yet this seem'd plainly to be but a shaking, and almost vibrating motion, whereinto it was put by the upper basis, which the string kept from a smooth and uniform descent; but not to proceed from any blast, issuing out of the cavity of the bladder. And, for further satisfaction, we caused some air to be let into the receiver; because there was a possibility that the slender pipe might, by some accident, be choaked: but tho', upon the return of the air into the receiver, the bases of the bellows were press'd closer together, yet it seem'd that some little air got thro' the pipe, into the cavity of the bladder; for

 when

PNEUMATICS. when we began again to withdraw the air that was let into the receiver, the bladder began to ſwell again, and, upon letting down the weight, to blow up, and ſuſtain the feather, as happen'd before the receiver had been ſo well exhauſted.

Continued. 39. I caus'd a crooked pipe to be made for the ſyringe, formerly mention'd, inſtead of its ſtrait one, whoſe ſhorter leg was parallel to the longer. *Fig.* 59. And this pipe, after being ſcrew'd on carefully, was cemented to the barrel; and becauſe the braſs-pipe could ſcarce be made ſmall enough, we cauſed a ſhort and ſlender pipe of glaſs, to be put into the orifice of the ſhorter leg, and carefully faſten'd to it with cement. Then the ſucker being made to go ſmooth, without leſſening the ſtaunchneſs of the ſyringe, there was faſten'd to the handle of the rammer, a weight made in the form of a ring, or hoop; which, by reaſon of its figure, might be ſuſpended from the handle of the rammer, and hang looſe on the outſide of the cylinder, and which, both by its figure and weight, might eaſily, and ſwiftly depreſs the ſucker, when drawn up. The ſyringe, thus furniſh'd, was faſten'd to a broad, heavy pedeſtal, to keep it in its vertical poſture, and to hinder it from tottering, notwithſtanding the weight that clogg'd it. Beſides all this, we took a feather, about two inches long, of which there was left, at the end, a part about the breadth of a man's thumb-nail, to cover the orifice of the ſlender glaſs pipe of the ſyringe; for which purpoſe, the other extremity of it was ſo faſten'd, with cement, to the lower-part of the ſyringe, that the broad end of the feather ſtood juſt over the little orifice of the glaſs, at ſuch a convenient diſtance, that when the ſucker was a little drawn up, and let go again, the weight would depreſs it faſt enough to blow up the broad part of the feather. The handle of the rammer, being now ty'd to the turn-key of a capp'd receiver, the ſyringe, and its pedeſtal, were incloſed in a capacious receiver; and the pump, being ſet on work, we, after ſome quantity of air was drawn out, rais'd the ſucker a little, by the help of the turn-key: and, then, turning the ſame key the contrary way, we ſuffer'd the weight to depreſs the ſucker, to ſee how the feather would be blown up; and, finding that it was impell'd, forcibly, we continu'd to pump, by pauſes; during each of which, we rais'd and depreſs'd the ſucker, as before; and obſerv'd, that as the receiver was gradually exhauſted of air, the feather was leſs briskly driven up, till, at length, when the receiver was well empty'd, the uſual elevations and depreſſions of the ſucker would not blow it up at all, tho' they were far more frequently repeated than before.

After we had long tried, in vain, to raiſe the feather, ſome air was let into the receiver; and tho', when but very little air was admitted, the motions of the ſucker had ſcarce any ſenſible effect upon the feather; yet, when the quantity began to be conſiderable, the feather began to move a little upwards; and ſo letting in air, not all at once, but ſucceſſively, and moving the ſucker up and down, in the intervals of thoſe times of admiſſion; we obſerv'd, that as the receiver contain'd more air, the feather was more briskly blown up.

But,

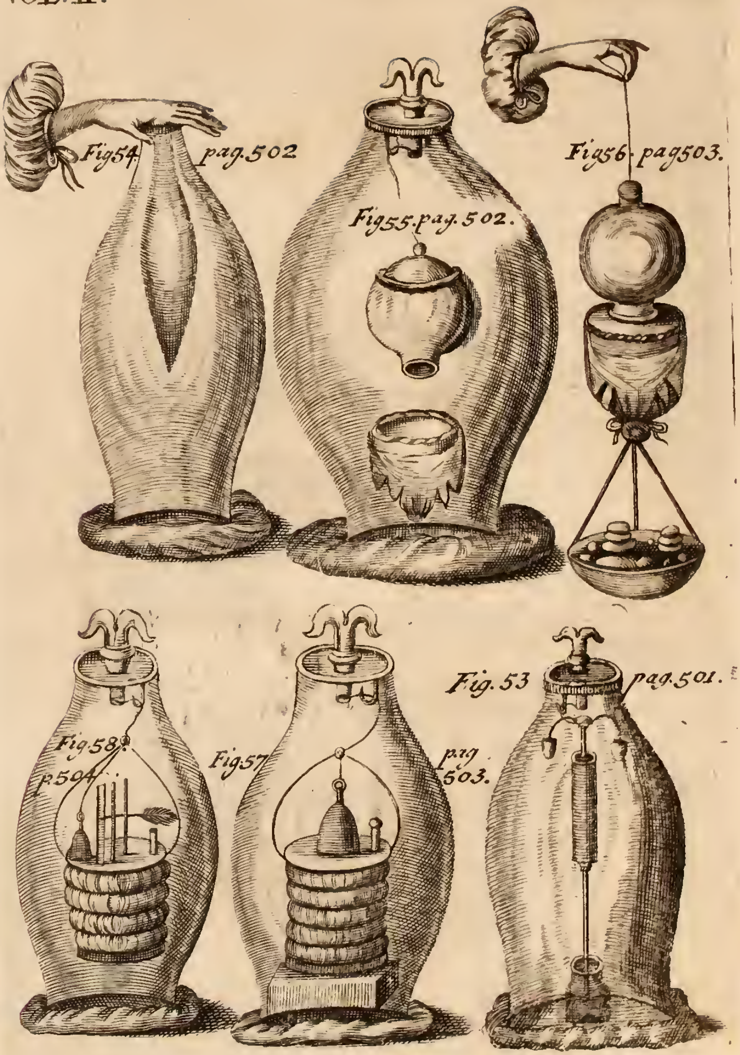


Fig54. pag.502
Fig55. pag. 502.
Fig56 pag503.
Fig.58. p.504
Fig57 pag 503.
Fig. 53 pag.501.

But, not content with a single trial, we caus'd the receiver to be again exhausted, and prosecuted the experiment with the like success; only having, after the receiver was exhausted, drawn up, and let fall the sucker, several times, ineffectually; having, hitherto, not, usually, rais'd it by more than one turn of the hand; we now used an instrument, that was tolerably long, and fit to take hold of the turn-key, so that we could easily raise the sucker between two and three inches at a time, and suddenly depress it again: yet, for all this, which would much have increas'd the blast, if there had been a matter fit for it in the cavity of the syringe, we could not, sensibly, blow up the feather, till we had let a little air into the receiver. But, now, instead of the brass-pipe, hitherto employ'd, we cemented one of glass to the syringe; its shorter leg, after it had, for a while, run parallel to the other, being bent off so, that above an inch and a half of it tended downwards; whereby the orifice of it might be immers'd in the water contain'd in a small open jar. The design of this contrivance was, that when the receiver should be well exhausted, we might try whether, by raising and depressing of the sucker, any such matter would be driven out at the nose of the pipe, as would produce bubbles in the incumbent water; which, air, tho' highly rarify'd, is capable of doing.

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Fig. 60.

The only particulars, wherein this experiment differ'd from the former, were these. As the air was here pump'd out of the receiver; that in the glass-pipe made its way thro' the water, in bubbles. And a little air having once, by a small leak, got in, and forc'd some of the water out of the jar into the pipe; when the receiver was again well empty'd, both that water, and the little stagnant quantity contain'd in the immers'd part of the pipe, produced so many bubbles, of several sizes, as quite disturb'd our observations. Wherefore, we let alone the receiver, exhausted as it was, for six or seven hours, that the water might free itself from air; and then causing what air might have stolen in, to be again pump'd out, till we perceiv'd, by the gage, that the receiver was well exhausted, we caus'd the sucker of the syringe to be rais'd and depress'd several times; and tho', even then, a bubble would, now and then, disturb our observations, yet, when we were not thus confounded, we sometimes observ'd, that the elevation and fall of the sucker, tho' repeated, did not drive out at the pipe, any thing that made discernible bubbles in the incumbent water: for tho' some small bubbles would rarely appear on the surface of the water, yet I could not perceive, that the matter which made them, issued out of the pipe; and some of them manifestly proceeded from aerial particles, still lurking in the water, as I concluded from the place and time of their rising. But, at length, we observ'd, the water, in the immers'd part of the pipe, which was very slender, to be about an inch higher than the rest of the stagnant water, and to continue at that height in the pipe, tho' the sucker were, several times together, rais'd and depress'd, between two and three inches at once; which seem'd to argue, that there was a vacuum in the cavity of the syringe: or if it were full of æther, this was so subtile, that the impulse it re-

Pneumatics. receiv'd from the falling ſucker, would not make it diſplace that very ſlender thread of water in the ſmall pipe; though it appear'd by the bubbles, which ſometimes diſclos'd themſelves in the water, after the receiver had been exhauſted, that far more water would be diſplaced, and carried up by a ſmall bubble, conſiſting of air, ſo rarified, that, according to my eſtimate, the particles of it did not, before the pump was firſt ſet on work, poſſeſs, in the water, a five hundredth part of the ſpace of a pin's head.

A light body falling in the exhauſted receiver. 40. We took a receiver twenty-two inches high; and, that we might let a body fall therein, we ſo faſten'd a ſmall pair of tongs to the inſide of its braſs-cover, that, by moving the turn-key, we might, by

Fig. 61. a ſtring, open them; which their own ſpring would, otherwiſe, keep ſhut: we then join'd, croſs-wiſe, four broad light feathers, each about an inch long, at their quills, with a little cement; into which we, alſo, ſtuck, perpendicularly, a ſmall label of paper, about the eighth of an inch in breadth, and ſomewhat more in height; by which the tongs might take hold of our light inſtrument, without touching the cement, which, elſe, might ſtick to them. By the help of this ſmall piece of paper, the little inſtrument, of which it made a part, was ſo held by the tongs, that it hung horizontal; and then the receiver, being cemented to the engine, the pump was diligently ply'd, till it appear'd, by a gage, that the receiver had been thoroughly exhauſted. Laſtly, our eyes, being attentively fixed upon the connected feathers, the tongs were, by the help of the turn-key, open'd, and the little inſtrument let fall; which, tho', in the air, it had made ſome turns in its deſcent from the ſame height, whence it now fell; yet it here deſcended like a dead weight, without being perceiv'd to make a ſingle turn, or a part of one. However, I cauſed the receiver to be taken off, and put on again, after the feathers were taken hold of by the tongs; whence, being let fall in the glaſs, unexhauſted, they made ſome turns in their deſcent; as they, alſo, did, being let fall a ſecond time, after the ſame manner.

But when, after this, the feathers being placed, as before, we repeated the experiment, carefully pumping out the air, we could not perceive any turning in the deſcent; yet, for farther ſecurity, we let them fall twice more, in the unexhauſted receiver; and found them to turn in falling: but when we did, a third time, ſet them looſe in the receiver, well exhauſted; they fell, after the ſame manner they had, in the ſame caſe, done before.

The propagation of ſounds in an exhauſted receiver. 41. We caus'd a cylinder of box to be turn'd of a length ſuitable to that of the receiver, wherein it was to be employ'd. Out of the lower baſis of this cylinder, which was about an inch and a half in diameter, there came a ſmaller cylinder or axle-tree, not a quarter ſo thick as the other, and leſs than an inch long: this was turn'd very true, that it might move ſmoothly in a little ring of braſs made for it in the midſt of a fix'd trencher, or piece of ſolid wood, ſhap'd like a mill-ſtone; being four or five inches in breadth, and between one and two in thickneſs: and the large round groove, purpoſely made, in the lower part of this trencher, I cauſed to be fill'd up with lead, to keep the trencher ſteady: and in the uppermoſt part of this trencher we intended to have holes made, to place bodies in at ſeveral diſtances, as occaſion ſhould require. The upper baſis of the cylinder had, alſo, another axle-tree coming out of the midſt of it, but wider than the former,

that

that into its cavity it might receive the lower end of the turn-key, to which 'twas to be fasten'd by a slender peg of brass, thrust thro' two correspondent holes, the one made in the turn-key, and the other in the socket of the axle-tree. There were also several horizontal perforations made in the pillar itself, to which this axis belong'd; which pillar we call the vertical cylinder. The general use of this contrivance, is, that the end of the turn-key being put into the socket, and the lower axis of the vertical cylinder into the trencher; by the motion of the key, a body fasten'd at one of the holes to the cylinder may be brought to, or remov'd from, or made to strike against another body, fasten'd, in a convenient posture, to the upper part of the trencher.

We caus'd then a hand-bell without its handle and clapper, to be so fasten'd, to a strong wire, that one end of the wire being fixed in the trencher, the other, which was bent downwards, took hold of the bell. In another hole, made in the circumference of the same trencher, was wedg'd a steel spring, to the upper part whereof was wedg'd a gad of steel less than an inch long, but considerably thick; the length of this spring made the upper part of the hammer, or piece of steel, of the same height with the bell; and the distance of the spring from the bell was such, that when forc'd back the other way, it might, at its return make the hammer strike briskly upon the outside of the bell. The trencher being thus furnish'd and plac'd in a capp'd receiver, the air was diligently pump'd out, and then, by the help of the turn-key, the vertical cylinder was made to go round, by which means, as often as one of the two stiff wires, or small pegs, that were fasten'd at right angles into holes made near the bottom of the cylinder, pass'd by the spring, they forcibly bent it in their passage from the bell, so that as soon as the wire was gone by, and the spring ceas'd to be press'd, it would fly back with violence enough to make the hammer give a smart stroke upon the bell. And, by this means, we could both continue the experiment at discretion, and make the percussions more equally strong than it would otherwise have been easy to do.

Now, when the receiver was well emptied, it sometimes appear'd doubtful whether any sound were produc'd or no; but to me, for the most part, it seem'd, that, after great attention, I heard a very faint and languid sound, and yet methought it had some shrilness in it, and seem'd to come from afar. But letting in the air, at competent intervals, it was easy to observe, that the vertical cylinder being still made to go round, when a little air was let in, the stroke of the hammer upon the bell became very audible: when more air was admitted, the sound grew greater, and so increas'd till the receiver was again replenish'd with air; tho', even then, the sound was observ'd to be much less than when the receiver did not interpose between the bell and the ear.

We now, also, suspended in the receiver a watch with a good alarum; and to make this experiment the more accurate, we employ'd a receiver that consisted of but one piece of glass furnish'd on the inside with a glass knob or button, to which a string might be ty'd: we also hung the watch, not by its chain, but by a very slender thread, whose upper end was fasten'd to the glass button. Then the air being carefully pump'd out, we silently expec-

ted.

PNEUMATICS. ted the ringing of the alarum; but hearing no noise so soon as we expected, it might have been doubted, whether the watch continued going, if we had not contriv'd a way to discern its motion: wherefore, I desired a gentleman to hold his ear exactly over the button, at which the watch was suspended, and very near to the receiver; who told us, that he could just perceive something of a sound, which seem'd to come from far; tho', neither we, who listen'd very attentively near other parts of the receiver, nor he, if his ears were no more advantageously plac'd, were satisfied, that we heard the watch at all. Then letting in some air, we did, with attention, begin to hear the alarum, whose sound was odd; and by returning the stop-cock, to keep any more air from entering, we kept the sound thus low for a considerable time; after which, a little more air, that was permitted to enter, made it become more audible; and when the air was yet more freely admitted, we could plainly hear the alarum at a considerable distance from the receiver *.

A glass-drop broke in an exhausted receiver.

42. The blunter part of a glass-drop being fasten'd to a stable body, and convey'd into the receiver, and the crooked stem being ty'd to one end of a string, whose other end was fasten'd to the turn-key, we carefully pump'd out the air; when the stem, by shortning the string, being broken off, the glass-drop was shatter'd into a thousand pieces.

This experiment was, afterwards, repeated with the like success; and having, at that time, no gage to try how far the air had been drawn out, we let the external air impel up the water out of the pump into the receiver, and thereby found, that the vessel had been well exhausted.

Light produced in the exhausted receiver.

Fig. 62.

43. Knowing, that hard sugar, being briskly scraped with a knife, affords a sparkling light; so that one would sometimes think sparks of fire flew from it; we caus'd a lump of hard loaf-sugar to be conveniently, and firmly plac'd in the cavity of our capp'd receiver; and, to the vertical cylinder, formerly mentioned, we fasten'd some pieces of a steel-spring, which, being but thin, might, in their passage along the sugar, grate or rub forcibly against it; and, then the receiver being well exhausted, in the night-time, and in a dark room, the vertical cylinder was made, for a pretty while, to move round, by help of the turn-key. Thus the irons that came out of the vertical cylinder, making, in their passage, vigorous impressions upon the sugar, that stood in their way, there were manifestly produced many little flashes; and sometimes too, tho' not frequently, there seem'd to be struck off small sparks of fire †.

44. We

* That sound cannot be propagated thro' a vacuum, appears more fully from an experiment of the late Mr. *Hauksbee*, who included a large bell in a receiver full of common air, and cover'd them both with another glass, out of which, the air being extracted, tho' sound was actually produced in the innermost, it could not be heard by the by-standers. *Philos. Trans.* N°. 321. p. 367.

From some other experiments of the same person, 'tis also evident, that sounds are as well augmented in condensed air, as diminish'd in that, which is rarified. See his *Physico-mechan. Experiments.* p. 129. 134.

† From a variety of experiments, relating to the attrition of bodies *in vacuo*, made by the late Mr. *Hauksbee*, it appears, that

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44. We took a large inverted cucurbit for a receiver, made very clear by wiping, and obſerv'd, that when the pump began to be work'd, if a large candle were held on the other ſide of the glaſs, upon turning the ſtop-cock to let the air out of the receiver into the cylinder, the glaſs would ſeem to be full of fumes, and a kind of halo appear about the flame of the candle; and this, at firſt, was commonly between a blue and a green, but after ſome ſucks, turn'd of a reddiſh or orange colour, both very vivid. The phenomenon, in my opinion, proceeded from hence, that the cement being ſomewhat ſoft, and abounding with turpentine, and having a hot iron apply'd to it, whereby it was both ſoften'd and heated, it ſeems rational to expect, that, upon withdrawing the air in the receiver, the aerial particles in the cement freed from their former preſſure, would extricate themſelves, and with the looſer ſteams of the turpentine, and perhaps of the bees-wax, expand themſelves, with a kind of exploſion, in the receiver; and by their interpoſition between the light and the eye, exhibit thoſe delightful colours we had ſeen. And, I afterwards found, that I could plainly perceive the colouring ſteams, juſt upon turning the ſtop-cock, to fly up from the cement towards the top of the glaſs; and, if we continued pumping, the receiver would grow clearer, and the colours more dilute, poſſibly becauſe the aerial and volatile particles of the upper part of the cement did, in that time, ſpend themſelves; and alſo, becauſe the agitation they receiv'd, from the heat communicated by the iron, continually decay'd. Beſides, when the receiver is more exhauſted, the want of air makes it more difficult for ſteams to float, and be ſupported in it.

A kind of halo, and colours produced in the exhauſted receiver.

But, for a farther confirmation, I caus'd ſome cement to be put into a ſmall crucible, warm enough to melt it; and conveying this into a clear receiver, I caus'd the pump to be work'd: upon which, it manifeſtly appear'd, that, opening the ſtop-cock, to let out the air, the ſteams would copiouſly be thrown about from the crucible into the capacity of the receiver; and, after having play'd there a little, fall down again. But, in theſe phenomena, the vividneſs, and ſometimes the kind of the exhibited colours ſeem'd much to depend on circumſtances, ſuch as the degrees of heat, the magnitude and ſhape of the receiver, the quantity of air that remain'd therein, and the nature of the cement itſelf.

45. Croſs the ſtable trencher, formerly mention'd, we faſten'd a ſtrong ſpring of ſteel, ſhaped almoſt like the lathe of a croſs-bow; and to the midſt of this ſpring was ſtrongly fix'd on the outſide a round piece of braſs, hollow'd almoſt like a concave burning-glaſs. To this piece of braſs, which was thin, and about two inches in diameter, we fitted a convex piece of the ſame metal, almoſt like a gage for a tool to grind glaſſes in, which had belong-

Heat produced by attrition in the exhauſted receiver.

Fig. 63.

that different ſorts of bodies afford lights greatly differing in colour, force, and vigour; that the effects of attrition vary with the different preparation and management of the bodies which ſuſtain it; that bodies, which have yielded a particular light, may, by attrition, be brought to yield no more thereof; and that a conſiderable light is producible, by the attrition of glaſs on glaſs, both *in vacuo*, in common air, and even in water. *Hauksb. Phyſico-mechan. Exp.* p. 40—44.

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PNEUMATICS. ing to it a square handle, whereinto, as into a socket, was inserted a square piece of wood, proceeding from the basis of a square wooden pillar, which we made use of, on this occasion, instead of our vertical cylinder. By the help of another piece of wood, coming from the other basis of the same pillar, the turn-key was join'd to this pillar, and made of such a length, that when the turn-key was forcibly kept down as low as the brass-cover, it was a part of, would permit, the convex piece of metal just describ'd, depress'd the concave piece a pretty way, notwithstanding a vigorous resistance of the subjacent spring. A little fine powder of emery was also put between the convex and concave pieces of brass, to make them fit the better, and to facilitate the motion that was to be made; and, to the upper part of the turn-key was fasten'd a good wimble, without which, we presum'd, that the turning of the key would not produce a sufficient motion. Things being thus in readiness, and a mercurial gage convey'd into the receiver, we caus'd the air to be diligently pump'd out, and then order'd a strong man to turn the wimble, and to continue to lean a little on it, that he might be sure to keep the turn-key from being lifted up by the spring. Whilst the man, with much agility and strength was moving the wimble, I watch'd the gage, to observe, whether the agitation of the stop-cock, and consequently the engine, did not prejudice the experiment; and for greater caution, I caus'd the pump to be almost all the while kept working. When the man was almost out of breath, we let in the air at the cover of the receiver, by lifting up the turn-key; and nimbly removing the receiver, we felt both the pieces of brass, betwixt which the attrition had been made, and found them very sensibly warm.

We afterwards caus'd the man to lay hold of the wimble again, when, by the gage, it appear'd, that the receiver was well exhausted; so that by further pumping the quick-silver seem'd not to be further depress'd. And, in this second trial, when we did, as before, hastily let in the air, and take out the bodies that had been rubb'd against one another, they were both of them, especially the uppermost, so hot, that I could not endure to hold my hand on either; and they did, for some time, retain a considerable degree of warmth. I also caus'd two bodies of wood to be turn'd, for size and shape like those of brass, which we had just before employ'd; the upper of these was of hard oak, the other of beech: but, tho' the wimble was swiftly turn'd, as before, by the same person, the wood seem'd not to me to have manifestly acquired any warmth; yet, that there had been a considerable attrition, appear'd by the great polish, which part of the wood had evidently acquired: however, upon repeating the experiment, with more obstinacy than before, the wood, especially the upper piece of it, was brought to a warmth unquestionably sensible.

Quick-lime slaked in the exhausted receiver.

46. Into an evaporating glass, we put a convenient quantity of water; and having convey'd it into a receiver, and well drawn out the air, we let down into it, by the turn-key, a large lump of strong lime; and observ'd not, that, at the first emersion, nor for some time after, there appear'd any considerable number of bubbles; but within about a quarter of an hour, the lime

began

began, (the pump continuing to be ply'd, from time to time,) to ſlake with much violence, and with bubbles wonderfully great, appearing at each new exſuction; ſo that the inſide of the receiver, tho' large, was, at length, lined with lime-water; and much of the mixture did, from time to time, overflow the veſſel, a great part whereof was purpoſely left unfill'd: nor did any thing, but our wearineſs, put a period to the bubbling of the mixture, whoſe heat was ſenſible even on the outſide of the receiver, and continued conſiderably hot, in the evaporating glaſs, for a quarter of an hour after the receiver was remov'd. The lime, employ'd in this experiment, was of a very good and ſtrong kind, made of hard ſtones, and not of chalk, as is that commonly uſed at *London*, which, probably, would not have been ſtrong enough to have afforded us the ſame phenomenon. PNEUMATICS.

47. To try, by means of our ſyringe, formerly mention'd, what weight a cylinder of uncompreſs'd air included in it, and conſequently of the ſame diameter with the cavity of the barrel, would be able to ſuſtain; we provided a ſtable frame, wherein the ſyringe might be kept firm and erect: we alſo provided a weight of lead, ſhaped like our braſs-ring, formerly deſcrib'd, that, by the advantage of its figure, it might be made to hang down, by ſtrings, from the top of the handle of the rammer, and ſo preſs evenly on all ſides, without rendring the upper part of the inſtrument top-heavy. We took care to leave between the bottom of the ſyringe, which was firmly cloſ'd with ſtrong cement, and that part of it, where the ſucker was, a convenient quantity of air to expand itſelf, and lift up the weight, when the air external to that included, ſhould be pump'd out of the receiver. And laſtly, the handle of the rammer, from which the annular weight depended, was ſo faſten'd to the turn-key of the cover of the receiver, that the weight might not compreſs the air included in the ſyringe, but leave it in its natural ſtate, till the air was withdrawn from the receiver. *An attempt is measure the force of the ſpring of included air.*

By this method, the included air would lift up a weight of ſeven, or eight pound; yet, when the rammer came to be clogg'd with a greater, the inſtrument prov'd not ſo ſtanch, but that it was eaſier for ſome particles of air to get away between the ſucker, and the inſide of the barrel, than to raiſe ſo great a weight. But, if an exact ſyringe can be procured, this ſeems to be one of the likelieſt, and leaſt exceptionable ways of meaſuring the force of the air's ſpring.

But, being unable to procure ſuch a ſyringe as I deſired, I got two hollow cylinders to be turn'd, whoſe ſides were of a ſufficient thickneſs to reſiſt the preſſure of the air to be impriſon'd in them; one an inch in diameter, and the other two: their depths were alſo unequal, that the one might receive a much larger bladder than the other. With the leſſer of theſe, I made a diligent trial; but found it very difficult to procure a bladder ſmall, and fine enough for the cylinder: and that which we, at length, procured, would not continue ſtanch for many trials; but, after a while, parted with a little air in the well exhauſted receiver, when 'twas clogg'd with the utmoſt weight it could ſuſtain: but whilſt it continued ſtanch, we made *Fig.* 64.

 one fair trial with it; from whence we concluded, that a cylinder of air of an inch in diameter, and less than two inches in length, was able visibly to raise a weight of above ten pound, averdupoize.

At another time, into a hollow cylinder of wood four inches deep, and two in diameter, furnish'd with a broad and solid body or pedestal, we put a lamb's-bladder very strongly ty'd at the neck; on which, we set a wooden plug, mark'd with ink, where the edge of the cylinder was contiguous to it: this plug being loaded with weights, amounting to thirty-five pound, the receiver was exhausted, till the mark appear'd very manifestly above the brim of the cylinder; and then, tho' the string was, by turning the key, quite slacken'd, yet the mark on the plug continued very visible. And, when so much air was let into the receiver, as made the weight depress the plug quite beneath the mark, upon pumping out the air again, the weight was, without the help of the turn-key, lifted up; and by degrees, all the mark of the plug was rais'd above three eighths higher than the edge of the cylinder. Wherefore, we substituted for the seven pound weight, one of fourteen; and using the same bladder, we repeated the experiment; only a little supporting the uppermost weight by the turn-key, till the bladder had attained its expansion; and then the weight, being gently let go, depress'd not the plug so low, but that we could yet see the mark on it; tho' that part of the plug where the mark was, appear'd manifestly more depress'd than the other.

An easy way of making a small quantity of included air raise a great weight. Fig. 65.

48. We took a brass-vessel, made like a cylinder, and having one of its orifices exactly cover'd with a flat plate firmly fasten'd to it, the other orifice being wide open: the depth of this vessel was four inches, and the diameter three and three quarters. To this hollow cylinder we fitted a wooden plug, like one of those described in the foregoing experiment; only it was not quite so long, and was furnished with a lip, which we, purposely, made of a considerable breadth, that it might afford a stable basis to the weight that should rest upon it: then, taking a middle-sized limber bladder, strongly ty'd at the neck, but not near full blown; we pressed it, by the help of the plug, into the cylinder, that it might the better fit itself to the figure thereof: then, taking notice, by a mark, how much of the plug was extant above the orifice of the vessel, we laid the weights upon the plug, whose lip hinder'd it from being depressed too deep into the cavity of the vessel; and, having convey'd them into the receiver, we found, that a common half hundred weight would very soon be manifestly raised by the spring of the included air.

Fig 64.

In another experiment, the bladder in a cylinder four inches broad, raised 75 pound weight, till the wooden plug disclosed the mark design'd to shew the height at which the air kept the plug, before it was compressed; and this, visibly, at the fifth exsuction; and at the seventh, that mark was raised $\frac{3}{16}$ above the edge of the cylinder. In the gage, where the mercury, in the open air, usually stood, about an eighth above the uppermost glass-mark, it was depressed an eighth below the second mark; and after we let in the air, it was a pretty while before the weight manifestly began to subside. The bladder being taken out, and the place it had possessed in the cylinder

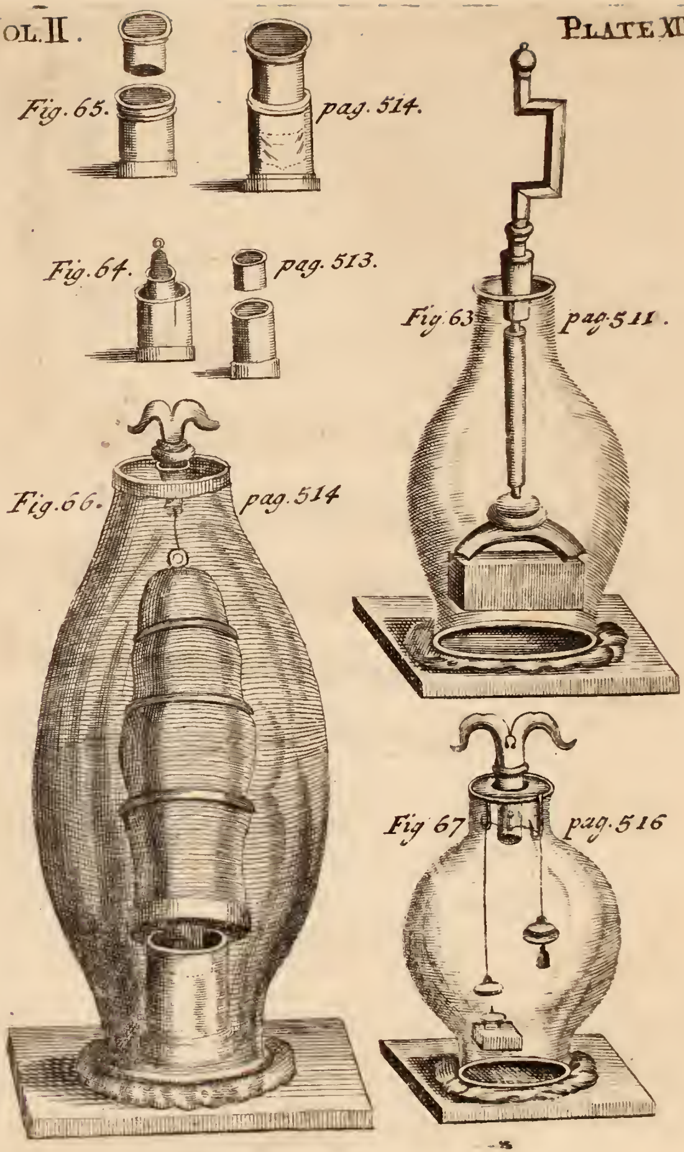
Fig. 65.
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cylinder being ſupply'd with a ſleeve, or ſome ſuch thing, and the weight laid again upon the plug; we found, that, at twenty-four exſuctions, the mercury was depreſſed to the loweſt mark of the gage; and the thirty-fourth, or thirty-fifth exſuction was made, before the receiver appear'd to be ſo exhauſted, as to ſtop the ſinking of the mercury, which was then above one eighth beneath the loweſt mark. But, having cauſed leaden-weights to be, purpoſely, caſt flat, and as broad as we could conveniently put into the receiver, that, by the advantage of this ſhape, we might be able to pile up the more of them, without much danger of their being ſhaken down; we laid ſeveral of them one upon another: and, then, the upper part of the receiver growing too narrow to admit any more; we added a weight, or two, leſs broad; when, exhauſting the receiver, till we perceiv'd, by the gage, that the air was manifeſtly withdrawn; we found, by the help of a mark, and a pair of compaſſes, the plug to be ſo far rais'd, that 'twas concluded, the elevation would have been much greater, if the included air had not found it eaſier to produce ſome leak at the neck of the bladder, than to lift up ſo great a weight; which was about a hundred pounds, averdupoize.

To ſhew the weight of air to that of water.

49. We weigh'd a ſeal'd bubble in the receiver, and found it above half a grain heavier when much of the air was exhauſted, than when it was full: afterwards, we took out this bubble, and found it to weigh ſixty-eight grains and a half; then, breaking off the ſmall tip of it under water, we found, that the heat, by which it was ſeal'd up, had rarify'd its included air, ſo that it admitted a hundred and twenty-five grains of water: for the admitted water and glaſs weigh'd a hundred ninety-three grains and a half. Then, filling it full of water, we found it to contain, in all, ſeven hundred and thirty-nine grains; for it weigh'd eight hundred and ſeven grains and a half: whence, 'tis evident, that the difference between the weight of water and air, was leſs than 1228 and 1. We, alſo, weigh'd, in the receiver, a bubble, the glaſs of which amounted to ſixty grains; the air that fill'd it, weigh'd, *in vacuo*, $\frac{27}{32}$ of a grain; the water that fill'd it, weigh'd ſeven hundred twenty grains and a quarter: ſo that, by this experiment, the proportion of the weight of water to air, is as 853 $\frac{17}{27}$ to 1.

But it is ſo deſirable a thing, and may prove of ſuch importance, to know the proportion in weight betwixt air and water, that I ſhall here mention an attempt tI made to diſcover it by another way.

A ſmall receiver, being exhauſted of air by the engine, and counterpois'd; whilſt it continu'd ſo, the ſtop-cock was turn'd, and the air re-admitted; which made it weigh thirty-ſix grains more than before: and this happen'd, alſo, upon repeating the experiment.

We, next, took a ſmall glaſs-receiver, fitted with a ſtop-cock; and, having exhauſted it of the air, counterpoiz'd it, and let in the outward air; we found the weight of the veſſel to be increas'd, by that admiſſion, thirty-ſix grains. This done, we took the receiver, after having well counterpoiz'd it, out of the ſcale; and having apply'd it a ſecond

Pneumatics. time to the engine, we once more withdrew the air; and then turning the ſtop-cock, to keep out the external air, we took care that none of the cement employ'd to join it to the engine, ſhould ſtick to it: when, weighing it again, we found it thirty-five grains heavier, than when 'twas laſt counterpois'd in the ſame balance. Then we immers'd the ſtop-cock into a baſon of fair water, and let in the liquor, that we might find how much of it would ſucceed in the place of the air drawn out. When no more water was impell'd in, we turn'd the ſtop-cock once more, to keep it from falling out; and, then, weighing it in the ſame ſcales, we found the water to be forty-ſeven ounces three drams, ſix grains; which, divided by thirty-five grains, the weight of the air, equal in bulk to this water, the quotient, is, nearly, ſix hundred and fifty grains, for the proportion of weight between air and water, of the ſame bulk, at the time when the experiment was made: the atmoſphere then appearing, by the barometer, wherein the mercury ſtood, at twenty-nine inches three quarters, to be very heavy.

Two marbles ſtrongly join'd together, ſeparated by withdrawing the air from them.

50. We took a pair of flat round marbles, each of them two inches, and about three quarters, in diameter; and, having put a little oil between them, to keep out the air; we hung, at a hook faſten'd to the lowermoſt, a pound weight, to ſurmount the coheſion which the tenacity of the oil, and the imperfect exhauſtion of the receiver might give them: then, having ſuſpended them in the cavity of a receiver, by a ſtick that lay croſs it, and the engine being made ready to work, we ſhook it more ſtrongly than we concluded it would be by the operation; and, beginning to pump out the air, we obſerv'd the marbles to continue join'd, till it was ſo far drawn out, that we ſuſpected they would not ſeparate. But, at the ſixteenth ſtroke, upon turning the ſtop-cock, which let the air paſs out of the receiver into the pump, the ſhaking of the engine being over, the marbles, ſpontaneouſly, fell aſunder; tho' they hung parallel to the horizon, and adhered very firmly together, when they were put in: and tho' a weight, of above eighty pounds, faſten'd to the lowermoſt marble, might be drawn up, together with the uppermoſt, by virtue of their firm coheſion.

Fig. 67. But, faſtening to the lowermoſt of the two marbles, a weight of a very few ounces, and having cemented a capp'd receiver, with the marbles in it, as before, to the pump; we, by means of a ſtring, (whereof one end was tied to the bottom of the turn-key, and the other to the uppermoſt marble, and paſſing thro' the hook belonging to the braſs-cover) and by turning round the key, drew up the upper marble; and, by reaſon of their coherence, the lowermoſt, alſo, together with the weight that hung at it. Being thus ſure that the two marbles ſtuck cloſe together, we began to pump out the air; and, after a while, the marbles fell aſunder.

But, having ſo order'd the matter, that the lower could fall but a little way from the other; we were able, by inclining and ſhaking the engine, to place them together again: and, then, letting in the air haſtily, that, by its ſpring, it might preſs them hard together; we could not only, by turn-

turning the key, make the uppermoſt marble take up the other, and the annex'd weight; but were oblig'd to make a much more laborious exhauſtion of the air, to procure the disjunction of the marbles, this ſecond time, than was neceſſary to do it at the firſt. PNEUMATICS.

And, when the marbles were thus aſunder, and the receiver exhauſted, we did not let in the air, till we made them fall upon one another, as before; but the little highly expanded air, that remain'd in the receiver, having not a ſpring near ſtrong enough to preſs them together, we very eaſily, by turning the key, rais'd the uppermoſt marble alone, without finding it to ſtick to the other. We, therefore, once more join'd the marbles together, and, then letting in the external air, found them, afterwards, to ſtick ſo cloſe, that a very ſtrong man could not ſeparate them.

That 'tis difficult to produce flame without air, ſhewn by an attempt to kindle ſulphur in vacuo.

51. Into a ſmall earthen melting-pot of a cylindrical figure, and well glaz'd, we convey'd a ſmall cylinder of iron, about an inch long, and an inch and a half in diameter, made red hot in the fire; and having ſuddenly exhauſted the receiver, wherein we plac'd them, we let down a piece of paper, containing a convenient quantity of flowers of ſulphur, upon the heated metal; whereby, the paper being immediately deſtroy'd, the included ſulphur would lie upon the iron, whoſe upper part was a little concave, to contain the flowers, when melted. But all the heat of the iron, tho' it made the paper and ſulphur ſmoke, would not actually kindle either.

Into a glaſs-bubble of a convenient ſize, furniſh'd with a neck fit for our purpoſe, we put a little flower of brimſtone, and having exhauſted the glaſs, and ſecur'd it againſt the return of the air; we laid it upon burning coals, where the ſulphur did not take fire, but roſe to the oppoſite part of the glaſs in the form of a fine powder; and that part being turn'd downward, and laid on coals, the brimſtone without kindling roſe again in the form of an expanded ſubſtance, which, when remov'd from the fire, was, for the moſt part, tranſparent like a yellow varniſh.

The efficacy of air in the production of flame.

52. To examine whether, when a heated iron would not keep the melted brimſtone ſo hot, as was requiſite to make it burn, without air, or with very little, it would yet ſuffice to kindle the ſulphur, if the air had acceſs to it, we made two or three ſeveral trials; and found, that, if ſoon after the flame was extinguiſh'd the receiver were remov'd, the ſulphur would preſently take fire again, and flame as vigorouſly as before. But, we ſuſpected, that the agency of the air, in the production of the flame, might be ſomewhat leſs, than theſe trials would perſuade; becauſe, by taking off the receiver, the ſulphur was not only expos'd to freſh air, but alſo advantaged, by a free liberty for the avolation of thoſe fumes, which, in a cloſe veſſel might be unfavourable to the flame.

And, to try at how great a degree of rarifaction of the air, it was poſſible to make ſulphur flame, by the aſſiſtance of an adventitious heat, we repeated the ſame experiment; the pumping being continued for ſome time after the flame of the melted brimſtone ſeem'd quite extinguiſh'd, till the receiver was judg'd to be very well exhauſted: then, without ſtirring the glaſs, we very warily let in a little air; upon which we could perceive, PNEUMATICS. ceive, tho' not a constant flame, yet several little flashes, as it were, disclose themselves, by their blue colour, to be sulphureous; yet the air that had sufficed to re-kindle the sulphur, was so little, that two exsuctions drew it out again, and put an entire stop to the phenomenon. And, when a little air was cautiously let in again, the like flashes began again to appear; which, upon two exsuctions more, quite vanished: tho', upon letting in a little fresh air, the third time, they, once more, re-appear'd.

An attempt to fire gun-powder in vacuo, by the sun's rays.

53. Having conveniently placed three or four grains of gun-powder in our receiver, and carefully drawn out the air, we threw the sun-beams, united by a good burning-glass, upon the powder, and kept them there, for a considerable time, to little purpose; till, at length, the powder, instead of taking fire, only melted, like a metal. And this was not the only experiment we then made, which discover'd a great indisposition, even in gun-powder, to be fir'd *in vacuo*.

By means of a hot iron.

54. We took a convenient weight of gun-powder, that was extraordinary strong, and well made; and, having placed a red-hot iron in our receiver, that was capable of holding sixteen pounds of water; when the air appear'd, by the mercurial gage, to have been well exhausted, we let down a small piece of thin paper, wherein the powder had been put, till it reach'd the plate; by whose heat, we hoped, the paper would be destroy'd, and the powder made to go off. But, tho' both of them had been previously well dry'd by the fire, no explosion of the powder ensued; yet there appear'd, upon the iron-plate, a broad blue flame, surprizingly durable, and resembling that of brimstone. At length, taking off the receiver, we found, that the paper, contiguous to the iron, was, in part, destroy'd by the heat; but most of the grains of the powder seem'd unalter'd, and retain'd their disposition to be fir'd, notwithstanding the consumption made of their brimstone.

Upon repeating this experiment, we found no explosion to be made for so long a time, that, thinking it in vain to wait, we let in the air; and, after we had, also, despair'd of any effect from hence, the powder suddenly went off, with a great flash, and a considerable shake of the receiver, that was yet standing on the engine: which shews, that such experiments should be made with caution; for tho' this receiver would contain two gallons of liquor, the powder, here employ'd, weigh'd but one grain.

A heated glass, emptied of air.

55. Into a large strong glass-bubble, we put a few small corns of gun-powder; and, having carefully exhausted the glass, and secured it against the return of the air, we put it upon live-coals, superficially cover'd with ashes; by the heat whereof, the sulphureous ingredient of the powder was, in part, kindled, and burn'd blue for a pretty while, and with a flame considerably great; upon the ceasing whereof, the powder, which, after all, did not take fire, appear'd to have sent up, besides the flame, a large quantity of sulphureous sublimate, that stuck to the upper part of the glass: and, being held against a lighted candle, it exhibited several vivid colours, like those of the rain-bow.

56. We

Pneumatics.

And by means of ſparks of fire in vacuo.

56. We took a ſmall, and very ſhort piſtol, and having well faſten'd it, with ſtrings, to a great weight, that was placed upon the iron-plate of our engine, we drew up the cock, and primed the pan with dry powder; then, over both the weight and piſtol, we whelm'd a receiver, capable of containing two gallons of liquor; and, having carefully cemented it on, we cauſed the air to be diligently pump'd out; having, before, put in a mercurial gage, to help us to diſcern when it was well exhauſted. Laſtly, ordering the pump to be plied, in the mean time, for fear ſome air ſhould ſteal in; we, by ſhortning a ſtring that was tied to the trigger of the piſtol, did all we could towards firing of the powder in the pan: but tho' the pan were made to fly open, the powder did not go off; then, letting in the air, and cocking the piſtol again, we drew out a little air, to be ſure that the receiver was cloſely cemented on; when, letting in the air at the top of the receiver, and ſtopping it in, we pull'd the trigger again: whereupon, tho' there had been no new powder put into the pan, nor any left in it, but the little that remain'd after the late trial; yet that little readily took fire, and flaſh'd in the pan: which made it the more probable, that, in the former trial, ſparks of fire had been ſtruck out, by the colliſion of the flint and ſteel. Beſides, in another trial, made, the ſame hour, in the ſame exhauſted receiver, a ſpark, or two, were ſeen to fly out, upon the falling of the cock. It appears, therefore, that, notwithſtanding the great indiſpoſition of gun-powder to be reduced into flame, *in vacuo*, yet even ſolid matter is not incapable of being fir'd there, if put into a motion ſufficiently vehement.

Two ways of making Aurum fulminans go off in vacuo.

57. The rays of the ſun, being thrown upon ſome *Aurum fulminans*, placed in an exhauſted receiver, made it go off, and violently ſcatter about the cavity of the glaſs a yellowiſh duſt, which other trials, in the free air, made us look upon as particles of the gold, that was the principal ingredient of this odd compoſition.

This experiment we repeated, long after, in another place, with other veſſels, and found the like ſucceſs. And once, in the night-time, putting upon a heated iron, $\frac{1}{5}$ of a grain of good *Aurum fulminans*, of our own preparing, looſely tied up in a piece of thin paper, we found, that after the powder had lain long enough upon the iron, to be throughly heated, it went off all together, and with a conſiderable flaſh.

Flame difficultly preserved, without air, in ſulphur.

58. Upon a thick, metalline plate, we put a convenient quantity of flowers of ſulphur; and, having kindled them in the air, ſuddenly convey'd them into a receiver, and made haſte to pump out ſome of the included air; as ſoon as the pump began to be ply'd, the flame appear'd to be ſenſibly decay'd; and continued leſs at every exſuction of the air; and, in effect, expired before the air was quite drawn out. And, upon the ſudden removal of the receiver, it only afforded, for a very little time, ſomewhat more ſmoke in the open air, than it appear'd to do before.

59. Upon a larger cylinder of iron, than the former, made red-hot, we let down a moderate lump of brimſtone, in a receiver moderately exhauſted; when, being kindled, it ſent up a great flame, with large fumes.

How-

PNEUMATICS. However, we ſtill ply'd the pump, drawing out, together with the air, much ſulphureous, and offenſive ſmoke; whereby, though the flame ſeem'd ſomewhat gradually impair'd, yet it manifeſtly continued burning much longer than, by the ſhort duration of other flames in our receivers, one could expect. And once, particularly, in making this experiment, the flame laſted, till the receiver was judg'd to be throughly exhauſted; and ſome thought it ſo surviv'd the exhauſtion, that it went not out for want of air-fewel; the brimſtone appearing, when we took off the receiver, either to have been conſumed by the fire that fed on it, or to have caſually ran off from the iron, the heat whereof had kept it conſtantly melted.

A durable flame of a metalline ſubſtance in vacuo.

60. Having obtain'd a ſaline ſpirit, which, by an uncommon way of preparation, was made exceeding ſharp, and piercing, we put into a vial, capable of containing three or four ounces of water, a convenient quantity of new filings of pure ſteel; which, being moiſten'd in the vial, with a little of the ſaline menſtruum, were, afterwards, drench'd with more; whereupon the mixture grew very hot, and yielded large and fetid fumes. And ſo inflammable was this ſmoke, that, upon the approach of a lighted candle, it would readily take fire, and burn with a bluiſh, and ſomewhat greeniſh flame, at the mouth of the vial, for a conſiderable time together; and that, tho' with little light, yet with more ſtrength than one would eaſily ſuſpect.

This flaming vial, therefore, we convey'd into a receiver, which he who uſed to manage the pump affirm'd, would be exhauſted by about ſix exſuctions; and the receiver being well cemented on, upon the firſt ſuck, the flame ſuddenly appear'd four or five times as great as before; becauſe, as we ſuppoſed, upon the withdrawing of the air, and, conſequently, the weakning of its preſſure, numerous bubbles were produced in the menſtruum; which breaking, ſupply'd the neck of the vial with inflammable ſteams; and theſe, we thought, took fire, with ſome noiſe. Upon the ſecond exſuction of the air, the flame blazed out, as before; and ſo it, likewiſe, did upon the third; but, after that, it went out: nor could we re-kindle any fire, by ſuddenly removing the receiver; only we found, that there remain'd ſuch a diſpoſition in the ſmoke to inflammability, that holding a lighted candle to it, a flame quickly enſued.

The flame of ſpirit of wine impregnated with a metal in vacuo.

61. Having ſo united highly rectified ſpirit of wine with a prepared metal, that they would afford a viſibly ting'd flame; we put this mixture into a ſmall glaſs lamp, furniſh'd with a very ſlender wieck, which the mixture would not burn, whilſt there was liquor enough left to moiſten it well; and putting this lighted lamp into a convenient part of a receiver, able to hold two gallons of water, we made haſte to cement on the glaſs to the engine; yet found not, in two or three ſeveral trials, that, after the pump began to be work'd, ſo little a quantity of ting'd flame laſted more than half a minute.

We alſo obſerv'd, in repeating this experiment, that when the flame began to decay, the turn-key, being now and then drawn almoſt out, the ting'd flame once laſted a minute and half, and another time longer; that longer; that the turn-key being, from the firſt, taken out, the flame laſted two minutes; that, in the ſame caſe, a pipe being bedded in the cement, at the bottom of the glaſs, and open at both ends, each almoſt as big as the orifice fill'd by the turn-key, the ting'd ſpirit ſeem'd to burn as if the flame would have laſted very long, had we permitted it; and laſtly, that the orifice, at the top, being ſtopp'd with the turn-key, tho' the pipe were left open at the bottom, it plainly, in a ſhort time, ſeem'd greatly to decay, and ready to expire; but cauſing one to blow in gently at the pipe, with a pair of bellows, tho' this did not keep the flame vigorous, yet it continued alive for above four minutes; and then obſerving it to be manifeſtly ſtronger than it was, when we began to refreſh it with the bellows, we ceas'd from blowing, and found, that tho' the glaſs pipe was ſtill left open, yet, within about one minute, the flame entirely vaniſh'd. PNEUMATICS.

62. Eminent writers, both ancient and modern, tell us, without ſcruple, that naptha and camphire will burn under water; but I had never the good fortune to ſee them do ſo; and doubt, theſe writers deliver not what they affirm from experience. And tho', in celebrated authors, I have met with many compoſitions, that will not only burn under water, but be kindled by it; yet I found thoſe I have had occaſion to conſider, to be ſo lamely, or ſo darkly, and ſome of them, I fear, ſo falſely ſet down, that by the following compoſition, how ſlight ſoever it may ſeem, I have been able to do more than with things they ſpeak very promiſingly of. *Flame preſerv'd under water.*

We took of gun-powder, three ounces; of well burn'd charcoal, one dram; of good ſulphur or flower of brimſtone, half a dram; of choice ſalt-peter, a dram and half: theſe ingredients being reduced to powder, and diligently mix'd without any liquor, we fill'd a large gooſe-quill with it; for the kindling whereof, the open orifice of the quill, or pipe, was carefully ſtopp'd with a convenient quantity of the ſame, made up with as little chymical oil, or water, as would bring it to a fit conſiſtence. This wild-fire we kindled in the air; and the quill, together with a weight to which 'twas tied, to keep it from aſcending, we ſlowly let down to a convenient depth, under water; where it would continue to burn, as appear'd by the great ſmoke it emitted, and other ſigns, as it did in the air; becauſe the ſhape of the quill kept the dry mixture from being acceſſible to the water, at any other part than the orifice; and there the ſtream of fired matter iſſued out with ſuch violence, as inceſſantly beat off the neighbouring water, and kept it from entering into the cavity that contain'd the mixture, which, therefore, would continue burning, till 'twas conſumed.

63. In trying to kindle a combuſtible ſubſtance, in our exhauſted receiver, it happen'd to fall beſide the iron, whereby our intended experiment was defeated; but whilſt we were conſidering what was to be done on this occaſion, and had not yet let in the air, nor brought in the lights that were removed out of the room, we were ſurprized to ſee ſomething burn like a pale, bluiſh flame, almoſt in the midſt of the cavity of the receiver; and, at firſt, ſuſpected it to be ſome deception of the ſight: but, all the by- *An odd phenomenon of the flame of a metal in vacuo.*

Pneumatics. ftanders perceiving it alike, and obferving that it grew very broad, we look'd at it with great attention, and found it to laft much longer, than I remember to have feen any flame in an exhaufted receiver. I fhould have expected that it proceeded from fome brimftone fticking, unobferv'd, to a part of the iron we had formerly employ'd to kindle fulphur, had we not, juft before, kept it red-hot in the fire. But tho' we much wonder'd whence this flame proceeded, we did not haften its extinction; and at length, when it expired of itfelf, we let in the air, and perceiv'd, upon the concave part of the iron, which we judg'd to be the place where the flame had appear'd, a piece of melted metal, fuppofed to have been faften'd to the ftring whereto the fewel we defign'd to kindle, had been tied, in order to let it down the more eafily; and this made us conceive, that the ftring happening to be burned, by the exceffive heat of the iron, the piece of metal fell into the cavity of it; and, that by the fame heat, the more combuftible part, which the chymifts call the fulphur, was melted, and kept on fire, and continued burning, as we have related. The piece of metal was judg'd to be lead, but having not, formerly, obferv'd fuch a difpofition in lead, to be inflam'd, I confider'd it attentively, and perceiv'd, that 'twas fome fragment of a mixture of lead and tin, that I caus'd to be melted in a certain proportion. Upon this account, it feems, the mixture of the ingredients had acquired fuch a new texture, as fitted the mafs to afford this odd phenomenon; which argues, that there may be flames of metalline fulphurs produced as eafily, without the concurrence of the air, as that of common fulphur; and continue to burn longer than that in our vacuum.

Actual flame propagated with difficulty in vacuo.

64. Having placed our cylindrical plate of iron, firft brought to be red-hot, in a receiver, capable of containing two gallons of water; and having, alfo, diligently pump'd out the air, we kindled a little fulphur, upon the heated plate; and then a piece of dry'd fpunck, tied to a ftring, was let down to the flame. When the experiment was finifh'd, and the fpunck taken out, we found it, in feveral places, not manifeftly alter'd fo much as in colour; and, in thofe parts that had been moft expos'd to the flame, it was turn'd to a fubftance very different from afhes; being black, and brittle as tinder, and, like that, exceedingly difpofed to kindle, upon the touch of fire.

An attempt to make flame kindle camphire, without the help of air.

65. Into the fame receiver, we convey'd the fame cylindrical plate of iron; and, when the air had been thoroughly pump'd out, we let a piece of fuch brimftone down upon the hot iron, as would there kindle with the heat. A little above this fulphur, we had tied to the fame ftring, a piece of camphire; that being a body exceedingly apt to take fire, or, as it were, to draw it at the flame of lighted brimftone: but our fulphur, melting with the heat of the iron, dropp'd from the ftring 'twas faften'd to. As foon as it came to the bottom, where it was diftant from the vehement heat of the metal, the flame expired; but a part of it, that happen'd to ftick to the fide of the iron, was inflamed by it, and the flame reach'd the camphire, without being able to make it blaze.

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We, alſo, attempted to kindle one piece of ſulphur *in vacuo*, by the flame of another, tied a little lower on the ſame ſtring, that it might firſt touch the heated iron, and be thereby ſet on fire; but tho' we could find nothing amiſs in the kind of ſulphur, we then uſed, yet we were not able, even by a repeated trial, to make it take fire upon the iron; where, neverthelefs, it melted, and ſeem'd, a little to boil. PNEUMATICS.

A third trial was not ſo unſucceſsful; for, having, in the receiver, well exhauſted, let down a card-match, upon a very hot iron, the lower extreme of it was kindled thereby. But though the ſulphurated part of the match thus flamed away, yet the remaining part, which was a mere piece of card, was not thereby turn'd into flame; nor, in moſt places, ſo much as ſenſibly ſcorch'd, or black'd, though it had been purpoſely dry'd before-hand.

Gun-powder, tho' fired itſelf, fires not the contiguous grains in VACUO.

66. Upon a paper, laid on a convenient part of the plate of the engine, we made a train of dry powder, as long as the glaſs would well cover; then, carefully faſtening on the receiver, we exactly pump'd out the air: which done, we took a good burning-glaſs, and, about noon, caſt the ſunbeams thro' it, upon a part of the train; but the indiſpoſition of the powder to fire was ſo great, that it ſmoked, and melted, without going off. We afterwards employ'd a thinner, and more tranſparent receiver, which ſo little weaken'd the ſun's rays, that being kept obſtinately upon the ſame part of the train, they were able to fire ſeveral parts, one after another, tho' they could not cauſe the flame to propagate; only thoſe parts that were melted, did, at length, kindle, and fly away, leaving the reſt unalter'd: ſo that I found ſeveral little maſſes of diſſolved matter, in ſeveral parts of the train, with the powder unchang'd in all the others. And ſome of theſe maſſes were contiguous to grains of the powder, which both appear'd unchang'd, and kindled readily, and flaſh'd all away, as ſoon as I caus'd the burning-glaſs to be applied to them in the open air.

67. For farther confirmation of ſo odd an experiment, I ſhall add, that to try whether by the help of one of thoſe little inſtruments, wherewith the ſtrength of powder is commonly examin'd, we could find any difference made by the abſence and preſence of the air, in the reſiſtance of the inſtrument, or the effects of the powder on it; we faſten'd it to a competently heavy, and commodiouſly ſhaped weight of lead: and when 'twas carefully fill'd, and primed with powder, we placed it in a receiver of a convenient bigneſs; whence we pump'd out the air after the uſual manner, tho', perhaps, with more than uſual diligence. But tho', at length, after the powder had long reſiſted the beams of the ſun, thrown on it by a good double convex-glaſs, it took fire at the touch-hole, and fill'd the receiver with ſmoke; yet this kindled powder could not propagate the flame, to that which was in the box, how contiguous ſoever the parcels were to one another: though, when the inſtrument was taken out into the air, where the touch-hole appear'd to be free; as ſoon as ever new priming, with the ſame ſort of powder, was put in, the whole very readily went off. And when we caus'd the inſtrument to be new charged; and, upon its

PNEUMATICS. firing only at the touch-hole in the exhausted receiver, order'd new priming to be added, without so much as taking the instrument out of the glass; tho', afterwards, this was clos'd again, but, without being exhausted, the powder, closely shut up in the glass, readily went off; as well that which was in the box, or cavity, of the powder-tryer, as that which lay on the outward part of it. And this experiment was repeated, with the like success.

Two different trials with different events, to kindle gun-powder in vacuo.

68. A few corns of gun-powder, being included in a very small bubble, freed from air, and secured against the return of it, and then apply'd warily to coals cover'd with ashes, did neither go off, nor burn; but afforded a little yellow powder, that seem'd to be sulphur, sublimed to the upper part of the glass. But two larger bubbles, tho' strong, whereof one had the air but in part, and the other totally evacuated, being provided, each of them, with a greater quantity of powder; a while after they were put upon quick-coals, they were both blown to pieces, with a report almost like that of a musquet: but, tho' this was done in a dark place, yet we did not perceive any real flame produced.

Experiments, shewing the relation betwixt air, and the Flamma vitalis *of animals; and, first, an animal, included with the flame of spirit of wine.*

69. We put a spoonful of highly rectify'd spirit of wine, into a small glass-lamp, conveniently shaped, and purposely blown, with a very small orifice, at which we thrust in a slender cotton-wieck; we, also, provided a tall glass-receiver, in length eighteen inches, that contain'd above twenty pints of water. This receiver, which was open at both ends, had its upper orifice cover'd with a brass-plate, fasten'd on very close with good cement; and, for the lower orifice, which was far the widest, we had provided a brass-plate, furnish'd with a competent quantity of the cement we employ'd to keep the air out of the pneumatical engine; by means whereof, we could sufficiently close the lower orifice of our receiver, and hinder the air from getting in at it. We, then, lighted up the small glass-lamp, and placed it, together with a green-finch, upon the brass-plate, and, in a trice, fasten'd it to the lower orifice of the receiver, and then watch'd the event; which was, that, within two minutes, the flame, after having, several times, almost disappear'd, was utterly extinguish'd: but the bird, tho', for a while, he seem'd to close his eyes, as tho' he were sick, appear'd lively enough, at the end of the third minute, when I caus'd him to be taken out.

After he had, by being kept in the free air, recover'd, and refresh'd himself, the former trial was repeated; and, at the end of the second minute, the flame of the lamp went out: but the bird seem'd not to be endanger'd, by being detain'd a while longer.

After this, we put in, with the same bird, two lighted lamps at once, whose flames lasted not one whole minute, before they went out together; but the bird appear'd unhurt, after having been kept five or six times as long, before we took off the receiver.

In the tall receiver, above-mention'd, we included a mouse, with a lighted lamp, fill'd with the spirit of wine; but, before the experiment was near finish'd, the mouse, being at liberty within the glass, made shift to

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extinguish the flame; which, being revived, without taking out either the lamp, or the animal, the spirit of wine burned about a minute longer; during which time, the mouse appear'd not be grown sick, no more than when, for some minutes after the extinction of the flame, he had been kept in the same close and infected air.

Pneumatics.

We, afterwards, placed the same mouse in another receiver, which seem'd less, by a third, than the former; and in it we, also, fix'd a piece of slender wax-candle, which continu'd burning, in this receiver, but for one minute; and, during that, it emitted much smoke: the animal, nevertheless, appear'd lively, even after we had kept him much longer in that infected air. And the same candle, without being taken out, was lighted again, but burned not so long as before; yet it sufficed to darken the receiver, and, therefore, probably, much clogg'd the included air: in which, nevertheless, the mouse being kept for eight or ten minutes longer, he appear'd, neither when taken out, nor a while before, to have receiv'd any considerable harm from his detention.

The duration of a bird's life, compared with the duration of a burning coal and candle, in vacuo.

70. We included a green-finch, and a piece of lighted candle, in a great capp'd receiver, capable of containing two gallons of water, and very carefully cemented on to the pump: in this glass, we suffer'd the candle to burn, till the flame expired, which it did within less than two minutes; whilst the bird seem'd to be in no danger of sudden death; and, tho' kept a while longer in that clogg'd and smoaky air, he appear'd well, when the receiver was remov'd. We, afterwards, put the same bird into the receiver, with a piece of a small wax-taper; whose flame, tho' it lasted longer than the other, yet the bird out-liv'd it: and, 'twas judged, he would have done so, tho' the flame had been much more durable. After this, we included the same bird, with the former candle, in the receiver, which we had caus'd to be often blown into with a pair of bellows, to drive out the smoke, and infected air; and, then, beginning to pump, we found, that the flame began to decay more suddenly, and the bird to be much more discompos'd, than in the former experiments: but still he surviv'd the flame, tho' not without convulsive motions.

We repeated the experiment with a piece of wax-taper, and the same bird, which, tho' cast into dangerous symptoms, upon the gradual evacuation of the air, out-liv'd, not only the flame, but the smoke too, that issued from the kindled wieck; a circumstance that was, also, observ'd in the preceding trial. Lastly, having freed the receiver from smoke, and supply'd it with fresh air; we put in, with the same bird, a piece of charcoal, of about two inches in length, and half an inch in breadth, which, just before, had been well blown with a pair of bellows; immediately pumping out the air, till none of the fire could be discern'd, and till it seem'd irrecoverable, by the admission of the outward air; which being, afterwards, admitted, the bird was, indeed, very sick, yet capable of a very quick recovery. And this experiment we, with the same animal, and re-kindled coal, made over again, with the same success.

PNEUMATICS. *Glow-worms, and their luminous matter, in vacuo.*

71. We took two glow-worms, that ſhone vividly, eſpecially one of them, whoſe light appear'd ſtrong, and ting'd, as if it had been tranſmitted thro' a blue glaſs; theſe we laid upon a little plate, which we included in a ſmall receiver, of finer glaſs than ordinary; and, having remov'd the candles, that no other light might obſcure that of the inſects, we waited in the dark, till it was conſpicuous, and then order'd the air to be pump'd out; and, upon the very firſt exſuction, there began to be a diminution of the light, which grew gradually dimmer, as the air was more withdrawn; till, at length, it quite diſappear'd. This darkneſs, having been ſuffer'd to continue a long while in the receiver, we let in the air again, whoſe preſence reſtor'd, at leaſt, as much light as its abſence had depriv'd us of. This experiment was repeated, with the addition of one more of thoſe inſects; when they all three gradually loſt their light, by the exhauſtion of the receiver, and regain'd it, by the return of the air. And here we let in the air by degrees, and with an interval or two; and obſerv'd, that as the light was gradually diminiſh'd, upon withdrawing the air; ſo the returning ſplendor was gradually increas'd, as we let more in upon the worms.

72. 'Tis known, that if glow-worms be kill'd, whilſt they are ſhining, their luminous matter may continue to ſhine long after 'tis taken out of their bodies. And, having put ſome of that, we took out of the foremention'd inſects, upon a little paper, and included it in the receiver we employ'd; the candles being remov'd; we perceiv'd it to ſhine vividly, before the pump was ſet on work; and, afterwards, to grow dimmer, by degrees, as the air was exhauſted, till, at length, it quite vaniſh'd: but it re-appear'd immediately upon the air's return. This experiment was repeated twice more, with the ſame ſucceſs. But we took notice, that the luminous matter, after the air was let in, ſeem'd not only to have regain'd its former degree of light, but to have acquir'd a greater; as it once happen'd, alſo, in the experiment made on the living worms. It was ſomewhat ſtrange, to obſerve, that ſo very ſmall a quantity of air, as we at firſt let in, before the light revived, was ſufficient to make it become plainly viſible, tho' dim: in which ſtate it continu'd, till we thought fit to admit more air.

73. Having, at another time, procured two more of thoſe inſects, whereof one was judg'd to be as large as three ordinary ones; when we had brought them out of the country to *London*, the great worm appear'd to be dead; but, finding him to retain a conſiderable degree of light, in the under part of his tail, we put him into the ſmall receiver, formerly mention'd, to try whether, after the death of the animal, the ſhining matter would retain its former properties; and, at the firſt ſtroke of the pump, the light was not aboliſh'd, but continu'd vivid: and ſo it did, when the air being let in, and again withdrawn, the trial was made a ſecond time. I, afterwards, cauſed the receiver to be exhauſted, once or twice more; and, at length, perceiv'd, that the light began to diminiſh, as the air was withdrawn; and, at laſt of all, it ſo diſappear'd, that we could not ſee it: but, upon the re-admiſſion of the air, the light ſhone vividly, as before, if not more bright.

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This experiment was repeated, with the same succeſs, and both times, the like happen'd to the light of the dead one, and of the living one, that we included with it; tho' there was this diſparity betwixt them, that the luminous part of the dead worm appear'd much larger than that of the living one: and the light of the latter was of a very greeniſh blue; but of the former, a white yellow.

Animals weigh'd before death, and after.

74. A mouſe, weighing about three drams and an half, being put in one ſcale of a very nice balance, was counterpois'd, together with a ſtring, put about his neck in a nooſe; and ſoon after, by drawing the ends of that, we ſtrangled him: when we judg'd him quite dead, we weigh'd him again; and, tho' nothing was ſeen to fall from him; yet, contrary to the receiv'd tradition, that bodies are much heavier dead than alive, we found his weight diminiſh'd about $\frac{7}{16}$ of a grain; which, probably, proceeded from the avolation of ſeveral ſubtile particles, upon his violent and convulſive ſtrugglings in death.

Afterwards, in a larger balance, made for nice experiments, we took a very young kitten, between ten and eleven ounces in weight, and caus'd him to be ſtrangled on the ſame ſcale, wherein he had been put. But, being not immediately diſpatch'd, as young animals of this kind are not eaſily deſtroy'd for want of reſpiration; we found him, by that time he was quite dead, lighter, by four grains.

Experiments to ſhew the nature of reſpiration made upon ducks.

75. Nature, having furniſh'd water-fowl with a peculiar ſtructure of ſome veſſels about the heart, to enable them, when they dive, to ſuſpend, for a while, the act of reſpiration under water, without prejudice; I thought fit to try, whether ſuch animals, would, much better than others, ſuſtain the want of the air in our exhauſted receiver.

We put a full grown-duck into a receiver, whereof ſhe fill'd, about a third part; but was unable to ſtand in any eaſy poſture therein: then pumping out the air, tho' ſhe ſeem'd, at firſt, to continue well, ſomewhat longer than a hen in her condition would have done; yet, within one minute, ſhe appear'd much diſcompos'd, and, between that and the ſecond minute, her convulſive motions encreaſed ſo much, that, her head, hanging careleſly down, ſhe ſeemed to be juſt at the point of death; from which, we preſently reſcued her, by letting in the air. And, to manifeſt, that it was not the cloſeneſs and narrowneſs of the veſſel, that produced this great, and ſudden change, we, ſoon after, included the ſame bird in the ſame receiver; and, having cemented it on very cloſe, we ſuffer'd her to ſtay, thus ſhut up with the air, five times as long as before, without perceiving her to be diſcompos'd; and, ſhe might, probably, have continued longer in the ſame condition.

76. Having procured a duckling, that was yet callow, we convey'd her into the ſame receiver, wherein the former had been included, and obſerved, that, tho', for a while, ſhe appear'd not much diſorder'd, whilſt the air was pumping out; yet, before the firſt minute was ended, ſhe gave manifeſt ſigns of being much diſcompoſed: and the operation being continued a while longer, convulſive motions enſued ſo faſt, that, before the

ſecond

PNEUMATICS. ſecond minute was expired, we were obliged to let in the air, whereby ſhe quickly recover'd.

When the receiver was pretty well exhauſted, the included bird, appear'd manifeſtly bigger, than before the air was withdrawn, eſpecially about the crop; tho' that was very turgid before. We, alſo, kept the ſame duckling, in the ſame receiver, very cloſe, for above ſix minutes, without perceiving her to grow ſick upon her impriſonment; which, yet laſted above thrice the time, that before ſufficed to reduce her to a gaſping condition.

It not being intended, that water-fowl, ſhould, any more than other birds, live in an exceeding rarified air, but, only be able to continue, upon occaſion, under water, it may ſuffice, that the contrivance of theſe parts relating to reſpiration, be barely fitted for that purpoſe.

Upon vipers. Vipers being endowed with lungs, tho' of a different ſtructure from thoſe of other animals; and their blood being, as to ſenſe, actually cold; I thought, it might be worth trying, what effect the abſence of the air would have upon them.

77. *Jan.* 2. We included a viper in a ſmall receiver, and as we drew out the air, ſhe began to ſwell, and afforded us theſe phenomena.

1. It was a long while after we had left pumping, before ſhe began to ſwell, ſo much as forced her to gape, which, afterwards, ſhe did.

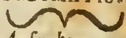
2. She continued, above two hours and a half, in the exhauſted receiver, without giving clear proof of her being killed.

3. After ſhe was once ſo ſwelled, as to be compell'd to open her jaws, ſhe appear'd ſlender and lank again; and yet, very ſoon after, appear'd ſwell'd again, and had her jaws disjoin'd as before.

78. Including a viper in a ſmall receiver, we emptied it very carefully; when the viper moved up and down within, as if it were to ſeek for air; and, after a while, foamed a little at the mouth, and left of the foam, ſticking to the inſide of the glaſs: her body ſwelled, not conſiderably, and her neck leſs, till a pretty while after we had left off pumping; but afterwards the body and neck grew prodigiouſly tumid, and a bliſter appear'd upon her back. An hour and an half after the receiver was exhauſted, the diſtended viper, gave, by motion, manifeſt ſigns of life; but, we obſerved none afterwards. The tumor reach'd to the neck, but did not ſeem much to ſwell the under-jaw. Both the neck, and a great part of the throat, being held betwixt the eye and the candle, were tranſparent, where the ſcales did not darken them. The jaws remain'd mightily open'd, and ſomewhat diſtorted; the *Epiglottis*, with the *Rimula Laryngis*, (which remain'd gaping) was protruded almoſt to the further end of the under-jaw. As it were, from beneath the *Epiglottis*, came the black tongue, and reached beyond it, but ſeemed, by its poſture, not to have any life; and the mouth alſo was grown blackiſh within: but, the air being re-admitted, after twenty-three hours in all, the viper's mouth was preſently cloſed, tho', ſoon after, it was opened again, and continued long ſo; whilſt ſcorching or pinching the tail, made a motion in the whole body, that argued ſome life.

79. *April*

79. *April* 25. We included an ordinary, harmleſs ſnake, together with a gage, in a portable receiver, which, being exhauſted, and well ſecured againſt the ingreſs of the air, was laid aſide in a quiet place, where it continued from about ten or eleven a-clock in the forenoon, till about nine the next morning: and then, looking upon the ſnake, though he ſeemed to be dead, and gave no ſigns of life, upon the ſhaking of the receiver, yet, upon holding the glaſs, at a convenient diſtance, from a moderate fire, he, in a ſhort time, manifeſted himſelf alive, by ſeveral tokens; and even by putting out his forked tongue. In this condition I left him, and came not to look upon him again, till the next day, early in the afternoon; at which time, he was grown paſt recovery, and his jaws, which were formerly ſhut, gaped exceeding wide, as if they had been ſtretched open by ſome external violence. PNEUMATICKS. *A ſnake.*



80. *Sept.* 9. We included a large, luſty frog, in a ſmall receiver, drew out the air, and left her not very much ſwell'd, and able to move her throat; tho' not ſo faſt as when ſhe breathed freely, before the exſuction of the air. She continued alive for about two hours, that we took notice of, ſometimes removing from one ſide of the receiver to the other; ſhe ſwell'd more than before, and did not appear, by any motion of her throat, or thorax, to exerciſe reſpiration; but her head was not very much ſwell'd, nor her mouth forced open. After ſhe had remained there above three hours, perceiving no ſign of life in her, we let in the air, upon which the tumid body ſhrunk very much, but ſeemed not to have any other change wrought in it; and tho' we took her out of the receiver, yet, in the free air, ſhe continued to appear ſtark dead. But, having cauſed her to be laid upon the graſs, in a garden, all night, we found her, the next morning, perfectly alive again. *Frogs.*

81. *June* 29. About eleven of the clock in the fore-noon, we put a frog into a ſmall receiver, containing about fifteen ounces, and one quarter, troy-weight, of water; out of which we had, tolerably well, drawn the air: (ſo that when we turn'd the cock under water, it ſuck'd in about thirteen ounces, and one quarter, of water,) the frog continued, the receiver being all the while under water, lively, till about five of the clock in the afternoon, when ſhe expired. At the firſt ſhe ſeemed not to be much alter'd by the exſuction of the air, but continued breathing, both with her throat and lungs.

82. *Sept.* 6. We included into a pretty large receiver, two frogs newly taken; the one not above an inch long, and proportionably ſlender; the other, very large and luſty. Whilſt the air was drawing out, the leſſer frog ſkipp'd up and down very lively, and, ſeveral times, clamber'd up the ſides of the receiver, ſo that he ſometimes wreſted himſelf againſt the ſides of the glaſs. When his body ſeemed to be perpendicular to the horizon, if not in a reclining poſture, he continued to skip up and down a while, after the exſuction of the air; but, within a quarter of an hour, we perceived him to lie ſtark dead, with his belly upwards. The other frog, that was very large and ſtrong, tho' he began to ſwell much upon withdrawing the air, and

PNEUMATICS. ſeemed to be diſtreſs'd, yet he held out half an hour; when it was remarkable, that the receiver, though it had withſtood the preſſure of the outward air, during that ſpace, notwithſtanding a piece of it had been crack'd out, but cemented in again, yet at the end of the half hour, the weight of the outward air ſuddenly beat it in, and thereby gave the impriſon'd frog relief.

83. *Sept.* 11. We convey'd a ſmall frog into a very ſmall portable receiver, and began to pump out the air. At firſt ſhe was lively, but when the air was conſiderably withdrawn, ſhe appear'd to be very much diſquieted; yet not ſo, but that, after the operation was ended, and the receiver taken off, ſhe was perfectly alive, and continued to appear ſo, for near an hour, tho' the abdomen was very much, and the throat ſomewhat extended; the latter having, alſo, left off the uſual panting motion, which is ſuppoſed to argue and accompany the reſpiration of frogs. At the end of about three hours and a quarter, after the removal of the receiver from the pump, the air was let in; whereupon the abdomen, which, by that time, was ſtrangely ſwell'd, not only ſubſided, but ſeemed to have a great cavity in it, as the throat, alſo, proportionably had; which cavities continued after the frog was gone paſt all recovery.

