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 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 *b b b b*, 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 *b b h b*. 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.

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its former weight, by the recess of the air, upon pricking it. Supposing, therefore, that the air is not destitute of weight, 'tis easy to conceive, that the part of the atmosphere wherein we live, is greatly compress'd by those directly over it, to the top of the atmosphere. And tho' the height of this atmosphere, according to *Kepler*, scarce exceeds eight miles, yet later astronomers extend it six or seven miles farther. The learned *Ricciolo* makes it reach fifty miles high. So that a column of air, several miles in height, pressing upon some elastic particles of the same fluid here below, may easily bend their little springs, and keep them bent; as if fleeces of wool, were piled to a vast height upon one another, the hairs of the lowest locks would, by the weight of all the incumbent parts, be strongly compress'd. Hence it is, that, upon taking off the pressure of the incumbent air, from any parcel of the lower atmosphere, the particles of the latter possess more space than before. If it be farther objected against this condensation 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 squeezes wool in his hand, he feels it make a continual resistance; so each parcel of the air, about the earth, constantly endeavours to thrust away such contiguous bodies as keep it bent, and hinder the expansion of its parts; which will fly out towards that part, where they find the least resistance. And, since the corpuscles whereof the air consists, tho' of a springy nature, are so very small, as to compose a fluid body, 'tis easy to conceive, that here, as in other fluids, the component parts are in perpetual motion, whereby they become apt to yield to, or be displaced by other bodies; and that the same corpuscles are so variously mov'd, that, if some attempt to force a body one way, others, whose motion hath an opposite determination, as strongly press it the contrary way; whence it moves not out of its place; the pressure, on all sides, being equal. For if, by the help of our engine, the air be drawn only from one side of a body, he, who thinks to move that body, as easily as before, will, upon trial, find himself mistaken.

2. Thus, when our receiver is tolerably exhausted, the brass stopple in the cover, is so difficult to lift, that there seems to be some great weight fasten'd to the bottom of it: for, the internal air being, now, very much dilated, its spring must be greatly weakned; and, consequently, it can but faintly press against the lower-end of the stopple, whilst the spring 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 manifestly felt to decrease; till, at length, the receiver being again filled with air, the stopple may be easily lifted.

It may seem surprizing, that we speak of the air shut up in our receiver, as of the pressure of the atmosphere; tho' the glass manifestly keeps the incumbent pillar of air from pressing upon that within the vessel. But, let us consider, that if a fleece of wool, by pressure, be thus directly reduced into a narrow compass, and convey'd into a close box, tho' the former force ceases to bend its numerous springy parts, yet they continue

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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-

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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, measur'd.

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 ⅓ 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.

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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.

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 ¼ 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.

The flame of tallow and of wax in vacuo.

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.

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 charcoal. 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. *Pneumaticks.*

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.

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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

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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.

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.

An attempt to kindle a combuſtible body, by the ſun's rays in vacuo.

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.

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.

An excited needle in vacuo affected by the magnet.

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

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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,

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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 $\frac{9}{10}$ 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.

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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.

Pneumatics.

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

volatile

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 Fig. 35.
"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 con-
"vey'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".

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 with 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.

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 $\frac{1}{3}$ 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 *The air contain'd in oil.*

Pneumatics. 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

PNEUMATICS. 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.

A pendulum made to ſwing in vacuo.

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 the ſame pendulum appear'd not to laſt ſenſibly longer.

28. That

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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 water, *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.

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.

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.

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

The nature of ſuction.

 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-

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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 *.

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.

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.

Their ascent in capillary tubes.

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

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 was

* 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 qui k-silver are more strongly attracted by each other, than by glass. See all these proved in the *Philos. Trans.* Nº 355. p. 739.

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. PNEUMATICS.

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-

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ceiver, as to render it all opake, and likely to fly in pieces. We ventur'd, however, to approach it, and observ'd, on the outside thereof, at a great distance from the juncture, there was settled a round, whitish spot, or two, which, at first, we thought might be some stain upon the glass; but after finding it, in several qualities, like the oil and salt of the concrete distill'd, we suspected, that the most subtile, and fugitive parts of the impetuous steams, had penetrated the substance of the glass, and, by the cold of the ambient air, were condensed on the surface of it. And, indeed, upon examining the whole matter, a number of us unanimously concluded, that the subtile parts of the distill'd matter, being violently agitated by the excessive heat, had pass'd through the pores of the glass made wide by the same heat. But this having never happen'd, more than once, in any of the distillations we have either made, or seen, it is much more reasonable to suppose, that the perviousness of our receiver, to a body much more subtile than air, proceeded from the looser texture of that particular parcel of metal, the receiver was made of; for all glass is not equally compact, and solid; and from the prodigious heat, which, together with the vehement agitation of the subtile spirits, open'd the pores of the glass; than to imagine, that such a substance as air, should be able to permeate the body of glass, contrary to the testimony of a thousand 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 seems to teach, that tho' air, when sufficiently compress'd, may, perhaps, get entrance into smaller cavities, than water; yet, unless the air be forc'd in, it will not pass them, whilst they may admit of water. I took a glass siphon, the lower end of whose longest leg was drawn so slender, that the orifice, at which the water was to fall out, would hardly admit a very small pin. This siphon being inverted, we so order'd it, that a little bubble of air was intercepted in the slenderest part, betwixt the little orifice, just mention'd, and the incumbent water; whence the air, being not to be forced thro' so narrow a passage, by so light a cylinder of water, as rested upon it, hinder'd the farther efflux of the water, as long as we let it stay in that narrow place: but when, by blowing a little at the wider end of the siphon, that small parcel of air was forc'd out, with some water; the remaining water that before continued suspended, began freely to drop down again, as before. And a glass pipe, either in the form of a siphon, or otherwise, half an inch in diameter, but at one end so slender, as to terminate in an orifice almost as small as a horse-hair, be fill'd with water, it will drop down freely thorough the slender extremity. But if the pipe be inverted, the air will not easily get in at the small hole, thro' which the water pass'd. For, in the sharp end of the pipe, some inches of water will remain suspended; which, probably, would not happen, if the air could get in to succeed it; since, if the orifice were a little wider, the water would immediately subside. And tho' when the pipe is many inches long, a great part of the water will run down at the wider orifice; yet that seems to

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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.

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 æolipile

* 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

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.

Pneumatics.

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.

The height of the atmosphere computed.

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 quick-silver; 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 DC, 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 DC, being $2\frac{13}{54}$ 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 $\frac{92}{1:1}$.

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

PNEUMATICS. 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 $1\frac{44}{171}$. 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 *Tolouse*, 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

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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.

Water made to freeze in vacuo.

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.

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.

A water-thermometer in vacuo.

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 *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

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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.

Birds and mice in the exhausted receiver.

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.

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 than

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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.

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

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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.

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, PNEUMATICS. 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

PNEUMATICS. 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 wind-mill, 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.

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 effervescence 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 effervescence 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 PQ, that is continu'd perpendicularly quite thro' it, and serves, together with the stick RS, for a valve, is to be stopp'd at the bottom of the cylinder, as at NO, when 'tis full of water; 'twas requisite to make the stick RP, two or three feet long. But the chief thing is, that, in the second figure, the pipe AB, whose end B, bends upwards, lies in a groove, purposely made, in the flat wooden board CDEF, 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 small

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.

