XIX. *Observations on different Kinds of Air.* *By* Joseph Prieſtley, *LL. D. F. R. S.*

Read March 5, 12, 19, 26, 1772.

THE following obſervations on the properties of ſeveral different kinds of air, I am ſenſible, are very imperfect, and ſome of the courſes of experiments are incomplete; but a conſiderable number of facts, which appear to me to be new and important, are ſufficiently aſcertained; and I am willing to hope, that when philoſophers in general are apprized of them, ſome perſons may be able to purſue them to more advantage than myſelf. I therefore think it my duty to give this Society an account of the progreſs I have been able to make; and I ſhall not fail to communicate any farther lights that may occur to me, whenever I reſume theſe inquiries.

In writing upon this ſubject, I find myſelf at a loſs for proper terms, by which to diſtinguiſh the different kinds of air. Thoſe which have hitherto obtained are by no means ſufficiently characteriſtic, or diſtinct. The terms in common uſe are, fixed air, mephitic, and inflammable. The laſt, indeed, ſufficiently characterizes and diſtinguiſhes that kind of air which takes fire, and explodes on the approach of flame; but it might have been termed fixed with

as much propriety as that to which Dr. Black and others have given that denomination, ſince it is originally part of ſome ſolid ſubſtance, and exiſts in an unelaſtic ſtate, and therefore may be alſo called factitious. The term mephitic is equally applicable to what is called fixed air, to that which is inflammable, and to many other kinds; ſince they are equally noxious, when breathed by animals. Rather, however, than to introduce new terms, or change the ſignification of old ones, I ſhall uſe the term fixed air, in the ſenſe in which it is now commonly uſed, and diſtinguiſh the other kinds by their properties, or ſome other periphraſis. I ſhall be under a neceſſity, however, of giving a name to one ſpecies of air, to which no name was given before.

Of FIXED AIR.

Fixed air is that which is expelled by heat from lime, and other calcareous ſubſtances, and, when deprived of which, they become quick-lime. It is alſo contained in alkaline ſalts, and is generated in great quantities from fermenting vegetables; and being united with water, gives it the principal properties of Pyrmont-water. This kind of air is alſo well known to be fatal to animals; and Dr. Macbride has demonſtrated, that it checks or prevents putrefaction.

Living for ſome time in the neighbourhood of a public brewery, I was induced to make a few experiments on this kind of air, there being always a large body of it, ready formed, upon the ſurface of the fermenting liquor, generally about nine inches

or

or a foot in depth, within which any kind of ſubſtance may be very conveniently placed; and though it muſt be continually mixing with the common air, and is far from being perfectly pure, yet there is a conſtant ſupply from the fermenting liquor, and it is pure enough for many purpoſes.

A perſon, who is quite a ſtranger to the properties of this kind of air, would be agreeably amuſed with extinguiſhing lighted candles, or chips of wood in it, as it lies upon the ſurface of the fermenting liquor; for the ſmoke readily unites with this kind of air, probably by means of the water which it contains; ſo that very little or none of the ſmoke will eſcape into the open air, which is incumbent upon it. It is remarkable, that the upper ſurface of this ſmoke, floating in the fixed air, is ſmooth, and well defined; whereas the lower ſurface is exceedingly ragged, ſeveral parts hanging down to a conſiderable diſtance within the body of the fixed air, and ſometimes in the form of balls, connected to the upper ſtratum by ſlender threads, as if they were ſuſpended. The ſmoke is alſo apt to form itſelf into broad flakes, parallel to the ſurface of the liquor, and at different diſtances from it, exactly like clouds. Theſe appearances will ſometimes continue above an hour, with very little variation. When this fixed air is very ſtrong, the ſmoke of a ſmall quantity of gunpowder fired in it will be wholly retained by it, no part eſcaping into the common air.

Making an agitation in this air, the ſurface of it, which ſtill continues to be exactly defined, is thrown into the form of waves, which it is very amuſing to look upon; and if, by this agitation, any of the fixed

air

air be thrown over the ſide of the veſſel, the ſmoke, which is mixed with it, will fall to the ground, as if it was ſo much water, the fixed air being heavier than common air.

The red part of burning wood was extinguiſhed in this air, but I could not perceive that a red-hot poker was ſooner cooled in it.

Fixed air does not inſtantly mix with common air. Indeed, if it did, it could not be caught upon the fermenting liquor; for a candle put under a large receiver, and immediately plunged very deep below the ſurface of the fixed air, will burn ſome time. But veſſels with the ſmalleſt orifices, hanging with their mouths downwards in the fixed air, will in time have the common air, which they contain, perfectly mixed with it. When the fermenting liquor is contained in veſſels cloſe covered up, the fixed air is rendered much ſtronger, and then it readily affects the common air which is contiguous to it; ſo that, upon removing the cover, candles held at a conſiderable diſtance above the ſurface will inſtantly go out. I have been told by the workmen, that this will ſometimes be the caſe, when the candles are held more than half a yard above the mouth of the veſſel.

Fixed air unites with the ſmoke of reſin, ſulphur, and other electrical ſubſtances, as well as with the vapour of water; and yet, by holding the wire of a charged phial among theſe fumes, I could not make any electrical atmoſphere, which ſurprized me a good deal, as there was a large body of this ſmoke, and it was ſo confined, that it could not eſcape me. I alſo held ſome oil of vitriol in a glaſs veſſel within the

the fixed air, and by plunging a piece of red hot glaſs into it, raiſed a copious and thick fume. This floated upon the ſurface of the fixed air like other fumes, and continued as long.

Conſidering the near affinity between water and fixed air, I concluded that if a quantity of water was placed near the yeaſt of the fermenting liquor, it could not fail to imbibe that air, and thereby acquire the principal properties of Pyrmont, and other medicinal mineral waters. Accordingly, I found, that when the ſurface of the water was conſiderable, it always acquired the pleaſant acidulous taſte that Pyrmont water has. The readieſt way of impregnating water with this virtue, in theſe circumſtances, is to take two veſſels, and to keep pouring the water from one into the other, when they are both of them held as near the yeaſt as poſſible; for by this means a great quantity of ſurface is expoſed to the air, and the ſurface is alſo continually changing. In this manner, I have ſometimes, in the ſpace of two or three minutes, made a glaſs of exceedingly pleaſant ſparkling water, which could hardly be diſtinguiſhed from very good Pyrmont.

But the moſt effectual way of impregnating water with fixed air is to put the veſſels which contain the water into glaſs jars, filled with the pureſt fixed air, made by the ſolution of chalk in diluted oil of vitriol, ſtanding in quickſilver. In this manner I have, in about two days, made a quantity of water to imbibe more than an equal bulk of fixed air, ſo that, according to Dr. Brownrigg's experiments, it muſt have been much ſtronger than the beſt imported Pyrmont; for though he made his experiments at the ſpring head,

head, he never found that it contained quite ſo much as half its bulk of this air. If a ſufficient quantity of quickſilver cannot be procured, oil may be uſed with ſufficient advantage, for this purpoſe, as it imbibes the fixed air very ſlowly. Fixed air may be kept in veſſels ſtanding in water for a long time, if they be ſeparated by a partition of oil, about half an inch thick. Pyrmont water made in theſe circumſtances, is little or nothing inferior to that which has ſtood in quickſilver.

The *readieſt* method of preparing this water for uſe is to agitate it ſtrongly with its whole ſurface expoſed to the fixed air. By this means alſo, more than an equal bulk of air may be communicated to a large quantity of water in the ſpace of a few minutes. Eaſy directions for doing this I have publiſhed in a ſmall pamphlet, deſigned originally for the uſe of ſeamen in long voyages, on the preſumption that it might be of uſe for preventing or curing the ſea ſcurvy, equally with wort, which was recommended by Dr. Macbride for this purpoſe, on no other account than its property of generating fixed air, by its fermentation in the ſtomach.

Water thus impregnated with fixed air readily diſſolves iron, as Mr. Lane has diſcovered; ſo that if a quantity of iron filings be put to it, it preſently becomes a ſtrong chalybeate, and of the mildeſt and moſt agreeable kind.

I have recommended the uſe of chalk and oil of vitriol as the cheapeſt, and, upon the whole, the beſt materials for this purpoſe; and whereas ſome perſons had ſuſpected that a quantity of the oil of vitriol was rendered volatile by this proceſs, I examined it by

by all the chemical methods that are in uſe; but could not find that water thus impregnated contained the leaſt perceivable quantity of the acid.

Mr. Hey, indeed, who aſſiſted me in this examination, found that diſtilled water, impregnated with fixed air, did not mix ſo readily with ſoap as the diſtilled water itſelf; but this was alſo the caſe when the fixed air had paſſed through a long glaſs tube filled with alkaline ſalts, which, it may be ſuppoſed, would have imbibed any of the oil of vitriol that might have been contained in that air *.

It is not improbable but that fixed air itſelf may be of the nature of an acid, though of a weak and peculiar ſort. Mr. Bergman of Upſal, who honoured me with a letter upon the ſubject, calls it the aërial acid, and, among other experiments to prove it to be an acid, he ſays that it changes the blue juice of tourneſole into red.

The heat of boiling water will expell all the fixed air, if a phial containing the impregnated water be held in it; but it will often require above half an hour to do it completely.

Dr. Percival, who is particularly attentive to every improvement in the medical art, and who has thought ſo well of this impregnation as to preſcribe it in ſeveral caſes, informs me that it ſeems to be much ſtronger, and ſparkles more, like the true Pyrmont water, after it has been kept ſome time. This circumſtance, however, ſhews that, in time, the fixed air is more eaſily diſengaged from the water, and

* An account of Mr. Hey's experiments will be found in the Appendix to theſe papers.

though, in this ſtate, it may affect the taſte more ſenſibly, it cannot be of ſo much uſe in the ſtomach and bowels, as when the air is more firmly retained by the water, though, in conſequence of it, it be leſs ſenſible to the taſte.

By the proceſs deſcribed in my pamphlet, fixed air may be readily incorporated with wine, beer, and almoſt any other liquor whatever; and when beer, wine, or cyder, is become flat or dead (which is the conſequence of the eſcape of the fixed air they contained) they may be revived by this means; but the delicate and agreeable flavour, or acidulous taſte, communicated by fixed air, and which is very manifeſt in water, can hardly be perceived in wine, or any liquors which have much taſte of their own.

I ſhould think that there can be no doubt, but that water thus impregnated with fixed air muſt have all the medicinal virtues of genuine Pyrmont water; ſince theſe depend upon the fixed air it contains. If the genuine Pyrmont water derives any advantage from its being a natural chalybeate, this may alſo be obtained by providing a common chalybeate water, and uſing it in theſe proceſſes, inſtead of common water.

Having ſucceeded ſo well with this artificial Pyrmont water, I imagined that it might be poſſible to give ice the ſame virtue, eſpecially as cold is known to promote the abſorption of fixed air by water; but in this I found myſelf quite miſtaken. I put ſeveral pieces of ice into a quantity of fixed air, confined by quickſilver, but no part of the air was abſorbed in two days and two nights; but upon bringing it into a place where the ice melted, the air

was abſorbed as uſual. I then took a quantity of ſtrong artificial Pyrmont water, and, putting it into a thin glaſs phial, I ſet it in a pot that was filled with ſnow and ſalt. This mixture inſtantly freezing the water that was contiguous to the ſides of the glaſs, the air was diſcharged plentifully, ſo that I catched a conſiderable quantity, in a bladder tied to the mouth of the phial. I alſo took two quantities of the ſame Pyrmont water, and placed one of them where it might freeze, keeping the other in a cold place, but where it would not freeze. This retained its acidulous taſte, though the phial which contained it was not corked; whereas the other, being brought into the ſame place, where the ice melted very ſlowly, had at the ſame time the taſte of common water only. That quantity of water which had been frozen by the mixture of ſnow and ſalt, was almoſt as much like ſnow as ice, ſuch a quantity of air bubbles were contained in it, by which it was prodigiouſly increaſed in bulk.

The preſſure of the atmoſphere aſſiſts very conſiderably in keeping fixed air confined in water; for in an exhauſted receiver, Pyrmont water will abſolutely boil, by the copious diſcharge of its air. This is alſo the reaſon why beer and ale froth ſo much *in vacuo*. I do not doubt, therefore, but that, by the help of a condenſing engine, water might be much more highly impregnated with the virtues of the Pyrmont ſpring, and it would not be difficult to contrive a method of doing it.

The manner in which I made ſeveral experiments to aſcertain the abſorption of fixed air by different fluid ſubſtances was to put the liquid into a diſh,

and holding it within the body of the fixed air at the brewery, to ſet a glaſs veſſel into it, with its mouth inverted. This glaſs being neceſſarily filled with the fixed air, the liquor would riſe into it when they were both taken into the common air, if the fixed air was abſorbed at all.

Making uſe of ether in this manner, there was a conſtant bubbling from under the glaſs, occaſioned by this fluid eaſily riſing in vapour, ſo that I could not, in this method, determine whether it imbibed the air or not. I concluded, however, that they did incorporate, from a very diſagreeable circumſtance, which made me deſiſt from making any more experiments of the kind. For all the beer, over which this experiment was made, contracted a peculiar taſte, the fixed air impregnated with the ether being, I ſuppoſe, again abſorbed by the beer. I have alſo obſerved, that water which remained a long time within this air has ſometimes acquired a very diſagreeable taſte. At one time it was like tar-water. How this was acquired, I was very deſirous of making ſome experiments to aſcertain, but I was diſcouraged by the fear of injuring the fermenting liquor. It could not come from the fixed air only.