84. *April* 14. A large frog was convey'd into a plated receiver, and the air being withdrawn, her body, by degrees, diſtended. The receiver, with the gage, was kept under water near ſeven hours; at the end of which, I found it tight, but the frog dead, and exceedingly ſwell'd: upon letting in the air, ſhe became more hollow and lank than ever.

Kitlings. 85. We took a kitling one day old, and put him into a very ſmall receiver, that it might be the ſooner exhauſted; and within about one minute after the air firſt began to be withdrawn, the little animal, which, in the mean time, gaſped for life, and had ſome violent convulſions, lay as dead, with his head downwards, and his tongue out; but, upon letting in the air, he, in a trice, ſhew'd ſigns of life; and, being taken out of the receiver, quickly recover'd. We then ſent for a kitling of the ſame litter, which being put into the ſame receiver, quickly began, like the other, to have convulſions, and after to lie as dead; but, obſerving very narrowly, I perceived ſome little motions, which made me conclude him alive. And accordingly, tho' we continued pumping, and could not perceive that the engine leaked, the kitling began to ſtir again, and, after a while, had ſtronger and more general convulſions than before; till at the end of full ſix minutes after the exſuction of the air was begun, the animal ſeeming quite dead, the outward air was re-admitted into the receiver; which not reviving him, as it had done the other, he was taken out of the veſſel, and lay with his mouth open, and his tongue lolling out, without any ſenſible reſpiration and pulſation; till having order'd him to be pinched, the pain, or ſome internal motion, produced by the external violence, made him, immediately, give manifeſt ſigns of life; tho' there was yet no ſenſible motion of the heart, or lungs: but afterwards gaping, and fetching his breath in an odd manner, and with much ſtraining, as I have ſeen ſome fœtus's do, when cut out of the womb, he, he, by degrees, within about a quarter of an hour, recover'd. We, afterwards, ſent for another, kitten'd at the ſame time; and incloſing that, alſo, in the receiver, obſerv'd the violent convulſions, and, as it were, gaſping for breath, into which he began to fall, at the ſecond or third ſuck, ended in a ſeeming death, within about a minute and a half. But, cauſing the pump to be ply'd, the kitling gave manifeſt ſigns of life, after he had endured ſeveral convulſions, as great as thoſe of the firſt fit, if not greater. When ſeven minutes, from the beginning of the exhauſtion, were compleated, we let in the air; upon which, the little creature, that ſeem'd ſtark dead before, made us ſuſpect he might recover: but, tho' we took him out of the receiver, and put *Aqua Vitæ* into his mouth, yet he, irrecoverably, died in our hands.

PNEUMATICKS

86. To determine the quantity of air harbour'd in the pores of fluids, ſeems as difficult as it would be uſeful. To ſhew how little the air, contain'd in water, would appear to leſſen the bulk of the water, if it were ſuffer'd to fly away in an open tube; we let it eſcape, in an exhauſted receiver, without any artifice to catch it: in which trial, the water did not part with any thing of its bulk, that made a diminution ſenſible to the eye. We, therefore, endeavour'd to make this loſs viſible, by ſome other experiments.

Experiments upon the air uſually harbour'd in the pores of water, &c.

A glaſs-tube, ſeal'd at one end, and about thirty-ſix inches in length, being fill'd with water, and inverted into a glaſs-veſſel, not two inches in diameter, and but a quarter of an inch, or little more, in depth; the glaſſes were convey'd into a fit receiver, and the air leiſurely pump'd out, and ſomewhat ſlowly re-admitted; when, the numerous bubbles, that had aſcended, during the operation, conſtituted, at the top, an aerial aggregate, amounting to $\frac{8}{10}$, wanting about an hundredth part of an inch.

87. Preſently after, another tube was fill'd with the ſame water, and inverted; when, the water, being drawn down to the ſurface of that in the veſſel, and the air let in again, the water was impell'd up to the very top, within a tenth, and half a tenth of an inch.

The latter tube was forty-three inches and a half above the ſurface of the ſtagnant water; the air, collected out of the bubbles, at the top of the water, was, the firſt time, above three quarters of an inch; and the ſecond time we eſtimated it, at one half, and one ſixteenth. The firſt time, the water, in the pipe, was made to ſubſide full as low as the ſurface of the ſtagnant water; the ſecond time, the loweſt that we made it ſubſide, ſeem'd to be four or five inches above the ſurface of the water in the open veſſel.

But the air, at the top of the tube, poſſeſs'd more room than its bulk abſolutely required; becauſe it was ſomewhat defended from the preſſure of the atmoſphere, by the weight of the ſubjacent cylinder of water, which, perhaps, was about three or four feet long.

88. We provided a clear round glaſs, furniſh'd with a pipe, or ſtem, about nine inches in length; the globular part of the glaſs being, on the outſide, about three inches and a half in diameter: the pipe of this glaſs

PNEUMATICS. was, within an inch of the top, melted at the flame of a lamp, and drawn out, for two or three inches, as slender as a crow's quill, that the decrease of the water, upon the recess of the air, harbour'd in its pores, might be the more easily observ'd, and estimated. Above this slender part of the pipe, the glass was, nearly, of the same size with the rest of the pipe; that the aerial bubbles, ascending thro' the slender part, might there find room to break, and so prevent the loss of any part of the water.

This vessel being filled, till the liquor reach'd to the top of the slender part; where, not being uniformly enough drawn out, it was somewhat broader than elsewhere; we convey'd the glass, together with a pedestal for it to rest upon, into a tall receiver; and, pumping out the air, there disclosed themselves numerous bubbles, ascending nimbly to the upper part of the glass, where they made a kind of froth: but, by reason of the figure of the vessel, they broke at the top of the slender part, and so never came to overflow.

This done, the pump was suffer'd to rest a while, to give the aerial particles, lodg'd in the water, time to separate themselves, and emerge; when, the pump was ply'd again. These vicissitutes of pumping, and resting, lasted for a considerable time; till, at length, the bubbles began to be very rare: soon after which, the external air was let into the receiver; when, it appear'd somewhat strange, that, notwithstanding so great a multitude of bubbles as had escaped out of the water, I could not, by attentively comparing the place where the surface of the water rested at first, (to which a mark had been affix'd) with that where it now stood, discern the difference to amount to above an hair's breadth: and the chief operator in the experiment profess'd he could perceive no difference at all.

89. Filling a glass of the same shape, and much of the same bigness as the former, with claret-wine, and, placing it upon a convenient pedestal, in a tall receiver, we caus'd some of the air to be pump'd out; whereupon there emerged, thro' the slender pipe, so very great a multitude of bubbles darted, as it were, upwards, as both pleas'd and surpriz'd us; but forc'd us to go warily to work, for fear the glass should break, or the wine overflow: wherefore, we, seasonably, left off pumping, before the receiver was near exhausted, and suffer'd the bubbles to get away as they could, till the danger was past: then, from time to time, we pump'd a little more air out of the receiver; the withdrawing a moderate quantity of air at a time sufficing, even at the latter end, to make the bubbles copiously and swiftly ascend, for above a quarter of an hour together.

The little instrument made use of in these experiments, being design'd to examine, among other things, the quantity of bubbles lurking in several liquors, may be apply'd to spirit of wine, and chymical oils. And some circumstances of our trials made us think, that it might be worth examining, what kind of substance may be obtain'd by this way of treating aerial and spirituous corpuscles.

Pneumatics.

Shell-fish in an exhausted receiver.

90. An oyster, being put into a very small receiver, and kept there long enough to have, successively, kill'd three or four birds, or beasts, &c. was not thereby kill'd, nor, for ought we could perceive, considerably disturb'd; only at each suck we perceiv'd, that the air, contain'd between the two shells, broke out at their commissure; as we concluded from the foam which, at those times, came out all around that commissure. About twenty-four hours after, I found, that both this oyster, and another, that had been put, at the same time, into the receiver, were alive.

On the same day we put a pretty large craw-fish into a large receiver, and found, that tho' he had been before injur'd by a fall, yet he seem'd not to be much incommoded, by being included, till the air was, in great measure, pump'd out; and then his former motion presently ceas'd, and he lay as dead, till, upon letting in a little air, he soon began to move afresh; and, upon withdrawing the air again, he presently, as before, became moveless. Having repeated the experiment two or three times, we took him out of the receiver, when he appear'd not to have suffer'd any harm.

91. Having put an oyster into a vial, full of water, before we included it in the receiver, that thro' the liquor the motion of the bubbles, expected from the fish, might be the better seen, and consider'd; this oyster prov'd so strong, as to keep itself close shut, and repress'd the eruption of the bubbles, that, in the other, forc'd open the shells, from time to time; and kept in its own air, as long as we had occasion to continue the trial.

92. A craw-fish, that was thought more vigorous, being substituted in the place of the former craw-fish, tho' once he seem'd to lose his motion together with the air; yet, afterwards, he continu'd moving in the receiver, in spight of our pumping.

A scale-fish in an exhausted receiver.

93. We took a receiver, shaped almost like a bolt-head, containing near a pint; and the globular part of it, being almost half full of water, we put into it, at the orifice, a small gudgeon, about three inches long; which, when it was in the water, swam nimbly up and down therein: then, having drawn out the air, so that about nineteen parts of twenty, or more, were exhausted; we secur'd the return of the air from injuring our experiment.

Now the neck of the glass, being very long, tho' there appear'd numerous bubbles all about the fish; yet the rest of the water, notwithstanding the withdrawing of so much air, emitted no froth, and but few bubbles.

The fish, both at his mouth and gills, for a long time, discharg'd such a quantity of bubbles, as appear'd strange; and for about half an hour, when ever he rested a while, new bubbles would adhere to many parts of his body, (as if they were generated there) especially about the fins and tail; so that he would appear almost beset with bubbles: and if, being excited to swim, he was made to shake them off, he would quickly, upon a little rest, be beset with new ones, as before.

Almost

PNEUMATICS.

Almoſt all the while, he would gape, and move his gills, as before he was included; tho', towards the end of the time, I watch'd him, he often neither took in, nor emitted any aerial particles that I could perceive.

After a while, he lay almoſt conſtantly with his belly upwards; and, yet, would, in that poſture, ſwim briſkly, as before. Nay, ſoon after, he ſeem'd to be more lively than at firſt putting in.

In about an hour and a half after he had been ſeal'd up, I found him almoſt free from bubbles, with his belly upwards, and ſeeming ſomewhat tumid, yet lively as before. But, an hour and a quarter after that, he ſeem'd to be moveleſs, and ſomewhat ſtiff; yet, upon ſhaking the glaſs, obſerving faint ſigns of life in him, by ſome languid motions he attempted to make, when excited; I open'd the receiever, under water, to try if that liquor, and air, would recover him; when, the external water ruſhing in, till it had fill'd the vacant part of the ball, and the greateſt part of the ſtem, the fiſh ſunk to the bottom of it, with a greater appearance than ever of being alive: in which ſtate, after he had continu'd a pretty while, I, by the help of the water he ſwam in, got him thro' the pipe, into a baſon of water, where he gave more manifeſt ſigns of life. But, yet, for ſome hours, he lay on one ſide or other, without being able to ſwim, or reſt on his belly, which appear'd very much ſhrunk in.

All the while he continu'd in the baſon of water, tho' he mov'd his gills, as before he had been ſeal'd up; yet I could not perceive, that he did, even in his new water, emit, as formerly, any bubbles; tho', two or three times, I held him by the tail in the air, and put him into the water again; where, at length, he grew able to lie conſtantly upon his belly; tho' that retain'd much of its former lankneſs. And he lived, in the baſon, eight or ten days longer; tho' ſeveral gudgeons, ſince taken, died there, in a much leſs time.

Two animals, with large wounds in their abdomen, included in the pneumatical receiver.

94. *Sept.* 12. A ſmall bird, having the abdomen open'd almoſt from flank to flank, without injuring the guts, was put into a little receiver, and the pump being ſet a-work, continued, for ſome little time, without giving any ſigns of diſtreſs; but at the end of about a minute and an half from the beginning, ſhe began to have convulſive motions in the wings; and, tho' the convulſions were not univerſal, or appeared violent, as is uſual in other birds when the air is withdrawn, yet, at the end of two full minutes, letting in the air, and then taking off the receiver, we found the bird irrecoverable, tho' there appear'd no notable alteration in the lungs; and the heart, or, at leaſt, the auricles of it, continued beating for a while after,

95. We took, alſo, on the ſame day, a pretty large frog, and having, without violating the lungs, or the guts, made two ſuch inciſions in the abdomen, that the two curl'd bladders, or lobes of the lungs, came out, almoſt totally, at them; we ſuſpended the frog, by the legs, in a ſmall receiver, and, after we had pump'd out a large part of the air, the animal ſtruggled very much, and ſeem'd to be much diſorder'd; and, when the receiver was well exhauſted, ſhe lay ſtill, for a while, as if ſhe had been dead;

dead; the abdomen and thigh being very much ſwell'd, as if ſome rarify'd air, or vapour, forcibly diſtended them. But as, when the frog was put in, one of the lobes was almoſt full, and the other almoſt ſhrunk up; ſo they continu'd to appear, after the receiver had been exhauſted: but, upon letting in the air, not only the body ceas'd to be tumid, but the plump bladder appear'd, for a while, ſhrunk up as the other; and the receiver being remov'd, the frog preſently revived, and quickly began to fill the lobe again with air.

The motion of the ſeparated heart of a cold animal in the exhauſted receiver.

96. The heart of an eel being taken out, and laid upon a plate of tin, in a ſmall receiver; when we perceiv'd it to beat there, as it had done in the open air, we exhauſted the veſſel, and ſaw, that tho' the heart grew very tumid, and, here and there, ſent out little bubbles, yet it continued to beat as manifeſtly as before, and ſeemed to do ſo more ſwiftly; as we tried by counting the pulſations it made in a minute, whilſt it was in the exhauſted receiver, and when we had re-admitted the air, and alſo, when we took it out of the glaſs, and ſuffer'd it to continue its motion in the open air. The heart of another eel, being, likewiſe, taken out, continued to beat in the empty'd receiver, as the other had done.

97. The heart of another eel, after having been included in an exhauſted receiver, and then accurately ſecured from leaking, tho' it appear'd very tumid, continu'd to beat there an hour; after which, finding its motion very languid, and almoſt ceas'd; by breathing a little upon that part of the glaſs where the heart was, it quickly regain'd motion, which I obſerv'd a while; and, an hour after, finding it almoſt quite gone, I was able to renew it, by the application of a little more warmth. At the end of the third hour, a bubble, that appear'd to be placed between the auricle and the heart, ſeem'd to have, now and then, a little trembling motion; but it was ſo faint, that I could no more, by warmth, excite it, ſo as plainly to perceive the heart to move: wherefore, I ſuffer'd the outward air to ruſh in, but could not diſcern, that, thereby, the heart regain'd any ſenſible motion, tho' aſſiſted with the warmth of my breath and hands.

The times wherein animals may be kill'd by drowning, or withdrawing of the air, compared.

98. *Sept.* 10. A green-finch, having his legs and wings tied to a weight, was gently let down into a glaſs body fill'd with water; the time of his total immerſion being mark'd. At the end of half a minute after that time, the ſtrugglings of the bird ſeeming finiſh'd, when being ſuddenly drawn up again, he was found quite dead.

A ſparrow, very luſty and quarrelſome, was tied to the ſame weight, and let down after the ſame manner; but tho' he ſeem'd to be more vigorous under water than the other bird, and continued ſtruggling almoſt to the end of half a minute, from the time of his total immerſion; (during which, there aſcended, from time to time, large bubbles from his mouth) yet, being drawn up as ſoon as ever the half minute was compleated, we found him, to our wonder, irrecoverably gone.

99. A ſmall mouſe, being held under water by the tail, emitted, from time to time, ſeveral aerial bubbles out of his mouth; and, at laſt, as a ſpectator affirm'd, at one of his eyes: being taken out, at the end of half a minute,

PNEUMATICS. nute, and a few seconds, he yet retain'd some motions: but they prov'd only convulsive ones, which, at last, ended in death.

100. We so tied a considerable weight of lead to the body of a duck, as not to hinder her respiration, yet keep her under water; which we had found a small weight unable to do, by reason of her strength; and even a great one, if ty'd only to her feet, in such a middle-sized vessel as ours was; because of the height of her neck and beak. With this clog, the duck was put into a tub full of clear water, under the surface whereof, she continued quietly for about a minute; but afterwards began to be much disturb'd: the fit being over, and perceiving no motion in her, at the end of the second minute, we took her out of the water; and, finding her in a good condition, after we had allow'd her some breathing-time, to recruit herself with fresh air, we let her down again into the tub, which, in the mean time, had been fill'd with fresh-water; lest the other, which had been troubled with the steams and foulness of the body of the animal, might either hasten her death, or hinder our perceiving what should happen.

The bird being thus under water, after a while, began, and, from time to time, continued to emit bubbles at her beak. There, also, came out at her nostrils several real bubbles, from time to time; and when the animal had continued about two minutes under water, she began to struggle very much, and to endeavour either to emerge, or change posture; the latter of which, she had libetty to do, but not the former. After four minutes, the bubbles came much more sparingly from her: then, also, she began to gape, from time to time; which we had not observ'd her to do before, but without emitting bubbles; and so she continued gaping till near the end of the sixth minute; at which time all her motions, some whereof were judg'd convulsive, and others that had been excited by rouzing her, appear'd to cease, and her head to hang carelesly down, as if she were quite dead. Notwithstanding which, we, for greater security, continued her under water a full minute longer; and then, finding no signs of life, we took her out; when, being hung by the heels, and gently press'd in convenient places, she was made to void a considerable quantity of water: but whether any of it had been received into the lungs themselves, we wanted time and opportunity to examine. All the means we used to recover the bird, proving ineffectual, we concluded, she had been dead a full minute before we removed her out of the water: so that, even this water-fowl, was not able to live in cold water, without taking in fresh air, above six minutes.

101. A duckling, having a competent weight ty'd to her legs, was let down into a tub of water, which reached not above an inch or two above her beak: during the most part of her continuance wherein, there came out numerous bubbles at her nostrils; but there seem'd to proceed more and greater, from a certain place in her head, almost equi-distant from her eyes, tho' somewhat less remote from her neck than they. Whilst she was kept in this condition, she seem'd, frequently, to endeavour at diving lower under water; and, after much struggling, and frequent gaping, she had, several

veral convulſive motions, and then let her head fall down backward, with her throat upwards. To this moveleſs poſture ſhe was reduced at the end of the third minute, if not ſooner ; but, a while after, there appear'd a manifeſt tremulous motion in the two parts of her bill ; which continued for ſome time, and was, perhaps, convulſive : but this alſo, ceaſing, at the end of the fourth minute, the bird was taken out, and found irrecoverable. *Pneumatics.*

102. A viper, that we kept in an exhauſted receiver, till concluded to have been quite dead, was, nevertheleſs, not thrown away, till I had try'd what could be done, by keeping her all night in a glaſs-body, and a warm digeſtive furnace : upon which, this viper was found, the next morning, not only reviv'd, but very lively.

We, therefore, put her into a tall glaſs-body of water, fitted with a cork to its orifice, and depreſs'd it with a weight, ſo that ſhe could have no air. In this caſe, we obſerv'd her, from time to time ; and, after ſhe had been duck'd a while, ſhe lay, with very little motion, for a conſiderable time. After an hour and a quarter, ſhe often put out her black tongue : at the end of near four hours, ſhe appear'd lively ; and, as I remember, about that time, alſo, put out her tongue ; ſwimming, all this while, as far as we obſerv'd, above the bottom of the water. At the end of about ſeven hours, or more, ſhe ſeem'd to have ſome life; her poſture being manifeſtly chang'd in the glaſs, from what it was a while before. Not long after, ſhe appear'd quite dead ; her head and tail hanging down moveleſs, and directed towards the bottom of the veſſel ; whilſt the middle of her body floated as much as the cork would permit it.

103. In the generality of our pneumatical experiments upon animals, it ſuited with our purpoſe, to rarify the air as much, and as ſuddenly as we could ; but I had other trials in view, wherein an extraordinary degree of rarifaction, yet not near the higheſt to which the air might be brought by our engine, ſeem'd likelieſt to conduce ; as particularly to afford ſome light in the nature of thoſe diſeaſes, that are thought, primarily, to affect reſpiration, or its organs. *Animals in air brought to a conſiderable degree of rarifaction.*

Wherefore, having gages, by help of which ſuch experiments might be much the better perform'd, I attempted ſeveral of them in the following manner.

Aug. 16. A linet being put into a receiver, able to hold about 4 pints and a half of water, the glaſs was well clos'd with cement and a cover ; but none of the air drawn out with the engine, or otherwiſe. And tho' no new air was let in, nor any change made in the impriſon'd air, yet the bird continued there three hours, without any apparent approach to death : and tho' ſhe ſeem'd ſomewhat ſick, yet being afterwards taken out, ſhe recover'd, and liv'd ſeveral hours.

104. *Aug.* 18. From the receiver above-mention'd, we drew about half the air, whilſt a linet was in the glaſs ; and in that rarified air (which appear'd by a gage to continue in the ſame ſtate) the bird liv'd an hour and near a quarter before ſhe ſeem'd in danger of death : after which, the air being let in, with-

out taking off the receiver, ſhe manifeſtly recover'd, and leap'd againſt the ſide of the glaſs; and being taken out into the open air, ſhe flew out of my hand to a conſiderable diſtance.

105. *Sept. 9.* Into a receiver, able to hold about 4 pints and a half of water, we convey'd a lark, together with a gage, by the help whereof we drew out $\frac{3}{4}$ of the air; then obſerving the bird, we perceiv'd it to pant very much. Having continued thus for a little above a minute and a half, the bird fell into a convulſive motion, that caſt it upon the back. And altho' we made great haſte to let in the air; yet, before the expiration of the ſecond minute, preceding the convulſion, the lark was gone paſt all recovery, tho' various means were uſed to effect it.

106. *Sept. 9.* Preſently after, we put into the ſame receiver, a greenfinch; and having withdrawn half the air, we ſoon began to obſerve the bird, and took notice, that, within a minute after, ſhe appear'd to be very ſick; and, ſhaking her head, vomited a certain ſubſtance againſt the inſide of the glaſs. Upon this evacuation, the bird ſeem'd to recover, and continue pretty well, but not without panting, till about the end of the fourth minute; when, growing very ſick, ſhe vomited again, but much more unqueſtionably than before; and, ſoon after, eat up again a little of her vomit; upon which, ſhe very much recover'd. And though ſhe had, in all, three fits of vomiting; yet, for the laſt ſeven or eight minutes, that we kept her in the receiver, ſhe ſeem'd to be much more lively than was expected: which may, in part, be attributed to a little air that, by an accident, got in, tho' it were immediately pump'd out again. At the end of a full quarter of an hour, from the firſt exhauſtion, the bird appearing not likely to die in a great while, we took her out.

107. *April* 12. A new-caught viper was included, together with a gage, in a portable receiver, able to hold about three pints and an half of water. This veſſel being exhauſted, and ſecured againſt the return of the air, the animal was obſerv'd, from time to time, not only to be alive, but nimbly to put out, and to draw back her tongue, for about thirty-ſix hours, after ſhe was ſhut up: we, therefore, continued the veſſel longer, in the ſame ſhady place; where, over-night, at the end of ſixty hours. ſhe appear'd very dull and faint, and not likely to live much longer. And, the next, by the afternoon, I found her ſtark dead, with her mouth open'd to a ſtrange wideneſs; wherefore, ſuffering water to be impell'd, by the outward air, into the cavity of the receiver, we found, by the water that was driven in, and afterwards pour'd out again, and meaſur'd, that five parts in ſix of the air in the veſſel, had been pump'd out: ſo that in an air rarify'd, till it expanded itſelf to five or ſix times its uſual dimenſions, our viper was able to live ſixty hours, and, perhaps, might have done ſo longer.

Animals in the ſame parcel of air changed, as to rarity and denſity.

108. In the preceding experiments, the animals were recover'd from a gaſping condition, by letting in freſh air, and not the ſame that had been withdrawn from them: wherefore, I thought proper, to try, whether the ſame portion of air, without being renew'd, would, by being expanded much beyond its uſual degree, and reduced to it again, ſerve to bring an animal

animal to near the point of expiring, and revive him again; ſince, by the ſucceſs of ſuch a tryal, it would notably appear, that the bare change of the conſiſtence of the air, as to rarity and denſity, may ſuffice to produce the above-mention'd effects. PNEUMATICS.

We included a mouſe in a fine, limber, clear bladder, made more tranſparent by oil, rubb'd on the outſide, that the ſmell of it might leſs offend the animal, to be included; clipping off as much of the bladder, at the neck, as we judg'd abſolutely neceſſary for letting in a mouſe: we, alſo, provided a round ſtick, ſomewhat leſs than the orifice; that, the wood being laid over, with a cloſe and yielding cement, we might tye the bladder faſt, and cloſe enough, upon the ſtopple thus fitted. In the bladder was left as much air, as we thought might ſuffice him, for the time the experiment was to laſt. Then, putting this limber, or dilatable receiver into an ordinary one of glaſs, and, placing this engine near a window, that we might ſee through both of them; the air, was, by degrees, pumped out of the external receiver, and, thereupon, the air included in the bladder proportionably expanded itſelf, and ſo diſtended the internal receiver, till, being arriv'd at a degree of rarifaction, which rendred it unfit for reſpiration, I perceiv'd ſigns, in this animal, of his being in great danger of ſudden death. Whereupon, the outward air being haſtily let into the external receiver, compreſs'd the ſwell'd bladder to its former dimenſions, and thereby, the included air to its former denſity; by which means, the mouſe was quickly revived. Having given him ſome convenient reſpite, the experiment was repeated with the like ſucceſs.

An attempt to prevent the neceſſity of reſpiration, by the production or growth of animals, in vacuo.

109. We put a large parcel of tadpoles, with a convenient quantity of water, into a portable receiver, of a round figure, and obſerv'd, that, at the firſt exſuction of the air, they roſe to the top of the water; tho', moſt of them ſubſided again, till the next exſuction raiſed them. They ſeem'd, by their active and wrigling motion, to be very much diſcompos'd. The receiver being exhauſted, they, all of them, continued moving, at the top of the water; and, tho' ſome of them ſeem'd to endeavour to go to the bottom, and dived part of the way, eſpecially with their heads, yet, they were immediately buoy'd up again. Within an hour, or a little more, they were all moveleſs, and lay floating on the water; wherefore, I open'd the receiver; upon which, the air ruſhing in, almoſt all of them preſently ſunk to the bottom, but none of them recover'd life.

110. We, afterwards, included a leſs number of tadpoles in a ſmaller glaſs, which was alſo exhauſted, with the like circumſtances, as the former. And, when I found the other tadpoles to be dead, I haſted to theſe, which did not, except, perhaps, one, give any ſigns of life; but, upon letting in the air, theſe having not been long kept from it, ſome few of them recover'd, and ſwam up and down lively enough; tho', after a while, theſe alſo died.

111. I repeated the ſame experiment in a portable receiver, of a convenient kind; and, tho', after the exhauſtion was perfected, the tadpoles, for a while, moved briskly enough, on the top of the water, only; yet,

PNEUMATICS. at the end of an hour, they ſeem'd to be, all of them, quite dead, bu continu'd floating. And, though, within half an hour after that, I let in the air; yet all the effect of it was, that the moſt of them, immediately ſunk to the bottom, as the reſt, ſoon after, did ; none of them, that I could obſerve, recovering vital mo tion.

112. We procur'd, by preſerving ſome rain-water, four or five of thoſe odd inſects, whereof gnats have, by ſome, been obſerved to be generated about the end of *Auguſt*, or beginning of *September*. Theſe, for ſome weeks, live all together in the water, as tadpoles do; ſwimming up and down therein, till they are ripe for a tranſmigration into flies: but including them, with ſome of their water, in a ſmall glaſs-receiver, which being exhauſted, and very exactly cloſed, we kept, in a ſouth-window; theſe little creatures continued to ſwim up and down therein, for ſome few days, without ſeeming to be much incommoded; but at length, and all much about the ſame day, they put off the habit they had, whilſt they lived as fiſhes, and appeared with their *Exuviæ*, or caſt-coats under their feet; ſhewing themſelves to be perfect gnats, that ſtood, without ſinking, upon the ſurface of the water, and diſcovering themſelves to be alive, by their motion, when they were excited to it; but I could not perceive them to fly in that thin medium: to which inability, whether the viſcoſity of the water might contribute, I know not; tho' they lived a pretty while, till hunger, or cold deſtroyed them.

The expanſion of the blood and other animal fluids.

113. The warm blood of a lamb or a ſheep, being taken as it was, immediately, brought from the butcher's, where the fibres had been broken, to hinder the coagulation, was, in a wide-mouth'd glaſs, put into a receiver, made ready for it; and the pump being ſet on work, the air was diligently drawn out: but the operation was not always, eſpecially at firſt, ſo early manifeſt, as the ſpirituouſneſs of the liquor would make one expect; yet, after a long expectation, the more ſubtile parts of the blood would begin to force their way thro' the more clammy, and ſeem to boil in large cluſters, ſome as big as great beans or nutmegs; and, ſometimes, the blood was ſo volatile, and the expanſion ſo vehement, that it boiled over the containing glaſs; of which, when it was put in, it did not fill above a quarter.

114. Having, alſo, included ſome milk, warm from the cow, in a cylindrical veſſel, about four or five inches high, tho' the pump was long ply'd, before any intumeſcence appear'd, yet, afterwards, when the external air was fully withdrawn, the milk began to boil, in a way, that was not ſo eaſy to deſcribe, as pleaſant to behold: and this it did for a pretty while, with ſo much impetuoſity, that it threw ſeveral of its parts out of the wide-mouth'd glaſs that contain'd it; tho' there were not above two or three ounces of the liquor, which only half fill'd the glaſs.

A yet greater diſpoſition to intumeſcence, we thought, we obſerv'd in the gall; which was but ſuitable to the viſcoſity of its texture.

The two laſt experiments were made with a deſign to ſhew, how far the deſtructive operation of our engine, upon the included animals, might be be imputed the withdrawing of the air, whereby, the little bubbles generated in the blood, juices, and ſoft parts of the body, may, by their vaſt number, and conſpiring diſtenſion, variouſly contract the veſſels in ſome places, and ſtretch them in others; eſpecially the ſmaller, that convey the blood and nouriſhment; and ſo, by choaking up ſome paſſages, and vitiating the figure of others, diſturb, or hinder the due circulation of the blood: for, ſuch diſtenſions may cauſe pains in ſome nerves, and membranous parts, which, by irritating them into convulſions, may haſten the death of animals, and deſtroy them ſooner by that irritation, than they would be deſtroy'd by the bare abſence or loſs of what the air is neceſſary to ſupply them with. And, to ſhew, that this production of bubbles reaches, even to very minute parts of the body, I ſhall add, that, I once obſerved in a viper, furiouſly tortured in our exhauſted receiver, the creature had a conſpicuous bubble moving to and fro, in the aqueous humour of one of its eyes.

115. To ſhew, that not only the blood and liquors, but alſo the other ſoft parts, even in cold animals, have aerial particles latent in them; we took the liver and heart of an eel, as, alſo, the head and body of another fiſh of the ſame kind, cut aſunder, croſs ways, beneath the heart; and putting them into a receiver, upon withdrawing of the air, we perceiv'd, that the liver manifeſtly ſwell'd every way; and, that both the upper part and lower of the fiſh, did ſo, likewiſe. At the place, where the diviſion had been made, there came out, in each portion of the fiſh, various bubbles; ſeveral of which ſeem'd to riſe from the *Medulla Spinalis*, the cavity of the back-bone, or the adjoining parts: and the external air being let in, both the portions of the eel preſently ſunk; ſome of the skin ſeeming to be grown flaccid in each.

116. We included, in a vial with a wide neck, (the whole glaſs being able to contain about eight ounces of water,) a ſmall young mouſe; then tyed ſtrongly upon the upper part of the glaſs's neck a fine thin bladder, out of which the air had been carefully expreſs'd; and convey'd this veſſel into a middle-ſiz'd receiver, in which, we alſo plac'd a mercurial gage. This done, the air was, by degrees, pump'd out, till it appear'd by the gage, that there remain'd but a fourth part in the external receiver; whereupon, the air in the internal receiver, expanding itſelf, appear'd to have blown the bladder almoſt half full; and the mouſe ſeeming very ill at eaſe, by leaping, and otherwiſe endeavouring to paſs out at the neck of his priſon; we, for fear the over-thin air would diſpatch him, let the air flow into the external receiver; whereby the bladder being compreſs'd, and the air in the vial reduced to its former denſity, the little animal quickly recover'd.

The power of uſe to enable animals to ſupport themſelves in air, by rarifaction made unfit for reſpiration.

117. A while after, without removing the bladder, the experiment was repeated, and the air, by help of the gage, reduced to its former degree of rarifaction; when, the mouſe, after ſome fruitleſs endeavours to get out of the glaſs, was kept in that thin air for full four minutes; at the end of which, he appear'd ſo ſick, that, to prevent his dying immediately, we

remov'd

PNEUMATICS. remov'd the external, and took out the internal receiver; whereupon, tho' he recover'd; yet 'twas not without much difficulty; being unable to ſtand any longer upon his feet; and, for a great while after, he continu'd, manifeſtly trembling.

118. But, having ſuffer'd him to reſt for a reaſonable time, preſuming that uſe had inured him to greater hardſhips, we convey'd him, again, into the external receiver; and, having brought the air to the former degree of rarifaction, we were able to keep him there for a full quarter of an hour; tho' the external receiver did not at all conſiderably leak; as appear'd both by the mercurial gage, and by the remaining diſtenſion of the bladder. And, 'tis worth noting, that, till near the latter end of the quarter of an hour, the animal ſcarce at all appear'd diſtreſs'd; remaining ſtill very quiet. And tho', when he was put in, his tremblings were yet upon him, and continu'd ſo for ſome time; yet, afterwards, in ſpight of the expanſion of the air he was then in, they ſoon left him. And, when the internal receiver was taken out, he not only recover'd from his fainting ſooner than before, but eſcaped thoſe ſubſequent tremblings.

119. Encouraged by this ſucceſs, after we allowed him ſome time to recover his ſtrength, we re-convey'd him, and the veſſel wherein he was included, into the former receiver, and pump'd out the air, till the mercury, in the gage, was drawn down near half an inch lower than before, that the air might be yet farther expanded. And, tho' this, at firſt, ſeem'd to diſcompoſe the little creature; yet, after a while, he grew very quiet, and continu'd ſo for a full quarter of an hour; when, we caus'd three exſuctions more to be made, before we diſcover'd him to be in manifeſt danger, (at which time, the bladder appear'd much fuller than before:) but, then, we were obliged to let the air into the outward receiver; whereupon, the mouſe was more ſpeedily revived, than one would have ſuſpected.

Now the air, in which the mouſe liv'd all this while, had been clogg'd, and infected, with the excrementious effluvia of his body; for 'twas the ſame all along; we having, purpoſely, forborn to take off the bladder, whoſe regular diſtenſions, and ſhrinkings, ſufficiently manifeſted, that the veſſel, whereof 'twas a part, did not leak.

Air, become unfit for reſpiration, may retain its uſual preſſure.

120. We took a mouſe, of an ordinary ſize, and, having convey'd him into an oval glaſs, fitted with a ſomewhat long, and conſiderably broad neck, that it might be wide enough to admit a mouſe, in ſpight of his ſtruggling; we convey'd in, after him, a mercurial gage, in which we had carefully obſerv'd, and mark'd the ſtation of the mercury; and which was ſo faſten'd to a wire, reaching to the bottom of the oval glaſs, that the gage, remaining in the neck, was not in danger of being broken by the motions of the mouſe in the oval part. The upper part of the long neck of the glaſs was, notwithſtanding the wideneſs of it, hermetically ſeal'd, by means of a lamp, and a pair of bellows, that we might be ſure the impriſon'd animal ſhould breathe no other air, than what fill'd the receiver, at the time when it was ſeal'd. This done, the mouſe was watched,

watched, from time to time: and tho', by reaſon of the largeneſs of the veſſel, he ſeem'd rather drooping, than very near death, at the end of the ſecond hour; yet, in about half an hour after, he was judg'd to be quite dead, tho' we ſhook the veſſel, to rouze him. The gage manifeſted no ſenſible change in the ſtation of the mercury; but, cauſing the ſeal'd part of the glaſs to be broken off, I obtain'd, after a while, ſome faint tokens of life: tho', I am not ſure, that they would have continu'd in a veſſel, where the air was ſo clogg'd and infected, if freſh air had not been frequently blown in by a pair of bellows, whoſe noſe was inſerted into the neck of the glaſs. This freſh air ſeem'd evidently, tho' but ſlowly, to revive the gaſping animal, which I could not, conveniently, take out of the glaſs, till he had gain'd ſtrength enough to make uſe of his legs; but, after that, without breaking the glaſs, we took him out, and ſoon found him able to walk up and down.

121. A like experiment we, alſo, made with like ſucceſs, upon a ſmall bird, included, with a gage, in a receiver that would hold about a quart of water. The bird, in about half an hour, appear'd to be ſick, and drooping; the faintneſs, and difficulty of breathing, increaſing for about two hours and a half after; at which time, the animal died; the gage being not ſenſibly alter'd, unleſs, perhaps, the mercury appear'd to be impell'd up a little higher than when put in; which, yet, might proceed from ſome accidental cauſe.

122. To ſhew, that it is not want of coldneſs, but ſomething elſe in the included air, that makes it deſtroy birds pent up therein, which, by the hot exhalations of their bodies, may be ſuppoſed to over-warm it; we made the following experiment.

In a glaſs-vial, able to hold about three quarts of water, we, hermetically ſeal'd up a ſmall bird; and found, that, in a few minutes, ſhe began to be ſick, and pant. Theſe ſymptoms I ſuffer'd to continue, and increaſe, till they had laſted juſt half an hour; at which time, having provided a veſſel of water, with ſal-armoniac, newly put therein, to refrigerate it; and the liquor being thus made exceeding cold, the vial, with the ſick bird, was immerſed in it, and ſo kept there for ſix minutes: yet it did not appear, that the great coldneſs which muſt be thus procured to the impriſoned air, ſenſibly revived or refreſh'd the drooping animal, who manifeſtly continued to pant exceedingly. So that this remedy, proving ineffectual, the vial was remov'd out of the water; and the bird, ſome time after, many times ſtrain'd to vomit: and, afterwards, had evacuations downward, before ſhe quite expired; which ſhe did, in almoſt an hour, from the beginning of her impriſonment.

The uſe of the air to raiſe and ſupport the ſteams of bodies, conſider'd, with regard to reſpiration.

123. We made, by diſtillation, a blood-red liquor, chiefly conſiſting of ſuch ſaline, and ſpirituous particles, as may be obtained from human blood; which is of ſuch a nature, that if a glaſs-vial, about half filled with it, be kept well ſtopp'd, it will reſt as quietly as an ordinary liquor, without ſending up any ſmoke, or viſible exhalation; but, if the vial be open'd ſo, that the external air is permitted to come in, and touch the ſurface of the liquor,

PNEUMATICS. within a quarter of a minute, or leſs, there will be elevated a copious white ſmoke, which not only fills the upper-part of the glaſs, but plentifully paſſes out into the open air, till the vial be again ſtopp'd.

When this vial has lain ſtopp'd and quiet for a competent time, the upper half of it appears deſtitute of fumes, whereof the air, it ſeems, will imbibe, and conſtantly retain but a certain moderate quantity; which may give ſome light towards the reaſon, why the ſame air, quite clogg'd with ſteams, will not long ſerve for reſpiration. And if the unſtopp'd vial were placed in our vacuum, it would emit no viſible ſteams at all, not ſo much as to appear in the upper part of the glaſs itſelf that held the liquor; but when the air was, by degrees, reſtored at the ſtop-cock, without moving the receiver, to avoid injuring its cloſeneſs, the returning air would preſently raiſe the fumes, firſt into the vacant part of the vial, whence they would aſcend into the capacity of the receiver: and likewiſe, when the air, requiſite to ſupport them, was pumped out, they alſo accompanied it, as their unpleaſant ſmell made manifeſt; whilſt this red ſpirit, though it remain'd unſtopp'd, emitted no more fumes, till new air was let in.

Snails, a ſlow-worm, and a leech, in vacuo.

124. Two ordinary white ſnails, without ſhells, differing in ſize, (the biggeſt being about an inch and a half, and the other about an inch in length) were included in a ſmall portable receiver, which was carefully exhauſted, and ſecured againſt the return of the air; and preſently after, being removed from the engine, it was eaſy to diſcern, that both the ſnails thruſt out, and drew in their horns, at pleaſure; though their bodies had, in the ſofter places, numerous newly generated bubbles ſticking to them: and tho' they did not loſe their motion near ſo ſoon, as other animals, in our vacuum; yet, after ſome hours, they appear'd moveleſs, and very tumid; and, at the end of twelve hours, the inward parts of their bodies ſeemed to be almoſt vaniſh'd, whilſt they appear'd to be two ſmall full-blown bladders: and, on letting in the air, they immediately ſo ſhrunk, as if the bladders having been prick'd, the receding air had left behind it nothing but skins: nor did either of the ſnails, afterwards, tho' kept for many hours, give any ſigns of life.

125. We included in a receiver, whoſe globular part was about the bigneſs of a large orange, one of that ſort of animals, vulgarly call'd, efts, or, ſlow-worms: having withdrawn the air, and ſecur'd the veſſel againſt the return of it, we kept him there about forty-eight hours; during which, he continued alive, but appeared ſomewhat ſwell'd in his belly; his under-chap moving on the very firſt night, but not the day and night following. At length, by opening the receiver, under water, we perceiv'd, that about half the air had been drawn out. As ſoon as the water was impell'd into the glaſs, the animal, which was before dull and torpid, ſeem'd, by very nimble and extravagant motions, to be ſtrangely revived.

126. We took a leech, of a moderate bigneſs, and having included it, together with ſome water, in a portable receiver, able to hold about ten or twelve ounces of that liquor; the air was pump'd out, after the uſual

manner;

manner, and the receiver being remov'd to a light place, we obſerv'd, that, the leech keeping herſelf under water, there emerg'd from ſeveral parts of her body, numerous bubbles, ſome of them in a diſperſed manner, but others, in rows, or files, that ſeem'd to come from determinate points. Tho' this production of bubbles laſted a pretty while, yet the leech did not ſeem to be very much diſcompoſed. This done, we ſet the receiver, which was well ſecur'd from the outward air, in a quiet place, where we viſited it, at leaſt, once a-day; and found the leech ſomewhat faſtned by her tail, to that part of the glaſs which was under water, and ſometimes wandring about that which was quite above water; and always, when we endeavour'd to excite her, ſhe quickly manifeſted herſelf to be alive; and, indeed, appear'd ſo lively, after the full expiration of five natural days, that expecting ſomething might have happen'd to the receiver, I open'd it under water; when the outward air, impell'd in ſo much of that liquor, as ſatisfy'd me the receiver was well exhauſted.

127. Five or ſix caterpillars, all of the ſame ſort, being put into a ſeparable receiver of a moderate ſize, had the air drawn away from them, and carefully kept from returning. But, notwithſtanding this, I found them, about an hour after, moving to and fro in the receiver; and even above two hours after that, I could, by ſhaking the veſſel, excite ſome motions in them, that I did not ſuſpect to be convulſive. But looking upon them again, about ten hours after they were firſt included, they ſeemed to be quite dead; and, tho' the air were forthwith reſtored to them, they continued to appear ſo: yet, leaving them all night in the receiver, I found, the next day, that three, if not four of them, were perfectly alive. *Creeping inſects in vacuo.*

128. We took from an hedge a branch, that had a large cob-web of caterpillars in it, and dividing it into two parts, we put them into like receivers; and in one of them ſhut up the caterpillars, together with the air, but from the other it was exhauſted. Now, in that which had the air, the little inſects, after a ſmall time, appeared to move up and down as before, and ſo continued to do for a day or two: but in that glaſs, whence the air had been extracted, and continued kept out, they ſhewed, after a very little while, no motion that we could perceive.

129. *Nov.* 12. About 8 a-clock at night, there were taken four middle-ſized fleſh-flies, which, having their heads cut off, were incloſed in a portable receiver, furniſh'd with a large pipe, and a bubble at the end. As ſoon as the receiver was exhauſted, the flies loſt their motion; an hour or two after, I approached them to the fire, which reſtored not their motion: wherefore I let in the air upon them; after which, in a very ſhort time, they began, one after another, to move their legs, and one or two of them to walk. And having kept them all night, in a warm place, they manifeſted, for a while, ſome ſmall motion. *Winged inſects in vacuo.*

130. *Sept.* 11. About noon we cloſed up ſeveral ordinary flies, and a bee, or waſp; all which, when the air was fully withdrawn, lay as dead; only, for a very few minutes, ſome of them had convulſive motions in their legs. They continued in this ſtate forty-eight hours; after which, the

Pneumatics. air was let in upon them; and that not producing any figns of life, they were laid in the noon-day fun: but none of them feem'd, in any degree, to recover.

131. *Dec.* 11. We put a great flefh-fly into a very fmall portable receiver, where, at firft, he appear'd to be very brisk and lively; but, as foon as the air was drawn out, he fell on his back, and feemed to have convulfive motions in his feet, and *Probofcis*; from whence he prefently recover'd, upon letting in the air; which being drawn out again, he lay as dead: but, within a quarter, or half an hour after, I perceiv'd, upon fhaking the receiver, that he ftirr'd faintly up and down. This was done pretty late over night, and next night I found the fly not to be foon revived, either by warmth, or letting in the air. However, in a while after, he recover'd; and being, next morning, fealed up again in that glafs, and kept forty-eight hours, tho' over the chimney, he died beyond recovery.

132. We took a large grafs-hopper, whofe body, befides the horns and limbs, was about an inch in length, and of great thicknefs, in proportion to that length; and convey'd him into a portable receiver of an oval form, and able to hold about a pint of water: and having, afterwards, pumped out the air, till, by the gage, it appear'd to have been pretty well drawn out, we took care that no air fhould re-enter. The fuccefs was this. Firft, tho' before the exhauftion of the air begun, the grafs-hopper appear'd lively, and continued fo for a while: yet, when the air began to be confiderably rarified, he feem'd to be very ill at eafe, and to fweat out of the abdomen, many little drops of liquor, which being united, trickled down the glafs like a little ftream, that made, at the bottom, a fmall pool of clear liquor, amounting to near a quarter of a fpoonful; and by that time the receiver was ready to be taken off, the grafs-hopper was fallen upon his back, and lay as dead. Secondly, tho' having, a little after, laid the glafs in a fouth-window, on which the fun then fhone, I perceived fome flow motions in the thorax, as if he ftrained to fetch breath; yet, I was not fure, but they were convulfive motions; however, they lafted but a while, and then the animal appeared to be quite dead, and to continue fo for three hours, from the removal of the receiver. Thirdly, that time being expired, the glafs was open'd, and the air let in; notwithftanding which, there appear'd no fign at all of life: but letting the glafs reft in a convenient pofture, that the water which came from him, might not endanger him, for a quarter, or half an hour; tho' I then perceived no figns of life, yet I caufed him to be carried into a fun-fhiny place, where the beams of a declining fun prefently began to make him ftir his limbs, and, in a fhort time, brought him perfectly to life again.

133. *April* 15. We took one of thofe fhining beetles called rofe-flies, and having included it in a very fmall round receiver, which we exhaufted, it ftruggled much whilft the air was withdrawing; yet prefently after, I could perceive but little motion: about fix hours after, the fly feemed quite dead, and difcover'd no motion upon that of the glafs. And within about an hour more, tho' I let the air rufh in, yet no fign of life enfued, neither immediately, nor for a pretty while after. So that fufpecting the beetle to be

be really dead, I yet, three or four hours after, found him lively. Whereupon, I caus'd the glafs to be again exhaufted, and fecur'd from the air; during which time, the animal feem'd to be much difquieted, but did not lofe his motion, foon after. PNEUMATICS.

134. With butter-flies I made feveral tryals, and, having obferv'd them, not only to live, but to move, longer than was expected; I chofe to include feveral of them in receivers, fomewhat large, that I might fee, whether, in fo thin a medium, fome or other of them, by help of their large wings, would be able to fly. But, tho', whilft the air continu'd in the glaffes, they flew actively, and freely up and down; and, tho', after the exhauftion of the air, they continu'd to live, and were not movelefs; nay, tho', at the bottom of the receiver, they would even move their wings and flutter a little; yet, I could not perceive any of them to fly, or have a progreffive motion, fupported by the medium, only. And, by frequently inverting the receiver, which was long, they would fall, like dead animals, without difplaying their wings, tho', juft as they came to touch the bottom, fome of them, would, fometimes, feem to make ufe thereof, but not enough to fuftain themfelves, or to break their falls.

135. A number of ants being included in a fmall portable receiver, exhaufted about noon; between fix and feven in the evening, they feem'd to be all quite dead; and the rather, becaufe, tho' they appear'd very lively juft before they were feal'd up, running briskly up and down the bubble they were in; yet, they grew almoft movelefs, as foon as the air was exhaufted; and a little while after appeared more fo: tho' I a little fufpected, that they were much incommoded, by fome glutinous fubftance, that feem'd to have got into the receiver, from the vapours of the cement. When upon opening the glafs, the air rufhed in, no fign of life appear'd, for a great while, in any of the ants: but next morning, about nine a-clock, I found many of them alive, and moving about. *The neceffity of air, to the motion even of ants and mites.*

We convey'd a number of mites, together with the mouldy cheefe, wherein they were bred, into three or four portable receivers, which were, all of them, very fmall, and not much differing in fize. From all of thefe, except one, we withdrew the air; and, then, making ufe of our peculiar contrivance to hinder its return, we took them, one after another, from the engine, and laid them by, for further obfervation. That wherein, to obferve the difference, we left the air, was fealed at a lamp-furnace, after the ufual manner. Our tryals afforded us the enfuing phenomena.

(1.) The mites, inclofed in the fmall glafs, that never came near the engine, continu'd alive, and able to walk up and down, for above a full week after they had been put in; and, poffibly, would have continu'd much longer, if the glafs had not been accidentally broken.

(2.) As foon as ever one of the receivers was remov'd from the engine, I look'd with great attention upon it; and, tho', juft before the withdrawing of the air, the mites were feen to move up and down in it; yet, within a few minutes, after the receiver was apply'd to the engine, I could difcern in them no life at all; nor was any perceiv'd by younger eyes than mine.

Nay, by the help of a double convex-glaſs, I was not able to ſee any of them ſtir up and down. And no motion was taken notice of in the other ſmall receiver of a like ſhape and bigneſs. About an hour after, I look'd upon the receiver attentively again, but could not perceive any of the mites to ſtir; and the like unſucceſsful obſervation I made two or three hours after that. And at firſt letting in the air, to try if the mites were not quite dead; I could not perceive, upon its ruſhing in, any of them to ſtir: yet, I left the receiver unſtopt as it was in the window, upon a ſuſpicion, that the air might not be able to exerciſe its operation upon them, in a ſhort time.

(3.) And, about two or three days after, I found a number of my little animals reviv'd; as an attentive eye might eaſily perceive, by the motion of certain little white ſpecks, when aſſiſted to obſerve it by little marks, that I made on the outſide of the glaſs, (which was purpoſely choſen thin and clear) near this, or that mite, with a diamond; by the approach to, or receſs from which marks, the progreſſive motion became, perhaps, within a minute, plainly diſcoverable; eſpecially, if, when the eye perceiv'd little white ſpecks, that look'd like mites, the receiver ſhould be ſo turn'd, that the bellies and feet of thoſe little creatures were uppermoſt; notwithſtanding which, they would not eaſily drop down, but continue their motion: and theſe ſpecks being made upon the concave ſurface of the thin glaſs itſelf, were thereby render'd much more eaſily viſible.

(4.) But becauſe it doth not, by the third phenomenon, appear, whether, in caſe our mites had been kept in a moveleſs ſtate, for a much longer time, than three or four hours, they would have been recoverable, by the admiſſion of the air; I ſhall add, that one of the portable receivers, above-mention'd, being exhauſted and carefully ſecured from the air, was kept from monday morning to thurſday morning: after all which time, being unable to diſcover any ſigns of life, among the included mites, the air was let in upon them, which, ſoon, had ſuch an operation upon them, that both I, and others could plainly ſee them creep up and down in the glaſſes, again.

An attempt to produce living creatures in vacuo.

136. Having procured a large number of ſilk-worms eggs, and cauſed three very ſmall receivers to be purpoſely made, that differ'd very little, either in ſize or figure; we convey'd into each of them, together with a ſmall ſtock of mulberry-leaves, ſuch a number of eggs as, we thought, made it morally certain, that, at leaſt, ſome of them ſhould prove prolific. This done, we carefully exhauſted one of the receivers, and ſecured it againſt the return of the air; and the two others we left full of air: but, having left in the one a little hole for the air to get in and out at, we ſtopped the other ſo cloſe, as to hinder all intercourſe between the internal and external air. Things being thus prepared, we expoſed the receivers to a ſouth window, where they might be quiet, and where I either came, or ſent to look on them, from time to time; the ſpring being then ſo far advanced, that, I ſuppos'd, the heat of the ſun would be, of itſelf, ſufficient to hatch them, in no long time. And both I, and others, took notice, that, in the unexhauſted receivers, there were ſeveral eggs hatch'd into little inſects, that perforated their ſhells, and crept out of them; tho', afterwards, for want

of

of change of food, or air, or both, few, or none of them, proved long-lived. But tho' the eggs, in these receivers, began to afford us little animals, in a few days; yet the eggs, in the exhausted receiver, afforded none in so many more, that we left off to expect any from them.

We took several of those little swimming creatures, which in autumn, especially towards the end of it, are turn'd into gnats; and, having put a convenient number of them together, in a fit quantity of rain-water, wherein they had been found and kept, into a small receiver; the air was pump'd out, and the vessel secured against its return, and then set aside in a place, where I could observe, that, on the day after, some of these little animals were yet alive, and swimming up and down, not without minute bubbles adhering to them; but, in a day or two after that, I could not perceive any of them alive: nor did any of them recover, upon the admission of fresh air. Indeed, the weather was so cold and unseasonable, that a number of these little creatures, put up with water in another small receiver, all died within a few days, tho' none of the air was exhausted. And several that I kept in an ordinary glass, which was often unstopp'd, to give them fresh air, perish'd very fast.

The surprizing rarifaction of air without heat.

137. We took a round glass-egg of clear metal, and furnish'd with a shank, some inches in length; this we fill'd with water, and convey'd both it, and a vial, containing water, into a receiver, of a convenient size; and by pumping the air out of it, we made bubbles both in the egg, and the vial, to disclose themselves in great numbers; so that the liquor, in the glass-egg, seem'd to boil, and caused all that was in the shank, to run over. When we thought the water was sufficiently freed from air, we took out the glasses, and fill'd up the shank of the egg with water taken out of the vial, and inverted it into more of the same water, in such manner, that the egg was quite full, shank and all, excepting a small bubble of air, that we, purposely, left, to gain the top of the egg; where we measured it as accurately as we could, and found it to be a tenth, and less than two hundredths of an inch. Then, putting the glasses again into the receiver, the pump was work'd, and the little bubble, after a while, began to expand itself; which, when it had once done, it, at each suck, strangely increas'd, till, at length, it drove all the water out of the round part of the glass. And, lest it might be objected, that 'twas only the subsiding of the water, upon the withdrawing of the outward air, that before kept it up to the top of the glass; we caus'd the pumping to be continu'd, till the expanded air had, several times, driven the water, in the pipe of the egg, a pretty way beneath the level of the external and surrounding water in the other glass. This done, we let in the air, by degrees, with a design to observe what bubble we should find at the top of the egg, when the water should be again driven up into its cavity. But the expanded air had forced over so much water, that there remain'd not enough to fill the globular part of the egg. We, therefore, made the experiment again; and, when we had proceeded thus far, compar'd the above-mention'd diameter of the small bubble, with that of the spherical

part

PNEUMATICS. part of the glaſs, which we took with a pair of callaper-compaſſes; and tho' we found it to be ſomewhat more than twenty times as great, yet we ſuppoſed the two diameters to be only as 1 to 20: and, conſequently, ſince the proportion between ſpheres is triplicate to that of their diameters, the air appear'd to have, by expanding itſelf, poſſeſs'd eight thouſand times the ſpace it took up before. Nor was it overſeen by us, that ſuch glaſſes as we uſed, are ſcarce ever ſpherical. But Dr. *Wallis*, who aſſiſted at the experiment, concluded, with me, that the cavity of the ſhank, from whence the expanded air drove the water, which we did not compute, would make abundant amends for any inaccuracies. After this, for farther ſatisfaction, we took water, laboriouſly freed from air; and, putting it into the ſame glaſs-egg, we inverted it, as before, but left not any bubble in it. This we did, that, in caſe we could make the water ſubſide, the experiment might prevent a ſuſpicion, that ſome air, latent in the water, increaſed the bubble, formerly left in it. Having, then, exhauſted the receiver, at leaſt as much as before, the water, in the egg, did not at all ſubſide: but, at length, with obſtinate pumping, a bubble diſcloſed itſelf, and drove all the water clear out of the round part of the glaſs. And tho', by reaſon of ſome ſmall leaks, that we could not find, or ſtop, we were not able, as before, to make the expanded air depreſs the water in the ſhank, beneath the ſurface of the external water; yet we wanted very little of it: and, then, giving over, we found, that when the water was impell'd up again into the egg, there was, at the top of it, a bubble, whoſe diameter we meaſur'd, and found it to be to the diameter of the globular part of the glaſs, as 1 to 14: ſo that, tho' the little bubble had been a perfect ſphere, it muſt, when expanded, have been 2744 times as big as when unexpanded. But Dr. *Wallis*, obſerving the great thinneſs of the bubble, poſitively affirm'd, that he could not eſtimate it to be, at moſt, any bigger than the third part of a perfect ſphere of that diameter: by which eſtimate, the expanſion of the bubble muſt have reach'd to 8232 times its natural dimenſions. Yet by letting as much water into the receiver as it would admit, we found, that we had not exhauſted all the air.

138. At another time, a ſmall, and almoſt inviſible bubble, expanded itſelf, when the ambient air was pretty well exhauſted to more than ten thouſand times its former extent. We took a ſmall bolt-head, blown at a lamp, which contain'd, in all, about eighty grains of water; and inverting the ſmall neck into a jar of water, it was included in the receiver; and the ambient air being exhauſted, numerous bubbles roſe out of the water, and, expanding, quickly drove all the water out of the bolt-head. Then, re-admitting the outward air, the bolt-head was preſently almoſt fill'd, and all the expanded air ſhrunk into a bubble little bigger than a ſmall pin's head; when, taking the bolt-head out of the water, and inverting it, that the bubble might get out at the neck, we carefully fill'd it up with the water that had been freed from air; and, then, inverting it, as before, into the jar with water, we again included it; and, after ſome exſuctions, found, that there was got out of the water, into

into the neck, a very confpicuous bubble, which, upon admitting the air, fhrunk almoft into an invifible one, and afcended into the head of the glafs. Then, again exhaufting the receiver very well, we found it expand itfelf, fo as to fill the capacity of the bolt-head, and to drive out almoft all the water. And, upon re-admitting the air, it again fhrunk into a bubble, whofe diameter (according to our beft eftimate) was not more than a two and twentieth part of the diameter of the head of the above-mention'd glafs; fo that, to fill the whole cavity of the head only, it expanded itfelf 10648 times: but, becaufe it fill'd, likewife, the greateft part of the neck, we found, by weighing the water which fill'd that part, and the water which fill'd the head, that the capacity of that part of the neck, was almoft a third of the capacity of the head; being as 141 to 481. If, therefore, 481, the capacity of the head, contain'd it 10648 times; 141, the capacity of the neck, muft contain it $3121\frac{167}{481}$ times; fo that, in all, the fmall bubble of air was expanded to above 13769 times its former bulk.

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The diameter of the fmall bubble contracted, was $\frac{1}{27}$ inch.

The diameter of the outfide of the head of the glafs was $\frac{29}{36}$ inch.

The water, that fill'd the head only, weigh'd fixty grains and a half.

The water that fill'd the head, and as much of the neck as the air had before expanded itfelf into, weigh'd feventy-eight grains, and one eighth; whence that part in the neck weigh'd feventeen grains, and five eighths. The bolt-head itfelf weigh'd fifteen grains.

139. We tried this experiment again, and found a fmall bubble, much about one twelfth of an inch in diameter, fill'd not only the ball at the end of the bolt-head, (which was an inch and a half in diameter,) but the whole neck, which contain'd near as much water as the head; and beat down the furface of the water within the pipe, much below that of the water external to it.

Thefe experiments may give rife to inquire, what figures and motions in the particles of the air, can explain fuch a wonderful rarifaction, perhaps, without quite lofing its durable fpring; how the air comes to be rarifiable fo many times more without heat, than hitherto we have found it by heat; and, laftly, what might, reafonably, be conjectur'd about that part of the cavity of an exactly clofed glafs, where, tho' the eye difcovers no vifible fubftance, it appears not, that the common air adequately fills fo much as the ten thoufandth part.

140. It has not, that I know of, been attempted to difcover, whether the air either in the utmoft, or in the intermediate degrees of rarifaction we can bring it to, retains a conftant and durable elafticity; and what other properties it either gains or lofes by confinement*.

The duration of the fpring of expanded air.

To attempt fomething of this kind, I caufed a good bubble of glafs, with a ftem, to be fo blown at the flame of a lamp, that whilft the ball was

* Mr. *Haukshee* has fhewn, by experiment, that the fpring of the air may be fo difturb'd by violent preffure, as to require a confiderable time to recover its natural tone and temper; and that this tone will be as the force employ'd, or its continuance in fuch a violent ftate. *Hauksb. Experim.* p. 110—112. and p. 162—166.

yet

PNEUMATICS. yet exceeding hot, and, consequently, contain'd none but highly rarify'd air, the stem was suddenly seal'd up. This bubble, many months after, I inverted into a bason of water; and, having broken off the seal under the surface of it, the liquor was violently impell'd into the cavity, yet was not able to fill it; a considerable part being defended from the farther ascent of the water, by the spring of the remaining air; which, for all the long stretch it had been put to, had not, that we observ'd, lost any thing of its spring. At another time, leaving a very small proportion of air in the folds of a fine limber bladder, whose neck was very closely tied; by the help of the air-pump, it was so expanded, that, at length, it seem'd to fill the whole bladder, and reduce it to the extent it had, just before 'twas empty'd. And the bladder, by a peculiar contrivance, was so included in another vessel, that, being protected from the outward air, it maintain'd its tumid figure; and in that unwrinkled state it continu'd for near three years.

I, afterwards, contrived an instrument, fit to discover how long air, brought to the greatest expansion I could conveniently reduce it to in my engine, will retain its spring; and by what degrees, or stages, and periods of time, the decrease, if any happen, is made. But I could not, by its means, observe any remarkable diminution in the air's elasticity, tho' it was press'd, and, as it were, clogg'd with a weight, which one would wonder how it could, when 'twas so highly rarify'd, support for one minute. And, in one of them, we found not, in ten weeks time, any considerable variation; for the little shrinking of the air, discoverable by an attentive eye, might be, probably, ascribed to the change of the weather to a far greater degree of coldness.

I, also, contriv'd a little portable instrument, wherein the air being expanded, as one may guess, to five or six hundred times, (perhaps a thousand times) its wonted extent, has not only, for a long time, preserv'd its spring; but, also, tho' very much dilated, without heat, the heat of the hand, apply'd to the outside of the vessel, has a quick, and very manifest operation; and, upon the withdrawing thereof, the air quickly returns to its former dimensions, and temper: so that it may be employ'd as a kind of weather-glass.

141. A cylindrical glass, blown at a lamp, and having a long stem coming out at the unseal'd end, was quite fill'd with water, and inverted into more, placed at the bottom of a large pipe, seal'd at one end, and of three or four feet in length: this external pipe was exhausted, till the air, that disclosed itself in the water of the internal one, had forc'd out the water in the cylindrical glass, as low as the upper part of the stem; at which great expansion of the air, the external pipe, being speedily and securely closed by a certain contrivance, the air, thus rarify'd, was kept sometimes in my own chamber, that was warmer; sometimes in an under-room; and, after it had been kept, from first to last, about eleven weeks, or three months, without any other remarkable variation, than that in the cold room, the water ascended a little at that part of the internal pipe

where

where the lower-end of the cylinder gradually lessen'd *itself* into the slender stem. At length we broke off the closed apex, when the water was but leisurely (because of the slenderness of the orifice made for the air to get into it) impell'd up into the deserted cavity of the cylinder, which it wholly fill'd, except a little bubble, exceeding shallow. We made use of our eyes, at a fit distance, and of compasses, both ordinary and callaper, to obtain these measures. The cylindrical part of the internal pipe was three inches in length; and three fifths of an inch, or less, in diameter, on the outside. The bubble was two tenths in diameter, and about two hundredths in depth. From all which, according to Dr. *Wallis*, who assisted in the experiment, the natural bubble was, to the space it possess'd, when expanded, as 1 to 1350.

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The condensation of the air by cold, and its compression without mechanical engines

142. After the middle of *September*, on a sun-shiny day, about noon, we took a bolt-head, or round vial, furnish'd with a long stem, and plac'd it in a frame purposely provided, so that the stem was perpendicular to the horizon, and the globular part supported by such a vessel, that thorough a hole made in its middle, the shank reach'd downwards, till the orifice of it was a little immers'd beneath the surface of a glass of water, placed at the bottom of the frame. This done, we took a large proportion of beaten ice, and mix'd it with a due quantity of bay-salt, and not only laid it round about the lower part of the ball; but the vessel, contiguous to that part, being purposely made with turn'd-up brims, we heap'd up the frigorific mixture, so as to bury the whole spherical part of the glass in it, and cover the very top of it therewith to a considerable thickness; whereby the air within being exceedingly cooled, the water, in which the shank terminated, was made to ascend fast along the cavity of that shank, till we perceived it would reach no higher: but, after a while, it began to subside again; which nick of time being carefully watch'd, we made a mark at the highest station of the fluid, and then taking out the bolt-head, we fill'd it with water; allowing for that small part of the stem which was immers'd at the beginning of the operation. This water weigh'd nineteen ounces, and six drams; then weighing as much water, as sufficed to fill the shank up to the mark before-mention'd, we found that to be one ounce and three drams; by which number, the former being divided, the quotient is $14\frac{4}{11}$ drams: so that the proportion of the two quantities of water, being as 11 to 158, the space into which the air was condensed by refrigeration, was to the space it possess'd in its former state of laxity, as 147, to 158; and, consequently, the greatest condensation, that such a time of the year, such weather, and so high a refrigeration could bring the air to, made it lose but $\frac{11}{158}$ of its former extent.

But, in the following condensation, or compression of air, tho' cold were, indeed, employ'd, yet that could not contract the air to any thing near such a degree, where the frigorific mixture did not primarily, or immediately, compress the included air; but only so affected the water that was shut up with it in the same vessel, as to make it swell, and, consequently, crowd the aerial particles into less room.

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The experiment was this. We took a new glaſs bolt-head, with a neck not long, and fill'd it ſo far with common water, that, being hermetically ſeal'd, the liquor reach'd within three inches of the top; and making an eſtimate of the ſharp end, left ſo for the conveniency of ſealing up the glaſs, we gueſs'd, it to be about a quarter of an inch in length; then, applying ſnow and ſalt to the lower part of the bolt-head, we readily drove out the water further and further into the neck, till at length it was got up to the baſis of the ſharp conical end, where the glaſs was ſeal'd; and then, juſt as I was looking upon it, the glaſs flew, with a noiſe, about my ears; being broke into many pieces, which argued the compreſſion of the air to have been very great. And Dr. *Wallis*, who was preſent, and meaſur'd it from time to time, deſired me to regiſter the experiment, with his eſtimate; which is, that the air was reduced into the fortieth part of its former dimenſions.

This condenſation of the air is the more ſurprizing, becauſe ſome of the greateſt mathematicians of our age, have not, with wind-guns, and other forcible engines, been able to crowd the air into leſs than the fifteenth part of its uſual extent.

The ſurprizing difference in extenſion of the ſame quantity of air rarified and compreſs'd.

143. Tho' we could not find, that cold, in our climate, would reduce the air into near the twentieth part of its natural ſpace, by condenſation; yet, heat will advance it to near ſeventy times its uſual laxity, by rarifaction.

But, as by engines, and artificial contrivances, the air may be two or three times more compreſs'd, than naturally it is, even in froſty weather; ſo, on the other ſide, it may, by means of art and inſtruments, be much more rarified, and expanded, than has been hitherto found, by the bare application of external heat, even that of an intenſe fire.

We may, alſo, obſerve, how much the utmoſt degree of its rarifaction by heat, mention'd by *Merſennus*, falls ſhort of the degree to which it has been advanced in our pneumatical engine; the proportion betwixt the two being that of about 1 to 70.

But the air, we make our trials with, upon the ſurface of the earth, is not, properly, in a free and indifferent ſtate, with regard to rarifaction, and condenſation; but already highly compreſs'd by the weight of the atmoſphere reſting upon it: whilſt the air to be rarified, has, by virtue of its ſpring, a ſtrong tendency to dilate itſelf.

Here, then, ſeems to be a ſurprizing mutability of the air, as to rarity and denſity; whereby the ſame quantity of air being, ſometimes, compreſs'd, and ſometimes dilated, may change its dimenſions to a degree, that ſeems, almoſt, to tranſcend the power of nature and art; and, by conſequence, might, probably, be rejected as incredible, if it were abruptly, and nakedly propoſed: for, we can ſcarce ſafely put determinate limits to the ſtupendous rarity, which the upper part of the atmoſphere, being, almoſt totally, uncompreſs'd, by incumbent particles of air, may be ſuppoſed to have by nature, unaſſiſted with art.

But

But to compare together the ſmalleſt extent, to which we have reduc'd the air, by condenſation, and the greateſt to which we have advanc'd it by rarifaction; the extent of the ſame quantity, highly rarified, is, to leave out ſome odd hundreds, 13,000 times greater than before; which, being multiplied by 40, the degrees of the air's compreſſure, it will amount to 520,000, for the number of times, by which the air, at one time, may exceed itſelf in bulk at another: a difference truly ſurprizing, tho', doubtleſs, it might be carried vaſtly higher! * PNEUMATICS.

SECT. III.

BEfore we proceed to our other pneumatical experiments, 'tis neceſſary to premiſe, what relates to the improvements of the chief engine, wherewith they were made, and to the other inſtruments employ'd therein.

A deſcription of an engine with a double barrel for exhauſting the air. Fig. 68.

In our engine, with a double barrel, for exhauſting the air, AA, are two pumps made of braſs.

BB, two ſuckers or *Emboli*, hollow within, and open below.

CC, two holes in the upper part of the ſuckers, with valves opening outwards, to let the air eſcape, and hinder it from coming in.

DDDD, iron rods, ſerving to move the *Emboli*, being annex'd to them.

EE, two flat iron ſtirrups, at the top of the rods DD, on which, the operator muſt ſtand to work the engine.

GGG, a cord join'd to the two ſtirrups, and running in the pully H.

* Air, near the earth's ſurface, poſſeſſes about 850 times the ſpace of an equal weight of water; and, therefore, ſays Sir *Iſaac Newton*, "a cylindrical column of "air, 850 feet high, is of the ſame weight "with a column of air a foot in height, "and of the ſame diameter. But a co-"lumn of air, reaching to the top of the "atmoſphere, is equal in weight to a co-"lumn of water, of about 33 feet high; if, "therefore, the lower part of the whole "aerial column of 850 feet high, be de-"ducted, the remaining upper part will "be equal, in weight, to a column of wa-"ter 32 feet high. Now, ſince the air "is compreſs'd, in proportion to the "atmoſphere that reſts upon it; and "ſince gravity is reciprocally as the "ſquare of the diſtance of the place "from the earth's center; I have found," ſays he, "that air, in aſcending from the "ſurface of the earth, to the height of "one ſemi-diameter thereof, is rarer than "with us in a far greater proportion, "than that of all the ſpace below the "orb of *Saturn*, to a ſphere of an inch "diameter. Conſequently, ſuch a ſphere "of our air, of the rarity it has at the "height of a ſemi-diameter of the earth, "would fill all the regions of the planets, "as far as the orbit of *Saturn*, and vaſtly "farther!" *Newton. Princip.* p. 470.

This prodigious degree of rarifaction, ſeems unintelligible to Sir *Iſaac Newton*, by feigning the particles of air to be ſpringy and ramous, or rolled up like hoops; or, by any other means than a repulſive power; which is much greater here than in other bodies, becauſe air is very difficultly generated out of very fix'd bodies; and ſcarce without the aſſiſtance of fermentation; for thoſe particles recede from one another with the greateſt force, and are moſt difficultly brought together, which, upon contact, cohere moſt ſtrongly. *Newton. Optic.* p. 371, 372.

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LL, two valves at the bottom of the pumps, opening inwardly, to admit the air out of the tube MM.

MM, a tube reaching from both pumps to the plate OO, by means of the curvature PPQQ; which ought to be ſo long, that the tube PQQ, may not hinder the pumper from ſtanding conveniently on the ſtirrups EE.

OO, a plate bored in the middle, on which, the receivers, to be evacuated, are placed; as R, for example.

Before this engine can be fit for uſe, it is to be put into a frame of wood, to ſupport it, as *Fig. 69.* and as much water is to be poured thro' the hole Q, in the plate OO, into the pumps, as will fill the cavities of the ſuckers, and a little more: then, a perſon muſt ſtand on the two iron ſtirrups EE, and alternately depreſs and elevate them. By this means, the ſuckers, following the motion of the ſtirrups, in their aſcent, will leave the ſpace in the bottom of the pumps empty; and ſince, as all other paſſage is denied from the air, that alone, which is contain'd in the receiver R, is convey'd into the pumps, by the tube QQPPM, and opens the valve L, which being preſently ſhut, hinders the ſame air from returning: wherefore, the ſucker afterwards deſcending, compreſſeth that air; whence of neceſſity, the valve C, muſt be open'd, and all the air paſs out at it; becauſe, the water in the bottom of the pumps, exactly fills all the ſpace, and alſo regurgitates thro' the valve C.

This double engine is, upon many occaſions, preferable to a ſingle one; ſince it doth, not only, produce a double effect, but, alſo, performs it much more eaſily: for, in thoſe engines, which are furniſh'd but with one tube, whilſt the ſucker is drawn up to evacuate the pump, the whole pillar of the air, incumbent on the ſucker, is to be elevated by force; and again, when the ſucker returns, it is alſo, by force to be reſtrain'd, leſt it ſhould be too ſwiftly impell'd by the air, and ſo break the bottom of the engine; but, in theſe double engines, the operator is, in a manner, wholly free from that toil. For, in the firſt ſuctions, the *Emboli* are eaſily lifted up, becauſe the air, immediately derived from the receiver R, into the pumps, preſſeth the ſuckers downwards, almoſt as ſtrongly, as the external air, incumbent on the oppoſite part; and, when the quantity of the internal air is diminiſh'd, the ſucker, to be depreſs'd, tends downward with the greater force, and ſo, by means of the cord GGG, compaſſing the pully, draws the other *Embolus* upwards, and, at the ſame time, hinders it from deſcending with too great velocity; and, by this means, both ſuckers, at one and the ſame time, will aſſiſt the pumper. And, as the *Emboli* make but a very ſmall reſiſtance, the two pumps of this engine may be ply'd with greater eaſe and expedition, than one pump in ſingle engines; whence, this contrivance is of great uſe in thoſe experiments, which cannot well be made ſlowly.

A mercurial gage. Fig. 70.

The whole gage ABCDE, conſiſts of three glaſs-tubes, all well cemented together, ſo, that a paſſage remains open, from one to the other; the firſt of theſe tubes A B, being open at the extremity A, is of leſs capacity, than the tube BCD, but of greater, than ED. The tube BCD, is is crooked in the middle, and the tube ED, ought to be hermetically ſeal'd, at the extremity E; but the part BCD, muſt be fill'd with mercury.

If this inſtrument, thus prepar'd, be put into a receiver, out of which, the air is to be extracted, the air remaining in the part ED, will, by its ſpring, compreſs the mercury DCB, and force it to aſcend into the part BA, and itſelf will be dilated in the cavity DC. If, then, the following proportions be duly obſerv'd, between the magnitude and length of the tubes, when the air is extracted, the mercury will almoſt reach to the top A, and the air in the other leg, being ſo dilated, that it cannot ſuſtain a greater body of mercury, will remain included in that ſpace.