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

PNEUMATICS. 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 $\frac{1}{4}$; 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 $\frac{1}{4}$ 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 deducting 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 $\frac{3}{8}$ 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

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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 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. *Fig.* 46.

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,

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.

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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.

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Glass bubbles broke by the spring of their own air.

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.

The external force of the spring of uncompress'd air upon solid bodies.

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.

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

either flatten it, or make a dent, though the orifice of the pipe be left open.

11. We took a brass pipe, bent like a siphon, and fitted at the bigger end with a stop-cock, &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 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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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 statics it ought to have done: because many aerial particles emerging from

the body of the water to the upper part of the glass, by their spring concurr'd with the gravity of the water to depress this liquor. And so when the quick-silver was three inches above the stagnant mercury, the water in the pipe was fallen several inches beneath forty-two; and several beneath twenty-eight, when the mercury had subsided an inch lower. But after the pump had been ply'd, to free the water from the latent aerial bubbles, we let in the external air; and having thereby impell'd both the fluids up again into their pipes, and remov'd the receiver; we took them both out, to free them from the air, and fill'd each of them with a little of their respective stagnant liquors; then inverting them again into their proper vessels, we repeated the experiment, and found it to require more pumping than before, to make the liquors begin to subside: so that when the mercury was fallen to three inches, or two, or one, the water subsided so near to the heights of forty-two, twenty-eight, or fourteen inches, that we suppos'd the little differences which appear'd between the several heights of the quick-silver, and fourteen times as great heights of the water, proceeded from some aerial corpuscles yet remaining in the water, and, by their spring, when once they had emerg'd, promoting the depression of it.

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.

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.

An elastic body bent in the exhausted receiver.

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

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. PNEUMATICS.

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

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.

And

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, in

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, a d 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 that *To make a barometer uſeful but at certain times.*

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

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ceeded well. It ſeem'd, by a trial or two, that if the tube were very ſlender, the experiment would not ſucceed.

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.

The ſpontaneous aſcent of ſalts along the ſides of glaſſes.

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 men-

 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 began, 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.

To eſtimate the gravity of cylinders of the atmoſphere in known weights.

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.

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.

The attractive virtue of the load-ſtone in an exhauſted receiver.

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

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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,

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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.

A syringe causing the pressure of the air to raise a considerable weight.

Fig. 52.

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.

The ascent of liquors in syringes owing to the pressure of the air.

Fig. 53.

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.

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.

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. *Fig.* 54. 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.

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 *Fig.* 55. 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

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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.

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.*

Bellows, with the noſe ſtopp'd, open of themſelves, when the preſſure of the air is taken off. Fig. 5.

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 the

PNEUMATICS. the bellows, when the ambient air was withdrawn, much more would the effect have been produced, if the bellows had been perfectly stanch.

An attempt to examine the hypothesis of æther, as to its existence.

38. Since, if there be such a thing as a celestial matter, or æther, it must compose far the greatest part of the universe known to us; it deserves to be enquir'd, whether we can, by sensible experiments, discover its existence, or qualities. To this end I thought our pneumatical engine might contribute, if I could manage therein such a pair of bellows as I design'd; for I proposed to fasten a convenient weight to the upper basis, and clog the lower with another, able to keep it horizontal, and immoveable, so that when, by the help of the turn-key, the upper basis should be rais'd to its full height, the cavity of the bellows might be brought to its full dimensions. This done, I intended to exhaust the receiver, and, consequently, the bellows, thus open'd; so that both the receiver, and they, might be carefully freed from air: after which, I purpos'd to let go the upper base of the bellows, that being hastily depress'd by the incumbent weight, it might suddenly fall down to the lower; and by thus greatly lessening 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 slender pipe, fasten'd either near the bottom of the bellows, or in the upper basis, against, or over the orifice of which pipe, there might be placed, at a convenient distance, either a feather, or the sail of a little wind-mill, made of some other light body, fit to be put into motion by the impulse of any matter which should be forc'd out of the pipe.

Now, if by this means, notwithstanding the absence of the air, it should appear, that a stream of other matter, able to set visible bodies in motion, should issue out at the pipe of the compress'd bellows, it would also appear, that there may be, plentifully, found a much subtiler body than common air, in places deserted by such air; and that it is not safe to conclude, from the absence of the air, in our receivers, and the upper part of the *Torricellian* tube, that there is no body, but an absolute vacuity. But if, on the other side, there should appear no motion at all to be produc'd, so much as in the feather, it should seem, that either the cavity of the bellows was absolutely empty; or that it would be very difficult to prove, by any sensible experiment, that it was full. And if, by any other means, it be demonstrable, that it was replenish'd with æther, we might suppose, from our experiment, that 'tis not easy to make it sensible by mechanical experiments; and that 'tis really so subtile, and yielding a matter, as does not either easily impel light bodies, or sensibly resist, like air, the motions of other bodies thro' it; but is able, freely, to pass the pores of wood, leather, and closer substances, which the air, in its natural state, doth not.

Fig. 58. And, to make the trial more accurate, I caus'd a small pair of bellows to be made with a bladder; and that this might remain entire, we glued the two bases, the one to the bottom, and the other to the opposite part thereof; so that the neck came out at a hole, purposely made for it, into the upper basis; whence, into the neck it was easy to fix what pipe we judg'd fit. We had, also, 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 pastboard, 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

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. Fig. 59. 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. 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,

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. 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-

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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

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. PNEUMATICS. *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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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.

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An attempt to meaſure the force of the ſpring of included air.

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.

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.

Fig. 64.

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

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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. 66.

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 $\frac{3}{8}$ 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

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.

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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.

To shew the weight of air to that of water.

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 I 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

 time

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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.

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. *Fig. 67.*

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.

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.

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.

That 'tis difficult to produce flame without air, shewn by an attempt to kindle sulphur in vacuo.

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.

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.

The efficacy of air in the production of 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.

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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.

PNEUMATICS. *And by means of ſparks of fire* 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.

Two ways of making Aurum fulminans *go off* in vacuo.

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, $\frac{1}{8}$ 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.

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.

Flame difficultly preſerved, without air, in ſulphur.

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.

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Pneumaticks. 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.

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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.

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Actual flame propagated with difficulty in vacuo.

64. Having placed our cylindrical plate of iron, firſt brought to be red-hot, in a receiver, capable of containing two gallons of water; and having, alſo, diligently pump'd out the air, we kindled a little ſulphur, upon the heated plate; and then a piece of dry'd ſpunck, tied to a ſtring, was let down to the flame. When the experiment was finiſh'd, and the ſpunck taken out, we found it, in ſeveral places, not manifeſtly alter'd ſo much as in colour; and, in thoſe parts that had been moſt expos'd to the flame, it was turn'd to a ſubſtance very different from aſhes; being black, and brittle as tinder, and, like that, exceedingly diſpoſed to kindle, upon the touch of fire.

An attempt to make flame kindle camphire, without the help of air.