Having imagined that fixed air coagulated the blood in the lungs of animals, and thereby cauſed inſtant death; I ſuffocated a cat in this kind of air, and examining the lungs preſently after, found them collapſed and white, having little or no blood in them.

In order to try the effect of this air upon the blood itſelf, I took a quantity from a fowl juſt killed, and divided it into two parts, holding one of them within the

the fixed air, and the other in the common air, and obſerved that the former was coagulated much ſooner than the latter. This I could wiſh to have tried again.

Inſects and animals which breathe very little are ſtifled in fixed air, but are not ſoon quite killed in it. Butterflies, and flies of other kinds, will generally become torpid, and ſeemingly dead, after being held a few minutes over the fermenting liquor; but they revive again after being brought into the freſh air. But there are very great varieties with reſpect to the time in which different kinds of flies will either become torpid in the fixed air, or die in it. A large ſtrong frog was much ſwelled, and ſeemed to be nearly dead, after being held about ſix minutes over the fermenting liquor; but it recovered upon being brought into the common air. A ſnail treated in the ſame manner died preſently.

Fixed air is preſently fatal to vegetable life. At leaſt ſprigs of mint, growing in water, and placed over the fermenting liquor, will often become quite dead in one day, or even in a leſs ſpace of time; nor do they recover when they are afterwards brought into the common air. I am told, however, that ſome other plants are much more hardy in this reſpect.

A red roſe, freſh gathered, loſt its redneſs, and became of a purple colour, after being held over the fermenting liquor about twenty-four hours; but the tips of each leaf were much more affected than the reſt of it. Another red roſe turned perfectly white in this ſituation; but various other flowers, of different colours, were very little affected. Theſe experiments

riments were not repeated, as I wiſh they might be done, in pure fixed air, extracted from chalk by means of oil of vitriol.

For every purpoſe, in which it was neceſſary that the fixed air ſhould be as unmixed as poſſible, I generally made it by pouring oil of vitriol upon chalk and water, catching it in a bladder, faſtened to the neck of the phial, in which they were contained, taking care to preſs out all the common air, and alſo the firſt, and ſometimes the ſecond, produce of fixed air; and alſo, by agitation, making it as quickly as I poſſibly could. At other times, I made it paſs from the phial in which it was generated through a glaſs tube, without the intervention of any bladder, which, as I found by experience, will not long make a ſufficient ſeparation between ſeveral kinds of air and common air.

I had once thought that the readieſt method of procuring fixed air, and in ſufficient purity, would be by the ſimple proceſs of burning chalk, or pounded lime-ſtone in a gun-barrel, making it paſs through the ſtem of a tobacco-pipe, or a glaſs tube carefully luted to the orifice of it; and in this manner I find that air is produced in great plenty; but, upon examining it, I found, to my very great ſurprize, that little more than one half of it was fixed air, capable of being abſorbed by water; and that the reſt was inflammable, ſometimes very weakly, but ſometimes pretty highly ſo. Whence this inflammability proceeds, I am not able to determine, the lime or chalk not being ſuppoſed to contain any other than fixed air. I conjecture, however, that it muſt proceed from the iron, and the ſeparation of it from

from the calx may be promoted by that ſmall quantity of oil of vitriol, which I am informed is contained in chalk, if not in lime-ſtone alſo. But it is an objection to this hypotheſis, that the inflammable air produced in this manner burns blue, and not at all like that which is produced from iron, or any other metal, by means of an acid. It has alſo the ſmell of that kind of inflammable air which is produced from vegetable ſubſtances. Beſides, oil of vitriol without water, will not diſſolve iron; nor can inflammable air be got from it, unleſs the acid be conſiderably diluted; and when I mixed brimſtone with the chalk, neither the quality nor the quantity of the air was changed by it. Indeed no air, or permanently elaſtic vapour, can be got from brimſtone, or any oil.

In the method in which I generally made the fixed air, and indeed always, unleſs the contrary be particularly mentioned, *viz.* by diluted oil of vitriol and chalk, I found by experiment that it was as pure as Mr. Cavendiſh made it. For after it had paſſed through a large body of water in ſmall bubbles, ſtill $\frac{1}{50}$ or $\frac{1}{60}$ part only was not abſorbed by water. In order to try this as expeditiouſly as poſſible, I kept pouring the air from one glaſs veſſel into another, immerſed in a quantity of cold water, in which manner I found by experience, that almoſt any quantity may be reduced as far as poſſible in little more than a quarter of an hour.

At the ſame time that I was trying the purity of my fixed air, I had the curioſity to endeavour to aſcertain whether that part of it which is not miſcible in water, be equally diffuſed through the whole maſs;

maſs; and, for this purpoſe, I divided a quantity of about a gallon into three parts, the firſt conſiſting of that which was uppermoſt, and the laſt of that which was the loweſt, contiguous to the water; but all theſe parts were reduced in about an equal proportion, by paſſing through the water, ſo that the whole maſs had been of an uniform compoſition. This I have alſo found to be the caſe with ſeveral kinds of air, which will not properly incorporate.

A mouſe will live very well, though a candle will not burn, in the reſiduum of the pureſt fixed air that I can make; and I once made a very large quantity for the ſole purpoſe of this experiment. This, therefore, ſeems to be one inſtance of the generation of genuine common air, though vitiated in ſome degree. It is alſo another proof of the reſiduum of fixed air being, in part at leaſt, common air, that it becomes turbid, and is diminiſhed by the mixture of nitrous air, as will be explained hereafter.

That fixed air only wants ſome addition to make it permanent, and immiſcible with water, if not, in all reſpects, common air, I have been led to conclude, from ſeveral attempts which I once made to mix it with air, in which a quantity of iron filings and brimſtone, made into a paſte with water, had ſtood; for, in ſeveral mixtures of this kind, I imagined that not much more than half of the fixed air could be imbibed by water; but, not being able to repeat the experiment, I conclude that I either deceived myſelf in it, or that I overlooked ſome circumſtance on which the ſucceſs of it depended.

Theſe experiments, however, whether they were fallacious or otherwiſe, induced me to try whether any

any alteration would be made in the conſtitution of fixed air, by this mixture of iron filings and brimſtone. I therefore put a mixture of this kind into a quantity of as pure fixed air as I could make, and confined the whole in quickſilver, leſt the water ſhould abſorbe it before the effects of the mixture could take place. The conſequence was, that the fixed air was diminiſhed, and the quickſilver roſe in the veſſel, till about the fifth part was occupied by it; and, as near as I could judge, the proceſs went on, in all reſpects, as if the air in the inſide had been common air.

What is moſt remarkable, in the reſult of this experiment, is, that the fixed air, into which this mixture had been put, and which had been in part diminiſhed by it, was in part alſo rendered inſoluble in water by this means. I made this experiment four times, with the greateſt care, and obſerved, that in two of them about one ſixth, and in the other two about one fourteenth, of the original quantity, was ſuch as could not be abſorbed by water, but continued permanently elaſtic. Leſt I ſhould have made any miſtake with reſpect to the purity of the fixed air, the laſt time that I made the experiment, I ſet part of the fixed air, which I made uſe of, in a ſeparate veſſel, and found it to be exceedingly pure, ſo as to be almoſt wholly abſorbed by water; whereas the other part, to which I had put the mixture, was far from being ſo.

In one of theſe caſes, in which fixed air was made immiſcible with water, it appeared to be not very noxious to animals; but in another caſe, a mouſe died in it pretty ſoon.

As the iron is reduced to a calx by this proceſs, I once concluded, that it is phlogiſton that fixed air wants, to make it common air; and, for any thing I yet know, this may be the caſe, though I am ignorant of the method of combining them; and when I calcined a quantity of lead in fixed air, in the manner which will be deſcribed hereafter, it did not ſeem to have been leſs ſoluble in water than it was before.

II.

Of Air in which a candle, or brimstone, has burned out.

It is well known that flame cannot ſubſiſt long without change of air, ſo that the common air is neceſſary to it, except in the caſe of ſubſtances, into the compoſition of which nitre enters; for theſe will burn *in vacuo*, in fixed air, and even under water, as is evident in ſome rockets, which are made for this purpoſe. The quantity of air which even a ſmall flame requires to keep it burning is prodigious. It is generally ſaid, that an ordinary candle conſumes, as it is called, about a gallon in a minute. Conſidering this amazing conſumption of air, by fires of all kinds, volcano's, &c. it becomes a great object of philoſophical inquiry, to aſcertain what change is made in the conſtitution of the air by flame, and to diſcover what proviſion there is in nature for remedying the injury which the atmoſphere receives by this means. Some of the following experiments will, perhaps, be thought to throw a little light upon the ſubject.

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The diminution of the quantity of air in which a candle, or brimſtone, has burned out, is various; but I imagine that, at a medium, it may be about one fifteenth, or one ſixteenth, of the whole; about one third as much as by animals breathing it as long as they can, by animal or vegetable ſubſtances putrifying in it, by the calcination of metals, or by a mixture of ſteel filings and pounded brimſtone ſtanding in it.

I have ſometimes thought, that flame diſpoſes the common air to depoſit the fixed air it contains; for if any lime-water be expoſed to it, it immediately becomes turbid. This is the caſe, when wax candles, tallow candles, chips of wood, ſpirit of wine, æther, and every other ſubſtance which I have yet tried, except brimſtone, is burned in a cloſe glaſs veſſel, ſtanding in lime-water. This precipitation of fixed air (if this be the caſe) may be owing to ſomething emitted from the burning bodies, which has a ſtronger affinity with the other conſtituent parts of the atmoſphere.

If brimſtone be burned in the ſame circumſtances, the lime-water continues tranſparent, but ſtill there may have been the ſame precipitation of the fixed part of the air; but that, uniting with the lime and the vitriolic acid, it forms a ſelenetic ſalt, which is ſoluble in water. Having evaporated a quantity of water thus impregnated, by burning brimſtone a great number of times over it, a whitiſh powder remained, which had an acid taſte; but repeating the experiment with a quicker evaporation, the powder had no acidity, but was very much like chalk. The burning of brimſtone but once over a

quantity of lime-water, will affect it in ſuch a manner, that breathing into it will not make it turbid, which otherwiſe it always preſently does.

Dr. Hales ſuppoſed, that by burning brimſtone repeatedly in the ſame quantity of air, the diminution would continue without end. But this I have frequently tried, and not found to be the caſe. Indeed, when the ignition has been imperfect in the firſt inſtance, a ſecond firing of the ſame ſubſtance will increaſe the effect of the firſt, &c. but this progreſs ſoon ceaſes. In many caſes of the diminution of air, the effect is not immediately apparent, even when it ſtands in water; for ſometimes the bulk of air will not be much reduced, till it has paſſed ſeveral times through a quantity of water, which has thereby a better opportunity of abſorbing that fluid part of the air, which had not been perfectly detached from the reſt. I have ſometimes found a very great reduction of a maſs of air, in conſequence of paſſing but once thorough cold water. If the air has ſtood in quickſilver, the diminution is generally inconſiderable, till it has undergone this operation, there not being any ſubſtance expoſed to the air that could abſorb any part of it.

I could not find any conſiderable alteration in the ſpecific gravity of the air, in which candles, or brimſtone, had burned out. I am ſatisfied, however, that it is not heavier than common air, which muſt have been manifeſt, if ſo great a diminution of the quantity had been owing, as Dr. Hales and others ſuppoſed, to the elaſticity of the whole maſs being impaired. After making ſeveral trials for this purpoſe, I concluded that air, thus diminiſhed in bulk,

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is rather lighter than common air, which favours the ſuppoſition of the fixed, or heavier part of the common air, having been precipitated.

An animal will live nearly, if not quite as long, in air in which candles have burned out, as in common air. This fact ſurprized me very greatly, having imagined that what is called the conſumption of air by flame, or reſpiration, to have been of the ſame nature; but I have ſince found, that this fact has been obſerved by many perſons, and even ſo early as by Mr. Boyle. I have alſo obſerved, that air in which brimſtone has burned, is not in the leaſt injurious to animals, after the fumes, which at firſt make it very cloudy, have intirely ſubſided.

Having read, in the Memoirs of the Society at Turin, Vol. I. p. 41. that air in which candles had burned out was perfectly reſtored, ſo that other candles would burn in it again as well as ever, after having been expoſed to a conſiderable degree of cold, and likewiſe after having been compreſſed in bladders (for the cold had been ſuppoſed to have produced this effect by nothing but condenſation): I repeated theſe experiments, and did, indeed, find, that, when I compreſſed the air in bladders, as the Count de Saluce, who made the obſervation, had done, the experiment ſucceeded: but having had ſufficient reaſon to diſtruſt bladders, I compreſſed the air in a glaſs veſſel ſtanding in water; and then I found, that this proceſs is altogether ineffectual for the purpoſe. I kept the air compreſſed much more, and much longer, than he had done, but without producing any alteration in it. I alſo find, that a greater degree of cold than that which he applied, and

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of longer continuance, did by no means reſtore this kind of air: for when I have expoſed the phials which contained it a whole night, in which the froſt was very intenſe; and alſo when I kept it ſurrounded with a mixture of ſnow and ſalt, I found it, in all reſpects, the ſame as before.

It is alſo advanced, in the ſame Memoir, p. 41. that heat only, as the reverſe of cold, renders air unfit for candles burning in it. But I repeated the experiment of the Count for that purpoſe, without finding any ſuch effect from it. I alſo remember that, many years ago, I filled an exhauſted receiver with air, that had paſſed through a glaſs tube made red-hot, and found that a candle would burn in it perfectly well. Alſo, rarefaction by the air-pump does not injure air in the leaſt degree.