But, that this inſtrument may exactly ſhew the quantity of the air produced in a receiver; the tubes AB, ED, are to be diſtinguiſh'd by marks into ſeveral parts: and, when the *Torricellian* experiment is made, upon the plate LM, of the pneumatic engine, as *Fig.* 70. a receiver FGE, is to be taken, perforated, at the top F, and the tube HI, is to be tranſmitted thro' the hole, that ſo the receiver may be apply'd to the plate: and, then the hole F, being ſtop'd, and the gage ABCDE, put into the receiver, the air is to be exhauſted: the air, then, being dilated in the receiver, the mercury cannot be ſuſtain'd ſo high in the tube HI, but muſt deſcend by degrees; and, at the ſame time, the air of the tube ED, gradually drives the mercury into the tube AB. Now, when the mercury, in the tube HI, deſcends to the height of twenty-nine inches, and remains at that height, if we mark how high the mercury hath aſcended into the tube AB, we may know, that, as often as the mercury in the gage ſhall reſt at that height, the air, in the ſame receiver, will be able to ſuſtain, only twenty-nine inches of mercury; whence that place in the gage muſt be marked with the figure twenty-nine: and ſo, every inch of the mercury's deſcent in the tube HI, may be marked in our mercurial gage, when the part AB, will ſhew all the degrees of the rarifaction of the air.

But, now, if the air be condens'd in the receiver, above its wonted preſſure, and all ways of its eſcape be ſtop'd, it may immediately be known, by the tube ED; for the mercury will be impell'd into it, by the incumbent air, thro' the open hole ſo much the higher, as the compreſſure of the air in the receiver ſhall be the greater; and how great that is, and what an altitude of the mercury it can ſuſtain, may eaſily be found, by computation, thus.

It has been prov'd, that the ſpace poſſeſs'd by air, is diminiſh'd in the ſame proportion, as the compreſſing force increaſes, and *vice verſâ.*

Let then, the ſpace A, be poſſeſs'd by a certain quantity of air, whilſt the compreſſing force is F: if we increaſe that force by the addition of G, which is equal to it, our ſelf-ſame quantity of air will be reduc'd to half its ſpace, ſo that B, the remaining ſpace, will be half of the total ſpace A, as the former preſſure F, is half of the total preſſure F and G. And, if we further increaſe the preſſure, by the addition of H, ſo that, the firſt preſſure F, is only one fourth of the total preſſure F and G and H, the air can poſſeſs only the ſpace C, which is one fourth of the total ſpace A. Thus, the remaining ſpace will always be in the ſame proportion to the total ſpace, as the firſt preſſure is to the total preſſure. So

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So that the remaining ſpace, being to the total ſpace, as the firſt preſſure is to the total preſſure; three of theſe terms being known, it will be eaſy to find a fourth, by the rule of proportion. For inſtance, in our gage, let the tube ED, be the total ſpace, into which the air is compreſs'd, by the uſual preſſure of the air, which, in *England*, is equivalent to thirty inches of mercury; the firſt preſſure, therefore, will be thirty inches of mercury. Now, if that preſſure be increaſed, and the air reduced into a leſs ſpace, ſuppoſe into the ſpace NE; to find the quantity of this preſſure, I meaſure the remaining ſpace NE, and conſtitute that, ſuppoſe ſix inches, for the firſt term of the proportion; then, the ſecond term, will be the total ſpace DE, ſuppoſe twelve inches; the third term, the height of thirty inches of the mercury, which was the firſt preſſure; and ſo the fourth term, or total preſſure, will be found to be ſixty inches of mercury: whence I conclude, that the preſſure of the air in the receiver can ſuſtain the mercury to the height of ſixty inches; and ſo of the reſt.

From the ſame principle, it will be eaſy to find, what ought to be the proportion, between the ſize of the tubes AB, and ED. For that depends on the length of the legs, which, the higher they are, ſo much the better they reſtrain, and keep in the air, but little dilated, in the ſeal'd part. For inſtance, let the length AB, be ten inches, which height of the mercury is one third of the accuſtom'd preſſure, and it is ſufficient, that the tube HB, be twice as big, as the tube ED; for, after the mercury hath aſcended to the top of the tube AB, the air included in the other leg, expanding itſelf into the ſpace forſaken by the mercury, will poſſeſs three times more than its former ſpace; and ſo one half of the firſt preſſure, which is ten inches, will be ſufficient to curb its ſpring. But, if the legs were ſhorter, the mercury would be expell'd, by the included air, at leaſt in part. And, therefore, the magnitude of the tube AB, ought to have a greater proportion to the magnitude of the tube ED, that the aſcending mercury may afford more ſpace to the air, to be dilated; ſo that the ſpring of the air being weaken'd, the weight of the mercury cannot be overcome. And, thus it would happen, if the height of the gage were to the height of thirty inches, in the ſame proportion with the firſt ſpace of the air, to the total ſpace it would poſſeſs *in vacuo.*

The height of the tube, ſhould rather be too long, than too ſhort; becauſe, if it be too ſhort, the mercury will be expell'd in part, and ſo, not ſhew all the degrees of rarifaction; but, if it be too long, the mercury will, only, not reach to the top, and ſo the gage will, nevertheleſs, ſhew all the variations, tho' they be leſs ſenſible.

But the tube DC, ought to contain a ſufficient quantity of mercury, at the leaſt, to fill the tube AB, before any paſſage be open'd for the air included in the tube ED.

A condenſer: Fig. 72.

In our engine to compreſs the air, AA, is a glaſs-veſſel, whoſe orifice is exquiſitely fitted to the flat plate BB.

BB, is a flat plate of braſs, made to cloſe the veſſel AA exactly.

CC, a

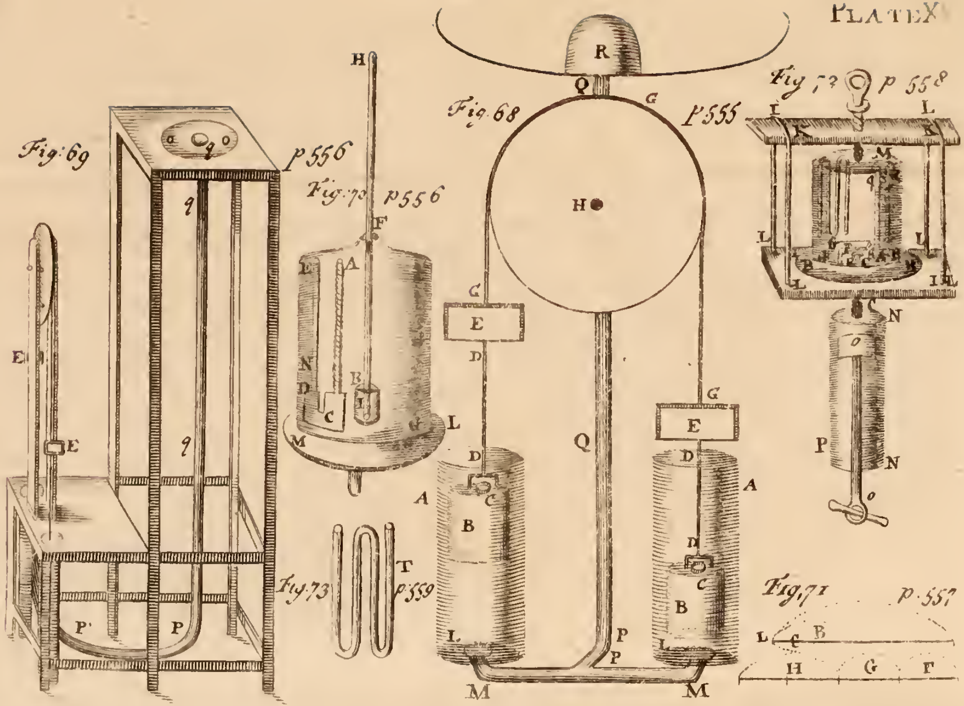


Fig: 69 P 556
Fig. 70 P 556
Fig. 68 P 555
Fig 72 P 558
Fig. 73 P 559
Fig. 71 P 557

CC, a ſmall tube of braſs, paſſing thro' the middle of the plate, and faſtened thereto. PNEUMATICS

E, a little valve, opening inwardly, to ſhut the ſmall tube C.

F, the ſpring depreſſing the valve E.

GGG, the gnomon faſtened to the plate BB, made for reſtraining the ſpring F.

II, a ſquare lath, ſuſtaining the plate BB, and bored thro' in the middle, to tranſmit the little tube C.

LLL, LLL, two iron-wires, which, paſſing thro' the holes in the lath II, and compaſſing the upper part of the iron-plate KK, hinder the plate from being much moved from the lath.

KK, an iron-plate, with a hole in the middle, formed into a female-ſcrew, to receive the male-ſcrew MM.

MM, an iron-ſcrew, ſtraitly to conjoin the receiver AA, with the plate BB; and, leſt the braſs-veſſel ſhould be broken, it is proper to put ſome wood and leather between the ſcrew, and the upper part of the receiver: leather, alſo, is to be put upon the plate BB, both to prevent the breaking of the glaſs, and the more exactly to ſhut the receiver.

NN, a pump faſtened to the tube C, below the plate BB.

OO, the ſucker of the pump NN.

P, a little hole in the lower part of the pump, by which the air enters into it, when the ſucker is brought to the loweſt part thereof.

To compreſs the air by means of this engine, we put the bodies, whereon the experiment is to be made, into the receiver AA; and laying it on the plate BB, firmly bind it thereto, by help of the ſcrew MM. This done, the ſucker or plug OO, is to be drawn, till the external air, by the hole P, can fill all the upper part of the pump; then, if the ſucker be drawn upwards, the air finding no other paſſage, will open the valve E, and enter into the receiver AA; from whence there is no regreſs, becauſe the valve E, is preſently depreſſed by the ſpring F, and ſhuts the hole C. And ſo we may repeat the compreſſion of the air into the veſſel AA, at pleaſure; whilſt the quantity thereof is eaſily known by the mercurial gages.

But I ſo faſhion the pump, that it may be fitted, by a ſcrew, to the tube C; for, thus, when one receiver is full, we may take away the pump, and uſe it to fill others.

Now, becauſe, in theſe engines, mercurial gages ſerve to ſhew the degrees of compreſſion; there is no occaſion for the gages before deſcribed; for thoſe are made with more difficulty, and, beſides, afford but a ſmall ſpace, wherein to note the degrees of compreſſion. It is, therefore, better to bend the glaſs-tube, ſeal'd at one end, in ſeveral places, as in the figure T, that a long tube may be contain'd in a ſhort receiver; ſo that the *Fig.* 73. mercury, being put in thro' the open end, as much as will ſuffice to fill the length of one inch; all the reſt of the ſpace, fill'd with air, will ſerve for marking the degrees of compreſſion, much more ſenſibly than can be done in a ſhorter tube.

Here

Pneumatics. Here we muſt note, that when the mercury tends downwards, in ſuch an inflected gage, the weight thereof forwards the external preſſure; but when it is impell'd upwards, the ſame weight reſiſts it: a difference to be regarded in very accurate experiments.

To mix liquors or powders in compreſs'd air, Fig. 72. In order to make mixtures in compreſſed air, let the receiver be AA, in which we would mix either liquors, or powders.

Let QQ, RR, be two tubes, each of them ſeal'd at one end, and open at the other.

Let RQS, be a veſſel of braſs, to be laid upon the orifice of the tubes, as in the figure.

The liquors to be mixed, muſt be poured into the tubes QQ, RR, each liquor in its own tube; and let the veſſel RQS, being inverted, be laid on the orifices of the tubes; and, in that poſture, let all be cover'd with the receiver AA; let the ſcrew be driven, and the air intruded after the manner juſt deſcribed: and when the gage TT, ſhews, that the compreſſure is arrived at the degree intended, the engine is to be inverted, and ſo the liquors will flow down from the tubes into the veſſel RQS, and be mix'd there. If more liquors, or powders, are to be mix'd, the number of the tubes is to be increas'd accordingly.

To make and remove artificial air from one receiver into another. To tranſmit air out of one receiver into another, we uſe the following contrivance.

AA, is a flat plate of metal, with a hole in the middle.

BB, is the ſtop-cock, faſtened to the hole in the middle of the plate AA, one of whoſe ends is form'd into a male-ſcrew.

Fig. 74. & 75. DC, is a copper-funnel, open below, with a broad orifice, (that it might be eaſily ſet upon the pneumatic engine, and there ſtand firm;) and the upper part of the orifice D, is faſhion'd into a female-ſcrew, to receive the male-ſcrew of the ſtop-cock BB.

EE, is a ſmall tube, open at both ends, which are cut into a female-ſcrew, to receive the male-ſcrew of the ſtop-cock BB.

FF, is the receiver laid on the plate AA, and exquiſitely fitted thereto.

Now, to make factitious air, we muſt put the matter which is to produce the air, into the receiver FF; and placing that on the plate AA, by means of the ſcrew, we ſtrongly faſten it thereto, as in our engine for compreſſing the air; the ſtop-cock BB, we inſert into the female-ſcrew D: then the orifice C, and with it the receiver, is to be placed upon the pneumatic-engine, and the ſtop-cock B, being open'd, the air is to be extracted. When the receiver FF, is emptied of air, the ſtop-cock B, is to be ſhut, that all paſſage to external air into the receiver may be denied; and the ſtop-cock, being taken out from the female-ſcrew D, the receiver is preſently to be immers'd in water; ſo that, at leaſt the plate AA, with the ſtop-cock, may be cover'd therewith: thus no air from without can find entrance; and the air, produced out of the matter in the receiver, will be preſerv'd unmix'd; whilſt the degrees of its rarifaction, or compreſſion, are known, as thoſe of common air.

But

Fig. 74 p. 560.

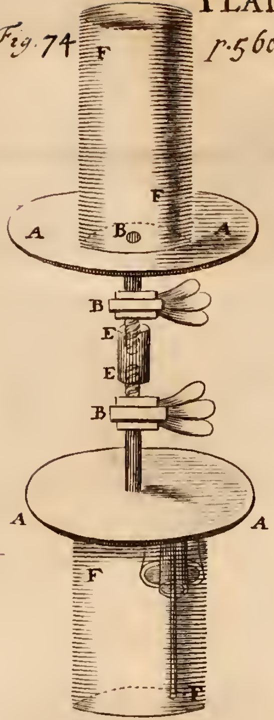


Fig. 75. p. 560.

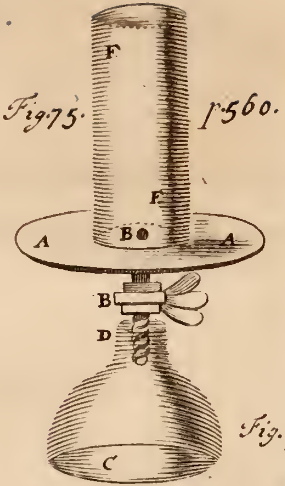
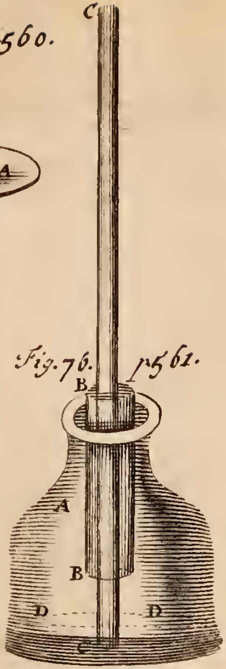


Fig. 76. p. 561.



But if we would tranſmit that air into another receiver; another receiver FF, with another plate AA, and a ſtop-cock BB, is to be procured, and evacuated: then, by means of the ſmall tube EE, we join the ſtop-cocks BB of, both receivers, when all ſuſpected places are to be ſtopp'd with cement, that no external air may find entrance. Then, the ſtop-cocks being open'd, the air, produced in the former receiver, flows into the latter; and the ſtop-cocks being again ſhut, and pluck'd out from the tube EE, the receivers may be kept a-part: when if there be any matter included in the latter receiver, we may eaſily view what influence the factitious air hath upon it. *Fig. 74.*

But, becauſe the mercurial gages, lately deſcrib'd, are ſpoil'd, if they be inverted, and the crooked gages preſently expel their mercury, if the air be rarify'd in their receivers; and, ſince the operation, here deſcrib'd, cannot be perfected, but both receivers muſt be inverted, and both, likewiſe, emptied of air; gages of another ſort are to be made, after the manner following.

AA, is a glaſs vial, fill'd with mercury to the ſuperficies DD. *Fig. 76.*

BB, is a glaſs tube, very well cemented, in the orifice of the vial.

CC, is another tube, tranſmitted thro' the tube BB, and reaching to the bottom of the glaſs. This tube muſt be ſeal'd above, and open below; neither muſt it ſo exactly fill the tube BB, but that paſſage may be given to the external air, within the glaſs AA.

If this inſtrument be put into a receiver, from which, the air muſt be, afterwards, extracted, both tubes will be exhauſted of air; and, when you invert the receiver, to take in new air, as in *Fig.* 74. the mercury will flow down to the orifices of the vial, and be there kept, below the orifice of the tube BB; when the new air entring, will eaſily fill both tubes, and the vial: then, the receiver being erected, the mercury will again reſt, in the bottom of the vial, and the orifice of the tube CC, will be plung'd in it. And, if any air be produc'd, out of the bodies included in the ſame receiver, the mercury will aſcend into the tube CC, and there, reducing the air into a narrower ſpace, ſhew the degrees of compreſſion.

The inſtrument wherewith we filtred air thro' water, was thus contrived. *To filter air thro' water.*

AA, is a glaſs receiver, whoſe orifice, laid upon the plate BB, agrees exquiſitely therewith. *Fig. 77.*

BB, is a plain plate with a hole in the middle, to tranſmit the tubes CC, DD.

CC, DD, are two tubes cementedto the plate BB; one of which is no higher than the plate, but the other reacheth almoſt to the top of the receiver.

EEEE, is a ſtop-cock, to whoſe holes the extremities of the tubes CC, DD, are faſtned.

FF, is the key of the ſtop-cock unperforated, wherein is only one chink GG.

Pneumatics. HH, is the receiver, compaſſing the end of the ſtop-cock, and faſtned to it, preventing the entrance of the outward air, and communicating with the pump II.

LL, is a glaſs veſſel.

M, is a hole in the top of the receiver, whoſe ſtopple is faſtned with a ſcrew.

Fig. 78. The next figure exhibits a ſtop-cock, cut tranſverſly, that the two tubes CC, DD, may be the better diſtinguiſhed, and their inſertion into the ſtop-cock be perceiv'd.

This inſtrument is thus to be uſed: we put the thing about which the experiment is to be made, into the veſſel; and the receiver AA, being laid on the plate BB, we pour water into the hole M, till the receiver be about half full, and the veſſel LL, with the matter contain'd therein, ſwims on the top thereof; then we ſtop the hole exactly, and faſten it with a ſcrew. The key is afterwards to be ſet ſo, that the chink GG, may communicate with the tube CC; then the plug being brought to the loweſt part of the pump, the air of the receiver AA, entring through the upper orifice of the tube CC, will flow down thro' the chink GG, into the receiver HH, and into the pump. Then the key being inverted, ſo that the chink GG, may anſwer to the inſertion of the tube DD, the plug is to be impelled upward; when the air will be expelled from thence, and, finding no other paſſage, be driven through the chink GG, into the tube DD; and from thence it will emerge to the upper part, through the water ſtagnant in the receiver. And by repeating this proceſs, we ſtrain the air thro' the water, as often as we pleaſe; and thence know whether it acquires any new qualities, in reſpect of the body included with it.

How to condenſe and rarify the ſame parcel of air. Fig. 79. Let the receiver AA be placed upon the plate BB, and ſcrewed on to it.

CC, is the ſtop-cock, faſtned to the hole in the midſt of the plate BB.

DD, is a pump joined to the ſtop-cock C, with a ſcrew.

E, is a veſſel, ſo large, that it may fluctuate in the receiver AA, without danger of being over-turn'd.

Let ſome animal be put into the veſſel E, and let the receiver AA, be put upon it, and ſcrewed to it, as the figure ſhews. Then let the pump be fill'd with water, and, by a ſcrew, be fitted to the ſtop-cock; the ſtop-cock, being then open'd, let the plug C, be forced upwards, and the water aſcending through the ſtop-cock will, in part, fill the receiver AA, and reduce the air, contained therein, into a narrower ſpace, without any addition of new air: if, then, you draw the plug downwards, the ſame numerical air will be again rarified. Thus you may both condenſe and rarify the ſame air as often as you pleaſe; and, by this means, you may find, whether the condenſation of the air contributes to prolong the life or health of animals.

A wind-gun. In our wind-gun AA, is a hollow copper globe.

Fig. 80. BB, a tube, faſtned to the globe.

F, a valve opening inwardly, and ſhutting the tube BB.

G, the

Fig. 80. p:562

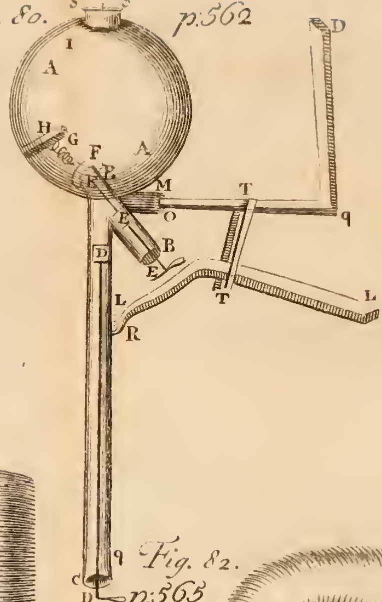


Fig. 81. p:564

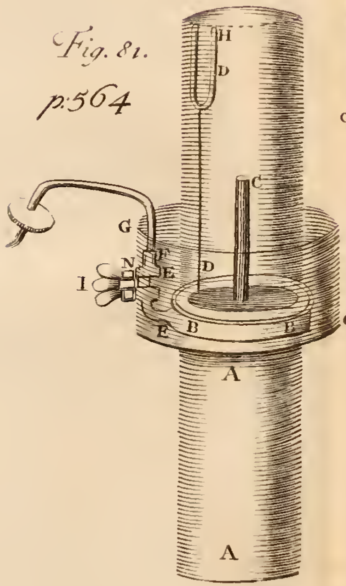


Fig. 82. p:565

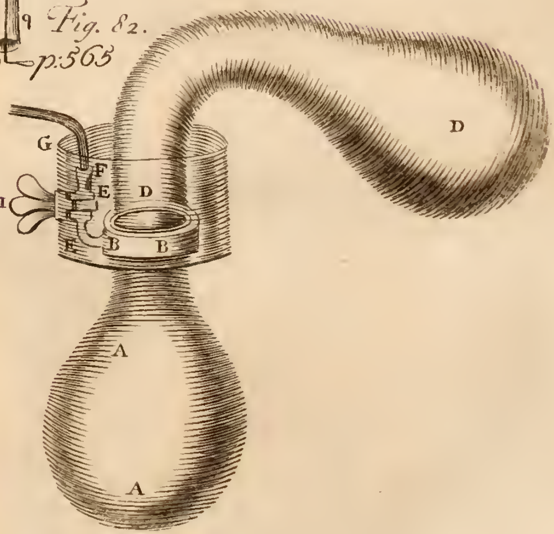


Fig. 77. p. 561.

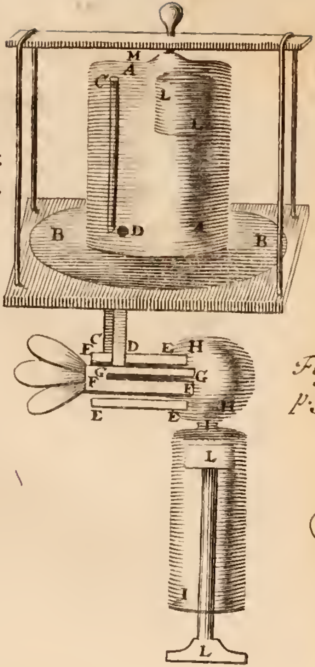


Fig. 78. p. 562.

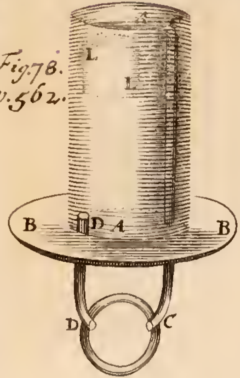
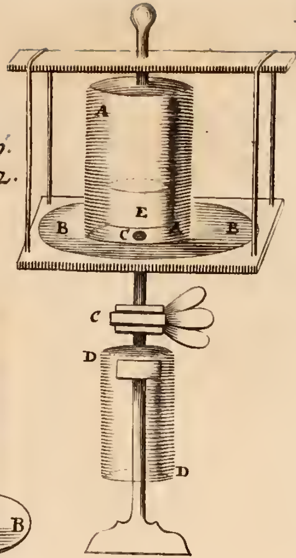


Fig. 79. p. 562.



G, the spring depressing the valve.

H, a gnomon affixed to the globe AA, and making fast the spring G.

CC, a tube of iron, fastned to the tube BB, and the globe AA.

DD, a plug exactly fitted to the tube.

EEE, another plug fitted also to the tube BB, with an iron wire, reaching almost to the valve F.

R, the protuberance of the tube CC, somewhat hollowed above, to receive the end of the iron LL.

LL, a crooked iron, moveable about the extremity in R, so that it serves as a lever to lift up the plug EEE.

OPO, a crooked iron, fastned in M, that the thumb resting in the angle P, the rest of the fingers may attract the lever L, and so force the plug EEE, upwards. But the curvature is design'd, that the one end O, might be applied to the shoulder, in aiming at a mark.

TT a rectangular piece of iron, compassing the lever LL and the iron OPO, to keep the lever in its posture; for, otherwise, the plug EEE, would be thrust far out, whilst the air is intruded into the globe AA.

II, an elliptic hole, in the upper part of the globe, very well shut with a valve, opening inwardly, to give liberty of inspection, and of amending what is amiss; for the valve may be drawn through the hole, by reason of its elliptic figure.

SS, a metalline plate transversly placed above the hole II, and perforated to transmit the screw V, by help whereof the valve, shutting the hole II, is sustained, and applied closely to it.

Q, a hole in the lower part of the tube CC, by which the air enters into the tube, whilst the plug D, is brought to the lowest part thereof.

The air is forced into this engine, by setting the foot upon the crooked end of the plug DD, that it may not be removed from the ground, and lifting the engine upward, till the upper part of the plug comes below the hole Q; and then the air entring through the hole, wholly fills the tube CC.

Then, by forcibly depressing the engine, the air, contained in the tube CC, opens the valve F, and is thrust into the globe AA; whence it cannot return, because the valve presently stops the passage: and thus, by repeated strokes, we may condense the air in the globe, till the force of its spring cannot be overcome by our strength.

If we would discharge the air so condensed, the plug DD, is wholly to be drawn out, and a bullet to be put into the bottom of the tube CC: then, by means of the lever LLL, the plug EEE, is to be impell'd upward, as we said before; when, the extremity of the iron-wire, opens the valve B, and the air breaking out therefrom, expels the bullet through the tube CC, with great violence.

But before the plug DD, is again put into the tube CC, for the compression of the air, about half an ounce of water is to be pour'd into the tube. For, by this means, no air at all can escape out by the plug; and, moreover, that water exactly filling the upper part of the tube CC, the

PNEUMATICS. whole compressed air will be intruded within the cavity AA; and so the condensation be perfected much sooner, than if, at every turn, part of the compress'd air remain'd below the valve F.

This engine has several advantages above the common wind-guns. 1. Because one valve serves, both for the letting in, and discharging the air; whence it is less subject to be spoiled, or impaired, than if two valves were used for that purpose. 2. If any disorder happen in other guns, they remain useless; but here, by the elliptic hole, we may take out the spring and the valve, and so mend whatever is amiss. 3. In other guns, the valves being cover'd with leather, are put in, before the engine is closed on every side; and therefore silver-solder could not be used in joining the parts, but only lead-solder, by which, the air, being much compressed, could, by no means, be restrained; but here all things are well cemented with silver-solder, without danger of burning; since the valve, cover'd with leather, is put in afterwards thro' the elliptic hole II. 4. But this engine is chiefly to be preferred before others, because, here we can put several bodies into the receiver, through the elliptic hole, and so make many experiments in highly compressed air.

An engine wherewith to distil in vacuo. *Fig.* 81.

We, also, contrived an engine, which should distil *in vacuo*, thus.

AA, is a brass vessel, shut below, and open above.

BB, a diaphragm of tin, whose edges are so polish'd on both sides, that they exquisitely agree and suit with the edges of the vessels AA, DD, which are also polished, and so keep out the external air.

CC, a tube fasten'd to a hole in the middle of the diaphragm BB.

DD, a brass vessel, whose aperture is applied to the diaphragm BB.

EE, a stop-cock fastned to the hole of the diaphragm BB.

FF, a tube reaching from the stop-cock EE, to the hole made for suction in the pneumatic engine.

GG, a metalline vessel, including the junctures of the vessels with the diaphragm, and also the stop-cock, that being filled with water, it may keep all safe from the external air. This is to be solder'd to the vessel AA.

To use this engine, we take away the diaphragm BB, and put the ingredients into the vessel AA, and set it in a convenient place, till it is to be evacuated; then putting on the diaphragm BB, and the vessel DD, we apply all to the pneumatic engine, and by means of the tube FF, the air is pumped out of the vessels, the vessel GG being yet first filled with water. Then the stop-cock is shut; and taking away the tube FF, we may place the evacuated engine on the fire, when the vapours, ascending through the tube CC, are condensed in the upper vessel, and so we have a liquor distilled *in vacuo*. The quantity of the generated air, is known by the mercurial gage H; but that must be kept in the top of the receiver, lest the mercury exhale, by reason of the heat.

Round pieces of paper, perforated in the middle, are to be laid over the orifices of the vessels AA, DD, that they may be the better joined with the diaphragm; the commissures of the tube FF, with the stop-cock, and pneumatic engine; are to be fortified with cement; and the stop-cock EE, is so to be disposed with the vessel GG, that part of the key may be prominent, without the vessel, thro' the hole, to be conveniently turned; nevertheless, the stop-cock, with the diaphragm, may be taken out of the vessel GG, whilst the vessel AA, is to be filled with the designed matter. And that is very easily done, because the key consists of two parts, one of which M, is turned in the stop-cock itself, by means of a certain chink, which receives the small protuberance of the other part OO, that exactly fills the small pipe NN, fastned to the vessel GG; and being prominent outwardly, may easily be turned in it, and communicate its motions to the other part M: but it is drawn outward, whilst the diaphragm BB, is to be taken out of the vessel GG.

Pneumaticks.

Fig. 82. shews another instrument, differing from the former, in that it, almost, wholly consists of glass, and affords a longer passage for the vapours.

BB, is not a diaphragm, but a small tube, polished at both ends, that it may exquisitely suit with the orifices of the vessels A, and D.

AA, DD, are two glass vessels, whose orifices are applied to the tube BB; whence the vapours are easily transmitted from the one to the other.

EE, FF, GG, and I, have the same use as in the former figure; and the whole instrument is to be evacuated after the same manner, and placed upon the fire; except that here the vessel AA, as being made of glass, must not be put on an open fire, but set *in balneo Mariæ*, or on sand; and the vapours will be condensed in the vessel DD.

(1.) *July* 11. 1676. I included a little piece of bread, very moist, and a little kneaded, with a mercurial gage, *in vacuo*.

Several ways to forward the production of air; and first, air produced from bread.

July 12. In six hours time, no air was produced yesterday; but this night, a little broke into the receiver, and sustain'd three inches of mercury; for I had neglected to fortify the cover with turpentine.

Towards the evening, I found the mercury higher by about an inch; and am very certain, that nothing had entred from without.

July 13. This night, also, the mercury ascended higher; but my gage was not exact enough to discover how many degrees.

July 26. The bread disjoined its receiver from the cover, by the force the air produced, and the smell of it was acid.

Hence it follows, that water is a fit menstruum to draw air out of bread.

(2.) *July* 11. I tried to extract air from bread, by the help of a burning-glass, wherewith I burnt bread *in vacuo*, and found it generate much air, which, ever and anon, broke out, as by fulmination; whence it seems probable, that air is contained in bread, but so closely compacted therein, that no easy operation can give it vent; but that if any thing could dissolve and loose that knot, it may then produce great effects.

(3.) *Sept.* 22. I took eight ounces of dry'd grapes, and, with seven ounces of water, included them in a receiver, able to hold twenty-two ounces of water. The grapes were bruised.

From grapes.

Sept. 23. The receiver lay buried under the water all this night, yet the mercury ascended two whole inches.

Sept.

PNEUMATICS. *Sept.* 30. In ſeven days time the mercury roſe to the height of thirteen inches.

Octob. 5. In five days more, the mercury aſcended twelve inches, and was now twenty-five inches high.

Octob. 18. The mercury continued not to aſcend with the ſame ſwiftneſs, and the air began to paſs out of the receiver; but not before this day; yet theſe grapes produced much more air, than thoſe which I included without water.

From raiſins. (4.) *July* 12. I included ten ounces of raiſins of the ſun, bruiſed *in vacuo*, with a ſufficient quantity of water, to promote fermentation.

July 14. In two days they had produced ten inches of air.

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

July 16. In the morning, I found the receiver ſever'd from its cover; and the air breaking out thro' the water, in which it was plunged, I included the ſame raiſins again *in vacuo.*

July 18. This day, in the morning, I found the air again breaking out.

July 19. I ſhut up the ſame raiſins in the ſame empty receiver.

July 21. This day I found the receiver full, and the air breaking out of it.

I again ſhut up the ſame raiſins in the ſame exhauſted receiver.

July 23. Yeſterday, about noon, I found the whole receiver almoſt full of air; and this day, in the morning, perceiv'd it to paſs out very often.

It appears, then, that grapes without water, can generate but little air; whence it is manifeſt, that water is a fit medium to draw air out of them: 'Tis alſo evident that the production of air is not begun preſently upon the affuſion of water, but proceeds with greater ſwiftneſs, after the parts of the water, in five or ſix days time, have more deeply ſunk into, and pervaded the grapes.

From plumbs. (5.) *Aug.* 13. 1677. I included pears in two exhauſted receivers, and plumbs in another.

Aug. 16. In three days time, all my receivers were filled with air, newly generated; and one of them, which included the pears, becauſe I had left it expoſed to the ſun, was, in the ſpace of 24 hours, ſeparated from its cover: whence we may conjecture, that the production of air is very much promoted by the heat of the ſun.

From grapes. (6.) *Octob.* 16. 1677. I took two ounces of grapes bruiſed, and ſecured them from the air, in an exhauſted receiver, capable of containing twenty ounces of water.

Octob. 17. The mercury roſe higher about one half-inch.

Octob. 18. Theſe laſt twenty-four hours, the mercury ran up about another half-inch.

Octob. 20. The height of the mercury was two inches.

On the twenty-ſecond, it was almoſt four. And, on the twenty-ſeventh it was almoſt ſix inches.

Jan.

Jan. 2. 1678. The mercury, yet, aſcended not to the height of ten inches. Pneumatics.

Octob. 16. 1677. I put three ounces of bruis'd grapes, with half an ounce of ſpirit of wine, into a receiver, able to hold thirty ounces of water; and then I exhauſted the air.

Octob. 17. The mercury aſcended but a very little.

Octob. 18. The mercury came not up to the height of one quarter of an inch.

Octob. 20. The mercurial gage was out of order.

Jan. 2. 1678. I, this, day found my receiver fill'd with air; and, alſo, when part of the liquor was pour'd out, ſome bubbles were form'd in the turpentine, about the orifice, and broke outwardly.

From this experiment, made in two receivers together, it ſeems to follow, that ſpirit of wine much advances the production of air *in vacuo*; tho', in common air, it wholly hinders it.

(7.) *July* 19. 1678. I put muſt, expreſſed from grapes bruis'd, and kept for ten months in a veſſel, ſtopp'd with a ſcrew, into the ſame receiver, being alſo ſtopp'd with a ſcrew. *From muſt.*

July 21. The mercury had not aſcended at all.

23*d.* The height of it was three.

24*th.* The height was five.

25*th.* In the morning it was an hundred and four.

Towards the evening, the height was an hundred and thirty-ſeven; and the muſt got out.

26*th.* The muſt was almoſt all got out of the receiver; and altho' the air now poſſeſs'd double the ſpace it did yeſterday, yet it kept up the mercury to the ſame height.

27*th.* About half of the remaining muſt broke out this night, becauſe I had omitted to ſet the ſcrew, leſt the receiver ſhould be broken.

From this experiment it follows, that grapes, kept for ſo long a time, rather acquire, than loſe a fermentative virtue.

(8.) *Jan.* 30. I put two quantities of apples, boil'd the day before, into two receivers, ſtopp'd with ſcrews; with one of them I mix'd a third part of ſugar, the other had no ſugar at all. Theſe receivers were quite full. *From apples.*

Jan. 31. I included raw apples, bruis'd, in three receivers; in one of them I mix'd a third part of ſugar; the ſecond was without ſugar, and ſo was the third; but it differ'd herein from the ſecond, that it was ſix times as big: for, by this means, we may know, whether the capacity of the veſſel, or the mixing of ſugar, or the crudity of the fruit, can promote, or retard the production of air.

Febr. 10. In that receiver, only, which contain'd the raw apples, with ſugar, ſome air was produc'd.

Febr. 14. The raw apples, with ſugar, had impell'd the mercury up to thirty inches; thoſe that were boiled with ſugar, to two only; in the other receivers no air was produced.

Febr. 18. In the receiver, containing the raw apples, with ſugar, the mercury came to the height of fifty-ſix inches; in that containing the boil'd apples with ſugar, the height was three: in the other receivers, there was, alſo, ſome air produced, except in that wherein the boiled apples, without ſugar, were put. I open'd that receiver, in which the apples had produced ſo great a quantity of air; yet the apples ſeem'd hardly to be fermented, but had a moſt pleaſant taſte.

Febr. 21. The boil'd apples, without ſugar, had loſt ſome of their juice; and, opening the receiver, I found the cover broke, and yet the apples were not at all rotten.

March 1. In the great receiver, containing the raw apples, the mercury was twenty-five inches high; in the little one, only ſeven: but in that where were the apples boil'd with ſugar, the mercury had aſcended to nine inches.

March 8. In the great receiver, the height of the mercury was twenty-nine; in the leſſer, twenty-two and a half; and where the boil'd apples, with ſugar, were, the altitude was nine inches.

March 17. The juice got out of the great receiver; in the little one, the height was ſixty-ſeven; where were the apples boil'd with ſugar, it was fifteen inches.

From this experiment it ſeems, that ſugar, the crudity of the fruit, and the largeneſs of the receiver, all contribute to the production of air.

Several ways to hinder the production of air; for inſtance, in paſte.

(9.) *December* 21. 1678. I made paſte of wheat-flower, without leaven, and put it into an exhauſted receiver; then I put the receiver in an apartment, with a fire, which there kept a greater heat than is uſual in the middle of ſummer; yet the paſte produced no air in ten hour's ſpace: whence it ſeems to follow, that if dough hath once ſuffer'd too much cold, it can ſcarce recover its faculty of fermenting; for, ſome years ago, when I made dough without leaven, in the ſummer-time, it ſoon produced very much air *in vacuo.*

(10.) *May* 23. I included three ounces of dough, kneaded with leaven, in a receiver, capable of holding fifty ounces of water; I, alſo, pour'd upon it ſome quantity of ſpirit of wine, to try whether fermentation would be hinder'd by that means.

May 24. The mercury was three inches high.
26. Little change.
27. No change.
May 29. No change.
June 2. It ſeem'd to have aſcended a little higher.
14. No change.

December 14. No more air being produced from the dough, I took it out of the receiver, and found the ſmell of it not grateful, but inclining to acid: I put it into an empty receiver, and there it ſwell'd to double its uſual ſpace, and made a little ebullition.

May 23. I included three ounces of dough, kneaded with leaven, in a receiver, able to hold fifty ounces of water; but here I mixed no ſpirit of wine.

May

May 24. The mercury was nineteen inches and a half high.

May 26. 'Twas 38 inches high.

27. There was no change.

December 14. The mercury continued at the ſame height; and, this day, opening the receiver, I found the dough had a very acid ſmell.

From this experiment it ſeems to follow, that ſpirit of wine, even in dough kneaded with leaven, hinders the production of air.

(11.) *Auguſt* 29. I included pears, with a mercurial gage, in a receiver full of water, and then intruded air into it, till the mercury reſted twenty-ſix inches higher than uſual; within a quarter of an hour, one of the pears was broken, and, afterwards, almoſt all of it reduced to pulp. *In pears.*

Aug. 30. In twenty-four hours ſpace, the pears ſeem'd to have afforded no air; but, on the contrary, the mercury in the gage was depreſſed an inch and a half.

Aug. 31. I found no change in the height of the mercury.

Sept. 1. The pears began to produce air, and the mercury was almoſt twenty-ſeven inches high.

Sept. 2. In twenty-four hours time, the mercury aſcended more than eight inches; and now 'twas thirty-five inches high.

Sept. 3. The height of the mercury was increaſed ſeventeen inches; ſo that now it was about fifty-two inches high.

Sept. 4. Within twenty-four hours, the mercury roſe ſeven inches higher, and then reſted at fifty-nine.

Sept. 5. It was ſixty-four inches high; and a pear, being broken, was become black.

Sept. 6. Three inches, and more, being added to the height of the mercury, it came now to ſixty-ſeven inches, and one fourth, beyond what it was accuſtom'd.

Sept. 7. It deſcended three inches, and reſted again at ſixty-four.

Sept. 8. The mercury was depreſſed to fifty-eight inches; and ſome of the water having broke out, I ſet the receiver with a ſcrew.

Sept. 9. The mercury aſcended full three inches, and was now ſuſpended above ſixty-ſeven.

Sept. 10. In twenty-four hours it mounted one and a half, and ſtopp'd almoſt at ſixty-nine.

Sept. 11. Now it began to deſcend again, and ſtood no higher than ſixty-ſeven inches; yet, I am certain, nothing had eſcaped out of the receiver; but it was a ſharp cold night.

Sept. 12. No change happen'd.

Sept. 13. The height of the mercury again decreaſed, and it was not above ſixty-four inches. The cold increaſed.

Sept. 14. In twenty-four hours, it became higher by ſix inches, reaching to ſeventy.

Sept. 16. It was about ſixty-nine inches high.

19. It remained the ſame.

Sept. 20. It again aſcended to 71.

23. The mercury was again depreſſed to ſixty-nine.

Octob. 1. It came to the height of ſeventy-five inches.

PNEUMATICS. *Octob.* 3. Yesterday I found no change at all in the mercury; but to-day it rested at seventy; and the cold was very severe.

Octob. 5. Yesterday the mercury remain'd in the same place; but this day it reach'd to seventy-five. It was a rainy day.

Octob. 7. It continu'd rainy; and the mercury continu'd at the same height.

Octob. 10. Hitherto the mercury was not changed; but this day I found it had descended to sixty-nine inches; tho' the rain ceas'd not.

Octob. 12. Yesterday the mercury stood still; but this day it was depress'd to sixty-five inches; and the cold weather return'd.

Octob. 13. The height of the mercury was sixty-four.

14. The height was sixty-nine.
15. The height was seventy-four.

24. The height was sixty-eight. It was a cold season.

Nov. 2. The height was sixty-four. The cold increas'd.

Nov. 5. The height was eighty and a half. The cold abated.

22. The height was sixty-five. It was a hard frost.

27. The height was sixty-eight. It thaw'd.

Decemb. 6. The height was sixty-one. It was a very severe frost.

From this experiment we may learn, that fruits, in a great compressure of the air, cannot produce so great a quantity of air; for when I made an estimate of the quantity of the fruits, and of the small space to be fill'd with air; I found that quantity of air was not one eighth part of what had been produced in a large empty receiver: tho' the coldness of the water might, also, hinder the generation thereof, as the following experiment will shew.

'Tis farther manifest, that the air is produced by starts, and, as it were, by reciprocations; as all bodies, in motion, by the force of their gravity, or of their spring, are carried beyond their point of rest, and so make many vibrations, or returnings. And tho' cold and heat are not the sole causes of such reciprocations, yet they seem to contribute much thereto.

In paste again. (12.) *Feb.* 22. 1677. I included ten ounces of paste in a receiver, that would hold twenty-two ounces of water; and, afterwards, I thrust as much air into it, as sufficed to sustain seventy-three inches of mercury, besides the wonted pressure. In two hours space I perceived no sensible change.

Febr. 23. In eighteen hours time, the mercury rose seven inches only, its height being eighty.

In six hours it ascended three; and its height was eighty-three.

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

And water seem'd to be express'd out of the mass.

	Its height was
March 2.	120
3.	121

4 & 5. It remain'd at 121.

March

March 8. During these two or three last days, the frost breaking, the mercury ran up four inches; and, the height thereof was one hundred and twenty-five. PNEUMATICS.

March 10. Yesterday, the mercury remain'd at the same height; but, this day, mounting six inches, it rested at one hundred and thirty-one.

March 21. The cold continuing long, no air was produc'd; but, in the three last days the mercury ascended seven inches, and remain'd at one hundred and thirty-eight.

April 4. Yesterday, the mercury had ascended, but I deferr'd measuring the quantity, till to-day; in the night, one of the iron wires, that straitned the receiver, was broken, and the receiver thrown to the distance of four or five foot.

Hence we may conjecture, that the compression of the air, very much hinder'd the production thereof; for, that is usually perform'd, in paste, in two or three days time. Cold, also, much hinders its production.

(13.) *March* 1. 1677. I included two ounces of bruised raisins of the sun, with six ounces of vinegar, in a receiver; upon which, numerous bubbles broke out. *In raisins and vinegar.*

March 2. The mercury, in twenty-four hours space, ascended not to the height of half an inch; yet, some bubbles still appear'd.

March 25. The vinegar always appear'd interspers'd amongst some of the bubbles; yet, the mercury ascended not to the height of one inch.

Hence it appears, that vinegar hinders the production of air and fermentation; for, raisins, of themselves, afford much air.

(14.) *April* 7. I included ten ounces of paste, in a receiver capable of holding twenty-two ounces of water; afterwards, I intruded as much air into it, as sufficed, to sustain one hundred and twenty-eight inches of mercury, besides its accustom'd height. *In paste.*

In six hours time, the mercury rose four inches, and rested at one hundred and thirty-two.

April 8. In sixteen hours the mercury ran up nine inches higher, and staid at one hundred and forty-one.

Nine hours after, the mercury manifested no change.

April 9. In the morning, I perceiv'd some air had broke forth, and the mercury was depress'd to one hundred and thirty inches; therefore, I screw'd the receiver tighter, and thrust in eleven inches of new air: the height was one hundred and forty-one.

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

April 27. For eight whole days the mercury kept its station; but, on the two last, it ascended seven inches, and continu'd at one hundred and ninety-eight, above its wonted height.

April 30. The mercury persisting at the same height, I eased the screw, so that some air might break out; and, when the mercury had so far

PNEUMATICS.

far descended, as to exceed its accustom'd height, only fifty inches, I presently set the screw; to see, whether, that remission of the spring of the air, would afford any place for new air to be generated; and, in two or three minutes time, I found the mercury to have ascended, sensibly higher.

Three hours after, the mercury was found twelve inches higher; for it came to sixty-two.

In five hours, it ascended one inch and a half.

May 1. In fifteen hours, the mercury rose, only, one inch.

May 3. Yesterday, it appear'd at the same height, but this day,' twas higher, by one and an half, and remain'd at sixty-six.

May 4. The mercury was not chang'd, and, therefore, I suffer'd all the air to escape; but, I could not quickly set the screw: whence it is probable, that very much air, which, at that time, was produc'd, got out of the receiver; nevertheless, after the receiver was again well stopp'd, I perceiv'd, that two inches of air, and more, had been produc'd in five or six minutes time.

May 7. The mercury, in three days, again amounted two inches.

May 8. The mercury was higher by half an inch.

May 11. During these two last days, the mercury, again, ran up half an inch. I set this mass, almost unfit, as it seem'd, to produce air *in vacuo*; when, in five minutes space, the mercury ascended to the height of one ch.

May 21. It ascended not quite three inches.

May 30. The mercury rested at the height of four inches and a half.

By this experiment, it appears, that all the air producible from paste, may, after a sort, be generated in a great compression; yet, it is somewhat restrain'd thereby; for, in a less compression, it will soon break out.

Hence, also, we see, that air is producible by starts; and, that it rises more slowly in compress'd than in free air: for, such a production in the latter, is usually over, in two or three days time.

Plumbs and apricocks in artificial air.

(15.) *July* 30. 1677. I included plumbs and apricocks, many of them being first cut asunder, in a receiver, and, afterwards, as much air, produced out of cherries, as was sufficient to sustain sixty-four inches of mercury.

August 1. The fruits had produced no air, but grew yellower than those which were in common air.

August 3. The mercury rose a little higher, and the apricock, which remain'd whole, seem'd full of drops, like water.

August 7. The whole apricock grew softer; the mercury stood, at fifty-nine inches above its usual station.

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

24. The height of it was seventy-seven; tho', I certainly knew, that nothing had issued out of the receiver.

29. Find-

29. Finding, neither the fruits, nor the height of the mercury, changed any more, I open'd the receiver, and perceiv'd, that the apricocks had kept their colour, very well; but the flesh of them was spongy, and their taste inclining to acid. Many bubbles had broke from them, at the time, they were freed from the surrounding pressure.

Plumbs and apricocks in common air.

July 30. 1677. I included the halves of the fruit, just mention'd, in a receiver full of common air; and, with them, others of the same kind, uncut.

July 31. The mercury had gain'd the height of eight inches.

August 1. At six a-clock, in the evening, the mercury was twenty-one inches high; but, in the other receiver it remain'd unmov'd.

August 3. They kept their firmness much better than those included with artificial air. The height of the mercury was thirty-five inches.

August 4. The height of the mercury was forty-two inches.

August 6. The whole apricock, seem'd not at all alter'd. The height of the mercury was fifty-seven.

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

The colour of the whole apricock, yesterday, began, and now proceeded to grow yellow. No moisture appear'd.

Aug. 11	The height of it, was	131	*Aug.* 15	The height of it, was	171
13		157	16		171
14		163	17 and, the days following, it remain'd at the same height		

August 27. The height was one hundred and eighty-two.

August 29. When, neither the fruit, nor the height of the mercury changed any more; I open'd the receiver, and found the apricocks of a more acid, and less grateful taste, than the others, in factitious air; tho' their pulp was of a very good colour, but spungy: they also yielded many bubbles, as did the others.

Hence, 'tis probable, that the artificial air of the cherries, greatly hindred the apricocks from producing air; tho' it promotes the alteration of their colour and firmness; and, also, serves to preserve their taste.

Grapes in common air.

(16.) *October* 10. 1677. I included an ounce and an half of bruised, unripe grapes, in a receiver, that would hold ten ounces of water; and drew out no air.

Octob. 11. The mercury ascended a little.

12. There was but a small change.

13. The height was half an inch.

17. The height was one inch.

18. The height was one and an half.

19. The height was, almost, four.

20. The height the same; but some mouldiness appear'd on their superficies.

21. The height was four and an half.

22, 23, 24. The height remain'd the same, but the mouldiness encreased.

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

Nov. 6.

Nov. 6	The height was	9
8		10
9		12
12		15
14		17
Nov. 18	The height was	23
21		26
Dec. 8		$36\frac{1}{2}$
12		39
27		39

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

Grapes with ſpirit of wine.

Octob. 10. 1677. I made the ſame experiment in another receiver, obſerving the ſame circumſtances; only here I mixed two drams of ſpirit of wine with the grapes.

Octob. 11. The mercury was not changed.
12. There was no change.
13. The mercury was not moved.
Oct. 17. It aſcended a little.
18. The height of it was not yet a quarter of an inch.
19. It was moved but a very little.

Jan. 6. The grapes, during all this time, had produced no air.

Whence it appears, that ſpirit of wine hinders fermentation.

A peach in an exhauſted receiver, with ſpirit of wine.

(17.) *Octob.* 17. 1677. I put a peach into an exhauſted receiver, with ſome quantity of ſpirit of wine, which could not touch the peach, unleſs in vapour.

March 27. 1678. I took out the peach, which had kept its colour, but loſt its firmneſs. Though the receiver was ſmall, yet it was not filled with air; for when open'd, the air ſeemed to ruſh into it: the peach being ſoftned, was ſo depreſſed, that the lower part of it touch'd the ſpirit of wine; the ſuperior part, alſo, had contracted the taſte of the ſpirit of wine, as well as that which was immerged in it.

Peaches in air with ſpirit of wine.

(18.) *Octob.* 17. I included five peaches in an unexhauſted receiver; and with them, ſome ſpirit of wine, which could not touch the peaches, unleſs it were elevated in vapour.

Octob. 18. The mercury aſcended not at all.
20. The height of the mercury was $3\frac{1}{2}$.

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

Jan. 6. 1678. it was 23.
March 28. 1678. it was $31\frac{1}{2}$

Peaches in air without ſpirit of wine.

Octob. 17. I included five peaches in a receiver full of common air, without ſpirit of wine.

Octob. 11. The mercury aſcended not at all.

Octob. 20. The height of the mercury was five inches.

Pneumatics.

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

Jan. 6. 1678. The height was 32.

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

April 15. The liquor in the lower part of the receiver, had all broke out, and the air followed it; then I took out the peaches.

Hence we learn, that the very vapour of ſpirit of wine, ſomewhat hinders fermentation; yet much leſs than the ſpirit itſelf.

(19.) *April* 27. 1678. I included an ounce and a half of paſte, mixed with leaven, in a receiver full of common air, able to hold twenty-three ounces and a half of water. *Paſte with leaven, in common air.*

April 28. The height of the mercury in the gage was two and a half.

April 30. The height of it was three and a quarter.

May 4. The mercury was depreſſed, tho' no air broke away, and the paſte was mouldy. The height of it was two and a half.

Date		The height of it was
May	6	2 $\frac{3}{4}$
	8	3
	10	3 $\frac{1}{2}$
	14	4
May	17	4 $\frac{1}{2}$
	20	5
	24	6
	28	8
June	2	9
	6	10
	14	10 $\frac{1}{2}$
July	5	13 $\frac{1}{2}$
	19	15

April 27. 1678. I included an ounce and a half of unleavened paſte, with common air, in a receiver, capable of holding twenty-three ounces and a half of water. *Paſte, without leaven, in common air.*

April 29. Hitherto the mercury had not aſcended; but this afternoon it roſe a quarter of an inch.

April 30. There was no change.

May 4. The mercury aſcended but very ſlowly, and the paſte was mouldy.

May 6. The height of the mercury was four inches.

Date		The height of it was
May	8	5 $\frac{1}{2}$
	10	7 $\frac{1}{2}$
	14	10 $\frac{1}{2}$
	17	12 $\frac{1}{2}$
	20	13 $\frac{1}{2}$
May	24	16
	28	18 $\frac{1}{2}$
June	2	20 $\frac{1}{2}$
	6	21 $\frac{1}{2}$
	14	25

Hence it ſeems, that leaven rather hinders than forwards the production of air, if the paſte be not made in a hot place.

(20.) *May* 23. I included an ounce and a half of unleavened paſte, in a receiver capable of holding twenty-five ounces of water, and pour'd ſpirit of wine upon it. *Paſte with ſpirit of wine.*

May 24. The mercury was one inch high.

May 26. It was almoſt two inches high.

27. It was two and a half.

31. There was no change.

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

July 19 No change.

Decemb. 14. When the height of the mercury alter'd no more, I open'd the receiver, and found that the paſte had an acid ſmell.

Paſte without ſpirit of wine.

May 23. I included an ounce and a half of unleavened paſte, in a receiver, capable of holding twenty-five ounces of water; but added no ſpirit of wine.

May 24. The mercury aſcended not.

May 26. It was three inches high.

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

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

19 The mercury a little exceeded thirty inches. This day the air broke out, and, therefore, I ſet the ſcrew.

Decemb. 14. The mercury return'd to the height of fifteen inches; when, I open'd the receiver, and found the paſte very acid.

Hence it ſeems to follow, that ſpirit of wine greatly obſtructs the production of air; and the more, if the paſte be fermented; and that unfermented paſte will, in tract of time, produce no leſs air than that which is fermented.

New ale included in receivers.

(21.) *Octob.* 11. I exactly fill'd a receiver with new ale, ſo that no air might be left; and included another quantity of the ſame in another receiver, wherein ſome ſpace was allow'd for the air.

Octob. 12. The cover of that receiver, which contain'd ſome air, was broken; and, therefore, I pour'd the ſame ale into another receiver, wherein there was room enough left for the air: in the receiver, exactly fill'd, the mercury aſcended a little.

Octob. 13. In the receiver, exactly fill'd, the height of the mercury was twelve inches; in the other, thirteen inches; tho' it had been ſhut up a ſhorter time, and a much larger ſpace was left, whereinto the air, newly produced, might have been dilated.

Octob. 14. In the full receiver, the height was thirteen; in the other, eighteen. Towards evening, the full receiver work'd the faſteſt; for the height of the mercury in it was twenty-two; and in the other but twenty.

Octob. 15. In the full receiver, the height of the mercury was forty-two; in the other, twenty-ſix. And ſome bubbles of air, which, in the full receiver, had poſſeſſed its upper part, wholly vaniſh'd; and the ale poſſeſſed a long ſpace, in the mercurial gage, wherein it was not found before.

Octob.

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

The bubbles appear'd again, yet nothing flowed out.

Nov. 8. The full receiver loſt much of its liquor; wherefore, I opened it; when, all the ale ſeem'd as if it would have vaniſh'd into froth, unleſs I had ſuddenly ſtopp'd the little hole, that gave it vent. I many times tried, that, if the hole were opened in the gage, the mercury would preſently deſcend; but, if the hole were again ſtopp'd, it would ſpeedily aſcend. The ale had a moſt pungent taſte.

Nov. 9. I opened the other receiver, and obſerved almoſt the ſame things.

Hence it ſeems to follow, that ale, if the air be wholly excluded from the containing veſſel, will ferment more ſlowly, than if ſome air be left therein; and that, in time, it makes a greater compreſſion, if no room be left for its dilatation.

Peaſe, with ſpirit of wine, in an exhauſted receiver.

(22.) *June* 27. I put green peaſe into an exhauſted receiver, with ſpirit of wine. Towards the evening, the receiver ſeem'd to admit the external air, and the mercury roſe to the height of eighteen inches, when I cloſed the cover with turpentine.

June 30. I perceived no more change in the height of the mercury.

July 7. No air was produced, even in the moſt vehement heat.

Peaſe without ſpirit of wine, in an exhauſted receiver.

June 27. I put other peaſe into an exhauſted receiver, without ſpirit of wine. The receiver, and the quantity of the peaſe, were the ſame as in the laſt experiment.

June 28. The receiver was full of air; tho', I think, it was not exactly ſhut; and, therefore, I again included the ſame peaſe. Towards evening, the height of the mercury was five inches.

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

July 8. The air got out of the receiver.

Hence it appears, that ſpirit of wine hinders the production of air in peaſe.

That the effects of artificial air differ from thoſe of the common, ſhewn in cherries.

(23.) *June* 9. 1677. I put cherries into an exhauſted receiver, and in ſix hours time the mercury came to the height of five inches and a half.

PNEUMATICS.

June 20. The mercury aſcended three and a half. Towards the evening it was two.

The aſcent is here always to be underſtood, as added to the former.

Date	The aſcent was	Date	The aſcent was
June 21	1 ½	*June* 26	3
22	1 ½	27	3
23	2	28	5
24	1 ½	30	1 ⅓
25	1 ½		
July 1	3	*July* 4	2 ½
2	4	5	3
3	2		

The height was forty-eight. But, tranſmitting the air into another receiver, the mercury was depreſſed to thirty-five inches.

July 6. The aſcent of the mercury was four inches, in one night's time.

7. The aſcent of it was five and a half in twenty-four hour's time.

Date	The aſcent of it was
8.	5.
9.	5.
10.	6.

11. The aſcent of it was twelve, in the ſpace of thirty-four hours.

12. The aſcent of it was ſeven.

13. The aſcent of the mercury was three; the height about ninety two inches: but the air being transferr'd into another receiver, the mercury reſted at fifty.

Date	The aſcent was	Date	The aſcent was
14	14	16	13
15	11	17	5

18. The aſcent of the mercury was 9; the height of it 102.

19. The height of the mercury was 92; for I tranſmitted part of the air into another receiver.

20. The aſcent of the mercury was 15.

22. Some air got out, and the height of the mercury was 63 ½.

23. The aſcent of it was 12 ½.

24. The aſcent of the mercury was 4; the height of it 79 inches; but the air, being tranſmitted into another receiver, the mercury reſted at 62.

Date	The aſcent was	Date	The aſcent was
25.	8	27.	4
26.	9	28.	5

30. The aſcent of it was ten, the height ninety-eight. Part of the air being tranſmitted into another receiver, it reſted at ſixty-four.

Date	The aſcent of the mercury was
31.	6.
Aug. 1.	9.
2.	4.

3. I tranſmitted the air into another receiver, and the mercury remain'd at ſixty-eight.

4. I tranſmitted the air again into another receiver, and the mercury reſted at fifty-four.

6. The

6.} The aſcent of the mercury was {7.
7.} {4.

8. There was no aſcent.

9. The aſcent was three inches.

The receiver being opened, I found the cherries of a whitiſh colour, and of very little taſte, tho' not ungrateful; their fleſh was ſpongy.

Hence it ſeems to follow, that cherries contain much air, and that they produce it very irregularly.

(24.) *July* 13. 1677. I put cherries into an exhauſted receiver; and then tranſmitted into the ſame, as much air, produced from other cherries, as ſufficed to ſuſtain fifty inches of mercury.

July 15. Yeſterday, the mercury had not aſcended at all; but this day it was two inches higher; that is, twenty-two, above its wonted ſtation.

July 16. The height of the mercury was twenty-three and a half.

July 17. The height of it was twenty-five.

26. The height of it was forty-three. Some air got out.

27. The height of the mercury was forty-five. More air eſcaped.

30. The height of it was fifty-two.

31. The height of it was ſixty-one inches.

Auguſt 1. The height of the mercury continued nearly the ſame, tho' the air broke out.

Auguſt 27. The air, having been all broke away for ſome time, I took out the cherries, and found them not to have loſt their colour, as in the former experiment: they had contracted no putrefaction, nor mouldineſs, but taſted a little more acid than uſual; and being open'd, there were many cavities in their fleſh, as in fermented paſte, or dough, but not quite ſo thick.

From this experiment, compared with the former, it may, probably, be inferred, that in artificial air, fruits produce leſs air, and ſo the better preſerve their colour and taſte; for the cherries, in the former experiment, remained in the receiver, not much longer than in this.

(25.) *Septemb.* 10. 1677. I put ſix ounces of unripe grapes into a receiver, with common air, capable of containing twenty-five ounces of water; and ſtop'd it firmly by means of a ſcrew. *Grapes common air.*

September 11. The mercury aſcended not at all.

Sept. 12. The mercury ſtop'd a little below one inch.

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

September 23. The height of it was 27. The grapes were not alter'd.

September 24. The height was 30.

25. The height was 31. The grapes began to grow yellow.

PNEUMATICS.

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

October 1. The height remained at 35.

Octob. 2.	The height was 36		*Octob.* 10	The height was	35
5			35	The height was	$32\frac{1}{2}$
6					

The air got not out, but the cold began, and increaſed.

Novemb. 9. The height remained the ſame.

Deceml. 19. Almoſt all the air eſcaped.

Decemb. 20. I took out the grapes, and found by their ſmell and their taſte, that they had contracted ſome mouldineſs, tho' not diſcernable by the eye. They were more firm than before.

Grapes in factitious air. (26.) *Septemb.* 10. 1677. I included two ounces of crude grapes in a receiver, capable of holding eight ounces of water; and to the common air added air produced out of pears, till the mercury reſted ten inches above its ordinary ſtation.

Sept. 11. The mercury deſcended, and its height was eight inches.

Sept. 12. The height of it was 11. the aſcent 3.

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

Sept. 17. The height was 28. The grapes turned yellow.

Sept. 18	The height of it was	29	*Sept.* 22	The height of it was	35
19		30	23		20
20		31	Some air had broke out; and the grapes were of a yellow colour.		
21		33			

Sept. 24	The height of the mercury was	25
25		22
26	The height almoſt the ſame.	
27	The height of it was	22
29		27
30		28

Octob. 1. and 2. The height 28.

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

Novemb. 9. The height was 13. Some air had got out.

December 19. The height of the mercury was 20 inches.

Decemb. 20. I took out the grapes, and their ſmell and taſte were more grateful than of others; their firmneſs rather increaſed, than diminiſhed.

Hence, factitious air ſeems fit to alter colour, and to preſerve taſte; but the firmneſs might be increaſed here, as in turpentine; the ſpirits, in time, being exhaled.

Oranges in common and factitious air. (27.) *July* 18. I took two pieces of orange, and, by the help of a ſcrew, ſtopped them cloſe up in a receiver, with common air; when, into the ſame receiver, I put air, produced out of cherries, as much as ſufficed to ſuſtain

12 inches of mercury. At the ſame time I put a piece of the ſame orange into another receiver, with common air alone, and uncompreſs'd. PNEUMATICS.

July 20. The orange in the common air began to contract a mouldineſs; but the other ſeemed not at all alter'd.

July 23. The mouldineſs of the orange, in the common air, increaſed; the other piece remained ſound.

July 16. The orange, in the common air, did not increaſe its mouldineſs, but ſeemed wholly rotten: the other alſo began to putrefy, but remained free from mouldineſs.

Aug. 1. Perceiving that the oranges were no longer ſenſibly changed, I open'd the receivers; and tho' the air, wherewith I had mix'd the artificial air, was ſo compreſſed in its receiver, that it could not now ſuſtain twenty-ſix inches of mercury above its wonted preſſure, yet the fruits were far better preſerved in it, than in the other; only the ſuperficies ſeemed to have loſt its juice; but all the inner parts, with the rind, were very well-colour'd, well-taſted, and firm: in the other receiver, the whole orange ſeemed almoſt rotten, as well as the rind. The orange was more corrupted in the compreſſed air, becauſe, as it ſeems, no factitious air had been mixed with it.

It ſeems worth obſerving, that the ſame air, generated from cherries, is apt to produce different effects, upon fruits of a different kind; for here it retarded the alteration of colour and firmneſs, which, when I included air with apricocks, it accelerated.

(28.) *July* 20. 1676. I included a ſmall piece of beef in an exhauſted receiver, and put as much air, produced from cherries, into it, as ſuſtain'd 27 inches of mercury. *Beef in factitious air.*

July 21, 22, 23, 25 } The mercury remained almoſt at the ſame height.

July 26. The beef had removed the receiver from its cover; and becauſe it was very fetid, we threw it away.

July 20. 1676. I put a piece of beef into a receiver full of common air, and carefully ſtopp'd it in, by means of the ſcrew. *Beef in common air.*

July 21. The mercury had not at all aſcended in the gage.

July 22. The height of the mercury was one inch.

23	The height of it was	5 $\frac{1}{2}$.		
25		9 $\frac{1}{2}$.		
26		14 $\frac{1}{2}$.	In the evening	18.
27		21 $\frac{1}{2}$.		25.

28. The ſcrew, not being tight, ſuffer'd the air to break out.

Hence it appears, that air produced from cherries, is a great hindrance to the production of air from fleſh.

(29.) *March* 14. 1676. I put two onions into a receiver, full of common air, with a mercurial gage; and faſtned the ſtopple with a ſcrew, to ſee whether vegetation would increaſe, or diminiſh the quantity of the air. *Onions in common air.*

March

PNEUMATICS.

March 28. The mercury ſeemed depreſſed one quarter of an inch; but it afterwards recover'd its former height, and two inches more; and now the air broke out, and the roots grew longer.

April 28. About ten or twelve days ſince, I perceived the roots to be corrupted; and now they were wholly putrefied.

May 9. The mercury continued at the ſame height; for the air had broke away: and, therefore, I took out the onions, and found their roots putrefied, but they were not at all mouldy.

Onions in factitious air.

(30.) *March* 17. 1676. I included two onions in an exhauſted receiver, and afterwards put air, produced from paſte, into it.

March 28. The onions took root, at leaſt, as well as thoſe which I kept in the common air.

April 28. The ends of the roots began to putrefy, yet they were in far better caſe, than thoſe ſurrounded with common air. Perhaps, the cauſe of this difference is, that a greater quantity of water was included with the artificial air. The mercury mounted higher by nine or ten inches.

May 18. Hitherto the onions ſeemed not at all corrupted; but this day I found one of them to be a little ſo; tho' different from a mouldineſs.

Hence we may gather, that artificial air doth not at all hinder vegetation; and that not only the ſenſible magnitude of the body, but alſo the quantity of the air, is increaſed by vegetation.

Unripe grapes in common air.

(31.) *Auguſt* 25. I included ſix ounces of unripe grapes in a receiver, capable of holding twenty-five ounces of water; but did not exhauſt the air.

Auguſt 26. The mercury aſcended a little.

27. The height of the mercury was 1 inch.

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

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

Auguſt 30. The mercury ſeemed to have deſcended, rather than aſcended. The colour of the grapes was leſs alter'd here, than in the receiver, containing air produced from pears.

Aug. 31. The receiver broke, and I left the grapes expoſed to the free air.

Sept. 7. The grapes being left in the free air, ſtill kept their green colour, and were of a grateful taſte, tho' leſs pungent than before.

Unripe grapes in factitious air.

Auguſt 25. I included two ounces of unripe grapes in a receiver, capable of holding eight ounces and a half of water; and having ſtopp'd it cloſe with a ſcrew, I filled it further with air, produced from pears, till it ſuſtained 15 inches of mercury.

Auguſt 26. Some air eſcaped, and therefore I crowded in new, produced out of the ſame pears, till the mercury reſted 17 inches above its wonted height.

Auguſt 27. The mercury was depreſſed below the 16th inch; yet no air had broke out. Towards evening, the mercury again aſcended to 17.

Aug. 28	The height of it was	19	*Aug.* 31	The height of it was	23 $\frac{1}{2}$
29		21	*Sept.* 1		24
30		22	2		24

Sept.

Sept. 4. The last height continued, and the grapes had all contracted a yellow colour.

Sept. 5. The air broke out.

Sept. 7. The air continuing to get away, by degrees; I took out the grapes, and found them very insipid, and of an ungrateful taste.

This experiment confirms the efficacy of artificial air, to alter the colour of fruits. 'Tis, also, very observable, that here it damaged the taste, and promoted the production of the air, contrary to what had happened in the former experiments. It might be worth while to try, whether the same would happen in all unripe fruits.

A July-flower in factitious air.

(32.) *August* 2. 1676. I shut up a July-flower in a receiver, with air produced from paste, made with meal, and not mixed.

August 4. The flower began to change its colour, and to grow moist.

August 9. The July-flower was a little alter'd.

August 12. The moisture gradually increased, but no mouldiness appear'd.

August 31. The July-flower seem'd little alter'd, tho' it was less fresh than those which were kept *in vacuo*.

In common air.

August 2. I shut up a July-flower in a receiver, with common air, not mixed.

August 4. The flower was not changed.

August 9. It grew moist, and had almost lost all its colour.

August 12. A great mouldiness cover'd all the flower.

July-flowers in vacuo.

Aug. 2. I included two July-flowers *in vacuo*, and took special care, that no humidity should be included with them.

Aug. 4. 1676. One of them began to appear moist.

Aug. 31. 1677. During the whole year, the July-flowers had suffered no change.

Hence it seems probable, that factitious air hastens the change of colour, yet it prevents mouldiness as a *Vacuum*.

Apricocks and plumbs in common air.

(33.) *July* 24. I put apricocks and some plumbs, several of which were cut in pieces, in a receiver full of common air, and stopped it firmly with a screw.

July 25. The mercurial gage was spoiled; so that I could not, by any means, perceive the quantity of the air generated.

July 30. The fruit seemed not at all alter'd, except that one of the cut plumbs had contracted something of mouldiness.

Aug. 2. I opened the receiver, and found all the fruit firm, of a good colour, and a grateful taste.

The same in artificial air.

July 24. I made the same experiment in another receiver, with the same circumstances; only into this last receiver, I intruded air, produced from cherries, till it sustain'd twenty-two inches of mercury.

July 25. The mercury descended three inches, and rested at nineteen. Toward the evening, it recover'd its former height, and rested at twenty-two.

July 26	The height of it was 28	*July* 28	The height of it was 36
27	34 ½	29	40

July

PNEUMATICS.

July 30. The height was forty-four. The apricocks which were cut, began to moiften, and diffolve into water.

July 31 } The height was { 51
Aug. 1 } { 60

Aug. 2. The height was fixty-five. Towards evening, when fome liquor had efcaped out of the receiver, I fcrewed it tighter; but one of the iron-wires being broken, all the air got away. Wherefore, I took out the fruits, and found them very foft; efpecially thofe, whofe lower parts were immers'd in the water: the reft were a little more firm, but all of them retain'd a grateful tafte.

Hence it feems, that air produced from cherries, promotes the alteration both of colour, and firmnefs in apricocks.

It appears, alfo, that fome part of fuch air is deftroyed at the firft.

Plumbs in common air, in artificial air, and in vacuo.

(34.) *July* 30. 1676. I put plumbs, cut afunder, into three receivers; one of which was full of artificial air, produced from goosberries; the fecond, full of common air; and the third exhaufted.

Aug. 2. In the artificial air, the plumbs were not changed; in the common air, they began to be mouldy; but in the evacuated receiver, they retain'd their colour, and were foft.

Aug. 5. In the artificial air, the plumbs had contracted a red colour, humidity, and foftnefs; in the common air, they feemed black and mouldy, yet retain'd their firmnefs; in the evacuated receiver, they were almoft diffolved.

Aug. 7. The plumbs, in the common air, began to foften.

Aug. 8. The plumbs, in the common air, feemed to have loft their black colour, and to have contracted a red one; as it happen'd three days before, to the plumbs in the artificial air.

In this experiment, artificial air feems to have promoted an alteration.

Peaches in common and artificial air, mixed.

(35.) *Sept.* 24. I put five peaches into a receiver, with common air, mixed with fome produced from grapes; and included the grapes themfelves in the fame receiver, that the common air might be the better faturated with the artificial.

Septemb. 25. The height of the mercury was twenty-one inches.

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

Octob. 2. The fame height continued.
3. The height of it was 52 and a half.
5. The height the fame; but the peaches feem'd moift.
6. The height of it was 58.
7. The height of it was the fame.
8. The height of it was 61.
11. The mercury afcended a little.
19. The height of it was 65.
25. The height of it was 61. The cold was fharp.
27. The cold abated, and the mercury afcended.
30. The height was 61, and a little more.

Nov.

Nov. 2. The height of the mercury was 59. 'Twas ſevere cold weather. PNEUMATICS.

6. The height was *61*. The froſt broke, and it thaw'd.

7. The mercury ſeemed ſomewhat higher.

9. The mercury perſiſted at the ſame height.

Dec. 9. In one month's ſpace, the mercury aſcended, by degrees, to the height of eighty inches.

April 1. 1678. The mercury came to ninety-ſix inches above its wonted height. I now opened the receiver, and whilſt the air was breaking out, the peaches emitted many bubbles thro' their skins, not without a violent noiſe; and the skin, in ſome of them, was broken: they had preſerved their taſte, and the colour of their pulp; but loſt their firmneſs, as if they had been boil'd: being left in the air for three hours, they were all rotten.

This experiment proves, that common air corrupts bodies, tho' much the leſs for being mixed with factitious air.

(36.) *Auguſt* 4. The firſt receiver. I cut five pears, each of them into four parts; and put one part of each into a receiver full of common air, and ſtopp'd it cloſe with a ſcrew. *Pears in common air.*

Aug. 6. The colour of them was little alter'd, and the mercury aſcended not at all.

Aug. 7. The pears were little alter'd; and the mercury was a little higher.

Aug. 8. The pears underwent no great change; the height of the mercury was four inches.

Aug. 9. The height of it was four and a half.

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

The pears began to be ſoft.

Aug. 15. The height of it was 21.

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

17. Now I found the air had eſcaped.

18. The air being almoſt all got out, ſince yeſterday in the evening, and the fruit looking worſe, I took the pieces out, and found them putrefied.

Aug. 4. The ſecond receiver. I took one quarter of each of the aforeſaid pears, and included them, after the ſame manner; and, afterwards, added air, produced out of cherries, till the mercury poſſeſs'd twenty-three inches extraordinary. *Pears in artificial air.*

Aug. 6. The fruit was not alter'd, except a little in their colour.