65. Into the ſame receiver, we convey'd the ſame cylindrical plate of iron; and, when the air had been thoroughly pump'd out, we let a piece of ſuch brimſtone down upon the hot iron, as would there kindle with the heat. A little above this ſulphur, we had tied to the ſame ſtring, 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 brimſtone: but our ſulphur, melting with the heat of the iron, dropp'd from the ſtring 'twas faſten'd to. As ſoon as it came to the bottom, where it was diſtant from the vehement heat of the metal, the flame expired; but a part of it, that happen'd to ſtick to the ſide of the iron, was inflamed by it, and the flame reach'd the camphire, without being able to make it blaze.

We,

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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, nevertheless, it melted, and seem'd, a little to boil.

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 sun-beams 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 ex-

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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.

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 char-coal, 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.

71. We

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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.

This

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 $\frac{7}{18}$ 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

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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. PNEUMATICS. 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 seemed

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,

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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. PNEUMATICS.

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.

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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. *Shell-fish in an exhausted receiver.*

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.

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. *A scale-fish in an exhausted receiver.*

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. PNEUMATICS.

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. *The motion of the separated heart of a cold animal in the exhausted receiver.*

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.

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. *The times wherein animals may be kill'd by drowning, or withdrawing of the air, compared.*

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-

PNEUMATICS. 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 ¾ 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.

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,

at

PNEUMATICS. at the end of an hour, they seem'd to be, all of them, quite dead, but 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.

The power of use to enable animals to support themselves in air, by rarifaction made unfit for respiration.

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.

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

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.

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,

The use of the air to raise and support the steams of bodies, consider'd, with regard to respiration.

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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.

Creeping inſects in vacuo.

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.

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.

Winged inſects in vacuo.

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.

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

 air was let in upon them; and that not producing any signs of life, they were laid in the noon-day sun: but none of them seem'd, in any degree, to recover.

131. *Dec.* 11. We put a great flesh-fly into a very small portable receiver, where, at first, he appear'd to be very brisk and lively; but, as soon as the air was drawn out, he fell on his back, and seemed to have convulsive motions in his feet, and *Proboscis*; from whence he presently 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 shaking the receiver, that he stirr'd faintly up and down. This was done pretty late over night, and next night I found the fly not to be soon revived, either by warmth, or letting in the air. However, in a while after, he recover'd; and being, next morning, sealed up again in that glass, and kept forty-eight hours, tho' over the chimney, he died beyond recovery.

132. We took a large grass-hopper, whose body, besides the horns and limbs, was about an inch in length, and of great thickness, 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 should re-enter. The success was this. First, tho' before the exhaustion of the air begun, the grass-hopper appear'd lively, and continued so for a while: yet, when the air began to be considerably rarified, he seem'd to be very ill at ease, and to sweat out of the abdomen, many little drops of liquor, which being united, trickled down the glass like a little stream, that made, at the bottom, a small pool of clear liquor, amounting to near a quarter of a spoonful; and by that time the receiver was ready to be taken off, the grass-hopper was fallen upon his back, and lay as dead. Secondly, tho' having, a little after, laid the glass in a south-window, on which the sun then shone, I perceived some slow motions in the thorax, as if he strained to fetch breath; yet, I was not sure, but they were convulsive motions; however, they lasted but a while, and then the animal appeared to be quite dead, and to continue so for three hours, from the removal of the receiver. Thirdly, that time being expired, the glass was open'd, and the air let in; notwithstanding which, there appear'd no sign at all of life: but letting the glass rest in a convenient posture, that the water which came from him, might not endanger him, for a quarter, or half an hour; tho' I then perceived no signs of life, yet I caused him to be carried into a sun-shiny place, where the beams of a declining sun presently began to make him stir his limbs, and, in a short time, brought him perfectly to life again.

133. *April* 15. We took one of those shining beetles called rose-flies, and having included it in a very small round receiver, which we exhausted, it struggled much whilst the air was withdrawing; yet presently after, I could perceive but little motion: about six hours after, the fly seemed quite dead, and discover'd no motion upon that of the glass. And within about an hour more, tho' I let the air rush in, yet no sign of life ensued, neither immediately, nor for a pretty while after. So that suspecting the beetle to be

be really dead, I yet, three or four hours after, found him lively. Whereupon, I caus'd the glass to be again exhausted, and secur'd from the air; during which time, the animal seem'd to be much disquieted, but did not lose his motion, soon after.

134. With butter-flies I made several tryals, and, having observ'd them, not only to live, but to move, longer than was expected; I chose to include several of them in receivers, somewhat large, that I might see, whether, in so thin a medium, some or other of them, by help of their large wings, would be able to fly. But, tho', whilst the air continu'd in the glasses, they flew actively, and freely up and down; and, tho', after the exhaustion of the air, they continu'd to live, and were not moveless; 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 progressive motion, supported by the medium, only. And, by frequently inverting the receiver, which was long, they would fall, like dead animals, without displaying their wings, tho', just as they came to touch the bottom, some of them, would, sometimes, seem to make use thereof, but not enough to sustain themselves, or to break their falls.

135. A number of ants being included in a small portable receiver, exhausted about noon; between six and seven in the evening, they seem'd to be all quite dead; and the rather, because, tho' they appear'd very lively just before they were seal'd up, running briskly up and down the bubble they were in; yet, they grew almost moveless, as soon as the air was exhausted; and a little while after appeared more so: tho' I a little suspected, that they were much incommoded, by some glutinous substance, that seem'd to have got into the receiver, from the vapours of the cement. When upon opening the glass, the air rushed in, no sign 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 necessity of air, to the motion even of ants and mites.

We convey'd a number of mites, together with the mouldy cheese, wherein they were bred, into three or four portable receivers, which were, all of them, very small, and not much differing in size. From all of these, except one, we withdrew the air; and, then, making use of our peculiar contrivance to hinder its return, we took them, one after another, from the engine, and laid them by, for further observation. That wherein, to observe the difference, we left the air, was sealed at a lamp-furnace, after the usual manner. Our tryals afforded us the ensuing phenomena.

(1.) The mites, inclosed in the small glass, 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, possibly, would have continu'd much longer, if the glass had not been accidentally broken.

(2.) As soon as ever one of the receivers was remov'd from the engine, I look'd with great attention upon it; and, tho', just before the withdrawing of the air, the mites were seen to move up and down in it; yet, within a few minutes, after the receiver was apply'd to the engine, I could discern in them no life at all; nor was any perceiv'd by younger eyes than mine.

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PNEUMATICS.

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, abovemention'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

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 conspicuous bubble, which, upon admitting the air, shrunk almost into an invisible one, and ascended into the head of the glass. Then, again exhausting the receiver very well, we found it expand itself, so as to fill the capacity of the bolt-head, and to drive out almost all the water. And, upon re-admitting the air, it again shrunk into a bubble, whose diameter (according to our best estimate) was not more than a two and twentieth part of the diameter of the head of the above-mention'd glass; so that, to fill the whole cavity of the head only, it expanded itself 10648 times: but, because it fill'd, likewise, the greatest 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 almost 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, must contain it $3121\frac{167}{481}$ times; so that, in all, the small bubble of air was expanded to above 13769 times its former bulk. PNEUMATICS.

The diameter of the small bubble contracted, was $\frac{1}{27}$ inch.

The diameter of the outside of the head of the glass was $\frac{29}{28}$ inch.