Though this experiment failed, I flatter myſelf that I have accidentally hit upon a method of reſtoring air which has been injured by the burning of candles, and that I have diſcovered at leaſt one of the reſtoratives which nature employs for this purpoſe. It is vegetation. In what manner this proceſs in nature operates, to produce ſo remarkable an effect, I do not pretend to have diſcovered; but a number of facts declare in favour of this hypotheſis. I ſhall introduce my account of them, by reciting ſome of the obſervations which I made on the growing of plants in confined air, which led to this diſcovery.

One might have imagined that, ſince common air is neceſſary to vegetable, as well as to animal life, both plants and animals had affected it in the ſame manner, and I own I had that expectation, when

when I firſt put a ſprig of mint into a glaſs-jar, ſtanding inverted in a veſſel of water; but when it had continued growing there for ſome months, I found that the air would neither extinguiſh a candle, nor was it at all inconvenient to a mouſe, which I put into it.

The plant was not affected any otherwiſe than was the neceſſary conſequence of its confined ſituation; for plants growing in ſeveral other kinds of air, were all affected in the very ſame manner. Every ſucceſſion of leaves was more diminiſhed in ſize than the preceding, till, at length, they came to be no bigger than the heads of pins. The root decayed, and the ſtalk alſo, beginning from the root; and yet the plant continued to grow upwards, drawing its nouriſhment through a black and rotten ſtem. In the third or fourth ſet of leaves, long hairy filaments grew from the inſertion of each leaf, and ſometimes from the body of the ſtem, ſhooting out as far as the veſſel in which it grew would permit, which, in my experiments, was about two inches. In this manner a ſprig of mint lived, the old ſtem decaying, and new ones ſhooting up in its place, but leſs and leſs continually, all the ſummer ſeaſon.

In repeating this experiment, care muſt be taken to draw away all the dead leaves from about the plant, leſt they ſhould putrefy, and affect the air. I have found that a freſh cabbage leaf, put under a glaſs veſſel filled with common air, for the ſpace of one night only, has ſo far affected the air, that a candle would not burn in it the next morning, and yet the leaf had not acquired any ſmell of putrefaction.

Finding

Finding that candles burn very well in air in which plants had grown a long time, and having had ſome reaſon to think, that there was ſomething attending vegetation, which reſtored air that had been injured by reſpiration, I thought it was poſſible that the ſame proceſs might alſo reſtore the air that had been injured by the burning of candles.

Accordingly, on the 17th of Auguſt, 1771, I put a ſprig of mint into a quantity of air, in which a wax candle had burned out, and found that, on the 27th of the ſame month, another candle burned perfectly well in it. This experiment I repeated, without the leaſt variation in the event, not leſs than eight or ten times in the remainder of the ſummer. Several times I divided the quantity of air in which the candle had burned out, into two parts, and putting the plant into one of them, left the other in the ſame expoſure, contained, alſo, in a glaſs veſſel immerſed in water, but without any plant; and never failed to find, that a candle would burn in the former, but not in the latter. I generally found that five or ſix days were ſufficient to reſtore this air, when the plant was in its vigour; whereas I have kept this kind of air in glaſs veſſels, immerſed in water many months, without being able to perceive that the leaſt alteration had been made in it. I have alſo tried a great variety of experiments upon it, as by condenſing, rarefying, expoſing to the light and heat, &c. and throwing into it the effluvia of many different ſubſtances, but without any effect.

Experiments made in the year 1772, abundantly confirmed my concluſion concerning the reſtoration of air, in which candles had burned out by plants growing

growing in it. The firſt of theſe experiments was made in the month of May; and they were frequently repeated in that and the two following months, without a ſingle failure.

For this purpoſe I uſed the flames of different ſubſtances, though I generally uſed wax or tallow candles. On the 24th of June the experiment ſucceeded perfectly well with air in which ſpirit of wine had burned out, and on the 27th of the ſame month it ſucceeded equally well with air in which brimſtone matches had burned out, an effect of which I had deſpaired the preceding year.

This reſtoration of air I found depended upon the vegetating ſtate of the plant; for though I kept a great number of the freſh leaves of mint in a ſmall quantity of air in which candles had burned out, and changed them frequently, for a long ſpace of time, I could perceive no melioration in the ſtate of the air.

This remarkable effect does not depend upon any thing peculiar to mint, which was the plant that I always made uſe of till July 1772; for on the 16th of that month, I found a quantity of this kind of air to be perfectly reſtored by ſprigs of balm, which had grown in it from the 7th of the ſame month.

That this reſtoration of air was not owing to any aromatic effluvia of theſe two plants, not only appeared by the eſſential oil of mint having no ſenſible effect of this kind; but from the equally complete reſtoration of this vitiated air by the plant called groundſel, which is uſually ranked among the weeds, and has an offenſive ſmell. This was the reſult of an experiment made the 16th of July, when the

plant had been growing in the burned air from the 8th of the ſame month. Beſides, the plant which I have found to be the moſt effectual of any that I have tried for this purpoſe is ſpinach, which is of quick growth, but will ſeldom thrive long in water. One jar of burned air was perfectly reſtored by this plant in four days, and another in two days. This laſt was obſerved on the 22d of July. In general this effect may be preſumed to have taken place in much leſs time than I have mentioned; becauſe I never choſe to make a trial of the air, till I was pretty ſure, from preceding obſervations, that the event which I had expected muſt have taken place, if it would ſucceed at all; leſt, returning back that part of the air on which I made the trial, and which would thereby neceſſarily receive a ſmall mixture of common air, the experiment might not be judged to be quite fair; though I myſelf might be ſufficiently ſatisfied with reſpect to the allowance that was to be made for that ſmall imperfection.

III.

Of inflammable Air.

I have generally made inflammable air in the manner deſcribed by Mr. Cavendiſh, in the Philoſophical Tranſactions, from iron, zinc, or tin; but chiefly from the two former metals, on account of the proceſs being the leaſt troubleſome: but when I extracted it from vegetable or animal ſubſtances, or from coals, I put them into a gun barrel, to the orifice of which I luted a glaſs tube, or the ſtem of a to-

a tobacco pipe, and to the end of this I tied a flaccid bladder, in order to catch the generated air.

There is not, I believe, any vegetable or animal ſubſtance whatever, nor any mineral ſubſtance, that is inflammable, but what will yield great plenty of inflammable air, when they are treated in this manner, and urged with a ſtrong heat; but, in order to get the moſt air, the heat muſt be applied as ſuddenly, and as vehemently, as poſſible. For, notwithſtanding the ſame care be taken in luting, and in every other reſpect, ſix or even ten times more air may be got by a ſudden heat than by a ſlow one, though the heat that is laſt applied be as intenſe as that which was applied ſuddenly. A bit of dry oak, weighing about twelve grains, will generally yield about a ſheep's bladder full of inflammable air with a briſk heat, when it will only give about two or three ounce meaſures if the ſame heat be applied to it very gradually. To what this difference is owing, I cannot tell.

Inflammable air, when it is made by a quick proceſs, has a very ſtrong and offenſive ſmell, from whatever ſubſtance it be generated; but this ſmell is of three different kinds, according as the air is extracted from mineral, vegetable, or animal ſubſtances. The laſt is exceedingly fetid; and it makes no difference, whether it be extracted from a bone, or even an old and dry tooth, or from ſoft muſcular fleſh, or any other part of the animal. The burning of any ſubſtance occaſions the ſame ſmell: for the groſs fume which ariſes from them, before they flame, is the inflammable air they contain, which is expelled by heat, and then readily ignited. The ſmell of in-

ſlammable air is the very ſame, as far as I am able to perceive, from whatever ſubſtance of the ſame kingdom it be extracted. Thus it makes no difference whether it be got from iron, zinc, or tin, from any kind of wood, or, as was obſerved before, from any part of an animal.

If a quantity of inflammable air be contained in a glaſs veſſel ſtanding in water, and have been generated very faſt, it will ſmell even through the water, and this water will alſo ſoon become covered with a thin film, aſſuming all the different colours. If the inflammable air have been generated from iron, this matter will appear to be a red okre, or the earth of iron, as I have found by collecting a conſiderable quantity of it; and if it have been generated from zinc, it is a whitiſh ſubſtance, which I ſuppoſe to be the calx of the metal. It likewiſe ſettles to the bottom of the veſſel, and when the water is ſtirred, it has very much the appearance of wool. When water is once impregnated in this manner, it will continue to yield this ſcum for a conſiderable time after the air is removed from it. This I have often obſerved with reſpect to iron.

Inflammable air, made by a violent efferveſcence, I have obſerved to be much more inflammable than that which is made by a weak efferveſcence, whether the water or the oil of vitriol prevailed in the mixture. Alſo the offenſive ſmell was much ſtronger in the former caſe than in the latter. The greater degree of inflammability appeared by the greater number of ſucceſſive exploſions, when a candle was preſented to the neck of a phial filled with it. It is poſſible, however, that this diminution of inflammability

ſlammability may, in ſome meaſure, ariſe from the air continuing ſo much longer in the bladder when it is made very ſlowly; though I think the difference is too great for this cauſe to have produced the whole of it. It may, perhaps, deſerve to be tried by a different proceſs, without a bladder.

Inflammable air is not thought to be miſcible with water, and when kept many months, ſeems, in general, to be as inflammable as ever. Indeed, when it is extracted from vegetable or animal ſubſtances, a part of it will be imbibed by the water in which it ſtands; but it may be preſumed, that in this caſe, there was a mixture of fixed air extracted from the ſubſtance along with it. I have indiſputable evidence, however, that inflammable air, ſtanding long in water, has actually loſt all its inflammability, and even come to extinguiſh flame much more than that air in which candles have burned out. After this change it appears to be greatly diminiſhed in quantity, and it ſtill continues to kill animals the moment they are put into it.

This very remarkable fact firſt occurred to my obſervation on the twenty-fifth of May 1771, when I was examining a quantity of inflammable air, which had been made from zinc, near three years before. Upon this, I immediately ſet by a common quart bottle filled with inflammable air from iron, and another equal quantity from zinc; and examining them in the beginning of December following, that from the iron was reduced near one half in quantity, if I be not greatly miſtaken; for I found the bottle half full of water, and I am pretty clear that it was full of air when it was ſet by. That which had been

been produced from zinc was not altered, and filled the bottle as at firſt.

Another inſtance of this kind occurred to my obſervation on the 19th of June 1772, when a quantity of air, half of which had been inflammable air from zinc, and half air in which mice had died, and which had been put together the 30th of July 1771, appeared not to be in the leaſt inflammable, but extinguiſhed flame, as much as any kind of air that I had ever tried. I think that, in all, I have had four inſtances of inflammable air loſing its inflammability, while it ſtood in water.

Though air tainted with putrefaction extinguiſhes flame, I have not found that animals or vegetables putrefying in inflammable air render it leſs inflammable. But one quantity of inflammable air, which I had ſet by in May 1771, along with the others above mentioned, had had ſome putrid fleſh in it; and this air had loſt its inflammability, when it was examined at the ſame time with the other in the December following. The bottle in which this air had been kept, ſmelled exactly like very ſtrong Harrowgate water. I do not think that any perſon could have diſtinguiſhed them.

I have made plants grow for ſeveral months in inflammable air made from zinc, and alſo from oak; but, though the plants grew pretty well, the air ſtill continued inflammable. The former, indeed, was not ſo highly inflammable as when it was freſh made, but the latter was quite as much ſo; and the diminution of inflammability in the former caſe, I attribute to ſome other cauſe than the growth of the plant.

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No kind of air, on which I have yet made the experiment, will conduct electricity; but the colour of a ſpark is remarkably different in ſome different kinds of air, which ſeems to ſhew that they are not equally good non-conductors. In fixed air, the electric ſpark is exceedingly white; but in inflammable air it is of a purple, or red colour. Now, ſince the moſt vigorous ſparks are always the whiteſt, and, in other caſes, when the ſpark is red, there is reaſon to think that the electric matter paſſes with difficulty, and with leſs rapidity: it is poſſible that the inflammable air may contain particles which conduct electricity, though very imperfectly; and that the whiteneſs of the ſpark in the fixed air, may be owing to its meeting with no conducting particles at all. When an exploſion was made in a quantity of inflammable air, it was a little white in the center, but the edges of it were ſtill tinged with a beautiful purple. The degree of whiteneſs in this caſe was probably owing to the electric matter ruſhing with more violence in an exploſion than in a common ſpark.

Inflammable air kills animals as ſuddenly as fixed air, and, as far as can be perceived, in the ſame manner, throwing them into convulſions, and thereby occaſioning preſent death. I had imagined that, by animals dying in a quantity of inflammable air, it would in time become leſs noxious; but this did not appear to be the caſe; for I killed a great number of mice in a ſmall quantity of this air, which I kept ſeveral months for this purpoſe, without its being at all ſenſibly mended; the laſt, as well as the firſt mouſe, dying the moment it was put into it.

I once imagined that, ſince fixed and inflammable air are the reverſe of one another, in ſeveral remarkable properties, a mixture of them would make common air; and while I made the mixtures in bladders, I imagined that I had ſucceeded in my attempt; but I have ſince found that thin bladders do not ſufficiently prevent the air that is contained in them from mixing with the external air. Alſo corks will not ſufficiently confine different kinds of air, unleſs the phials in which they are confined be ſet with their mouths downwards, and a little water lie in the necks of them, which, indeed, is equivalent to the air ſtanding in veſſels immerſed in water. In this manner, however, I have kept different kinds of air for ſeveral years.