Aug. 7. Almoſt all the pieces ſeem'd rotten; the mercury remaining at the ſame height.

Aug. 8. The pears were not alter'd much; but I could not ſee the mercury.

Aug. 10. They, gradually, grew ſofter; and the mercury was forty inches above its wonted height.

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

Aug. 16. The mercury defcended; yet nothing had got out.

Aug. 17. The mercury exceeded not fixty-feven inches in height; yet the air could by no means efcape.

Aug. 18. The mercury remain'd at the fame height; but, fuffering the air to break out, it had a fharp odour; and the tafte of the fruit feem'd very acid, and the pulp exceeding foft.

Pears in an unftopp'd receiver.

Auguft 4. 1677. The third receiver. I put a quarter of each of the aforefaid pears into a receiver, not exactly fhut.

Aug. 6. The pears feem'd to change their colour.

Aug. 7. One of the pieces began to lofe its firmnefs; but, in the artificial air, another piece yefterday feem'd wholly rotten.

Aug. 8. One piece was mouldy; the reft were foft.

Aug. 9. The pears gradually grew more rotten.

Aug. 11. They were wholly mucid, and rotten.

This receiver, compared with the firft, fhews, that corruption begins not in the free air fooner than in included air; but, when begun, that it is much more violent and fudden; becaufe the included air may be fatiated.

Pears in vacuo.

Auguft 4. 1677. The fourth receiver. I included one quarter of each of the faid pears *in vacuo.*

Aug. 6. The height of the mercury was 5.

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

20. Hitherto the pears had undergone no alteration; but this day they began to be foft. The mercury afcended not.

Aug. 26. Neither the pears, nor the height of the mercury, were at all alter'd.

This production of the air feems very regular.

Hence we find the aptnefs of artificial air to foften fruits.

And that the production of air was here promoted by artificial air, is very probable; tho' it had fucceeded otherwife with apricocks.

Apricocks in common air.

(37.) *Auguft* 21. 1677. The firft receiver. I divided fix apricocks, each into four parts; and put one piece of each into a receiver full of common air, and ftopp'd it firmly with a fcrew.

Aug. 22. The apricocks feemed riper than yefterday; but no air was produced by them.

Aug. 23. One piece, contiguous to the water, began to be mouldy, and the reft inclined to putrefaction. The mercury feemed to have afcended a little.

Aug. 24. A piece next the water, was cover'd with much mouldinefs; another piece, more remote from the water, was fomewhat mouldy alfo; but all were rotten.

Aug.

Aug. 25. The fruit contracted no more mouldiness; but the putrefaction increased. The height of the mercury was seven inches. PNEUMATICS.

Aug. 26. The height of the mercury was 15.
28. The height of it was 30.
29. The same height continued.
30. The height of it was 33. The fruits were almost all dissolved.
31. The height of the mercury was 38.

Septemb. 1. The height of it the same.
2. The same height still.
3. The mercury ascended a little.

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

Septemb. 9. The same height continued.

Sept. 22. Little or no change appear'd in the height of the mercury; but the fruit was almost dissolved into water.

Octob. 1. When the mercury continued at the same height, and the fruit seem'd almost vanish'd, I open'd the receiver, and found the apricocks very much impaired and soft; yet they retained a taste not unpleasant, but tending to acid.

Aug. 21. 1677. The second receiver. I cover'd one quarter of each of the aforesaid apricocks, with a receiver, not defended against the external air. *Apricocks in an open receiver.*

Aug. 22. They were flaccid, as if they had been dry, or wither'd.

Aug. 23. Many of them appear'd rotten and mouldy.

Aug. 24. The apricocks were wholly putrefied, and mouldy.

Aug. 21. The third receiver. I included firmly, by the help of a screw, one quarter of each of the aforesaid apricocks, in an unexhausted receiver; to which I, afterwards, added air produced from pears, till it sustain'd 20 inches of mercury. *The same in one unexhausted, with an addition of factitious air.*

Aug. 22. The mercury ascended not at all; but the fruit seemed to have acquired a greater degree of maturity, than that included in common air.

Aug. 23. These seemed less alter'd, than those which were in common air.

Aug. 24. They remain'd unalter'd.

Aug. 25. The fruits began to produce air, but I could not discern the quantity.

Aug. 26. Little alteration in the fruit.

Aug. 28. It began to moisten, yet was far less alter'd than that which remain'd in common air.

Aug. 30. The mercury emerg'd above the bodies, by which it was hid. Its height above the wonted station, was thirty inches.

Aug. 31. The height of the mercury was forty inches.

Sept. 1. The height of it was the same.
2. The same height continues.
3. The height 45.
8. The height was little changed.
9. The height 40; yet no air got out.
11. The height was 38.

 12. The

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12. The mercury continued to deſcend.

13. The height of it was 33.

Sept. 14. The mercury was ſo depreſſed, as to appear no more.

Sept. 22. The mercury emerged again; its height was 33. The fruit was cover'd with a kind of mucor.

Octob. 1. When neither the apricocks, nor the height of the mercury, were any more alter'd, and the mucor vaniſhed, I open'd the receiver, and found the apricocks not impaired, but of a good colour, their pulp ſpongy and ſoft, and of a taſte inclining to acid.

Apricocks in an unexhauſted receiver, whoſe air was afterwards condenſed.

Aug. 21. The fourth receiver. I took a quarter of each of the ſaid apricocks, and ſhut them up firmly, with a ſcrew, in an unexhauſted receiver; into which, afterwards, I intruded air, till the mercury roſe 90 inches above its ſtandard height.

Aug. 22. Our receiver broke into an hundred pieces, by the force of the air compreſſed within it; whereupon, I put the fruit into another, and added only ſuch a quantity of air as was able to ſuſtain ſixty inches of mercury.

Aug. 25. The apricocks had contracted much mouldineſs; and I added new air.

Aug. 26. They were wholly infected with mouldineſs and rottenneſs.

This receiver, if compared with the former, ſhews, that the quantity of corruption depends on the quantity of the air.

Hence we have it confirm'd, that alterations are made more ſuddenly in factitious air; and that, in time, the corruption is far greater in common air.

That the effects of compreſſed air differ from thoſe of the common, ſhewn by onions in condenſed air.

(38.) *March* 21. 1677. I put two onions into a receiver, which was to be ſtopp'd cloſe with a ſcrew; and intruded ſo much common air thereinto, as raiſed the mercury ſixty inches above its uſual ſtation.

March 28. The onions took root as well as other onions which I included in common air at the ſame time.

April 28. The onions included in common air, eight days ago, were cover'd with mouldineſs, though, in the beginning, they had ſhot numerous roots: the onions in the other receiver began to corrupt at the ends of their roots; but the compreſſed air, ten days before, had found a gradual paſſage out, and now was almoſt wholly eſcaped. I, therefore, put in new air, till the mercury had attain'd to the height of ſixty inches above its uſual ſtandard.

April 29. The onions in the compreſſed air, were cover'd all over with mouldineſs.

Hence it ſeems to follow, that a little compreſſure doth not prejudice bodies to be expanded by vegetation.

And the new air, which was intruded, ſeems to have promoted the mouldineſs, though, probably, in the beginning, the compreſſure of the air retarded both the mouldineſs, and the corruption.

Tulips and lark-ſpurs in common and compreſſed air.

(39.) *May* 9. I put two equal quantities of tulips and lark-ſpurs, into two receivers of an equal bigneſs, and ſtopp'd them up firmly with ſcrews:

I left one of them with common air only, but compreſſed the other by the intruſion of new air, till the mercury exceeded its wonted height by ſeventy inches. PNEUMATICS.

May 11. Two tulips, in the common air, contracted mouldineſs; but all things remained unalter'd in the compreſſed air.

May 12. A third tulip, in common air, began to be finewed; but nothing like it happen'd in the compreſs'd air.

May 14. One tulip, in the compreſs'd air, was finew'd; but thoſe in the common air, were all very mucid; and one of the lark-ſpurs, in the common air, had alſo contracted a mucor.

May 17. Three of the tulips in compreſs'd air, had contracted a finew; but not half ſo much as thoſe in the common air. Two of the lark-ſpurs, in the common air, appear'd finewed alſo; but thoſe ſhut up in compreſs'd air, were preſerved freſh, and wholly free from mouldineſs, or finew.

May 21. The flowers in the common air, were all rotten and putrefied; but thoſe in the compreſſed air, received no further alteration: and the tulips, which had contracted ſome finew, ſeemed rather to loſe it, than to acquire new.

May 30. When the flowers, in the common air, being wholly putrefied, were diſſolved into water, I took them out, and kept the liquor in the veſſel, to try whether any inſects would breed therein. In the compreſſed air, the flowers ſuffer'd no more ſenſible alteration; I, therefore, took them out, and found them moiſt, and of an acid odour.

Hence, it ſeems that compreſſed air hinders putrefaction and mouldineſs, in ſome plants.

Orange in compreſſed and common air.

(39.) *May* 21. 1677. I cut an orange into two equal parts, and incloſed one of the halves in a receiver, with air ſo compreſſed, that it would ſuſtain an hundred inches of mercury above its wonted height: I left the other half in another cloſe receiver, only with common air.

May 25. Each half of the orange had contracted mouldineſs; but that in the common air was much more mucid than the other.

May 26. The compreſſed air had entirely got out, and therefore I put in new.

May 30. I every day perceiv'd ſome air had eſcaped, and, therefore, daily ſupplied freſh. And the orange, by receiving new air ſo often, contracted a mucor, notwithſtanding the compreſſure, much more than the other piece that was left in the ſame air without preſſure.

June 1. I took out the two half oranges; and that which lay in the compreſſed air, ſeemed to have contracted a corruption, at leaſt, three times greater than that which had continued in the common air.

Hereby the diſpoſition of compreſſed air, to retard corruption, is confirmed; yet, in time, 'tis very probable, that the quantity of corruption may depend upon the quantity of the air.

Roſes in common and compreſſed air.

(40.) *May* 31. 1677. I included two equal quantities of roſes, in two receivers, ſtopp'd by the help of ſcrews; into one of which I intruded as much

PNEUMATICS. much air as would ſuſtain ninety inches of mercury, beſides its accuſtomed preſſure; but I left the other with common air only.

June 11. The roſes in the common air were free from mouldineſs, only they ſeemed to have loſt ſomething of their colour: but thoſe ſhut up in the compreſſed air, had almoſt all contracted a yellow colour, as if they had wither'd in the open air; yet they were not mucid, or finewed.

June 18. This laſt week, the flowers, in the common air, ſuffer'd not the leaſt change; but thoſe in the compreſſed air, grew yellower. I open'd both receivers, and found the roſes to have retain'd their ſcent, yet it was ſomewhat alter'd; neither were they dry, or wither'd. I kept them apart in the open air, and found that thoſe taken from the compreſſed air, were not ſo ſoon alter'd by the contact of new air, as thoſe which had remained in uncompreſſed air.

Hence it ſeems to follow, that compreſſed air is ſometimes fitter to alter colour than common air. And, perhaps, it may be worth our notice, that roſes ſo included, contract not a mouldineſs, but only a yellow colour; tho' in tulips and lark-ſpurs, 'tis otherwiſe.

Orange in compreſs'd air, and common.

(41.) *June* 1. 1677. I put the two halves of the ſame orange into two receivers; in the one I increaſed the quantity of air till it ſuſtained the mercury an hundred inches above its wonted height; but left the other uncompreſſed, only exactly ſhut.

June 6. Each half of the orange grew mouldy; eſpecially that, whoſe ambient air was compreſſed. But new air was every day ſupplied; for the compreſſed air, in 24 hours time, had almoſt all got out. But in the former, it had remain'd very well ſhut in, for ſix whole days.

June 11. The orange, in the common air, contracted no more mouldineſs; but, in the compreſſed air, the mouldineſs gradually increaſed.

June 18. Finding the mouldineſs of the orange, in the common air, to diminiſh, rather than increaſe, I took it out; and perceiving further, that, in compreſſed air, the orange was not more mucid, after I had ceaſed to intrude new air, I was willing to try, whether the new air ſupplied new ſtrength to the orange, to exert and thruſt out its mouldineſs; and therefore, made the mercury in the gage, by means of the air intruded, to exceed its wonted height 80 inches.

June 20. Two days after I had intruded new air into the receiver, the mouldineſs of the orange appeared to be manifeſtly greater.

Hence we may gather, that the quantity of the mouldineſs depends on the quantity of the air.

Shrew-mice in common and compreſſed air.

(42.) *June* 17. 1677. I put two ſhrew-mice into two receivers, of equal bigneſs, and ſtopped them up carefully; in one of them I left only common air; into the other, I intruded air, till the mercury was higher, by 30 inches, than uſual: the mouſe, in the common air, was included about 52 minutes paſt 5 of the clock; and 6 minutes after the other.

The mouſe, in the compreſſed air, ſeemed to loſe his ſtrength much ſooner than the other, the motion of his breaſt being leſs frequent: yet, about 18 minutes after 6 o'clock, the mouſe in the common air, which ſeemed the

ſtronger

ſtronger, fell into convulſive fits,and died ;but that in the compreſſed air, ſeemed then, and ſome time after, to be as well, as he was an hour and half before. Pneumatics.

About eleven of the clock, the mouſe in the compreſſed air, ſtill breathed ; but, about four in the morning, he was found dead, in the ſame poſture wherein he was ſeven hours before : whence we may conjecture, that he was free from convulſive fits.

I muſt not omit, that the mouſe, in the common air, had conſumed ſomething of that air ; ſo that the mercury ſtood at 29 inches, and, when the receiver was opened, preſently aſcended to 30.

Hence we learn, that compreſſed air ſeems fitter than the common, to prolong life ; ſince the one mouſe lived ſo much longer, tho' only a double quantity of air was included in the receiver.

(43.) *June* 13. 1677. I put four flies into a receiver, and aftewards intruded air, till the mercury roſe ſixty inches above its wonted height ; and at the ſame time, included three other flies, in another receiver, with common air not compreſſed. *Flies in common and compreſſed air.*

June 14. In the morning, all the flies were well. In the afternoon, I found two of them dead in the compreſſed air ; but in the common, they were all alive. About five a-clock, one of the flies, in the compreſſed air, was alive, and three in the common air.

June 15. This morning I found all the flies in the common air dead ; but that ſingle one which remain'd alive in the compreſſed air, ſeem'd ſtill to be very well ; and, being taken out of the receiver, flew briſkly away.

Hence it ſeems, that flies are not very ſenſible of the air's compreſſure ; and that they die more for hunger, than want of air : for the fly which remain'd ſo long well, fed upon the carcaſſes of thoſe which were dead ; ſo that ſhe ſeem'd not to be diſtemper'd.

(44.) *June* 15. I repeated the preceding experiment, only including four flies in each receiver, and compreſſing the air ſomewhat more.

June 16. This morning I found two of the flies, in the common air, dead ; and but one in the compreſſed air.

About two in the afternoon, the four flies, in the common air, ſeemed to be dead ; but, in the compreſſed air, the three were alive.

Hence, the compreſſure of the air ſeems of ſmall conſequence to flies ; and, indeed, they are not prejudiced by the rarifaction of it, without great difficulty, and unleſs there be almoſt a compleat vacuum.

(45.) *June* 18. I included two frogs in two receivers, and ſtopped them by the help of ſcrews ; the one only contain'd common air, the other, air compreſſed, till it ſuſtain'd ſeventy inches of mercury. *Frogs in common and compreſſed air.*

June 19. Both the frogs were alive ; and the height of the mercury, in both receivers, remained the ſame.

June 20. Neither of the frogs were dead ; and they ſeemed rather to

dimi-

PNEUMATICS. diminiſh, than increaſe the air: but the difference was ſo ſmall, that I dare not be poſitive therein.

June 21. In the morning, both the frogs were alive; but, towards evening, that in the common air was found dead.

June 22. At evening the frog, in the compreſſed air, was alive.

June 23. In the morning I found it dead.

Oranges in common and compreſſed air. (46.) *June* 18. 1677. I ſhut the two halves of the ſame orange, in two receivers, and ſtopped them by the help of ſcrews; the one with common air, the other, with air compreſſed to ſuſtain ninety inches of mercury.

June 22. This morning I found the orange, in the common air, mouldy; but the other was ſound.

At three in the afternoon, the orange, in the compreſſed air, ſeemed, alſo, to have contracted ſome mucor.

June 23. The orange, in the common air, was far more mucid than the other.

June 24. The orange, in the common air, did not increaſe its mouldineſs; but the other was cover'd all over with it.

June 28. The mouldineſs, produced in the common air, was now wholly vaniſhed: in the other receiver, I perceiv'd no further alteration in the fruit.

June 30. Both remaining in the ſame ſtate, I took them out. The part which was kept in common air, ſeemed half rotten; but the other, beſides its finew, appear'd wholly putrefied.

Hence 'tis confirm'd, that the quantity of the mouldineſs depends on the quantity of the air.

It ſeems alſo worth obſerving, that the mouldineſs appear'd a little later in the compreſſed air, than in the common, tho', afterwards, it increaſed much more.

Roſes in common and compreſſed air. (47.) *June* 29. 1677. I included roſes in two receivers, ſtopp'd by the help of ſcrews; I left one with common air only, but filled the other with ſo much, that the mercury aſcended ninety inches above its uſual height.

July 14. Four or five days ago, I found the roſes, in the compreſs'd air, wither'd, and degenerated to a yellow colour. There was not the leaſt alteration in the other receiver.

July 17. When I perceived, that this experiment proceeded after the ſame manner as that above-mention'd, I took out the roſes. Thoſe kept in the compreſſed air, were very much corrupted, and of a very ungrateful ſmell; but the others were little alter'd, and their ſcent not unpleaſant.

Hence we have a further confirmation, that the quantity of corruption depends on the quantity of the air.

Lemmons in common and compreſſed air. (48.) *July* 4. I cut a lemmon aſunder, and put the halves into two receivers, to be ſtopp'd by ſcrews; the one I left with common air only, but the other was fill'd with ſo much compreſſed air, that it ſuſtain'd ninety inches of mercury above its uſual ſtandard.

July

July 7. This day, both parts of the lemmon seem'd to grow mouldy at the same time. PNEUMATICS.

July 17. That in the compressed air had contracted much more hoar than the other; and, perceiving no farther alteration, I took them out, and found the lemmon, in the compressed air, far more putrid than the other.

Hereby it is confirm'd, that the quantity of corruption depends on the quantity of the air.

It seems also, that a triple compression of the air, in respect of a lemmon, is too weak, sensibly to retard the production of finew.

(49.) *July* 18. 1677. I included two parcels of July-flowers, equal in number, in two equal receivers, and stopp'd them close with screws; I fill'd the one with compressed air, till it sustain'd an hundred inches of mercury, extraordinary; but the other was left with common air alone. *July-flowers in common and compressed air.*

July 23. In the compressed air, the July-flowers shew'd some hoariness; the others appear'd only moist; but the mercury exceeded its wonted height only seventy inches; for some of the air had got out.

July 25. In the compressed air, the July-flowers proceeded to corrupt much faster than the others. They had wholly lost their colour.

July 26. In the compressed air, the July-flowers were wholly putrefied, and cover'd with a hoary finew; the others were moist only in some places.

August 1. Perceiving no farther alteration, I took the flowers out of their receivers; those which were kept in compressed air, were rotten, and fetid; the other kept their colour, and their smell was not offensive; but they were moist.

And this is a farther confirmation, that the quantity of the air increases corruption.

We may, also, observe, that the mouldiness is not produced, but in compressed air; nor is it probable, that this happen'd by chance; since, in each receiver, there were three or four July-flowers included.

(50.) *July* 21. 1677. I included a shrew-mouse in a receiver, with common air, and shut him in firmly with a screw, to try whether he would produce, or consume air. *A shrew-mouse confined in common air.*

After two hours, the mouse died, and some air was consumed; but a less quantity than in the former experiment of this kind.

July 24. Hitherto I found no change in the height of the mercury.

Towards evening, it seem'd a little higher.

July 25. This morning much air was produced *de novo*.

July 26. The quantity of the produced air increased.

Hence we have a confirmation, that living animals consume air; but dead ones produce new.

(51.) *August* 31. I put pears into a receiver; whereto, after it was well stopped I added as much air, as sufficed to sustain thirty inches of mercury, extraordinary. *Pears in compressed air.*

September 1. The mercury was depressed.

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Sept. 2. The height of the mercury decreaſed; it exceeded not twenty-five inches.

Sept. 3. The mercury roſe one inch higher; and ſtaid at 26.

Sept. 4. The height thereof was 28.

Sept. 8. The receiver leaking, I put in new air; and, this day, opening the receiver, to compare the taſte of this fruit with that of the other; I found, that five of the pears had loſt their firmneſs, but two retain'd it.

Pears in common air. *Auguſt* 31. I included pears, of the ſame kind, in another receiver, with common air only, not compreſs'd.

September 1. The mercury was a little depreſs'd, as if it had been in compreſs'd air; the cauſe whereof might be only the cold.

Sept. 2. The mercury varied not.

Sept 3. The height of the mercury was one inch, above the uſual ſtandard.

Sept. 4	The height of it was	4	*Sept.* 6	The height of it was	6 ¼
5		6 ¾	7		12

Sept. 8. The height of the mercury was twenty. The pears, being taken out of the receiver, had preſerv'd their taſte much better than thoſe included *in vacuo.* They, alſo, retain'd their firmneſs.

Pears in vacuo. *Auguſt* 31. I included pears of the ſame ſort *in vacuo*; but ſome external air brake in, and the height of the mercury was one inch.

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

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

Hence it ſeems to follow, that, in a greater compreſſure, a leſs quantity of air is produced.

A ſmall bird in compreſſed air. (52.) *December* 7. I ſhut up a ſmall bird in a receiver, capable of holding twenty ounces of water. The bird began to be ill, before I had ſet the ſcrew; but, after I had intruded ſo much air, as to ſuſtain thirty inches of mercury, above its wonted height, ſhe ſeem'd to recover; but, ſoon after, began again to be ſick: and, therefore, I intruded air the ſecond time, till the mercury reſted forty-five inches above its uſual height; whereby the bird was again reſtored: but, in a little time, ſhe began to gaſp again; then, opening the receiver, after ſhe had ſtaid in it twenty-eight minutes, ſhe flew out, and was very well.

A ſhrew-mouſe in compreſſed air. (53.) *January* 20. 1678. I put a ſhrew-mouſe into the receiver of the wind-gun, above-deſcribed; and, immediately, ſo far condens'd the air, till it was reduced to about the twentieth part of its ſpace: then I preſently diſcharged that air, and the elliptic hole being open'd, I ſuſpected that the mouſe had been only a little convulſed; but, when he was taken out, there were no ſigns of life in him. Whether the cauſe of his death were to be aſcribed to the narrowneſs of the receiver, or to the compreſſure of the air, is a queſtion.

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I put another mouse into the same receiver; and the air, being reduced to a third or fourth part of its natural space, I open'd the receiver, but not so carefully as in the former experiment; yet the mouse, taken out therefrom, was found to be very well.

I, afterwards, repeated the experiment; the air being about seven or eight times condens'd; and the mouse seem'd to suffer no inconvenience thereby.

I made the same experiment again, in air compressed seven times, and left the mouse included for twenty-four minutes; which time being elapsed, I discharged the air, and, opening the hole, perceiv'd the mouse to fetch many deep groans, as it were: I took him out, but he could not recover.

Hence it is manifest, that a great compression of air is pernicious and destructive to animals.

A shrew-mouse in condensed air.

(54.) *January* 28. 1678. I put a shrew-mouse into a glass, to whose neck we tied a bladder, that stopp'd the orifice. These were put into a receiver for compressing of the air. Soon after, when the mouse began to be sick, I compressed the air, and the bladder was straitned; so that the mouse was in condensed air, whilst no new air could pass to him: then he seem'd to be much better, and his heart beat less frequent; when, opening the receiver, he was, in a short time, as well as ever.

I repeated the experiment, and left the mouse so long, that he could hardly breathe, whilst I began to compress the air; and the compressure seem'd again to abate his respiration: the receiver being open'd, and the mouse exposed to the air, he could not breathe much more freely; but, if I blew the air on him with bellows, he seem'd to be something reliev'd. Being, again, committed to the compress'd air, he breathed less frequently, and, at last, died.

March 25. Because, in the preceding experiment, it was not clear, whether the air enter'd thro' the ligature of the bladder, I used the instrument described, *Fig.* 79. And when I perceiv'd the mouse was sick, and breathed seldom, I intruded water into the receiver, so that the air was reduced to half of its space; and, then, the mouse breath'd more rarely. But if, extracting the water, I left the whole space entire for the air, his respiration seem'd more vivid; and the air being thus, many times, contracted and dilated, the sick mouse seem'd to respire more freely in the common air than in the compressed. Whence, I conjectur'd, that air is to animals, like food; the quantity whereof ought to bear some proportion to their strength. And, that I might more certainly know this, I put the same mouse into my pneumatic engine, and rarify'd the air, so that it possess'd more than double the usual space. Whilst the air was rarifying, the mouse presently began to be better; yet, a little while after, he seem'd sick: and when the air was restor'd, it in no wise affected him. I, thus, repeated the rarifaction three times, with the same success; but, at last, the mouse died.

The effects of artificial air upon animals; and, first, upon [illegible] a bee included with distilled vinegar, and powder'd coral.

(55.) *May* 5. 1677. I put a bee, with distill'd vinegar, and pulveriz'd coral, into a receiver; and the air, being wholly exhausted, I consider'd the

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Hence a ſuſpicion ariſes, that artificial air is unfit to preſerve the life of animals.

Flies in artificial air of gooſberries.

(56.) *Auguſt* 12. 1676. I put two flies into a receiver, and, exhauſting the common air, ſubſtituted, in its ſtead, air produced from gooſberries, till it ſuſtain'd twenty-ſix inches of mercury.

Afterwards, I put two other flies, alſo, *in vacuo*; but with this difference, that I let ſo much common air into them, as could ſuſtain twenty-three inches of mercury.

Within a quarter of an hour, the latter flies, upon the reſtitution of the air, recover'd that power of motion, which they had loſt *in vacuo*, and flew about in the rarified air; but the former lay without any motion, tho' they had receiv'd a greater quantity of air.

Aug. 13. The flies, in the artificial air, ſeem'd ſtill dead; but the others were luſty.

The flies, taken out of the artificial air, and expoſed to the common air, remain'd ſo, all this whole day, without recovering life.

Aug. 18. I repeated the experiment, with the ſame ſucceſs; tho' I had reſtored a greater quantity of artificial air.

Hence we have an high confirmation, that artificial air is noxious to animals.

Flies included with fire in the artificial air of paſte.

(57.) *June* 22. 1677. I put paſte into three receivers, out of which, I, afterwards, exhauſted the air.

June 23. When the three receivers did, this day, regurgitate with air produced from the paſte, I kindled a perfumed cone, and put it into one of the receivers; which, being preſently ſtopp'd, the fire, within one minute, went out: then, by blowing, I expell'd the artificial air from the receiver, and put fire into it, as before, and it burn'd bright for a conſiderable time; tho' I had ſhut the receiver as ſpeedily, and as accurately, as before.

I made another experiment, after the ſame manner, with a fly; and, in the artificial air, ſhe preſently ſeem'd to be dead; but, afterwards, being expoſed to the ſun, ſhe, in a ſhort time, grew well again. Then, I blow'd common air into the receiver; but the fly, included as before, ſuffer'd no inconvenience thereby.

I repeated the ſame experiment, with the ſame fly, in the third receiver, fill'd with artificial air, with the ſame ſucceſs; only this fly, when taken from the artificial air, could not be reſtored, without longer time, becauſe ſhe was left longer therein.

Hence it appears, that factitious air is prejudicial to fire, as well as to the life of animals.

Flies and frogs in artificial air.

(58.) *June* 25. 1677. I put paſte into four receivers; and totally exhauſting the air from two of them, I pump'd out only half the air from the other two.

June

June 26. I found the two receivers, which I had left half full of common air, to be quite fill'd with air newly produced; but know not, whether they had, for some time, regurgitated or no, so that the quantity of common air was much diminish'd. However, I put two flies, at once, into one of the receivers, after the manner before described; and, soon after they touch'd the bottom of the receiver, they remain'd without motion. I put a third fly into the receiver, after the same manner, and found she liv'd a little longer there, than the former. A fourth fly, being put in, maintain'd her life longest of all; yet, at last, suffering some convulsion, she lay moveless on her back. All the flies, after some stay in the artificial air, being taken out, and expos'd to the common air, grew well in a short time.

I made the same experiments in another receiver, half full of artificial air, and, in a manner, with the same success; but the flies in that receiver, into which only common air was admitted, soon recover'd their strength and motion.

June 27. I found one of the receivers, which was wholly evacuated of common air, to be full of artificial air; but, it being casually thrown down upon the ground, entrance was, thereby, given to the external air; yet I put a frog into it, which seem'd not to be very sick therein.

June 30. The fourth receiver, by the power of the produced air, seem'd, at length, forc'd away from its cover. I put a frog into it, and she fell into high convulsions, for five minutes space; and then lay without motion. In four minutes after, I open'd the receiver, and, taking out the frog, she remain'd, for forty-six minutes, without motion; but, afterwards, in four or five minutes more, she grew very well.

Hence it is evident, that artificial air is very hurtful to the life of animals; but that, if mixed with common air, it doth not so readily produce its effects.

(59.) *June* 28. 1677. I put paste into four receivers, three of which I caus'd to be wholly exhausted of common air; but the fourth was left half full of air.

June 29. One of the receivers, that were wholly exhausted, was found full of air, newly produced; and a frog, being put into it for four or five minutes, had strong convulsions: then, for one minute, she lay without motion; whereupon, I took her out, and, in five minutes, she began to move, and, a while after, became well again.

I took another receiver, fill'd with artificial air, and, putting a frog into it, in seven minutes she ceas'd to be convulsive. And, afterward, when she had lain one minute there, without motion, I open'd the receiver, and, taking her out, found that she began to struggle, and move; tho' I judg'd those motions to be the remains of her convulsions: for, after that, she continu'd moveless for half an hour, and more; yet, at last, she grew well again.

The receiver, from which I exhausted only half its air, had so long regurgitated with produced air, that, very probably, much common air

had

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I put a frog into a receiver, full of common air, to try whether, the paste being now taken out, she would live any longer there.

July 1. In the afternoon, I found her dead, tho' she breath'd in the morning; so that she liv'd about forty-eight hours.

June 30. I put a frog into the fourth receiver, which was wholly fill'd with artificial air; for seven minutes and an half, she was vehemently convulsive, and, at last, died; then, after two minutes, she was taken out, but recover'd no motion at all.

July 1. Perceiving the frog to remain in the same posture, I threw her away.

Hence we have a confirmation, that artificial air is the more hurtful to animals, the freer it is from common air.

A shrew-mouse, snail, and flies, in artificial air of paste.

(60.) *June* 30. I included paste in two receivers, and then exhausted the air.

July 4. I put a shrew-mouse into one of the receivers, filled with artificial air, where he suffered vehement convulsions, and in one minute, died. I presently took him out, and exposed him to the common air; but no power of motion could be thereby recover'd.

Then I took the other receiver, and, putting a snail into it, with some wonder observed, that he continued to move very strongly, for a whole quarter of an hour; but, afterwards, his motion was slower, till in about another quarter of an hour, he lay still, as if he were dead; but then, being taken out of the receiver, and exposed to the air, he soon grew well.

I put flies into the same receiver; but now it had admitted too great a quantity of external air, for the flies received no hurt.

Hence we gather, that artificial air kills animals by some venomous quality, and not only by the defect of common air; for the snails liv'd longer *in vacuo*.

A frog in air produced from cherries.

(61) *July* 5. 1677. I took a receiver filled with air produced from cherries, and transmitted it out of that, into another receiver, full of common air, in which a frog was kept: matters were so order'd, that the water gave place only to the artificial air entring in; and the water itself flowed out. And thus the frog, being included in pure artificial air, for a quarter of an hour, and more, suffer'd convulsions, and, at last, lay still without motion; yet, being afterwards taken out, and exposed to the open air, she grew quickly well.

Hence it seems probable, that air produced from cherries, is less hurtful to frogs, than that produced from paste.

Flies, and a shrew-mouse, in artificial air of goos-berries.

(62.) *July* 9. 1677. I put goosberries into three exhausted receivers.

July

July 20. I found one of them sever'd from its cover, by the force of the produced air: I cast a fly into it, which died instantly; a second fly being, likewise, cast into the receiver, presently died; a third put into the same, seemed, for a little while, to be convulsive; but less than a fourth fly, that I included therein; which yet, in one quarter of a minute, lay moveless. Afterwards, I dispelled the artificial air out of the receiver, by blowing, and, in a little time, the flies grew well.

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July 24. I took another receiver, filled with air produced from goosberries; and putting a shrew-mouse into it, found that he died there in half a minute.

Probably, therefore, the air produced from fruit, is less hurtful to animals, than that produced from minerals: for, on the 20th day of *July*, I found that a mouse liv'd not above a quarter of a minute in air produced from gun-powder.

(63.) *July* 5. 1677. I included paste in four exhausted receivers.

A shrew-mouse, a bird, and an adder, in artificial air of paste.

July 6. One of them, being filled with factitious air, was forced from its cover, which I again stopped; yet not so soon, but some common air might mix with the artificial: I put a shrew-mouse into it, which was presently highly convulsed, and, after one minute and an half, remained moveless; and, being presently taken out, he seemed to have some convulsive motions, and died.

July 7. I took a second receiver, filled with artificial air; and having included a little bird therein, suddenly stopped it: she presently fell into convulsions, and, within about a quarter of a minute, died: I took her out, but 'twas too late, for she never stirred after.

I blew out the artificial air from the receiver, and then, another bird of the same kind, being put in, was very well, though she staid there four minutes.

July 9. I took a third receiver full of artificial air, and put that bird into it, which, in the last experiment, had continued well, and yet seemed to be lively and sound; before she had been there a full quarter of a minute, she lay without motion, and being presently taken out, there appeared no sign of life in her.

In the afternoon I put an adder into my fourth receiver, and, within two minutes, he began to be sick, to gape and pant; yet he was not wholly deprived of motion, till after twenty-four minutes. Then, in six minutes more, I took him out of the receiver, motionless as he was, and exposed him to the free air, but he did not recover.

July 10. The adder remained in the same state, and gave no signs of recovery.

(64.) *July* 12. 1678. I put a bird into a receiver full of air produced from raisins of the sun; she died in a quarter of a minute; and tho' I took her out presently, yet she never stirred more.

A bird and a shrew-mouse, in artificial air of raisins.

July 18. I, likewise, put a shrew-mouse into a receiver full of air, produced from raisins of the sun; but a thread, left on the edge of the receiver, hinder'd me from stopping it close; yet the mouse presently began

 gan to be very ill, and, after two minutes, he lay, as it were, without any motion; yet, being taken out, he was well again in two or three minutes time.

Shrew-mice included in common air. (65.) *October* 1. 1678. About ten in the morning, I included a ſhrew-mouſe with common air, in a receiver, fortified againſt the external air; about eleven, the mouſe could hardly breathe: I threw another ſtrong luſty mouſe into the ſame receiver, and preſently put on the cover; but the firſt mouſe, having conſumed ſome of the air, the external air was forcibly impelled into the receiver, and ſo diſpelled a great part of the air ſtagnant there: upon which, the firſt mouſe ſeemed to be much better; neither did he die much ſooner than the other, but both of them died about noon. About four in the afternoon, I put another ſtrong mouſe into the ſame receiver; and, leſt the external air might again expel the included air, I put him in very leiſurely: this third mouſe lived not three minutes entire.

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

Snails in factitious air of paſte. (66.) *April* 28. In the morning, I put ſo great a quantity of paſte into an exhauſted receiver, that, in the afternoon, I found the receiver full of factitious air; whereupon I put a ſnail into it, which preſently frothed very much, and often expanded and contracted itſelf; but, in four minutes, he ceas'd to move: yet I took him not out, till he had ſtaid in the receiver for a quarter of an hour; and then, being releas'd, he ſeem'd as if quite dead: for, tho' he were prick'd with a pin, yet he diſcover'd no ſign of life; tho', after another quarter of an hour, being prick'd, in the ſame manner, he mov'd a little.

I blew out the factitious air, and put in another ſnail: he remain'd very well in the receiver, and did not froth at all.

Hence we have a confirmation, that factitious air is a greater enemy to animals, than a *Vacuum*.

Snails in the factitious air of peaſe. (67.) *June* 22. 1678. In the morning, I put green peaſe into an exhauſted receiver; and, towards evening, the mercury had almoſt attain'd the height of ten inches.

June 23. The height of the mercury was almoſt thirty inches.

June 24. The mercury did not yet exceed thirty inches in height. The cover no longer ſtuck to the receiver; yet nothing, hitherto, had eſcaped.

June 26. I included the ſame peaſe in the ſame evacuated receiver.

July 29. When I now found that the receiver was fill'd with factitious air, I thruſt a ſnail into it, which froth'd much, and very often ſhot out and contracted his horns; but, in ſix minutes time, he lay ſtill, as if he had been dead, and continued thus for two or three minutes; then the receiver being open'd, and the ſnail taken out, mov'd himſelf a little, if he were pricked. Whence it ſeems to follow, that air produced from peaſe is leſs prejudicial to ſnails, than air from paſte. I blew new air into the receiver, and a ſnail then put into it, did very well.

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In this experiment it seems observable, that pease quickly produce air *in vacuo*; but, that in the usual compressure of air, they generate little. PNEUMATICS.

(68.) *June* 22. 1676. I put a butter-fly into an exhausted receiver, and it was almost three hours before he was wholly deprived of motion; at length, perceiving him to lie unmoved, I let the air into the receiver, and, presently, the butter-fly moved. Then I bound him, by one of his horns, with a thread, and suspended him in the receiver; when, he was carried very freely from one part of it to the other, by the motion of his wings; but, after the air was extracted, the clapping of his wings was in vain, for he could not, in the least, move the thread from its perpendicular posture. *Animals* in vacuo, *and first a butter-fly.*

(69.) *July* 12. 1676. Yesterday I put two flies into a receiver, in which I left $\frac{1}{4}$ of air, (*i.e.*) as much as would sustain ten inches of mercury; the biggest of the flies seemed to die presently; but the other, which was a small-bodied one, lived almost twenty-four hours. *Flies in a receiver partially evacuated.*

When both the flies lay, as if they were dead, I suffer'd some air to enter in; the mercury was fifteen inches high, when the lesser fly began to move her feet, but the other continued still without motion.

Hence it appears, that air, highly rarified, may serve for insects to breathe in; and that it doth not kill them so soon as artificial air.

(70.) *May* 1. I put two snails into an exhausted receiver, and, for a whole hour, they seemed to be well enough, and crept up to the top of the vessel; but, in two hours time, they fell down from thence, and lay without motion. *Snails* in vacuo.

After they had remained *in vacuo* for six hours, I took them out, and, within half an hour, they began to move a little. During the time they were included, they produced near as much air as sustain'd the mercury at the height of a quarter of an inch.

These snails liv'd longer *in vacuo*, than did others included in artificial air.

(71.) *August* 12. 1676. I put the eggs of flies into an exhausted receiver, to try if they would there produce worms. *Flies eggs* in vacuo.

Aug. 14. Worms were formed, but the air had crept into the receiver, so as to sustain fifteen inches of mercury.

Hence it appears, that insects may be produced, and live, if not *in vacuo*, yet, at least, in air very highly rarified.

August 16. 1677. I put flies eggs into an exhausted receiver.

No worms being produced, I admitted the air into the receiver, and left all things in the same posture, to try whether the eggs had lost their faculty of producing worms.

Sept. 9. The eggs produced nothing.

This experiment seems to shew, that insects may be generated, and live in air highly rarified; but not at all *in vacuo*.

(72.) *March* 17. 1677. I put two equal quantities of frog-spawn, into two glass vessels of equal bigness; I left the one in an exhausted receiver, exposed to the sun; but the other, being in a receiver full of common air, *Frog-spawn included* in vacuo, *and in common air.*

PNEUMATICS. I fortified against the accefs of the external air. The frog-fpawn *in vacuo*, all fwell'd into bubbles.

May 2. No frogs were produced in either receiver; and the fpawn, kept *in vacuo*, remain'd ftill full of bubbles: but, about three days ago, all the bubbles vanifh'd, and the fpawn was changed to a green liquor.

July 2. The receivers remained in a window, expofed to the noon-day fun; fo that fome water, mix'd with the frog-fpawn *in vacuo*, and the very fpawn itfelf was elevated into vapours; and afterwards, fticking to the fides of the receiver, out of its own veffel, was there condenfed: but the veffel, kept in the common air, ftill contained all its water, together with the fpawn.

A frog in vacuo. (73.) *June* 15. I fhut a frog in an exhaufted receiver, at about feven of the clock in the evening, and about nine the frog died.

June 16. I repeated the experiment, and again perceived, that the dead frog, in two hours fpace, had produced fome air, rather than confumed it.

June 18. The frog, hitherto left *in vacuo*, was fwollen very much; but the air, now entring, made her far more flaccid and lank than before.

Hence it appears, that a receiver, void of artificial air, is lefs hurtful to fuch kind of animals.

Fly-blowings in vacuo. (74.) *Auguft* 3. 1678. I put fly-blowings, upon flefh, into an exhaufted receiver.

Aug. 12. No worms were generated.

Aug. 15. Perceiving no change in the eggs, I open'd the receiver, to try whether they would generate in the free air.

Sept. 15. Nothing was produced from them.

Hence we fee, that animals, which may be generated, and live in highly rarified air, are killed *in vacuo*.

Vinegar-eels in vacuo. (75.) *Aug.* 22. 1678. I included vinegar full of fmall eels, or vinegar worms, in an exhaufted receiver.

Aug. 29. The worms ftill moved, but were fewer than at firft.

September 6. Yefterday fome of the worms ftill moved, but now I could not fee one; and ufing a microfcope, I found them all dead: but, in the vinegar, which I had left in the open air, the eels had as brisk a motion, as at the beginning.

Hence it appears, that very minute animals are alfo affected by the prefence and abfence of the air.

Fire in comprefsed air, and firft perfumed cones, included, and fired, in condenfed and common air. (76.) *May* 14. I took a perfumed cone, which being once kindled in the free air, will, by degrees, wholly confume; and put it into a receiver firmly ftopp'd with a fcrew: then I intruded air into it, till the mercury rofe one hundred and twenty inches above its wonted height; when, by a burning-glafs, I kindled the cone, which prefently darkned all its receiver with fmoke, and, after fome time, feven eighths of an inch thereof, in length, were reduced to afhes; but taking out the cone, and blowing away the afhes, I found only the fuperficies thereof confumed; the inner parts remaining entire.

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I included another cone of the ſame ſort in a much greater receiver, but did not compreſs the air therein: the cone, fired by the ſame burning-glaſs, was not taken out, till all the fumes abated and fell down; yet, much leſs of this cone was burnt, than of the former.

(77.) *May* 19. I weigh'd a perfumed cone exactly, and then firmly included it in a receiver with common air, and kindled it by means of a burning-glaſs; when the fumes were condenſed, I took the cone out of the receiver, and found, it had loſt of its weight, almoſt one grain.

Afterwards, the ſame cone, obſerving the ſame circumſtances, was again included and kindled; but firſt I had intruded as much air into its receiver as ſuſtain'd ninety inches of mercury; and, by means of a pair of ſcales, found the loſs of weight, now, to be four times more than before.

Hence it ſeems to follow, that the conſumption of matter by fire is greater in proportion to the quantity of air contain'd in the receiver.

(78.) *May*. 17. 1677. I included a perfumed cone in a receiver firmly ſtopp'd by the help of a ſcrew; and the air compreſſed to ſuſtain ſixty inches of mercury above its uſual height, I fir'd it with a burning-glaſs; the cone being afterwards taken out, had loſt three grains and an half in weight.

I repeated the ſame experiment in air, ſo compreſſed, that the mercury reached one hundred and twenty inches higher than uſual; then the cone was ſeven grains and three quarters lighter; and ſo, tho' the quantity of the air was not double, yet the conſumption of the matter by fire, was more than twice as much as in the former experiment.

May 17. I made the ſame experiment in air, compreſſed to ſuſtain ninety-ſeven inches of mercury; and, then, the loſs of weight ſeem'd to be ſix grains.

Hereby we are taught, that the matter is the more conſumed by fire, as the compreſſure of the air in the receiver is the greater; or rather, that the conſumption is made in a proportion greater than that of the compreſſure.

May 18. 1677. I intruded a perfumed cone, as before, in a receiver ſeven times larger than that uſed in the former experiments; and crowded no air at all into it. The cone, kindled there, loſt three grains and a quarter of its weight; whereas, in the ſame quantity of air, if reduced to a fifth part of its ſpace, it would have loſt ten grains.

Hence it ſeems to follow, that the ſame quantity of air, reduced to leſs than its accuſtom'd ſpace, cauſeth a greater conſumption, than in its natural ſtate.

(79.) *May* 19. 1677. I repeated the laſt experiment, in the ſame receiver, cloſely ſtopp'd with a ſcrew, that nothing might get out or in. The cone loſt but one grain and a quarter of its weight; whence I ſuſpect, that it was not well kindled.

May 21. I made the ſame experiment, in the ſame manner; and this day the cone was lighter by four grains; whence I more certainly collected, that it was not well ſet on fire in the preceding experiment.

PNEUMATICS. *May* 23. I repeated the ſame experiment twice, but ſuſpect, that the cone was not well kindled; ſince, at one time, it loſt, only three quarters, and at another, one grain of its weight.

May 24. I made the ſame experiment again; and this day alſo the loſs of weight was found only one grain and a quarter. Then I open'd the receiver, and having wiped and cleans'd away the ſoot, repeated the experiment; when the cone took fire very well; for the loſs of its weight amounted to ſix grains and an half.

I tried the ſame experiment again, in an uncleans'd receiver, and the cone loſt only three grains.

May 25. I made the ſame experiment in a receiver well waſh'd, and the cone was lighter by ſix grains and an half.

I made the ſame experiment, in the like manner, in a well cleans'd receiver, and the cone loſt ſeven grains and an half.

I made the ſame experiment again, in an unwaſh'd receiver, and then I could not ſufficiently kindle the cone.

May 26. I made the ſame experiment in an unwaſh'd receiver, about the middle of the day; the ſun being clear, and bright; and remov'd not the burning-glaſs for a long time, ſo that it took fire very well, and became eight grains lighter.

Hence it is manifeſt, that the quantity of a cone to be conſumed in the ſame quantity of air is not fix'd and certain, but ſometimes greater, ſometimes leſs, as the cone ſhall be more or leſs kindled. Beſides, the imperfect mixture of the matter may cauſe ſome difference; yet it ſeems certian, that fire is more eaſily kindled in compreſſed air, than in common; and the conſumption will be the greater in a certain quantity of air, if that air be reduc'd into a narrower ſpace, than if it poſſeſs'd no more than uſual.

(80.) *May* 22. I put a perfumed cone into a receiver made for compreſſing the air; and intruding the air till the mercury reſted thirty inches above its uſual height, I kindled the cone, and found its weight to be abated one grain and three quaters.

May 23. I made the ſame experiment again, after the ſame manner, and with the ſame ſucceſs.

I repeated the ſame experiment, but the cone did not kindle well. Whence we have a confirmation, that fire is more eaſily kindled in air much compreſſed, than in common air, or that which is but a little condenſed.

I repeated the ſame experiment, and after I had remov'd the burning-glaſs, whilſt I was intent to ſee, whether the cone would proceed to be conſumed, the receiver brake into an hundred pieces, ſome of which, ſtruck my head and wounded it: which I mention, that no man may be confident his glaſs will not break, whilſt he is about ſuch experiments, becauſe he has found, that at other times it reſiſted a greater preſſure. For this very glaſs, had contain'd air four times more compreſs'd. It had alſo reſiſted air, ſuſtaining one hundred and ninety-eight inches of mercury above its wonted height; yet, now it was broken by a preſſure, more than

ſix

six times less; and, therefore, while a man looks into such receivers, his head should be guarded. *Pneumatics.*

(81.) *June* 4. 1676. I burnt paper, besmeared with sulphur *in vacuo*, and found, that it prodced some air; which was not at all diminish'd for two days. *Fire made use of to produce air, and first paper, besprinkled with sulphur burnt in vacuo.*

This air must be ascribed to the paper, for none is produc'd out of sulphur alone.

(82.) *June* 15. I burnt harts-horn *in vacuo*, and found, that the fumes, issuing therefrom, contain'd some air. *Harts-horn burnt in vacuo.*

June 17. On these two last days, I repeated the same experiment, and always observ'd, that air produced from harts-horn, was soon, in part, destroy'd; but that, which preserved its elasticity for an hour after the burning-glass was remov'd, seem'd, afterwards, not to lose it at all.

June 19. I took the harts-horn out of the receiver, and found no volatile salt, but only a fetid oil to be produc'd therefrom.

(83.) *June* 21. I burnt amber *in vacuo*, and, at first, could not find that the fumes ascended above the height of one inch; yet, in a receiver full of air, they would be carried up to the top, and from thence return downwards; yet, afterwards, even in the *vacuum* itself, the fumes reached almost to the top of the receiver, but the mercury varied not at all in its gage. *Amber burnt in vacuo.*

June 22. This night, a great deal of that water, in which I had immersed the receiver, found a passage into it, tho' the cover was so well fitted to the aperture, that I never perceiv'd any water to get in betwixt them, before. Hence a suspicion arose, that some volatile salt had, probably attracted the aqueous parts, by reason of the congruity betwixt them.

July 8. I still kept the receiver immerged in water, but no more water entered in; as if, the salts being washed away, the external water, destitute of assistance, could no longer insinuate.

(84.) *Jan.* 18. 1677. I put two drams of camphire into an exhausted receiver; and the juncture of the cover, with the receiver, being fortified against external air, I put the camphire on a digesting furnace. *Camphire sublimed in vacuo.*

Jan. 19. The camphire sublimed into flowers, but no air was produced.

(85.) *May* 24. 1676. I included *Sulphur vivum* in an exhausted receiver, and melted it by the help of a burning-glass; but found, that the fumes, produced therefrom, contain'd no air, because the mercury ascended to the aperture of its gage, as is usual, while the receiver is evacuating; yet, when that was cool'd, the mercury return'd to its former height: and, therefore, that change was, probably, owing to the air included in the seal'd leg of the gage, being rarified, and driving the mercury into the other part. *Sulphur vivum fused in vacuo.*

(86.) *July* 19. Having included paste, nine days ago, *in vacuo*, and perceiving that it now contain'd no more air, I endeavour'd to fire it with a burning-glass. The subsiding fumes had tinged the superficies of the paste, of a curious yellow; and, I conjectur'd, that some air was produced, because the receiver, which, before, was closely join'd to its cover, might now, with ease, be pluck'd therefrom. *Paste expos'd to the rays of a burning-glass in vacuo.*

(87.)

PNEUMATICS.

The production of air from grapes in vacuo.

(87.) *September* 9. 1676. I exhaufted the air out of a receiver, half full of dried grapes, and fortified it againft the external air.

Sept. 10. In twenty-four hours time, the height of the mercury was $\frac{1}{8}$.

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

14.	The afcent of the mercury was	$\frac{3}{8}$
17.		$\frac{3}{8}$
22.		$\frac{5}{8}$

27. The afcent was $\frac{5}{8}$. The height three inches.

From figs.

October 11. The height of the mercury was now about fix inches.

September 9. 1676. I put dried figs into a receiver, and fill'd about half of it with them; then I extracted the air, till the mercury refted at the height of three inches.

Sept. 10. No air was produced.

Sept. 17. Perceiving no air to iffue out of the figs, I open'd the receiver.

Hence we learn, that dried fruits, put into an exhaufted receiver, produce very little air with regularity.

From pears and apricocks.

(88.) *Auguft* 5. 1676. I included pears and apricocks *in vacuo.*

Aug. 6. In eighteen hours time, the mercury rofe two inches; in ten hours more, it reach'd to three.

Aug. 7.	The height of it was	5
8.		$6\frac{1}{2}$

9. In fourteen hours fpace, the mercury mounted three quarters. Its height was feven and a quarter.

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

Aug. 29. The height of the mercury was forty-one.

Sept. 1. The height of it was forty-two and a half.

4. The height of it was forty-four.

7. The three laft days, being hotter than the foregoing, the afcent of the mercury was two and a quarter; its height, forty-fix and a quarter.

Sept. 10. The height of the mercury was forty-feven and a half.

13. The mercury was depreffed; its height only forty-four inches.

23. The mercury, by degrees, mounted again to forty-eight inches.

27. The height of the mercury was fifty and a half.

Nov. 5. The mercury afcended, gradually, to fifty-two and a half.

Nov. 28. The apricocks were reduced to water; the skin being fever'd from the pulp, yet no more air produced.

Jan. 10. 1677. Whilft it froze very hard, the mercury rofe to fifty-feven inches; but, when it thaw'd, it funk to twenty-three. Whether the ftrength of the froft open'd fome way for the air to get out, I know not.

March

March 3. The mercury could aſcend no higher, becauſe the air was got out. This day I found the receiver tumbled on the ground; and the apricocks, when the froſt broke, were putrefied, and had loſt their colour. PNEUMATICS.

Hence it ſeems to follow, that apricocks produce air almoſt as eaſily in their wonted preſſure, as *in vacuo*.

(89.) June 22. *1676*. I put four cherries into two exhauſted receivers, and, proceeded with both alike, except that in the one, the cherries were whole, in the other, cut aſunder; in two hours, the whole cherries had impell'd the mercury into the gage, to the height of ten lines, and the cut ones to about twenty. *From cherries.*

June 21. In twenty-four hours, the mercury, in the receiver containing the whole cherries, roſe to the height of three inches; but, in the other, the gage was ſpoil'd.

June 26. The whole cherries had not yet produced ſo much air, as to ſuſtain fifteen inches of mercury; but the cut cherries had wholly fill'd their receiver with air.

July 9. The receiver of the whole cherries was removed from its cover; I eat one of them, which taſted pleaſant enough. I included the reſt again *in vacuo*; many of them were broke, and, in one hour's time, they impell'd the mercury to the height of about two inches.

July 10. During theſe laſt twenty-four hours, the mercury aſcended not: whether the gage was damaged, I am not certain.

July 15. I found the cover ſever'd from the receiver; whence it was clear, that the gage was hurt.

Hence it appears, that ſome cut fruit, ſooner produce their air, than what is whole.

(90.) June 9. *1676*. I put ſome cherries, that were not acid, into an exhauſted receiver; and, within an hour, found as much air produced from them, as ſuſtain'd a quarter of an inch of mercury.

June 10. In eighteen hours, the mercury roſe to eleven inches.

June 11. The fruits produced leſs air, gradually; ſo that, this day, towards the evening, the mercury came not up to fifteen inches.

June 12. The mercury was a little higher than fifteen inches.

13. The height of the mercury was twenty-two inches.

16. The mercury, yet, came not up to thirty.

18. Perceiving no more air to be produced, I open'd the receiver.

Such a ſmall production of air ſeems very remarkable, becauſe I had found fruit, of the ſame kind, in *France*, to fill their receivers in two days time. Probably, fruits of the ſame kind, in ſeveral countries, differ much amongſt themſelves.

(91.) June 12. *1676*. I put cabbages, cut in pieces, into an exhauſted receiver, with a mercurial gage; and, in an hour's time, the mercury aſcended one line. *From cabbages.*

June 13. The mercury was now come almoſt to the height of ten inches.

17. It was come almoſt to the top of its gage; and, the receiver being open'd, I found the cabbages little alter'd.

June

PNEUMATICS. *June* 19. Being left for two days in the open air, they were wholly corrupted, and blackiſh. I put them again *in vacuo*, to try whether the putrefaction begun, would promote, or retard the production of air.

June 19. The mercury, in half an hour, ran up half an inch.

22. For three whole days, the mercury got higher, only by ten lines. Its height was one, and a third of an inch.

June 23. Finding the cabbages produce no more air, I took them out of the receiver; their ſmell was very bad.

Hence a ſuſpicion aroſe, that bodies, when they putrefy, have produced almoſt all their air.

From oranges. (92.) *May* 29. 1676. I took pieces of orange, weighing four ounces, and put them into a receiver, capable of holding ten ounces of water; and exhauſted the air.

June 10. The receiver was remov'd from its cover, by the force of the air produced; ſo that I took out the oranges, and preſently put them into another exhauſted receiver, capable of containing eight ounces of water; when, the mercury, within half an hour, was elevated to the height of half an inch.

June 13. This ſudden aſcent of the mercury was not durable; for it, yet came not to the height of two inches.

June 16. The mercury, during the laſt twenty-four hours, aſcended about three lines.

June 21. The mercury, the laſt twenty-four hours, did not aſcend the ſpace of one line.

July 18. I perceiv'd no more alteration in the height of the mercury, but ſome mouldineſs appear'd; tho', I am certain, that no air, from without, had enter'd the receiver.

From a tulip. (93.) *April* 27. 1676. I put a tulip into an exhauſted receiver, with a mercurial gage; but, before it was fortified againſt the external air, enough got in to ſuſtain two inches of mercury.

May 2. The tulip which, at firſt, appear'd ſtriped with various colours, was now wholly changed into a dark red, become moiſt, and produced very little air.

Half a lemmon. (94.) *April* 22. 1676. I put half of a lemmon into an exhauſted receiver, with a mercurial gage, ſo ſhort, that the mercury could not aſcend three inches.

April 24. In two days ſpace, the mercury came to the height of an inch and a half.

April 25. The mercury was now two inches high.

April 27. Yeſterday the mercury aſcended four lines; but, this day, only one.

April 29. During the two laſt days, the mercury mounted higher by one line.

May 3. In four days ſpace, the mercury aſcended one line, and a little more.

May 3. 1677. The mercury came to the top of the gage, yet no air got out; but the lemmon was a little alter'd.

Jan.

Jan. 1.1678. Hitherto no air eſcaped out of the receiver; but the lemmon had contracted a yellow colour, and a moiſture. PNEUMATICS.

(95.) *March* 16. 1677. I put two apples, of the ſame ſort, into two exhauſted receivers; one of the apples having begun to putrefy, but the other was only bruis'd by a few blows. *Apples.*

May 15. 1677. Hitherto they ſeem'd in very good caſe; but now the apple, which was bruis'd, appear'd wholly rotten, and the receiver was forc'd from its cover: the other apple remain'd without change.

Auguſt 20. 1677. The apple, which before began to be rotten, ſuffer'd no farther alteration; but, finding that the receiver was now parted from its cover, and fearing leſt the apple would be ſpeedily putrefied, I took it out: its taſte was grateful, inclining to acid, as if it had been fermented; but the pulp ſomewhat reſembled meal in conſiſtence.

Hereby it ſeems confirm'd, that fruits have produced the greateſt part of their air, when putrefaction begins in them; ſince the putrid apple did not fill its receiver, but in a much longer time than the other.

(96.) *May* 17. 1676. I pour'd two equal quantities of milk into two glaſs-receivers, of equal bigneſs; the one I left in the free air, and the other I included in an exhauſted veſſel, with a mercurial gage. *Milk.*

May 18. The cream floated on the top of the milk, left in the free air; but that *in vacuo*, was only cover'd with bubbles, and the gage not alter'd at all.

May 19. The bubbles gradually ſwell'd; and the mercury, in the gage, was a little higher.

May 20. The bubbles, *in vacuo*, ſwell'd yet more, and that milk ſeem'd curdled; but the other, in the free air, was, manifeſtly, curdled. The mercury, *in vacuo*, came almoſt to the top of its gage.

May 22. The milk, *in vacuo*, proceeded to generate more air; and now it evidently appear'd to be curdled. Whence, it is manifeſt, that the coagulation of milk, when the air is taken away, is retarded. Almoſt all the bubbles were now broke.

June 20. The milk, *in vacuo*, was no longer cover'd with bubbles, and remain'd ſtill coagulated in the ſame ſtate. But the milk, in the free air, became very fetid, and was full of worms. When it was put on the engine, and the air extracted, it emitted many very large bubbles, for a long time; and the worms mov'd very vehemently, but not one of them died in four hours time.

May 19. 1677. Three or four months ago, ſome whey, *in vacuo*, was pour'd out of a veſſel into a receiver, and it ſeem'd clear and limpid, like water; yet there was whey enough left in the veſſel, to ſeparate the butyrous from the caſeous part, at a ſufficient diſtance. *Whey.*

This day the milk, ſtagnant in the receiver, ſeem'd to have got out of it; ſo that it is clear, the air, in the receiver, had a greater force than the external air; for the cover, alſo, was ſeparated from the receiver. Towards night, I took that milk out of the receiver, and found it to be acid, both in ſmell and taſte, yet it was not ungrateful to the palate; but, after

PNEUMATICS. a ſhort time, the whey, which hitherto had remained limpid between the caſeous and butyrous part, began to diſappear, and to be blended with the reſt.

May 24. The butyrous part wholly vaniſh'd; tho', as yet, it had ſuffer'd no ſenſible mutation: but the milk began to ſmell ill.

June 1. Our milk had not, yet, contracted the worſt of ſcents; neither had it produced any worms: but it grew dry by degrees, and, this night, the mice eat it up, as, perhaps, they had the butyrous part before.

Here we ſee, that the coagulation of milk, when air is extracted therefrom, is ſomewhat retarded; that the weight of the butter, of whey, and of cheeſe, is not the ſame in the air, as *in vacuo*; for, in the air, they are confuſedly mixed; but, *in vacuo*, one ſwims on the top of the other; that the putrefaction of milk, when air is extracted, is hinder'd, or very much retarded; and, laſtly, that milk, by continuing long *in vacuo*, is made unfit to generate worms, even in common air.

Urine. (97.) *September* 5. 1677. I took the ſame receiver, and the ſame veſſel, uſed before to preſerve milk in *vacuo*, and included urine therein, as I had done milk before. The quantity of urine was about three ounces, and three drams, and the receiver capable of holding ten ounces of water.

Sept. 7. The mercury reach'd to the height of almoſt two inches.

Sept. 8. The mercury was ſomewhat higher than yeſterday.

December 5. The mercury aſcended not above three inches; and, for the whole month paſt, was not changed. The urine ſeem'd not to be at all alter'd.

Decemb. 6. I ſet other urine under a receiver, not defended againſt the external air.

Decemb. 16. The urine, *in vacuo*, ſtill kept unalter'd; but the other, in ten days time, ſeem'd turbid, and to have contracted ſome mouldineſs on its ſuperficies.

This experiment, compar'd with the former, makes it probable, that urine contains leſs air than milk.

But the power of the air to corrupt urine, ſeems very obſervable.

Paſte. (98.) *May* 19. I took paſte, very much diluted, and without leaven, and putting it into a glaſs-veſſel, included it in an exhauſted receiver: and tho' the veſſel which contain'd it, were not half full, before all the air was exhauſted; yet the paſte had ſwollen above the brim of the veſſel.

May 20. The paſte continued to ſwell, and was interſpers'd with many cavities.

May 22. The paſte was much more tumid than before, and much air was generated therefrom

May 23. In the morning I found the cover ſever'd from the receiver, by the force of the produced air, and ſome of the paſte ſpread above the edges of the receiver; yet its ſwelling was ſomewhat abated. In the afternoon, its ſwelling was much more abated, yet it took up twice more ſpace than before it was put into the receiver. The taſte of it was not acid; and, therefore, I think, that bread, thus made, is very light.

(99.)

PNEUMATICS. *Beef.*

(99.) *July* 20. *1676.* I put a quantity of beef into an exhausted receiver, defended against the external air; and another equal quantity into a receiver, neither exhausted, nor closely stopp'd.

July 21. In thirty hours time, the exhausted receiver was fill'd with air, so that I suspected some air had got in: and, therefore, included the same beef again, and so closed it; there was no fear any external air should enter.

July 22. In fourteen hours time, the mercury rose to the height of fifteen inches.

July 25. For three whole days and more, the beef did not produce air enough to fill one half of the receiver.

July 26. The receiver was sever'd from the cover; and in one hour's time, I perceiv'd the beef, being again included *in vacuo*, had produced air enough to sustain ten inches of mercury.

July 28. I found the receiver again fill'd with air, and re-exhausting it, much air was in a short time again produc'd from the beef.

July 30. The receiver being again fill'd, I again included the beef *in vacuo*, and found, that the air produced from it in one hour, sustain'd ten inches of mercury.

August 1. The receiver being this day fill'd again, the beef stunk abominably, and we threw it away.

Hence it appears, that flesh, whilst it putrefies, produces much more air, than before it putrefies; but 'tis otherwise in fruit.

(100.) *July* 18. *1676.* I put some goosberries, which I had long kept in receivers, to produce air, into one that was exhausted. *Goosberries.*

Within half an hour the mercury ascended to the height of one inch.

In an hour and a half, the mercury mounted another inch.

July 19. In twenty-four hours time, the receiver was almost all filled with air.

July 20. The cover was forced from the receiver, and much juice run out.

July 29. I left the same goosberries in a receiver, not defended against the external air; but this day I included them again *in vacuo*, to try, whether they could produce any more air.

July 30. In sixteen hours time, the goosberries drove up the mercury an inch and a half into the gage.

July 30. *1677.* The goosberries could not wholly fill the receiver; and they always remain'd in the same state; but a while since they had almost lost their red colour, and inclined to white.

From hence it seems to follow, that this fruit, after it has produced all its air, suffers very little alteration; as if that air itself were the cause of corruption.

(101.) *August* 23. I put pears into an exhausted receiver with a mercurial gage; and before the receiver could be well defended against the external air, the mercury was risen one inch and a half. *Pears.*

In two hours time it ascended four inches; its height being almost six.

August 24. The height of the mercury was twelve inches.

August 25. The height thereof was sixteen.

Aug. 26	The height of it was	18	*Aug.* 28	The height of it was	23
27		21	31		30
Sept. 1	The height of it was	32	*Sept.* 4	The height of it was	$44\frac{1}{2}$
2		35	5		$45\frac{1}{2}$
3		$38\frac{1}{2}$	6		50

Sept. 7. The height of it was the same, because some air had escaped; but I prevented that for the future.

Sept. 8	The height of the mercury was	$53\frac{1}{2}$
9		$54\frac{1}{2}$
10		58

Sept. 12. Yesterday the mercury remain'd at the same height; but now it seem'd to be depressed: whence I conjecture, that some air had got out. The height of it was fifty-three and a half.

Sept. 13. I transmitted the air into another receiver: the height of it was thirty-two and a half.

Sept. 16. I perceiv'd that the air had got out; and opening the receiver, found the pears very rotten.

These pears produced their air irregularly, sometimes quicker, sometimes slower.

Dried plumbs.

102.) *Sept.* 17. I put dried plumbs into an evacuated receiver.

Sept. 19. The mercury seem'd to have ascended a little.

Sept. 22. I perceiv'd not, that the height of the mercury was alter'd.

Novemb. 9. When the plumbs produc'd no more air, I open'd the receiver.

By this experiment, 'tis confirm'd, that dried fruit is very unfit to produce air.

And nut-kernels.

(103.) *Sept.* 28. I put fresh nut-kernels, cut to pieces, into an evacuated receiver, with a mercurial gage.

Sept. 29. The mercury ascended a little.

Sept. 30. The height of it was two inches.

Octob. 5. The mercury continu'd to ascend by degrees: the height of it exceeded six inches.

Octob. 15. The height thereof was ten inches.

Octob. 22. The height of it was fifteen.

Nov. 28. The mercury was come to twenty inches, or more; but now the receiver was thrown down and broken, and the nut-kernels scatter'd: they were preserv'd very well, both as to colour and taste.

Hence we may conjecture, that air, without sensible putrefaction, is producible from fruits, even of a hard consistence.

The production of air above its usual pressure, in pease, raisins, and water, in common air.

(104.) *June* 22. I included new pease in a receiver, with a glass full of raisins of the sun bruised, and mixed with water; and did not exhaust the air.

Towards evening, the mercury had mounted to twelve inches; but a great part of that air was produced from the raisins, not from the pease.

June 23. The height of the mercury was forty-nine.

June

PNEUMATICS.

June 24	The height of it was	75	*June* 26	The height of it was	90
25		90	28		100

The peaſe ſweat, as it were, and grew yellow.

June 30. The height of the mercury was one hundred and ten.

July 1. The mercury aſcended not, yet no air eſcaped.

July 4. The height of the mercury was one hundred and twenty-four.

July 7. The height of it was one hundred and forty.

July 10. The height remain'd the ſame, but the liquor, which diſtill'd from the peaſe, got out.

July 12. New liquor was produc'd from the peaſe; but the mercury continu'd at the ſame height.

July 13. The liquor got out of the receiver, and ſome air beſides; whereupon I ſet the ſcrew, and new liquor, being in a ſhort time collected, fortify'd the cover within.

July 15. The receiver was broken in pieces; but the peaſe, being ſofter than ordinary, were eaſily ſtript of their husks, as if they had been parboil'd; but they kept their ordinary taſte.

(105.) *Sept.* 15. 1676. I put unripe plumbs into an exhauſted receiver; *In plumbs in* vacuo. and before the receiver could be guarded againſt the external air, the mercury aſcended an inch.

Sept. 16. In twenty-four hours time, the mercury ran up five inches, and its height was ſix.

Sept. 17. The height of the mercury was eight.

Sept. 18	The height of it was	10	*Sept.* 23	The height of it was	18
19		12	24		19
20		14	26		23
22		18	28		26

Octob. 1. The height of the mercury was thirty.

Octob. 4. The height of it was thirty-one, the weather ſomewhat cold.

Octob. 5	The height of it was	32	*Octob.* 9	The height was $33\frac{1}{2}$
7		33	11	

Octob. 15. For theſe two laſt days, the cold being abated, the mercury aſcended ſwifter; its height was thirty-ſeven.

Octob. 17	The height of it was	38	*Octob.* 29	The height of it was	45
19		$39\frac{1}{2}$	*Nov.* 2		46
22		41	5		47
26		43	20		53

In this experiment, the air ſeems to be produc'd ſometimes regularly, and at others irregularly.

(106.) *July* 6. 1676. I put gooſberries into an exhauſted receiver; but *In Gooſberries* before we could prevent the entrance of the external air, it had impell'd the in vacuo. mercury half an inch; and, afterwards, in half an hour, the air, produced from the gooſberries, impell'd it another half inch.

In ſeven hours time, the mercury aſcended four inches higher; and reſted at five.

July 7. In fourteen hours, the aſcent of the mercury was two inches and a half.

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

July 8. In 14 hours, the aſcent of the mercury was 1 $\frac{1}{2}$.

In 10 hours, the aſcent was 2.

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

In 10 hours its aſcent was 1 $\frac{1}{2}$.

July 10. In 14 hours the aſcent of it was 1 $\frac{1}{2}$.

In 10 hours the aſcent of it was 3.

July 11. In 24 hours, the aſcent of the mercury was 4.

July 12. In 24 hours, the aſcent was 4.

Now the mercury was brought to its wonted preſſure.

July 13. In the morning, I found the cover broken; and becauſe it was faſtned by a ſcrew, to prevent its being ſever'd from the receiver, I ſuſpected this happen'd from the internal air. I ſubſtituted another cover in its ſtead.

July 14, 15, 16, 17, 18. I perceived no change in the height of the mercury, becauſe the cover was not exactly cloſed; and therefore I took out the fruit, and put part into another evacuated receiver, and the reſt I ſtop'd up cloſely with common air, that nothing might get out.

In 4 hours, the mercury aſcended 4 inches.

July 19. In 14 hours, the aſcent of the mercury was 1 $\frac{1}{2}$; but, ſuſpecting the air to have eſcaped, I ſet the ſcrew.

In 9 hours, the aſcent of the mercury was 11 inches.

The cover broke, and the air eſcaped.

This experiment ſeems to prove, that gooſberries contain much air, which, as ſoon as it is freed from the wonted preſſure of the air, more readily breaks out, than when reſtrained by ſome ambient air, till the gooſberries begin to ferment; for then air is produced in a far larger quantity, tho' the compreſſure be greater.

In paſte in vacuo.

(117.) *July* 8. 1676. I included paſte in an exhauſted receiver; and, before it was guarded againſt the external air, the mercury was come to the height of three inches; the air making an irruption from without: whence the paſte, which was much ſwollen, loſt about the third part of its tumidity.

A little while after, it ſwell'd again; and, within half an hour, the mercury mounted higher by two inches.

In one hour's time, the aſcent of the mercury was two and a half; and the paſte continued to ſwell.

In another hour, the aſcent of the mercury was three inches and a half.

In an hour more, the aſcent of it was four inches and a half; and it reſted at ſixteen.

July 9. In fourteen hours ſpace, the aſcent of it was twenty-one inches, and the height of the mercury thirty-ſeven. I ſuſpected that ſome air had got out. When I ſet the ſcrew, the cover broke; and, upon admiſſion of the external air, the paſte, which always roſe, now abated about two inches of its tumidity; though it was leſs compreſſed than before.

In

In five hours, the aſcent of the mercury was fifteen inches. Pneumatics.

But, when I again endeavour'd to ſet the ſcrew, the cover broke, ſo that the air eſcaped; and the paſte was preſently ſomewhat depreſſed.

In four hours, the aſcent of the mercury was ten inches: the paſte again ſwell'd, as before; but, being willing to ſubſtitute a better ſcrew, I permitted the air to enter; yet the paſte did not now ſubſide, as before.

July 10. This night the paſte roſe again; yet it ſeemed to have produced no air.

In four hours there was no aſcent of the mercury.

In ſeven hours, the aſcent of it was four inches.

July 12. I perceived no aſcent of the mercury.

13. It ſeemed to have aſcended a little.

17. Seeing no more air produced, I took out the paſte, and found it to have a ſouriſh ſmell.

This experiment ſeems to prove, that air may be produced out of paſte, in compreſſed air, as well as *in vacuo.*

But the paſte was twice depreſſed, becauſe the compreſſed air, ſuddenly finding a way to eſcape, was dilated; as happens in ſprings, when carried beyond their point of reſt: but, when that air was immediately repell'd by the external air, the paſte pitch'd, and was depreſs'd.

(108.) *July* 13. 1677 I included ſome horſe-beans *in vacuo*, with water; when, thoſe which were bruiſed, ſeemed to ſwell much; but thoſe which were left whole, ſuffer'd no ſenſible alteration. *In beans in vacuo.*

In two hours ſpace, I ſaw no air produced, tho' the beans continued to ſwell.

July 14. In twenty-four hours, the aſcent of the mercury was ſeven inches.

July 15. In ſixteen hours, the aſcent of the mercury was three inches and a half.

In eight hours, the aſcent of it was one and a half; the height of it twelve.

July 16. In fourteen hours, the aſcent of it was three.

17. In twenty-ſix hours, the aſcent of it was ſix.

18. In twenty-four hours, the aſcent of the mercury was almoſt nine.

19. I ſtopp'd the receiver firmly with a ſcrew, becauſe the air had got out. In nine hours the aſcent was one inch.

20. In twenty-four hours, the aſcent was three and a half.

21. In twenty-four hours, the aſcent was five and a half.

22. In fourteen hours, the aſcent of the mercury was two.

23. In twenty-four hours the aſcent of the mercury was eighteen.

24. In fourteen hours, the aſcent of the mercury was almoſt five. The height of it thirty-five above the wonted preſſure.

25. The receiver could not ſuſtain a greater preſſure. I found the beans of a fetid ſmell, not much unlike that of putrefied fleſh.

Hence it ſeems to follow, that beans contain much air, and that it is produc'd in a moderate preſſure, as well as *in vacuo*; ſometimes more ſuddenly, PNEUMATICS. ly, ſometimes more ſlowly. But, eſpecially, that great inequality, which happen'd *July* 23, is obſervable.

In goosberries in vacuo.

(109.) *July* 23. I included goosberries in an exhauſted receiver, and guarded them very well againſt the external air.

In two hours, the mercury aſcended one inch.

July 24. The height of the mercury was ſeven inches and a half.

July 25	The height of it was	12	*July* 27	The height of it was	20
26		17	28		24 ½

July 29. The height of it was almoſt 30.

30. The height of it was almoſt 31. I tranſmitted ſome air out of this receiver, into another evacuated receiver; and the height of the mercury was 26.

31. The height of the mercury was 35.

Auguſt 1. The height of the mercury was 39. But ſome air had eſcaped; and going to ſtop the receiver cloſe, I ſuffer'd more air to get out.

The height of the mercury was 30.

Aug. 2. The height of the mercury was 39. I tranſmitted ſome air into another receiver.

The height of the mercury was 31.