The water, that fill'd the head only, weigh'd sixty grains and a half.

The water that fill'd the head, and as much of the neck as the air had before expanded itself into, weigh'd seventy-eight grains, and one eighth; whence that part in the neck weigh'd seventeen grains, and five eighths. The bolt-head itself weigh'd fifteen grains.

139. We tried this experiment again, and found a small 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 surface of the water within the pipe, much below that of the water external to it.

These experiments may give rise to inquire, what figures and motions in the particles of the air, can explain such a wonderful rarifaction, perhaps, without quite losing its durable spring; how the air comes to be rarifiable so many times more without heat, than hitherto we have found it by heat; and, lastly, what might, reasonably, be conjectur'd about that part of the cavity of an exactly closed glass, where, tho' the eye discovers no visible substance, it appears not, that the common air adequately fills so much as the ten thousandth part.

140. It has not, that I know of, been attempted to discover, whether the air either in the utmost, or in the intermediate degrees of rarifaction we can bring it to, retains a constant and durable elasticity; and what other properties it either gains or loses by confinement*. *The duration of the spring of expanded air.*

To attempt something of this kind, I caused a good bubble of glass, with a stem, to be so blown at the flame of a lamp, that whilst the ball was yet

* Mr. *Hauksbee* has shewn, by experiment, that the spring of the air may be so disturb'd by violent pressure, as to require a considerable time to recover its natural tone and temper; and that this tone will be as the force employ'd, or its continuance in such a violent state. *Hauksb. Experim.* p.110—112. and p. 162—166.

PNEUMATICKS. 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.

PNEUMATICS.

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.

PNEUMATICS.

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! *

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.

PNEUMATICS.

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 AB, 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.

...d 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

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 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. Fig. 73.

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

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. PNEUMATICS. *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.

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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.

BB, a tube, fastned to the globe.

Fig. 80. F, a valve opening inwardly, and shutting the tube BB.

G, the

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 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. PNEUMATICS.

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.

Several ways to forward the production of air; and first, air produced from bread.

(1.) *July* 11. 1676. I included a little piece of bread, very moist, and a little kneaded, with a mercurial gage, *in vacuo*.

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.

From grapes.

(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.

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. Pneumaticks.

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.

PNEUMATICS.

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.

PNEUMATICS.

(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.		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.

Febr. 24.	Its height was	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.

March 2.	Its height was	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, *In raisins and* with six ounces of vinegar, in a receiver; upon which, numerous bubbles *vinegar.* broke out.

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 *In paste.* 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 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

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 ½
27		6
30		6 ½
Nov. 2		7 ½

Nov. 6.

PNEUMATICS.

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.

Octob.

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

Jan. 6. 1678. The height was 32.

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

April 15. The liquor in the lower part of the receiver, had 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.

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

June 2	The height of it was	9	*July* 5	The height of it was	13 $\frac{1}{2}$
6		10	19		15
14		10 $\frac{1}{2}$			

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.

May 8	The height of it was	5 $\frac{1}{2}$	*May* 24	The height of it was	16
10		7 $\frac{1}{2}$	28		18 $\frac{1}{2}$
14		10 $\frac{1}{2}$	*June* 2		20 $\frac{1}{2}$
17		12 $\frac{1}{2}$	6		21 $\frac{1}{2}$
20		13 $\frac{1}{2}$	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

Pneumatics.

May 24. The mercury was one inch high.

May 26. It was almost two inches high.	*June* 2, 6, 10 } The height of it was { $3\frac{1}{2}$, 4, $4\frac{1}{2}$
27. It was two and a half.	*July* 19 No change.
31. There was 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, 28, 29, 31, *June* 2 } The height of it was { $4\frac{1}{2}$, $5\frac{1}{2}$, 7, $9\frac{1}{2}$, 12	*June* 6, 10, *July* 4 } The height of it was { 17, 22, 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.

(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. *Pease, with spirit of wine, in an exhausted receiver.*

June 30. I perceived no more change in the height of the mercury.

July 7. No air was produced, even in the most vehement heat.

June 27. 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. *Pease without spirit of wine, in an exhausted receiver.*

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.

(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. *That the effects of artificial air differ from those of the common, shewn in cherries.*

PNEUMATICS.

June 20. The mercury afcended three and a half. Towards the evening it was two.

The afcent is here always to be underftood, as added to the former.

June 21	$1\frac{1}{2}$
22	$1\frac{1}{2}$
23 The afcent was	2
24	$1\frac{1}{2}$
25	$1\frac{1}{2}$

June 26	3
27 The afcent was	3
28	5
30	$1\frac{1}{2}$

July 1	3
2 The afcent was	4
3	2

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

The height was forty-eight. But, tranfmitting the air into another receiver, the mercury was depreffed to thirty-five inches.

July 6. The afcent of the mercury was four inches, in one night's time.

7. The afcent of it was five and a half in twenty-four hour's time.

8.	5.
9. The afcent of it was	5.
10.	6.

11. The afcent of it was twelve, in the fpace of thirty-four hours.

12. The afcent of it was feven.

13. The afcent of the mercury was three; the height about ninety two inches: but the air being transferr'd into another receiver, the mercury refted at fifty.

14 The afcent was	14
15	11

16 The afcent was	13
17	5

18. The afcent of the mercury was 9; the height of it 102.

19. The height of the mercury was 92; for I tranfmitted part of the air into another receiver.

20. The afcent of the mercury was 15.

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

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

24. The afcent of the mercury was 4; the height of it 79 inches; but the air, being tranfmitted into another receiver, the mercury refted at 62.

25. The afcent was	8
26.	9

27. The afcent was	4
28.	5

30. The afcent of it was ten, the height ninety-eight. Part of the air being tranfmitted into another receiver, it refted at fixty-four.

31.	6.
Aug. 1. The afcent of the mercury was	9.
2.	4.

3. I tranfmitted the air into another receiver, and the mercury remain'd at fixty-eight.

4. I tranfmitted the air again into another receiver, and the mercury refted at fifty-four.

6. The

Pneumatics.

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

October 1. The height remained at 35.

Octob. 2, 5, 6. The height was 36. | *Octob.* 10. The height was 35. 35. The height was $32\frac{1}{2}$.

The air got not out, but the cold began, and increased.

Novemb. 9. The height remained the same.

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

Sept. 17. The height was 28. The grapes turned yellow.

Sept. 18, 19, 20, 21. The height of it was 29, 30, 31, 32. | *Sept.* 22, 23. The height of it was 35, 20. Some air had broke out; and the grapes were of a yellow colour.

Sept. 24, 25. The height of the mercury was 25, 22.

26 The height almost the same.

27, 29, 30. The height of it was 22, 27, 28.

Octob. 1. and 2. The height 28.

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

Novemb. 9. The height was 13. Some air had got out.

December 19. The height of the mercury was 20 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

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 ½		
25		9 ½		
26		14 ½	In the evening	18.
27		21 ½		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

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.

(32.) *August* 2. 1676. I shut up a July-flower in a receiver, with air produced from paste, made with meal, and not mixed. *A July-flower in factitious air.*

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.*

August 2. I shut up a July-flower in a receiver, with common air, not mixed. *In common air.*

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.