Whatever methods I took to promote the mixture of fixed and inflammable air, they were all ineffectual. I think it my duty, however, to recite the iſſue of an experiment or two of this kind, in which equal mixtures of theſe two kinds of air had ſtood near three years, as they ſeem to ſhew that they had in part affected one another, in that long ſpace of time. Theſe mixtures I examined April 27, 1771. One of them had ſtood in quickſilver, and the other in a corked phial, with a little water in it. On opening the latter in water, the water inſtantly ruſhed in, and filled almoſt half of the phial, and very little more was abſorbed afterwards. In this caſe the water in the phial had probably abſorbed a conſiderable part of the fixed air, ſo that the inflammable air was exceedingly rarefied; and yet the whole quantity that muſt have been rendered non-elaſtic was ten times more than the bulk of the water, and it has not

not been found that water can contain much more than its own bulk of fixed air. But in other caſes I have found the diminution of a quantity of air, and eſpecially of fixed air, to be much greater than I could well account for by any kind of abſorption.

The phial which had ſtood immerſed in quickſilver had loſt very little of its original quantity; and being now opened in water, and left there, along with a another phial, which was juſt then filled, as this had been three years before, with air half inflammable and half fixed, I obſerved that the quantity of both was diminiſhed, by the abſorption of the water, in the ſame proportion.

Upon applying a candle to the mouths of the phials which had been kept three years, that which had ſtood in quickſilver went off at one exploſion, exactly as it would have done if there had been a mixture of common air, with the inflammable. As a good deal depends upon the apertures of the veſſels in which the inflammable air is fixed, I mixed the two kinds of air in equal proportion in the ſame phial, and after letting it ſtand ſome days in water, that the fixed air might be abſorbed, I applied a candle to it; but it made ten or twelve exploſions (ſtopping the phial after each of them) before the inflammable matter was exhauſted.

The air which had been confined in the corked phial exploded in the very ſame manner as an equal mixture of the two kinds of air in the ſame phial, the experiment being made as ſoon as the fixed air was abſorbed, as before; ſo that, in this caſe, the two kinds of air did not ſeem to have affected one another at all.

Conſidering inflammable air as air united to or loaded with phlogiſton, I expoſed to it ſeveral ſubſtances, which are ſaid to have a near affinity with phlogiſton, as oil of vitriol, and ſpirit of nitre (the former for above a month), but without making any ſenſible alteration in it.

I obſerved, however, that inflammable air, mixed with the fumes of ſmoaking ſpirit of nitre, goes off at one exploſion, exactly like a mixture of half common and half inflammable air. This I tried ſeveral times, by throwing the inflammable air into a phial full of ſpirit of nitre, with its mouth immerſed in a baſon containing ſome of the ſame ſpirit, and then applying the flame of a candle to the mouth of the phial, the moment that it was uncovered, after it had been taken out of the baſon. This remarkable effect I haſtily concluded to have ariſen from the inflammable air having been in part deprived of its inflammability, by means of the ſtronger affinity, which the ſpirit of nitre had with phlogiſton, and therefore I imagined that by letting them ſtand longer in contact, and eſpecially by agitating them ſtrongly together, I ſhould deprive the air of all its inflammability; but neither of theſe operations ſucceeded, for ſtill the air was only exploded at once, as before. And laſtly, when I paſſed a quantity of inflammable air, which had been mixed with the fumes of ſpirit of nitre, through a body of water, and received it in another veſſel, it appeared not to have undergone any change at all, for it went off in ſeveral ſucceſſive exploſions, like the pureſt inflammable air. The effect abovementioned muſt, therefore, have been owing to the fumes of the ſpirit of nitre ſupplying the

the place of common air for the purpoſe of ignition, which is analogous to other experiments with nitre.

Having had the curioſity, on the 25th of July 1772, to expoſe a great variety of different kinds of air to water out of which the air it contained had been boiled, without any particular view; the reſult was, in ſeveral reſpects, altogether unexpected, and led to a variety of new obſervations on the properties and affinities of ſeveral kinds of air with reſpect to water. Among the reſt three fourths of that which was inflammable was abſorbed by the water in about two days, and the remainder was inflammable, but weakly ſo.

Upon this, I began to agitate a quantity of ſtrong inflammable air in a glaſs jar, ſtanding in a pretty large trough of water, the ſurface of which was expoſed to the common air, and I found that when I had continued the operation about ten minutes, near one fourth of the quantity of air had diſappeared; and finding that the remainder made an efferveſcence with nitrous air, I concluded that it muſt have become fit for reſpiration, whereas this kind of air is, at the firſt, as noxious as any other kind whatever. To aſcertain this, I put a mouſe into a veſſel containing 2½ ounce meaſures of it, and obſerved that it lived in it twenty minutes, which is as long as a mouſe will generally live in the ſame quantity of common air. This mouſe was even taken out alive, and recovered very well. Still alſo the air in which it had breathed ſo long was inflammable, though very weakly ſo. I have even found it to be ſo when a mouſe has actually died in it.

Inflammable air thus diminiſhed by agitation in water, makes but one exploſion on the approach of a candle exactly like a mixture of inflammable air with common air.

From this experiment I concluded that, by continuing the ſame proceſs, I ſhould deprive inflammable air of all its inflammability, and this I found to be the caſe; for, after a longer agitation, it admitted a candle to burn in it, like common air, only more faintly; and indeed by the teſt of nitrous air it did not appear to be near ſo good as common air. Continuing the ſame proceſs ſtill farther, the air which had been moſt ſtrongly inflammable a little before, came to extinguiſh a candle, exactly like air in which a candle had burned out, nor could they be diſtinguiſhed by the teſt of nitrous air.

I found, by repeated trials, that it was difficult to catch the time in which inflammable air obtained from metals, in coming to extinguiſh flame, was in the ſtate of common air, ſo that the tranſition from the one to the other muſt be very ſhort. I readily, however, found this ſtate in a quantity of inflammable air extracted from oak, which air I had kept by me a year, and in which a plant had grown, though very poorly, for ſome part of the time. A quantity of this air, after being agitated in water till it was diminiſhed about one half, admitted a candle to burn in it exceedingly well, and was even hardly to be diſtinguiſhed from common air by the teſt of nitrous air.

I took ſome pains to aſcertain the quantity of diminution, in freſh made and very highly inflammable air from iron, at which it ceaſed to be inflammable,

mable, and, upon the whole, I concluded that it was ſo when it was diminiſhed a little more than one half: for a quantity which was diminiſhed exactly one half had ſomething inflammable in it, but in the ſlighteſt degree imaginable.

Finding that water would imbibe inflammable air, I endeavoured to impregnate water with it, by the ſame proceſs by which I had made water imbibe fixed air; but though I found that diſtilled water would imbibe about one fourteenth of its bulk of inflammable air, I could not perceive that the taſte of it was ſenſibly altered.

IV.

Of Air infected with animal respiration, or putrefaction.

That candles will burn only a certain time, is a fact not better known, than it is that animals can live only a certain time, in a given quantity of air; but the cauſe of the death of the animal is not better known than that of the extinction of flame in the ſame circumſtances; and when once any quantity of air has been rendered noxious by animals breathing in it as long as they could, I do not know that any methods have been diſcovered of rendering it fit for breathing again. It is evident, however, that there muſt be ſome proviſion in nature for this purpoſe, as well as for that of rendering the air fit for ſuſtaining flame; for without it the whole maſs of the atmoſphere would, in time, become unfit for the purpoſe of animal life; and yet there is no reaſon to think that it is, at preſent, at all leſs fit for reſpiration than

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it has ever been. I flatter myſelf, however, that I have hit upon two of the methods employed by nature for this great purpoſe. How many others there may be, I cannot tell.

When animals die upon being put into air in which other animals have died, after breathing in it as long as they could, it is plain that the cauſe of their death is not the want of any *pabulum vitæ*, which has been ſuppoſed to be contained in the air, but on account of the air being impregnated with ſomething ſtimulating to their lungs; for they almoſt always die in convulſions, and are ſometimes affected ſo ſuddenly, that they are irrecoverable after a ſingle inſpiration, though they be withdrawn immediately, and every method has been taken to bring them to life again. They are affected in the ſame manner, when they are killed in any other kind of noxious air that I have tried, viz. fixed air, inflammable air, air filled with the fumes of brimſtone, infected with putrid matter, in which a mixture of iron filings and brimſtone has ſtood, or in which charcoal has been burned, or metals calcined, or in nitrous air, &c.

If a mouſe (which is an animal that I have commonly made uſe of for the purpoſe of theſe experiments) can ſtand the firſt ſhock of this ſtimulus, or has been habituated to it by degrees, it will live a conſiderable time in air in which other mice will die inſtantaneouſly. I have frequently found that when a number of mice have been confined in a given quantity of air, leſs than half the time that they have actually lived in it, a freſh mouſe has been inſtantly thrown into convulſions, and died upon being put to them. It is evident, therefore, that if the

the experiment of the Black Hole were to be repeated, a man would ſtand the better chance of ſurviving it, who ſhould enter at the firſt, than at the laſt hour. I have alſo obſerved, that young mice will always live much longer than old ones, or than thoſe which are full grown, when they are confined in the ſame quantity of air. I have ſometimes known a young mouſe to live ſix hours in the ſame circumſtances in which an old mouſe has not lived one. On theſe accounts, experiments with mice, and, for the ſame reaſon, no doubt, with other animals alſo, have a conſiderable degree of uncertainty attending them; and therefore, it is neceſſary to repeat them frequently, before the reſult can be abſolutely depended upon.

The diſcovery of the proviſion in nature for reſtoring air, which has been injured by the reſpiration of animals, having long appeared to me to be one of the moſt important problems in natural philoſophy, I have tried a great variety of ſchemes in order to effect it. In theſe, my guide has generally been to conſider the influences to which the atmoſphere is, in fact, expoſed; and, as ſome of my unſucceſsful trials may be of uſe to thoſe who are diſpoſed to take pains in the farther inveſtigation of this ſubject, I ſhall mention the principal of them.

The noxious effluvium with which air is loaded by animal reſpiration, is not abſorbed by ſtanding without agitation in freſh or ſalt water. I have kept it many months in freſh water, when, inſtead of being meliorated, it has ſeemed to become even more deadly, ſo as to require more time to reſtore it, by the methods which will be explained hereafter, than

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air which has been lately made noxious. I have even ſpent ſeveral hours in pouring this air from one glaſs veſſel into another, in water, ſometimes as cold, and ſometimes as warm, as my hands could bear it, and have ſometimes alſo wiped the veſſels many times, during the courſe of the experiment, in order to take off that part of the noxious matter, which might adhere to the glaſs veſſels, and which evidently gave them an offenſive ſmell; but all theſe methods were generally without any ſenſible effect. The motion, alſo, which the air received in theſe circumſtances, it is very evident, was of no uſe for this purpoſe.

This kind of air is not reſtored by being expoſed to the light, or by any other influence to which it is expoſed, when confined in a thin phial, in the open air, for ſome months.

Among other experiments, I tried a great variety of different effluvia, which are continually exhaling into the air, eſpecially of thoſe ſubſtances which are known to reſiſt putrefaction; but I could not by theſe means effect any melioration of the noxious quality of this kind of air.

Having read, in the Memoirs of the Imperial Society, of a plague not afflicting a particular village, in which there was a large ſulphur work, I immediately fumigated a quantity of this kind of air; or (which will hereafter appear to be the very ſame thing) air tainted with putrefaction, with the fumes of burning brimſtone, but without any effect.

I once imagined, that the nitrous acid in the air might be the general reſtorative which I was in queſt of; and the conjecture was favoured, by finding

ing that candles would burn, and animals live, in air extracted from ſaltpetre. I therefore ſpent a good deal of time in attempting, by a burning-glaſs, and other means, to impregnate this noxious air with ſome effluvium of ſaltpetre, and, with the ſame view, introduced into it the fumes of the ſmoaking ſpirit of nitre; but both theſe methods were altogether ineffectual.

In order to try the effect of heat, I put a quantity of air, in which mice had died, into a bladder, tied to the end of the ſtem of a tobacco-pipe, at the other end of which was another bladder, out of which the air was carefully preſſed. I then put the middle part of the ſtem into a chafing-diſh of hot coals, ſtrongly urged with a pair of bellows; and, preſſing the bladders alternately, I made the air paſs ſeveral times through the heated part of the pipe. I have alſo made this kind of air very hot, ſtanding in water before the fire. But neither of theſe methods were of any uſe.

Rarefaction and condenſation by inſtruments were alſo tried, but in vain.

Thinking it poſſible that the earth might imbibe the noxious quality of the air, and thence ſupply the roots of plants with ſuch putreſcent matter as is known to be nutritive to them, I kept a quantity of air, in which mice had died, in a phial, one half of which was filled with fine garden mould; but, though it ſtood two months in theſe circumſtances, it was not the better for it.

I once imagined that, ſince ſeveral kinds of air cannot be long ſeparated from common air, by being confined in bladders, in bottles well corked, or even

cloſed with ground ſtopples, the affinity between this noxious air and the common air might be ſo great, that they would mix through a body of water interpoſed between them; the water continually receiving from the one, and giving to the other, eſpecially as water receives ſome kinds of impregnation from, I believe, every kind of air to which it is contiguous; but I have ſeen no reaſon to conclude, that a mixture of any kind of air with the common air can be produced in this manner. I have kept air in which mice have died, air in which candles have burned out, and inflammable air, ſeparated from the common air, by the ſlighteſt partition of water that I could well make, ſo that it might not evaporate in a day or two, if I ſhould happen not to attend to them; but I found no change in them after a month or ſix weeks. The inflammable air was ſtill inflammable, mice died inſtantly in the air in which other mice had died before, and candles would not burn where they had burned out before.