Aug. 3. The height of the mercury was 39.

4. The height of the mercury was 41.

5. The height of the mercury was 43. I tranſmitted the air into another receiver.

The height of the mercury was thirty inches.

6. The height of the mercury was 43.

7. The height thereof was 47.

8. The height thereof was 48. But the air being tranſmitted into another receiver, the height of it was 36.

9. The height of the mercury was 41, in fourteen hours.

Aug. 10. The height of the mercury was 47; the air being tranſmitted into another receiver, the height of it was 35, in twenty-four hours.

11. The height of the mercury was 38 and a half, in fourteen hours.

12. The height of the mercury was 42, in twenty-four hours. I extracted the air, and the height of the mercury was 26.

13. The height of the mercury was 33, in twenty-four hours.

14		36		17		44	
15	The height of it was	39	24	18	The height was	47	24
16		41 ½		19		50	

I tranſmitted the air into another receiver; and the mercurial gage was ſpoiled. I took out the goosberries, and found they had loſt their colour, and almoſt all their acidity.

From hence we may infer, that goosberries produce their air regularly, unleſs ſomething be extracted out of the receiver; for then they acquire a power to produce new air more ſpeedily.

(110.)

PNEUMATICS. *In grapes* in vacuo.

(110.) *Sept.* 12. I put crude grapes into an exhausted receiver; but before they could be fenced from the external air, as much had got in as sustain'd three inches of mercury.

Sept. 13	The height of it was	5	*Sept.* 17	The height was	19
14		10	19		23
16		17	20		25

Sept. 22. The height of the mercury was 30. I stopped the receiver with a screw.

23. The height of the mercury was about 30 and a half.

24. The height thereof was 32.

Sept. 26	The height of it was	34 $\frac{1}{2}$	*Octob.* 2	The height of it was	39 $\frac{1}{2}$
27		36 $\frac{1}{4}$	4		39 $\frac{1}{2}$
28		36 $\frac{1}{4}$	5		40 $\frac{1}{2}$
29		37 $\frac{1}{4}$	7		41 $\frac{1}{2}$
30		37 $\frac{1}{2}$	9		41 $\frac{1}{2}$

Octob. 15. The height of the mercury was 46. It ascended chiefly on these two last days, when the frost was dissolved.

Nov. 2. The height of the mercury was 54.

5. The height was 58.

Jan. 10. 1677. The mercury was come to the height of 70 inches; yet I perceived no sensible change in the mercurial gage, even when the cold was sharpest; tho' the grapes and their juice were turn'd to ice.

September 21. Hitherto the grapes seemed not alter'd; but the mercury had ascended a little, because the air found a passage out. I open'd the receiver, and when the air broke forth, many of the grapes seemed to be wrinkled. The grapes had kept their taste, but it was much more pungent: the juice continued tinged of a curious red colour.

This experiment seems to inform us, that grapes produce not all their air, but in a long tract of time.

In pears in vacuo.

(111.) *August* 10. 1677. I put pears, cut asunder into an exhausted receiver. Towards evening the mercury was risen ten inches.

Aug. 11	The height of it was	20	*Aug.* 15	The height was	55
13		38	16		60
14		48	17		68

The air baing transmitted into another receiver, the height of the mercury remained at 53 and a half.

Aug. 18	The height of it was	61	*Aug.* 20	The height of it was	70
19		64	21		72

The air being transmitted into another receiver, the mercury remained at 61.

Aug. 22	The height of it was	68	*Aug.* 24	The height of it was	79
23		74	25		81

The air being transmitted into another receiver, the height of the mercury was 61.

Aug. 26. The height of the mercury was 56. Some air having got out, I tranſmitted the reſt into another receiver, and the mercury remain'd at 52.

Aug. 27, 28, 29. The height of it was 60, 68, 75. | *Aug.* 30, 31, *Sept.* 1. The height was 83, 88, 93.

Septemb. 2. The height of it was 100.

Sept. 3. The height of it was 89; ſome air having eſcaped, which made me cautious to prevent the like for the future.

Sept. 4. The height of the mercury was 100.

5. The ſame height continued.

7. The ſame height ſtill continued, tho' no air eſcaped.

9. The height of the mercury was 107.

10. The height of the mercury was the ſame.

The air being tranſmitted into another receiver, the mercury reſted at 99.

Sept. 11. The mercury moved not.

13. The height of the mercury was 105.

October 8. I found that the air had got out.

This experiment ſeems to inform us, that pears produce their air by fits.

Miſcellaneous experiments, and firſt, melted lead and tin cooled in vacuo.

(112.) *March* 16. I melted down lead, with a fire, in a braſs veſſel, whoſe diameter was an inch and half; but before the lead was concreted by cold, I put it into a receiver, out of which I ſuddenly exhauſted the air; whence the figure of the lead was concave, and the parts of it the more depreſſed, nearer the center: but lead, congealed in common air, exhibits a convex figure, except in the middle, where there is a little cavity.

I made the ſame experiment on tin, with the ſame ſucceſs; and tho' both metals being fluid, and very hot, had remained long *in vacuo*, yet no bubbles ſeemed to riſe from either; but all other hot liquors ſeem to yield numerous bubbles *in vacuo*.

Salt and water in vacuo.

(113.) *Sept.* 2. I put water, ſaturated with ſalt, *in vacuo*; to try whether it would be there converted into cryſtals, and the ſalt be carried above the ſuperficies of the water, as happens in the free air.

Sept. 15. The water, with the diſſolved ſalt, abiding in the ſame ſtate, I open'd the receiver; and, as no vapours could eſcape, 'tis reaſonable to judge, that the ſalt could not there be converted into cryſtals.

The air of gooſberries in vacuo.

(114.) *Auguſt* 8. 1676. I put air produced from gooſberries, into an evacuated receiver, furniſhed with a mercurial gage.

March 1. $167\frac{6}{7}$. I perceived no change in the height of the mercury, and therefore, open'd the receiver.

The weight of air to that of water.

(115.) *Auguſt* 8. I took a vial, able to hold ſeven ounces, five drams, and three grains of water, and exhauſted the air out of it; and, when, in a balance, it was ſuſpended in equilibrium, with another weight, I

pierced

pierced the bladder which cover'd the orifice, with a needle; and then, the vial being fill'd with air, appear'd heavier by four grains and a half; which latter weight to the former, is as 1 to 814: whence it follows, that water is about, at leaſt, 800 times more ponderous than air of an equal bulk. This day was hot and clear; and ſome air is always left in the receivers after exhauſtion. PNEUMATICS.

(116) *Jan.* 16. 1677. I put *Aqua fortis*, with fixed nitre, into a receiver; and, having exhauſted the air as much as I could, poured one of them on the other, and found much air produced. I marked the height of the mercury in the gage. Aqua fortis, *and fixed nitre* in vacuo.

March 5. Finding the produced air was not deſtroy'd, and that the mercury perſiſted at the ſame height, I open'd the receiver, and found nitre produced *in vacuo* from the mixture.

(117.) *May* 12. 1677. I fill'd a long and very narrow-neck'd vial, with oil, up to the middle of the neck, and put it into a receiver, firmly ſtopp'd by the help of a ſcrew; into which, I afterwards intruded air, till it ſuſtain'd 120 inches of mercury above its wonted height. The oil, in the neck of the vial, appear'd depreſſed about a quarter of an inch; the cauſe whereof I judg'd to be the compreſſure of the air: but, having eaſed the ſcrew, and thereby ſuffer'd the air to break in, and be dilated, the oil did not aſcend at all; ſo that, I ſuppoſe, it was condenſed only by cold. *Oil, water, and ſpirit of wine, in compreſs'd air.*

Auguſt 5. I made the ſame experiment, after the ſame manner, uſing water inſtead of oil; yet could perceive no change of the height of the water in the neck of the glaſs; tho' the heat, being moderate, might have produced a ſenſible effect.

Jan. 14. 1678. Finding, by ſome experiments, that compreſſed air enters into the pores of the water, and pierces even to the bottom, a ſuſpicion might ariſe, that the water was not condenſed by the compreſſed air, becauſe the air entring into the pores, made the preſſure within equal to that from without; I, therefore, filled the aboveſaid glaſs with ſpirit of wine; leaving only the length of three inches in the top of the neck thereof, which was filled with air only. Then applying my hands to the glaſs, the ſpirit of wine, being heated, ſoon filled the whole neck to the top. The glaſs being now inverted into a veſſel of mercury, I removed my hands, when the ſpirit of wine being ſoon cooled, ſuffer'd the mercury to poſſeſs three inches in height. I put the veſſel, and the glaſs, in that poſture into a receiver, and afterwards compreſſed the air therein, till the mercury exceeded its wonted height 90 inches; yet there was no ſenſible condenſation of the ſpirit of wine, nor any aſcent of the mercury: however, it is certain, that no air had crept in, becauſe the mercury hinder'd it; and the receiver being open'd, when the air, that compreſſed from without, was dilated, no bubbles appear'd in the ſpirit of wine.

Here it ſeems worth enquiring, how the ſpirit of wine was ſo ſenſibly condenſed by a moderate cold, and not at all by a great compreſſure of the air.

PNEUMATICS.

Spirit of wine, and oil of turpentine in vacuo.

(118.) *May* 12. 1676. I poured ſpirit of wine into a glaſs veſſel; and added ſome drops of oil of turpentine thereto, which ſwimming upon the ſpirit of wine, began to be there whirl'd about. I put the glaſs veſſel on the pneumatic engine, and cover'd it with a receiver; yet the bubbles did not at all ceaſe to move up and down. Then I pump'd out ſome of the air; when, the bubbles, emerging from the ſpirit of wine, adhered to the drops of oil, and carried them to the ſides of the veſſel, and there detained them; yet two drops, free from ſuch bubbles, proceeded to have a further motion. Afterwards, I wholly exhauſted the receiver, and ſome drops roſe to the top thereof, by the force of the bubbling ſpirit of wine; but the remaining drops continued to be moved a little, and ſoon after reſted. The air being admitted, the drops began again to renew their motion, but it was ſlow, and quickly ceaſed.

I repeated the experiment with ſpirit of wine, and oil of turpentine, purged from air; and no ebullition was then made, nor did any bubble appear: but the drops of the oil of turpentine were moved *in vacuo*, as in the open air.

Hence, it ſeems to follow, that the cauſe of the motion of the drops, is not owing to the diſſolution; for all diſſolutions *in vacuo*, have, hitherto, ſeemed to me, to produce bubbles.

Radiſhes and claret in vacuo.

(119.) *May* 19.1676. Yeſterday I left two radiſhes *in vacuo*, one of them hanging with the root downwards, the other in a contrary poſture; and both cut tranſverſly, reſted over a veſſel, which contain'd red wine. Theſe remaining for a whole night *in vacuo*, ſeem'd well purged of their air. Opening the receiver, I added two other radiſhes to the former, cut after the ſame manner, having firſt taken off their thick skin. Then exhauſting the receiver, I immerg'd the cut part of all the radiſhes, at once, into the ſubjacent wine; upon which, many bubbles ſeem'd to ariſe from them: and more bubbles proceeded from thoſe radiſhes which were purged of air, for a whole night, than from thoſe which had not remained above half an hour *in vacuo*, with their skins off.

Hence bubbles ſeem to be formed of particles of air, ſwimming in water; and becauſe, in the skin there are ſome canals, fit to retain parts of air, the peeled radiſhes afforded no opportunity for the formation of ſo many bubbles.

The liquor aſcended no leſs into thoſe radiſhes which hung with their roots upwards, than into the others.

A ſmall glaſs tube, plunged in water, the infuſion of nephritic wood, and ſpirit of wine, in vacuo.

(120.) *May* 4. 1676. I immerged one end of a ſmall open glaſs tube, into water ſtagnant *in vacuo*, and preſently the water aſcended up into it, as uſual in common air, and to the ſame height; but, ſoon after, many bubbles being formed there, raiſed the water higher, and kept it ſuſpended in three different places, intercepted by many bubbles; and ſeveral other bubbles ſeemed to paſs out from the end immerſed in water.

Then ſealing the other end of the tube hermetically, and making the experiment in common air, the water aſcended not up into the tube at the open end. But, *in vacuo*, it aſcended therein, as if it had been open at both

both ends ; and many bubbles fuddenly formed, feparated the water, contained in the tube, to a great diftance, as before : in the mean time, many other bubbles feemed inceffantly to pafs out from the end of the tube immerfed, tho' they afterwards appear'd lefs frequent. PNEUMATICS.

But the water being fufpended higher in the tube, feemed to contain no bubbles, whilft the end only emitted fo many.

Then I took out that end from the water, and no more bubbles appear'd, tho' it was wholly fill'd with a cylinder of water.

May 5. I repeated the experiment; but before I had immers'd the end of the tube in water, a drop, which ran over from the upper aperture of the receiver, flowed down to the open end of the tube, and penetrated into it to the height of two lines; and no bubble was formed there in a full half hour. I, afterwards, plunged the end of the tube into the water of the veffel, and bubbles foon began to be formed as before ; fome of which fucceeded others within half a minute : but, afterwards, they were lefs frequent. Repeating this experiment, many times, I perceived, that when the water was extracted from the tube, no bubbles appear'd ; but if it were immerged in water, fome would adhere to the end of it, either fooner or later.

May 6. I made the fame experiment, with the infufion of nephritic wood, with a like fuccefs ; excepting that the bubbles emerged, and penetrated the liquor, before they had acquir'd any confiderable bignefs : whence we may conjecture, that this liquor is very thin, and hath no vifcofity to refift a pervading body.

May 10. I repeated the fame experiment with fpirit of wine, mixed with a certain oil, made *per deliquium*, but found nothing new ; only the liquor afcended not fo high into the tube.

Hence the bubbles feem to be formed, at the extremity of the tube, of aerial particles, fwimming in the water ; which finding fome impediment at that end, cannot pafs by, and fo, new ones coming upon them, they fwell into bubbles.

(121.) *July* 18. 1676. Two days ago, I took fome horfe-beans, and included them in an iron tube, clofely ftopped; firft pouring water on the compreffed beans, till the tube feemed wholly full ; to try whether the expanfive force of the beans would break the tube. This day the tube feem'd not to be alter'd, but, the ftopple being loofen'd, fome air broke out, and much water, which was not imbibed by the beans, fell upon the ground : then we heard a noife, as it were, of bubbling water, for above an hour. *Horfe-beans and water included in an iron tube.*

July 25. The tube remain'd in the fame pofture; but now one of the ends of it being unftopp'd, and fome beans taken out, the murmur of the bubbling water was heard as before.

From hence it feems to follow, that beans contain air, which, in a great compreffure, cannot efcape ; but breaks out, if freed from the compreffing force.

(122.)

PNEUMATICS.

Spirit of sal-armoniac, and copper filings in vacuo.

(122.) *March* 4. 1677. I put a glass, half full of spirit of sal armoniac, and copper filings, into a well exhausted receiver, and stopp'd it up: in 15 minutes, the liquor had contracted a blue colour, very much diluted; but, the air being admitted, in three minutes the blue colour appeared vivid and thick. I put the liquor, so tinged, again *in vacuo*, to try whether that colour would, in time, vanish.

April 4. The blue colour almost quite disappear'd, but quickly return'd, upon admission of the air.

A certain oil, and spirit of wine in vacuo.

(123.) *May* 8. I put a certain oil, made *per deliquium*, and spirit of wine, into an exhausted receiver: the spirit always swam on the top; and, lest the spirit should bubble over the edges of the vessel, I extracted the air, by degrees; when, at first, great bubbles arose from the spirit, and but very small ones from the oil; after one hour, the oil afforded large bubbles, which, from being small at the bottom, fill'd, in their ascent, the whole breadth of their vessel: and, after another hour, some bubbles broke out with so great force, that they hit against the top of the receiver.

May 9. I repeated the experiment in a glass somewhat long and narrow, that I might the better perceive the motion of the bubbles; and I saw the bubbles passing out of the oil into the spirit of wine, without any great increase of their quantity: but being distant only one quarter of an inch from the superficies, they were suddenly expanded.

Aqua fortis, *spirit of wine, and iron* in vacuo, *and common air.*

(124.) *May* 3. 1676. I mixed a quantity of *Aqua fortis*, with a larger of spirit of wine; then distributed the mixture equally into three glass vessels, and put three equal pieces of iron into them, to each vessel one. This done, I included one of the three vessels *in vacuo*; and there many great ebullitions were made. In a quarter of an hour I took out the vessel, and found the liquor black and turbid; whilst the other two vessels had their liquor not alter'd in colour; only some black powder appear'd at the bottom.

One of these two vessels I put *in vacuo*, and there arose ebullitions, great indeed, but much less than the former: in one quarter of an hour, I took out the vessel, and found the liquor black, yet less so than the former; but that which was left always in the air, remain'd, in a manner, unchanged.

May 4. In the morning, the liquors in the two vessels, put *in vacuo*, appear'd clear and green.

But that in the open air bubbled more strongly, than it did yesterday, and was of a red colour. I put the three vessels together *in vacuo*, and perceiv'd no remarkable ebullition; only some bubbles appear'd larger in the red liquor, than in the other two.

From hence it seems to follow, that spirit of wine accelerates ebullition *in vacuo*.

Spirit of sal-armoniac and copper filings in artificial air of paste.

(125.) *Jan.* 21. 1678. I had a glass half full of spirit of sal armoniac, and filings of copper, the mouth whereof was so exactly stopp'd, that the blue colour, induced by the external air, now wholly disappear'd. The stopple was made of leather, prepar'd after a particular manner.

This glass I set *in vacuo*, with unfermened paste, that the receiver being full of air, from the paste, I might perforate the leather that stopp'd the glass;

glaſs; and try, whether the contact of the air, generated from the paſte, would alſo communicate a colour to the liquor. PNEUMATICS.

Jan. 22. There was no need to perforate the leather; for I found the liquor already tinged: whence it is probable, that air produc'd from paſte, is endu'd with ſuch minute particles, as to penetrate leather, which is impervious to common air.

Jan. 25. The liquor became almoſt colourleſs; whence it appears, that common air is too thick to penetrate all paſſages, which are pervious to air, produc'd from paſte.

Feb. 2. I put the ſame vial *in vacuo*, but did not cement the receiver to the cover; ſo that the air, gradually entring, in twenty-four hours, fill'd the receiver, as it was leiſurely fill'd with the air produc'd from paſte; yet the liquor ſtill remain'd colourleſs.

Feb. 15. I put the ſame glaſs again *in vacuo*, with ſome quantity of paſte; but, this time, the air produc'd from thence, did not pervade the leather, as it had done before, and the liquor was not at all tinged.

A ſhrew-mouſe in an engine that filtres air thro' water.

(126.) *April* 2. 1678. I put a ſhrew-mouſe into the filtrating engine; and, when I perceiv'd him reduc'd to extremity, I began to ſtir the pump, that the air, might be, as it were, filtred thro' the water. The mouſe, a while after, ſeem'd to be better, yet not wholly reſtor'd; and having been long kept faſting, I am uncertain, whether he died for want of aliment, or of new air.

April 12. I repeated the experiment with a ſmall weakly mouſe, that had been kept a long time without food. And finding the ſame ſucceſs as before, I took out the mouſe before he was dead, but he recover'd not: ſo that more experiments are requir'd, to ſhew the effect of this filtration.

Frog-ſpawn in vacuo, common air, and compreſs'd air.

(127.) *May* 2. 1678. Six weeks ago, I included frog-ſpawn in three receivers, the firſt of which was exhauſted; the ſecond contain'd common air; and into the third, I intruded ſo much air, that the mercury reſted ſixty inches above its uſual height.

In fifteen days, the mercury in the evacuated receiver roſe an inch. The ſpawn in the common air ſeem'd corrupted, and of a blackiſh colour; but that in the compreſſed air, remain'd unalter'd in colour; tho' no frogs were generated.

In a month's time, the ſperm *in vacuo* had not changed its colour, excepting the black round ſpots; but ſeem'd reduc'd into water: the colour of that in the common air was very black, but in the compreſs'd air the ſpawn began to be reddiſh.

As yet, no change was perceiv'd, either in the ſpawn *in vacuo*, or that in the common air; but in the compreſs'd air it appear'd redder.

May 22. The ſperm *in vacuo* was not chang'd; in the compreſs'd air it remain'd red; but in the common air it again became colourleſs.

June 23. The ſperm *in vacuo*, and in common air was not tinged, but in the compreſs'd air it inclin'd to green.

Octob. 15. I took the ſpawn from all the veſſels; that kept *in vacuo* was almoſt exhaled out of its veſſel, and appear'd ſtagnant in the receiver, like clear water: that in the common air remain'd colourleſs; but that in the compreſs'd air ſtill kept its red colour.

PNEUMATICS.

Oranges in receivers, with and without water.

(128.) *May* 9. 1678. Six days ago, I included two pieces of the ſame orange in two receivers, not quite of equal bigneſs; in the greater, there was left ſome quantity of water, ſo that the ſame ſpace remain'd for the air in that, as in the leſs. The orange included with water, tho' it were not touch'd by it, was four times more mouldy, than that kept without water.

And, therefore, in repeating this experiment, I put two pieces of the ſame orange into two receivers; but fill'd the third part of one of them with water, yet ſo, that it did not reach the orange.

June 15. Neither of the pieces had contracted any mouldineſs.

May 16. I repeated the experiment with the ſame ſucceſs; only, neither orange had acquired any mouldineſs in the ſpace of more than a month; tho', in former experiments, all ſuch oranges grew mouldy.

The cauſe of the difference, ſeems to be ſome particular diſpoſition of the air.

Turpentine included in a wind-gun.

(129.) *June* 1. 1678. I put a ſmall glaſs tube, half full of *Venice* turpentine, into our wind-gun; and had ſcarce reduc'd the air to the tenth part of its wonted ſpace, but the leather, ſpread over the elliptic valve, was driven out; ſo that, the air having eſcap'd, I drew the glaſs-tube out of the engine, and found many bubbles formed in the ſuperficies of the turpentine. I, therefore, ſuſpected, that the air had pervaded the turpentine; and that it would have penetrated deeper into it, if they had remain'd longer thus inclos'd together. I plac'd the ſame tube in the ſame gun, and there left it in air reduc'd to about the fifteenth part of its natural ſpace.

June 3. I open'd the engine, and, taking out the tube, found the turpentine almoſt free from bubbles; yet, by degrees, many were formed therein, in the parts remote from the ſuperficies.

June 4. I put new turpentine into the ſame tube, and included it *in vacuo*, that it might be the better purged of air; then I pour'd the water upon it, and ſhut up all in the wind-gun.

June 8. I open'd the engine, and, at firſt ſight, both the water and the turpentine in the tube, ſeem'd to be very free from bubbles; but ſoon after I perceiv'd, that bubbles were form'd in the turpentine, and that they aſcended by degrees: ſome of them ſeem'd to be made, almoſt at the very bottom, about half an inch below the ſuperficies of the turpentine. Whence we may conjecture, that all the water, and ſo great an height of the turpentine, were pervaded by the air, which formed thoſe bubbles.

Spirit of ſal-armoniac, and copper-filings in vacuo.

(130.) *Auguſt* 11. 1678. I included ſpirit of ſal-armoniac, with a mercurial gage, *in vacuo*; and after the ſpirit ceas'd to emit any bubbles, I mix'd copper-filings therewith, which caus'd many bubbles to riſe again; but they were ſo far from producing any air, that they conſum'd what was there before. But the liquor became greeniſh and turbid.

Decemb. 5. The ſpirit was almoſt all exhaled out of the containing veſſel, and, being condens'd in the receiver, remain'd ſtill turbid, by reaſon of much filth, which was included there: but that which was not exhaled out of the veſſel, appear'd clear like water. The mercury, alſo, was wholly

expell'd

expell'd out of the gage. Whence I conjecture, that the air in the receiver, was gradually more consumed. *Pneumatics.*

(131.) *Sept.* 2. 1678. I put two cylinders, one of tin, the other of lead, *in vacuo*; their lowest parts were immersed in mercury; and, at the same time, I immersed two other cylinders, like the former, after the same manner in mercury: but these latter were left in the free air. *Cylinders of tin and lead immersed in mercury, in* vacuo, *and in common air.*

Sept. 6. I open'd the exhausted receiver, and the mercury in the tin cylinder was risen four inches and a half, above the superficies of the stagnant mercury; and cutting the cylinder transversly, in the middle of that height, the amalgam seem'd to have penetrated into the cylinder, about half a line. And cutting the cylinder transversly again, in that part, which was distant only one inch, from the superficies of the stagnant mercury, I found the thickness of the amalgam equal to one line.

In the lead-cylinder, the mercury rose two inches and a half; but, only as far as the superficies; and the very part, immersed in the mercury, was not penetrated by it, to any sensible thickness.

Sept. 7. I took the tin-cylinder left in the air, out of the mercury, in which it was immersed, and found the mercury to have ascended to the height of five inches.

Sept. 10. The same cylinder being left in the mercury, seem'd to be besmeared therewith to the very top, six inches, and more, above the superficies of the stagnant mercury. When the cylinder was transversly cut in several places, the mercury appear'd to have pierc'd the deeper into the tin, the nearer it came to the stagnant mercury; so that in the part adjacent to the mercury, almost the whole diameter of the cylinder, three lines broad, was penetrated thereby.

In the lead-cylinder, the mercury exceeded not the height of three inches and a half; neither had it penetrated to any sensible thickness. Whence it appears, that the weight of the air, contributes little or nothing to the ascent of mercury into metals.

(132.) *Decemb.* 12. 1678. I took a small whiting, and having cut off his head, divided him transversly into five pieces; the first whereof, I included *in vacuo*. The second in common air. The third in air so compress'd, as to sustain mercury fifty inches above its wonted height. These three receivers were closed with screws. The fourth piece was put into a receiver, full of air, produc'd from paste, which was presently stopp'd. The fifth was left in the free air. *A whiting included in* vacuo, *in common air, in air compress'd, in artificial air, and left in the open air.*

Decemb. 15. In the morning, that part of the whiting, which was left in the free air, began to shine; and, towards evening, it gave a more vivid light.

Decemb. 16. In the morning, the whiting left in the free air, ceas'd to shine; but towards evening shone again.

Decemb. 17. This morning, the same part of the whiting shone a little, yet less than yesterday in the evening.

PNEUMATICS.

Decemb. 18. In the morning, there appear'd no light, tho' I long fix'd my eyes upon the receiver in a dark place; but the night coming on, the light appear'd again.

Decemb. 20. Hitherto the ſame part of the whiting left in the air, continued to ſhine; but all the other parts did not yet begin to do ſo.

Decemb. 22. Yeſterday, the light of the whiting, left in the air, had not quite ceas'd, but this day it appear'd no more.

Decemb. 24. The part of the whiting in the free air, entirely gave over ſhining; that included in common air, did, yeſterday, yield a faint light; but this day it ſhone no more.

Decemb. 26. No more light appear'd in that in the common air: but the three other pieces did not begin to ſhine.

Jan. 26. 1679. I perceiv'd no more ſhining in any one of the receivers.

Artificial air deſtroy'd; and firſt that of cherries tranſmitted into a receiver full of common air.

(133.) *Aug.* 3. 1677. I tranſmitted air, produc'd from cherries, into a receiver full of common air, but ſo ſtopp'd with a ſcrew, that the mercury aſcended to twenty-five inches above its uſual height.

Aug. 4. The mercury was depreſs'd about two inches. The height of it, this day, was only twenty-three.

Aug. 6. The height thereof was reduced to twenty.

Aug. 7. The height thereof the ſame.

Aug. 8. The mercury was ſomewhat depreſs'd.

Aug. 10. The height of it was nineteen and a half, above its uſual ſtandard: and perceiving little or no alteration, I open'd the receiver.

Hence we have a confirmation, that air, produc'd from fruits, at the beginning, is in part deſtroy'd; but, that the reſt can very long retain the form of air.

That of ſal-armoniac, and oil of vitriol, in vacuo.

(134.) *May* 26. 1676. I put ſix grains of ſal-armoniac into a receiver, with a ſufficient quantity of oil of vitriol: then, the air being exhauſted, I forc'd down the ſalt into the oil; whereupon, a great ebullition preſently follow'd, and the mercury aſcended in the gage, almoſt to its wonted height; but preſently after it ſunk again, and return'd to its former ſtate.

May 27. I repeated the experiment; the ſalt remaining ten hours *in vacuo*, before it was put into the oil; but the ebullition proceeded as before; yet, the air was produced much more ſlowly, nor could it wholly be deſtroy'd, in ſeven or eight hours time; yet at laſt the mercury deſcended to the very bottom.

May 29. I made the ſame experiment again; leaving the materials for twenty-four hours *in vacuo*: the ebullition ſeem'd much leſs, and the air was produc'd, both in a leſs quantity, and more ſlowly than before. I obſerv'd alſo, that whilſt the materials remain'd *in vacuo*, before their mixture, the mercury came nearer to the open end of the gage, as if ſome air had been either extracted or deſtroy'd.

And of oil of vitriol with a fifth part of common air.

June 8. I put oil of vitriol, alone, into a receiver, in which, I left only a fifth part of common air; to try, whether this oil, without ſal-armoniac, would diminiſh the elaſtic force of the air: but the force of the air was increas'd, and the mercury in one hour's time ſeem'd to have aſcended a

little

little into the gage; tho', afterwards, for twenty-four hours no change happen'd. PNEUMATICS.

This experiment shews, that some artificial air may be destroy'd; but why this destruction happens, sometimes sooner, sometimes later, deserves a further enquiry.

(135.) *July* 10. 1676. I put paste, made two days before, and now grown sourish, into a receiver, and stopp'd it firmly with a screw.

The different celerity wherewith air is produc'd in vacuo, *and in common air, shewn from paste in common air.*

In one hour, the height of the mercury was one inch.

In seven hours, the height of it was six.

July 11. The height of it was eleven.

July 12. The height of the mercury was twenty-four.

July 13. The height thereof was thirty.

July 14. The height of the mercury was sensibly greater.

July 15. The mercury ascended a little. Measuring its height exactly, I found it thirty-eight inches.

July 19. No more air was produc'd from the paste.

July 10. 1676. I put another quantity of the same paste, much less than the former, into an exhausted receiver.

Paste in vacuo.

Tho' the quantity of the paste was less, yet, in one hour's time, the height of the mercury was two inches.

In seven hours, the mercury came almost to the top of the gage; but it was a short one.

July 19. The paste was not able to move the receiver from its cover; tho', at the beginning, it had produc'd a greater quantity of air, than the paste in common air. I endeavour'd to fire it with a burning-glass, and the fumes, elevated therefrom, afterwards falling upon the paste, tinged the superficies thereof, with a pleasant yellow colour: and that air was thus produced, I conjectur'd, because the cover was afterwards easily sever'd from its receiver.

Hence we learn, that air is sometimes generated much more easily *in vacuo*, than in common air.

(136.) *August* 20. 1676. I put paste, kept for 24 hours, int a receiver full of common air; to which I added new air, so that the mercury exceeded its wonted height, four inches, and a half.

In six hours, the mercury gained almost 4 inches; and its height was 8.

Aug. 21. The ascent of the mercury was 4 and $\frac{1}{2}$.

Aug. 22. The ascent of it was about 1.

23. The ascent of it was half an inch.

26. For three whole days, the ascent of the mercury was only half an inch.

27. There was no ascent of it at all.

29. The paste, taken out of the receiver, smelt acid.

August 20. I put another quantity of the same paste into an exhausted receiver, and observ'd the same proportion between the quantity of the paste, and the capacity of the vessel, as in the former experiment.

The mercury presently seemed to have ascended. Its height was two inches

PNEUMATICS.

Aug. 21. The ascent of the mercury was 5.
22. The ascent of it was 3.
23. The ascent of the mercury was 1.
26. For three whole days, the ascent of it was 2.
27. There was no ascent of the mercury.
29. I took out the paste, exhausted of its air, from the receiver.

This experiment farther confirms, that air is, sometimes, more easily produced *in vacuo*, than in common air.

Filberd-kernels in vacuo.

(137.) *Sept.* 4. 1677. I put the kernels of filberds into an exhausted receiver.

Sept 5. The height of the mercury was 5 inches.

Sept. 6	The height of it was	10	*Sept.* 11	The height of it was	18
7		10	12		23
8		12	13		27
9		15	14		29

Sept. 15. The height of it was almost the same.
17. The height of it was 30.
18. This day the air began to get out of the receiver; for some bubbles appear'd in the turpentine, which closed the juncture of the receiver, and cover.

And filberd-kernels in common air.

(138.) *September* 4. I put kernels of filberds into a receiver with common air.

In the afternoon, the quantity of air seemed to be lessen'd.

Sept. 5. The height of the mercury was less than half an inch.
9. The height of it was the same.
7. The height of it was 1 inch.
8. The same height continued.
18. The same height continued.

This experiment confirms, that sometimes air is produced much more easily *in vacuo*, than in common air.

Raisins with water in vacuo.

(139.) *September* 15. 1677. I included 8 ounces of raisins of the sun, bruised and diluted with a little water, in an exhausted receiver, able to hold 22 ounces of that fluid.

Sept. 16. The height of the mercury was six inches.

Sept. 17	The height of it was	10	*Sept.* 29	The height of it was	29
18		15	20		29

Sept. 21. This day I found the receiver forced from its cover.

Sept. 24. I took out some of the raisins; but those that remain'd, I enclosed in the same evacuated receiver.

Sept. 25. The raisins forced the receiver, now full of air, from its cover.

And raisins with water in common air.

September 15. 1677. I put 8 ounces of raisins of the sun, bruised and diluted with a little water, into a receiver, able to hold 22 ounces of water; but did not exhaust the air at all.

Sept. 16. The mercury was three quarters of an inch above its usual height.

Sept. 17. The height of the mercury was 1 and a half.
18. The height of it was 3.

Sept.

Sept. 19	The height of it was	5	*Sept.* 22	The height was	11
20		7	23		12
21		9	24		15

Permitting the air to break out, many bubbles emerged from the raiſins.

This experiment further teaches, that air is ſometimes much more eaſily produced *in vacuo*, than in common air.

(140.) *February* 17. 1677. I put three onions into an exhauſted receiver. *Onions* in vacuo.

Feb. 19. The height of the mercury was one inch.

21. The aſcent thereof was again 1. The onions were not alter'd.

25. The whole aſcent of the mercury was 9. The onions not alter'd.

May 4. The onions had yet ſuffer'd no alteration.

18. Neither were they yet alter'd.

June 19. I found the receiver forced from its cover, and the onions rotten.

Feb. 17. I incloſed 3 onions in air, ſo rarified, that it could ſuſtain only ten inches of mercury. *Onions in rarified air.*

Feb. 19. There was no aſcent of the mercury.

21. There was yet no aſcent thereof. The onions did not ſprout, but contracted a mouldineſs.

25. The aſcent of the mercury was about 7 inches.
The onions received no further alteration.

May 4. The onions were not alter'd.

18. The onions were not yet alter'd; but the receiver, by the force of the produced air, was removed from its cover.

February 17. I put 3 onions in a receiver not exactly ſhut. *And onions in common air.*

21. They contracted no mouldineſs, but ſprouted.

25. They gradually took root.

May 4. The onions began to be mouldy.

This experiment makes it probable, that ſome bodies produce their air not much more eaſily *in vacuo*, than in rarified air.

It hence alſo appears, that vegetation is hinder'd, not only by the evacuation, but alſo by the rarifaction of the air.

It likewiſe deſerves our obſervation, that the onions, as long as their roots ſprouted, contracted no mouldineſs.

(141.) *Auguſt* 23. 1677. I put bruiſed pears into an exhauſted receiver, with a mercurial gage. *The difference betwixt whole, and bruiſed fruits, ſhewn in bruiſed pears in* vacuo.

Auguſt 25. The height of the mercury was five inches.

Aug. 26	The height of it was	10	*Aug.* 29	The height was	21
27		14	30		25
28		18	31		28

Sept. 1. The height of it was 30.

2. The receiver was forced from the cover.

Auguſt 23. I put whole pears into an exhauſted receiver; the quantity of the pears, and the capacity of the receiver, being the ſame with thoſe juſt mention'd. *And whole, in* vacuo.

Aug. 25. The height of the mercury was 11.

Aug.

Aug. 26	The height of it was 17	*Aug.* 28	The height of it was 28
27	25	29	30

Aug. 30. The mercury aſcended no higher; the receiver being forced from the cover.

This experiment ſeems to prove, that bruiſed fruits do not produee air ſo ſoon as entire ones.

In whole apples in vacuo.

(142.) *Auguſt* 24. I encloſed whole apples *in vacuo*, with a mercurial gage.

Auguſt 25. The height of the mercury was 5 inches.

Aug. 26	The height of it was 9	*Aug.* 29	The height was 19
27	12	30	25
28	15	31	28

September 1. The height of it was 29.

2. The height of it was 30.

3. The receiver was forced from the cover.

And bruiſed apples in vacuo.

Auguſt 24. I put an equal quantity of bruiſed apples into an evacuated receiver, of the ſame capacity with the former.

Aug. 25. The height of the mercury was 1 inch.

26. The height of it was 3.

27. The height of it was 4.

Sept. 3. The mercury continued at the ſame height.

25. The mercury aſcended not.

This experiment ſeems to inform us, that bruiſed fruits produce air, ſlower than whole ones.

In bruiſed grapes in vacuo.

(143.) *Auguſt* 25. 1677. I put unripe grapes, bruiſed, into an evacuated receiver.

Aug. 26. The height of the mercury was one inch.

27. The height of it was two inches.

28. The height of it was 2 and a half.

29. The height of the mercury was the ſame.

Sept. 15. The mercury did not aſcend, but its height remained at 2 $\frac{1}{2}$.

And whole grapes in vacuo.

Auguſt 25. 1677. I put whole unripe grapes into an evacuated receiver.

Aug. 26. The height of the mercury was three inches.

27. The height of the mercury was five.

Aug. 28	The height of it was 7	*Aug.* 30	The height of it was 12
29	10	31	13

Sept. 1. The height of the mercury was 15.

2. The height of it was 16.

3. The height of it was 18.

4. The height of it was the ſame.

Sept. 5. The height of the mercury continued the ſame; but almoſt all the grapes had contracted a yellow colour.

Sept. 7. The mercury reſted at the ſame height; and all the grapes were yellow.

Sept. 15. The height of the mercury was twenty.

This

This experiment ſhews, that whole fruits produce air more readily than bruis'd.

In whole grapes.

(144.) *Sept.* 10. 1677. I put two ounces of grapes, not bruis'd, into a receiver able to hold ten ounces of water.

Sept. 11. The height of the mercury was ſix inches.

Sept. 12	The height of it was	9	*Sept.* 15	The height of it was	20
13		12	16		25
14		15	17		28

Sept. 18. The height of the mercury was thirty. The grapes were not at all alter'd,

Sept. 19. The height of the mercury was the ſame.

Sept. 20. The receiver was not yet forced from the cover. The grapes were not alter'd, but appear'd only a little riper.

Sept. 21. The receiver was forc'd from the cover, tho' nothing had eſcap'd.

Sept. 22. In the morning, the grapes began to rot; I, therefore, included them again *in vacuo.*

Sept. 23. The height of the mercury was five inches.

Sept. 24	The height of it was	9	*Sept.* 27	The height of it was	20
25		14	29		27
26		17	30		28

Octob. 10. The receiver was not forc'd from the cover, till to-day: the grapes, by their colour, ſeem'd rotten, yet kept their firmneſs.

And bruis'd grapes.

Sept. 10. 1677. I included two ounces of ripe, bruis'd grapes in a receiver capable of holding ten ounces of water.

Sept. 11	The height of the mercury was	4	*Sept.* 15	The height of it was	15
12		7	16		18
13		10	17		20
14		12	18		25

Sept. 19. The grapes had ſever'd the receiver from the cover, and much juice was ſpilt.

Sept. 20. I again put the ſame grapes into the ſame receiver; but, becauſe they had ſpilt their juice by ebullition, I did not exhauſt all the air: the mercury reſted at the height of five inches.

Sept. 21. In the morning, the receiver, being now full of air, no longer adher'd to the cover; ſo that I took out the grapes, and tranſmitted them into another receiver, which I ſtopp'd cloſe with a ſcrew, but extracted no air from it.

Sept. 22. The height of the mercury was eleven inches; tho' the receiver was able to hold twenty-ſix ounces of water.

Sept. 23. The height of the mercury was nineteen.

Sept. 24. The height of it was the ſame.

Sept. 30. The height of it was twenty.

Octob. 3. When the grapes produc'd no more air, I took them out, and found them of a bitter taſte; being not yet perfectly ripe.

PNEUMAT. This experiment, compar'd with that before related, of unripe grapes, ſeems to intimate, that unripe grapes produce leſs air when they are bruis'd, than when whole; but that ripe grapes do the contrary.

And in ſound and bruis'd apples, in vacuo. (145.) *Nov.* 19. 1678. I put apples into three evacuated receivers. In the firſt was a ſound apple; in the ſecond an apple bruis'd, and laid looſe in the open veſſel; in the third, was alſo a bruis'd apple: and the cover of this ſo fitted the including veſſel, that it ſtraitly compreſs'd the parts of the apple; but in exhauſting the receiver, the air, formed between the parts of the apple, expell'd all the juice.

Nov. 21. In the firſt receiver, the height of the mercury was five inches; in the ſecond, three; in the third, none.

Nov. 23. In the firſt receiver, the height of the mercury was ſeven; in the two others there was no change.

Decemb. 7. In the firſt receiver, the height of the mercury was eleven. There was no alteration in the other two.

Jan. 23. The firſt receiver was now ſever'd from its cover, by the force of the air produc'd a-new. In the two others there was no air generated.

May 20. 1679. The third receiver was forc'd from its cover; but the ſecond had produc'd no air.

This experiment informs us, that bruis'd fruits produce leſs air *in vacuo*, than ſound ones; contrary to what happens in common air. The reaſon whereof, may, perhaps, be this, that fruits bruis'd are very much rarify'd *in vacuo*; whence the ſeveral principles, of which they conſiſt, cannot act upon one another: but unbruis'd fruits, by reaſon of the entireneſs of their ambient skin, ſuffer leſs rarifaction.

That air is ſometimes unfit to produce mouldineſs, ſhewn by roſes in common and compreſs'd air. (146.) *July* 12. 1678. I put roſes into two receivers, to be ſtopt with ſcrews. One of them contain'd common air uncompreſs'd; but I intruded ſo much air into the other, as ſuſtain'd the mercury ſixty inches above its wonted height.

Aug. 2. The roſes in the common air, were, four days ago, turn'd yellow, as if they had been wither'd; but thoſe in the compreſs'd air, kept their colour very well.

Feb. 10. 1679. Thoſe in the compreſs'd air, retain'd their freſh colour.

This experiment, compar'd with that made, laſt year, with roſes, informs us, that the air, at different times, is differently affected; ſo that ſometimes it hath a power to hinder corruption, and ſometimes to promote it.

And by tulips and lark-ſpurs. (147.) *May* 22. Fifteen days ago, I included two equal quantities of flowers, in two receivers: into one of them, I thruſt ſo much air as ſuſtain'd the mercury ſixty inches above its wonted height; but in the other, I left common air uncompreſs'd. The flowers were tulips and lark-ſpurs.

Since that time no mouldineſs appear'd, except, only, that ten days ago, one half of the tulip, in the common air, being cut aſunder, ſeem'd ſomewhat mouldy; and now the other half of the ſame tulip in compreſs'd air, ſeem'd alſo a little mouldy.

Some of the flowers ſeem'd as freſh, as when firſt put in; eſpecially thoſe in the common air; but in the compreſs'd air, they ſeem'd moiſter.

June

PNEUMATICS.

June 22. No more mouldineſs appear'd: whence we have it confirm'd, that the air is, ſometimes, unfit to produce mouldineſs; ſince, laſt year, all this kind of flowers, contracted a great mouldineſs.

The change of weight made by the ſun's rays, in veſſels hermetically ſealed, ſhewn, by expoſing red-lead thereto in an open glaſs.

(148.) *Sept.* 4. 1678. I expoſed one dram of minium, in an open glaſs, to the ſun-beams, concentrated by a burning-glaſs; and found that it loſt $\frac{3}{4}$ grain of its weight, though much of the minium had not been touch'd by the rays.

Calcin'd coral in a ſealed one.

(149.) *Sept.* 6. I took calcined coral, and endeavour'd to calcine it further, by the rays of the ſun, in a ſealed glaſs; and the whiteneſs of the calx was ſomewhat increaſed hereby.

Sept. 10. I expoſed the ſame coral again to the ſun-beams, in the ſame glaſs hermetically ſealed, for two whole hours; and, then weighing the glaſs, found it had loſt about $\frac{1}{16}$ part of a grain, ſince it was firſt ſealed.

And the calx of tin, minium, and ſulphur.

(150.) *May* 23. I put calx of tin in a light glaſs vial, hermetically ſealed, and weigh'd it exactly: afterwards I expoſed it to the beams of the ſun, for a long time, by the help of a large lens; then the glaſs, being again weigh'd, ſeemed to have loſt $\frac{1}{64}$ part of a grain of its weight.

May 29. I repeated the experiment with minium, inſtead of calx of tin, and the loſs of weight came to $\frac{1}{32}$ part of a grain.

May 30. I endeavour'd to calcine the ſame minium again, but ſuch plenty of air was produced, that the glaſs broke, with a great noiſe, into an hundred pieces.

June 6. I made the ſame experiment again with minium; and then $\frac{1}{64}$ part of a grain was wanting of the weight.

Attempting again to burn minium, the glaſs alſo broke.

July 15. I uſed wood-coals for the ſame experiment, but the ſun did not at all affect them.

July 20. I expoſed *Sulphur vivum*, to the beams of the ſun, in the ſame manner; and tho' it was eaſily melted, and emitted many fumes, yet I found no change at all in the weight.

Aug. 1. I kept the ſame vial ſtill, with the flower of ſulphur; and often expoſed it to the fire of the burning-glaſs, without danger of being broken; becauſe ſulphur produces no air: the fumes roſe, and, at firſt, the ſulphur bubbled; but the weight remain'd the ſame.

Bodies preſerved chiefly in vacuo, *and firſt ſome roaſted rabbet.*

(151.) *Nov.* 6. A piece of roaſted rabbet, being exactly cloſed up, in an exhauſted receiver, was two months, and ſome few days after, taken out, without appearing to be corrupted, or ſenſibly alter'd, in colour, taſte, or ſmell.

Bread.

(152.) *March* 11. A ſmall glaſs receiver, being half fill'd with pieces of white bread, was exhauſted, and ſecured.

April 1. The receiver being open'd, part of the bread was taken out, and appear'd not to have been impair'd in that time; only the outſide, of ſome pieces of crumb, ſeem'd to be a very little leſs ſoft and white, than before. There appear'd no drops, or the leaſt dew, on the inſide of the glaſs. The remaining bread was, again, ſecured ſoon after.

PNEUMATICS. *April* 18. The bread was taken out again, and tasted much as it did the last time; the crust being, also, soft, and no drops of water appearing on the inside of the glass.

Milk. (153.) *March* 9. I open'd a small exhausted, and secured receiver, wherein, about three months ago, we had included some milk, which was well-colour'd, and turn'd, partly, into a kind of whey, and, partly, into a kind of soft curd. The taste was not offensive, only a little sourish, like whey; nor the scent fetid, but somewhat like that of sourish milk.

Violets. (154.) *March* 5. Violet-leaves, put up, freed and secured from air, being open'd, *April* 7. appear'd not to have chang'd their shape, colour, or consistence; but their odour could not be well judg'd of; because he who included them, had crush'd many of them together, in thrusting them down; since, by such a violation of their texture, 'tis natural for violets to lose their fragrancy, and acquire an earthy smell.

(155.) Having carefully placed some violets in an exhausted receiver, of a convenient size, and bigness, and secur'd it from immediate commerce with the external air; after seven months, we look'd upon them again, and found they were not putrefied, or resolved into any mucilaginous substance, but kept their shape entire; some of them retaining their colour, but more of them having so lost it, as to look like white violets.

Sheep's blood. (156.) *Nov.* 5. We convey'd into a conveniently shaped receiver, some ounces of sheep's blood, taken from the animal, kill'd that afternoon. After the exhaustion of the air, during which, numerous bubbles were generated, that made the liquor swell considerably; the included blood was kept in a warm place for twenty days; and, during one or two of the first, the blood seemed to continue fluid, and of a florid colour; but afterwards, degenerated into one, that tended more to blackness.

Nov. 25. We let in the external air; and the glass, containing the blood, being held in a light place, the greatest part of the bottom of it seem'd to be thinly overlaid with a coagulated substance, of a higher colour than what swam above it; which, though it appear'd dark, and almost blackish in the glass, whilst view'd in the bulk, yet, if it was shook, those parts of it that fell down along the inside of the glass, appear'd of a deep fair colour. But, whilst the blood continu'd in the glass, it was suppos'd not to stink; since, even when it was pour'd out, tho' its scent seem'd, to me, somewhat offensive, yet, to others, it seem'd to smell like the blood of a dog, newly kill'd.

Cream. (157.) *March* 17. Some cream being put up, and secur'd in an exhausted receiver, appear'd, a year after, to be more thick, and almost like butter, at the top, than in other parts; and afterwards, by being well shaken together, in the glass, it was easily enough reduced to butter, whose butter-milk, by the judgment of those who were more used hereto than I, appear'd not different from ordinary butter-milk; and, I found it had, like that, a grateful sourness. The butter was judg'd to be a little sourer than ordinary, but was not, as they speak, made.

(158.) *Feb.* 18. We look'd upon three vials, that had been exhausted, and secured *Sept.* 15 last; the one of these had in it some slices of roasted beef, the other some shivers of white bread, and the last some thin pieces of cheese; all which, seem'd to be free from putrefaction, and look'd, much, as when they were first put in; we, therefore, let not the air into the receiver, but left them, as they were, to prolong the experiment. PNEUMATICS. *Beef &c.*

(159.) *Feb.* 18. There was a fourth vial, wherein, about six months before, had been inclos'd, and secured some july-flowers, and a rose; yet, these being kept in the same place with the rest, tho' they seem'd a little moist, retain'd their shape and colour, especially the rose, which look'd, as if it had been lately gather'd. We observ'd, in none of these four receivers any great drops, or so much as dew in the parts situated above the included matter. *Flowers.*

(160.) *June.* 4. We left some strawberries in an exhausted receiver, and coming to look upon them after the beginning of *November*, we found them to be discolour'd, but not alter'd in shape, nor mouldy; we, therefore, left them still in the receiver for further trial. *Straw-berries.*

(161.) *May* 2. 1669. A piece of roast-beef, secur'd *September* 15. last, appear'd to be not at all alter'd; no more did a piece of cheese, secured in another receiver, and some pieces of a *French* roll, secured, on the same day, in a third. *Cheese, &c.*

Flowers, seal'd up *August* 12. 1668, being this day look'd upon, appear'd fresh.

(162.) *June* 17. A pint of small beer, being put into a conveniently shaped glass, afterwards exhausted, and secured from the air; the most part of *August* proving extraordinarily hot; towards the latter end, there was, at several times, great thunder, which turn'd the beer in our cellar, and in most of those of the neighbourhood, sour. *Sept.* 1. The beer was open'd, but did not seem to be sour. *Small-beer.*

(163.) To try, whether the thunder would have such an effect upon ale, exactly stopt in glass vessels, as it often has on it in ordinary casks; I caus'd some ale, moderately strong, to be put into a conveniently shaped receiver, and having exhausted the air, and secur'd the glass vessel, 'twas put into a quiet, but not a cool place. About six weeks after the liquor had been inclos'd, there happen'd some very loud thunder; and our beer, upon this, tho' the cask was kept in a good cellar, being generally noted to have been turn'd sour; I stay'd yet a day or two longer, that the operation upon our included liquor might be the more certain and manifest; and then taking out the ale, found it good, and not at at all sour'd. *Ale.*

(164.) Some black-berries, included in an exhausted receiver, *Sept.* 21. 1670. were open'd *June* 20. 1673. and found free from all mouldiness, and ill scent; only there was some sour liquor, which being taken out, the berries were secur'd again. At the same time, another parcel of the same berries was exactly clos'd up in a receiver, whence the air was not pump'd; but coming, *Octob* 11. 1673. to look upon the glass, we found it crack'd, and the fruit all cover'd with a thick mould. Nor was this the only *Black-berries.*

PNEUMATICS. only vessel, wherein, trials made to preserve fruits without any exhaustion of the air, miscarried.

Octob. 11. 1674. The former berries *in vacuo*, being look'd upon, appear'd much less black than before; but did not seem putrefy'd, either by loss of shape, or by any stinking smell; nor was the least mouldiness observ'd upon them, tho' they had been kept in the same receiver for above four years.

Ale. (165.) *June* 14. We put a convenient quantity of ale into a bolt-head, and seal'd it up hermetically; the next year, *July* 5. we broke off the seal, and found the liquor very good, and without any sensible sourness. The next day, it was seal'd up again, and set by for thirteen months; when, the neck of the glass being broken, the ale was found sour. We see, however, that a small quantity of ale was preserv'd good, at least, above a year; which is much longer, than that liquor usually keeps.

Claret. (166.) *June* 14. 1670. In a large bolt-head, we hermetically seal'd up above a pint of *French* claret, which, when we came to look upon *July* 5. 1671. appear'd very clear and high colour'd, and had deposited a large sediment at the bottom of the glass, but fasten'd no tartar, that we could perceive, to the sides. Upon breaking the seal'd end of the glass, we thought there was an eruption of included air, or steams; and, high above the surface of the wine, there appear'd a certain white smoke, almost like a mist, and then gradually vanish'd: the wine continu'd well tasted, and was a little rough upon the tongue, but not at all sour.

The bolt-head was seal'd up again *July* 6. 1671. and set by, till *August* 5. 1672. at which time it was open'd again, and the wine still tasted very well.

June 26. 1673. The bolt-head, with the same claret, being open'd, was found very good, and seal'd up again. *Octob.* 11. 1674. the same wine was open'd again, and appear'd of a good colour; it was not sour, but seem'd somewhat less spirituous, than other good claret; perhaps, because of the cold weather.

Bodies preserv'd in compress'd liquors, and first apricocks with raisins and water. (167.) *Aug.* 3. 1678. I included two apricocks in two receivers, one of which was exactly fill'd with bruis'd raisins of the sun, and with water; but in the other, there were only lodg'd a few raisins, so that the apricock was not touch'd, by them, or their moisture.

Sept. 10. I took out the apricock, inclos'd with the water; and, whilst the air broke out, the fruit bubbled very much: the raisins had lost, almost all their taste, but the apricock preserv'd a pleasant relish; and seem'd more pleasant than such fruit usually is at that season of the year.

Feb. 10. 1678. The apricock, inclos'd without water, kept its colour and figure, only seem'd to have lost its firmness.

This experiment informs us, that the taste of some fruits may be preserv'd in an infusion of raisins of the sun; at least in vessels able to resist a great compressure of the air.

Peaches in an infusion of raisins. (168.) *Sept.* 17. 1678. I included peaches, with an infusion of raisins, in two receivers, shut with a screw.

Sept.

Sept. 21. Too great a quantity of air produced in one of the receivers, expell'd ſome part of the liquor. The other receiver retain'd its liquor. PNEUMATICS.

Sept. 25. The receiver, out of which the liquor was expell'd, loſt ſome more of it ; ſo that a fifth, or ſixth part, now ſeem'd empty : but, ſetting the ſcrew, the liquor was then preſerv'd. The other receiver remain'd unalter'd.

Sept. 26. The ſame receiver began, again, to leak, and run over : I ſet the ſcrew again.

Nov. 27. Our receiver, hitherto, ſeem'd to be exactly ſhut; but now I open'd it; and, whilſt the air was getting out, the peaches bubbled very much : one of them, which was of that ſort whereto the ſtone uſually adheres, preſerv'd its firmneſs, and a pleaſant taſte ; but the other, being of the yellow-colour'd kind, was very ſoft ; yet the taſte thereof ſeem'd to be more pleaſant than of the other. The liquor was very grateful.

Decemb. 28. The other receiver ſeem'd unalter'd ; but, when I open'd it, innumerable bubbles immerg'd from the liquor, and from the peach. The peach, on one ſide, had preſerv'd its firmneſs ; on the other, it had loſt it : but the whole was grateful to the palate, tho' ſomewhat ſharp..

This experiment ſeems to teach, that liquors may grow ſour, tho' no ſpirits have evaporated from them.

(169.) *September* 20. I included peaches, with unripe grapes, in two receivers, and filled them exactly ; the one with apples bruiſed to the conſiſtence of a pultice ; and the other, with an infuſion of raiſins of the ſun. *Peaches with grapes, apples, and an infuſion of raiſins.*

Sept. 25. The receiver, fill'd with pulp of apples, hitherto ſeem'd unalter'd ; but, in the other, the air, which was generated, had thruſt out half of the contain'd liquor, and impel'd the mercury into the gage, to the height of 100 inches; wherefore, I open'd the receiver, and the peach, whilſt the air got out, was almoſt reduced to the conſiſtence of a pultice : the taſte of it was pleaſant.

I put another peach into the ſame receiver, and ſubſtituted a new infuſion of raiſins of the ſun, inſtead of that which was loſt.

Sept. 26. The mercury roſe to 30 inches above its uſual height.

Sept. 27. The height of the mercury was 72.

28. The height of it was 90. The liquor work'd out.

30. The ſame height remain'd ; but the liquor was all eſcaped.

October 1. All the air had, alſo, eſcaped ; wherefore, opening the receiver, I found the peaches very ſoft, but of a pleaſant taſte.

Octob. 3. The receiver, filled with the pulp of apples, had loſt nothing ; but now I perceiv'd, that almoſt all the juice of the apples had run out : I open'd the receiver, and found its contents very much fermented. The peach was very ſoft, but not unpleaſant in taſte.

This experiment informs us, that fruits cannot be long kept in pulp of apples, becauſe of the great production of air ; tho' that happens a little later in the infuſion of raiſins.

(170.) *Sept.* 23. 1678. I included peaches, with crude grapes, in two receivers; one of which was exactly fill'd with pulp of apples, the other with unripe grapes, bruiſed. *Peaches with grapes, and the pulp of apples.*

Octob.

PNEUMATICS.

Octob. 1. The receiver, fill'd with pulp of apples, seem'd to have suffer'd no alteration; but the other, was empty of liquor: this, therefore, I open'd, and found one of the peaches to have retain'd its firmness and taste; but the other had lost its firmness, yet retained a grateful taste.

Feb. 5. 1679. The receiver, containing the pulp of apples, seem'd unalter'd: I open'd it, and the great ebullition which arose thereupon, manifested, that a great compression of the air was made. The pulp of apples, and the peach, retain'd a grateful taste, but somewhat more pungent than ordinary.

This experiment shews, that juice of crude grapes cannot, conveniently, be used for the preservation of fruits, by reason of the too great production of air.

Pears included with the pulp of apples.

(171.) *Sept.* 25. 1678. I included two pears, called buttter-pears, in a receiver, exactly fill'd with pulp of apples.

Sept. 28. I perceiv'd no alteration in the height of the mercury.

Octob. 5. The mercury was now risen 15 inches.

Octob. 6. The height of the mercury was above 16.

Octob. 12. The mercury was not changed.

Octob. 20. Three days ago, the mercury was depressed, though nothing had escaped.

Octob. 26. This day the receiver was crack'd; though I did not find that the air was compressed within it; but, perhaps, the screw was set too high. The pulp of the apples was of a very grateful taste; so were the pears, tho soft, and one of them inclined to rottenness.

Perhaps, the crack in the receiver, was the cause of so little air being produced in this experiment.

Peaches inclosed with the pulp of apples, and unripe grapes.

(172.) *Octob.* 1. 1678. I inclosed peaches in two receivers; one of which was filled with pulp of apples, and the other with unripe grapes, bruised.

Octob. 5. Much air was produced in the second receiver, and some of the juice ran out. The height of the mercury was 64 inches.

Octob. 6. The juice continu'd to run out: the height of the mercury was 70.

Octob. 8. Now the juice seem'd to be all run out of the receiver; and the height of the mercury was 86.

Octob. 12. The mercury remain'd at 86.

Octob. 18. The receiver, emptied of its juice, held the air very well; and the mercury in it rested at 86. The other receiver, filled with pulp of apples, had, for these five last days, suffer'd some juice to flow out.

Decemb. 4. I open'd the receiver, fill'd with pulp of apples; and tho' all the juice was gone, yet it still retain'd the air, very much compressed; and many bubbles broke out, not without noise, after the receiver was quite open'd. The peach was very soft, and of a pungent taste, like to that of strong wine.

Jan. 22. 1679. After the effusion of the juice in the other receiver, the mercury rested at the same height. I open'd the receiver; the peaches emitted many bubbles, and were wrinkled, but their colour was little changed: their taste was most pungent, and inclining to acid.

This

This experiment confirms the conclusions drawn from the former.

Peaches included with ale, beer, and wine.

(173.) *Octob.* 4. 1678. I put peaches into three receivers; the first of which was filled with ale; the second, with hopp'd beer; the third, with wine.

Octob. 5. The height of the mercury, in the first receiver, was 15; in the second, 10; in the third, 9.

Octob. 6. The height of it, in the first receiver, was 25; in the second, 15; in the third, 20.

Octob. 8. The height of the mercury, in the first receiver, was 35; in the second, 15; in the third, 20.

Octob. 12. The height in the first receiver, was 63; in the second, 15; in the third, 28.

15. The height of the mercury, in the first receiver, was 81; in in the second, 15; in the third, 30.

16. There was no more change perceived in any of the three receivers.

18. The mercury rather descended, than ascended in all the three.

22. In the wine, only, the mercury ascended, or descended, according to the degrees of heat and cold.

24. The height of the mercury, in the first receiver, was 96; in the second, 15; in the third, 30.

30. The height, in the first receiver, was 115; in the second, 20; in the third, 30.

Nov. 3. The height, in the first receiver, was 117; in the second, 20; in the third, 30.

6. The height, in the first receiver, was 120; in the second, 31; in the third, 31.

11. The height of the mercury, in the first receiver, was 105; in the second, 31; in the third, 28.
The weather was cold.

Nov. 16. The height of the mercury was the same. The peach, which hitherto lay at the bottom, now mounted to the upper part of the liquor, in the second receiver; the rest staid at the bottom.

Nov. 25. The height, in the first receiver, was 140 inches; in the second, 47; in the third, 32.

Nov. 28. The height, in the first receiver, was 96; in the second, 36; in the third, 28. It was very cold weather.

Decemb. 13. The height, in the first receiver, was 96; in the second, 47; in the third, 33. I open'd the third receiver, and found the peach firm, and of a laudable colour; but it had contracted much of its taste from the wine, and might yet be improved by sugar. The wine, also, was grateful to the palate.

Decemb. 30. The height of the mercury, in the first receiver, was 96 inches; in the second, 47. I open'd the first receiver; when, the peaches, which had lain, till then, at the bottom of the liquor, presently emerg'd to the upper part, and emitted many bubbles: the taste of the ale, of which they had greatly partook, became pleasant, with sugar.

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

(174.) *Sept.* 5. 1678. I included one whole peach, with another cut to pieces, in a receiver; into which, I afterwards poured old wine, till it was exactly fill'd, and then shut it with a screw.

Nov. 20. Nothing, hitherto, seem'd to be alter'd; but, this day, I perceiv'd some of the wine run out.

Nov. 30. A third part of the wine was lost.

Decemb. 8. The wine beginning again to run out, and there being but little of it left, I open'd the receiver, and found the peaches very much fermented, yet of a grateful, but most pungent taste. The wine, also, was pleasant.

From this experiment, compared with the third receiver, in the former, we may conjecture, that wine hinders the fermentation of peaches, if used in a sufficient quantity; but here the quantity was not sufficient, because the pieces of the cut peach fill'd the whole receiver, so that no room was left for the wine, but in the interstices.

(175.) *Octob.* 11. 1678. I put two unripe peaches, one whole, the other cut to pieces, into a receiver fill'd with hopp'd and fermented beer.

Octob. 12. In one night's time, the mercury ascended three inches.

Octob. 15. The height of the mercury was 15.

16. The height of it was 15.

18. The height of it 12. It was very cold.

20. The height of it remained at 12.

22. The mercury ascended again. The cold abated.

Nov. 2. The height of the mercury was 20.

3. The mercury descended a little. It was cold weather.

6. The height of the mercury was 28. The weather grew hotter.

8. The height of it was 33.

11. The height of the mercury was 40.

12. The height remained at 40. Some of the beer work'd out.

16. The height of it was 46.

19. The height of it was 43. But much of the beer was lost.

21. The mercury ascended not, but the beer continued to work out.

23. When the beer was almost all work'd out, I open'd the receiver, and found the peaches very soft, yet of a grateful taste; tho' they were kept for 9 hours in the free air, after the receiver was open'd.

From this experiment, compared with the second receiver, we may infer, that beer hinders the fermentation of peaches, and the production of air, if used in a sufficient quantity: but here there was only a little beer, contain'd in the interstices, which was unable to hinder the fermentation of the peaches.

Raw beef included with stale beer, and common air.

(176.) *Octob.* 19. 1678. I included raw beef in three receivers; the first of which was exactly fill'd with stale beer, forcibly intruded; so that the mer-

mercury exceeded its wonted height by sixty inches. The second was, also, exactly fill'd with stale beer, but here there was no compressure made. The third was fill'd, partly with the beef, and partly with common air. Pneumatics.

Octob. 20. In the first receiver, the mercury was depress'd to twenty inches below its usual height; tho' nothing at all had escaped out. In the second, also, it descended; but in the third, it ascended a little.

Octob. 26. In the first receiver, the mercury sometimes ascended, and then descended, very irregularly; in the second, it began to ascend slowly, two days ago; in the third, it was not mov'd at all.

Octob. 27. A piece of the same beef, which was left in the air, began to smell ill; and the mercury in the third receiver, began to ascend; in the second, it continu'd to ascend gradually; but in the first, it seem'd rather to descend.

Nov. 3. The mercury in the first receiver ascended not; in the second, the height of it was twenty inches; in the third, ten.

Nov. 5. I open'd all the receivers, and the two first had no offensive smell, only contracted a scent from the beer. The flesh boil'd in the same beer was very tender, but its taste was bitter; perhaps, by reason of the too great quantity of beer. The beef included with common air, presently smelt fetid, upon being open'd; yet, when taken out, and applied to the nose, it scarce seem'd to stink. I included the same flesh in the same receiver, to try whether new air being admitted, would promote corruption.

Nov. 6. The height of the mercury was three inches.

Nov. 11. The height of it was nine.

Nov. 25. The height of it was twenty.

I open'd the receiver, and found the flesh so fetid, that I was forc'd to throw it away.

From hence it seems to follow, that beer may help to preserve flesh, especially if it be forcibly intruded into the receiver; but this compressure is soon abated, because the air, compress'd in the same receiver, is apt to enter into, and gradually pervade the pores of the beer.

(177.) *Nov.* 12. I included beef, press'd together as close as I was able, in three receivers: into the first of them I pour'd water, mix'd with one fortieth part of salt, which fill'd up all the interstices, left betwixt the parts of the flesh; the second, in like manner, contain'd some salt water; but it was so forcibly intruded, that the mercury in the gage ascended fifteen inches, above its wonted height: into the third receiver, I pour'd no water, and therefore those few interstices, which could not be possess'd by the flesh, were left for the air. *Beef included with salt-water, and common air.*

Nov. 13. The mercury descended in all the receivers, especially in the second, wherein was the compress'd liquor.

Nov. 18. The two receivers, which were uncompress'd, did not drive the depress'd mercury upward: but that, whose mercury had been impell'd to fifteen inches, and afterwards had descended most, now return'd almost to its former height. A piece of the same beef, being left in the air, began to smell ill.

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Nov. 23. In all three receivers, air was produced a-new; but to-day the mercury, in the ſecond, deſcended three inches, and the height of it was twenty; in the other two 'twas about ſixteen. I open'd the firſt receiver, and the fleſh was not at all corrupted.

Nov. 30. I took that fleſh out of the receiver, which was put in without ſalt, and it did not ſtink at all; but, being boil'd, was very tender, and of a pleaſant taſte.

Decemb. 6. I open'd the receiver, into which I had forcibly introduced ſalt water. The mercury exceeded its wonted height by twenty-five inches. The fleſh ſmelt ſtrong, yet did not ſtink: that *in vacuo* yielded many bubbles, which ceas'd not, till a pretty while after the receiver, in which it was included, was taken from the pneumatic engine; then mercury, in one hour's time, came to the height of three or four inches. I, afterwards, immers'd the ſame receiver ſo exhauſted, in hot water; and the liquor, contain'd therein, bubbled very much, tho' the water, from which it borrow'd all its heat, did not boil; but ſo great a quantity of air was produc'd, or had enter'd from without, that the receiver was quickly full. The liquor, contain'd therein, did not, afterwards, bubble, or boil, tho' it were immerg'd in boiling water. I took out the fleſh, and found it pleaſant and tender, yet leſs ſo, than I expected; perhaps, becauſe it was not boil'd enough.

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

Oiſters with their ſhells, and without, included in ſalt-water, common air, and in vacuo.

(178.) *Nov.* 29. 1678. I inclos'd oiſters in four receivers: in the firſt, the oiſters were without their ſhells, and exactly fill'd the whole ſpace; in the ſecond, the oiſters, with their ſhells, were included with common air; in the third, the oiſters alſo were included in their ſhells; the remaining ſpace of the receiver, being exactly fill'd with ſalt-water. Theſe three veſſels were firmly clos'd with ſcrews. The fourth receiver was exhauſted of air, and contain'd three oiſters in their ſhells, and eight taken out of their ſhells. When the air was pump'd out of this receiver, the oiſters freed from their ſhells, emitted many large bubbles; but the three others ſuffer'd no ſenſible change, only one of them gaped.

Nov. 30. In the three receivers, ſtopp'd with ſcrews, air ſeem'd to be conſumed, rather than produced; but the mercury *in vacuo* aſcended a little.

Decemb. 4. Whilſt the weather was cold, the mercury aſcended not; but now, when the cold began to abate, the height of it in the firſt receiver was ſeven inches; in the ſecond, none; in the third, three; and in the fourth, three.

Decemb. 5. The height of the mercury in the firſt receiver was twenty inches; in the ſecond, one; in the third, three; in the fourth, five.

Decemb. 7. The height of the mercury in the firſt receiver was thirty inches; in the ſecond, one; in the third, three; in the fourth, eight. Other oiſters, left, at the ſame time, in the air, ſmelt ill.

Decemb. 9. In the firſt receiver, the height was thirty; in the fourth, eleven. The reſt were not chang'd.

Decemb.

Decemb. 13. There was no change in the three firſt receivers; but in the furth, the height was fourteen inches. PNEUMATICS.

Decemb. 20. In the firſt receiver, the height was forty-ſix; in the fourth, twenty-four; the reſt were not chang'd.

Decemb. 21. In the firſt receiver, the height was fifty-two; in the fourth, twenty-five; in the reſt, no change.

Decemb. 22. The height of the mercury in the firſt receiver was ſixty; in the fourth, twenty-ſeven; no change in the reſt.

Decemb. 27. In the fourth receiver, the height was twenty-nine; the reſt were not chang'd.

Jan. 1. *1679*. The oiſters in the third receiver, had ting'd the water black.

Jan. 25. The mercury *in vacuo* ſeem'd ſtill to remain, almoſt, at the ſame height. But this day, ſome bubbles were form'd in the turpentine, by the internal air, about the juncture of the cover with the receiver. I, therefore, open'd the receiver, and found the oiſters very fetid. I, likewiſe, open'd the other receivers, and found the oiſters of an ill ſcent, and turn'd to a kind of viſcid gelly.

This experiment ſeems to inform us, that fiſh produce leſs air than fleſh; yet will be corrupted, tho' defended againſt the air.

(179.) *Nov.* 29. *1678*. I exactly fill'd a glaſs veſſel, with freſh and unſalted butter; then ſtopt it with a ſcrew. A mercurial gage was included in the ſame veſſel. *Butter included in a receiver.*

Nov. 30. In the night, the cold being very ſharp, the butter was condens'd; for the mercury approach'd nearer to the aperture of its gage.

Decemb. 2. The mercury came ſtill nearer to the aperture of its gage; perhaps, becauſe the cold daily increas'd.

Decemb. 5. The cold being abated, the mercury return'd almoſt to its former height.

Part of the ſame butter, being left in the air, began to have a very bad ſmell.

Decemb. 7. The cold returning, the mercury, again, came to the top of its gage. The butter left in the air, ſmelt worſe than before, tho' it was ſtill edible.

Decemb. 24. The butter had produced no air; being taken out of the receiver, it was of a grateful taſte, except, only, a little of the ſuperficies, which lay contiguous to the leather ſpread over the cover.

It follows, that butter may be kept a great while, if it be defended from the external air.

(181.) *Nov.* 30. *1678*. I fill'd two receivers with whitings; and that no air might be left in the vacant ſpaces, into the one I pour'd wine; and into the other, oiſters, with their juice; ſo that both receivers were exactly fill'd. When I had afterwards cloſ'd their covers with ſcrews, the air in the mercurial gages was compreſs'd; but in three hours ſpace the mercury again return'd to its former mark. *Whitings and wine, and whitings and oiſters, included in receivers.*

Decemb. 2. The cold increaſing, the mercury came nearer to the aperture of its gage in both receivers.

PNEUMATICS.

Decemb. 4. The cold ceasing, the mercury ascended very much in that receiver wherein the oisters were; but, in the other, it moved not.

Decemb. 5. In the receiver, containing the oisters, the height of the mercury was 20 inches; but, in the other, it was not yet return'd to its usual height.

Decemb. 7. In the receiver with oisters, the height of the mercury was 40; in the other, it continued still below its standard.

Decemb. 9. The mercury, in both receivers, was changed little or nothing.

Decemb. 20. When the mercury alter'd no more, I open'd the receivers, and both of them were very fetid. It here seemed new to me, that the receiver, in which the wine was, had admitted of corruption, without producing air; for, hitherto, all bodies, whilst they were corrupting, had produced some.

Beef with spice, included in receivers.

(181.) *Decemb.* 3. 1678. I put raw beef into two large receivers, with pepper and cloves; and that no air might be left in the interstices, I pour'd beer upon them; and, in no long time after, found the pressure of the air, in the receivers, to be abated; the mercury, in the gages, coming to the open ends.

Decemb. 8. The mercury ascended not in either of the receivers. I open'd the one, that I might boil the flesh; which had contracted a sweet scent from the cloves; and the liquor, contain'd in the same receiver, before it was boil'd, smell'd like hippocras.

Jan. 2. 1679. I open'd the other receiver, and found no air produced therein: the flesh was not at all corrupted; and, when I boil'd it *in vacuo*, I observ'd, that if a more intense fire were made, the air, or some spirits, broke thro the stop-cock, which was fasten'd to the top of the receiver. The receiver, being cooled, all the night, was, the day after, found, almost, quite empty of air. The flesh was very tender, and well tasted, only it was a little over-boil'd; for it had been kept on the fire full six hours.

Hence we have a confirmation, that beer may be useful to preserve flesh, especially if the bitter taste thereof be corrected by aromatics.

Larks, with beef and ale, included in a receiver.

(182.) *Decemb.* 4. 1678. I included two larks, with some beef, in a receiver, and fill'd all the spaces, unpossess'd by the flesh, with ale; at the same time, I fill'd another receiver, with the same sort of beef, adding beer, also, but no larks.

Decemb. 9. Some pieces, cut off from the larks, and exposed to the air, began to smell ill; but those included in the receiver, had produced little air; for the mercury was not yet come five inches above its wonted height. In the other receiver it was not moved.

Decemb. 19. In the receiver, which contained the larks, the mercury ascended no higher; for the cover being broken, suffer'd the liquor to run out. Wherefore, I open'd the receiver, and boil'd both the beef and the larks, which were not at all corrupted, but very grateful to the palate. The beef had contracted a pleasant taste; partly from the larks, and partly from the beer.

Decemb.

Decemb. 23. I open'd the other receiver, and the flesh being boiled, seem'd pleasant; yet not so pleasant as that which received a venison-like taste from the larks. PNEUMATICS.

Hence birds may be long preserv'd by the help of beer, or ale.

(183.) *December* 14. I included apples in four receivers: in the first was a whole apple, and all the interstices were fill'd with powder'd sugar: in the second, was an apple cut in pieces, and the spaces fill'd with sugar, as before: in the third, was, also, an apple, cut; but the rest of the receiver was fill'd with water, wherewith a tenth part of sugar had been mixed: in the fourth, the apple was also cut, and the spaces fill'd with a solution of one part sugar, and five of water. *Apples included in receivers.*

Decemb. 21. In the first receiver, the mercury began to ascend a little, yet the sugar did not dissolve; in the second receiver, all the sugar was melted, and the pieces of apple were shrivel'd: they produced much air, when first put into the receiver. In the two other receivers, the mercury began, also, to ascend; but, in the third, the pieces of apple were very much corrupted, their skin being taken off.