Aug. 2. I included two July-flowers *in vacuo*, and took special care, that no humidity should be included with them. *July-flowers in vacuo.*

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.*

(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. *Apricocks and plumbs in common air.*

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.

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. *The same in artificial air.*

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

July

PNEUMATICS.

July 30. The height was forty-four. The apricocks which were cut, began to moisten, and dissolve into water.

July 31	The height was	51
Aug. 1		60

Aug. 2. The height was sixty-five. Towards evening, when some liquor had escaped out of the receiver, I screwed 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 soft; especially those, whose lower parts were immers'd in the water: the rest were a little more firm, but all of them retain'd a grateful taste.

Hence it seems, that air produced from cherries, promotes the alteration both of colour, and firmness in apricocks.

It appears, also, that some part of such air is destroyed at the first.

Plumbs in common air, in artificial air, and in vacuo.

(34.) *July* 30. 1676. I put plumbs, cut asunder, into three receivers; one of which was full of artificial air, produced from goosberries; the second, full of common air; and the third exhausted.

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 soft.

Aug. 5. In the artificial air, the plumbs had contracted a red colour, humidity, and softness; in the common air, they seemed black and mouldy, yet retain'd their firmness; in the evacuated receiver, they were almost dissolved.

Aug. 7. The plumbs, in the common air, began to soften.

Aug. 8. The plumbs, in the common air, seemed to have lost 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 seems 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 some produced from grapes; and included the grapes themselves in the same receiver, that the common air might be the better saturated 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
18		39	*Octob.* 1		48

Octob. 2. The same height continued.

3. The height of it was 52 and a half.

5. The height the same; but the peaches seem'd moist.

6. The height of it was 58.

7. The height of it was the same.

8. The height of it was 61.

11. The mercury ascended a little.

19. The height of it was 65.

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

27. The cold abated, and the mercury ascended.

30. The height 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 of it was	6	*Aug.* 13	The height of it was	16
11		10	14		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 of it was	51	*Aug.* 14	The height of it was	67
13		61	15		73

Aug. 16. The mercury descended; yet nothing had got out.

Aug. 17. The mercury exceeded not sixty-seven inches in height; yet the air could by no means escape.

Aug. 18. The mercury remain'd at the same height; but, suffering the air to break out, it had a sharp odour; and the taste of the fruit seem'd very acid, and the pulp exceeding soft.

Pears in an unstopp'd receiver. *August* 4. 1677. The third receiver. I put a quarter of each of the aforesaid pears into a receiver, not exactly shut.

Aug. 6. The pears seem'd to change their colour.

Aug. 7. One of the pieces began to lose its firmness; but, in the artificial air, another piece yesterday seem'd wholly rotten.

Aug. 8. One piece was mouldy; the rest were soft.

Aug. 9. The pears gradually grew more rotten.

Aug. 11. They were wholly mucid, and rotten.

This receiver, compared with the first, shews, that corruption begins not in the free air sooner than in included air; but, when begun, that it is much more violent and sudden; because the included air may be satiated.

Pears in vacuo. *August* 4. 1677. The fourth receiver. I included one quarter of each of the said pears *in vacuo.*

Aug. 6. The height of the mercury was 5.

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

20. Hitherto the pears had undergone no alteration; but this day they began to be soft. The mercury ascended not.

Aug. 26. Neither the pears, nor the height of the mercury, were at all alter'd.

This production of the air seems very regular.

Hence we find the aptness of artificial air to soften fruits.

And that the production of air was here promoted by artificial air, is very probable; tho' it had succeeded otherwise with apricocks.

Apricocks in common air. (37.) *August* 21. 1677. The first receiver. I divided six apricocks, each into four parts; and put one piece of each into a receiver full of common air, and stopp'd it firmly with a screw.

Aug. 22. The apricocks seemed riper than yesterday; but no air was produced by them.

Aug. 23. One piece, contiguous to the water, began to be mouldy, and the rest inclined to putrefaction. The mercury seemed to have ascended a little.

Aug. 24. A piece next the water, was cover'd with much mouldiness; another piece, more remote from the water, was somewhat mouldy also; but all were rotten.

Aug.

PNEUMATICS.

Aug. 25. The fruit contracted no more mouldineſs; but the putrefaction increaſed. The height of the mercury was ſeven inches.

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.

PNEUMATICS.

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:

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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. *Pneumaticks.*

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.

(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. *Orange in compressed and 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.

(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 *Roses in common and compressed air.*

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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.

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.

Flies in common and compreſſed air.

(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.

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.

Frogs in common and compreſſed air.

(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.

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 $\frac{1}{4}$
5		6 $\frac{1}{2}$	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. PNEUMATICS.

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.

(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. *A shrew-mouse in condensed air.*

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 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 order'd the

PNEUMATICS. matter so, that the coral fell into the glass of vinegar: but the air, produced from thence, did not restore any power of motion to the bee; but, when she was exposed to the open air, she soon began to move herself.

Hence a suspicion arises, that artificial air is unfit to preserve the life of animals.

Flies in artificial air of gooseberries.

(56.) *August* 12. 1676. I put two flies into a receiver, and, exhausting the common air, substituted, in its stead, air produced from gooseberries, 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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had got out together with it. A frog, being put into it, seem'd to be vehemently mov'd, and convulsed, for ten minutes, as the rest did, and, then, she seem'd quite dead; but, after a full minute, I open'd the receiver, when, the frog, being expos'd to the open air, within a quarter of an hour, began to move.

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

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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.

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

PNEUMATICS. 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.

In

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 access of the external air. The frog-spawn *in vacuo*, all swell'd into bubbles.

May 2. No frogs were produced in either receiver; and the spawn, kept *in vacuo*, remain'd still full of bubbles: but, about three days ago, all the bubbles vanish'd, and the spawn was changed to a green liquor.

July 2. The receivers remained in a window, exposed to the noon-day sun; so that some water, mix'd with the frog-spawn *in vacuo*, and the very spawn itself was elevated into vapours; and afterwards, sticking to the sides of the receiver, out of its own vessel, was there condensed: but the vessel, kept in the common air, still contained all its water, together with the spawn.

A frog in vacuo.

(73.) *June* 15. I shut a frog in an exhausted receiver, at about seven 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 space, had produced some air, rather than consumed it.

June 18. The frog, hitherto left *in vacuo*, was swollen 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 less hurtful to such kind of animals.

Fly-blowings in vacuo.

(74.) *August* 3. 1678. I put fly-blowings, upon flesh, into an exhausted 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 see, 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 small eels, or vinegar worms, in an exhausted receiver.

Aug. 29. The worms still moved, but were fewer than at first.

September 6. Yesterday some of the worms still moved, but now I could not see one; and using a microscope, 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 also affected by the presence and absence of the air.

Fire in compressed air, and first perfumed cones, included, and fired, in condensed and common air.

(76.) *May* 14. I took a perfumed cone, which being once kindled in the free air, will, by degrees, wholly consume; and put it into a receiver firmly stopp'd with a screw: then I intruded air into it, till the mercury rose one hundred and twenty inches above its wonted height; when, by a burning-glass, I kindled the cone, which presently darkned all its receiver with smoke, and, after some time, seven eighths of an inch thereof, in length, were reduced to ashes; but taking out the cone, and blowing away the ashes, I found only the superficies thereof consumed; the inner parts remaining entire.