Since air tainted with animal or vegetable putrefaction is the ſame thing with air rendered noxious by animal reſpiration, I ſhall now recite the obſervations which I have made upon this kind of air, before I treat of the method of reſtoring them.

That theſe two kinds of air are, in fact, the ſame thing, I conclude from their having ſeveral remarkable common properties, and from their differing in nothing that I have been able to obſerve. They equally extinguiſh flame, they are equally noxious to animals, they are equally, and in the ſame way, offenſive to the ſmell, they are equally diminiſhed

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in their quantity, they equally precipitate in lime-water, and they are reſtored by the ſame means.

Since air which has paſſed through the lungs is the ſame thing with air tainted with animal putrefaction, it is probable that one uſe of the lungs is to carry off a putrid effluvium, without which, perhaps, a living body might putrefy as ſoon as a dead one.

When a mouſe putrefies in any given quantity of air, the bulk of it is generally increaſed for a few days; but in a few days more it begins to ſhrink up, and generally, in about eight or ten days, if the weather be pretty warm, it will be found to be diminiſhed $\frac{1}{6}$, or $\frac{1}{5}$ of its bulk. If it do not appear to be diminiſhed after this time, it only requires to be paſſed through water, and the diminution will not fail to be ſenſible. I have ſometimes known almoſt the whole diminution to take place, upon once or twice paſſing through the water. The ſame is the caſe with air, in which animals have breathed as long as they could. Alſo, air in which candles have burned out may almoſt always be farther reduced by this means. All theſe proceſſes, as I obſerved before, ſeem to diſpoſe the compound maſs of air to part with ſome conſtituent part belonging to it; and this being miſcible with water, muſt be brought into contact with it, in order to mix with it to the moſt advantage, eſpecially when its union with the other conſtituent principles of the air is but partially broken.

I have put mice into veſſels which had their mouths immerſed in quickſilver, and obſerved that the air was not much contracted after they were dead or cold; but upon withdrawing the mice, and admitting

lime-water to the air it immediately became turbid, and was contracted in its dimenſions as uſual.

I tried the ſame thing with air tainted with putrefaction, putting a dead mouſe to a quantity of common air, in a veſſel which had its mouth immerſed in quickſilver, and after a week I took the mouſe out, drawing it through the quickſilver, and obſerved that for ſome time there was an apparent increaſe of the air perhaps about $\frac{1}{20}$. After this, it ſtood two days in the quickſilver, without any ſenſible alteration; and then admitting water to it, it began to be abſorbed, and continued ſo, till the original quantity was diminiſhed about $\frac{1}{6}$. If, inſtead of common water, I had made uſe of lime water in this experiment, I make no doubt but it would have become turbid.

If a quantity of lime-water in a phial be put under a glaſs veſſel ſtanding in water, it will not become turbid, and provided the acceſs of the common air be prevented, it will continue lime-water, I do not know how long; but if a mouſe be left to putrefy in the veſſel, the water will depoſit all its lime in a few days. This may be owing to the fixed air being transferred from the putrid mouſe into the water, and yet it is evident that there is a putrid effluvium intirely diſtinct from this kind of air, and which has very different properties.

It is a doubt with me, however, whether the putrid effluvium be not chiefly fixed air, with the addition of ſome other effluvium, which has the power of diminiſhing common air. The reſemblance between the true putrid effluvium and fixed air in the following experiment, which is as deciſive as

as I can poſſibly contrive it, appeared to be very great; indeed, much greater than I had expected. I put a dead mouſe into a tall glaſs veſſel, and having filled the remainder with quickſilver, and ſet it, inverted, in a pot of quickſilver, I let it ſtand about two months, in which time the putrid effluvium iſſuing from the mouſe had filled the whole veſſel, and part of the diſſolved blood, which lodged upon the ſurface of the quikſilver, began to be thrown out. I then filled another glaſs veſſel, of the ſame ſize and ſhape, with as pure fixed air as I could make, and expoſed them both, at the ſame time, to a quantity of lime-water. In both caſes the water grew turbid alike, it roſe equally faſt in both the veſſels, and likewiſe equally high; ſo that about the ſame quantity remained unabſorbed by the water. One of theſe kinds of air, however, was exceedingly ſweet and pleaſant, and the other inſufferably offenſive; one of them alſo would have made an addition to any quantity of common air with which it had been mixed, and the other would have diminiſhed it. This, at leaſt, would have been the conſequence, if the mouſe itſelf had putrefied in any quantity of air.

It ſeems to depend, in ſome meaſure, upon the time, and other circumſtances, in the diſſolution of animal or vegetable ſubſtances, whether they yield the proper putrid effluvium, or fixed, or inflammable air; but the experiments which I have made upon this ſubject, have not been numerous enough to enable me to decide with certainty concerning thoſe circumſtances. Putrid cabbage, green, or boiled, infects the air in the very ſame manner as putrid animal ſubſtances. Air thus tainted is equally contracted

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in its dimenſions, it equally extinguiſhes flame, and is equally noxious to animals; but they affect the air very differently if the heat that is applied to them be conſiderable. If beef or mutton, raw, or boiled, be placed ſo near to the fire, that the heat to which it is expoſed ſhall equal, or rather exceed, that of the blood, a conſiderable quantity of air will be generated in a day or two, about $\frac{1}{7}$th of which I have generally found to be abſorbed by water, while all the reſt was inflammable; but air generated from vegetables, in the ſame circumſtances, will be almoſt all fixed, and no part of it inflammable. This I have repeated again and again, the whole proceſs being in quickſilver; ſo that neither common air, nor water, had any acceſs to the ſubſtance on which the experiment was made; and the generation of air, or effluvium of any kind, except what might be abſorbed by quickſilver, or reſorbed by the ſubſtance itſelf, might be diſtinctly noted.

A vegetable ſubſtance, after ſtanding a day or two in theſe circumſtances, will yield nearly all the air that can be extracted from it, in that degree of heat; whereas an animal ſubſtance will continue to give more air or effluvium, of ſome kind or other, with very little alteration, for many weeks. It is remarkable, however, that though a piece of beef or mutton, plunged in quickſilver, and kept in this degree of heat, yield air, the bulk of which is inflammable, and contracts no putrid ſmell (at leaſt, in a day or two), a mouſe treated in the ſame manner, yields the proper putrid effluvium, as, indeed the ſmell ſufficiently indicates; and this effluvium does either

either itſelf extinguiſh flame, or has in it ſuch a mixture of fixed air, as to give it that property.

That the putrid effluvium will mix with water ſeems to be evident from the following experiment. If a mouſe be put into a jar full of water, ſtanding with its mouth inverted in another veſſel of water, a conſiderable quantity of elaſtic matter (and which may, therefore, be called air) will ſoon be generated, unleſs the weather be ſo cold as to check all putrefaction. After a ſhort time, the water contracts an extremely fetid and offenſive ſmell, which ſeems to indicate that the putrid effluvium pervades the water, and affects the neighbouring air; and ſince, after this, there is often no increaſe of the air, that ſeems to be the very ſubſtance which is carried off through the water, as faſt as it is generated; and the offenſive ſmell is a ſufficient proof that it is not fixed air. For this has a very agreeable flavour, whether it be produced by fermentation, or extracted from chalk by oil of vitriol; affecting not only the mouth, but even the noſtrils, with a pungency which is peculiarly pleaſing to a certain degree, as any perſon may eaſily ſatisfy himſelf who will chuſe to make the experiment. If the water in which the mouſe was immerſed, and which is ſaturated with the putrid air, be changed, the greater part of the putrid air will, in a day or two, be abſorbed, though the mouſe continues to yield the putrid effluvium as before; for as ſoon as this freſh water becomes ſaturated with it, it begins to be offenſive to the ſmell, and the quantity of the putrid air upon its ſurface increaſes as before. I kept a mouſe producing putrid air in this manner for the ſpace of ſeveral months.

Six ounce meaſures of air not readily abſorbed by water, appeared to have been generated from one mouſe, which had been putrefying eleven days in confined air, before it was put into a jar which was quite filled with water, for the purpoſe of this obſervation.

Air thus generated from putrid mice ſtanding in water, without any mixture of common air, extinguiſhes flame, and is noxious to animals, but not more ſo than common air only tainted with putrefaction. It is exceedingly difficult and tedious to collect a quantity of this putrid air, not miſcible in water, ſo very great a proportion of what is collected being abſorbed by the water, in which it is kept; but what that proportion is, I have not endeavoured to aſcertain.

Though a quantity of air be diminiſhed by any ſubſtance putrefying in it, I have not yet found the ſame effect to be produced by a mixture of putrid air with common air; but, in the manner in which I have hitherto made the experiment, I was obliged to let the putrid air, paſs through a body of water; which might inſtantly abſorb whatever it was in the putrid ſubſtance, that diminiſhed the common air.

Inſects of various kinds live perfectly well in air tainted with animal or vegetable putrefaction, when a ſingle inſpiration of it would have inſtantly killed any animal. I have frequently tried the experiment with flies and butterflies. I have alſo obſerved, that the *aphides* will thrive as well upon plants growing in this kind of air, as in the open air. I have even been frequently obliged to take plants out of the putrid air in which they were growing, on purpoſe to bruſh away the ſwarms of

theſe inſects which infected them; and yet ſo effectually did ſome of them conceal themſelves, and ſo faſt did they multiply, in theſe circumſtances, that I could ſeldom keep the plants quite clear of them.

When air has been freſhly and ſtrongly tainted with putrefaction, ſo as to ſmell through the water, ſprigs of mint have preſently died, upon being put into it, their leaves turning black; but if they do not die preſently, they thrive in a moſt ſurprizing manner. In no other circumſtances have I ever ſeen vegetation ſo vigorous as in this kind of air, which is immediately fatal to animal life. Though theſe plants have been crouded in jars filled with this air, every leaf has been full of life; freſh ſhoots have branched out in various directions, and have grown much faſter than other ſimilar plants, growing in the ſame expoſure in common air.

This obſervation led me to conclude, that plants, inſtead of affecting the air in the ſame manner with animal reſpiration, reverſe the effects of breathing, and tend to keep the atmoſphere ſweet and wholeſome, when it is become noxious, in conſequence of animals living and breathing, or dying and putrefying in it.

In order to aſcertain this, I took a quantity of air, made thoroughly noxious, by mice breathing and dying in it, and divided it into two parts; one of which I put into a phial immerſed in water; and to the other (which was contained in a glaſs jar, ſtanding in water) I put a ſprig of mint. This was about the beginning of Auguſt 1771, and after eight or nine days, I found that a mouſe lived perfectly well

in that part of the air, in which the ſprig of mint had grown, but died the moment it was put into the other part of the ſame original quantity of air; and which I had kept in the very ſame expoſure, but without any plant growing in it.

This experiment I have ſeveral times repeated; ſometimes uſing air, in which animals had breathed and died; ſometimes uſing air tainted with vegetable or animal putrefaction, and generally with the ſame ſucceſs.

Once, I let a mouſe live and die in a quantity of air, which had been noxious, but which had been reſtored by this proceſs, and it lived nearly as long as I conjectured it might have done in an equal quantity of freſh air; but, this is ſo exceedingly various, that it is not eaſy to form any judgment from it; and in this caſe the ſymptom of *difficult reſpiration* ſeemed to begin earlier than it would have done in common air.

Since the plants that I made uſe of manifeſtly grow and thrive in putrid air; ſince putrid matter is well known to afford proper nouriſhment for the roots of plants; and ſince it is likewiſe certain that they receive nouriſhment by their leaves as well as by their roots, it ſeems to be exceedingly probable, that the putrid effluvium is in ſome meaſure extracted from the air, by means of the leaves of plants, and therefore that they render the remainder more fit for reſpiration.

Towards the end of the year ſome experiments of this kind did not anſwer ſo well as they had done before, and I had inſtances of the relapſing of this reſtored air to its former noxious ſtate. I therefore

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ſuſpended my judgment concerning the efficacy of plants to reſtore this kind of noxious air, till I ſhould have an opportunity of repeating my experiments, and giving more attention to them. Accordingly I reſumed the experiments in the ſummer of the year 1772, when I preſently had the moſt indiſputable proof of the reſtoration of putrid air by vegetation; and as the fact is of ſome importance, and the ſubſequent variation in the ſtate of this kind of air is a little remarkable; I think it neceſſary to relate ſome of the facts pretty circumſtantially.

The air, on which I made the firſt experiments, was rendered exceedingly noxious by mice dying in it on the 20th of June. Into a jar nearly filled with one part of this air, I put a ſprig of mint, while I kept another part of it in a phial, in the ſame expoſure; and on the 27th of the ſame month, and not before, I made a trial of it, by introducing a mouſe into a glaſs veſſel, containing 2½ ounce meaſures filled with each kind of air; and I noted the following facts.

When the veſſel was filled with the air in which the mint had grown, a very large mouſe lived five minutes in it, before it began to ſhew any ſign of uneaſineſs. I then took it out, and found it to be as ſtrong and vigorous as when it was firſt put in; whereas in that air which had been kept in the phial only, without a plant growing in it, a younger mouſe continued not longer than two or three ſeconds, and was taken out quite dead. It never breathed after, and was immediately motionleſs. After half an hour, in which time the larger mouſe

(which I had kept alive, that the experiment might be made on both the kinds of air with the very ſame animal) would have been ſufficiently recruited, ſuppoſing it to have received any injury by the former experiment, was put into the ſame veſſel of air; but though it was withdrawn again, after being in it hardly one ſecond, it was recovered with difficulty, not being able to ſtir from the place for near a minute. After two days, I put the ſame mouſe into an equal quantity of common air, and obſerved that it continued ſeven minutes without any ſign of uneaſineſs; and being very uneaſy after three minutes longer, I took it out. Upon the whole, I concluded that the reſtored air wanted about one fourth of being as wholeſome as common air. The ſame thing alſo appeared when I applied the teſt of nitrous air.