Decemb. 22. Air was produced in all the receivers; but the quantities did not bear the same proportion amongst themselves, as the quantities of the sugar: for, in the second receiver, much air was produced; but, in the fourth, the mercury ascended less than in the third. Some air was, also, generated in the first.

Decemb. 27. In the three first receivers, the height of the mercury was ten inches; but in the fourth, only six.

Decemb. 31. In the first and second receiver, the height of the mercury was 13; in the third, 15; in the fourth, only *9*.

Jan. 2. *1679*. In the first and second receiver, the height of the mercury was almost 14; in the third, 17; in the fourth, 11.

Jan. 7. In the second, the height of the mercury was *16*; in the third, *36*; in the fourth, 15; but, in the first, the mercury had not ascended, and something had escaped out of the receiver: I, therefore, eased the screw, that I might dispose it the better, and then the air made an escape.

Jan. *9*. In the first receiver, the height was six inches; in the second *16*; in the third, *39*; in the fourth, 15.

Jan. 17. In the first receiver, the height was 13; in the second, *19*; in the third, 5*6*; in the fourth, 17.

Jan. 30. In the third receiver, the height of the mercury was *76* inches, and the liquor got out; I, therefore, open'd it, and found the fruit to have lost much of its taste; but the water had contracted it, and was pleasant to the palate. In the second receiver, the mercury ascended no more. I open'd this, also, and found the fruit much more pleasant than the other; yet much of its taste was imparted to the sugar, which was turn'd into a very good syrup.

Feb. *16*. The height of the mercury, in the first receiver, was 22 inches; but, in the fourth, 33. This I open'd, and found the fruit to have

PNEUMATICS. have loſt much of its taſte; and that the ambient water had got it, and was thereby turn'd into a pleaſant drink.

Feb. 27. In the firſt receiver, the height of the mercury was thirty nches.

March 15. In the firſt receiver, the height of the mercury was not changed; but, now, ſomething eſcaped out of the receiver: I open'd it, and found the apple of a laudable colour; but the pulp was ſpongy, and had loſt much of its taſte.

This experiment ſeems to teach, that ſugar is not ſo fit to preſerve fruits, as fermented liquors.

A lark included with milk.

(184.) *December* 23. I fill'd a glaſs veſſel with milk, then ſtopp'd it with a ſcrew; and, into another receiver, I put a lark with milk, and ſtopp'd it cloſe.

Decemb. 24. This evening I perceiv'd, that the caſeous part was ſeparated from the butyrous, in the cloſed receivers; as well as in the milk, which, at the ſame time, I left expoſed to the air.

Decemb. 27. I found no air produced in the receiver which held the lark; but, in the other, the mercurial gage was ſpoiled.

Decemb. 31. The mercury aſcended in that receiver which contain'd the lark; but the milk left in the air, at the ſame time that I ſtopp'd the receivers, ſtunk three days ago.

Jan. 1. In the receiver which held the lark, the height of the mercury was ten inches.

Jan. 2. The height of the mercury was 14 $\frac{1}{2}$. The milk ſtagnant below the butyrous part, appear'd of a red colour.

Jan. 4. The height of the mercury was 19. Some white ſediment was concreted at the bottom of the milk.

Jan. 9. The height of the mercury was 29 inches.

Jan. 25. I open'd both receivers: the lark ſmelt only ſtrong, tho' it had been kept 32 days; when boil'd, it was of a pleaſant taſte. In the other receiver, the caſeous part of the milk was ſub-acid, and grateful; but the butyrous part was not ſour at all.

This experiment informs us, that, ſometimes, milk may be ſucceſsfully uſed to preſerve fleſh.

A lark included in a receiver with butter.

(185.) *Decemb.* 24. 1678. I put a lark into a ſmall receiver, and pour'd butter upon it, melted over a ſlow fire, till all the interſtices were exactly fill'd; then I cloſed the cover with a ſcrew.

Decemb. 27. The mercury approached nearer to the aperture of its gage. The butter ſeem'd to be alter'd; for the loweſt part of it was yellower, and the middle whiter than before. The upper-part was fluid.

Jan. 5. 1679. The mercury return'd, by degrees, to its wonted height.

Jan. 9. The mercury was ſomewhat higher.

Jan. 28. The mercury was little changed. I open'd the receiver, and found that part of the butter, contiguous to the leather, ſpread over the cover, to be white, and of a very unpleaſant taſte. The butter, more remote from the leather, was yellow, and ſomething fetid, tho' edible. But the

the lark being roasted, was grateful to the palate, tho' it had been kept 34 days. This experiment seems to shew, that hot melted butter is not very successfully used to preserve flesh. PNEUMATICS.

(186.) *Jan.* 4. 1679. I included boil'd flesh in an exhausted receiver, stopp'd with a screw; and fill'd the interstices, exactly, with broth of the same flesh, which seem'd a little too salt. Whilst I set the screw, all things in the receiver were compress'd; and the mercury ascended to the height of six inches into the gage; but it soon return'd to its wonted height. *Boiled flesh in vacuo.*

Jan. 28. The air was, gradually, more consumed, so that the mercury now descended eight inches below its usual standard. I open'd the receiver, and found the flesh very sweet and tender. The broth, also, had an acidish, but a very grateful taste.

This experiment shews, that boil'd flesh may be long preserv'd good; which is a great convenience at sea, where, perhaps, there might be no occasion for salt meat. For, after raw flesh hath been included in screw'd vessels, as long as experience shews there is no danger of its corrupting, it may be taken out, and, being perfectly boil'd, be again included in the same receivers; and so, doubtless, it may be kept for a great while without salt.

(187.) *Jan.* 30. 1679. I put raw flesh into two receivers; to the first, I added pepper and cloves; in the second, I mixed nothing. *Raw flesh included, with and without spice.*

Feb. 11. The height of the mercury, in the first receiver, was three inches; in the second below 1 $\frac{1}{2}$.

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

Feb. 13. In he first receiver, the height of the mercury was above six inches; in the second, three. I boil'd the flesh of the first receiver, and it was very pleasant, and tender.

Feb. 14. The height of the mercury, in the second receiver, was five.

Feb. 19. The height of the mercury, in the second receiver, was eight.

Feb. 20. The height of the mercury, in the second receiver, was 11. I boil'd the flesh, and found it very tender, tho' it remain'd over the fire *in Balneo Mariæ*, only for three quarters of an hour. I put some part of this flesh, before it was boil'd, into a receiver, and filled all the vacuities, as exactly as I could, with the same flesh, to try how long the flesh might be preserv'd, when the air was thus excluded.

Feb. 28. The mercury ascended very little.

March 20. The height of the mercury was about 16 inches. I open'd the receiver, and the flesh seem'd of a pleasant taste, yet inclining to corruption.

(188.) *February* 10. I put raw beef into three receivers: in the first, the beef was season'd with pepper and cloves; in the second, it was encompass'd with salt-water; in the third, I put neither salt nor spice. *Raw beef included with spice, with salt-water, and a-part.*

Feb. 19. Four days ago, the mercury ascended in the third receiver; n the first, also, it began to ascend; but, in the second, not at all.

Feb.

PNEUMATICS.

Feb. 21. In the firſt receiver, the height of the mercury was four inches and a half; in the third, ten; but in the ſecond, there was no aſcent at all.

Feb. 25. The height of the mercury in the firſt receiver was ſix; in the third, nineteen; in the ſecond half an inch.

Feb. 26. This night, there was no aſcent of the mercury in any of the receivers. I open'd the third, and the fleſh, after boiling, was very good.

By the former experiment, ſpices ſeem to hinder the production of air; but the preſent experiment proves the contrary. Whence this contrariety ſhould proceed, I know not; unleſs, perhaps, becauſe, I had left a ſpace large enough for the air in theſe receivers; but in the former experiment, fill'd all as exactly as I could with fleſh.

March 9. The height of the mercury, in the firſt receiver, was eight inches; in the ſecond, none.

March 12. The height of the mercury in the firſt receiver was twelve; in the ſecond, one.

April 3. The height of the mercury in the firſt receiver was eleven; but in the ſecond it exceeded not, one. I open'd the receiver, and boiling the fleſh, found it very tender, and of an excellent taſte.

Hence the ſaltneſs of water, included with fleſh, ſeems to hinder the production of air; but there being ſo ſmall a quantity of water, compar'd with the quantity of fleſh, I rather incline to think, that leſs air was produced in the ſecond receiver, becauſe it was more exactly fill'd. And, indeed, freſh water being uſed inſtead of ſalt, has the ſame effect; but the chief art to preſerve fleſh without ſalt, conſiſts in excluding all air from it, and making a great compreſſure in the receiver.

Theſe experiments, about the preſervation of aliment, may be very uſeful in tranſporting fruits, veniſon, &c. from remote places, and towards affording better nouriſhment to mariners.

Boiling and diſtillation practiſed in vacuo; *and firſt, beef boiled in an exhauſted receiver.*

(189.) *Decemb.* 12. 1678. I put two ounces, and ſix drams of beef into an exhauſted receiver, able to hold twenty-two ounces of water; then I left it in boiling water for three hours; which done, I expos'd it to the air, to cool for a whole night: afterwards, uſing my pneumatic engine, I perceiv'd, that the air, formed in the receiver, could ſcarce ſuſtain three inches of mercury: whence fleſh in boiling, cannot form air enough to make an entire preſſure in a receiver, capable of holding a double weight of water: that is, if you include one pound of fleſh in an exhauſted receiver, able to hold two pounds of water, it will not generate air enough to remove the cover from the receiver, unleſs heat greatly contribute to produce the effect: but, our fleſh, I confeſs, was not boil'd enough.

(190.) *Decemb.* 23. I inclos'd three ounces of raw beef in a receiver able to hold thirty-two ounces of water; and in boiling, after it had been long on the fire, the cover was forc'd from the receiver, and ſo ſuffer'd the vapours to paſs out: but being preſently ſhut again, and the fire remov'd, the receiver ſoon loſt its internal preſſure; ſo that being re-plac'd on the fire, it was a long time before it could force away the cover a ſecond time. I tried this

this again, and again; and unless the receiver had been expos'd to a very strong fire, the cover would never have been remov'd; but if the fire burns well, sweet exhalations continually pass out. PNEUMATICS.

Decemb. 24. The receiver having been cool'd, during the whole night, was, this day, by the use of the pneumatic engine, almost wholly evacuated. Whence we seem to have a confirmation, that the divulsion of the cover is not made by that air, which can keep the form of air, but from the steams exhaling from the flesh, and subsiding again therein; provided they be kept in, as they easily may, if we use not too fierce a fire to the evacuated receiver, whereby the loss of those sweet vapours may be prevented.

(191.) *Jan.* 21. 1679. I put paste, without leaven, into an exhausted receiver; and included another part of the same paste in a second receiver, full of common air. I inclos'd these two receivers *in Balneo Mariæ*, stopp'd with a screw; and when they had remain'd there, for three hours, expos'd to a moderate fire, I open'd the receivers: the paste *in vacuo* I found reddish on the superficies; but the other had admitted water; and the paste was not boil'd enough: and, therefore, I put both receivers again *in Balneo Mariæ*, where they staid a whole night. *Paste boil'd in* vacuo, *and in common air.*

Jan. 22. This morning, I found the *Balneum Mariæ* quite cold; and the paste, when taken out, was boil'd enough, but cover'd with no crust. That which I included *in vacuo*, was interspers'd with many cavities, but it seem'd too insipid; the other had no cavities, but a more pleasant taste. Both the receivers were found almost wholly empty'd of air.

(192.) *Feb.* 3. 1679. I inclos'd leaven'd paste *in vacuo*, and, as soon as it had fill'd its receiver with factitious air, transmitted it into the receiver; I used to boil flesh *in Balneo Mariæ*; but, when the paste was thus remov'd, it pitch'd much; yet, when it had remain'd for three hours in a hot *Balneum Mariæ*, the bread made of it was interspers'd with many cavities, but cover'd with no crust. *Leaven'd paste boil'd* in Balneo Mariæ, *after it had yielded its air* in vacuo.

Feb. 5. I repeated the experiment, but now the paste was included *in vacuo*, in the same receiver, which was afterwards put *in Balneo Mariæ*; and therefore, there was no need to remove the paste, and expose it to the air. Hence, the bread made thereof, was much lighter than the former.

(193.) *Feb.* 12. I included rosemary, with water, in the distilling vessel; and, when the air was pump'd out, I put the vessel *in Balneo Arenæ*, and there came over a water of a very sweet smell, and some drops of essential oil of a very sweet scent, and not empyreumatical. But when I open'd the stop-cock, to let in the air, the noise so soon ceas'd, that I judg'd much air was produced from the rosemary. *Rosemary and water distill'd* in vacuo.

Feb. 13. I put the same rosemary into the same evacuated vessel, and administred a more intense fire, yet could extract no oil, sweet, or fetid; and the water was less fragrant than the former.

(194.) *Feb.* 10. 1679. I boil'd one pound of flesh *in vacuo*, in a vessel describ'd, which would contain almost four pounds of water: its upper part, which was made of glass, held the mercurial gage; by the help whereof, I perceiv'd, that the mercury ascended not three inches, tho' *Flesh boil'd in* vacuo.

PNEUMATICS. the flesh had boil'd for three hours, and more. It was not boil'd enough, and its taste was ungrateful: the liquor, form'd of the condens'd vapours, had, also, an unpleasant taste.

Feb. 11. I repeated the experiment, but now sprinkled the flesh with pepper and cloves: the mercury ascended to the height of six inches, tho' the flesh boil'd no longer than the other: it seem'd very grateful to the palate; and the liquor, form'd from the vapours, had a most pungent taste of pepper; but contracted nothing ungrateful from the flesh, as in the former experiment.

From these experiments, made *in vacuo*, it seems, that such vessels may be very useful for distilling, and boiling of such bodies, as contain thin, and very volatile spirits: for every thing will here be preserv'd, and nothing be suffer'd to fly away.

Boiling in screw'd vessels or digestors; and first, beef and water boil'd in Balneo Mariæ.

(195.) *Jan.* 29. Eight days ago, I fill'd a screw-vessel, with beef and water together; and when it had continu'd over a moderate fire for eight or nine hours *in Balneo Mariæ*, stopp'd also with a screw, I took the flesh out; but it was boil'd a great deal too much, and the taste of it was very unpleasant. I boil'd other beef in the same vessel, after the same manner; only this was season'd with pepper and cloves, and remain'd expos'd to the fire but for three hours. This flesh preserv'd a most pleasant taste. I boil'd other flesh, without spices, for three hours, in the same vessel, and after the same manner: when the flesh was taken out, it tasted well; whence I conjectur'd, that what spoil'd the first flesh, was over-boiling: yet the spices may be convenient to correct some part of the ungrateful taste; for I left a place to condense the vapours, in the top of the vessel, and found, that the liquor, there formed, had an unpleasant taste; but not so when the flesh was season'd with pepper and cloves.

Apples boil'd in a screw-vessel.

(196.) *Jan.* 29. I boil'd apples, after the same manner as I did the flesh, before mention'd; but mix'd no water with them. They were set upon a moderate fire, for almost two hours. They were very soft, and of a very good taste; but some pieces, which lay in the upper part of the receiver, where the vapours ascending from the lower part condens'd, were of an unpleasant taste; and the drops, form'd from the same vapours, had an ungrateful scent.

Flesh season'd with spice boil'd in a screw-vessel.

(197.) *February* 4. I inclosed flesh, with pepper and cloves, in a receiver stopped with a screw, but used no water to fill up the interstices; only compressed the flesh as much as I could, and then put the receiver *in Balneo Mariæ*, already hot, and stopp'd it with a screw: when it had remained there, over a moderate fire, for an hour, the flesh was rather over-boil'd than under; but, when I open'd the *Balneum Mariæ*, all the water burst out of it, with a great force; the liquor being hot, and now finding vent.

Feb. 5. I inclosed some part of this flesh in a receiver, stopp'd with a screw.

March 12. The flesh included five weeks ago, was, this day, found very good. I do not doubt, but that perfect boiling contributed something to

its

its preservation: for I find, by experiments made upon other bodies, that boiling, the more perfect it is, hinders fermentation the more. PNEUMATICS.

(198.) *February* 10. I boil'd a cow-heel, after the same manner as I had done the flesh above-mention'd; but left it, for four hours, or more, upon a moderate fire: then, the vessels being unstopp'd, we found the flesh excellently well boiled, and the bones so soft, that they might be easily cut with a knife, and eaten. *A cow-heel boil'd till the bones were tender.*

Feb. 12. I repeated the experiment, and let the vessels remain exposed to the fire for twelve hours; and tho' the water of the *Balneum Mariæ* every where secured the vessel immersed in it, yet the flesh had contracted a very empyreumatical taste and smell; but the juice, which, in the former experiment, concreted into a very firm gelly, did not here congeal at all.

Hence it appears, that many bones, and hard tendons, which we daily throw away as unprofitable, may, by the help of a *Balneum Mariæ*, stopp'd with a screw, be converted into good nourishment.

(199.) *February* 10. I boil'd a fish, after the same manner, in a screw'd *Balneum Mariæ*, but mix'd no water therewith. The fish remain'd upon the fire for two hours only; when, the vessel being cool'd and open'd, it was found of a very good taste; and its bones were so soft, that they yielded to the pressure of the finger; and the head of it might be eaten like its flesh. The juice of it, in a short time, concreted into a gelly of a hard consistence. *A fish boil'd in a screw'd* Balneum Mariæ.

This method is useful for boiling such fish as are very bony.

(200.) *February* 15. I put hart's-horn into a receiver, to be stopp'd with a screw, and fill'd the interstices with water; I included the receiver, thus stopp'd, in a screw'd *Balneum Mariæ*, and so exposed it, for four hours, to a moderate fire: the vessels being open'd, the hart's-horn was found soft, and the juice soon concreted into a very firm gelly. *Hart's-horn boil'd soft.*

Feb. 17. I repeated the experiment, but no water was included with the hart's-horn, and the fire lasted six hours under the *Balneum Mariæ*; after this, the hart's-horn was found very soft; but a little juice had sweat out of it, and adhered to the external parts of the hart's-horn, like drops of gelly.

The excellency of such a *Balneum Mariæ* appears from this experiment; for since even hart's-horn can be boil'd by means thereof, without water, all the fresh water, usually consumed in boiling flesh at sea, may be preserved for other uses.

A

DEFENCE

OF THE

Phyſico-Mechanical Experiments,

AGAINST

The Objections of FRANC. LINUS; his Hypotheſis examined, and his Anſwers to particular Experiments conſider'd.

The objections againſt the air's ſpring examin'd.

OUR author confeſſes, that the air hath both a ſpring and weight; but denies that ſpring to be great enough to perform what I aſcribe thereto; and, particularly, labours to prove it unable, in a cloſe place, to ſuſtain the mercury in the *Torricellian* experiment. For, ſays he, "if a tube, only twenty inches long, be not entirely fill'd with quick-"ſilver, but a ſmall ſpace be left betwixt it, and the finger that cloſes the "upper end, with nothing but air there; and the tube be open'd at the "bottom, the finger will not only be drawn downwards; but the quick-ſil-"ver will deſcend, conſiderably; that is, as far as ſo ſmall a parcel of air "can be ſtretch'd by the deſcending weight; and, therefore, if, inſtead of "air, any other liquor, not ſo eaſily extended, be here uſed, the quick-ſilver "will not fall: but, if the external air cannot ſuſtain twenty inches of mer-"cury, how ſhould it ſupport twenty-nine and a half?" But to this argument, he has himſelf furniſh'd us with an anſwer in theſe words. "But, "you'll ſay that the mercury deſcends, becauſe 'tis impell'd downwards, by "the air dilating itſelf by its own ſpring." Which I think ſufficient for "the objection, notwithſtanding the two exceptions he makes to it.

For, firſt, when he ſays, that then "the finger ought rather to be repell'd from, than fix'd to the tube, ſince the expanſion is made every way;" he conſiders not, that tho' the included air extends itſelf at firſt, every way, yet the expanſion, in our caſe, muſt neceſſarily be made downward; becauſe, the finger that ſtops the tube, being expos'd on the upper parts, and the ſides, to the external air, has the whole weight and preſſure of the atmo-ſphere

ſphere upon it; and, conſequently, cannot be thruſt away, but by a force, able to ſurmount that preſſure; whilſt, on the lower ſide of the included air, there is the weight of the whole mercurial cylinder to aſſiſt the ſpring of the air to ſurmount the weight of the atmoſphere, that gravitates upon the ſtagnant mercury. So that the air included, endeavouring to expand itſelf, finding no reſiſtance upwards, and a conſiderable one downward, it is very natural, that it ſhould expand itſelf that way, where it finds the leaſt reſiſtance: as will happen, till the ſpring of the air be ſo far weaken'd by expanſion, that its preſſure, together with the weight of the mercury, that remains ſuſpended, will but balance the preſſure of the outward air upon the ſtagnant mercury. And, if, inſtead of quick-ſilver, you employ water, and leave, as before, in the tube an inch of air, and then inverting it, open it under water, the included inch of air will not dilate itſelf near half ſo far, as it did when the tube was almoſt fill'd with mercury; becauſe, the weight of ſo ſhort a cylinder of water does but equal that of between an inch and an inch and half of quick-ſilver; and, conſequently the internal air is far leſs aſſiſted to dilate itſelf, and ſurmount the preſſure of the outward, by the cylinder of water, than by that of mercury.

As for what our author ſays, that, " if, inſtead of air, or water, ſome " other liquor be left at the top of the tube, the quick-ſilver will not de- " ſcend;" we can readily ſolve that phenomenon, ſince water has either no ſpring at all, or but an exceeding weak one; and ſo ſcarce preſſes, but by its weight, which, in ſo ſhort a cylinder, is inconſiderable.

Hence we ſee, why the finger is ſo ſtrongly faſten'd to the upper orifice of the tube it ſtops: for the included air, being ſo far dilated, that an inch, for example, left, at firſt, in the upper part, reaches twice or thrice as far, as before the deſcent of the quick-ſilver, its ſpring muſt be proportionably weaken'd; and, conſequently, that part of the finger within the tube will ſuſtain much leſs preſſure it, from the dilated internal air, than the upper part of the ſame finger, from the unrarify'd air without. By which means, the pulp of the finger will be thruſt in.

Our author's ſecond objection runs thus. " If you take a tube, open at " both ends, of a conſiderable length, ſuppoſe forty inches, fill it with " mercury, place your finger on the top, and open the lower end; the mer- " cury will deſcend to its wonted ſtation, and your finger, on the top, be " ſtrongly drawn within the tube, and ſtick cloſe to it. Whence, again, " it is evident, that the mercury at its own ſtation, is not there ſuſtain'd " by the external air, but by a certain internal cord, whoſe upper end, " being faſten'd to the finger, draws, and fixes it, after this manner, in " the tube."

But this argument, being much of the ſame nature with the former, the anſwer made to that, may ſerve here, alſo; eſpecially, becauſe, in the preſent caſe, the pulp of the finger ſuſtains leſs preſſure on the inſide of the tube, than in the other; the preſſure of the atmoſphere being here kept off from it, by the ſubjacent mercury; whereas there is nothing of that preſſure abated againſt the other part of the finger, that kept it off from the deſerted cavity of the tube; only from the pulp, contiguous to the tube, there may be ſome taken off, by the weight of the glaſs itſelf. But as for that part of the finger, which immediately covers the orifice, whether there be any ſpring in its own fibres, or other conſtituent ſubſtance, which finding no reſiſtance in the place deſerted by the quick-ſilver, may contribute to its ſwelling; he, who duly conſiders the account already given of this intruſion, will find no need of our author's internal *Funiculus*, which ſeems more difficult to conceive, than to ſolve the phenomena, in controverſy, without it.

Our author propoſes this as a clear demonſtration; and it is, indeed, the principal thing in his book. "Take a tube of about 20 inches long, "with both ends open, let its orifice be immers'd in ſtagnant mercury, and, "one finger being plac'd underneath, that the mercury to be pour'd in, may "not run thro', let it be fill'd with quick-ſilver, and then another finger ap-"ply'd to cloſe its orifice. This done, if you take away the lower finger, the "upper will be ſtrongly drawn, and ſuck'd into the tube, and adhere to it "ſo firmly, that it will elevate the tube itſelf, with all the quick-ſilver, "and make it hang pendulous in the veſſel. Since, then, the quick-ſilver "in ſuch a tube muſt be thruſt upwards by the preponderating air; it can "never be hence explain'd, how the finger is ſo drawn downwards, and "made ſo ſtrongly to adhere to the tube. For it cannot, by the air for-"cing upwards, be drawn downwards." In anſwer hereto, I alledge, that a good account may be given of this experiment, upon our hypotheſis, which is ſufficient to ſhew the argument not to be unanſwerable.

I deny then, that the finger is drawn downward or made, by ſuction, to adhere to the tube, otherwiſe than we have already explain'd.

He ſays indeed, that the air, which thruſt up the quick-ſilver, cannot ſo ſtrongly draw down the finger: as if the air were not a fluid body, but a ſingle and entire pillar of ſome ſolid matter.

However, when the tube is fill'd with quick-ſilver, the finger that ſtops the upper orifice is almoſt equally preſs'd above, and at the ſides, by the contiguous air; and when the lower finger is remov'd, the cylinder of mercury, which before gravitated upon the finger, comes to gravitate upon the ſtagnant mercury, and, by its intervention, preſſes againſt the outward air; ſo that, againſt thoſe parts of the finger, that are contiguous to the air, there is all the wonted preſſure of the external air; but againſt that pulp contiguous to the mercury, not ſo much preſſure, as againſt the other parts of the finger, by, about two thirds; becauſe the mercurial cylinder, in this experiment, is ſuppos'd to be twenty inches high; and if it were but a little more than thirty inches high, the weight of the quick-ſilver would take off not two thirds only, but the whole preſſure of the outward air from the pulp of the finger. For, in that caſe, the quick-ſilver would quite deſert it, and ſettle below it. Wherefore, ſince I have before ſhewn, that the preſſure of the outward air is taken off from the body that remains in the upper part of the tube, according to the weight of the liquor ſuſpended in it; and ſince, on our hypotheſis, the preſſure of the

the outward air is able to keep thirty inches of quick-ſilver, or thirty-two, or thirty-three feet of water ſuſpended; 'tis no wonder, if a preſſure of the ambient air, equal to the weight of a cylinder of water, of near twenty-two feet long, ſhould be able to thruſt in the pulp of the finger, at the upper orifice of the tube, and make it ſtick cloſely to the top of it. PNEUMATICS.

I know our author affirms, that no preſſure from without, can ever effect ſuch an adheſion of the finger to the tube: but this ſhould be proved. Nor could I, upon trial, find the adheſion of the finger to the tube, to be near ſo ſtrong as our author relates: but, if you endeavour to thruſt the pulp of your finger into the orifice of the tube, you may, through the glaſs, perceive it to be manifeſtly tumid, in the cavity of the pipe. And if, by preſſing your finger againſt the orifice, you ſhould not make the pulp adhere quite ſo ſtrongly to the tube, nor ſwell quite ſo much within it, as may happen in ſome mercurial experiments; it is to be conſider'd, that the air being a fluid, as well as a heavy body, does not preſs only againſt the upper part of the finger, but, upon as much of it as is expoſed thereto, almoſt every way uniformly and ſtrongly; and ſo, by its lateral preſſure, thruſts the pulp of the finger into the orifice, where there is leaſt reſiſtance.

Hence, we need not borrow the objection, our author offers to lend, that, in the experiment under conſideration, the quick-ſilver is preſs'd downward by the ſpring of ſome air lurking betwixt it and the finger; (tho' ſuch a thing might eaſily happen) ſince we lately proved the contrary. And as for what he adds to confirm his argument, that "if the preponderating air "ſucceed in the place of the lower finger, which was withdrawn; that "is, if it ſuſtain the quick-ſilver after the ſame manner, as by the "lower finger apply'd under it; it is manifeſt, that the finger, on the "top, ought to be no more drawn downwards, after the lower finger "is removed, than before: but, experience teacheth the contrary;" we muſt conſider, that the tube being ſuppoſed perfectly full of mercury, the finger, which ſtops the lower orifice, is uſually kept ſtrongly preſs'd againſt it, leſt any of that ponderous fluid ſhould get out: ſo that tho' the lower finger keeps up the mercury in the tube, and the preſſure of the outward air would do ſo too; yet there is this difference, that the preſſure of the atmoſphere, depending upon its weight, cannot be increaſed, and weaken'd as we pleaſe, like the undermoſt finger. And, therefore, whereas the atmoſpherical cylinder will not ſuſtain one of quick-ſilver, above 30 inches high, thoſe who make the *Torricellian* experiment, often keep up, with the finger, a mercurial cylinder of, perhaps, 50 inches: ſo that, in our caſe, before the removal of the under finger, the pulp of the uppermoſt muſt ſuſtain about the ſame preſſure, where it is contiguous to the mercury, as the other part of the ſame finger; after the removal of the under finger, there is as much preſſure of the atmoſphere taken off from the pulp, as balances a cylinder of quick-ſilver 20 inches high.

Our

PNEUMATICS.

Our author's laſt experiment is thus propoſed: "This opinion is falſe, "becauſe thence it would follow, that quick-ſilver, thro' a like tube, might "be ſuck'd with the ſame eaſe out of a veſſel, as water; which is contrary "to experience: for, according to this opinion, that the fluid underneath, "whether water, or mercury, may ſo aſcend, no more is required, than "that the air, in the tube, be drawn upwards by ſuction; when, the "liquor below, will immediately aſcend, being impell'd by the external air, "which now preponderates." But we formerly ſhew'd, that when the mercurial cylinder, which reſts upon the ſtagnant mercury, has, at the other end of it, air kept from any communication with the atmoſphere, that included air has ſo much of the preſſure of the external air taken off from it, as balances the mercurial cylinder. And the finger expoſed to the whole preſſure of the ambient air, in ſome of its parts, and in others but to the much fainter preſſure of the included air, ſuſtains an unuſual preſſure from the preponderating power of the atmoſphere. Thus, the thorax, and the muſcles of the abdomen, which ſerve for reſpiration, ſuſtain the preſſure of the whole ambient air; tho' theſe muſcles are able, without any conſiderable reſiſtance, to dilate the thorax; becauſe, as faſt as they open the cheſt, and, by dilating it, weaken the ſpring of the included air, the external air ruſhing in, for want of the uſual reſiſtance there, keeps that within the thorax, in an equilibrium to that without. We ſay, then, that if a cylinder of mercury be, by ſuction, rais'd in the tube to any conſiderable height, the preſſure of the air in the thorax, is leſſen'd by the whole weight of that mercurial cylinder; and, conſequently, the reſpiratory muſcles are thereby diſabled from dilating the cheſt, as freely as uſual. But, if inſtead of mercury, you ſubſtitute water, ſo ſhort a cylinder of that takes off ſo little of the preſſure of the included air, that it comes into the lungs with almoſt its uſual ſtrength; and, conſequently, with almoſt the ſame force wherewith the external air preſſes againſt the thorax.

And there is an experiment of M. *Paſchal*'s, which ſhews clearly, that if we could free the upper part of a tube, from the preſſure of all internal air, the quick-ſilver (as our author ſays, it ſhould) would, by the preſſure of the outward air, be impell'd up into the tube, as well as water, till it had attain'd a height ſufficient to make its weight equal to that of the atmoſphere. The experiment itſelf is this: "If a glaſs ſyringe be made of a ſufficient length; "and after the ſucker is thruſt into the utmoſt orifice, it be plung'd in the "mercury, as ſoon as the ſucker is drawn out, the mercury follows, and aſ"cends to the height of two feet, three inches, and a half. And when, af"terwards, tho' no greater force be added, the ſucker is drawn higher, "the mercury ſtands, and follows no farther; whence that ſpace remains "empty, which lies between the mercury, and the ſucker." So that we may well explain our author's experiment, by ſaying, that in a more forcible reſpiration, the mercurial cylinder is rais'd higher than in a more languid one; becauſe, in the former, the cheſt being more dilated, the included air is alſo more expanded, whereby its weakned ſpring cannot, as before,

before, enable the mercurial cylinder to balance the preſſure of the ambient air. And the reaſon why the quick-ſilver is not, by reſpiration, rais'd as high as 'tis kept ſuſpended in the *Torricellian* experiment, is not the preſſure of the outward air being unable to raiſe it ſo high; but becauſe the free dilatation of the thorax, is oppoſed by the preſſure of the ambient air; which preſſure being againſt ſo great a ſuperficies, and but imperfectly reſiſted by the weakned ſpring of the air in the thorax, will be very conſiderable; ſince, in our engine, the preſſure of the external air againſt the ſucker of leſs than three inches diameter, was able to raiſe an hundred weight. And, by the way, when we ſtrongly ſuck up quick-ſilver in a glaſs tube, tho' the elevation thereof proceeded from our author's *Funiculus*, contracting itſelf every way; and tho' there be a communication betwixt the internal ſurface of the lungs, and the cavity of the tube; yet we feel not, in our lungs, any endeavour of the ſhrinking cord to tear off that membrane they are lined with. PNEUMATICS.

Our author further ſays, that "the ſpring of the air can perform neither more nor leſs, in a cloſe place, than its equilibrium in an open one." But I allow of this opinion, only in ſome caſes; for, in others, we have performed much more by the ſpring of the air, which we can, within certain limits, increaſe at pleaſure, than can be perform'd by the bare weight, which, for ought we know, remains always nearly the ſame. And of this difference, we formerly gave an inſtance; when, by compreſſing the air, in the receiver, we impell'd the mercurial cylinder higher than the ſtation at which the balance of the air ſuſtains it.

Our author adds, that "ſince the experiments of the adheſion of the finger, *&c.* ſucceed alike in a cloſe and open place, the arguments produced againſt the equilibrium, make alſo againſt the ſpring of the air." This has, already, been anſwer'd; but ſince he ſays, that the experiments, concerning the adheſion of the finger, *&c.* ſucceed equally in a cloſe and open place, I wiſh he had told us what way he took to make them; for, in ordinary rooms, there ſcarce ever wants a communication betwixt the internal and external air, by means whereof, the weight of the atmoſphere has its effect within the room.

Our author ſuppoſes, that what we aſcribe to the ſpring, and weight of the air, is performed by a ſort of *Funiculus*, conſiſting of a thin ſubſtance, greatly expanded; which, lying between two bodies, endeavours to contract itſelf, and to bring theſe bodies together, to avoid a vacuum; by nature's abhorrence whereof, he, at length, ſolves all phenomena. *The funicular hypotheſis examin'd.*

His firſt argument for this is, that the finger would not be drawn down, by the deſcent of the mercury in the *Torricellian* tube, were there not a *Funiculus*; and that, were no thin ſubſtance there extended, a vacuum muſt enſue.

But this argument being deduced from the ſuction of the pulp of the finger, upon the deſcent of the mercury, has been anſwer'd already. Another argument, which he alledges againſt a vacuum, is, the tranſparency of that part of the tube, where 'tis ſaid to be: for, were there a vacuum, PNEUMATICS. he ſays, it would be like a black pillar, neither able to afford any thing viſible, nor to permit objects to appear thro' it.

But the invalidity of this aſſertion appears from the doctrine of the atomiſts, who teach, that light is made up of ſuch ſubtile effluvia, as are able to penetrate glaſs, and, therefore, may leave many vacuities, tho' the cavity of the cylinder ſeems full of it; and no doubt, were the parts of the lucid matter contracted, they would not fill one tenth of that ſpace; ſince the ſmoke, which fill'd our receiver, ſo as to make it appear opake, poſſeſs'd, when condens'd, only a ſmall part thereof.

Thus a room may appear full of the ſmoke of a perfume, tho', if all the corpuſcles that compoſe the ſmoke were re-united, they would make up but a ſmall paſtil. A little camphire, alſo, will fill a room with its odour; but having, in well clos'd glaſſes, caught the fumes of it driven over by heat, and again reduced them into true camphire, I found its bulk very inconſiderable, in compariſon of the ſpace it poſſeſſes, when its ſcented corcorpuſcles are ſcatter'd thro' the air.

I might add, that if the *Torricellian* experiment ſucceed in the dark, it may well be doubted, whether our author's argument will hold. For if he endeavours to prove, that the place in queſtion was full in the dark, becauſe, upon letting in the light, a light appears within it; we may reply, that this light is a new one, flowing from the lucid body, that darts its corporeal rays thro' the glaſs and ſpace in diſpute, which, for want of ſuch corpuſcles, were not, juſt before, viſible.

And, ſuppoſing light to be made by a propagation of the impulſe of lucid bodies thro' tranſparent ones, yet it will not thence follow, that the deſerted part of the tube muſt be full: in one of our experiments, tho' many of thoſe groſs aerial particles, that appear neceſſary to convey a languid ſound, were ſoon drawn out of the receiver, yet there remain'd ſo many, that the others were not miſs'd, till a far greater number was extracted; and thus there may remain matter enough to tranſmit the impulſe of light, tho' betwixt the particles of that matter there ſhould be numberleſs vacuities: yet our author pretends to prove abſolutely, that there is no vacuity in the diſputed ſpace. And ſhould a *Carteſian* ſay, the deſerted part of the tube is filled with *Materia ſubtilis*, he muſt allow the preſſure of the outward air to be the cauſe of the ſuſpenſion of the quick-ſilver; for tho' the *Materia ſubtilis* may readily fill the ſpaces deſerted by the mercury; yet that within the tube cannot hinder ſo ponderous a liquor from ſubſiding as low as the ſtagnant mercury: ſince the whole tube, being pervious to that ſubtile matter, it may, with like facility, ſucceed, in whatever part of it ſhall be forſaken by the quick-ſilver.

Our author's next argument is, that the mercurial cylinder, reſting at its wonted ſtation, does not gravitate; as appears by applying the finger to the immerſed, or lower orifice of the tube: whence he infers, that it muſt, of neceſſity, be ſuſpended from within the tube. And, indeed, if the finger be applied to the open end of the tube, before 'tis quite lifted out of the ſtagnant mercury, the experiment will ſucceed;

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the finger, however, will feel a gravitation, or preſſure, of the glaſs-tube, and the contained mercury, as of one body; but no ſenſible preſſure of the mercury a-part, as if it endeavoured to thruſt away the finger from the tube. Now, according to our hypotheſis, the mercurial cylinder, and the air, balancing one another, the finger ſuſtains not any preſſure, ſenſibly differing from the ambient air, that preſſes againſt the nail, and ſides of it, and from the included quick-ſilver that preſſes againſt the pulp. But if the mercurial cylinder ſhould exceed the uſual length, then the finger would feel ſome preſſure from that additional quick-ſilver, which the air does not aſſiſt the finger to ſuſtain: ſo that this phenomenon may as well be ſolved on our hypotheſis, as on our author's. But how comes the mercury in the tube, when of a due altitude, to run out, upon removal of the finger beneath, if it be ſuſtained only by an internal cord; and, when that ſuſtains it, to reſemble a ſolid body, if the preſſure of the external air has no ſhare in it?

PNEUMATICS.

If it be here ſaid, that the finger muſt feel great pain, by being ſqueezed betwixt a pillar of thirty inches of quick-ſilver, and an equivalent preſſure from the atmoſphere; we muſt obſerve, that, in fluids, a ſolid has not that ſenſe of preſſure from ſurrounding bodies, which men are apt to imagine; as appears from divers: and I am informed, that the learned *Maignan*, tho' he purpoſely thruſt his hands, three or four palms deep, into quick-ſilver, his fingers were not ſenſible of any weight, or preſſure.

Laſtly, our author tells us, that "thoſe remarkable vibrations the "quick-ſilver makes, in its deſcent, favours his hypotheſis." But this phenomenon, alſo, is eaſily ſolved on our hypotheſis: for when the experiment is made in a cloſe place, as our receiver is, mercury, by its ſudden deſcent, acquires an impetus, beſides the preſſure it has upon account of its gravity: whence it, for a while, falls below its ſtation, and thereby compreſſes the air that reſts upon the ſtagnant mercury; which air, by its own ſpring, again forcibly dilating itſelf, to recover its former extenſion, expands beyond it, and thereby impels up the quick-ſilver ſomewhat above its wonted ſtation; in its fall from whence, it again acquires a power to compreſs the air: and this reciprocation of preſſure, betwixt the quick-ſilver, and the external air, decreaſing by degrees, at length wholly ceaſes, as the mercury loſes that additional preſſure it acquired by falling from parts of the tube, higher than its due ſtation. But this way of explicating theſe vibrations, is not neceſſary in the free air; for if we conſider the atmoſphere only as a weight, and allow an impetus acquired by deſcent, the phenomenon will be eaſily explained by a balance, wherein one of the ſcales chancing to be depreſſed, they do not, till after many vibrations, regain their equilibrium.

I took a glaſs-ſiphon, whoſe two legs, unequal in length, were parallel; and both perpendicular to that part of the pipe which joined them; and poured quick-ſilver into it, till 'twas ſome inches high, and equal in both legs; then the ſiphon, being inclined, 'till moſt part of the quick-ſilver was fallen into one of the legs, I ſtopped the orifice of the other with my finger: and, erecting the ſiphon again, tho' the quick-ſilver were forced to aſcend a little in that ſtopped leg; yet, becauſe my finger prevented the air from getting away, the quick-ſilver was kept much lower in the ſtopped leg than in the other: but if, by ſuddenly removing my finger, I gave vent to the included compreſſed air, the preponderant quick-ſilver, in the other leg, would, with the mercury in the open one, make ſeveral undulations before, in both legs, it reſted in an equilibrium. Now, in this caſe, there is no pretence for a *Funiculus* of violently diſtended air, to cauſe the vibrations of the mercury.

But there are many particulars which render the funicular hypotheſis improbable.

And, firſt, our author acknowledges, that quick-ſilver, water, wine, &c. as well one as another, will deſcend in tubes, exactly ſeal'd at the top, in caſe the cylinder of liquor exceed the weight of a mercurial cylinder of twenty-nine inches and a half, but ſubſide no longer than till it is a balance to a cylinder of quick-ſilver of that height. Now it's very ſtrange, that, whatever the liquor be, there ſhould be juſt the ſame weight, or ſtrength, to extend them into a *Funiculus*. And this is the more ſurprizing, becauſe our author makes ſo great a difference betwixt the diſpoſition of bodies of various conſiſtences to be extenuated into a *Funiculus*, that he will not allow any human force able to produce one by the divulſion of two flat marbles, in caſe the contact of their ſurfaces were ſo exquiſite, as quite to exclude all air; tho' his reaſoning plainly agrees with experience, that adhering marbles may be forcibly ſever'd; and, therefore, according to him, the ſuperficial parts may be diſtended into a *Funiculus*, that prevents a *Vacuum*. But our hypotheſis labours not under this difficulty; for the weight of the external air, being that, which keeps liquors ſuſpended in ſeal'd tubes, it matters not of what nature or texture the ſuſpended liquor is, provided its weight be the ſame with that of a mercurial cylinder equiponderant to the aerial one.

In the next place, I obſerve, that the account our author gives of his *Funiculus*, is much more ſtrange than ſatisfactory, and not made out by any unqueſtionable parallel operations of nature: whereas, the weight and ſpring of the air may be inferr'd from ſuch certain experiments, as are not concern'd in the preſent controverſy. For the gravity of the air may be manifeſted by a pair of ſcales; and its ſpring is diſclos'd ſo clearly in wind-guns, and other inſtruments, that our author does not deny it. But in the explanation of his *Funiculus*, he would have us remark two things; firſt, "that the quick-ſilver which fills the whole tube, doth not only touch the "top of it, but firmly ſtick to it; and that the finger adheres to the mercury; "ſince tho' the orifice of the tube be oil'd, that will not hinder it from ſtick-"ing, as firmly as before." But two bodies, by truſion, may eaſily be made to ſtick together, as much as the tube and finger do, tho' one of them be oil'd; beſides, this adheſion of the finger to the tube will happen, not only when the ſurface of the included quick-ſilver is contiguous to the finger, but many inches below it. Water and quick-ſilver, he ſays, aſcend

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by ſuction, " becauſe the parts of the air included in the tube, are now ſo " firmly glued to one another, that they make a ſtrong chain, whereby the " water and quick-ſilver are drawn up." Which way of wreathing a little rarified air into ſo ſtrong a rope, is highly improbable. Pneumatics.

Secondly, he ſays, that " the rarifaction, or extenſion of a body, ſo " as to make it take up more ſpace, is not only cauſed by heat, but by " diſtenſion, or, a certain disjoining power; as condenſation is not only " made by cold, but, alſo, by compreſſion." And, 'tis true, and obvious, that the condenſation of bodies, (taking that word in a large ſenſe) may be made as well by compreſſion, as cold. But, I wiſh he had more clearly expreſs'd, what he means, in this place, by that rarifaction, which, he ſays, is to be made by a disjoining power; whereof, he tells us, there are innumerable inſtances. For, as far as may be gather'd, from the three examples he ſubjoins, 'tis only the air that is capable of being ſo extended, as his hypotheſis requires quick-ſilver, and, even ſtones, to be. And, how will he prove, that even air may be thus extended, to fill two thouſand times the ſpace it poſſeſs'd before? For, that the ſame air, adequately fills more ſpace at one time, than another, he proves but by the ruſhing of water into the evacuated glaſs, and almoſt filling it; which, he ſays, is done by the diſtended air, that contracting itſelf, draws up the water with it. The explanation he gives of his *Funiculus*, is this: " Since 'tis manifeſt, " that the quick-ſilver ſticks to the top of the tube; and that rarifaction " is made by the mere diſtenſion of a body, it happens, that the deſcending quick-ſilver leaves its upper ſuperficies fix'd to the top of the tube; " and, by its weight, ſo ſtretches, and extenuates it, till it becomes eaſier to leave another ſuperficies, in like manner, than to extend that any " further. It leaves, therefore, a ſecond; and, by its deſcent, extends " that a little further, till it becomes eaſier to ſeparate a third, than to " extend that any more; and ſo on, till, at length, it hath no power to " ſeparate, or extend any more ſurfaces, when it comes to the height of " 29 ½ inches, where it reſts." Hence 'tis eaſy to diſcern, that he is oblig'd to aſſign his *Funiculus* a ſtrange and unparallel'd way of production. Now, I muſt demand, by what force, upon the bare ſeparation of the quick-ſilver, and the top of the tube, the new body, he mentions, comes to be produced; or, how it appears, that the mercury leaves any ſuch thing, as he ſpeaks of, behind it? For, the ſenſe perceives nothing of it at the top of the tube; nor, is it neceſſary to explain the phenomena; as we have formerly ſeen. And how ſhould the bare weight of the deſcending mercury, be able to extend a ſurface into a body? Beſides, the ſucceſſion of ſurfaces is a chimera: or, ſuppoſing ſome of the quick-ſilver were turn'd into a thin, ſubtile ſubſtance, yet, how comes that ſubſtance to be contriv'd into a *Funiculus* of ſo ſtrange a nature, that ſcarce any weight can break it; and that, contrary to all other ſtrings, it may be ſtretch'd, without becoming more ſlender, and obtains other very odd properties?

Our author ſays, indeed, that " theſe ſurfaces ſeem to be ſeparated " from the quick-ſilver, and to be extended into a moſt ſlender ſtring, by

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PNEUMATICS. " the falling weight, after the ſame manner, that, in a lighted candle, ſur-
" faces, of like ſort, are ſeparated from the wax, or tallow, underneath, by " the heat above, and extenuated into a moſt ſubtile flame; which, doubt- " leſs, takes up above a thouſand times more ſpace, than the part of the " wax, of which the flame conſiſted, poſſeſs'd: ſo our *Funiculus* takes up a " thouſand times more ſpace, than the ſmall particle of mercury from " whence it aroſe." And this is the only example whereby our author endeavours to illuſtrate the generation of his *Funiculus*. But here intervenes a conſpicuous, and powerful agent, actual fire, to ſever and agitate the parts of the candle; and beſides, there is a manifeſt waſting of the wax, or tallow, turn'd into flame: and we muſt not admit that the fewel, when turn'd to a flame, really fills ſo much as twice the genuine ſpace, as the wax 'twas made of. For, the flame is little leſs than an aggregate of thoſe corpuſcles, which, before, lay upon the upper ſuperficies of a candle, and were, by the violent heat, divided into minuter particles, vehemently agitated, and brought from lying flat, to beat off one another, and make up, about the wieck, ſuch a figure as is uſual in the flame of candles, burning in the free air. Nor will it neceſſarily follow, that the ſpace, which the flame ſeems to take up, ſhould contain neither air, nor æther, or any thing beſides the parts of that flame, becauſe the eye can diſcern no other body there; for, even the ſmoke, aſcending from the ſnuff of a candle, newly extinguiſh'd, appears a dark pillar, tho' there are many aerial, and other inviſible corpuſcles mix'd with it: ſo that if all thoſe parts of ſmoke, which ſhew large in the air, were collected, and contiguous, they would not, perhaps, amount to the bigneſs of a pin's head; as may appear from the great quantity of ſteams, that, in chymical veſſels, go to make up one drop of ſpirit. And, therefore, as our author, to inforce his former example, alledges, the turning of a particle of quick-ſilver into vapour, by fire; if ſuch be the rarifaction of mercury, 'tis not at all likely to make ſuch a *Funiculus* as he talks of; ſince thoſe mercurial fumes appear, by various experiments, to be mercury divided, and thrown out into minute parts; whereby, tho' the body obtain more of ſurface, than it had before, yet, it really fills no more of true and genuine ſpace; ſince, if all the particular little parts, fill'd by theſe ſcatter'd corpuſcles, were reduced into one, as the corpuſcles themſelves often are, in chymical operations, they wou'd amount but to one whole equal to that of the mercury before rarifaction.

I farther demand, how the *Funiculus* comes by hooks, or parts proper to take faſt hold of all contiguous bodies; and even the ſmootheſt, ſuch as glaſs, the calm ſurface of quick-ſilver, water, oil, &c. and how theſe ſlender, and inviſible hooks, find innumerable loops, in ſmooth bodies, to take hold on ſo ſtrongly, as to lift up a tall cylinder of quick-ſilver; and draw inwards the ſides of ſtrong glaſſes, ſo forcibly, as to break them to pieces? 'Tis, alſo, ſomewhat ſtrange, that water, and other fluid bodies, ſhould, when the *Funiculus* once lays hold on their ſuperficial corpuſcles, preſently, like conſiſtent bodies, be drawn up, in one entire continued piece; though, even

even in the exhaufted receiver, they appear, by many figns, to continue fluid.

I know, that by calling this extenuated fubftance, a *Funiculus*, he intimates, that it has its fpring inwards, like lute-ftrings, and ropes forcibly ftretch'd ; but there is no fmall difparity betwixt them : for, in ftrings, there is requir'd either wreathing, or fome peculiar, and artificial texture of the component parts ; but, a rarifaction of air, does not infer any fuch contrivance of parts, as is requifite to make bodies elaftic. And, fince lute-ftrings, *&c.* muft, when they fhrink inwards, either fill up, or leffen their pores, and increafe in thicknefs, as they diminifh in length; our author's *Funiculus* differs widely from them ; fince it has no pores to receive the fhrinking parts ; and contracts its length, without increafing its thicknefs. Nor does it, to me, feem very probable, that when, for inftance, part of a polifh'd marble is extended into a *Funiculus*, that *Funiculus* ftrongly afpires to turn into marble again. And 'tis very unlikely, that the fpace, our author would have replenifh'd with his funicular fubftance, fhould be full of little, highly-ftretch'd ftrings, that lay faft hold on the furfaces of all contiguous bodies, and always violently endeavour to pull them inwards. For, a pendulum being fet a moving, in our exhaufted receiver, vibrated as freely, and with the ftring as much ftretch'd, as in the common air. Nay, the balance of a watch did there move freely ; which is hard to conceive, if the moving bodies were to break thro' a medium confifting of innumerable ftrings, exceedingly ftretch'd. And 'tis ftrange, if thefe ftrings, thus cut, or broken, by the paffage of thefe bodies thro' them, could fo readily have their parts re-united, and immediately be made entire again. And, in this cafe, the two divided parts of each fmall ftring, do not, like thofe of other broken ftrings, fly back from one another, but meet, and unite again ; yet, when in the *Torricellian* experiment, the tube, with the contain'd mercury, is fuddenly lifted out of the ftagnant quick-filver into the air, the *Funiculus* fo ftrangely contracts itfelf, that it quite vanifhes ; fo that the afcending mercury may rife to the very top of the tube.

But this is not all that renders our author's hypothefis improbable ; for it neceffarily fuppofes fuch a rarifaction, and condenfation, as is unintelligible.

The nature of rarifaction confider'd.

We muft here premife, that a body is commonly faid to be rarified, or dilated, when it acquires greater dimenfions than it had before ; and to be condenfed, when it is reduced to lefs dimenfions, that is, into a lefs fpace ; and that there are three ways of explaining rarifaction : for, either we muft fay, that the corpufcles whereof the rarified body confifts, depart from each other, fo that no other fubftance comes in between them, to fill up the deferted fpaces ; or, that thefe new interftices, are but dilated pores, replenifh'd, as thofe of a tumid fpunge by water, with fome fubtile ethereal fubftance ; or, laftly, that the fame body does not only obtain a greater fpace in rarifaction, and a leffer in condenfation ; but, adequately, and exactly fills it : and fo, when rarified, acquires larger dimenfions, menſions, without leaving any vacuities betwixt its component corpuſcles, or admitting any new, and extraneous ſubſtance between them.

'Tis to this laſt way of rarifaction, that our author has recourſe, in this hypotheſis ; tho', I confeſs, it appears to me ſo difficult to be con-*ſtotelian* rarifaction. For the eaſier conſideration of this matter, let us ceiv'd, that I doubt whether any phenomenon can be explain'd by it. Let us ſuppoſe, that in the *Magdeburg* experiment, he ſo often urges to prove his hypotheſis, that the undilated air, which, as he tells us, poſſeſs'd about half an inch of ſpace, conſiſted of 100 parts, 'twill not be deny'd, that as the aggregate is adequate to the whole ſpace it fills, ſo each of the 100 parts is, likewiſe, adequately commenſurate to its reſpective ſpace, which is 100th part of the whole. Now, our author ſays, that "if a body poſſeſſes twice as much ſpace, "each part of that body muſt do the ſame." Whence the whole capacity of the ſphere, which, according to him, was 2000 times bigger than the ſpace poſſeſs'd by the unexpanded air, there muſt, likewiſe, be 2000 parts of ſpace, commenſurate, each of them, to one of the aforeſaid 100th parts of air ; and, conſequently, when he affirms, that half an inch of air poſſeſs'd the whole cavity of the globe, if we will not admit, as he does not, either vacuities, or ſome ſubtile ſubſtance in the interſtices of the aerial particles, each part of air muſt, adequately, fill 2000 parts of ſpace. Now, that this ſhould be reſolutely taught, to be really, and regularly done, in the *Magdeburg* experiment, will, queſtionleſs, appear very abſurd to the *Carteſians*, and thoſe other philoſophers, who take extenſion to be but notionally different from body; and, conſequently, impoſſible to be acquir'd, or loſt, without the addition, or detraction of matter : and will, I doubt not, appear ſtrange to every one who conſiders how generally extenſion is allow'd inſeparable, and immediately to flow from matter ; and bodies to have a neceſſary relation to a commenſurate ſpace. Nor do I ſee, if one portion of air may ſo eaſily be brought, exactly to fill a ſpace 2000 times as great as that it did but fill before, without the addition of any new ſubſtance, why the matter contain'd in each of theſe 2000 parts of ſpace, may not be farther brought to fill 2000 more, and ſo on ; ſince each of theſe newly repleniſh'd ſpaces, is preſum'd to be exactly fill'd with body; and no ſpace, and, conſequently, that which the un-rarified air repleniſh'd, can be more than adequately full. And ſince, according to our author, not only fluids, but even ſolids, as marble, are capable of ſuch a diſtenſion; why may not the world be made many thouſand times bigger than it is, without either admitting a vacuity betwixt its parts, or being increas'd with the addition of one atom of new matter ?

He further alledges, that the phenomena of rarifaction cannot be explain'd, either by vacuities, or the ſub-ingreſſion of an ethereal ſubſtance ; and that there are two ways of explaining that kind of it, which he contends for.

After our author's objections againſt the two ways of rarifaction propoſed ; the one by the vacuiſts, and the other by the *Carteſians*, who admit the moſt ſolid bodies, and, even glaſs itſelf, to be pervious to an ethereal,

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or subtile matter, he attempts to explain the manner by which his own rarifaction is perform'd; and having premis'd, that the explanation of the way how each part of the rarified body becomes extended, depends upon the quality of the parts into which the body is ultimately resolv'd; and, having truly observ'd, that they must, necessarily, be either really indivisible, or endlesly divisible, he endeavours to explain the *Aristotelian* rarifaction, according to these two hypotheses. But tho' he thus proposes two ways of making out his rarifaction, yet they are irreconcilable; and he speaks of them very doubtfully, and obscurely. PNEUMATICS.

And, first, having told us how rarifaction may be explain'd, if we admit bodies to be divisible *in infinitum*, he makes an objection against the infinity of parts in a continuum, whereto he gives so dark an answer, that, I confess, I do not understand it.

And 'tis not clear to me, that even such a divisibility of a continuum, as is here supposed, would make out the rarifaction he contends for: since, let the integrant parts of a continuum be more or less finite, or infinite in number, still each part, being a corporeal substance, must have some particle of space commensurate to it; and if the whole body be rarified, for instance, to twice its former magnitude, then will each part be, likewise, extended to double its former dimensions; and fill both the place it took up before, and another equal to it; and, consequently, two places. I will not, however, pretend to affirm which of the two ways, by atoms, or by parts infinitely divisible, our author declares himself for: but, whichsoever of them it be, I think he has not intelligibly made it out; as himself seems willing to confess. So that, in his discourse of rarifaction, to which our author frequently refers, as that which should make good what seems the most improbable, he has, instead of a probable hypothesis, substituted a doctrine which himself dares not pretend capable of being well freed from the difficulties with which it may be charged.

As for the other way of explaining rarifaction, by supposing that a body is made up of parts indivisible, he is, upon this hypothesis, reduced to allow, that "one and the same part must be in two places adequately; "for since it is indivisible, and takes up a greater space than before, it "must, of necessity, be also in every point of that space; or be virtually "extended thro' all that space." When, therefore, he, presently after, affirms, that by this virtual extension of the parts, the difficulties which have, for so many ages, perplex'd philosophers, may be easily solved, he must give me leave to desire he would explain what this *extensio virtualis* is; and how it will remove the difficulties charged upon the *Aristotelian* rarifaction. For the easier consideration of this matter, let us resume what we lately suppos'd, that, in the *Magdeburgic* experiment, the half-inch of undilated air, consisted of a hundred corpuscles; I demand, how the indivisibility of these corpuscles will qualify them to make out such a rarifaction, as our author imagines? For, what does their being indivisible, in this case, but make it the less intelligible, how they can fill above

PNEUMATICS. 100 parts of ſpace? He will anſwer, they are virtually extended. But, not here to queſtion, how this indiviſibility makes them capable of being ſo; I demand, whether by an atom's being virtually extended, its corporeal ſubſtance does really fill more ſpace than it did before, or not? If it do, then 'tis a true, and real, and not barely a virtual extenſion: but ſuch an extenſion, we have ſhewn, will not ſerve the turn; and our author ſeems to confeſs as much, by deviſing this virtual extenſion, to avoid the inconveniencies to which he ſaw his doctrine of rarifaction would otherwiſe be expoſed. But if it be ſaid, that when an atom is virtually extended, its corporeal ſubſtance fills no more ſpace than before; I demand, how that which is not a ſubſtance, can fill a ſpace; and how this improper, and only metaphorical extenſion, will ſolve the phenomena of rarifaction? As how the half-inch of air, at the top of the fore-mention'd ſphere, ſhall, without a corporeal extenſion, fill the whole cavity of 2000 times its bigneſs, when the water is ſuck'd out of it, and act at the lower-part of the ſphere? For, our author teaches, that the whole globe was fill'd with a certain thin ſubſtance, which, by its contraction violently ſnatch'd up the water wherein the neck of the glaſs was immers'd. And, in a parallel caſe, he makes it his grand argument, to prove, there is no vacuum in the deſerted part of the tube, in the *Torricellian* experiment, that the attraction of the finger cannot be but from ſome real body.

Our author's *Funiculus*, alſo, ſuppoſes a condenſation, that, to me, appears incumber'd with no leſs difficulties. For, ſince he teaches, that a body may be condens'd, without either having any vacuities for the compreſs'd parts to retire into; or, having its pores fill'd with any ſubtile, and yielding matter, that may be ſqueezed out of them; it follows, that the parts of a body to be condens'd, immediately touch each other: which ſuppoſed, I demand, how bodies, that are already contiguous, can be brought cloſer, without penetrating each other? So that I ſee not how this condenſation can be perform'd, without penetration of dimenſions. In the *Magdeburgic* experiment, he tells us, that the whole capacity of the globe is fill'd with an extremely rare body; which, according to him, intercepts neither pores, nor any heterogeneous ſubſtance. Now let us conſider, that before the admiſſion of water into the exhauſted globe, there was, according to him, 2000 half-inches of a true and real body; and that, after the admiſſion of the water, there remain'd, in the ſame globe, no more than one half-inch of body beſides. Since, then, our author does not pretend, that the 1999 half-inches of matter, that now appear no more, travers'd the body of water; and ſince he will not allow, that it gets away thro' the pores of the glaſs; I demand, what becomes of ſo great a quantity of matter? For that 'tis annihilated, I ſuppoſe, he is too rational to pretend; and to ſay, that ſo many parts of matter, ſhould be retir'd into that one part of ſpace that contains the half-inch of air, is little leſs incredible: for, that ſpace was ſuppos'd perfectly full of body before; and how a thing can be more than perfectly full, who can conceive? In ſhort, according to our author's way of condenſation, two, or, perhaps,

perhaps, two thouſand bodies, may be crowded into a ſpace that is adequately fill'd with one of them apart. And, if this be not penetration of dimenſions, I deſire to be inform'd what is.

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The preſſure and ſpring of the air confirm'd.

But as the hypotheſis I am oppoſing, is a kind of inverſion of ours; ſuppoſing the ſpring, or motion of reſtitution in the air, to tend inwards, as, according to us, it tends outwards, many of the phenomena would, if it were true, be plauſibly explicable by it; the ſame motions, in an intermediate body, being, in many caſes, producible alike, whether we ſuppoſe it to be thruſt, or drawn; provided, both the endeavours tend the ſame way. But then we may be ſatisfied, whether the effect be to be aſcrib'd to pulſion, or to traction, if we can find out an experiment, wherein there is a reaſon that ſuch an effect ſhould follow, in caſe pulſion be the cauſe inquir'd after; and not, in caſe it be traction. And ſuch an *experimentum crucis* is afforded us by M. *Paſchal*, who obſerv'd, that the *Torricellian* experiment, being made at the foot, and in different parts of a very high mountain, after he had aſcended an hundred and fifty fathom, the quick-ſilver was fallen two inches and a quarter below its ſtation at the foot of the mountain; and that at the very top of the hill, it had deſcended above three inches below the ſame ſtation. Whence it appears, that the quick-ſilver being carried up towards the top of the atmoſphere, falls down the lower, proportionably to the height of the place wherein the obſervation is made: the reaſon of which, on our hypotheſis, is, that the nearer we come to the top of the atmoſphere, the ſhorter, and lighter is the cylinder of air, incumbent upon the ſtagnant mercury; and, conſequently, the leſs weight of mercury will that air be able to balance, and keep ſuſpended. And, ſince this noble phenomenon, thus clearly follows upon ours, and not upon our author's hypotheſis, it ſeems to determine the controverſy; becauſe, in this caſe, it cannot be pretended, that the deſcent of the quick-ſilver, in the tube, is caus'd from the preternatural rarifaction, or diſtenſion of the external air, when, by trying to reſtore itſelf, it endeavours to draw up the ſtagnant mercury: for, there appears no ſuch forcible dilatation of that air, as in many of the phenomena of our engine, he is here pleaſed to imagine.

To this experiment he replies but two things, which, neither ſingly, nor together, will amount to a ſatisfactory anſwer.

And firſt, he queſtions the truth of the obſervation itſelf, becauſe, having made trial on a low hill, the event did no ways anſwer his expectation. But *Gaſſendus* relates, that the obſervation was five times repeated, with circumſtances, which ſufficiently argue the diligence wherewith the experiment was made: and, I can confirm theſe obſervations, by two more made on hills in *England*. But, however the proportion of the deſcent of the quick-ſilver may vary according to the different conſiſtence, and other accidents of the air, in the particular places, and times of the experiments being made; yet all obſervations agree in this, that nearer the top of the atmoſphere the quick-ſilver falls lower, than when further from it. And, in one of theſe experiments, a determinate quantity of air being left in the tube,

 before

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We, alſo, attempted a trial, wherein we hoped to find a ſenſible difference in the weight of the atmoſphere, in a far leſs height, than that of an ordinary hill. But inſtead of a common tube, we made uſe of a weather-glaſs, and inſtead of quick-ſilver, employ'd common water in the pipe belonging to the glaſs; that ſmall changes in the weight or reſiſtance of the atmoſphere, in oppoſition to the included air, might be the more diſcernible.

The inſtrument, we made uſe of conſiſted only of a glaſs AB, with a broad foot, a narrow neck, and a ſlender glaſs pipe CD, open at both ends; the pipe ſo plac'd, that the bottom of it, almoſt reach'd to the bottom of the bigger glaſs AB, within whoſe neck A, it was faſten'd with a cloſe cement, that both kept the pipe in its place, and hinder'd all communication betwixt the inward II, and the outward air KK, except by the cavity of the pipe CD. Now we choſe this glaſs AB, more than ordinarily capacious, that the effect of the dilatation of the included air II, might be the more conſpicuous. Then conveying a convenient quantity of water HD, into this glaſs, we carry'd it to the leads of the abbey-church at *Weſtminſter*, and there blew in a little air, to raiſe the water to the upper part of the pipe, that, being above the veſſel AB, we might the more preciſely mark the ſeveral ſtations of the water. Afterward, having ſuffer'd the glaſs to reſt a pretty while upon the leads, that the air II, within, might be reduced to the ſame ſtate with KK, that without; having mark'd the ſtation of the water F, we gently let down the veſſel by a ſtring to the foot of the wall, where one attended to receive it, who having ſuffer'd it to reſt upon the ground, told us, that it was ſubſided about an inch below the mark F; whereupon, having order'd him to put a mark at this ſecond ſtation of it E; we drew up the veſſel again, and ſuffering it to reſt a while, obſerv'd the water to be re-aſcended to the firſt mark F, which was, indeed, about an inch above E: and this we did a ſecond time, with almoſt a like ſucceſs; tho', two or three days after, the wind blowing ſtrongly upon the leads, we found not the experiment to ſucceed quite ſo regularly; yet the water, always, manifeſtly, fell lower at the foot of the wall, than at the top. But, to avoid miſtakes, and prevent objections, we made the experiment within the church, at the ſame height with the leads; but the upper part of the pipe being, accidentally, broken off, we order'd the matter ſo, that the ſurface G, of the remaining water in the pipe, ſhould be about an inch higher than the ſurface of that in the veſſel. And then, letting down the glaſs, I found that, almoſt, as ſoon as it was ſettled upon the pavement, it was not only fallen as low as the other water, but the outward air depreſs'd it ſo far, as, whilſt I was looking on

Fig. 83.

to break in below the bottom of the pipe, and afcend thro' the water in bubbles; after which, the glafs being drawn up again, the water was, very manifeftly, re-afcended. Hence 'tis evident, that the atmofphere gravitates more, *cæteris paribus*, near the furface of the earth, than in the more elevated parts of the air: for the leads, on which we made our trials, were found, in perpendicular height, but 75 feet. PNEUMATICS.

But, for an experiment of the fame kind, made at a greater height, take the following, communicated by Dr. *Power*.

On the 15th of *October*, 1661, we took a weather-glafs AB, about two feet in length, and carrying it to the bottom of *Hallifax-hill*, the water ftood in the fhank at 13 inches above that in the veffel: thence carrying it, thus fill'd, with the whole frame, immediately to the top of the faid hill, the water fell down to the point D; that is, an inch and a quarter lower than it was at the bottom of the faid hill; which proves the elafticity of the air: for the internal air AC, which was of the fame power and extenfion with the external, at the bottom of the hill, manifefted a greater elafticity, than the mountain-air there manifefted preffure; and fo extended itfelf further by CD. Fig. 84.

The like experiment, I hear, the fame ingenious perfon has lately repeated, and found the defcent of the water to be greater than before. And tho' fome have thought it ftrange, that, on a hill, far inferiour to the *Alps*, and *Appennines*, fo fhort a cylinder of water fhould fall fo much; yet I fee not any reafon to diftruft, upon this ground, either this experiment, or ours made at *Weftminfter*; but rather wonder the water fell no more, if the hill be confiderably high: for their fufpicion feems grounded on a miftake; as if becaufe the quick-filver, in the *Torricellian* experiment, made without purpofely leaving any air in the tube, would not at the top of the mention'd hill, have fubfided above an inch, the water, that is near 14 times lighter, fhould not fall above a 14th part of that fpace; whereas, in the *Torricellian* experiment, the upper-part of the tube has little, or no air left in it, while the correfpondent part of the weather-glafs contain'd air, whofe preffure was little lefs than that of the atmofphere at the bottom of the hill; and, confequently, muft be much greater, than the preffure of the atmofphere at the top of the hill.

Another particular, which confirms our hypothefis, is that experiment made by the fame M. *Pafchal*, by carrying a flack-blown foot-ball, from the bottom to the top of an high mountain; for, the foot-ball gradually fwell'd, the higher it was carry'd: fo that at the top of the mountain it appear'd as if it were full-blown; and became gradually lank again, as it was carry'd downwards; fo that, at the foot of the hill, it was flaccid as before. We have here an experiment to prove our hypothefis, wherein recourfe cannot be had to any body, forcibly, and preternaturally diftended, fuch as is pretended to remain in the deferted fpace of the tube, in the *Torricellian* experiment.

But, further, our author's hypothefis is needlefs; for, he denies not that the air has fome weight and fpring, but affirms it very infufficient

to

PNEUMATICS. to counterpoiſe a mercurial cylinder of 29 inches. We ſhall, therefore, now endeavour to manifeſt by experiments, purpoſely made, that the ſpring of the air is capable of performing far more than is neceſſary to ſolve the phenomena of the *Torricellian* experiment. We took a long glaſs tube, ſo bent at the bottom, that the part turned up, was almoſt parallel to the reſt of the tube; and the orifice of this ſhorter leg being hermetically ſeal'd, the length of it was divided into inches, each of which was ſub-divided into eighths, by a liſt of paper carefully paſted along it: then putting in as much quick-ſilver as fill'd the bended part of the ſiphon, that the mercury ſtanding in a level, might reach, in the one leg, to the bottom of the divided paper, and juſt to the ſame height, in the other; we took care, by frequently inclining the tube, that the air, at laſt, included in the ſhorter cylinder, ſhould be of the ſame laxity with the reſt of the air about it. This done, we began to pour quick-ſilver into the longer leg of the ſiphon; which, by its weight, preſſing upon that in the ſhorter, gradually ſtraitned the included air; and continuing to pour in quick-ſilver, till the air, in the ſhorter leg, was, by condenſation, reduced to take up but half the ſpace it poſſeſs'd before, we obſerv'd, in the longer leg of the glaſs, on which was, likewiſe, paſted a liſt of paper, divided into inches, and parts, that the quick-ſilver was 29 inches higher than in the other. Hence we ſee, that as, according to our hypotheſis, the air, in that degree of denſity, and correſpondent meaſure of reſiſtance, whereto the weight of the incumbent atmoſphere reduces it, is able to balance, and reſiſt the preſſure of a mercurial cylinder of about 29 inches; ſo, here, the ſame air, brought to a degree of denſity, about twice as great as it had before, obtains a ſpring twice as ſtrong; being able to ſuſtain, or reſiſt a cylinder of 29 inches, in the longer tube, together with the weight of the atmoſpherical cylinder, that reſted upon thoſe 29 inches of mercury.

The elaſtic force of compreſs'd and dilated air, meaſured.

After ſome other trials, one of which we made in a tube, whoſe longer leg was perpendicular; and the other, that contain'd the air parallel to the horizon; we, at laſt, procured a tube, which, tho' large, was ſo long, that the cylinder, whereof the ſhorter leg of it conſiſted, admitted a liſt of paper divided into 12 inches, and their quarters; and the longer leg another, ſeveral feet in length, and divided after the ſame manner: then quick-ſilver being poured in, to fill up the bended part of the glaſs, that the ſurface of it, in either leg, might reſt in the ſame horizontal line; more quick-ſilver was pour'd into the longer tube: and notice being taken, how far the mercury roſe therein, when it appear'd to have aſcended to any of the diviſions in the ſhorter; the ſeveral obſervations that were thus ſucceſſively made, and ſet down, afforded us the following table.

Fig. 85.

A TABLE of the Condensation of the AIR.

A	A	B	C	D	E
48	12	00	Added to 29 $\frac{1}{8}$ makes	29 $\frac{2}{16}$	29 $\frac{2}{16}$
46	11 $\frac{1}{2}$	01 $\frac{7}{16}$		30 $\frac{9}{16}$	30 $\frac{6}{16}$
44	11	02 $\frac{13}{16}$		31 $\frac{15}{16}$	31 $\frac{12}{16}$
42	10 $\frac{1}{2}$	04 $\frac{6}{16}$		33 $\frac{8}{16}$	33 $\frac{1}{7}$
40	10	06 $\frac{3}{16}$		35 $\frac{5}{16}$	35
38	9 $\frac{1}{2}$	07 $\frac{14}{16}$		37	36 $\frac{15}{19}$
36	9	10 $\frac{2}{16}$		39 $\frac{5}{16}$	38 $\frac{7}{8}$
34	8 $\frac{1}{2}$	12 $\frac{8}{16}$		41 $\frac{10}{15}$	41 $\frac{2}{17}$
32	8	15 $\frac{1}{16}$		44 $\frac{3}{16}$	43 $\frac{11}{16}$
30	7 $\frac{1}{2}$	17 $\frac{15}{16}$		47 $\frac{1}{16}$	46 $\frac{3}{5}$
28	7	21 $\frac{3}{16}$		50 $\frac{5}{16}$	50
26	6 $\frac{1}{2}$	25 $\frac{3}{16}$		54 $\frac{5}{16}$	53 $\frac{10}{13}$
24	6	29 $\frac{11}{16}$		58 $\frac{13}{16}$	58 $\frac{2}{8}$
23	5 $\frac{3}{4}$	32 $\frac{3}{16}$		61 $\frac{5}{16}$	60 $\frac{18}{25}$
22	5 $\frac{1}{2}$	34 $\frac{15}{16}$		64 $\frac{1}{16}$	63 $\frac{6}{11}$
21	5 $\frac{1}{4}$	37 $\frac{15}{16}$		67 $\frac{1}{16}$	66 $\frac{4}{7}$
20	5	41 $\frac{9}{16}$		70 $\frac{11}{16}$	70
19	4 $\frac{3}{4}$	45		74 $\frac{2}{16}$	73 $\frac{11}{19}$
18	4 $\frac{1}{2}$	48 $\frac{12}{16}$		77 $\frac{14}{16}$	77 $\frac{2}{3}$
17	4 $\frac{1}{4}$	53 $\frac{11}{16}$		82 $\frac{12}{16}$	82 $\frac{4}{17}$
16	4	58 $\frac{2}{16}$		87 $\frac{14}{16}$	87 $\frac{3}{8}$
15	3 $\frac{3}{4}$	63 $\frac{15}{16}$		93 $\frac{1}{16}$	93 $\frac{1}{5}$
14	3 $\frac{1}{2}$	71 $\frac{5}{16}$		100 $\frac{7}{16}$	99 $\frac{6}{7}$
13	3 $\frac{1}{4}$	78 $\frac{11}{16}$		107 $\frac{13}{16}$	107 $\frac{7}{13}$
12	3	88 $\frac{7}{16}$		117 $\frac{9}{16}$	116 $\frac{4}{8}$

AA The number of equal ſpaces in the ſhorter leg, containing the ſame parcel of air, differently expanded.

B The height of the mercurial cylinder, in the longer leg, that compreſs'd the air into thoſe dimenſions.