I

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.

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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 produced 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.*

PNEUMATICS.

The production of air from grapes in vacuo.

(87.) *September* 9. 1676. I exhausted the air out of a receiver, half full of dried grapes, and fortified it against the external air.

Sept. 10. In twenty-four hours time, the height of the mercury was $\frac{1}{2}$.

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

14.	The ascent of the mercury was	$\frac{3}{8}$
17.		$\frac{3}{8}$
22.		$\frac{5}{8}$

27. The ascent was $\frac{5}{8}$. The height three inches.

October 11. The height of the mercury was now about six inches.

From figs.

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 rested at the height of three inches.

Sept. 10. No air was produced.

Sept. 17. Perceiving no air to issue out of the figs, I open'd the receiver.

Hence we learn, that dried fruits, put into an exhausted receiver, produce very little air with regularity.

From pears and apricocks.

(88.) *August* 5. 1676. I included pears and apricocks *in vacuo*.

Aug. 6. In eighteen hours time, the mercury rose 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 space, the mercury mounted three quarters. Its height was seven 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 last days, being hotter than the foregoing, the ascent of the mercury was two and a quarter; its height, forty-six and a quarter.

Sept. 10. The height of the mercury was forty-seven and a half.

13. The mercury was depressed; 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 ascended, gradually, to fifty-two and a half.

Nov. 28. The apricocks were reduced to water; the skin being sever'd from the pulp, yet no more air produced.

Jan. 10. 1677. Whilst it froze very hard, the mercury rose to fifty-seven inches; but, when it thaw'd, it sunk to twenty-three. Whether the strength of the frost open'd some 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.) *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. PNEUMATICS. *Beef.*

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, 27	The height of it was	18, 21	*Aug.* 28, 31	The height of it was	23, 30
Sept. 1, 2, 3	The height of it was	32, 35, $38\frac{1}{2}$	*Sept.* 4, 5, 6	The height of it was	$44\frac{1}{2}$, $45\frac{1}{2}$, 50

Sept. 7. The height of it was the ſame, becauſe ſome air had eſcaped; but I prevented that for the future.

Sept. 8, 9, 10 — The height of the mercury was $53\frac{1}{2}$, $54\frac{1}{2}$, 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.

PNEUMATICS.

June 24	The height of it was	75
25		90
June 26	The height of it was	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; and before the receiver could be guarded against the external air, the mercury ascended an inch. *In plumbs in vacuo.*

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
19		12
20		14
22		18
Sept. 23	The height of it was	18
24		19
26		23
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
7		33
Octob. 9	The height was	$33\frac{1}{2}$
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
19		$39\frac{1}{2}$
22		41
26		43
Octob. 29	The height of it was	45
Nov. 2		46
5		47
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 before we could prevent the entrance of the external air, it had impell'd the mercury half an inch; and, afterwards, in half an hour, the air, produced from the goosberries, impell'd it another half inch. *In Goosberries in vacuo.*

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.

In

PNEUMATICS.

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 $\frac{3}{4}$.

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

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.

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	The height of it was	36	24	17	The height was	44	24
15		39		18		47	
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.)

(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. *In grapes in vacuo.*

Sept. 13	The height of it was	5
14		10
16		17
Sept. 17	The height was	19
19		23
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 $\frac{1}{2}$
27		36 $\frac{1}{4}$
28		36 $\frac{1}{4}$
29		37 $\frac{1}{4}$
30		37 $\frac{1}{2}$
Octob. 2	The height of it was	39 $\frac{1}{2}$
4		39 $\frac{1}{2}$
5		40 $\frac{1}{2}$
7		41 $\frac{1}{2}$
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.

(111.) *August* 10. 1677. I put pears, cut asunder into an exhausted receiver. Towards evening the mercury was risen ten inches. *In pears in vacuo.*

Aug. 11	The height of it was	20
13		38
14		48
Aug. 15	The height was	55
16		60
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
19		64
Aug. 20	The height of it was	70
21		72

The air being transmitted into another receiver, the mercury remained at 61.

Aug. 22	The height of it was	68
23		74
Aug. 24	The height of it was	79
25		81

The air being transmitted into another receiver, the height of the mercury was 61.

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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	The height of it was	60	*Aug.* 30	The height was	83
28		68	31		88
29		75	*Sept.* 1		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

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both ends; and many bubbles suddenly formed, separated the water, contained in the tube, to a great distance, as before: in the mean time, many other bubbles seemed incessantly to pass out from the end of the tube immersed, tho' they afterwards appear'd less frequent.

But the water being suspended higher in the tube, seemed to contain no bubbles, whilst the end only emitted so 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 vessel, and bubbles soon began to be formed as before; some of which succeeded others within half a minute: but, afterwards, they were less 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, some would adhere to the end of it, either sooner or later.

May 6. I made the same experiment, with the infusion of nephritic wood, with a like success; excepting that the bubbles emerged, and penetrated the liquor, before they had acquir'd any considerable bigness: whence we may conjecture, that this liquor is very thin, and hath no viscosity to resist a pervading body.

May 10. I repeated the same experiment with spirit of wine, mixed with a certain oil, made *per deliquium*, but found nothing new; only the liquor ascended not so high into the tube.

Hence the bubbles seem to be formed, at the extremity of the tube, of aerial particles, swimming in the water; which finding some impediment at that end, cannot pass by, and so, new ones coming upon them, they swell into bubbles.

(121.) *July* 18. 1676. Two days ago, I took some horse-beans, and included them in an iron tube, closely stopped; first pouring water on the compressed beans, till the tube seemed wholly full; to try whether the expansive force of the beans would break the tube. This day the tube seem'd not to be alter'd, but, the stopple being loosen'd, some air broke out, and much water, which was not imbibed by the beans, fell upon the ground: then we heard a noise, as it were, of bubbling water, for above an hour. *Horse-beans and water included in an iron tube.*

July 25. The tube remain'd in the same posture; but now one of the ends of it being unstopp'd, and some beans taken out, the murmur of the bubbling water was heard as before.

From hence it seems to follow, that beans contain air, which, in a great compressure, cannot escape; but breaks out, if freed from the compressing force.

(122.)

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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.

(128.)

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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.

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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.

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This experiment shews, that some artificial air may be destroy'd; but why this destruction happens, sometimes sooner, sometimes later, deserves a further enquiry.

The different celerity wherewith air is produc'd in vacuo, and in common air, shewn from paste in common air.

(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.

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.

Paste in vacuo.

July 10. 1676. I put another quantity of the same paste, much less than the former, into an exhausted receiver.

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 beoinning, 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 $\frac{2}{3}$.

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

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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, 20, 21	The height of it was 5, 7, 9	Sept. 22, 23, 24	The height was 11, 12, 15

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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, 27, 28	The height of it was 10, 14, 18	Aug. 29, 30, 31	The height was 21, 25, 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 pears in vacuo.*

Aug. 25. The height of the mercury was 11.

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Aug. 26, 27	The height of it was	17, 25	*Aug.* 28, 29	The height of it was	28, 30

Aug. 30. The mercury aſcended no higher; 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, 27, 28	The height of it was	9, 12, 15	*Aug.* 29, 30, 31	The height was	19, 25, 28

September 1. The height of it was 29.