In the ſeven days, in which the mint was growing in this jar of noxious air, three old ſhoots had extended themſelves about three inches, and ſeveral new ones had made their appearance in the ſame time. Dr. Franklin and Sir John Pringle happened to be with me, when the plant had been three or four days in this ſtate, and took notice of its vigorous vegetation, and remarkably healthy appearance in that confinement.

On the 30th of the ſame month, a mouſe lived fourteen minutes, breathing naturally all the time, and without appearing to be much uneaſy, till the laſt two minutes, in air which had been rendered noxious by mice breathing in it almoſt a year before, and which I had found to be moſt highly noxious on the 19th of this month, a plant having grown in it,

but

but not exceedingly well, theſe eleven days; on which account, I had deferred making the trial ſo long. This reſtored air was affected by a mixture of nitrous air, almoſt as much as common air.

As this putrid air was thus eaſily reſtored to a conſiderable degree of fitneſs for reſpiration, by plants growing in it, I was in hopes that by the ſame means it might in time be ſo much more perfectly reſtored, that a candle would burn in it; and for this purpoſe I kept plants growing in the jars which contained this air till the middle of Auguſt following, but did not take ſufficient care to pull out all the old and rotten leaves. The plants, however, had grown, and looked ſo well upon the whole, that I had no doubt but that the air muſt conſtantly have been in a mending ſtate; when I was exceedingly ſurprized to find, on the 24th of that month, that though the air in one of the jars had not grown worſe, it was no better, and that the air in the other jar was ſo much worſe than it had been, that a mouſe would have died in it in a few ſeconds. It alſo made no effervescence with nitrous air, as it had done before.

Suſpecting that the ſame plant might be capable of reſtoring putrid air to a certain degree only, or that plants might have a contrary tendency in ſome ſtages of their growth, I withdrew the old plant, and put a freſh one in its place; and found that, after ſeven days, the air was reſtored to its former wholeſome ſtate. This fact I conſider as a very remarkable one, and well deſerving of a farther inveſtigation, as it may throw more light upon the principles of vegetation. It is not, however,

a ſingle fact; for I had ſeveral inſtances of the ſame kind in the preceding year; but it ſeemed ſo very extraordinary, that air ſhould grow worſe by the continuance of the ſame treatment by which it had grown better, that, whenever I obſerved it, I concluded that I had not taken ſufficient care to ſatisfy myſelf of its previous reſtoration.

That plants are capable of perfectly reſtoring air injured by reſpiration, may, I think, be inferred with certainty from the perfect reſtoration, by this means, of air which had paſſed through my lungs, ſo that a candle would burn in it again, though it had extinguiſhed flame before, and a part of the ſame original quantity of air ſtill continued to do ſo. Of this one inſtance occurred in the year 1771, a ſprig of mint having grown in a jar of this kind of air, from the 25th of July to the 17th of Auguſt following; and another trial I made with the ſame ſucceſs the 7th of July 1772, the plant having grown in it from the 29th of June preceding. In this caſe alſo I found that the effect was not owing to any virtue in the leaves of mint; for I kept them conſtantly changed in a quantity of this kind of air, for a conſiderable time, without making any ſenſible alteration in it.

Theſe proofs of a partial reſtoration of air by plants in a ſtate of vegetation, though in a confined and unnatural ſituation, cannot but render it highly probable, that the injury which is continually done to the atmoſphere by the reſpiration of ſuch a number of animals, and the putrefaction of ſuch maſſes of both vegetable and animal matter, is, in part at leaſt, repaired by the vegetable creation.

And,

And, notwithſtanding the prodigious maſs of air that is corrupted daily by the abovementioned cauſes; yet, if we conſider the immenſe profuſion of vegetables upon the face of the earth, growing in places ſuited to their nature, and conſequently at full liberty to exert all their powers, both inhaling and exhaling, it can hardly be thought, but that it may be a ſufficient counterbalance to it, and that the remedy is adequate to the evil.

Dr. Franklin, who, as I have already obſerved, ſaw ſome of my plants in a very flouriſhing ſtate, in highly noxious air, was pleaſed to expreſs very great ſatisfaction with the reſult of the experiments. In his anſwer to the letter in which I informed him of it, he ſays,

"That the vegetable creation ſhould reſtore the "air which is ſpoiled by the animal part of it, "looks like a rational ſyſtem, and ſeems to be of "a piece with the reſt. Thus fire purifies water "all the world over. It purifies it by diſtillation, "when it raiſes it in vapours, and lets it fall in "rain; and farther ſtill by filtration, when, keeping it fluid, it ſuffers that rain to percolate the "earth. We knew before, that putrid animal ſubſtances were converted into ſweet vegetables, "when mixed with the earth, and applied as "manure; and now, it ſeems, that the ſame putrid ſubſtances, mixed with the air, have a ſimilar effect. The ſtrong thriving ſtate of your "mint in putrid air ſeems to ſhew that the air is "mended by taking ſomething from it, and not "by adding to it." He adds, "I hope this will "give ſome check to the rage of deſtroying trees

" that grow near houſes, which has accompanied " our late improvements in gardening, from an " opinion of their being unwholeſome. I am cer- " tain, from long obſervation, that there is no- " thing unhealthy in the air of woods; for we " Americans have every where our country habi- " tations in the midſt of woods, and no people on " earth enjoy better health, or are more prolific."

Having rendered inflammable air perfectly innoxious by continued agitation in a trough of water, deprived of its air, I concluded that other kinds of noxious air might be reſtored by the ſame means; and I preſently found that this was the caſe with putrid air, even of more than a year's ſtanding. I ſhall obſerve once for all, that this proceſs has never failed to reſtore any kind of noxious air on which I have tried it, viz. air injured by reſpiration or putrefaction, air infected with the fumes of burning charcoal, and of calcined metals, air in which a mixture of iron filings and brimſtone, or that in which paint made of white lead and oil has ſtood, or air which has been diminiſhed by a mixture of nitrous air. Of the remarkable effect which this proceſs has on nitrous air itſelf, an account will be given in its proper place.

If this proceſs be made in water deprived of air, either by the air pump, by boiling, by diſtillation, or if freſh rain water be uſed, the air will always be diminiſhed by the agitation; and this is certainly the faireſt method of making the experiment. If the water be freſh pump water, there will always be an increaſe of the air by agitation, the air contained in the water being ſet looſe, and joining

joining that which is in the jar. In this caſe, alſo, the air has never failed to be reſtored; but then it might be ſuſpected that the melioration was produced by the addition of ſome more wholeſome ingredient. As theſe agitations were made in jars with wide mouths, and in a trough which had a large ſurface expoſed to the common air, I take it for granted that the noxious effluvia, whatever they be, were firſt imbibed by the water, and thereby tranſmitted to the common atmoſphere. In ſome caſes this was ſufficiently indicated by the diſagreeable ſmell which attended the operation.

After I had made theſe experiments, I was informed that an ingenious phyſician and philoſopher had kept a fowl alive twenty-four hour, in a quantity of air in which another fowl of the ſame ſize had not been able to live longer than an hour, by contriving to make the air, which it breathed, paſs through no very large quantity of acidulated water, the ſurface of which was not expoſed to the common air; and that even when the water was not acidulated, the fowl lived much longer than it could have done, if the air which it breathed had not been drawn through the water. As I ſhould not have concluded that this experiment would have ſucceeded ſo well, from any obſervations that I had made upon the ſubject, I took a quantity of air in which mice had died, and agitated it very ſtrongly, firſt in about five times its own quantity of diſtilled water, in the manner in which I had impregnated water with fixed air; but though the operation was continued a long time, it made no ſenſible change in the properties of the air. I alſo repeated the operation with

pump water, but with as little effect. In this caſe, however, though the air was agitated in a phial, which had a narrow neck, the ſurface of the water in the baſon was conſiderably large, and expoſed to the common atmoſphere, which muſt have tended a little to favour the experiment. In order to judge more preciſely of the effect of theſe different methods of agitating air, I transfered the very noxious air, which I had not been able to amend in the leaſt degree by the former method, into an open jar, ſtanding in a trough of water; and when I had agitated it till it was diminiſhed about one third, I found it to be better than air, in which candles had burned out, as appeared by the teſt of the nitrous air; and a mouſe lived in 2 ½ ounce meaſures of it a quarter of an hour, and was not ſenſibly affected the firſt ten or twelve minutes.

In order to determine whether the addition of any *acid* to the water, would make it more capable of reſtoring putrid air, I agitated a quantity of it in a phial containing very ſtrong vinegar; and after that in *aqua fortis*, only half diluted with water; but, by neither of theſe proceſſes was the air at all mended, though the agitation was repeated at intervals during a whole day, and it was moreover allowed to ſtand in that ſituation all night.

Since, however, water in theſe experiments muſt have imbibed and retained a certain portion of the noxious effluvia, before they could be tranſmited to the external air, I do not think it improbable but that the agitation of the ſea and large lakes may be of ſome uſe for the purification of the atmoſphere, and the putrid matter contained in water may be imbibed

imbibed by aquatic plants, or be depoſited in ſome other manner.

Having found, by ſeveral experiments above-mentioned, that the proper putrid effluvium is ſomething quite diſtinct from fixed air, and finding, by the experiments of Dr. Macbride, that fixed air corrects putrefaction; I once concluded that this effect was produced, not by ſtopping the flight of the fixed air, or reſtoring to the putrefying ſubſtance the very ſame thing that had eſcaped from it; and which was the common vinculum of all its parts (which is that ingenious author's hypotheſis) but by an affinity between the fixed air and the putrid effluvium. It therefore occurred to me, that fixed air, and air tainted with putrefaction, though equally noxious when ſeparate, might make a wholeſome mixture, the one correcting the other; and I was confirmed in this opinion by, I believe, not leſs than fifty or ſixty inſtances, in which air, that had been made in the higheſt degree noxious, by reſpiration or putrefaction, was ſo far ſweetened, by a mixture of about four times as much fixed air that afterwards mice lived in it exceedingly well, and in ſome caſes almoſt as long as in common air. I found it, indeed, to be more difficult to reſtore old putrid air by this means; but I hardly ever failed to do it, when the two kinds of air had ſtood a long time together, by which I mean about a fortnight or three weeks.

The reaſon why I do not abſolutely conclude that the reſtoration of air in theſe caſes was the effect of fixed air, is that, when I made a trial of the mixture, I ſometimes agitated the two kinds

of air pretty ſtrongly together, in a trough of water, or at leaſt paſſed it ſeveral times through the water, from one jar to another, that the ſuperfluous fixed air might be abſorbed, not ſuſpecting at that time that the agitation could have any other effect; but having ſince found that very violent, and eſpecially long continued agitation in water, without any mixture of fixed air, never failed to render any kind of noxious air in ſome meaſure fit for reſpiration (and in one particular inſtance the mere transferring of the air from one veſſel to another through the water, though for a much longer time than I ever uſed for the mixtures of air, was of conſiderable uſe for the ſame purpoſe); I began to entertain ſome doubt of the efficacy of fixed air, for that purpoſe. In ſome caſes alſo the mixture of fixed air had by no means ſo much effect on the putrid air as, from the generality of my obſervations, I ſhould have expected.

I was always aware, indeed, that it might be ſaid, that, the reſiduum of fixed air not being very noxious, ſuch an addition muſt contribute to mend the putrid air; but, in order to obviate this objection, I once mixed the reſiduum of as much fixed air as I had found. by a variety of trials, to be ſufficient to reſtore a given quantity of putrid air, with an equal quantity of putrid air, without making any ſenſible melioration of it.

Upon the whole, I am inclined to think that this proceſs could hardly have ſucceeded ſo well as it did with me, and in ſo great a number of trials, unleſs fixed air have ſome tendency to correct air tainted with reſpiration or putrefaction; and it is

perfectly agreeable to the analogy of Dr. Macbride's diſcoveries, and may naturally be expected from them, that it ſhould have ſuch an effect.

By a mixture of fixed air I have made wholeſome the reſiduum of air generated by putrefaction only, from mice plunged in water. This, one would imagine, *à priori*, to be the moſt noxious of all kinds of air. For if common air only tainted with putrefaction be ſo deadly, much more might one expect that air to be ſo, which was generated from putrefaction only; but it ſeems to be nothing more than common air tainted with putrefaction, and therefore requires no other proceſs to ſweeten it. In this caſe, however, we ſeem to have an inſtance of the generation of genuine common air, though mixed with ſomething that is foreign to it. Perhaps the reſiduum of fixed air may be another inſtance of the ſame nature.

Fixed air is equally diffuſed through the whole maſs of any quantity of putrid air with which it is mixed; for dividing the mixture into two equal parts, they were reduced in the ſame proportion by paſſing through water. But this is alſo the caſe with ſome of the kinds of air which will not incorporate, as inflammable air, and air in which brimſtone has burned.