C The height of a mercurial cylinder, that balanced the preſſure of the atmoſphere.

D The aggregate of the two laſt columns B and C, exhibiting the preſſure ſuſtain'd by the included air.

E What that preſſure ſhould be, ſuppoſing it in reciprocal proportion to the expanſion.

For the better underſtanding of this experiment, it is proper to obſerve the following particulars. 1. The tube being very tall, we were obliged to uſe it on a pair of ſtairs, which were very well illumined; and for preſervation, it was ſuſpended by ſtrings. 2. The lower, and bent part of the pipe, was placed in a ſquare wooden box, large and deep, to prevent the loſs of the quick-ſilver. 3. We were two, to make the obſervation together; the one to take notice at the bottom, how the quick-ſilver roſe in the ſhorter cylinder; and the other, to pour it in at the top of the longer. 4. The quick-ſilver was pour'd in but ſlowly, according to the direction of him who obſerv'd below. 5. At the beginning of the operation, that we might the more truly diſcern where the quick-ſilver reſted, from time to time, we made uſe of a ſmall looking-glaſs, held in a convenient poſture, to reflect to the eye what we deſired to ſee. 6. When the air was crowded into leſs than a quarter of the ſpace it poſſeſſed before, we try'd whether the cold of a linen-cloth, dipp'd in water, would condenſe it: and it, ſometimes, ſeem'd a little to ſhrink, but not ſo manifeſtly, that we dare build upon it. We then try'd, likewiſe, whether heat would dilate it; and, approaching the flame of a candle to that part where the air was pent up, it had a more ſenſible operation than the cold before; ſo that we ſcarce doubted the expanſion of the air would, notwithſtanding the weight that oppreſs'd it, have been made conſpicuous, if the fear of breaking the glaſs had not kept us from increaſing the heat. This

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This ſufficiently proves the principal thing for which I here alledge it; ſince 'tis evident, that as common air, when reduced to half its natural extent, obtain'd a ſpring, about twice as forcible as it had before; ſo the air, thus compreſs'd, being farther crowded into half this narrow room, thereby obtain'd a ſpring as ſtrong again as that it laſt had, and conſequently, four times as ſtrong as that of common air. And, there is no cauſe to doubt, that if we had been furniſh'd with a greater quantity of quick-ſilver, and a very ſtrong tube, we might, by a further compreſſion of the included air, have made it balance the preſſure of a far taller, and heavier cylinder of mercury. For no man, perhaps, yet knows, how near to an infinite compreſſure the air may be reduced, by a force competently increas'd. So that, here our author may plainly ſee, the ſpring of the air can reſiſt, not only the weight of twenty-nine inches, but, in ſome caſes, above one hundred inches of quick-ſilver; and this, without the aſſiſtance of his *Funiculus*, which, in our preſent caſe, has no pretence to be employ'd. And, to ſhew, that the weight of the incumbent atmoſphere, made a part of the weight reſiſted by the impriſon'd air; when the mercurial cylinder, in the longer leg of the pipe, was about one hundred inches high, we caus'd a man to ſuck at the open orifice, whereupon the mercury in the tube conſiderably aſcended: which phenomenon cannot be aſcrib'd to our author's *Funiculus*; ſince, by his own confeſſion, that cannot pull up a mercurial cylinder of above twenty-nine or thirty inches. And, therefore, the preſſure of the atmoſphere, being in part taken off, by expanding itſelf into the man's dilated cheſt, the impriſon'd air, was, thereby enabled, manifeſtly, to dilate, and repel the mercury that compreſs'd it, till there was an equality of force betwixt the ſtrong ſpring of the compreſs'd air on the one part, and the tall mercurial cylinder, with the contiguous dilated air, on the other.

Now, if to what we have deliver'd concerning the compreſſure of the air, we add ſome obſervations of its ſpontaneous expanſion, it will the better appear, how much the phenomena of theſe mercurial experiments depend upon the different meaſures of ſtrength to be met with in the air's ſpring, according to its various degrees of compreſſion and laxity.

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A TABLE of the Rarifaction of the Air.

A	B	C	D	E
1	00	Subſtracted from 29 3/4 leaves	29 3/4	29 3/4
1 1/2	10 5/8		19 1/8	19 5/6
2	15 3/8		14 3/8	14 7/8
3	20 2/8		9 4/8	9 15/12
4	22 5/8		7 1/8	7 7/16
5	24 1/8		5 5/8	5 19/20
6	24 7/8		4 7/8	4 23/24
7	25 4/8		4 7/8	4 1/4
8	26		3 6/8	3 28/32
9	26 1/8		3 3/8	3 11/36
10	26 6/8		3	3 39/40
12	27 1/8		2 5/8	2 13/48
14	27 4/8		2 2/8	2 1/8
16	27 6/8		2	1 55/64
18	27 7/8		1 7/8	1 47/72
20	28		1 6/8	1 9/80
24	28 2/8		1 4/8	1 28/96
28	28 1/8		1 3/8	1 1/16
32	28 4/8		1 2/8	0 119/128

A. The number of equal ſpaces at the top of the tube, that contain'd the ſame parcel of air.

B. The height of the mercurial cylinder, that together with the ſpring of the included air balanc'd the preſſure of the atmoſphere.

C. The preſſure of the atmoſphere.

D. The complement of B to C, exhibiting the preſſure ſuſtain'd by the included air.

E. What the preſſure ſhould be, according to the hypotheſis.

* To make the experiment of the debilitated force of expanded air the plainer, we muſt mention ſome particulars, eſpecially with relation to the manner of performing it. 1. We made it on a light pair of ſtairs, and with a box lin'd with paper to receive the mercury, that might be ſpilt; and in a glaſs tube about ſix feet long, hemetically ſeal'd at one end. 2. We alſo provided a ſlender glaſs pipe about the bigneſs of a ſwan's quill, and open at both ends, all along which, was paſted a narrow liſt of paper, divided into inches and half quarters. 3. This ſlender pipe, being thruſt into the greater tube, almoſt fill'd with quick-ſilver, the glaſs help'd to make it ſwell to the top of the tube; and the quick-ſilver getting in at the lower orifice of the pipe, fill'd it up till the mercury, included in that, was near upon a level with the ſurface of the ſurrounding mercury in the tube. 4. There being little more than an inch of the ſlender pipe left above the ſurface of the ſtagnant mercury, and, conſequently, unfill'd therewith, the prominent orifice was carefully clos'd with melted ſealing-wax; after which, the pipe was let alone for a while, that the air, dilated by the heat of the wax, might, upon refrigeration, be reduced to its wonted denſity. And then we obſerv'd, by help of the liſt of paper, whether we had included more or leſs than an inch of air, and in either caſe, we rectify'd the error, by a

* "The open air, in which we breathe," ſays Sir *Iſ. Newton*, "is 8 or 900 times "lighter than water, and by conſequence "8 or 900 times rarer. And ſince the air "is compreſs'd by the weight of the incumbent atmoſphere, and the denſity "of the air is proportionable to the "compreſſing force, it follows, by computation, that at the height of about 7 "*Engliſh* miles from the earth, the air is "four times rarer than at the ſurface of "the earth; and at the height of 14 "miles it is 16 times rarer than at the "ſurface of the earth; and at the height "of 21, 28, or 35 miles it is reſpectively "64, 256, or 1024 times rarer, or thereabouts; and at the height of 70, 140, and "210 miles, it is about 1.000000, 1.000000.000000, or 1.000000.000000.000000." &c. *Newton. Optic.* p.341. 342.

 ſmall hole made with a heated pin in the wax, and afterwards clos'd it up again. 5. Having thus included a juſt inch of air, we lifted up the ſlender pipe, by degrees, till the air was dilated to an inch, an inch and a half, two inches &c. and obſerv'd, in inches and eighths, the length of the mercurial cylinder, which at each degree of the air's expanſion was impell'd above the ſurface of the ſtagnant mercury. 6. The obſervations being ended, we preſently made the *Torricellian* experiment with the above-mention'd large tube, ſix feet long, that we might know the height of the mercurial cylinder for that particular day and hour; which we found to be twenty-nine inches and three quarters. 7. Our obſervations, made after this manner, furniſh'd us with the preceding table, in which here would not, probably, have been found the difference here ſet down betwixt the force of the air, when expanded to double its former dimenſions, and what that force ſhould have been, preciſely, according to the theory, but that the included inch of air receiv'd ſome little acceſſion during the trial; which this difference cauſing us to ſuſpect, we found, by plunging the pipe again into the quick-ſilver, that the included air had gain'd about half an eighth; which we gueſs'd to have come from ſome little aerial bubbles in the quick-ſilver contain'd in the pipe.

Here we find that the inch of air, when firſt included, ſuſtain'd no other preſſure than from the incumbent air, and was no more compreſs'd than the reſt of the air we breath'd and mov'd in; that this inch of air, when expanded to twice its former dimenſions, was able, with the help of a mercurial cylinder, of about fifteen inches, to counterpoiſe the weight of the atmoſphere; and that this was impell'd up into the pipe by the external air gravitating upon the ſtagnant mercury, which, alſo, ſuſtain'd above 28 inches of mercury, when the internal air had its ſpring too far weakned, to make any conſiderable reſiſtance: from whence 'tis plain, that the free air, here below, is, almoſt, as ſtrongly compreſs'd by the weight of the incumbent atmoſphere, as it would be by the weight of a mercurial cylinder, 28, or 30 inches high; and, conſequently, is not in ſuch a ſtate of laxity, as men uſually imagine; but acts like ſome mechanical agent, with a force decreaſing, in a ſtricter proportion to its increaſe of dimenſion, than has been, hitherto, taken notice of.

And hence, at length, we ſee, that our author's hypotheſis is unneceſſary to ſolve the phenomena in diſpute: which is no ſmall acquiſition, ſince the two principal things, that induced him to reject our hypotheſis, are, nature's abhorrence of a vacuum; and that, tho' the air have ſome weight and ſpring, yet theſe are inſufficient to make out the known phenomena, for which, we muſt, therefore, have recourſe to his *Funiculus*. But, he has not diſprov'd a vacuum, yet we have manifeſted, that the ſpring of the air may perform greater things, than what our explanation of the *Torricellian* experiments, and thoſe of our engine, require.

We come now to the laſt part of our defence, wherein we are to conſider what our author objects to ſome particular experiments.

Againſt

Against our first experiment, he objects nothing, but that, by applying the finger to the orifice of the valve, when the pump is freed from air, the sucker will not appear to be thrust inward by the external air, but, as the finger, to be drawn inwards, by the internal. But this phenomenon has been, formerly, accounted for, upon our hypothesis.

Pneumatics. *Particular pneumatical experiments defended.*

Of our third experiment, he says; that " it very well agrees with his " principles; for, since by this depression of the sucker, the air, in the " cavity of the cylinder, is separated from the cylinder, and descends, " together with the sucker, in that whole depression; new surfaces are " taken from that descending air, and stretch'd out, as in the case of de- " scending water. Since, therefore, such surfaces, are as easily slipp'd " off, and extended at the end of the depression, as at the beginning, it is " no wonder there should be the same difficulty of depressing, in both ca- " ses." By which, he seems to intend an opposition to a part of the third experiment, which I oppos'd not against his opinion: yet he offers nothing at all to invalidate my inference; but, instead of that, proposes a defence of his own opinion, which supposes the truth of his hypothesis; and is unsatisfactory, even according to that, or else, disagrees with what himself hath taught us, but a little before. For, 'tis evident, that the more the sucker is depress'd, the more the cylinder is exhausted of air. And, speaking of the air, in the receiver, he affirms, that "'tis " the more extended and rarified, the more is drawn out; and, there- " fore, acquires the greater force to contract itself." Though here he would have us believe, that the little internal air, in the cavity of the shank of the stop-cock, as strongly attracts the sucker, or resists its depressure, when the sucker is near the top of the cylinder, as when, being forced down to the lower part thereof, the same portion of remaining air must be exceedingly more distended.

To the fourth experiment, our author objects nothing, but endeavours explain it his own way, whereto he says, this circumstance excellently agrees, that, upon the return of the external air, into the receiver, the tumid bladder immediately shrinks; because the air in the receiver, which drew the sides of the bladder outward, from the middle of it, is hereby relax'd: which explication, whether it be more natural than ours, let any one judge, who has consider'd what we have alledg'd against the *Funiculus.*

To the breaking a glass receiver, not of a globular figure, by exhausting most of the internal air, whereby its diminish'd pressure became unable to resist that of the outward air; our author confidently says, " it seems " incredible, that the most soft air should so vehemently compress such a " glass, on all sides, as to break it." As if it were more credible, that the air within, should be able to act more powerfully upon the glass, than that without, which himself confesses to be a heavy body; and which, not only reaches from the surface of the earth, to the top of the highest mountains, but may, for ought we know to the contrary, be heap'd upon the receiver, to the height of some hundreds of miles.

After a recital of the ninth experiment, he proposes his objection to it thus. "But this seems far remov'd from truth, because, if the pressure "of the air, which descends by that tube into the vial, be so great as to "break the vial itself, it should, certainly, first, very much move the wa- "ter, in which the tube is immers'd, excite bubbles in it, &c. yet it "is certain, that the water, before the vial breaks, doth not move at all." But, for all this, I think our explanation true: for, we put the water into the vial that was broken, upon a particular design; and, in the second trial, the water was omitted. But, notwithstanding this water, the sides of the glass being expos'd to the pressure of the atmosphere, wholly sustain'd it, before the exhaustion of the receiver: so that there needed no such blowing in of the air a-fresh, as our author imagines, to break the vial; it being sufficient for that purpose, that the pressure against the convex superficies of it, was taken off, by exhausting the receiver; the pressure against the concave superficies, remaining as great as ever. And, therefore, we need not altogether deny what he says, that "tho' the "tube had been clos'd at the top, the vial would still have broke." For, since, in such cases, the air is shut up, with the whole pressure of the atmosphere upon it, this may, almost, as easily, break the glass, as if it were unstopp'd; and, accordingly, we mention the breaking of a thin glass, hermetically seal'd, upon the extraction of the ambient air. But, as confidently as our author speaks, such thin vials are subject, upon withdrawing the ambient air, to stretch a little; whereby the spring of the included air, may, in some cases, be so far weakned, as not to be able to break them, unless assisted by the pressure of the atmosphere: and when the vial actually begins to break, the ensuing pressure of the outward air, upon that within the vial, may help to throw the parts of the glass more forcibly asunder.

The author, having recited our conjecture, as to the reason why two flat smooth marbles stick so closely together, approves my way of examining that conjecture. But, I say, tho' the marbles were kept together, by the pressure of the ambient air, yet they did not fall asunder, in our exhausted receiver, because of some small leak in the receiver; yet he tells us, with his usual confidence, that this very experiment sufficiently shews, that opinion false. But, possibly, he would have spoken less resolutely, if he had made all the trials, about the adhesion of marbles, that I have. For he speaks, as if all that we ascribe to the air, in such experiments, were to sustain the lower marble, with the weight, perhaps, of a few ounces; whereas, if the air be wholly kept from getting between the stones, it may sustain a weight equal to that of a pillar of air, as broad as the basis of the lower marble, and as high as the atmosphere; or, to the weight of a column of quick-silver, of the same thickness, and about 30 inches long. And, therefore, since when we had exhausted our receiver, as far as we could, there remain'd air enough to sustain in the tube, a cylinder of quick-silver an inch high; and since the broader the contiguous marbles are, the greater weight, fasten'd to the lowermost, may be sustain'd, by the

the refiftance of the air; it's no wonder, that the air, remaining in the receiver, fhould fupport the lower-moft marble, whofe diameter was near two inches, and a weight of four ounces; thofe two weights being inferior to that of a mercurial cylinder, of the fame diameter, and an inch in length: and tho' they were not, yet, perhaps, the receiver was lefs empty'd, when we made the 31ft experiment, than when we made the 17th. And 'twas with the fame pair of marbles, that, before an illuftrious affembly, the upper-moft drew up the lower-moft, tho' clogg'd with a weight of above 430 ounces.

As for the account our author gives of this phenomenon, few, I believe, will acquiefce in it: for, not to infift upon the objection, which himfelf takes notice of, that, according to him, the diftended air in the receiver, fhould draw the adhering marbles afunder, his explanation fuppofes, that there cannot, naturally, be a vacuum; whence he infers, that "the "ftone could not defcend, but by leaving fuch a thin fubftance behind "it, as happens in the defcent of quick-filver, or water." He adds, that the adhefion, in our cafe, proves obftinate, becaufe fuch a fubftance is far more difficult to be feparated from marble, than from quick-filver, or any other kind of body; but this affertion is precarious. And though I have made numerous experiments, with ftones of feveral fizes, yet I could never find, that, by their cohefion, they would fuftain a weight greater than that of a pillar of the atmofphere, that prefs'd againft the loweft; which is a confiderable circumftance, that much better agrees with our explanation, than with his.

Of the fudden extinction of animals, included in our receiver, which I afcribe to the exceffive thinnefs of the air therein, he fays, "it feems "impoffible they fhould die fo foon, merely thro' want of a thick air:" but gives no other reafon, than the fuddennefs of the effect; which, too, feems grounded upon a miftake: for, the creatures, he mentions, were a bee, a fly, and a caterpillar; and thofe included in a fmall receiver, which could be fuddenly exhaufted; and thefe, indeed, became movelefs, within a minute. And tho' thefe infects did, in fo fhort a time, grow movelefs, yet they were not fo foon kill'd, as appears by the narrative. The fanguineous animals, that did, indeed, die, were kill'd, more flowly. And having, purpofely, enquir'd of a diver, how long he could, before he was accuftom'd to dive, remain without breathing, or the ufe of a fpunge; he told me, that at firft he could hold out about two or three minutes, at a time: which made me think, that divers become able to continue under water fo long, either by a peculiar conftitution of body, or, a gradual exercife. And, I am apt to think, that, as 'tis ufual, he hereby meant a much fhorter time than, when exactly meafur'd, it amounts to. For, having made trial upon two live moles, one of them, included in a fmall receiver, was between two and three minutes in killing; whereas, the other being detain'd under water, did not there continue full a minute and a quarter, before it finally ceas'd from giving any fign of life. Hence 'tis not impoffible, that the want of refpiration, fhould difpatch an animal in as little time, as is mention'd in the experi-

PNEUMATICS. experiment. And, indeed, our author ſhould either have prov'd it impoſſible, for the want of air to deſtroy animals ſo ſoon; or have given us ſome better account of the phenomenon.

'Twere a needleſs task, to examine any more of our author's objections to particular experiments, ſince they wholly proceed upon the ſuppoſition of his *Funiculus*; which has been ſufficiently proved a chimæra: whereas the ſpring, as well as the weight of the air, is not only allow'd by himſelf, but demonſtrable by experiments uncontroverted betwixt us.

Mr.

Mr. *HOBBS*'s Physical Dialogue,

ABOUT THE Nature of the AIR,

EXAMIN'D,

With relation to the Physico-mechanical Experiments of the Spring and Effects of the AIR.

MR. *Hobbs*, in disputing against me, seems, generally to have misapprehended my notion of the air. For, when I say the air has gravity, and an elastic power; or, that the air is, in great part, pump'd out of the receiver; 'tis plain, that I take the air, in the obvious sense, for part of the atmosphere, which we breathe, and wherein we move: nor do I find, that any other of my readers understand me otherwise. But Mr. *Hobbs* thinks he has sufficiently confuted me, if, in some cases, he proves, that there is a subtile substance, or æther, in some places, which I take not to be fill'd with air; and that the æther has, or wants some properties, which I deny, or ascribe to the air: but I do not deny that the atmosphere, or fluid body, which surrounds the terraqueous globe, may, besides the grosser, and more solid corpuscles, wherewith it abounds, consist of a thinner matter, which, for distinction sake, I, also, agree to call æthereal.

The weight and spring of the air asserted.

But he does not, that I remember, deny the truth of any of the matters of fact, I have deliver'd; nor attempt to prove, that the explanations I have given of my experiments, are contradictory to the doctrine I advance: but rather rejects our two grand hypotheses, the weight and spring of of the air. It will here, therefore, ſuffice to prove, what he is unwilling to grant.

And firſt, that the air, in my ſenſe of the word, is not deſtitute of weight, we have ſhewn by various experiments: one of them is, that a blown bladder, carefully weigh'd, in an exact pair of ſcales, was found manifeſtly heavier when full of air, than when empty of air.

Secondly, it has, alſo, been obſerv'd, that an æolipile, being well heated, and the little orifice, left at the top of the pipe, being ſtop'd, whilſt it was thus hot; upon opening of this hole, when the æolipile was grown cold again, the external air ruſhing in at the foremention'd orifice, caus'd the æolipile to weigh ſo much more than it did juſt before the external air got in, as amounted to near a thouſandth part of the weight of an equal bulk of water.

Thirdly, in the *Magdeburgic* experiment, the great receiver they were to exhauſt, being weigh'd both before and after the extraction of the air, they found the difference to be 1 ounce $\frac{1}{10}$, "which," ſays the learned *Schottus*, "is a very clear demonſtration of the gravity of the air."

Fourthly, we have weigh'd the air ſhut up in bodies, in our exhauſted receiver, wherein of two materials, different in nature, a blown bladder, and a glaſs bubble, each equi-ponderant to a more ſolid weight, before the air was pump'd out, we found that which included a large quantity manifeſtly to preponderate after the exhauſtion. To theſe we might add other proofs to the ſame purpoſe; but theſe afford ſuch a variety of caſes, that it would be ſuperfluous.

Let us now ſee what Mr. *Hobbs* objects againſt the experiment of the bladder, weigh'd in the exhauſted receiver; for he quarrels not with the reſt. "That the ſcale which contains the bladder, is more depreſs'd than "the other, they may be certain by ſight; but that this proceeds from the "natural gravity of the air, they cannot be certain, eſpecially if they know "not the efficient cauſe of gravity." But can we not be ſure, that lead is, *in ſpecie*, heavier than cork, unleſs we know the efficient cauſe of gravity? The reaſon, he gives, why the bladder outweighs, is this. "That "the bladder, whether blown up with a pair of bellows, or with human "breath, is heavier than when flaccid, I will not deny, becauſe of the "greater quantity of atoms, or of fuliginous corpuſcles: but there's no-"thing certain in this experiment. They ought to have put into the ſcales "two veſſels of equal weight, whereof one ſhould be ſhut, and the other open: "for thus air, not blown in, but barely inclos'd, had been weigh'd; when, "therefore, air ſhall be ſo weigh'd, we will afterwards conſider what "may be ſaid to the phenomenon." The firſt part of this paſſage does not deny the gravity of that we call air, but only endeavours to ſhew, what parts they are that make it heavy. And, as to the ſecond, he ſeems to miſtake the preſent caſe: for there is no need that the air in the bladder, before the exhauſtion of the receiver, ſhou'd be heavier than the outward air. Wherefore, when he ſubjoins, that from this experiment we can deduce nothing certain, the affirmation is precarious. And Mr. *Hobbs* might

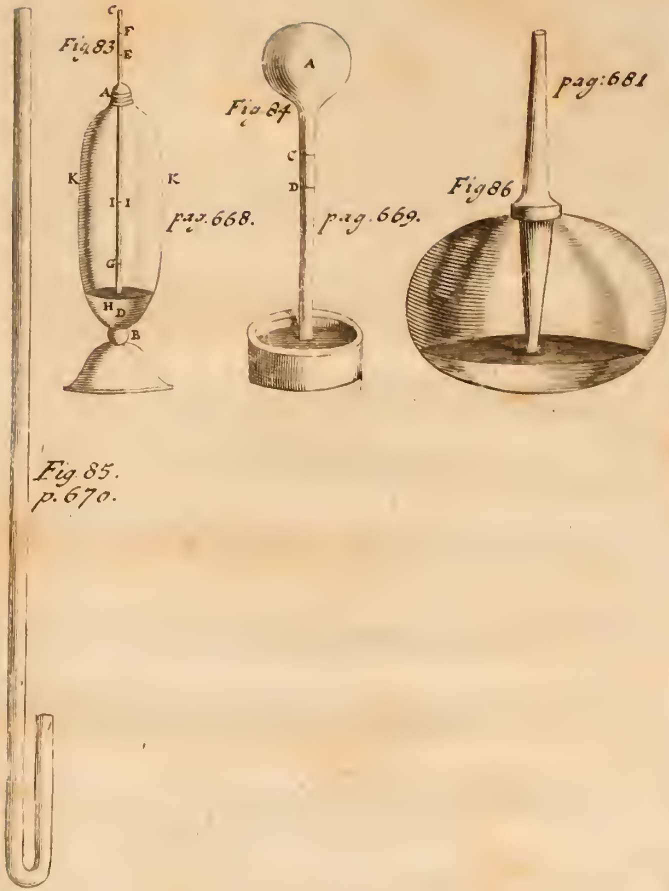
Fig 83
pag. 668.
Fig 84
pag. 669.
Fig. 85.
p. 670.
Fig 86
pag:681
A
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might eaſily have perceiv'd, that we did make a trial much of the ſame nature with that he deſires : for, we weigh'd the air in a glaſs, hermetically ſeal'd, wherein it was not blown in, but barely included. And ſince, in his elements of philoſophy, he grants, and gives his reaſon for it, that, "if air be blown into a hollow cylinder, or, into a bladder, it "will increaſe the weight of either of them a little ;" and, ſince here he likewiſe confeſſes, that there are mix'd with the æther, many aqueous and earthy particles: he confeſſes, that the air is not deſtitute of weight ; and it concerns us no more than himſelf, to ſhew how the corpuſcles, upon whoſe account the air is heavy, make it ſo. PNEUMATICS.

This is all which Mr. *Hobbs*, in ſeveral places, thinks fit to object againſt the gravity of the air ; leaving the experiment of the æolipile, and ſome others, unanſwer'd ; which, alone, prove the air has a manifeſt weight, even when uncompreſs'd, and in its laxity. Let us now examine whether the air has not, alſo, a ſpring. This, tho' he calls it (as he likewiſe does the weight of the air) a dream, yet himſelf, in effect, grants all that is requiſite to prove the ſpring of the air. For, delivering that known experiment, wherein the air is compreſs'd in a glaſs bottle, by the forcible injection of water, which, when the glaſs is unſtopp'd, the air again throws out, in recovering its former dimenſions; he ſays, that "the air, with which the "ſpherical glaſs was fill'd, being mov'd by earthy corpuſcles, in a ſimple, circular motion ; and being compreſs'd by the force of the injection, "that of it, which is pure, gets out into the open air, and gives place to "the water. It follows, that theſe earthy corpuſcles have leſs ſpace left, "wherein to exerciſe their natural motion ; therefore, beating one upon "another, they force the water to fly out, when the external air penetrates it, and ſucceſſively takes up the place of the evacuated air, till "the corpuſcles of the ſame quantity of air being reſtored, regain a liberty natural to their motion." But, to paſs by ſeveral other of his conceſſions, to this purpoſe, we can prove the ſpring of the air by many phenomena of our engine, of which he offers no other explanation. *Fig.* 86

If the *Torricellian* experiment be made in a tube, between two feet and a half, and three feet in length ; and if, when the mercury reſts at its wonted ſtation, you dextrouſly ſtop the orifice of the tube, with your finger, that orifice being rais'd as near the ſurface of the ſtagnant mercury, as poſſible, without admitting the external air ; if, then, you quite lift up the tube, thus ſtopp'd, into the free air, you ſhall feel, upon your finger, little or no preſſure from the weight of the mercurial cylinder, diſtinct from the weight of the tube ; becauſe the gravity of the quick-ſilver, is balanc'd by that of the outward air, which thruſts your finger againſt it : but, if you invert the tube, and having let in the air at the orifice, ſtop it again with your finger ; and again let the mercurial cylinder reſt upon that finger, you will find your finger ſtrongly preſs'd, and ready to be thruſt away ; which new preſſure, ſince it cannot come from the mercury, nor from the weight of the admitted air, to what can we, rationally, aſcribe it, but to the ſpring of the included air ? And the force hereof will be as well manifeſt to the

PNEUMATICS. eye, as the finger, if the tube be unſtopp'd under the ſurface of the ſtagnant mercury; for then that in the glaſs, will not reſt, as before, at the uſual ſtation, but be depreſs'd far beneath it. And, if you make the *Torricellian* experiment, in a ſhort, open tube, ſtopp'd, above and below, with your fingers, upon unſtopping the upper orifice, a new, and forcible preſſure will be felt upon the finger that ſtops the lower orifice, made by the gravitation of the external air, which was before kept from reſting upon the mercurial cylinder, by the upper finger, the pulp of which, by that gravitating air, was, before, thruſt into the deſerted cavity of the tube; which demonſtrates both the ſpring of the air, and the gravity of the atmoſphere.

But to the experiment of the ſwelling and ſhrinking of a bladder, hung in our receiver, as the ambient air is withdrawn, and ſuffer'd to return; Mr. *Hobbs* replies, "that every ſkin is made up of ſmall threads, or "filaments, which, by reaſon of their figures, cannot exactly touch in "all points. The bladder, therefore, being a ſkin, muſt be pervious, not "only to air, but to water alſo; whence, there is the ſame compreſſure "within the bladder, as without. The endeavour of which (the way of "its motions being every way croſs) tends to the concave ſuperficies of "the bladder; wherefore it muſt, of neceſſity, ſwell every way, and the "vehemency of the endeavour increaſing, be, at laſt, torn." But, if this be a ſufficient anſwer to ſuch an experiment, I fear, it will be harder than we are yet aware of, to prove any thing by experiments. For, firſt, how improbable is it, that ſuch bladders, as we uſed, are readily pervious to the air; when eaſy experience ſhews us, that, by leiſurely compreſſing ſuch blown bladders, betwixt our hands, we ſhall rather break them, than ſqueeze out the air at their pores? So that the reſt of his anſwer being built upon what is ſo repugnant to common experience, will not need a particular confutation: however, we ſhew, that by the exhauſtion of the air, even a glaſs, hermetically ſeal'd, was broke: and to ſay, that glaſs, alſo, is pervious to the air, were to affirm what the greateſt part of his book ſuppoſes to be falſe. Beſides, there is not any ſenſible, and unqueſtionable phenomenon, to prove that the receiver is full of any ſuch air, as he ſpeaks of; for we ſee, plainly, that when the air, manifeſtly, gets into the receiver, the bladder it not, thereby, made to ſwell, but to ſhrink. Moreover, according to Mr. *Hobbs*, the bladder is pervious to the air; and the air, within the receiver, is univerſally compreſs'd, as well that which is within the bladder, as that which is without it; how then comes the air, that bears gainſt the convex ſurface of the bladder, not to reſiſt that which is contiguous to the concave ſuperficies of the ſame; at leaſt, how comes the bladder to be broken by the air, which, according to Mr. *Hobbs*, can get in and out at pleaſure? And, laſtly, to ſhew, that to the ſwelling of the bladder, there needs nothing but the ſpring of the included air; and no ſuch vehement agitation of the ambient air, as he ſuppoſes in our engine; it appears, by the experiment of M. *Paſchal*, that, in the free, and ordinary air, a foot-ball, half blown, will gradually ſwell, the nearer it is carried

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ed to the top of an high mountain, where the incumbent cylinder of the atmoſphere is ſhorter, and its weight the leſs; and will, for the contrary reaſon, grow more flaccid, the nearer it approaches to the foot of the mountain.

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Mr. Hobbs's *explanation of the phenomena of the air-pump, examin'd.*

Mr. *Hobbs* attempts to explain the phenomena of our engine, by ſuppoſing, that "many earthy particles are interſpers'd with the air, which "have a ſimple, circular motion, congenite to their nature; and that there "is a greater quantity of theſe particles in the air, near the earth, than "remote from it." But this aſſumption, to me, ſeems very precarious; for, I know no unqueſtionable example, or experiment, whereby it can be made out, that any ſmall parcel of matter, has ſuch a ſimple, circular motion, as he aſcribes to each of theſe innumerable earthy, and, as he adds, aqueous particles. The only argument he here brings to prove, that each atom would have this motion, if all the reſt of the earth were annihilated, does not ſeem clear to me. For, it is not always true, that each minute part of a homogeneous body, has, in every reſpect, the ſame qualities with the whole: as the roundneſs which a ſmall drop of water, or quick-ſilver, is obſerv'd to have upon a dry plain, is not to be met with in a large portion of either of theſe fluids, tho' plac'd upon the ſame plain. And Mr. *Hobbs*, as well as we, makes the terreſtrial atoms in the air to have gravity; a quality that does not properly belong to the whole globe of the earth: nor is it manifeſt why, becauſe the terreſtrial globe moves in a vaſt circle about the ſun, each particular atom of it muſt deſcribe a ſmall circle in the air, about I know not what center. And, tho' he aſſerts, that the air, near the earth, abounds with ſuch terreſtrial corpuſcles, 'tis not likely they ſhould have ſuch a regular motion, as he attributes to them; but, ſtriking againſt one another, they muſt, in probability, be put into, almoſt, as various, and confuſed a motion, as *Des Cartes* aſcribes to his tereſtrial particles, ſwimming in the atmoſphere.

Mr. *Hobbs* farther endeavours to prove, that, by the exhauſtion of our cylinder, no vacuum is produc'd; and gives a very different account of the experiment itſelf: he ſays, that "while the ſucker is drawn back, the more "ſpace is left within, the leſs is left to the external air; which being "thruſt backwards, by the motion of the ſucker, towards the outer-"moſt parts, moves, in like manner, the contiguous air; and that, the "next; and ſo forwards: ſo that, of neceſſity, at laſt, the air muſt be "compell'd into the ſpace deſerted by the ſucker, and enter between the "convex ſurface of the ſucker, and the concave of the cylinder. For, "the parts of the air being infinitely ſubtile, muſt inſinuate themſelves "that way by which the ſucker is drawn down; ſince the contact of "thoſe ſurfaces cannot be perfect in all points, becauſe the ſurfaces them-"ſelves cannot be made infinitely ſmooth: and then, that force, which is "applied to draw back the ſucker, in ſome meaſure diſtends the cavity of "the cylinder; and if, betwixt the two ſurfaces, one ſingle hard atom "ſhould enter, pure air will enter by the ſame way, tho' with a ſmall "force. And thus air, for the ſame reaſon, inſinuates itſelf through

PNEUMATICS. "the valve of the cylinder; and, therefore, the retraction of the ſucker "will not prove a vacuum. It follows, alſo, that the air, which is dri-"ven up into the ſpace deſerted by the ſucker, becauſe it is forcibly im-"pell'd, has a very ſwift, and circular motion, betwixt the top and the "bottom of the cylinder; becauſe there is nothing there to weaken its "motion; and nothing can give motion to itſelf, or diminiſh it." But, many exceptions may be made to this reaſoning. And, firſt, I know not why Mr. *Hobbs* ſhould, here, confine his diſcourſe to the pump, without taking notice of the glaſs it is deſign'd to evacuate. We will, therefore, conſider how he can account for the exhauſtion of the receiver, as well as of the cylinder, ſince we uſually employ them both together. And he being obliged to explain the exhauſtion of the one, as well as the other, it will be convenient to take into conſideration the receiver, becauſe that being of glaſs, and tranſparent, we can better ſee what happens in it, than in the opake cylinder. This premis'd, I do not clearly perceive, by this explanation, how he avoids a vacuum; for, according to his firſt words, the external air is diſplac'd by the motion of the ſucker outward, and this diſplac'd air muſt move that which is next to it; and that the next, and ſo onward, till, at length, the air muſt be compell'd into the ſpace deſerted by the ſucker: ſo that till this returning air get in betwixt the ſucker and the cylinder, how appears it, from this diſcourſe, that the deſerted ſpace was not empty for ſome little time? Certainly all theſe motions of the air, forward and backward, could not be perform'd in an inſtant; as may appear by the motion of ſounds and echoes, whoſe velocity is reducible to meaſure. Secondly, tho' he take his adverſaries to be vacuiſts, yet he here ſuppoſes the plenitude of the world. I wiſh, thirdly, that Mr. *Hobbs* had declar'd, from whence the return of the air's impulſe ſhould begin; for that may well be requir'd from one, who, making the world full, and, for ought appears, fluid, allows us to believe it infinite, if the magiſtrate ſhall enjoin us that belief. Fourthly, I demand, what neceſſity there is for ſo forcible a return of the impulſe, as is requiſite to thruſt in the air at ſo narrow a paſſage as that between the ſucker and cylinder? For, why may not that impulſe, when diffuſed in the vaſt ambient medium, be ſo communicated, and blended among the different motions of the other parts of it, as not to return again from whence it begun? As a voice, tho' ſtrong, will not move the air, beyond a certain diſtance, ſmartly enough to be reflected in an echo, to the ſpeaker; and, as a ſtone caſt into a lake, will have the waves, it makes, diverted from returning to the place they began at. Fifthly, I do not, likewiſe, ſee, that 'tis probable, what Mr. *Hobbs* affirms of ſo thick a cylinder as ours, that it ſhould be diſtended by depreſſing the ſucker. But this I inſiſt not on; the principal thing, peculiar in Mr. *Hobbs*'s explanation, is, that as much air as is driven away by the ſucker, preſently gets in again, betwixt that and the cylinder. But, by the air thus ſuppos'd to get in, he either means in the uſual ſenſe, and in ours, the common air, ſuch as we live and breathe in; or, he does not.

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If he ſpeaks of ſuch air, I can plainly prove, by ſeveral experiments, that our engine is, in great part, deſtitute of it. For, firſt, if there be a contrivance made, whereby the whole pump may be cover'd with water, we may, as we have try'd, plainly ſee the air that is drawn out of the receiver, at each reciprocation of the ſucker, paſs, in great bubbles, out of the valve thro' the water.

Next, it appears, by the *Magdeburgic* experiment, that, by reaſon of the receſs of the air, the globe of glaſs, whence it went out, was diminiſh'd in weight, above an ounce. Thirdly, the ſame truth may be prov'd by the experiments formerly mention'd, of the ſwelling of a bladder, and the breaking of an hermetically ſeal'd glaſs, upon the receſs of the ambient air; theſe experiments having been already vindicated from Mr. *Hobbs*'s very improbable ſolutions. Fourthly, the ſame may be prov'd, by the breaking of weak, or ill-figur'd receivers, inwards; of which, on our hypotheſis, the reaſon is clear; but not on his. And, fifthly, what I contend for, may be ſufficiently prov'd from this one phenomenon; that tho', if the receiver being full of common air, the key be turn'd under water, the water will not at all aſcend at the open orifice; yet the like being done, after the exhauſtion of the receiver, we have had ſeveral gallons of water violently impell'd into the cavity of the glaſs: which could not happen, if it were full of air, both in regard there can be no probable cauſe aſſign'd why the water ſhould be thus ſpurted up; and becauſe the receiver being already full of air, either two bodies muſt be contain'd in one place, and ſo we muſt allow penetration of dimenſions; or elſe common air, to which glaſs is impervious, muſt paſs thro' the water; which, we conclude, it does not, becauſe no ſuch bubbles are made in the external water, as would appear, if common air paſs'd thro' it. Nay, ſo little of this common air was, ſometimes, left in the globe uſed at *Magdeburg*, that when the water was ſuffer'd to ruſh in, it reduced the air into leſs than the thouſandth part of the capacity of the globe; and even if our receiver be unſtopp'd, not under water, but in the open air, the ambient air will, violently, preſs in, with a great noiſe, durable enough to argue, that the glaſs was far from being full of ſuch air before.

And thus we may argue againſt Mr. *Hobbs*, if he would have the engine, when we call it exhauſted, fill'd with common air; as his words ſeem to intimate. But becauſe, by ſome other paſſages of this dialogue, he may be favourably thought to mean, that the pure air is that which gets in by the ſides of the ſucker, into the pump, and ſo into the receiver, let us conſider his explanation in this ſenſe alſo. I deſire it may be obſerv'd, that if Mr. *Hobbs* takes the air in this ſecond ſenſe, he does not oppoſe what I have deliver'd; the air, I pretend to be pump'd out of the receiver, being the common air, which conſiſts, in great part, of groſſer corpuſcles, than the æthereal ſubſtance. Yet, even this explanation will be liable to the two firſt inconveniencies, lately objected againſt the other, in favour of the vacuiſts; and to ſeveral objections beſides. I obſerve, again, that tho' the pump be kept all the while under water,

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PNEUMATICS. yet the exhauſtion of the cylinder, and receiver, will proceed as well as in the open air. I demand, then, how the pure air gets in by the ſides of the ſucker, immers'd in water? I preſume, for want of a more plauſible anſwer, Mr. *Hobbs* will here ſay, that the air paſſes thro' the body of the water, to fill up the deſerted ſpace, that muſt, otherwiſe, be void. But then I appeal to any rational man, whether I am obliged to believe ſo unlikely a thing, upon a bare affirmation; for he does not ſo much as pretend, by any phenomenon, to countenance this aſſertion: and there are phenomena that make againſt it. Many experiments ſhew us, that when air paſſes thro' water, it makes bubbles there, which, in our caſe, do not appear. Beſides, why ſhould not the outward air, rather impel the water, as we ſee it frequently does, than be ſuppos'd to dive ſo ſtrangely and imperceptibly thro' it? When, alſo, the throughly exhauſted receiver is unſtopp'd, under water, he, who obſerves how the water ruſhes in with a ſtream, as big as the paſſage admits, will hardly imagine, that at the ſame time, as much air as water can paſs thro' the ſame orifice unperceiv'd. But, it may be ſaid, in Mr. *Hobbs*'s behalf, that either his explanation, or a vacuum, muſt be admitted. To which I reply, firſt, that he has not evinc'd there can be no vacuum. Next, that we have made it probable, that, by his explanation, he does not avoid the neceſſity of a vacuum. And, thirdly, that a pleniſt, having recourſe to Mr. *Hobbs*'s precarious diving of the air, may, more probably, decline the neceſſity of yielding a vacuum, by ſaying, that the æther is, by the impulſe of the depreſs'd ſucker, and the reſiſtance of the ambient bodies, ſqueez'd thro' the pores of the glaſs, or cylinder, into the cavity of the veſſel, as faſt as room is there made for it. And, I confeſs, I wonder that Mr. *Hobbs* ſhould be ſo averſe to this way of ſolving the objection, ſince he ſuppoſes the parts of the air to be infinitely ſubtile; which, if they are, no pores can be too narrow to admit them. But, to preſs this no farther, I muſt here take notice, that whether the cavity of the receiver, be reſolv'd to be empty, or full of Mr. *Hobbs*'s æthereal body, or the *Carteſian* celeſtial matter; the violent ruſhing in of the water, when the veſſel is unſtopp'd under that liquor; with ſeveral other phenomena, which cannot be aſcrib'd to the ſubtile matter within; ſufficiently argue, that there is, in the external air, a far greater power of preſſing inwards, than there is within of reſiſting; and, conſequently, ſuch a weight, or ſpring in that air, as we plead for.

Mr. *Hobbs*, too, will have the air, impell'd by the ſucker, to move very ſwiftly betwixt the top and bottom of it; as alſo, when it gets into the cavity of the receiver; yet, when a light bladder is ſuſpended in the cavity of the receiver, it betrays no ſuch motion: nay, the flame of a taper was not blown out, nor ſtirr'd by this ſuppoſed wind; and ſmoke, produced in the exhauſted receiver, was not, by this vehement motion of the air, blown about the receiver. But, if the common external air be admitted at the ſtop-cock, that, indeed, will ruſh in with noiſe and violence, and whirl about the bladder, which hung quietly before.

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In explaining the *Torricellian* experiment, he ſpends many words to prove, that the place deſerted by the ſuſpended mercury, is full of air. But this expoſition ſuppoſes a plenum: and, if he takes the air in the common ſenſe of the word, 'tis manifeſtly repugnant, to ſeveral phenomena; as that, if the experiment be carefully made, we may, by inclining the tube, impel the mercury from its wonted ſtation to the top; which will not happen, in caſe the air were, before inclination, let into the deſerted ſpace; that if, when the mercury is ſettled at its uſual ſtation, the tube be lifted up out of the ſtagnant quick-ſilver, the outward air will drive up the heavy mercurial cylinder, oftentimes, with force enough to beat out the ſeal'd end; and, laſtly, the quick-ſilver reſting at its ſtandard height, if you carefully ſtop the lower orifice, under the ſurface of the ſtagnant quick-ſilver, and then lifting up the tube into the air, keep it well ſtopp'd, and firſt depreſs one end, and then the other; the quick-ſilver will fall againſt the depreſs'd end of the tube, with a ſurprizing force and ſwiftneſs: whereas, if unſtopping the tube, whilſt the ſame quantity of mercury remains in it, you let the outward air into the cavity, unpoſſeſs'd by the mercury; and then, again, ſtop the orifice with your finger, and proceed as before, you ſhall perceive the motion of the included fluid, to be much ſlower, and leſs violent than formerly, by reaſon of the reſiſtance of the admitted air; which, alſo, manifeſtly diſcloſes itſelf, by the conflict, and bubbles produced betwixt the air and quick-ſilver, in haſtily paſſing by one another, to the oppoſite ends of the tube. But, Mr. *Hobbs*, not pretending that any attraction intervenes in the caſe; I ſee not how he can poſſibly make out, to omit other phenomena, the gradual deſcent of the mercury, in the tube, beneath its wonted ſtation, upon the exhauſtion of the receiver; and the re-aſcent of the ſame, in the ſame tube, as we let in more or leſs of the outward air, without admitting as much of ſpring or preſſure in the air, as I contend for. The weight of the terreſtrial particles, by which he endeavours to account for the quick-ſilver's falling lower at the top, than at the bottom of a hill, will by no means ſerve his turn; it being utterly improbable that the air, contain'd in ſo little a veſſel as one of our receivers, can, by its weight, counter-balance ſo ponderous a cylinder of quick-ſilver: whence we may be allow'd to argue, that the air ſuſtains it by ſuch a preſſure, or ſpring, as we plead for, whether that proceed from the texture of the aerial particles, from their motion, or from both.

The laſt of Mr. *Hobbs*'s principal explanations, is of the experiment wherein above 100 pound weight, being hung at the depreſs'd ſucker, the ſucker was, notwithſtanding, impell'd up again, by the air, to the top of the cylinder. This phenomenon Mr. *Hobbs* accounts for thus. "The air being beaten back by the retraction of the ſucker, and finding no "void place, wherein to diſpoſe of itſelf, beſides that which it may make, "by driving out other bodies, is, by perpetual truſion, at length, forc'd "into the cylinder, with ſo great ſwiftneſs, between the concave ſurface "of the cylinder, and the convex ſurface of the ſucker, as may anſwer

"the

PNEUMATICS. "the power neceſſary to draw back the ſucker. Now, the air within "retains the ſwiftneſs wherewith it enters; and then, every way, diſtends "the ſides of the braſs cylinder, which is elaſtic. Therefore, the air in "the cylinder, being vehemently mov'd, thruſts againſt all parts of the "concave ſurface of the cylinder, tho' in vain, till the ſucker is drawn "back; but, as ſoon as the ſucker, being looſe, ceaſes to impel the air, "that air which was before driven in, will, by reaſon of its endeavour a-"gainſt every point of the internal ſuperficies of the cylinder, and of the "elaſtic force of the air, inſinuate between the ſame ſurfaces, with a velo-"city equal to that by which it was impell'd; that is, with a velocity which "anſwers the ſtrength of the impulſe: when, the ſwiftneſs wherewith the "the ſame air goes out of the cylinder, finding no where to diſpoſe of "itſelf, will, again, impel the ſucker to the top of the cylinder; for the "ſame reaſon that the ſucker, a little before, made an impulſe upon the "air." But this whole conceit, of the air's running in and out, with a ſtrange velocity, between the ſucker and the cylinder, is precarious; nor does he propoſe one phenomenon to countenance it. Farther, we found, in an engine ſo contriv'd, that the pump lay cover'd with water, when the ſucker was drawn back, the atmoſphere would ſtrongly preſs the water a-gainſt it; and if the handle were let go, ſwiftly repel up the ſucker into the deſerted cavity of the cylinder; which being a caſe parallel to that under conſideration, let any unbiaſs'd perſon judge how likely it is that the air could perform all theſe excurſions, without exciting bubbles, not-withſtanding the water's conſtant interpoſition betwixt it and the cylinder. And there is as little probability in what our author teaches of the air's *conatus*. I might here repeat, what we formerly mention'd, of the break-ing of our receivers inwards, not outwards; and add, that I ſee no rea-ſon why the *conatus* of the included air, if granted, ſhould be fruſtrated, when the ſucker is depreſs'd; but, if the air within have ſo ſtrong an en-deavour outwards, as to ſtretch the thick ſides of the braſs cylinder, as Mr. *Hobbs* would have it, I demand, why the air does not throw out the wooden peg, or valve, which we have often, to our trouble, ſeen thrown out with great force and noiſe, when, the depreſs'd ſucker being thruſt up again, whilſt there was air in the cylinder, we forgot to leave the valve open; tho', in this caſe, the air, that drove out the peg, was far enough from ſtretching the cylinder. I further demand, how it comes to paſs, that if, having ſtopp'd the hole of the cylinder, with your finger, inſtead of the peg, you ſwiftly depreſs the ſucker, you ſhall be ſo far from feeling a preſſure outwards, againſt the pulp of the finger, from any thing con-tain'd in the cavity of the cylinder, that your finger will be ſtrongly preſs'd in by the ambient air. But, as to the laſt part of the paſſage, under conſi-deration, I do not underſtand why the air that was impell'd in ſo ſwiftly, betwixt the cylinder and the ſucker, ſhould not reſiſt the ſwift egreſs which Mr. *Hobbs* aſcribes to the included air, by the ſame paſſage; nor why this impell'd air, that has ſo ſtrong an endeavour outward, ſhould ne-ver depreſs the ſucker, as well as the ſame air, diffuſing its motion thro' the

the vaſt ambient medium can enable the external air to thruſt up the ſucker again; eſpecially, ſince during ſuch a depreſſion of the ſucker, made by the rebound of the air, forcibly impell'd in from the cloſe bottom of the cylinder, the air from without, may, all this while, according to Mr. *Hobbs*'s principles, get in between the ſaid ſucker and the cylinder. But, I further ſay, that the lifting up of the ſucker, either is not neceſſary to prevent a vacuum; or, that, in ſome caſes, it will be hard to ſhew how a vacuum can, by Mr. *Hobbs*, be avoided. For, when the depreſs'd ſucker is ready to be thruſt up again, if you hang a much greater weight at it, than a hundred, it will not be lifted up at all. And whereas this progreſs, and regreſs of the impulſe of the external air, cannot reaſonably be ſuppos'd to be very laſting, you may, by a competent weight, detain the ſucker depreſs'd, till the ambient air is as quiet as uſual; yet, if you then take off the overplus of weight, or, perhaps, a little more; in caſe the pump has been very ſtanch, the ſucker, and the great weight hung on, will, nevertheleſs, be carried up: which 'tis no way likely it could be, by the impulſe of the outward air, that had time to decay, and be confounded. And, as for the inward air, we have prov'd, that it has no ſuch endeavour outwards, as Mr. *Hobbs* pretends; otherwiſe, why ſhould not that throw out the ſucker, rather than cauſe it to be impell'd inward; it being no way likely, that, in caſe ſome air ſhould get out of the cavity of the cylinder, it could ſo move the outward air, as that the reflex of that impulſe ſhould make that free outward air bear more ſtrongly againſt the outſide of the ſucker, than the inſide of the ſame ſucker is preſs'd againſt by the included air, whoſe impetus is, incomparably, leſs diffuſed? In ſhort, whatever is reſolv'd to be in the cavity of the cylinder, when the ſucker is depreſs'd, yet, ſince 'tis manifeſt, that 'tis at leaſt, in great part, deſtitute of common air; and ſince the ſucker, with the weight, may, if the inſtrument leak not, be impell'd up, when, in all probability, thoſe forc'd undulations of the air, that may be ſuppos'd to have been made by the ſucker, have ceas'd; the *Carteſians*, Mr. *Hobbs*, and others, who will not have recourſe to the unintelligible attraction of ſome rarified ſubſtance within, muſt aſcribe ſo ſtrange a phenomenon to the preſſure of the air without. *

PNEUMATICS.

The reaſon why water aſcends in ſmall glaſs tubes, Mr. *Hobbs* tells us, is, that "the ſmall particles interſpers'd in the air, near the water, by "their motion beat upon the ſurface of the water, ſo that, the fluid "muſt, of neceſſity, aſcend into the pipe; and the more ſenſibly into "one exceeding ſlender." But 'tis manifeſt, that I did not, in relating this phenomenon, take upon me to aſſign the true reaſon of it. However, I

* That experiment of Mr. *Hauksbee*'s, whereby it appears, that 140 pound weight was requiſite to ſeparate two hemiſpheres, 3 ½ inches in diameter, ſqueez'd together, by an additional atmoſphere of air; proves the preſſure of that fluid, beyond all contradiction, againſt the *Funicular* hypotheſis, that of ſuction, &c. eſpecially conſidering that, when the air was exhauſted out of theſe hemiſpheres, and an atmoſphere injected upon their external ſurfaces, the weight of 280 pound was neceſſary to cauſe their ſeparation. See *Hauksbee*'s *Experiments*, p. 88—93.

PNEUMATICS. am no way satisfied with Mr. *Hobbs*'s explanation: for, to say nothing of the motion he ascribes to the particles dispers'd thro' the air, he leaves the difficulty unsolved; since there being common air, as well within the cavity of the slender pipes, as without, he offers no reason why the pressure of the air within, should not resist the pressure of the same kind of air without, as we see it does in greater pipes. And, possibly, he would have past by this particular, if he had not overlook'd the advertisement I gave, that it would concern those who should undertake to shew the causes of this phenomenon, to consider, also, of a reason why, if the experiment be try'd with quick-silver, instead of water, the surface of that fluid will, instead of being higher, be lower within the pipe, than without it; whereas, if Mr. *Hobbs*'s explication be sufficient, why should not the contrary happen in quick-silver, as well as in water? The next passage to be consider'd, is this: "If any one, after the frequently repeated impulse, and retraction of the sucker, endeavour to draw out the stopple of the upper orifice of the receiver, he shall find it gravitate very much; as if a weight of many pounds hung upon it. Whence comes this? From a strong, circular endeavour of the air within the receiver, made by the violent ingress of the air, between the convex surface of the sucker, and the concave of the cylinder; procured by the repeated impulse, and revulsion of the sucker, improperly call'd the exsuction of the air: for, by reason of the fulness of nature, the stopple cannot be drawn out; but whilst that circulates very swiftly, it comes out very hardly; that is, it seems to be very heavy: for, as soon as fresh air is, by degrees, let into the receiver, it likewise, by degrees, loses this seeming gravity." To this I answer, first, that if there be such a vehement, circular endeavour of the air, in the receiver, by which motion he teaches, that the air rushes out with violence enough to make the atmosphere lift up, in our cylinder, above 100 pound weight; I see not, why it should not, rather, throw out the stopple, under consideration, than hinder its extraction: and why, when the external air is re-admitted at the stop-cock, into the exhausted receiver, and thereupon there sensibly follows, for a little while, a rotation of the included air; the stopple, that, just before, seem'd so much to resist the being drawn out, should cease to make any such resistance. Nor do I see how the plenitude of nature should, as is here intimated, hinder the extraction of the stopple; for, according to the plenists, the world, and the receiver, must be, at all times, equally full. And if the contiguous air must, for Mr. *Hobbs*'s reason, necessarily be extracted with the stopple, in one case, I see not why the like should not happen in another. Now, it appears, by our experiments, that there is no such very strong, circular endeavour, in the exhausted receiver, as he pretends; but, indeed, an endeavour of the ambient atmosphere, to press the parts of the glass inwards, and cover those that are contiguous to it. For, as I have noted already, a light bladder, suspended in the receiver, manifested no such motion, as he here supposes; neither were a pair of scales suspended within the same; nor a long magnetical needle, that

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rested upon the point of another needle, at all whirl'd about, by this imaginary motion of the air. Besides, if you leisurely loosen the brass stopple, so that it may be very near, but not contiguous to the sides of the socket, you shall, manifestly, perceive a strong current of air to flow into the receiver, at that passage; and more than once, when, instead of that piece of brass, we stopp'd the hole in the cover, with our cement, we observ'd sometimes whilst we were pumping, sometimes after we had done pumping, that the outward air, by degrees, depress'd the superficies of the cement expos'd to it, and made it concave; and, now and then, would break through it, thrusting it inward with great violence and noise.

Mr. *Hobbs*, afterwards, attempts to rectify another of our explanations, in these words. "We have seen, also, that water let down into the receiver, after some returns of the motion of the sucker, has bubbled so, as if it had boil'd over a fire; which happens by reason of the swiftness of the circulating air: unless, perhaps, you find the water hot too, whilst it bubbles. For if we were sure it was hot, we must find out some other cause of the phenomenon. But we are certain it is not sensibly hot; how, therefore, can the greater, or less motion of the atmosphere, promote such a motion as this?" But, I confess, I see not how the circular motion of the air, within the receiver, could, in a vial, with a long neck, produce such effects as we mention; especially I see not how such a wind, passing along the surface of the water, could raise so many very large bubbles, which seem'd, generally, to rise from the lower parts of the water, thro' the long neck of the vial, and spout it, as it were, into the receiver.

As for what he says, about the gravity of the atmosphere, 'tis plain enough, that my conjecture ascribes the phenomenon to the taking off the pressure of the air within the receiver; tho' I see not why the removal of the weight of the atmosphere, if it could be effected, out of the engine, should not have a like operation. And that the greater, or less pressure of the air concurs to this phenomenon, appears from common experience; for water, by being heated, is expanded, and generates bubbles: and our experiments have made it appear, that there are aerial particles in water, and other liquors, which tend to expand themselves; and actually do so in numerous bubbles, when the pressure of the incumbent air is considerably lessen'd. In the present phenomenon, that pressure being, by the exhaustion of the receiver, taken off, the aerial particles, and agitated vapours abounding in the hot water, are allow'd to expand themselves; and to make such numerous, and great bubbles, as thereby to carry off a large part of the water out of the vial. So that I wonder what makes Mr. *Hobbs* speak, as if there were no sensible heat in the water, under consideration; since 'tis expresly said, that 'twas put in hot; and, if it were put in cold, could, by no pumping, be brought to the least shew of effervescence. And, I have already shewn, by experiments, that there is, in our exhausted receiver, no such peculiar motion of the air, as he ascribes the phenomenon to. Nay, when there is, manifeſtly, a whirling motion of the air in the glaſs, upon the admiſſion of the external air, the production of numerous bubbles in the water, preſently ceaſes. And, therefore, Mr. *Hobbs* might have let alone my conjecture, unleſs he could either have diſprov'd it better, or ſubſtituted a more probable one in its ſtead.

Mr. *Hobbs*, having recited our experiment of killing animals, in the exhauſted receiver, in two or three minutes, asks; "if theſe animals were "ſo quickly kill'd, for want of air; how do divers live under wa"ter: for ſome of them, accuſtom'd to it from their childhood, will ſuſtain "the want of air for a whole hour? Certainly, that moſt vehement "motion, by which included bladders are diſtended, and broken, kills "theſe animals." But, I ſtill think, we want ſome clearer diſcovery about this matter, notwithſtanding what has been deliver'd concerning it, by Mr. *Hobbs*; for, his argument, againſt my conjecture, is anſwer'd by himſelf, ſince he plainly intimates, that divers, who can live without air for a whole hour, are accuſtom'd to it from their childhood. Wherefore, unleſs the animals that died in our engine, had been, for a long time, train'd up, by degrees, to live without air, it will not follow, that the want of it would not ſoon diſpatch them; as ordinary men may be drown'd in a few minutes. And, having purpoſely let down ſome mice, and ſmall birds, into a deep glaſs, fill'd with water; and kept them from emerging, by a weight tied to their legs, or tails; tho' ſome liv'd longer than others, yet I obſerv'd them to be kill'd faſt enough, to keep my conjecture from being incredible: eſpecially, the laſt we made trial of, tho' a large and luſty mouſe, appear'd to be quite dead, within ſomewhat leſs than a minute. And, we particularly took notice, that, before drowning, ſeveral bubbles, which ſeem'd to be the reſpired air, came out of their mouths, and aſcended thro' the water. We have already diſprov'd the ſuppoſition whereon Mr. *Hobbs*'s opinion, as to the cauſe of the death of animals, in the receiver, is built; for no ſuch vehement motion, as he pretends, is there to be found. Nor does it, at all, clear the difficulty, that he would have this motion the ſame whereby included bladders are diſtended, and broken. For, 'tis very hard to conceive, how the tenacity of the air; or its beating from all parts, upon the convex ſurface of an almoſt empty bladder, (for, in ſuch, alſo, the experiment will ſucceed) ſhould make it burſt outwards. And, we have already prov'd, that the diſtenſion, and breaking of bladders, in our receiver, proceeds not from any ſuch motion of the neighbouring air, as is here preſumed, but from a quite different cauſe.

Mr. *Hobbs* has a long diſcourſe concerning the extinction of fire, in our receiver, upon exhauſting the air; and, he endeavours to reduce what happens to kindled coals, placed in our engine, to what happens in certain mines, wherein, when ſome thick damps aſcend, both charcoal, and candles are ſoon extinguiſh'd. He, alſo, attempts to ſhew, that, by the reciprocation of the ſucker, the air, impell'd firſt into the cylinder, and then into the receiver, is put into ſuch a motion, as gives it a certain middle consistence, betwixt that of pure air, and that of water. But I shall not need to examine this second part of his discourse, because I deny the first; and being able to disprove the thickness of the air, in the exhausted receiver, I need not spend time about what he teaches of the manner. His history of the damp, to be met with in mines, is more largely set down in his elements of philosophy; where he seems to mention it as a story of doubtful credit, which 'tis likely he would not have done, if he had then seen it; and if he has not, since, observ'd the thing himself, there may easily be a mistake in some of the circumstances: as, for instance, the number of minutes wherein the thick air choaks the fire; and 'tis upon that circumstance, the validity of what he deduces from the observation, chiefly depends. But, however the matter stands with these subterraneal damps, we have already prov'd, by several of the experiments of our engine, that, in the exhausted receiver, there is no such motion of the air, as is here supposed. And it may be sufficiently prov'd, that whatever remains in the receiver, is not such a substance as Mr. *Hobbs* would have it; for, he here tells us, it is of a consistence betwixt air and water; and, in his elements, says, 'tis not much lighter than water. But, by the *Magdeburgic* experiment, 'tis evident, that the receiver, by being exhausted of common air, is so far from growing heavier, that it lost above an ounce of its former weight. And to this agrees, what happens in æolipiles, that grow lighter when the air is expell'd. Besides, if the receiver be, in the present case, fill'd with a substance, whose consistence is so much nearer that of water, than is our common air, as Mr. *Hobbs* would have it, why does not a pendulum move sensibly slower in it; when, in water, the vibrations are so exceedingly slacken'd? And, the breaking of an hermetically seal'd bubble, in our receiver, outwards, when the air was much exhausted, and not before, with several other experiments, that might be easily apply'd to this purpose, sufficiently prove, that 'tis not a thicker, and far heavier air, but a more yielding, and lighter, that remains, after pumping, in the cavity of our receiver. But, as for the thing itself, when I related it, I thought it might admit a farther enquiry: and, indeed, there may be many ways of extinguishing fire; so that, as 'tis not, in all cases, easy to assign the true cause of its extinction; 'tis unsafe to conclude, with Mr. *Hobbs*, that because a candle, or a live coal, may be extinguish'd by a thick damp, therefore the effect must proceed from the like cause, in our receiver, where there is no sign of any damp, or unusual thickness of the air, but of the contrary.

Mr. *Hobbs* next, pretends to shew, that neither the spring, nor weight of the air, have any thing to do with the two flat polish'd marbles, not falling asunder in our receiver; "because," says he, "the weight of "the atmosphere can do nothing there:" he adds, "that a stronger, or "more evident argument could not be brought against a vacuum, than "this experiment. For, if two coherent marbles should, either of them, "be thrust forwards that way that their surfaces lie contiguous, they

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PNEUMATICS. "would eaſily be ſever'd; the neighbouring air ſucceſſively flowing into "the deſerted place. But ſo to pull them aſunder, that, at one time, "they ſhould loſe their whole contact, is impoſſible; the world being "full: for then, either motion muſt be made from one term to another, "in an inſtant; or, two bodies, at the ſame time, muſt be in the ſame "place: to ſay either of which, is abſurd." But how this ſhould be ſo cogent, and manifeſt an argument, againſt the vacuiſts, I do not well diſcern; for what it proves, if it prove any thing, ſeems to be, that in caſe the cohering marbles could be ſo ſever'd, as to loſe, at once, their whole contact, the world might be concluded not to be full; but, I ſee not how it thence follows, that therefore there can be no vacuum. For my part, I would demand, whether the ſtrong adheſion of the marbles be neceſſary, or not, to the plenitude of the world? If it be, how chance a ſufficient weight, hung to the lower marble, can immediately draw them aſunder? And, if it be not, why does not Mr. *Hobbs* aſſign ſome other cauſe of their ſo ſtrong adheſion, if it depend neither upon the ſpring, nor weight of the air? As for the non-ſeparation of the two marbles, in our receiver, I have ſaid, that the cauſe may, probably, be the preſſure of the air, remaining in the receiver; for, 'tis no way unlikely, that the remaining air ſhould be able to ſuſtain a weight of four or five ounces, hanging at the lower marble; ſince the free air was able to ſupport between four and five hundred ounces, hanging at the ſame.

But Mr. *Hobbs* tells us, that the cauſe I aſſign of the coheſion of our marbles, is liable to great inconveniencies; "for they confeſs," ſays he, "that all ponderation is an endeavour, every way, by right lines, "to the center of the earth: ſo that it is made not by the figure of a "cylinder, or column, but by a pyramid, whoſe top is the center of "the earth; and whoſe baſis is part of the ſurface of the atmoſphere." As if it were material whether a body, whoſe baſis is ſcarce two inches diameter, and length ſome thouſands of miles, be conſider'd as a cylinder, or a pyramid. Certainly, *Stevinus*, and other learned hydroſtatical writers, would ſcarce have made this an objection; ſince they ſcruple not to poſtulate, that all perpendiculars, not very diſtant, be look'd upon as parallel; tho' they allow ſuch perpendiculars would meet in the center of the earth. What he adds, "that, therefore, the endeavour of all "the points, which ponderate, will be propagated to the ſurface of "the upper marble, before it can be propagated further to the earth;" to prove that the whole endeavour of the pyramid, that reſts upon the upper marble, is terminated there; and that there is no endeavour of the atmoſphere, againſt the under ſuperficies of the lower-moſt marble; ſeems grounded, partly, upon a conceit of his, about the nature of gravity: according to which, I ſee not why any body, plac'd between the ſides of that pyramid, or cone, whereof the upper ſuperficies of the higher marble is the baſis, ſhould deſcend upon the account of gravity; and, partly, from a miſtake of my opinion: for, I no where ſpeak as if

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I thought the lower marble was ſuſtain'd by little globules, or other minute bodies, protruding one another, directly towards the center of the earth, and rebounding from a perfectly ſmooth ſurface. Nor, need I ſay, that the lower ſtone is ſuſtain'd by the preſſure of the ſame pillar of the atmoſphere, as reſts on the upper; ſince other parts of the atmoſphere, ſome on the one hand, and ſome on the other, preſſing obliquely upon the uneven ſurface of the earth, may have their preſſure upward, terminated againſt the lower ſurface of the undermoſt marble. Thus, an æolipile being, by heat, well freed from air; if the little orifice, at the extremity of the neck, by which the air gets in and out, be ſoon carefully ſtopp'd with wax, and afterwards ſuffer'd to cool, there will not be, in the cavity of the æolipile, a reſiſtance near equal to the preſſure of the outward air. And, therefore, if you perforate the wax, that air will be violently impell'd in, at the unſtopp'd orifice, whether the neck be held parallel, or perpendicular to the horizon; or, in any other ſituation, in reſpect of the center of the earth. And the like will happen, if the æolipile be unſtopp'd under water. PNEUMATICS.

And, I lately met with the relation of an experiment, which abundantly makes out the power of the ambient atmoſphere, to preſs bodies againſt each other, when it cannot get between their internal ſurfaces. The ingenious author of the *Magdeburgic* experiment, writes to the induſtrious *Schottus*, that having caus'd two copper plates to be made, almoſt in the form of ſcales, a little above half an ell in diameter, and exactly fit; if when laid upon one another, "the air be drawn "out, they are kept ſo compreſs'd, and united, by the gravity of the "external air, that ſix ſtrong men cannot pull them aſunder; but if, at "length, they are disjoin'd, they make a noiſe equal to the report of "a muſquet; yet as ſoon as ever there is the leaſt entrance given to "the air, they fall aſunder of their own accord." And, if a glaſs vial have a pipe open at both ends, ſo fitted to it, that no air can get in or out, betwixt the neck and it; and if the vial be ſo far fill'd with water, that the lower-end of the pipe be well immers'd therein; if you ſuck at the upper-end of the pipe, the water will aſcend to a great height; which argues its being forc'd upwards, by the oblique preſſure of the air in the vial: for 'tis only in the pipe, and not in the vial, that there is any air in the ſame perpendicular with the water that is impell'd.

As to the aſcent of liquors by ſuction, Mr. *Hobbs* ſays, "whoever "ſucks water into his mouth by a pipe, firſt ſucks up the air between; "whereby he removes the external diſtended air, which can have no "place, but by removing the next; and ſo, by continual pulſion, the "water is, at length, driven into the pipe, and ſucceeds the air which "is ſuck'd out." And this account of the phenomenon is, alſo, given by the learned *Gaſſendus*, and other atomiſts; and ſeems generally acquieſc'd in by the modern philoſophers. But, though I deny not, that

many

PNEUMATICS. many phenomena of nature, may be, probably, explain'd by this propagation, and return of motion; yet there are some others, here below, which I see not how the *Cartesians*, the atomists, or Mr. *Hobbs*, can solve, without admitting the spring of the air; and which, perhaps, may, by the spring of the air, be explain'd, without recurring to such a propagation, and return of impulse. I took a glass vessel, consisting of two parts; the one, a vial, capable of containing about a pound of water; and the other, a pipe, open at both ends; the lower of which reach'd within two inches of the bottom of the vial: this pipe was fasten'd into the neck of the vial, with melted glass, of a good thickness; and into the vessel, by the open pipe, I pour'd water enough to swim a pretty way above the lower-end of the pipe; and then often inclin'd the vessel to give a free intercourse betwixt the air within the vial, and that without it: whence, if the external air were compress'd by the affusion of the water, it might free itself, as it readily did, by ascending in bubbles along the inclined pipe, till the outward and inward air were reduced to an equality of pressure. Now, if all suction were produced by the pressure of the air, thrust away by the dilated chest of him that sucks, and so thrusting the fluid into the pipe, it seems evident, in our case, that the water would not ascend by suction; since by the contrivance of the vessel, the air thrust away by him that sucks, cannot, at all, come to press upon the water. Yet, whether the pipe were inclin'd, or erect, the water ascended, upon suction, to the top of the pipe, and ran over into my mouth, and that with tolerable ease: for tho' the spring of the air, pent up in the vial, were able, upon the decrease of the pressure of the outward air, occasion'd by my sucking, to impel the water strongly into the pipe; yet, when a moderate quantity of water had been so impell'd, the included air, gaining, thereby, more room to expand its spring, was so much weaken'd, that the water ascended far less easily than in ordinary suction. And, to shew that the ascent of the water, in the pipe, did not proceed from any such tendency, in the water itself, to ascend, for prevention of a vacuum; I carefully took out the water, by degrees, till the lower-end of the pipe lay but very little under the surface of the water; tho' in the cavity of the pipe, the water was considerably higher: then suffering the vessel to rest, and sucking at the upper-end of the pipe, the water was impell'd up, yet without reaching near the top, till the surface of it was fallen a little below the bottom of the pipe. But then, though I continu'd sucking, no more water ascended into the pipe; but the air passing thro' it, towards my mouth, in its passage toss'd up the water that was already in the pipe, and turn'd it into bubbles, which broke with noise one after another. And thus the ascending air long kept the water, in the pipe, from falling back to that in the vial; but, when I remov'd my mouth, the spring of the air, remaining in the cavity of the vial, being weaken'd by the recess of the air, I had suck'd out,

it

it was not able to reſiſt the preſſure of the outward air; and, accordingly, the water in the pipe, was not only depreſs'd into the vial, but the external air forc'd its way, in many bubbles, and not without ſome noiſe, through the water contiguous to the bottom of the pipe, till the preſſure of the included air, and that of the atmoſphere, were reduced to an equality. PNEUMATICS

REMARKS

UPON

Mr. *HOBBS*'s Problems,

ABOUT A

VACUUM.

I Here only propofe to confider, whether Mr. *Hobbs* has cogently prov'd his affertion, that there is no fuch thing as a vacuum ; and whether he has rightly explain'd thofe phenomena of nature, which he undertakes to folve; and efpecially thofe produc'd by means of our engine.

Arguments againft a vacuum confider'd.

And, firft, as I account for the adhefion of two flat polifh'd marbles, by the preffure of the air, Mr. *Hobbs* thinks this phenomenon fo fully proves the plenitude of the world, that tho' he tells us, he has many ftrong arguments to make it out, yet he mentions but this one, as abundantly fufficient; for, fays he, fince the vacuifts allow interfpers'd vacuities, not only in the air that furrounds the join'd marbles, but in the reft of the ambient air, there is no reafon why there fhould be any difficulty in feparating the marbles; or, at leaft, any greater difficulty than in moving them in that air, after their feparation. But they will eafily anfwer, that, notwithftanding the vacuities they admit in the ambient air, a manifeft reafon may be given, on their hypothefis, of a difficulty in the divulfion of the marbles: for, the vacuities they admit, being but interfpers'd, and very fmall ; and the corpufcles of the atmofphere being, according to them, endow'd with gravity, there reft fo many upon the upper furface of the marble, that this ftone cannot be, at once, perpendicularly drawn up from the lower, contiguous to it, without a force able to furmount the weight of the aerial corpufcles that prefs upon it. And this weight has already fo condens'd the neighbouring parts of the ambient air, that he, who would per-

perpendicularly raiſe the upper marble from the lower, needs a conſiderable force to make the revulſion, and compel the contiguous parts of the incumbent air, to enter the pores, or intervals intercepted between them. For, the force of him, who endeavours to raiſe the upper marble, whilſt the lower ſurface of it is fenc'd from the preſſure of the atmoſphere, by the contact of the lower, which ſuffers no air to come in between them, is not aſſiſted by the weight, or preſſure of the atmoſphere; which, when the marbles are once ſeparated, preſſing as ſtrongly againſt the under ſurface of the upper marble, as the incumbent atmoſpherical column does againſt the upper ſurface of the ſame marble; the hand which endeavours to raiſe it, in the free air, has no other reſiſtance, than that ſmall one of the marble's own weight to ſurmount.

And as for the reaſon which Mr. *Hobbs*, and, as he thinks, all others give of the difficulty of this divulſion, "that the whole ſpace between "the two ſeparated marbles, cannot be inſtantaneouſly fill'd by the air, "let the ſeparation be ever ſo ſudden;" the pleniſts may give a more plauſible account of this experiment, than he has here done; and, therefore, abſtracting from the two oppoſite hypotheſes, I may ſay, that the genuine cauſe of the phenomenon ſeems to be that which I have already aſſign'd; and that the difficulty of raiſing the upper ſtone, attending the air's not being able to come in all at once, to poſſeſs the ſpace left between the ſurfaces of the two marbles, upon their ſeparation, proceeds from hence; that, till this ſpace be fill'd with the atmoſpherical air, the hand, which would raiſe the upper marble, cannot be fully aſſiſted by the preſſure of the air, againſt the lower ſurface of that marble: for, ſince I declare not for the hypotheſis of the pleniſts, as 'tis maintain'd by Mr. *Hobbs*, I am not bound to allow, what the common explanation, adopted by my adverſary, ſuppoſes, that either nature abhors a vacuum; or, that there could be no divulſion of the marbles, unleſs at the ſame time, the air were admitted into the ſpace that divulſion makes for it. And a vacuiſt may ſay, provided the ſtrength employ'd, to draw up the upper marble, be able to ſurmount the weight of the aerial corpuſcles, accumulated upon it; the divulſion would enſue, though no air, or other body, ſhould be permitted to fill the room made for it, by the divulſion; and that the air's ruſhing into this ſpace, does not, neceſſarily, accompany, but, in order of nature, and time, follow, upon a ſeparation of the marbles; the air that ſurrounded their contiguous ſurfaces, being, by the weight of the collateral air above, impell'd into the room newly made by the divulſion. But, having purpoſely, in our pneumatic receiver, accommodated two flat and poliſh'd marbles, ſo that the lower being fix'd, the upper might be laid upon it, and drawn up again, as there ſhould be occaſion; I found, that if, when the receiver was well exhauſted, the upper marble was, by a certain contrivance, laid flat upon the lower, they would not then cohere, as before, but, with great eaſe, be ſeparated; tho' it did not, by any phenomenon, appear, that any air could ruſh in, to poſſeſs the place given it by the receſs of the upper marble; whoſe

PNEUMATICS. very eaſy avulſion, ſeem'd owing to the preſſure of that little air remaining in the receiver, being too faint to make any conſiderable reſiſtance to the ſeparation of the upper marble ; whence the hand, that drew it up, had very little more to overcome, than the ſingle weight of the ſtone.