2. The height of it was 30.

3. The receiver was forced from 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, 29	The height of it was	7, 10	*Aug.* 30, 31	The height of it was	12, 13

Sept. 1. The height of the mercury was 15.

2. The height of it was 16.

3. The height of it was 18.

4. The height of it was the ſame.

Sept. 5. The height of the mercury continued the ſame; but 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.

(144.) *Sept.* 10. 1677. I put two ounces of grapes, not bruis'd, into a receiver able to hold ten ounces of water. *In whole grapes.*

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		17	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.

Sept. 10. 1677. I included two ounces of ripe, bruis'd grapes in a receiver capable of holding ten ounces of water. *And bruis'd grapes.*

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.

This

PNEUMATICS. 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

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.

(159.)

(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 cut 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.

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.

Pneumatiqs.

[illegible] 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 butter-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.

(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.

PNEUMATICS. *Peaches included with ale, beer, and 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. Hence

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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. 13. There was no change in the three first receivers; but in the fourth, 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.

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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.

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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.

Hence birds may be long preserv'd by the help of beer, or ale.

Apples included in receivers.

(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.

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, 56; 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 waterhad 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

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Feb. 27. In the first receiver, the height of the mercury was thirty inches.

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.

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

his 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 atmosphere

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

PNEUMATICS. 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 quicksilver, 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.

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

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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,

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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.

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.

The funicular hypothesis examin'd.

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.

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; the

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?

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

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 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 sticking, 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 by 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, ſurfaces, of like ſort, are ſeparated from the wax, or tallow, underneath, by "the heat above, and extenuated into a moſt ſubtile flame; which, doubtleſs, 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 exhausted receiver, they appear, by many signs, to continue fluid. PNEUMATICS.

I know, that by calling this extenuated substance, a *Funiculus*, he intimates, that it has its spring inwards, like lute-strings, and ropes forcibly stretch'd; but there is no small disparity betwixt them: for, in strings, there is requir'd either wreathing, or some peculiar, and artificial texture of the component parts; but, a rarifaction of air, does not infer any such contrivance of parts, as is requisite to make bodies elastic. And, since lute-strings, *&c.* must, when they shrink inwards, either fill up, or lessen their pores, and increase in thickness, as they diminish in length; our author's *Funiculus* differs widely from them; since it has no pores to receive the shrinking parts; and contracts its length, without increasing its thickness. Nor does it, to me, seem very probable, that when, for instance, part of a polish'd marble is extended into a *Funiculus*, that *Funiculus* strongly aspires to turn into marble again. And 'tis very unlikely, that the space, our author would have replenish'd with his funicular substance, should be full of little, highly-stretch'd strings, that lay fast hold on the surfaces of all contiguous bodies, and always violently endeavour to pull them inwards. For, a pendulum being set a moving, in our exhausted receiver, vibrated as freely, and with the string as much stretch'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 consisting of innumerable strings, exceedingly stretch'd. And 'tis strange, if these strings, thus cut, or broken, by the passage of these bodies thro' them, could so readily have their parts re-united, and immediately be made entire again. And, in this case, the two divided parts of each small string, do not, like those of other broken strings, fly back from one another, but meet, and unite again; yet, when in the *Torricellian* experiment, the tube, with the contain'd mercury, is suddenly lifted out of the stagnant quick-silver into the air, the *Funiculus* so strangely contracts itself, that it quite vanishes; so that the ascending mercury may rise to the very top of the tube.

But this is not all that renders our author's hypothesis improbable; for it necessarily supposes such a rarifaction, and condensation, as is unintelligible.

The nature of rarifaction consider'd.

We must here premise, that a body is commonly said to be rarified, or dilated, when it acquires greater dimensions than it had before; and to be condensed, when it is reduced to less dimensions, that is, into a less space; and that there are three ways of explaining rarifaction: for, either we must say, that the corpuscles whereof the rarified body consists, depart from each other, so that no other substance comes in between them, to fill up the deserted spaces; or, that these new interstices, are but dilated pores, replenish'd, as those of a tumid spunge by water, with some subtile ethereal substance; or, lastly, that the same body does not only obtain a greater space in rarifaction, and a lesser in condensation; but, adequately, and exactly fills it: and so, when rarified, acquires larger dimensions,

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.

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-mentioned 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. PNEUMATICS.

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.

The pressure and spring of the air confirm'd.

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, PNEUMATICS. before the mouth of it was open'd, under the ſtagnant mercury, and notice taken how low ſuch a quantity of that air depreſs'd the mercurial cylinder, 'twas obſerv'd, that at the mountain's foot, the included air was not able to depreſs the quick-ſilver ſo much. Whence we infer, that the cylinder of air, at the top of the hill, being ſhorter and lighter, did not ſo ſtrongly preſs againſt the included air, as did the ambient air at the bottom of the hill, where the aerial cylinder was longer, and heavier.

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

to break in below the bottom of the pipe, and aſcend thro' the water in bubbles; after which, the glaſs being drawn up again, the water was, very manifeſtly, re-aſcended. Hence 'tis evident, that the atmoſphere gravitates more, *cæteris paribus*, near the ſurface 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.

But, for an experiment of the ſame kind, made at a greater height, take the following, communicated by Dr. *Power*.

On the 15th of *October*, 1661, we took a weather-glaſs AB, about two feet in length, and carrying it to the bottom of *Hallifax-hill*, the water ſtood in the ſhank at 13 inches above that in the veſſel: thence carrying it, thus fill'd, with the whole frame, immediately to the top of the ſaid 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 ſaid hill; which proves the elaſticity of the air: for the internal air AC, which was of the ſame power and extenſion with the external, at the bottom of the hill, manifeſted a greater elaſticity, than the mountain-air there manifeſted preſſure; and ſo extended itſelf further by CD. Fig. 84.

The like experiment, I hear, the ſame ingenious perſon has lately repeated, and found the deſcent of the water to be greater than before. And tho' ſome have thought it ſtrange, that, on a hill, far inferiour to the *Alps*, and *Appennines*, ſo ſhort a cylinder of water ſhould fall ſo much; yet I ſee not any reaſon to diſtruſt, upon this ground, either this experiment, or ours made at *Weſtminſter*; but rather wonder the water fell no more, if the hill be conſiderably high: for their ſuſpicion ſeems grounded on a miſtake; as if becauſe the quick-ſilver, in the *Torricellian* experiment, made without purpoſely leaving any air in the tube, would not at the top of the mention'd hill, have ſubſided above an inch, the water, that is near 14 times lighter, ſhould not fall above a 14th part of that ſpace; whereas, in the *Torricellian* experiment, the upper-part of the tube has little, or no air left in it, while the correſpondent part of the weather-glaſs contain'd air, whoſe preſſure was little leſs than that of the atmoſphere at the bottom of the hill; and, conſequently, muſt be much greater, than the preſſure of the atmoſphere at the top of the hill.