If fixed air tend to correct air which has been injured by animal reſpiration or putrefaction, lime-kilns, which diſcharge great quantities of fixed air, may be wholeſome in the neighbourhood of populous cities, the atmoſphere of which muſt abound with putrid effluvia. I ſhould think alſo that phyſicians might avail themſelves of the application

of

of fixed air in many putrid diſorders, eſpecially as it may be ſo eaſily adminiſtered by way of clyſter, where it would often find its way to much of the putrid matter. Nothing is to be apprehended from the diſtention of the bowels by this kind of air, ſince it is ſo readily abſorbed by any fluid or moiſt ſubſtance. Since fixed air is not noxious *per ſe*, but, like fire, only in exceſs, I do not think it at all hazardous to attempt to breathe it. It is however eaſily conveyed into the ſtomach, in natural or artificial Pyrmont water, in briſkly fermenting liquors, or a vegetable diet. It is poſſible, however, that a conſiderable quantity of fixed air might be imbibed by the abſorbing veſſels of the ſkin, if the whole body, except the head, ſhould be ſuſpended over a veſſel of ſtrongly fermenting liquor; and in ſome putrid diſorders this treatment might be very ſalutary. If the body was expoſed quite naked, there would be very little danger from the cold in this ſituation, and the air having freer acceſs to the ſkin might produce a greater effect. Being no phyſician, I run no riſk by throwing out theſe random, and perhaps whimſical, propoſals.

Having communicated my obſervations on fixed air, and eſpecially my ſcheme of applying it by way of *clyſter* in putrid diſorders, to Mr. Hey, an ingenious ſurgeon in this town, a caſe preſently occurred, in which he had an opportunity of giving it a trial; and mentioning it to Dr. Hird and Dr. Crowther, two phyſicians who attended the patient, they approved the ſcheme, and it was put in execution: both by applying the fixed air by way of clyſter, and at the ſame time making the

patient drink plentifully of liquors ſtrongly impregnated with it. The event was ſuch, that I requeſted Mr. Hey to draw up a particular account of the caſe, deſcribing the whole of the treatment, that the public might be ſatisfied that this new application of fixed air is perfectly ſafe, and alſo have an opportunity of judging how far it had the effect which I expected from it; and as the application is new, and not unpromiſing, I ſhall beg leave to ſubjoin his letter to me on the ſubject, by way of *Appendix* to theſe papers.

V.

Of Air in which a mixture of brimstone and filings of iron has stood.

Finding in Dr. Hales's account of his experiments, that there was a great diminution of the quantity of air in which a mixture of powdered brimſtone and filings of iron, made into a paſte with water, had ſtood, I repeated the experiment, and found the diminution greater than I had expected. The diminution of air by this proceſs is made as effectually, and as expeditiouſly, in quickſilver as in water; and it may be meaſured with the greateſt accuracy, becauſe there is neither any previous expanſion nor increaſe of the quantity of air, and becauſe it is ſome time before it begins to have any ſenſible effect. The diminution of air by this proceſs is various; but I have generally

generally found it to be between $\frac{1}{4}$ and $\frac{1}{5}$ of the whole.

Air thus diminiſhed is not heavier, but rather lighter than common air; and though lime-water does not become turbid when it is expoſed to this air, it is probably owing to the formation of a ſelenitic ſalt, as was the caſe with the ſimple burning of brimſtone abovementioned. That ſomething proceeding from the brimſtone ſtrongly affects the water which is confined in the ſame place with this brimſtone, is manifeſt from the very ſtrong ſmell that it has of the volatile ſpirit of vitriol. I conclude the diminution of air by this proceſs is of the ſame kind with the diminution of it in the other caſes, becauſe when this mixture is put into air which has been previouſly diminiſhed, either by the burning of candles, by reſpiration, or putrefaction, though it never fails to diminiſh it ſomething more, it is, however, no farther than this proceſs alone would have done it. If a freſh mixture be introduced into a quantity of air which had been reduced by a former mixture, it has little or no farther effect.

I obſerved, that when a mixture of this kind was taken out of a quantity of air in which a candle had before burned out, and in which it had ſtood for ſeveral days, it was quite cold and black, as it always becomes in a confined place; but it preſently grew very hot, ſmoaked copiouſly, and ſmelled very offenſively; and when it was cold, it was brown, like the ruſt of iron.

I once put a mixture of this kind to a quantity of inflammable air, made from iron, by which means it was diminiſhed $\frac{1}{9}$ or $\frac{1}{10}$ in its bulk; but, as far as I could

I could judge, it was ſtill as inflammable as ever. Another quantity of inflammable air was alſo reduced in the ſame proportion, by a mouſe putrefying in it; but its inflammability was not ſeemingly leſſened.

Air diminiſhed by this mixture of iron filings and brimſtone, is exceedingly noxious to animals, and I have not perceived that it grows any better by keeping in water. The ſmell of it is very pungent and offenſive.

The quantity of this mixture which I made uſe of in the preceding experiments, was from two to four ounce meaſures; but I did not perceive, but that the diminution of the quantity of air (which was generally about twenty ounce meaſures) was as great with the ſmalleſt, as with the largeſt quantity. How ſmall a quantity is neceſſary to diminiſh a given quantity of air to a *maximum*, I have made no experiments to aſcertain.

As ſoon as this mixture of iron filings, with brimſtone and water, begins to ferment, it alſo turns black, and begins to ſwell, and it continues to do ſo, till it occupies twice as much ſpace as it did at firſt; and the force with which it expands is great; but how great it is I have not endeavoured to determine.

When this mixture is immerſed in water, it generates no air, though it becomes black, and ſwells.

VI.

Of nitrous Air.

Ever ſince I firſt read Dr. Hales's moſt excellent Statical Eſſays, I was particularly ſtruck with that experiment of his, of which an account is given, Vol. I. p. 224, and Vol. II. p. 280; in which common air, and air generated from the Walton pyrites, by ſpirit of nitre, made a turbid red mixture, and in which part of the common air was abſorbed; but I never expected to have the ſatisfaction of ſeeing this remarkable appearance, ſuppoſing it to be peculiar to that particular mineral. Happening to mention this ſubject to the Hon. Mr. Cavendiſh, when I was in London, in the ſpring of the year 1772, he ſaid that he did not imagine but that other kinds of pyrites might anſwer as well as that which Dr. Hales made uſe of, and that probably the red appearance of the mixture depended upon the ſpirit of nitre only. This encouraged me to attend to the ſubject; and having no pyrites, I began with the ſolution of the different metals in ſpirit of nitre, and catching the air which was generated in the ſolution, I preſently found what I wanted, and a good deal more.

Beginning with the ſolution of braſs, on the 4th of June 1772, I firſt found this remarkable ſpecies of air; one effect of which, though it was caſually obſerved by Dr. Hales, he gave but little attention to; and which, as far as I know, has paſſed altogether unnoticed ſince his time, inſomuch that no name has been given to it. I therefore found myſelf, contrary

to

to my firſt reſolution, under an abſolute neceſſity of giving a name to this kind of air myſelf. When I firſt began to ſpeak and write of it to my friends, I happened to diſtinguiſh it by the name of nitrous air, becauſe I had procured it by means of ſpirit of nitre only; and though I cannot ſay that I altogether like the term, becauſe this air is not got from all the metals by the ſame ſpirit, neither myſelf nor any of my friends, to whom I have applied for the purpoſe, have been able to hit upon a better; ſo that I am obliged, after all, to content myſelf with it.

I have found that this kind of air is readily procured from iron, copper, braſs, tin, ſilver, quickſilver, biſmuth, and nickel, by the nitrous acid only, and from gold and the regulus of antimony by aqua regia. The circumſtances attending the ſolution of each of theſe metals are various, but hardly worth mentioning, in treating of the properties of the air which they yield, which, from what metal ſoever it is extracted, has, as far as I have been able to obſerve, the very ſame properties.

One of the moſt conſpicuous properties of this kind of air is the great diminution of any quantity of common air with which it is mixed, attended with a turbid red, or deep orange colour, and a conſiderable heat. The ſmell of it, alſo, is very ſtrong, and remarkable, but very much reſembling that of ſmoking ſpirit of nitre.

The diminution of a mixture of this and common air is not an equal diminution of both the kinds, which is all that Dr. Hales could obſerve, but of the common air chiefly, though not wholly. For if one meaſure of nitrous air be put to two meaſures of

common air, in a few minutes (by which time the effervescence will be over, and the mixture will have recovered its transparency) there will want about one ninth of the original two measures. I hardly know any experiment that is more adapted to amaze and surprize than this is, which exhibits a quantity of air, which, as it were, devours a quantity of another kind of air half as large as itself, and yet is so far from gaining any addition to its bulk, that it is diminished by it. If, after this full saturation of common air with nitrous air, more nitrous air be put to it, it makes an addition equal to its own bulk, without producing the least redness, or any other visible effect.

That this diminution is chiefly in the quantity of common air, is evident from this observation, that if the smallest quantity of common air be put to any larger quantity of nitrous air, though the two together will not occupy so much space as they did separately, yet the quantity will be still larger than that of the nitrous air only. One ounce measure of common air being put to near twenty ounce measures of nitrous air, made an addition to it of about half an ounce measure. This, however, being a much greater proportion than the diminution of common air, in the former experiment, seems to prove that part of the diminution in the former case is in the nitrous air. Besides, it will presently appear, that nitrous air is subject to a most remarkable diminution; and as common air, in a variety of other cases, suffers a diminution from one fifth to one fourth, I conclude, that in this case also it does not exceed that proportion, and therefore that the remainder of the diminution respects the nitrous air.

In

In order to judge whether the water contributed to the diminution of this mixture of nitrous and common air, I made the whole proceſs ſeveral times in quickſilver, uſing one third of nitrous, and two thirds of common air, as before. In this caſe the redneſs continued a very long time, and the diminution was not ſo great as when the mixtures had been made in water, there remaining one ſeventh more than the original quantity of common air. This mixture ſtood all night upon the quickſilver; and the next morning I obſerved that it was no farther diminiſhed upon the admiſſion of water to it, nor by pouring it ſeveral times through the water, and letting it ſtand in water two days. Another mixture, which ſtood about ſix hours on the quickſilver, was diminiſhed a little more upon the admiſſion of water, but was never leſs than the original quantity of common air. In another caſe, however, in which the mixture ſtood but a very ſhort time in quickſilver, the farther diminution, which took place upon the admiſſion of water, was much more conſiderable; ſo that the diminution, upon the whole, was very nearly as great as if the proceſs had been intirely in water. It is evident from theſe experiments, that the diminution is in part owing to the abſorption by the water; but that when the mixture is kept a long time, in a ſituation in which there is no water to abſorb any part of it, it acquires a conſtitution, by which it is afterwards incapable of being abſorbed by water.

In order to determine whether the fixed part of common air was depoſited in the diminution of it by

by nitrous air, I incloſed a veſſel full of lime water in the jar in which the proceſs was made, but it occaſioned no precipitation of the lime; and when the veſſel was taken out, after it had been in that ſituation a whole day, the lime was eaſily precipitated by breathing into it as uſual.

It is exceedingly remarkable that this effervescence and diminution, occaſioned by the mixture of nitrous air, is peculiar to common air, or air fit for reſpiration; and, as far as I can judge, from a great number of obſervations, is at leaſt very nearly, if not exactly, in proportion to its fitneſs for this purpoſe; ſo that by this means the goodneſs of air may be diſtinguiſhed much more accurately than it can be done by putting mice, or any other animals, to breathe in it. This was a moſt agreeable diſcovery to me, as I hope it may be an uſeful one to the public; eſpecially as, from this time, I had no occaſion for ſo large a ſtock of mice as I had been uſed to keep for the purpoſe of theſe experiments, uſing them only in thoſe which required to be very deciſive; and in theſe caſes I have ſeldom failed to know beforehand in what manner they would be affected.

It is alſo remarkable that, on whatever account air is unfit for reſpiration, this ſame teſt is equally applicable. Thus there is not the leaſt effervescence between nitrous and fixed air, or inflammable air, or any ſpecies of diminiſhed air. Alſo the degree of diminution being from nothing at all to more than one third of the whole of any quantity of air, we are by this means in poſſeſſion of a prodigiouſly large ſcale, by which we may diſtinguiſh

very ſmall degrees of difference in the goodneſs of air. I have not attended much to this circumſtance, having uſed this teſt chiefly for greater differences; but, if I did not deceive myſelf, I have perceived a real difference in the air of my ſtudy, after a few perſons have been with me in it, and the air on the outſide of the houſe. Alſo a phial of air having been ſent me, from the neighbourhood of York, it appeared not to be ſo good as the air near Leeds; that is, it was not diminiſhed ſo much by an equal mixture of nitrous air, every other circumſtance being as nearly the ſame as I could contrive. It may perhaps be poſſible, but I have not yet attempted it, to diſtinguiſh ſome of the different winds, or the air of different times of the year, by this teſt.

By means of this teſt I was able to determine what I was before in doubt about, *viz.* the kind as well as the degree of injury done to air by candles burning in it. I could not tell with certainty by means of mice, whether it was at all injured with reſpect to reſpiration; and yet if nitrous air may be depended upon for furniſhing an accurate teſt, it muſt be rather more than one third worſe than common air, and have been diminiſhed by the ſame general cauſe of the other diminutions of air. For when, after many trials, I put one meaſure of thoroughly putrid and highly noxious air, into the ſame veſſel with two meaſures of good wholeſome air, and into another veſſel an equal quantity, *viz.* three meaſures of air in which a candle had burned out; and then put equal quantities of nitrous air to each of them, the former was diminiſhed rather more than the latter. It agrees with

with this obſervation, that burned air is farther diminiſhed both by putrefaction, and a mixture of iron filings and brimſtone; and I therefore, take it for granted, by every other cauſe of the diminution of air. It is probable, therefore, that burned air is air ſo far loaded with phlogiſton, as to be able to extinguiſh a candle, which it may do long before it is fully ſaturated.