But the cauſe Mr. *Hobbs* aſſigns for this phenomenon, is improbable ; and a better has been aſſign'd already. For, firſt, he requires, to the divulſion of the marbles, a force great enough to ſurmount the hardneſs of the ſtone. But, this is *gratis dictum* ; and, it ſeems very unlikely, that ſo ſmall a weight, as will ſuffice to ſeparate two coherent marbles, of about an inch, for inſtance, in diameter, ſhould be able to ſurmount the hardneſs of ſuch ſolid ſtones, as we uſually employ in this experiment. And, tho' it be, generally, judg'd more eaſy to break a broader piece of marble, *cæteris paribus*, than a much narrower ; yet, neither I, nor, I believe, Mr. *Hobbs*, ever obſerv'd any difference in the reſiſtance of marbles to ſeparation, ariſing from the greater or leſs thickneſs of the ſtones. Yet, I find, by conſtant experience, that, *cæteris paribus*, the broadneſs of the coherent marbles, exceedingly increaſes the difficulty of disjoining them.

But Mr. *Hobbs*, upon the ſuppoſition of the world's plenitude, illuſtrates our phenomenon, by drawing aſunder the oppoſite parts of a piece of wax : a very improper inſtance, ſurely ! For, the parts to be divided in the wax, are of a ſoft, and yielding conſiſtence, and, according to him, of a ductile nature ; and not, as the parts of the coherent marbles, very ſolid and hard. The parts of the wax, alſo, do not ſtick together, barely, by a ſuperficial contact of two ſmooth planes, as do the marbles we are ſpeaking of ; but have their parts intangled with one another ; and, therefore, they are far from a diſpoſition to ſlide aſunder, like the marbles. Beſides, 'tis manifeſt, that the air has opportunity to ſucceed in the places ſucceſſively deſerted by the receding parts of the attenuated wax : but 'tis neither manifeſt, nor well proved by Mr. *Hobbs*, that the air, after the ſame manner, ſucceeds between the two marbles ; which, as I lately noted, are not forced aſunder, after ſuch a manner ; but, as himſelf ſpeaks, ſever'd, in all their points, at the ſame inſtant.

In the ſecond place, a better ſolution of the phenomenon has been already given, from the preſſure of the atmoſphere, upon all the ſuperficial parts of the upper marble, except thoſe that touch the plane of the lower. And to ſhew, that when two coherent marbles are ſuſtain'd, horizontally, in the air, the cauſe why they are not to be forc'd aſunder, if they have two or three inches in diameter, without the help of a conſiderable weight, is the preſſure of the ambient air ; I caus'd two ſuch coherent marbles to be ſuſpended in a large receiver, with a weight at the lower, that might help to keep them ſteddy ; but was very inconſiderable to that which their coheſion could have ſurmounted : then cauſing the air to be pump'd, by degrees, out of the receiver, the marbles long ſtuck cloſe together ; becauſe, during that time, the air could not be ſo far exhauſted, but that there remain'd enough to ſuſtain the ſmall weight, which endeavour'd their divulſion. But, when the air was further evacuated, at length, the ſpring

of

of the little expanded air, that remain'd, being grown too weak to ſuſtain the lower marble, and its ſmall clog, they dropp'd off. And, to confirm my explanation of our phenomenon, I ſhall add, that as this trial, which I had, ſeveral times, occaſion to repeat, ſhews the coheſion of our two contiguous marbles, would ceaſe, upon withdrawing the preſſure of the atmoſphere; ſo, by another experiment, it appears, that ſuch a preſſure ſufficed to cauſe that coheſion: for, having found, that when the receiver was well exhauſted, two marbles, tho' conſiderably broad, being laid upon one another, after the requiſite manner, their adheſion became ſo weak, that the upper would be eaſily drawn up from the other, we laid them again one upon the other; and then letting the external air flow into the receiver, we found that the marbles now adhered ſo well together, that we could not raiſe the upper without the lower.

But, farther, Mr. *Hobbs*, in arguing againſt our engine, unjuſtly ſuppoſes the whole deſign of it to be the proof of a vacuum; for, or againſt which, I have never yet declared. *

Beſides, he ſeems not to have rightly underſtood, or, at leaſt, not to have ſufficiently regarded, in what, chiefly, conſiſts the advantage which the vacuiſts may make of the air-pump, againſt him. In ſeveral places he is very ſollicitous to prove, that the cavity of our pneumatical receiver, is not, altogether, empty; but, the vacuiſts may tell him, that, ſince he aſſerts the abſolute plenitude of the world, he muſt reject, not only great vacuities, but, alſo, thoſe very ſmall, and interſpers'd ones, which, they ſuppoſe, are intercepted between the ſolid corpuſcles of other bodies, and particularly

* The moſt cautious, and reſerv'd philoſopher, need, now, make no ſcruple to aſſert a vacuum. Sir *Iſaac Newton* has furniſh'd us with ſeveral arguments, which are deciſive of this point. Thoſe, indeed, who make the eſſence of matter to conſiſt in extenſion, may well deny a vacuum; but the nature of gravity, which is proportionable to the matter, or ſolid content of bodies, fully demonſtrates, much the largeſt part of the univerſe to be empty ſpace. The motions of the comets, alſo, demonſtrate a vacuum; for, theſe move in all manner of planes, and directions, thro' the vaſt celeſtial ſpaces, without any ſenſible reſiſtance. The vibrations of pendulums confirm the ſame; for, if they be made to move in a ſpace free from air, they, likewiſe, meet with no reſiſtance; and, therefore, there is no ſenſible matter remaining in that ſpace. If there were no vacuum, a projectile in the air, or, in a ſpace deſtitute of air, would move as ſlowly as in quick-ſilver; for, the reſiſtance of fluid mediums is nearly in proportion to their denſity: and, 'tis a great miſtake, to think that the reſiſtance of projectiles will decreaſe *in infinitum*, by an infinite diviſion of the parts of the fluid; for, 'tis plain, that reſiſtance cannot be conſiderably leſſen'd, by dividing the parts of the fluid. For, why ſhould not the ſame quantity of matter have the ſame power of reſiſtance, when divided into many ſubtile parts, as when divided into a few that are larger? A ſphere, moving in a compreſs'd fluid at reſt, and of the ſame denſity with itſelf, would loſe half its motion, before it had gone twice the length of its diameter.

In another place, the ſame great philoſopher argues thus. " The reſiſtance of " water ariſes principally, and, almoſt, " intirely, from the *Vis inertiæ* of its mat" ter; and, by conſequence, if the hea" vens were as denſe as water, they would " not have much leſs reſiſtance than wa" ter; if as denſe as quick-ſilver, they " would

PNEUMATICS. larly, of the air: whence it would not confute them to prove, that, in our receiver, when moſt thoroughly exhauſted, there is not one great and abſolute vacuity; or, as they ſpeak, a *vacuum coacervatum*; ſince ſmaller, and diſſeminated vacuities would ſerve their turn. And, therefore, they may think their pretenſions highly favour'd, as by ſeveral particular effects, ſo by this general phenomenon of our engine, that the common, or atmoſpherical air, which, before the pump was work'd, poſſeſs'd the whole cavity of our receiver, is, by the contrivance of the pump, made, in a great meaſure, to paſs out of the cavity into the open air, without being able, at leaſt, for a while, to get in again; yet it does not appear, by any thing Mr. *Hobbs* has alledg'd, that any other body ſucceeds, adequately to fill the ſpaces deſerted by ſuch a multitude of aerial corpuſcles.

Glaſs impervious to air.

And, tho' he endeavours to prove our receiver to be always full of air; yet, that the common air cannot enter thro' the pores of glaſs, appears by the following experiment. We took a bubble of thin white glaſs, about the bigneſs of a nutmeg, with a very ſlender ſtem, four or five inches long, and of the thickneſs of a crow's quill; and holding the end of this ſtem in the flame of a lamp, blown with a pair of bellows, it was readily ſeal'd up; and preſently the ſpherical part of the glaſs being held by the ſtem, was kept turning in the flame, till it became red-hot, and ready to melt; then, being a little removed from the heat, as the included air began to loſe of its agitation, and ſpring, the external air manifeſtly, and conſiderably preſs'd in one of the ſides of the bubble. But the glaſs being again,

" would not have much leſs reſiſtance " than quick-ſilver; if abſolutely denſe, " or full of matter, without any vacuum, " let the matter be never ſo ſubtile and " fluid, they would have a greater reſiſt- " ance than quick-ſilver. A ſolid globe, " in ſuch a medium, would loſe above " half its motion, in moving three times " the length of its diameter; and, a globe, " not ſolid, ſuch as are the planets, would " be retarded ſooner. And, therefore, " to make way for the regular, and laſt- " ing motions of the planets, and co- " mets, 'tis neceſſary to empty the hea- " vens of all matter, except, perhaps, " ſome very thin vapours, ſteams, or effluvia, " ariſing from the atmoſphere of the earth, " planets, and comets. A denſe fluid can " be of no uſe for explaining the pheno- " mena of nature; the motions of the pla- " nets, and comets, being better explain'd " without it. It ſerves only to diſturb, " and retard the motions of thoſe great " bodies, and make the frame of nature " languiſh: and, in the pores of bodies, it " ſerves only to ſtop the vibrating motion " of their parts, wherein their heat and " activity conſiſts. And, as it is of no uſe, " and hinders the operations of nature, " and makes her languiſh; ſo there is no " evidence for its exiſtence, and, there- " fore, it ought to be rejected."

" If all ſpace, was equally full, the " ſpecific gravity of the fluid, where- " with the region of the air is fill'd, " would, by reaſon of the vaſt denſity " of its matter, be as heavy in ſpecie, " as quick-ſilver, gold, or any other " the moſt denſe body; whence neither " gold, nor any other matter, would " deſcend in that air: for, no bodies " deſcend in fluids, that are not ſpeci- " fically lighter than they."

'Tis farther demonſtrable, that all bodies have more pores, than ſolid parts; and, that ſome have infinitely more pores than others; whence ariſes another proof of a vacuum. See *Newton. Optic.* p. 310, 311, *&c.* *Princip.* p. 316, 317. 328. 342, 343. 368.

before

before the cold crack'd it, held, as before, in the flame; the rarified air diftended and plump'd up the bubble; which, being the fecond time remov'd, was a fecond time comprefs'd; and being, the third time, brought back to the flame, fwell'd as before, and removed, it was again comprefs'd; till, at length, having fatisfied ourfelves, that the included air was capable of being condens'd, or dilated, without the ingrefs or egrefs of air, properly fo called, we held the bubble fo long in the flame, ftrengthen'd by nimble blafts, that not only it had its fides plump'd up, but a hole violently broken in it, by the over-rarified air, tho', till then, it was no way crack'd.

Hence, it eafily appears, how impervious our thick pneumatic receivers are to common air; fince a thin glafs bubble, when its pores were open'd, or relax'd by flame, would not give paffage to the fpringy particles of the air, tho' violently agitated: for, if thefe particles could have got out of the pores, they never would have broke the bubble; nor, probably, would the compreffion, that afterwards enfued, of the bubble, by the ambient air, be check'd near fo foon, if thofe fpringy corpufcles had not remain'd within to make refiftance.

The nature of rarifaction, and condenfation.

Yet, what I principally defign'd, in this experiment, was, to fhew, and prove at once, by an inftance not liable to the ordinary exceptions, the true nature of rarifaction, and condenfation, at leaft, of the air: for, 'tis here plain, that when the bubble, after the glafs had been firft thruft in, towards the center, was expanded again by heat, the included air poffefs'd more room than before; yet it could perfectly fill no more room than before; each aerial particle taking up, both before and after the heating of the bubble, a portion of fpace adequate to its own bulk: fo that in the cavity of the expanded bubble, we muft either admit vacuities interfpers'd between the corpufcles of the air; or allow, that fome fine particles of the flame, or other fubtile matter, came in, to fill up thofe interftices; which matter muft have enter'd the cavity of the glafs at its pores. And, afterwards, when the red-hot bubble was remov'd from the flame, it is evident, that fince the groffer particles of the air could not get thro' the glafs, which they were unable to do, even when vehemently agitated by an ambient flame, the compreffion of the bubble, and the condenfation of the air, neceffarily confequent upon it, cou'd not, fuppofing the plenitude of the world, be perform'd without fqueezing out fome of the fubtile matter, contain'd in the cavity of the bubble, whence it could not iffue but at the pores of the glafs.

The preffure of the air manifefted to the eye.

Mr. *Hobbs* is pleas'd to compare our pneumatic engine to a pot-gun, and attributes the phenomena, exhibited in the exhaufted receiver, to the expanfive endeavour of the air outwards: I fhall, therefore, fhew, that there is, in our exhaufted receiver, no fuch ftrong endeavour outwards, as he fuppofes; but that the weight of the atmofpherical air, when 'tis not refifted by the counter-preffure of any internal air, is able to perform what a weight of many pounds would not fuffice for. Glafs not being a yielding body, cannot, by the alteration of its figure, from an external uniform preffure, fhew when fuch an one is exercis'd upon it; and, therefore,

fore,

PNEUMATICS. fore, inſtead of a receiver of glaſs, we provided one of pewter, and applied it to the engine, after the uſual manner. And tho' the inverted veſſel, by reaſon of its ſtiffneſs, thickneſs, and the convexity of its ſuperficies, were ſtrong enough to have ſupported a great weight, without changing its figure; yet, as ſoon as by an exſuction or two, the remaining part of the included air was brought to ſuch a degree of expanſion, that its weaken'd ſpring was able to afford little aſſiſtance to the tenacity and firmneſs of the metal, the weight of the pillar of the incumbent atmoſphere, preſently depreſs'd the upper-part of the veſſel; at once leſſening its capacity, and changing its figure; ſo that, inſtead of the convex ſurface, it gain'd a concave one. The experiment ſucceeded, alſo, with a common pewter porringer. And, ſometimes I found, alſo, that the veſſel would be thruſt in, not at the top, but on the ſide; if that were the only part made too thin to reſiſt the external preſſure.

Whether air penetrates quick-ſilver, in the Torricellian *experiment.*

Mr. *Hobbs*, afterwards, proceeds to the *Torricellian* experiment; which he will needs have perform'd by means of air in the tube; and attempts to prove, that air may paſs thro' quick-ſilver, becauſe a blown bladder, forcibly detain'd under it, will, of itſelf, emerge, when the detaining force is remov'd.

This, I confeſs, is ſurprizing. It concern'd Mr. *Hobbs* to prove, that as much air as was diſplac'd by the deſcending mercury, did, at the orifice of the tube, immers'd in ſtagnant mercury, inviſibly aſcend to the upper-part of the pipe; and, he tells us, that a bladder full of air, being depreſs'd in quick-ſilver, will, when the hand that depreſs'd it is remov'd, be ſqueez'd up by the very weight of the mercury; whence it follows, that air may penetrate quick-ſilver. But, who ever deny'd that air, ſurrounded with quick-ſilver, may, thereby, be ſqueez'd upwards? And, ſince even very ſmall bubbles of air, may be ſeen to move in their paſſage thro' mercury, how will this example help Mr. *Hobbs?* For, 'tis by mere accident that the air, included in the bladder, comes to be buoy'd up, becauſe the bladder itſelf is ſo; and, if it were fill'd with water, inſtead of air; or with ſtone, inſtead of water; it would, nevertheleſs, emerge, as himſelf confeſſes, if it were iron, or any matter, except gold; becauſe all other bodies are lighter in ſpecie, than quick-ſilver. But, ſince the emerſion of the bladder is manifeſt enough to the ſight, how does this prove, that the air gets into the *Torricellian* tube inviſibly; ſince our eyes diſcover no ſuch motion of the air, which muſt not only paſs unſeen thro' the ſuſtain'd quick-ſilver, but, likewiſe, imperceptibly dive, in ſpight of its comparative lightneſs, beneath the ſurface of the ſtagnant mercury, to get in at the orifice of the erected tube.

But, to clear up this matter, having made the *Torricellian* experiment in a ſtrait tube, after the ordinary manner, we took a piece of fine bladder, and, raiſing the pipe a little in the ſtagnant mercury, but not ſo high as the ſurface thereof, we dextrouſly convey'd it into the quick-ſilver, ſo as to be apply'd by the finger to the immers'd orifice of the pipe, without letting

ting the air get into the cavity of it; then the bladder was, cautiouſly, ty'd very tight to the lower end of the pipe, whoſe orifice it cover'd before, and the pipe, being now ſlowly lifted out of the ſtagnant mercury, the ſuſtain'd quick-ſilver appear'd to preſs, but very lightly upon the bladder; being ſo near an exact equilibrium with the atmoſpherical air, that if the tube were but a very little inclin'd, whereby the perpendicular gravitation of the quick-ſilver came to be ſomewhat leſſen'd, the bladder would immediately be driven into the orifice of the tube, and to the eye, plac'd without, appear to have acquir'd a concave ſuperficies, inſtead of the convex it had before. And, when the tube was again erected, the bladder would no longer appear ſuck'd in, but be again ſomewhat protuberant. And, if, when the mercury in the tube was made to deſcend, a little below its ſtation, into the ſtagnant quick-ſilver, the piece of bladder were, at the juncture, nimbly and dexterouſly apply'd, as before, to the immers'd orifice, and faſten'd to the ſides of the pipe; upon lifting the inſtrument out of the ſtagnant mercury, the cylinder of quick-ſilver being now ſomewhat ſhort of its due height, was no longer able, fully to counterpoiſe the weight of the atmoſpherical air; which, conſequently, tho' the glaſs were held erect, would preſs up the bladder into the orifice of the tube, and cauſe a cavity, ſenſible both to the eye and touch. PNEUMATICS.

This experiment fully ſhews, that the preſſure of the external air is able to ſuſtain a cylinder of twenty-nine or thirty inches of mercury, and, upon a ſmall diminution of the gravity of that ponderous fluid, to preſs it up higher into the tube. But a farther uſe may be made of it againſt Mr. *Hobbs*. For, when the tube is again erected, the mercury will ſubſide as low as at firſt, and leave as great a ſpace, as formerly was left deſerted at the top; into which, how the air ſhould get to fill it, will not appear eaſy to them, who know, that a bladder will rather be burſt by air, than afford it paſſage. And if it ſhould be pretended, either, that ſome air from without had got thro' the bladder, or, that the air preſum'd to have been, juſt before, included between the bladder and the mercury, made its way from the lower part of the inſtrument to the upper; we reply, 'tis no way likely, that it ſhould paſs all along the cylinder, unperceiv'd by us; ſince when there are really any aerial bubbles, tho' ſmaller than pins heads, they are eaſily diſcernible. And, in our caſe, there is no ſuch reſiſtance of the air to the aſcent of the ſtagnant mercury, as Mr. *Hobbs* pretends in the *Torricellian* experiment, made after the uſual way.

But further, we took a cylindrical pipe of glaſs cloſed at the upper end, and ſo long, that being dexterouſly bent at ſome inches diſtance from the bottom, the ſhorter leg was made parallel to the longer. In this glaſs, we found an expedient to make the *Torricellian* experiment; the quick-ſilver in the ſhorter leg ſerving inſtead of the ſtagnant portion in the uſual baroſcope; and that in the longer leg, reaching above the mercury in the ſhorter about eight or nine and twenty inches. Then, by another artifice, the ſhorter leg, into which the mercury did not riſe within an inch of the top, was ſo order'd, that it could in a trice be hermetically ſeal'd up; an inch of common air be-

PNEUMATICS. ing left in it, without diſordering the mercury: and having, in this manner, ſhut up ſuch a quantity of uncompreſs'd air, we warily held a pair of heated tongs near the outſide of the glaſs, whereby, the air being agitated, was enabled to expand itſelf to double its former dimenſions; and, conſequently had its ſpring ſo ſtrengthen'd, that it was able to raiſe all the quick-ſilver in the longer leg, and ſuſtain a mercurial cylinder above nine and twenty inches high; when, were it not for the heat, it would have loſt half the force of its elaſticity.

Now Mr. *Hobbs* will find it very difficult to ſhew, what keeps the mercury ſuſpended in the longer leg of ſuch a barometer, when the ſhorter leg is unſtop'd, at which it may run out; ſince this inſtrument is portable. And when the ſhorter leg is ſeal'd, it will be very hard for Mr. *Hobbs* to ſhew there the odd motions of the air, to which he aſcribes the *Torricellian* experiment. For if you warily incline the inſtrument, the quick-ſilver will riſe to the top of the longer leg, and immediately ſubſide, when the inſtrument is again erected; and yet no air appears to paſs thro' the quick-ſilver interpos'd between the ends of the longer, and the ſhorter leg. But that, which I would chiefly take notice of in this experiment, is, that upon the external application of a hot body to the ſhorter leg, when ſeal'd up, the included air was expanded from one inch to two; and ſo rais'd the whole cylinder of mercury in the longer leg; and whilſt the heat continu'd undiminiſh'd, kept it from ſubſiding again. For if the air were able to get, unſeen, thro' the body of the quick-ſilver, why had it not been much more able, when rarify'd by heat, to paſs thro' the quickſilver, than for want of doing ſo, to raiſe and ſuſtain ſo great a weight of mercury?

How water comes to aſcend in glaſſes after ſome of their air is drawn out.

The laſt thing attempted by Mr. *Hobbs*, in his problems, is to account for the riſe of water into a vial plung'd therein, with the mouth downwards, after ſome air has been ſuck'd out of it: where he demands, upon ſuppoſing a *Vacuum*, and, conſequently, interſpers'd vacuities in the air of the vial, "how it happens, that the water would not aſcend before the ſuction was made?" To this the vacuiſts will eaſily anſwer, by acknowledging, that there were, indeed, interſpers'd vacuities in the air contain'd in the vial, before the ſuction; but adding that there was no reaſon why the water ſhould aſcend to fill them, becauſe, being a heavy body, it cannot riſe of itſelf, but muſt be rais'd by ſome prevalent weight or preſſure, which was then wanting. Beſides, there being interſpers'd vacuities, as well in the reſt of the air that was very near the water, as in that contain'd in the vial, there is no reaſon why the water ſhould aſcend to fill the vacuities of one portion of air, rather than that of another. But, when once, by ſuction, many of the aerial corpuſcles were made to paſs out of the vial, the ſpring of the remaining air being weaken'd, whilſt the preſſure of the ambient air, which depends upon its conſtant gravity is undiminiſh'd, the ſpring of the internal becomes unable to reſiſt the weight of the external air; which is therefore able to impel the interpos'd water, with ſome violence, into the cavity of the glaſs, till the air, remaining in that cavity, being reduced, almoſt, to its uſual denſity, is able, by its ſpring, and the weight of the

the water got up into the vial, to hinder any more from aſcending. For, as to what Mr. *Hobbs* affirms, that "the perſon, who ſucks the vial, draws "nothing into his ſtomach, lungs or mouth;" how he will reconcile this with what he elſewhere delivers about ſuction, I leave him to conſider. I cannot, however, but wonder at his confidence, who can poſitively aſſert a thing ſo repugnant to the common ſentiments of men of all opinions, without offering any proof for it. But, I ſuppoſe, they who are, by trial, acquainted with ſuction, and have felt the air come in at their mouth, will preer their own experience to his authority. And as to what he adds, that the perſon who ſucks, agitates the air, and turns it within the vial into a kind of circulating wind, that endeavours every where to get out; I wiſh he had ſhewn us, by what means a man in ſucking makes this odd commotion in the air, eſpecially, in ſuch vials as I employ about this experiment, the orifice whereof is ſometimes leſs than a pin's head.

But that real air may be extracted, by ſuction, out of a glaſs, appears by an experiment made with a receiver exhauſted by our pump; and, conſequently by ſuction. For, when we had counterpois'd it in very nice ſcales, and, afterwards, by turning a ſtop-cock, let in the outward air, there ruſh'd in as much to fill the ſpace deſerted by the evacuated air, as weigh'd ſome ſcruples, tho' the receiver were not of the largeſt ſize.

Mr. *Hobbs* pretends, that as ſoon as the neck of the vial is unſtopp'd under water, the air that whirl'd about before, makes a ſally out, and forces in as much water. But if the orifice be any thing large, you, will, inſtead of feeling an endeavour to thruſt away your finger that ſtop'd it, find the pulp of it, ſo thruſt inward, as to appear to be ſuck'd in. And this may be the reaſon why the lip of him who ſucks, is often ſtrongly faſten'd to the orifice of the vial's neck; which Mr. *Hobbs* aſcribes to a moſt exquiſite contact, but without clearly telling us how that extraordinary contact is effected. And, when your finger is remov'd, inſtead of perceiving any air go out of the vial thro' the water; which, if any ſuch thing happen'd, would eaſily be diſcover'd by the bubbles; you ſhall ſee the water briſkly ſpring up in a ſlender ſtream to the top of the vial; which it could not do, if the cavity were already full of air. And, to prove, when the air really paſſes in and out of the vial immers'd under water, that 'tis very eaſy to perceive its motions; if you dip the neck of the vial in water, and then apply to the globular part of it, either your warm hands, or any other competent heat; the internal air being rarify'd, a portion of it, anſwerable to the degree of heat apply'd, will manifeſtly paſs thro' the water in ſucceſſive bubbles, whilſt yet no water gets into the vial to fill the place deſerted by that air. And, if, when you have fill'd the neck, and part of the belly of the vial with water, you immerſe the orifice into ſome that is ſtagnant, and apply your warm hands to the ſpherical part as before; the water in the vial will be driven out, before any bubbles paſs out of the vial into the ſurrounding water; which ſhews, that the air is not ſo forward to dive under water, as Mr. *Hobbs* ſuppoſes.

But to clear up this matter ſtill farther, we took a glaſs bubble with a ſlender cylindrical ſtem, and by applying a convenient heat to the outſide of the ball, we expell'd ſo much of the air, that, when the end of the pipe was dipt in water, and the inward air had time to recover its former coolneſs, the water aſcended to the top of the pipe. This done, we gently, and warily rarify'd the air in the cavity of the bubble, till, by its expanſion, it had driven out, almoſt all the water, which had got up into the ſtem; ſo that it might attain, as near as poſſible, to that degree of heat and meaſure of expanſion, it had, when the water began to riſe in it. And, we left two or three drops of water, unexpell'd, at the bottom of the pipe; to be ſure, that none of the included air was, by this ſecond rarifaction, driven out at the orifice of it: as the depreſſion of the water ſo low aſſured us, on the other ſide, that the included air wanted nothing conſiderable of the expanſion it had when the water began to aſcend into the pipe. Whilſt the air was in this rarify'd ſtate, we preſently removed the little inſtrument, out of the ſtagnant water, into ſtagnant quick-ſilver, which, in a ſhort time, began to riſe in the pipe. Now, if the aſcent of the liquor were the effect of nature's abhorrence of a *Vacuum*, or of ſome internal principle of motion, or of the compreſſion or propagated truſion of the outward air, by that which had been expell'd; why ſhould not the mercury have aſcended to the top of the pipe, as the water did before? But in fact, it did not aſcend near half ſo far; and if the pipe had been long enough, as well as 'twas ſlender enough, I queſtion, whether the mercury would have aſcended, in proportion to the length of the ſtem, half ſo high as it did.

Now, of this experiment, which we try'd more than once, I ſee not how any good account can be given without our hypotheſis, according to which 'tis clear; for the aſcent of liquors, being an effect of the prevalency of the external air's preſſure againſt the reſiſtance it meets with in the cavity of the inſtrument, and the quick-ſilver being bulk for bulk many times heavier than water; the ſame ſurplus of preſſure that was able to impel up water to the top of the pipe, ought not to be able to impel up the quick-ſilver to any thing near that height. And if it be here objected, as it very plauſibly may, that the rais'd cylinder of mercury was much longer than it ought to have been, with regard to a cylinder of water, the proportion in gravity between thoſe two fluids being conſider'd; I anſwer, that when the cylinder of water reach'd to the pipe, the air poſſeſſed no more than the cavity of the ſpherical part of the inſtrument; being very little aſſiſted to dilate itſelf by ſo light a cylinder as that of water: but when the quick-ſilver came to be impell'd into the inſtrument; by the weight of the external air; that ponderous body did not ſtop its aſcent, as ſoon as it came to be equiponderant to the expell'd cylinder of water; becauſe to attain that height it reach'd but a little way into the pipe, and left all the reſt of the cavity to be fill'd with part of that air, which formerly was all ſhut up in the bubble; by which means, the air included in the whole inſtrument muſt needs be in a ſtate of expanſion, and, thereby

thereby have its ſpring weaken'd, and, conſequently, diſabled to reſiſt the preſſure of the external air, as much as the ſame included air did before, when it was leſs rarify'd: on which acccount, the undiminiſh'd weight or preſſure of the external air was able to raiſe the quick-ſilver gradually higher, till it had obtain'd that height, at which the preſſure, compounded of the weight of the mercurial cylinder, and the ſpring of the internal air, now leſs rarify'd than before, was equivalent to the preſſure of the atmoſphere. Pneumatics.

And, to confirm this experiment, by a kind of inverſion of it, we, by heat, drove a little air out of the bubble, and dipt the open end of the pipe into quick-ſilver, which, by this means, aſcended, till it had fill'd about a fourth part of the pipe when held erect. Then, carefully, removing it, without letting fall any quick-ſilver, or letting in any air, we held the orifice of the pipe a little under the ſurface of a glaſs full of water; and applying a moderate heat to the outſide of the ball, we warily expell'd the quick-ſilver, yet leaving a little, to be ſure, that no air was driven out with it; then ſuffering the included air to cool, the external made the water, not only aſcend to the very top of the pipe, and thence ſpread itſelf a little into the cavity of the ball, but carry'd up before it, the quick-ſilver that had remain'd unexpell'd at the bottom of the ſtem. And, if, in making the experiment, we firſt rais'd, as we ſometimes did, a greater quantity of quick-ſilver, and afterwards drove it out; the quantity of water that would be impell'd into the cavity of the pipe, and ball, was accordingly increas'd.

In this experiment, 'tis manifeſt, that ſomething is driven out of the cavity of the glaſs, before the water or quick-ſilver begins to aſcend in it. And here, alſo, we ſee not, that the air can paſs thro' the pores of quick-ſilver or water, but that it drives them on before it, without eaſily mixing with them. And there appears no circular wind, as Mr. *Hobbs* fancies in the ſuck'd vial, nor any tendency outwards of the included air, upon the account of ſuch a wind: but that inſtead of theſe, the aſcent of the liquors into the cavity of the pipe, depends upon the external air preſſing them up, appears from hence, that the ſame weight of the atmoſphere, impell'd into the pipe ſo much more of the lighter fluid, water, than of the heavier, mercury.

And that there is no need of the ſallying of air out of a vial to make the atmoſpherical air preſs againſt a body that cloſes the orifice of it, when the preſſure of the internal air is much weaken'd, I have ſhewn, by ſucking out, by the help of an inſtrument, a conſiderable portion of the air contain'd in a glaſs: for, having then, inſtead of unſtopping the orifice under water, ſuddenly apply'd a flat body to it, the external air preſs'd that body ſo forcibly againſt it, as to keep it faſten'd and ſuſpended, tho' 'twere clogg'd with a weight of many ounces. Or if there be ſuch a circular wind, as Mr. *Hobbs* pretends, produced by ſuction, in the cavity of the vial, it muſt needs be ſtrangely laſting. For I have ſeen more than once, that when, by an inſtrument, much air has been ſuck'd out of a vial, which was af-

terwards

PNEUMATICS. terwards carefully closed, and thus kept for some months; yet, when 'twas open'd under water, a convenient quantity of the liquor would be briskly impel'd up into the neck and belly of the vial.

And, having with the sun-beams produced smoke in one of those well-stop'd vials; this circular wind did not at all appear to blow it about, but suffer'd it to rise, as it would have done, if the included air had been very calm.

I shall add but one experiment more, which will not be liable to some of the objections, invalid as they are, that Mr. *Hobbs* has alledg'd in his account of the suck'd vial; and which will shew, that the weight of the atmosphere is a very considerable thing; and also, that whilst Mr. *Hobbs* does not admit a more subtile matter than common air to pass thro' the pores of close and solid bodies; the air he has recourse to, will sometimes come too late to prevent a *Vacuum*. Having caus'd an æolipile very light, considering its bulk, to be made by a famous artist, I had occasion to put it so often into the fire, for several trials, that the copper scal'd off by degrees, and left the vessel much thinner than when it first came out of the artificer's hands; and now, after a long interval, I had occasion to employ it, as formerly, to weigh the air it would contain. To make this experiment the more exactly, the air was, by a strong fire, carefully driven away; when clapping a piece of sealing-wax to the pin-hole, at which it had been forc'd out, we prevented a communication betwixt the cavity of the instrument, and the external air; and supposing the æolipile to be very well exhausted, we laid it by, that when it should be grown cold, we might, by opening the orifice again, let in the outward air, and observe what increase of weight it would make. But the instrument had been so far exhausted, that what air remain'd, being unable by its spring to assist the æolipile to support the weight of the surrounding air; this external fluid did, by its weight, so strongly compress it, and thrust it so considerably inwards, and, in more than one place, so change its figure, that when I shew'd it to the Gentlemen assembled at *Gresham* College, they were pleas'd to command it of me, to be kept in their repository.

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INQUIRY
INTO THE
Cauſe of Attraction
BY
SUCTION.

SUCTION being, generally, look'd upon as a kind of attraction, it will be requiſite to premiſe ſomething about the latter, in order to clear the nature of the former. The cauſe, nature, and notion of attraction, are, generally, either left untouch'd, or happen to be but very obſcurely deliver'd. *The Nature of Attraction.*

How general and antient ſoever, the common opinion may be, that attraction is a kind of motion quite different from pulſion, I confeſs, it ſeems to me, a ſpecies of pulſion; at leaſt, among inanimate bodies. I have not, yet, obſerv'd any thing which ſhews attraction cannot be reduc'd to pulſion: for they ſeem but extrinſical denominations of the ſame local motion, in which, if a moved body precede the movent, or tend to get at a greater diſtance from it, we call it pulſion; and if, upon account of the motion, the ſame body follow the movent, or approach to it, attraction. But this difference may conſiſt only in an accidental reſpect; which does not phyſically alter the nature of the motion, but is founded upon the reſpect which the line, wherein the motion is made, happens to have to the ſituation of the movent. And, that which ſeems to have been the chief cauſe of miſtaking attraction, for a motion oppoſite to pulſion, is, that men look upon both the moving, and moved bodies, in a popular and ſuperficial manner; and conſider in the movent, rather the ſituation of the conſpicuous, and more bulky part of the agent, than the ſituation of that part of it, which immediately impreſſes the motion upon the mobile.

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PNEUMATICS. Whoever attentively regards this, may eaſily obſerve, that ſome part of the body, or of the inſtrument, which, by reaſon of their conjunction, in this operation, is to be look'd on as but making one with it, is really plac'd behind ſome part of the body to be drawn; and therefore cannot move onwards itſelf, without thruſting the body forward. Thus, when a man draws a chain after him, tho' his body precede the chain, yet his finger, or ſome other part of the hand, wherewith he draws it, has ſome part, or other, which reaches behind the fore-part of the firſt link: and the hinder-part of this link comes behind the firſt part of the ſecond link; and ſo each link has one of its parts placed behind ſome part of the next link after it, till you come to the laſt link of all. And ſo as the finger, that is in the firſt link, cannot move forwards, but it muſt thruſt on that link, by this ſeries of truſions, the whole chain is thruſt forwards: and, if any other body be drawn by that chain, you may perceive ſome part of the laſt link comes behind ſome part of that body, or of ſome intervening body, which, by its coheſion with it, ought, in our preſent caſe, to be conſider'd as part of it. And thus attraction ſeems to be but a ſpecies of pulſion; and uſually belongs to that kind of it, which, for diſtinction ſake, is called truſion; whereby we underſtand that kind of pulſion, wherein the movent goes along with the moved body, without quitting it, whilſt the progreſs laſts; as happens in propelling a wheel-barrow, without letting go the hold of it.

But, it may be ſaid, there are attractions where it cannot be pretended that any part of the attrahent comes behind the attracted body; as in magnetical, and electrical attractions; and in that which is made of water, when drawn up into ſprings, and pumps. Now, the *Carteſians*, and other modern philoſophers, have recourſe, either to ſcrew'd particles, and other magnetical emiſſions, to ſolve phenomena of this kind; and, according to ſuch hypotheſes, we may ſay, that many of theſe magnetical, and electrical effluvia, come behind ſome parts of the attracted bodies; or, at leaſt, of the little ſolid particles, that are, as it were, the walls of their pores; or procure ſome diſcuſſion of the air, that may make it thruſt the moveable towards the load-ſtone, amber, *&c.* But, if there were none of theſe, nor any other ſubtile agents, to cauſe this motion, by a real, though unperceiv'd pulſion, I ſhould make a diſtinction betwixt other attractions, and theſe which I ſhould then call attractions by inviſibles. However, in raiſing water into the barrel of a ſyringe, there is no true attraction made of the liquor. For, by the aſcending rammer, as a part of which, I here conſider the obtuſe end, plug, or ſucker, there is no attraction made of the contiguous and ſubjacent water, but only room made for it to riſe into, without being expos'd to the preſſure of the ſuperior air. For, if we ſuppoſe the whole rammer to be annihilated, and, conſequently, incapable of exerciſing any attraction; yet, provided the ſuperior air were kept off from the water, by any other way, as well as 'twas by the rammer, the liquor would as well aſcend into the cavity of the barrel; ſince the ſurface of the

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terraqueous globe, being continually preſs'd on by the incumbent atmoſphere, the water muſt, by that preſſure, be impell'd into any cavity, here below, where there is no air to reſiſt it; as, by ſuppoſition, there is not, in the barrel of our ſyringe, when the rammer was annihilated. Thus, if the external air, and conſequently, its preſſure, be, by the air-pump, withdrawn from about the ſyringe, you may raiſe the ſucker as much as you pleaſe; but none of the ſub-jacent water will follow. In ſhort, let us ſuppoſe, that a man, ſtanding in an inner-room, does, by his utmoſt reſiſtance, keep ſhut a door, that is neither lock'd, nor latch'd, againſt another, who, with equal force, endeavours to thruſt it open; in this caſe, if one ſhould forcibly pull away the firſt man, it could not be ſaid, that he, by his receſs from the door he endeavour'd to preſs outwards, did truly and properly draw in his antagoniſt, though, upon that receſs, the coming in of his antagoniſt, would preſently enſue; ſo it cannot, properly, be ſaid, that, by the aſcent of the rammer, which diſplaces the ſuperior air, either the rammer itſelf, or the expell'd air, properly attracts the ſubjacent water, tho' the ingreſs of that liquor, into the barrel, thereupon neceſſarily enſues. And that, as the compariſon ſuppoſes, there is a preſſure of the ſuperior air againſt the upper-part of the ſucker, may be eaſily perceiv'd, if, having well ſtopp'd the lower orifice of the ſyringe with your finger, you forcibly draw up the ſucker to the top of the barrel; for, if then you let go the rammer, you will find it impell'd downwards, by the incumbent air, with a conſiderable force.

Having thus premis'd ſomething, in general, about the nature of attraction, as far as 'tis neceſſary for my preſent deſign, we may proceed to the conſideration of that kind of it, employ'd to raiſe liquors, which is, by a diſtinct name, called ſuction.

The cauſe of ſuction inquired into.

About the cauſe of this, there is great contention, between the modern philoſophers, and the *Peripatetics*. The former aſcribe the aſcent of liquors, upon ſuction, to nature's abhorrence of vacuum: for, ſay they, when a man dips one end of a ſtraw, or reed, into ſtagnant water, and ſucks at the other, the air contain'd in the cavity of the reed, paſſes into that of his cheſt; and, conſequently, the reed would be left empty, if no other body ſucceeded in the place it deſerts: but there are only two bodies that can ſucceed, the air, and the water; now the air cannot do it, becauſe of the interpoſition of the water, that denies it acceſs to the immers'd orifice of the reed; and, therefore, it muſt be the water itſelf, which, accordingly, aſcends, to prevent a vacuum, abhorr'd by nature.

But many of the modern philoſophers look upon this *Fuga vacui* as an imaginary cauſe of ſuction. The atomiſts, who willingly admit vacuities, properly ſo called, both within our world, and out of it, cannot think that nature dreads a vacuum, and declines her uſual courſe to prevent it. And the *Carteſians*, tho' they, as well as the *Peripatetics*, deny a vacuum, yet, ſince they affirm, not only that there is none in nature, but that there can be none, they will not grant nature to be ſo indiſcreet, as to ſtrain herſelf to prevent the making of a thing that is impoſſible to be made.

The *Peripatetic* opinion, about the cause of suction, though commonly defended by the schools, as well modern as antient, supposes in nature, such an abhorrence of a vacuum, as neither has been well prov'd, nor agrees with the late phenomenon of suction: since, according to their hypothesis, water, and other liquors, should ascend, upon suction, to any height, to prevent a vacuum, which yet is not agreeable to experience. For, I have carefully tried, that by working a pump, far more stanch than those that are usually made, and, indeed, as well clos'd as we could, possibly, bring it to be, we could not, by all our endeavours, raise water, by suction, to above 33 ½ feet. The *Torricellian* experiment shews, that the weight of the air is able to sustain, and some of our experiments shew, 'tis able to raise a mercurial cylinder, equal, in weight, to as high a cylinder of water, as we were able to raise by pumping. For, mercury being near 14 times as heavy as water, of the same bulk, if the weight of the air be equivalent to that of a mercurial cylinder of 29, or 30 inches, it must be able to counterpoise a cylinder of water, near 14 times as long; that is, from 34, to 36 feet. And, very disagreeable to the common hypothesis, but consonant to ours, is the experiment that I have, more than once, made, by taking a glass pipe, about three feet long, dipping one end of it in water, and sucking at the other, whereby the water will be, suddenly, made to flow briskly into one's mouth; when if, instead of water, you dip the lower-end into quick-silver, though you suck as strongly as ever you can, provided, in this case, as in the former, you hold the pipe upright, you will never be able to raise the quick-silver near so high as your mouth: so that, if the water ascended, upon suction, to the top of the same pipe, because there would, otherwise, have been a vacuum left in the cavity of it; why should not we conclude, that when we have suck'd up the quick-silver, as strongly as we can, so much of the upper-part of the tube, as is deserted by the air, and yet not fill'd by the mercury, admits, in part at least, a vacuum, as to air; of which, consequently, nature cannot, reasonably, be supposed, to have so great and unlimited an abhorrence, as the *Peripatetics* presume. Yet, I will not determine whether there be any more than many little vacuities, or spaces destitute of air, in the cavity of the pipe, unfill'd by the mercury, so that the whole cavity is not one entire empty space; it being sufficient, for my purpose, that the experiment affords a good argument against the *Peripatetics*; and warrants us to seek for some other cause, than a *Fuga vacui*, why a much stronger suction, than that which made the water ascend, with ease, into the mouth, will not also raise quick-silver to near the same height.

Those modern philosophers, who admit not a *Fuga vacui* to be the cause of the ascent of liquors in suction, generally agree in referring it to the action of the thorax: for, when a man endeavours to suck up a liquor, he, by means of the muscles, inlarges the cavity of his chest; which he cannot do, but, at the same time, he must thrust away those parts of the ambient air, that were contiguous to his chest; when, the displaced air, according to some, compresses the contiguous air, and that, the next to it,

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and ſo onwards, till the preſſure, ſucceſſively, paſſing from one part of the air to the other, arrive at the ſurface of the liquor; ſo that all other places being, as to ſenſe, full, the impell'd air cannot find place, but by thruſting the water into the room made for it in the pipe, by the receſs of the air, that paſs'd into the lungs of the perſon. And, they differ not much from this explanation, who, without taking in the compreſſion of the ambient air, made by the thorax, refer the phenomenon to the propagated motion, or impulſe, impreſs'd on the air, diſplac'd by the thorax, in its dilatation, and yet unable to move in a world perfectly fill'd, as they ſuppoſe ours to be, unleſs the liquor be impell'd as much into the cavity of the pipe, as faſt as 'tis deſerted by the air, ſaid to be ſuck'd up. But, tho' I readily confeſs this explanation to be ingenious, and ſuch as I wonder not they ſhould acquieſce in, who are only acquainted with the obvious phenomena of ſuction; and tho' I am not ſure, that in the moſt familiar caſes, the cauſes aſſign'd by them, may not contribute to the effect, yet I cannot acquieſce in this theory: for, I think the cauſe of ſuction aſſign'd, is, in many caſes, not neceſſary; in others, not ſufficient. And, firſt, as to the condenſation of the air, by the dilatation of the cheſt; when I conſider the extent of the ambient air, and how ſmall a compreſſion, no greater an expanſion than that of the thorax is likely to make; I can ſcarce think ſo ſlight a condenſation of the free air, ſhould have ſo conſiderable an effect on the ſurface of the liquor, to be rais'd, as the hyotheſis requires: and, that this impulſe of the air, by the dilated thorax, though it accompanies the aſcent of the water, procured by ſuction, is not of abſolute neceſſity thereto, will, I preſume, be eaſily granted: even a propagated pulſion, abſtracted from any condenſation of the air, is not ſo neceſſary, but that the effect may be produc'd without it. Now, ſuppoſe ſo much air as is diſplaced by the thorax, annihilated, yet the aſcent of the liquor would ſtill enſue; for, when a man begins to ſuck, there is an equilibrium between the preſſure which the air contain'd in the pipe, has, by virtue of its ſpring, upon that part of the ſurface of the water, that is ſurrounded by the ſides of the pipe, and the preſſure which the atmoſphere has, by virtue of its weight, upon all the reſt of the ſurface of the ſtagnant water: ſo that when, by the dilatation of the thorax, the air, within the pipe, comes to be rarified, and, conſequently, to loſe of its ſpring, the weight of the external air continuing, in the mean time, the ſame; it muſt neceſſarily happen, that the ſpring of the internal air will be too weak, to balance, any longer, the gravitation of the external; and, conſequently, that part of the ſurface of the ſtagnant water, which is included in the pipe, being leſs preſs'd upon than all the other parts of the ſame ſurface, muſt neceſſarily give way, and, therefore, be impell'd up into the pipe, where the air, having had its ſpring weaken'd, is no longer able to reſiſt it, as before. Thus, conceiving that, within a chamber, three men thruſt all together, with their utmoſt force, againſt a door, to keep it ſhut, at the ſame time that three others, of equal ſtrength, endeavour to thruſt it open; though, whilſt their oppoſite endeavours are equal, the door will continue

 ſhut,

PNEUMATICS. ſhut, yet, if one of the three men, within the room, ſhould ceaſe to act, there would need no new force, in the three men, to make them prevail, and thruſt open the door, againſt the reſiſtance of the two.

The long neck of a glaſs bubble, being ſeal'd up, and, almoſt, all the air, by heat, driven out of the whole cavity thereof, the glaſs was laid aſide, for ſome hours; afterwards, the ſeal'd apex of the neck was broken off, under water: I demand, now, of a *Peripatetic*, whether the liquor ought to be ſuck'd, or drawn into the cavity of the glaſs; and why? If he ſays, as, queſtionleſs, he will, that the water wou'd be attracted, to hinder a vacuum, he, thereby, acknowledges, that, till the glaſs was unſtopp'd, under water, there remain'd ſome empty ſpace in it: for, 'till the ſeal'd end was broken off, the water cou'd not get in; and therefore, if the *Fuga vacui* had any thing to do in the aſcent, the liquor muſt riſe, not to prevent an empty ſpace, but to fill one that was made before. Nor does our experiment much more favour the other philoſophers, I diſſent from: for there is, here, no dilatation made of the ſides of the glaſs, as in ordinary ſuction there is made of the thorax; but only ſo much air driven out of the cavity of the bubble, into whoſe room, ſince neither common air, nor water, is permitted to ſucceed, it appears not how the propagated, and returning impulſe, or the circle of motion, as to common air and water, takes place. Again, I demand, what becomes of the air, that has been, by heat, driven out, and is, by the hermetical ſeal, kept out of the cavity of the bubble? If it be ſaid, that it diffuſes itſelf into the ambient air, and mixes with it; this is to grant, what I contend for, that ſo little air, as is uſually diſplac'd in ſuction, cannot make any conſiderable compreſſion of the free ambient air: for, what can one cubic inch of air, which is, ſometimes, more than one of our glaſſes contains, do, towards condenſing a whole chamber full, when the expell'd corpuſcles, are evenly diſtributed among thoſe of the ambient? And how comes this inconſiderable condenſation to have ſo great an effect in every part of the room, as to be able there to impel into the glaſs, as much water, in extent, as the whole air that was driven out of the cavity of it? But, if it be ſaid, that the expell'd air only condens'd the contiguous air; 'tis no way probable that the expell'd particles of the air, ſhould not, by the different motions of the ambient air, be quickly made to mix with it; but ſhould rather wait 'till the veſſels, whence 'twas driven out, were unſtopp'd again. But though this could, probably, be pretended, it cannot, truly, be aſſerted: for, if you carry the ſeal'd glaſs quite out of the room, and unſtop it at ſome other place, tho' two or three miles diſtant, the aſcent of the water will, as I found by trial, nevertheleſs, enſue; in which caſe, I preſume, it cannot be ſaid, that the air expell'd out of the glaſs, and which condens'd the contiguous air, attended the bubble in all its motions; and was ready at hand, to impel in the water, as ſoon as the ſeal'd apex of the vial was broken off.

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In our own hypothesis of suction, we suppose, first, without disputing either the existence or the nature of elementary air, that the common air, we breathe in, and which I call atmospherical, abounds with corpuscles not destitute of weight, and endow'd with elasticity, whereby the lower parts, compress'd by the weight of the upper, incessantly endeavour to expand themselves; by which expansion, and in proportion to it, the spring of the air is weaken'd, the more they are permitted to stretch themselves.

Next, we suppose, that the terraqueous globe, being surrounded with this gravitating and springy air, has its surface, and the bodies placed on it, press'd by as much of the atmosphere, as either perpendiculary rests on them, or can otherwise come to bear upon them. This pressure is, by the *Torricellian* and other experiments, found to be equivalent to a perpendicular cylinder of about twenty-nine or thirty inches of quick-silver.

Lastly, we suppose, that, air being contain'd in a pipe or other hollow body, having but one orifice open to the free air, if this orifice be hermetically seal'd, or otherwise clos'd, the included air, whilst it continues without any farther expansion, will have an elasticity equivalent to the weight of as much of the outward air as did before press against it. For, if the weight of the atmosphere had been able to compress it further, it would have done it; and then the closing of the orifice, at which the internal and external air communicated, as it fenc'd the included air from the pressure of the incumbent, so it hinder'd the same included air from expanding itself; whence, as it was shut up with the pressure of the atmosphere upon it, that is in a state of as great compression as the weight of the atmosphere could bring it to, so being shut up, and thereby kept from weakening that pressure by expansion, it must retain a springiness proportionable to the pressure 'twas before expos'd to. But if, as was said in the first supposition, the included air should come to be dilated or expanded, its spring, like that of other elastic bodies, would be weaken'd, answerably to that expansion.

To me then it seems, in general, that liquors are, upon suction, rais'd into the cavities of pipes, and other hollow bodies, when there is a less pressure on the surface of the liquor in the cavity, than on the surface of the external liquor, that surrounds it; whether that pressure of those parts of the external liquor, which are from time to time impell'd up into the orifice of the pipe, proceed from the weight of the atmosphere, the propagated compressure, or impulse, of some parts of the air, the spring of the air, or some other cause; as the pressure of bodies quite distinct from air.

Upon a general view of this hypothesis, it seems very agreeable to mechanical principles. For, if there be, on the different parts of the surface of a fluid body unequal pressures, 'tis plain, as well by the nature of the thing, as by what has been demonstrated by *Archimedes*, that the greater force will prevail against the less; and that such a part of the water's surface must give way, as is the least press'd.

To proceed to some experiments made in favour of this hypothesis. We took a glass pipe bended like a siphon; but so, that the shorter leg was

parallel

parallel to the longer, and hermetically ſeal'd at the end: into this ſiphon we convey'd water, ſo that the crooked part being held downwards, the liquor reach'd to the ſame height in both legs; about an inch and an half of uncompreſs'd air being ſhut up in the ſhorter. This little inſtrument, about fifteen inches long, being thus prepared, 'tis plain, that, according to the hypotheſis I diſſent from, there is no reaſon why the water ſhould aſcend upon ſuction. For, tho' we ſhould admit, that the external air were conſiderably compreſs'd, or receiv'd a notable impulſe when the cheſt is enlarg'd, yet, in our caſe, that compreſſion or protruſion will not reach the ſurface of the water in the ſhorter leg; becauſe it is there fenced from the action of the external air, by the ſides of the glaſs, and the hermetical ſeal at the top: but if a perſon ſuck'd ſtrongly at the open orifice in the longer leg, the water in the ſhorter would be depreſs'd, and that in the longer aſcend, at one ſuck, about an inch and a half; of which the reaſon is clear on our hypotheſis. For the ſpring of the included air, together with the weight of the water in the ſhorter leg, and the preſſure of the atmoſpherical air, aſſiſted by the weight of the liquor in the longer leg, balanced one another before the ſuction began; but when, afterwards, upon ſuction, the air in the longer leg came to be dilated, and thereby weaken'd, 'twas render'd unable to reſiſt the undiminiſh'd preſſure of the air included in the ſhorter leg; which, conſequently, expanding itſelf, by virtue of its elaſticity, depreſs'd the contiguous water, and made it proportionably riſe in the oppoſite leg, till its ſpring being by the expanſion gradually more weaken'd, it balanced the gravitation or preſſure of the atmoſphere. And this is the reaſon why, when the perſon who ſuck'd had rais'd the water in the longer leg leſs than three inches higher, by repeated endeavours, and that, without once ſuffering the water to fall back again, he was not able to elevate the water in the longer ſo much as three inches above its firſt ſtation. And if in the ſhorter leg, there was only an inch and a quarter of ſpace left for the air; by ſeveral acts of ſuction, skillfully repeated, he could not raiſe the liquor in the longer leg above two inches; becauſe, by that time, the air included in the ſhorter leg had, by expanding itſelf further, proportionably weaken'd its ſpring, till, at length, it became as much rarify'd as the air in the cavity of the longer leg; and, conſequently, was able to to thruſt away the water with no more force than the air in the longer leg was able to reſiſt. And hence it appear'd, that the rarifaction uſually made of air by ſuction, is not near ſo great as one would expect; probably, becauſe, by the dilatation of the lungs, the air, being ſtill ſhut up, is but moderately rarify'd; and that in the longer leg can by them be brought to no greater degree of rarity than the air within the cheſt. For whereas the included air in our inſtrument was not expanded, at one ſuck, to above double its former dimenſions, and by ſeveral ſucceſſive ſucks was expanded, but from one inch and a half to leſs than four inches and a half, if the ſuction could have been conveniently made with a great ſtanch ſyringe, the rarifaction of the air would, probably, have been far greater; ſince, in our pneumatic engine, air may without

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heat, and by a kind of ſuction, be brought to poſſeſs many hundreds of times the ſpace it took up before. From this rarifaction of the air in both the legs of our inſtrument, proceeds another phenomenon readily explicable by our hypotheſis. For if, when the water was impell'd up as high as the ſuction could raiſe it, the inſtrument were taken from the perſon's mouth, the elevated water would, with violence, return to its wonted ſtation. For the air, in both legs of the inſtrument, having by ſuction loſt much of its ſpring, and, conſequently of its power of preſſing; when once the orifice of the longer leg was left open, the atmoſpherical air came again to gravitate upon the water in that leg; and the air included in the other, having its ſpring weaken'd by the preceding expanſion, was not able to hinder the external air from violently repelling the elevated water, till the included air was thruſt into the ſpace it poſſeſs'd before the ſuction; in which ſpace it had denſity and elaſticity enough to reſiſt the preſſure that the external air exercis'd againſt it, thro' the interpos'd water.

But our hypotheſis about the cauſe of ſuction, needs only the aſſiſtance of our pneumatic engine. For by trials, purpoſely devis'd and carefully made therewith, we found, that a good ſyringe being ſo convey'd into the receiver, that the open orifice of the pipe, or lower part, remain'd under water; if the engine were exhauſted, and the handle of the ſyringe drawn up, the water did not follow it, which yet it would do if the external air were let in again. The reaſon of which is plain; for the air that ſhould have preſs'd upon the ſurface of the ſtagnant water having been pump'd out, there was nothing to impel up the water into the deſerted cavity of the ſyringe, till the receiver was fill'd with air.

I ſhall next offer ſome eaſy experiments to make out theſe three propoſitions.

1. That a liquor may be rais'd by ſuction, when the preſſure of the air, neither as it has weight nor elaſticity, is the cauſe of it.

2. That the weight of the atmoſphere is ſufficient to raiſe up liquors in ſuction.

3. That, in ſome caſes, there will be no ſuction, tho' there is a dilatation of the thorax; and no danger of a *Vacuum*, if the liquor ſhould aſcend.

Suction may raiſe a fluid without the preſſure or elaſticity of the air.

1. And firſt to ſhew how much the riſing of liquors, in ſuction, depends upon the weight or preſſure of the impelling body; and how little neceſſity there is, where ſuch a preſſure happens, that in the place deſerted by the liquor ſuck'd, there ſhould ſucceed air, or ſome other viſible body; I devis'd the following experiments. We took a glaſs pipe, fit for the *Torricellian* experiment, but much longer than was neceſſary for that uſe; this being hermetically ſeal'd at one end, the other was ſo bent as to be reflected upwards, and make, as it were, the ſhorter leg of a ſiphon parallel to the longer; ſo that the tube was now ſhaped like an inverted ſiphon, with legs of a very unequal length. This we fill'd with mercury in an inclining poſture, and then erecting it, the mercury ſubſided in the longer leg, as in the *Torricellian* experiment, and attain'd to between two feet and a quarter, and two feet and a half, above the ſurface of the mercury in the ſhorter leg; which

PNEUMATICS. in this inſtrument anſwers to the ſtagnant mercury in an ordinary barometer. Out of the ſhorter leg of this tube, we warily took as much mercury as was thought convenient; and this we did ſo as to hinder any air from getting into the deſerted cavity of the longer leg; by which means, the mercurial cylinder retain'd the ſame height above the ſtagnant mercury in the ſhorter. The upper and cloſed part of this portable barometer muſt have been free from common air, becauſe, if gently inclin'd, the quick-ſilver would aſcend to the top of the tube; which it could not do, if the place formerly deſerted by it were poſſeſs'd by the air. The inſtrument being thus fitted, I caus'd a perſon to ſuck at the ſhorter leg of it; whereupon there preſently enſued an aſcent of four or five inches of mercury in that leg, and a proportionable ſubſidence in the longer; yet in this caſe the riſe of the mercury cannot proceed from the preſſure of the air. For the weight of the atmoſphere is fenc'd off, by that which cloſes the upper end of the longer tube: and the ſpring of the air has here nothing to do; ſince the ſpace deſerted by the mercury, is not poſſeſs'd by the included air: and the pulſion or condenſation of the air, ſuppos'd by ſeveral modern philoſophers to be made by the dilatation of the cheſt, and to preſs upon the ſurface of the liquors that are to be ſuck'd up, cannot here be pretended; becauſe the ſurface of the liquor in the longer leg is every way fenc'd from the preſſure of the ambient air. It remains, therefore, that the cauſe which rais'd the quick-ſilver in the ſhorter leg, upon the ſuction, was the weight of the collateral quick-ſilver, ſuperior in the longer leg; which being equivalent to the weight of the atmoſphere, there is a plain reaſon why the ſtagnant mercury in the ſhorter leg ſhould be rais'd ſome inches by ſuction, as mercury ſtagnant in an open veſſel will be rais'd by the weight of the atmoſphere, when the ſuction is made in the open air: for, in both caſes, there is a pipe that reaches to the ſtagnant mercury, and a competent weight to impel it into that pipe, when the air in the cavity of it has its ſpring weaken'd by the dilatation that accompanies ſuction.

The weight of the atmoſphere may, alone, raiſe liquors in ſuction.

2. That the weight of the air is ſufficient to raiſe liquors in ſuction, may appear by arguments drawn from the *Torricellian* experiment; and much more clearly from ſome we have made with our air-pump. And with the like view, having provided an inſtrument in imitation of the portable barometer, lately mention'd, but whoſe legs were not ſo unequally long; and having in it made the *Torricellian* experiment, after the manner deſcrib'd, we order'd the matter ſo, that there remain'd in the ſhorter leg the length of ſeveral inches unfill'd with ſtagnant mercury. Then I caus'd a perſon to raiſe the quick-ſilver, by ſuction, ſo to the orifice of the ſhorter leg, that the orifice being ſeaſonably and dexterouſly cloſed, the mercury continu'd to fill that leg as long as we thought fit; and then having put a mark at the ſurface of the mercury in the longer leg, we unſtop'd the orifice of the ſhorter; whereupon the mercury that before fill'd it, was depreſs'd, till in the longer leg it was rais'd five inches, or more, above the mark, and continu'd at that height. This mercury, rais'd by ſuction, was depreſs'd; becauſe its own weight

weight could not here make it fall; ſince a mercurial cylinder of five inches was far from being able to raiſe ſo tall a cylinder of mercury, as made a counterpoize in the longer leg; and therefore, the depreſſion we ſpeak of, is to be referr'd to the gravitation of the atmoſpherical air upon the ſurface of the mercury in the ſhorter leg. And I ſee no cauſe to doubt, that if we could have procur'd an inſtrument, into whoſe ſhorter leg, a mercurial cylinder many inches higher might have been ſuck'd up, it would, by this contrivance, have appear'd, that the preſſure of the atmoſphere could eaſily impel up a far taller cylinder of mercury than it did in our experiment. For, if the gravity of an incumbent pillar of the atmoſphere be able to compreſs a parcel of included air as much as a mercurial cylinder equivalent, in weight, to between thirty and thirty-five feet of water is able to condenſe it, the ſame atmoſpherical cylinder may well be able, by its weight, to raiſe and counterbalance twenty-eight or twenty-nine inches of quick-ſilver, or an equivalent pillar of water in tubes, where the reſiſtance of theſe two fluids, to be rais'd and ſuſtain'd by the air, depends, only, upon their own unaſſiſted gravity. Pneumatics.

To confirm our doctrine of the gravitation of the atmoſphere, upon the ſurface of the liquors expos'd to it, I ſhall ſubjoin an experiment, deviſ'd to ſhew, that the incumbent air, in its uſual ſtate, would compreſs other air in the like natural ſtate, as much as a cylinder of twenty-eight or twenty-nine inches of mercury could condenſe it. But in order hereto, I ſuppoſe it known, that about twenty-nine or thirty inches of quick-ſilver will compreſs air ſhut up, in its uſual ſtate, in the ſhorter leg of our portable barometer into half the ſpace it poſſeſs'd before.

We provided a portable barometer, wherein the mercury in the longer leg was kept ſuſpended by the counterpoize of the air that gravitated on the ſurface of the mercury in the ſhorter, which, we had ſo order'd, that it reach'd not, by about two inches, to the top of the ſhorter leg. Then making a mark at the place where the ſtagnant mercury reſted, 'twas manifeſt, according to our hypotheſis, that the air in the upper part of the ſhorter leg was in its natural ſtate, or of the ſame degree of denſity with the outward air, with which it freely communicated at the open orifice of the ſhorter leg; ſo that this ſtagnant air was equally preſs'd upon by the weight of the collateral ſuperior cylinder of mercury in the longer leg, and the equivalent weight of a directly incumbent pillar of the atmoſphere. Then the upper part of the ſhorter leg that had been before, purpoſely, drawn out to, almoſt, a capillary ſmallneſs, was hermetically ſeal'd; which, tho' the inſtrument remain'd erect, was ſo ſuddenly done, by reaſon of the ſlenderneſs of the pipe, that the included air did not appear to be ſenſibly heated: after this, we open'd the lower end of the longer leg without ſhaking the veſſel; by which means, the atmoſpherical air, gaining acceſs to the mercury included in the longer leg, did, by its gravitation upon it, ſo compreſs the air included in the ſhorter leg, that, according to the eſtimate we made, with the help of a ruler, it was crowded into near half the ſpace it took up before; and conſequently ſuffer'd a compreſſure like that,

 which

PNEUMATICS. which a mercurial cylinder of about twenty-nine inches would have given it.

That the ascent of liquors, by suction, depends upon pressure.

This experiment was made a second time, with like success.

3. And to make it yet further appear, how much the ascent of liquors, by suction, depends upon pressure, rather than upon nature's imaginary abhorrence of a *Vacuum*, or the propagated pulsion of the air; I will subjoin an instance, wherein that presum'd abhorrence cannot be pretended. The experiment was thus made.

A glass siphon, like those lately describ'd, with one leg far longer than the other, was hermetically seal'd at the shorter leg, and then, by degrees, we put in, at the orifice of the longer leg, as much quick-silver, as by its weight sufficed to compress the air in the shorter leg, into about half the space it possess'd before; so that, according to the *Peripatetic* doctrine, the air must be in a state of preternatural condensation, and that to a far greater degree, than 'tis usually brought by cold intense enough to freeze water. Then, measuring the height of the quick-silver in the longer tube above the superficies of that in the shorter, we found it not to exceed thirty inches. Now if liquors rose, in suction, for fear of a *Vacuum*, there is no reason why this quick-silver in the longer part of the siphon should not easily ascend upon suction, at least till the air in the shorter leg had regain'd its former dimensions; since it cannot, in this case, be pretended, that if the mercury should ascend, there would be any danger of a *Vacuum* in the shorter leg of the tube; because the contiguous included air is ready at hand to succeed, as fast as the mercury subsides in the shorter leg of the siphon. Nor can it be alledg'd, that, to fill the place deserted by the quick-silver, the included air must suffer a preternatural rarifaction; since 'tis plain, that, on the contrary, as long as the air continues in the state whereto 'tis reduced by the weight of the quick-silver, it is kept in a violent state of compression, because in the shorter leg it was in its natural state, when the mercury pour'd into the longer leg did, by its weight, thrust it into about half the space it took up before. Yet, having caus'd several persons to suck, several times, as strongly as they could, they were not able, so much as for a minute, to raise the mercury in the longer, and make it subside in the shorter far more than an inch. And to shew, that the experiment was not favourably made for me; the height of the mercurial cylinder in the longer leg, above the surface of that in the shorter, was, at the time of suction, an inch or two short of thirty; and the compress'd air in the shorter leg, was so far from having been expanded, by the exsuction, beyond its natural and first dimensions, that it did not, when the contiguous mercury stood as low as we could make it subside, regain so much as one half of the space it had lost by the precedent compressure; and, consequently, was in a preternatural state of condensation, when it had been freed from that state as far as suction could do it. Whence it seems evident, that it was not *ob fugam vacui*, that the quick-silver did, upon suction, ascend one inch; for

upon

upon the ſame ſcore it ought to have aſcended two, or perhaps more inches; ſince there was no danger, that by ſuch an aſcent, any *Vacuum* ſhould be produc'd, or left in the ſhorter leg of the ſiphon. But, according to our hypotheſis, a clear cauſe of the phenomenon is aſſignable: for, before the ſuction began, there was an equilibrium between the weight of the ſuperior quick-ſilver, in the longer leg, and the ſpring of the compreſs'd air, included in the ſhorter; but, when the perſon began to ſuck, his cheſt being widen'd, ſome part of the air, included in the upper-part of the longer leg, paſs'd into it, and what remain'd, had, by that expanſion, its preſſure ſo weaken'd, that the air, in the ſhorter leg, finding no longer the former reſiſtance, was able, by its own ſpring, to expand itſelf, and, conſequently, to depreſs the contiguous mercury, in the ſame ſhorter leg, and raiſe it as much in the longer.

But here it may be objected, that if the compreſs'd air, in the ſhorter leg, had a ſpring equivalent to the weight of the mercury in the longer leg, why is not the mercury ſuck'd up in this inſtrument, as well as in the free air; ſince, according to me, the preſſure of the included air, upon the ſubjacent mercury, muſt be equivalent to the weight of the atmoſphere: yet experience ſhews, that the weight of the atmoſphere will, upon ſuction, raiſe quick-ſilver to the height of ſeveral inches?

To clear this difficulty, and ſhew, that 'tis not inſuperable, let us conſider, that I make, indeed, the ſpring of the compreſs'd air, to be equivalent to the weight of the compreſſing mercury; and, I have a manifeſt reaſon to do it; becauſe, if the ſpring of the air were not equivalent to that weight, the mercury muſt neceſſarily compreſs the air further; which 'tis granted, in fact, not to do. But then, in our caſe, there ought to be a great difference between the operation of the ſpring of the included air, and the weight of the atmoſphere, after ſuction has been once begun. For, the weight of the atmoſphere, that impels up mercury, and other fluids, when ſuction is made in the open air, continues ſtill the ſame; but the force, or preſſure of the included air, is equal to the counter-preſſure of the mercury, no longer than the firſt moment of ſuction; after which, the force of the impriſon'd air, ſtill, gradually, decreaſes; ſince this compreſſed air, being, more and more, expanded, muſt needs have its ſpring proportionably weaken'd: ſo that 'tis no wonder that the mercury was not ſuck'd up any more than we have ſaid; for there was nothing to make it aſcend to a greater height than that at which the weaken'd ſpring of the expanded air was brought to balance the undiminiſh'd, and, indeed, ſomewhat increas'd weight of the mercurial cylinder, in the longer leg; and the preſſure of the cylinder of air, in the ſame leg, leſſen'd by the action of him who ſuck'd. For, when the orifice of this leg ſtood open, the mercury was preſs'd upon by a cylinder of the atmoſpherical air, equivalent to about thirty inches of quick-ſilver; but, by the mouth and action of him who ſuck'd, the tube was freed from the external air; and, by the dilatation of his thorax, the

PNEUMATICS. neighbouring air, that had a free paſſage through his wind-pipe to it, was proportionably expanded, and had its ſpring and preſſure weaken'd; by which means, the compreſs'd air in the ſhorter leg of the ſiphon, was enabled to impel up the mercury, till the equilibrium was attain'd. And, I muſt here take notice, that, as the quick-ſilver was rais'd by ſuction but a little way, ſo the cylinder rais'd, was a very long one; whereas, when the mercury is ſuck'd up in the free air, it is ſeldom rais'd to half that length; tho', as I noted before, the impellent cauſe, which is the weight of the atmoſphere, continued ſtill the ſame: but, in our ſiphon, when the mercury was ſuck'd up only an inch, the compreſs'd air, poſſeſſing double the ſpace it did before, had, by this expanſion, already loſt a very conſiderable part of its former ſpring and preſſure.

Among the more familiar phenomena of the air-pump, none leaves ſo great a ſcruple, in the minds of ſome ſort of men, as that, when one's finger is laid cloſe upon the orifice of the little pipe, by which the air paſſes from the receiver into the exhauſted cylinder, the pulp of the finger is made to enter, conſiderably, into the cavity of the pipe; which doth not happen without a moderate ſenſe of pain in the lower part of the finger: for moſt of thoſe who are ſtrangers to hydroſtatics, perſuade themſelves, that they feel this painful protuberance of the pulp of the finger, to be effected not by preſſure, but diſtinctly by attraction.

To this, we anſwer, that common air being a body not deſtitute of weight, the phenomenon is clearly explicable by the preſſure of it; for, when the finger is firſt laid upon the orifice of the pipe, no pain, nor ſwelling is produced; becauſe the air, which is in the pipe, preſſes as well againſt that part of the finger which covers the orifice, as the ambient air doth againſt the other parts of the ſame finger. But when, by pumping, the air in the pipe is made to paſs out of that, into the exhauſted cylinder, there is nothing left in the pipe, whoſe preſſure can any thing near balance the undiminiſh'd preſſure of the external air, on the other parts of the finger; and, conſequently, that air thruſts the moſt yielding and fleſhy part of the finger into the place where its preſſure is unreſiſted; that is, into the cavity of the pipe, where this forcible intruſion cauſeth pain.

To illuſtrate this, we took a glaſs pipe, of a convenient length, open at both ends, whoſe cavity was near an inch in diameter. To one end of this pipe, we caus'd to be firmly tied, a piece of very fine bladder, that had been oil'd, to make it both very limber, and unapt to admit water; and care was taken, that the piece of bladder, tied on, ſhould be large enough, not only to cover the orifice, but to hang looſe, ſomewhat beneath it. This done, we put the cover'd end of the pipe into a tall glaſs-body; and the pipe being held ſo, that the end of it reach'd, almoſt, to the bottom of the glaſs, we caus'd water to be pour'd, both into this veſſel, and into the pipe, at its upper orifice, which was left open, that the water might aſcend, equally, both without and within ſide of it. And when the glaſs-body was full of water, and the ſame liquor level to it,

or

or a little higher within the pipe, the bladder at the lower orifice was kept plump; becauſe the water, within the pipe, did, by its weight, preſs as forcibly downwards, as the external water, in the large glaſs, endeavour'd to preſs it inwards and upwards. Then we caus'd part of the water in the pipe, to be taken out of it, by a piece of ſpunge, or by ſuction with a ſmaller pipe; upon which, the water remaining in the pipe, being no longer able, thro' want of weight, to preſs againſt the inſide of the bladder, near ſo forcibly as it did before; the external water, whoſe weight was not leſſen'd, preſs'd the ſides and bottom of the bladder, whereto it was contiguous, into the cavity of the pipe, and thruſt it up therein ſo ſtrongly, that the diſtended bladder made a kind of hemiſphere within the pipe. Here, then, we have a protuberance, like that above-mention'd of the finger, effected by pulſion, not attraction; and, in a caſe where there can be no juſt pretence for having recourſe to nature's abhorrence of a vacuum; ſince the upper orifice of the pipe being left wide open, the air might, freely, paſs in and out. PNEUMATICS.

The like ſwelling of the bladder, we could procure, without taking out any of the internal liquor, by plunging the pipe deeper into the water; for then the external liquor having, by its increaſe of depth, a greater preſſure on the outſide of the bladder, than the internal liquor had on the inſide of it, the bladder muſt yield to the ſtronger preſſure, and conſequently be impell'd up.

If the bladder, lying looſe at the lower-end of the pipe, the upper-end were carefully cloſed, that the air might not get out; and if the pipe, thus cloſed, were thruſt, almoſt, to the bottom of the water, the bladder would not be protuberant inwards, as formerly; becauſe the included air, by virtue of its ſpring, reſiſted, from within, the preſſure of the external water againſt the outſide of the bladder. But the upper orifice of the pipe being unſtopp'd, the air, before compreſs'd, having liberty to expand itſelf, and its elaſticity being weaken'd thereby; the external water would, ſuddenly, with noiſe, drive up the bladder into the cavity of the pipe, and there keep it very protuberant.

To obviate an objection, that might be brought, thro' want of skill in hydroſtatics, I caus'd ſuch a pipe, as the former, to be ſo bent, near the lower-end, that the orifice of it ſtood quite on one ſide, in a right angle. This lower orifice being fitted with a bladder, and the pipe, with its contain'd liquor, being thruſt under water, after the former manner; the lateral preſſure of the water forc'd the bladder into the ſhort horizontal leg, and made it protuberant there, as it had done when the pipe was ſtreight.

Laſtly, that the experiment might not appear confin'd to one liquor; inſtead of water, we put into the ſtreight pipe, as much red-wine as was requiſite to keep the bladder bulging, when near the bottom of the water; and then ſaw the ſuperficies of the red liquor, in the pipe, was much higher than that of the external water; and, if the depth of both

Pneumatics. both liquors, were proportionably leſſen'd, the difference of height betwixt the two ſurfaces, would, indeed, as it ought, decreaſe; but ſtill the ſurface of the wine would be the higher of the two; becauſe, being lighter in ſpecie, than common water, the equilibrium between the preſſures of the two liquors, upon the bladder, would not be maintain'd, unleſs a greater height of wine ſupply'd its want of ſpecific gravity. And, if the pipe were thruſt deeper into the water, the bladder would be made protuberant inwards, as when it contain'd water. 'Tis, therefore, evident, that theſe phenomena, without recourſe to attraction, may be explain'd barely from the equilibrium of liquors.

The End of the Second Volume.