Another particular, which confirms our hypotheſis, is that experiment made by the ſame M. *Paſchal*, by carrying a ſlack-blown foot-ball, from the bottom to the top of an high mountain; for, the foot-ball gradually ſwell'd, the higher it was carry'd: ſo 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; ſo that, at the foot of the hill, it was flaccid as before. We have here an experiment to prove our hypotheſis, wherein recourſe cannot be had to any body, forcibly, and preternaturally diſtended, ſuch as is pretended to remain in the deſerted ſpace of the tube, in the *Torricellian* experiment.

But, further, our author's hypotheſis is needleſs; for, he denies not that the air has ſome weight and ſpring, but affirms it very inſufficient

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.

AA	A	B	C	D	E
48	12	00	Added to 29 1/8 makes	29 2/16	29 2/16
46	11 1/2	01 7/16		30 9/16	30 6/16
44	11	02 13/16		31 15/16	31 12/16
42	10 1/2	04 6/16		33 8/16	33 1/7
40	10	06 3/16		35 5/16	35
38	9 1/2	07 14/16		37	36 15/19
36	9	10 2/16		39 5/16	38 7/8
34	8 1/2	12 8/16		41 10/15	41 2/17
32	8	15 1/16		44 3/16	43 11/16
30	7 1/2	17 15/16		47 1/16	46 3/5
28	7	21 3/16		50 5/16	50
26	6 1/2	25 3/16		54 5/16	53 10/13
24	6	29 11/16		58 13/16	58 2/8
23	5 3/4	32 3/16		61 5/16	60 18/23
22	5 1/2	34 15/16		64 1/16	63 6/11
21	5 1/4	37 15/16		67 1/16	66 4/7
20	5	41 9/16		70 11/16	70
19	4 3/4	45		74 2/16	73 11/19
18	4 1/2	48 12/16		77 14/16	77 2/3
17	4 1/4	53 11/16		82 12/16	82 4/17
16	4	58 2/16		87 14/16	87 3/8
15	3 3/4	63 15/16		93 1/16	93 1/5
14	3 1/2	71 5/16		100 7/16	99 6/7
13	3 1/4	78 11/16		107 13/16	107 7/13
12	3	88 7/16		117 9/16	116 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

Pneumatics.

This sufficiently proves the principal thing for which I here alledge; 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.

A TABLE of the Rarifaction of the Air.

A	B	C	D	E
1	00	Subtracted from 29 ¾ leaves	$29\frac{3}{4}$	$29\frac{3}{4}$
$1\frac{1}{2}$	$10\frac{5}{8}$		$19\frac{1}{8}$	$19\frac{5}{6}$
2	$15\frac{3}{8}$		$14\frac{3}{8}$	$14\frac{7}{8}$
3	$20\frac{3}{8}$		$9\frac{3}{8}$	$9\frac{15}{12}$
4	$22\frac{5}{8}$		$7\frac{1}{8}$	$7\frac{7}{16}$
5	$24\frac{1}{8}$		$5\frac{5}{8}$	$5\frac{19}{20}$
6	$24\frac{7}{8}$		$4\frac{7}{8}$	$4\frac{23}{24}$
7	$25\frac{4}{8}$		$4\frac{2}{8}$	$4\frac{1}{4}$
8	26		$3\frac{6}{8}$	$3\frac{23}{32}$
9	$26\frac{3}{8}$		$3\frac{3}{8}$	$3\frac{11}{36}$
10	$26\frac{6}{8}$		3	$3\frac{39}{40}$
12	$27\frac{1}{8}$		$2\frac{5}{8}$	$2\frac{23}{48}$
14	$27\frac{2}{8}$		$2\frac{4}{8}$	$2\frac{1}{8}$
16	$27\frac{6}{8}$		2	$1\frac{55}{64}$
18	$27\frac{7}{8}$		$1\frac{2}{8}$	$1\frac{47}{72}$
20	28		$1\frac{6}{8}$	$1\frac{9}{80}$
24	$28\frac{2}{8}$		$1\frac{4}{8}$	$1\frac{23}{96}$
28	$28\frac{3}{8}$		$1\frac{3}{8}$	$1\frac{1}{16}$
32	$28\frac{4}{8}$		$1\frac{3}{8}$	$0\frac{119}{128}$

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 in- "cumbent atmosphere, and the density "of the air is proportionable to the "compressing force, it follows, by com- "putation, 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 there- "abouts; 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

PNEUMATICS. 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

PNEUMATICS. *Particular pneumatical experiments defended.*

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.

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.

PNEUMATICS. 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 resistance of the air; it's no wonder, that the air, remaining in the receiver, should support the lower-most marble, whose diameter was near two inches, and a weight of four ounces; those two weights being inferior to that of a mercurial cylinder, of the same diameter, and an inch in length: and tho' they were not, yet, perhaps, the receiver was less empty'd, when we made the 31st experiment, than when we made the 17th. And 'twas with the same pair of marbles, that, before an illustrious assembly, the upper-most drew up the lower-most, tho' clogg'd with a weight of above 430 ounces.

As for the account our author gives of this phenomenon, few, I believe, will acquiesce in it: for, not to insist upon the objection, which himself takes notice of, that, according to him, the distended air in the receiver, should draw the adhering marbles asunder, his explanation supposes, that there cannot, naturally, be a vacuum; whence he infers, that "the "stone could not descend, but by leaving such a thin substance behind "it, as happens in the descent of quick-silver, or water." He adds, that the adhesion, in our case, proves obstinate, because such a substance is far more difficult to be separated from marble, than from quick-silver, or any other kind of body; but this assertion is precarious. And though I have made numerous experiments, with stones of several sizes, yet I could never find, that, by their cohesion, they would sustain a weight greater than that of a pillar of the atmosphere, that press'd against the lowest; which is a considerable circumstance, that much better agrees with our explanation, than with his.

Of the sudden extinction of animals, included in our receiver, which I ascribe to the excessive thinness of the air therein, he says, "it seems "impossible they should die so soon, merely thro' want of a thick air:" but gives no other reason, than the suddenness of the effect; which, too, seems grounded upon a mistake: for, the creatures, he mentions, were a bee, a fly, and a caterpillar; and those included in a small receiver, which could be suddenly exhausted; and these, indeed, became moveless, within a minute. And tho' these insects did, in so short a time, grow moveless, yet they were not so soon kill'd, as appears by the narrative. The sanguineous animals, that did, indeed, die, were kill'd, more slowly. And having, purposely, enquir'd of a diver, how long he could, before he was accustom'd to dive, remain without breathing, or the use of a spunge; he told me, that at first he could hold out about two or three minutes, at a time: which made me think, that divers become able to continue under water so long, either by a peculiar constitution of body, or, a gradual exercise. And, I am apt to think, that, as 'tis usual, he hereby meant a much shorter time than, when exactly measur'd, it amounts to. For, having made trial upon two live moles, one of them, included in a small 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 sign of life. Hence 'tis not impossible, that the want of respiration, should dispatch 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.

The weight and spring of the air asserted.

MR. *Hobbs*, in disputing against me, seems, generally to have mis-apprehended 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 other-wise. 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, a-gree to call æthereal.

But he does not, that I remember, deny the truth of any of the mat-ters 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 ad-vance: but rather rejects our two grand hypotheses, the weight and spring

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PNEUMATICS. 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

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.

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 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 carri-

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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.

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 driven up into the space deserted by the sucker, because it is forcibly impell'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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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 thoroughly 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

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