Inflammable air with a mixture of nitrous air burns with a green flame. This makes a very pleaſing experiment when it is properly conducted. As, for ſome time, I chiefly made uſe of copper for the generation of nitrous air, I firſt aſcribed this circumſtance to that property of this metal, by which it burns with a green flame; but I was preſently ſatisfied that it muſt ariſe from the ſpirit of nitre, for the effect is the very ſame from whichever of the metals the nitrous air is extracted, all of which I tried for this purpoſe, even ſilver and gold. A mixture of oil of vitriol and ſpirit of nitre in equal proportions diſſolved iron, and the produce was nitrous air; but a leſs degree of ſpirit of nitre in the mixture produced air that was inflammable, and which burned with a green flame. It alſo tinged common air a little red, and diminiſhed it, though not much.

The diminution of common air by a mixture of nitrous air, is not ſo extraordinary as the diminution which nitrous air itſelf is ſubject to from a mixture of iron filings and brimſtone, made into a paſte with water. This mixture, as I have already obſerved, diminiſhes common air between one fifth and one fourth, but has no ſuch effect upon

any kind of air that has been diminiſhed, and rendered noxious by any other proceſs; but when it is put to a quantity of nitrous air, it diminiſhes it ſo much, that no more than one fourth of the original quantity will be left. The effect of this proceſs is generally perceived in five or ſix hours, about which time the viſible efferveſcence of the mixture begins; and in a very ſhort time it advances ſo rapidly, that in about an hour almoſt the whole effect will have taken place. If it be ſuffered to ſtand a day or two longer, the air will ſtill be diminiſhed farther, but only a very little farther, in proportion to the firſt diminution. The glaſs jar, in which the air and this mixture have been confined, has generally been ſo much heated in this proceſs, that I have not been able to touch it.

Nitrous air thus diminiſhed has not the peculiar ſmell of nitrous air, but ſmells juſt like common air in which the ſame mixture has ſtood; and it is not capable of being diminiſhed any farther, by a freſh mixture of iron and brimſtone.

Common air ſaturated with nitrous air is alſo no farther diminiſhed by this mixture of iron filings and brimſtone, though the mixture ferments with great heat, and ſwells very much in it.

Plants die very ſoon, both in nitrous air, and alſo in common air ſaturated with nitrous air, but eſpecially in the former.

Neither nitrous air, nor common air ſaturated with nitrous air, differs in ſpecific gravity from common air, or, at leaſt, ſo little, that I could

not be ſure of it, ſometimes about three pints of it ſeeming to be about half a grain heavier, and at other times as much lighter than common air.

Having, among other kinds of air, expoſed a quantity of nitrous air, to water out of which the air had been well boiled, in the experiment to which I have more than once referred, as having been the occaſion of ſeveral new and important obſervations, I found that $\frac{19}{20}$ of the whole was abſorbed. Perceiving, to my great ſurprize, that ſo very great a proportion of this kind of air was miſcible with water, I immediately began to agitate a conſiderable quantity of it, in a jar ſtanding in a trough of the ſame kind of water; and with about four times as much agitation as fixed air requires, it was ſo far abſorbed by the water, that only about one fifth remained. This remainder extinguiſhed flame, and was noxious to animals. Afterwards I diminiſhed a pretty large quantity of it to one eighth of its original bulk, and the remainder ſtill retained much of its peculiar ſmell, and diminiſhed common air a little. A mouſe alſo died in it, but not ſo ſuddenly as it would have done in pure nitrous air. In this operation the peculiar ſmell of nitrous air is very manifeſt, the water being firſt impregnated with the air, and then tranſmitting it to the common atmoſphere.

This experiment gave me the hint of impregnating water with nitrous air, in the manner in which I had before done it with fixed air; and I preſently found that diſtilled water would imbibe about one tenth of its bulk of this kind of air, and

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that it acquired a remarkably acid and aſtringent taſte from it. The ſmell of water thus impregnated is at firſt peculiarly pungent. I did not chuſe to ſwallow any of it, though, for any thing that I know, it may be perfectly innocent, and perhaps, in ſome caſes, ſalutary.

This kind of air is retained very obſtinately by water. In an exhauſted receiver a quantity of water thus ſaturated emitted a whitiſh fume, ſuch as ſometimes iſſues from bubbles of this air when it is firſt generated, and alſo ſome air bubbles; but though it was ſuffered to ſtand a long time in this ſituation, it ſtill retained its peculiar taſte; but when it had ſtood all night pretty near the fire, the water was become quite vapid, and had depoſited a filmy kind of matter, of which I had often collected a conſiderable quantity from the trough in which jars containing this air had ſtood. This I ſuppoſe to be a precipitate of the metal by the ſolution of which the nitrous air was generated. I have not given ſo much attention to it as to know, with certainty, in what circumſtances this depoſit is made, any more than I do the matter depoſited from inflammable air abovementioned; for I cannot get it, at leaſt in any conſiderable quantity, when I pleaſe; whereas I have often found abundance of it, when I did not expect it at all.

The nitrous air with which I made the firſt impregnation of water was extracted from copper; but when I made the impregnation with air from quickſilver, the water had the very ſame taſte, though the matter depoſited from it ſeemed to be of a dif-

ferent kind; for it was whitiſh, whereas the other had a yellowiſh tinge. Except the firſt quantity of this impregnated water, I could never deprive any more that I made of its peculiar taſte. I have even let ſome of it ſtand more than a week, in phials with their mouths open, and ſometimes very near the fire, without producing any alteration in it.

Whether any of the ſpirit of nitre be properly contained in the nitrous air, and be mixed with the water in this operation, I have not yet endeavoured to determine. This, however, may probably be the caſe, as the ſpirit of nitre is in a conſiderable degree volatile.

It will perhaps be thought, that the moſt uſeful, if not the moſt remarkable, of all the properties of this extraordinary kind of air, is its power of preſerving animal ſubſtances from putrefaction, and of reſtoring thoſe that are already putrid, which it poſſeſſes in a far greater degree than fixed air. My firſt obſervation of this was altogether caſual. Having found nitrous air to ſuffer ſo great a diminution as I have already mentioned by a mixture of iron filings and brimſtone, I was willing to try whether it would be equally diminiſhed by other cauſes of the diminution of common air, eſpecially by putrefaction; and for this purpoſe I put a dead mouſe into a quantity of it, and placed it near the fire, where the tendency to putrefaction was very great. In this caſe there was a conſiderable diminution, *viz.* from $5\frac{1}{4}$ to $3\frac{1}{4}$; but not ſo great as I had expected, the antiſeptic power of the nitrous air having checked

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the tendency to putrefaction; for when, after a week, I took the mouſe out, I perceived, to my very great ſurprize, that it had no offenſive ſmell.

Upon this I took two other mice, one of them juſt killed, and the other ſoft and putrid, and put them both into the ſame jar of nitrous air, ſtanding in the uſual temperature of the weather, in the months of July and Auguſt of 1772; and after 25 days, having obſerved that there was little or no change in the quantity of the air, I took the mice out; and, examining them, found them both perfectly ſweet, even when cut through in all places. That which had been put into the air when juſt dead was quite firm; and the fleſh of the other, which had been putrid and ſoft, was ſtill ſoft, but perfectly ſweet.

In order to compare the antiſeptic power of this kind of air with that of fixed air, I examined a mouſe which I had incloſed in a phial full of fixed air, as pure as I could make it, and which I had corked very cloſe; but upon opening this phial in water, about a month after, I perceived that a large quantity of putrid effluvium had been generated; for it ruſhed with violence out of the phial; and the ſmell that came from it, the moment the cork was taken out, was inſufferably offenſive. Indeed Dr. Macbride ſays, that he could only reſtore very thin pieces of putrid fleſh by means of fixed air. Perhaps the antiſeptic power of theſe kinds of air may be in proportion to their acidity. If a little pains were taken with this ſubject, this remarkable antiſeptic power of nitrous air might poſſibly be applied to various uſes, perhaps to the preſervation

preſervation of the more delicate birds, fiſhes, fruits, &c. mixing it in different proportions with common or fixed air. Of this property of nitrous air anatomiſts may perhaps avail themſelves, as animal ſubſtances may by this means be preſerved in their natural ſoft ſtate; but how long it will anſwer for this purpoſe, experience only can ſhew.

I calcined lead and tin in the manner hereafter deſcribed in a quantity of nitrous air, but with very little ſenſible effect; which rather ſurprized me; as, from the reſult of the experiment with the iron filings and brimſtone, I had expected a very great diminution of the nitrous air by this proceſs, the mixture of iron filings and brimſtone, and the calcination of metals, having the ſame effect upon common air, both of them diminiſhing it in nearly the ſame proportion.

Nitrous air is procured from all the proper metals by ſpirit of nitre, except lead, and from all the ſemi-metals that I have tried, except zinc. For this purpoſe I have uſed biſmuth and nickel, with ſpirit of nitre only, and regulus of antimony and platina, with aqua regia.

I got little or no air from lead by ſpirit of nitre, and have not yet made any experiments to aſcertain the nature of this ſolution. With zinc I have taken a little pains.

Four penny weights and ſeventeen grains of zinc diſſolved in ſpirit of nitre, to which as much water was added, yielded about twelve ounce meaſures of air, which had, in ſome degree, the properties of nitrous air, making a ſlight effervеſcence with common air, and diminiſhing it about as much as nitrous

trous air, which had been itſelf diminiſhed one half by waſhing in water. The ſmell of them both was alſo the ſame; ſo that I concluded it to be the ſame thing, that part of the nitrous air which is imbibed by water being retained in this ſolution.

In order to diſcover whether this was the caſe, I made the ſolution boil in a ſand heat. Some air came from it in this ſtate, which ſeemed to be the ſame thing, as nitrous air diminiſhed about one ſixth, or one eighth, by waſhing in water. When the fluid part was evaporated, there remained a brown fixed ſubſtance, which was obſerved by Mr. Hellot, who deſcribes it, Ac. Par. 1735, M. p. 35. A part of this I threw into a ſmall red hot crucible; and covering it immediately with a receiver, ſtanding in water, I obſerved that very denſe red fumes roſe from it, and filled the receiver. This redneſs continued about as long as that which is occaſioned by a mixture of nitrous and common air; the air was alſo conſiderably diminiſhed within the receiver. This ſubſtance, therefore, muſt certainly have contained within it the very ſame thing, or principle, on which the peculiar properties of nitrous air depend. It is remarkable, however, that though the air within the receiver was diminiſhed about one fifth by this proceſs, it was itſelf as much affected with a mixture of nitrous air, as common air is, and a candle burnt in it very well. This may perhaps be attributed to ſome effect of the ſpirit of nitre, in the compoſition of that brown ſubſtance.

Nitrous air, I find, will be conſiderably diminiſhed in its bulk by ſtanding a long time in water,

ter, about as much as inflammable air is diminiſhed in the ſame circumſtances. For this purpoſe I kept for ſome months a quart bottle full of each of theſe kinds of air; but as different quantities of inflammable air vary very much in this reſpect, it is not improbable but that nitrous air may vary alſo.

From one trial that I made, I conclude that nitrous air may be kept in a bladder much better than moſt other kinds of air. The air to which I refer was kept about a fortnight in a bladder, through which the peculiar ſmell of the nitrous air was very ſenſible for ſeveral days. In a day or two the bladder became red, and was much contracted in its dimenſions. The air within it had loſt very little of its peculiar property of diminiſhing common air.

I did not endeavour to aſcertain the exact quantity of nitrous air produced from given quantities of all the metals which yield it; but the few obſervations which I did make for this purpoſe I ſhall recite in this place:

dwt.	gr.		
6	0	of ſilver yielded	17½ ounce meaſures
5	19	of quickſilver	4½
1	2½	of copper	14½
2	0	of braſs	21
0	20	of iron	16
1	5	of biſmuth	6
0	12	of nickel	4

VII. Of

VII.

Of Air infected with the fumes of burning charcoal.

Air infected with the fumes of burning charcoal is well known to be noxious; and the Honourable Mr. Cavendiſh favoured me with an account of ſome experiments of his, in which a quantity of common air was reduced from 180 to 162 ounce meaſures, by paſſing through a red-hot iron tube filled with the duſt of charcoal. This diminution he aſcribed to ſuch a deſtruction of common air as Dr. Hales imagined to be the conſequence of burning. Mr. Cavendiſh alſo obſerved, that there had been a generation of fixed air in this proceſs, but that it was abſorbed by ſope leys. This experiment I alſo repeated, with a ſmall variation of circumſtances, and with nearly the ſame reſult.

Afterwards, I endeavoured to aſcertain, by what appears to me to be an eaſier and a more certain method, in what manner air is affected with the fumes of charcoal, viz. by ſuſpending bits of charcoal within glaſs veſſels, filled to a certain height with water, and ſtanding inverted in another veſſel of water, while I threw the focus of a burning mirror, or lens, upon them. In this manner I diminiſhed a given quantity of air one fifth, which is nearly in the ſame proportion with other diminutions of air.

Some fixed air ſeems to be contained in charcoal, and to be ſet looſe from it by this proceſs; for if I made uſe of lime-water, it never failed to become

turbid, preſently after the heat was applied. This was the caſe with whatever degree of heat the charcoal had been made. If, however, the charcoal had not been made with a very conſiderable degree of heat, there never failed to be a permanent addition of inflammable air produced; which agrees with what I obſerved before, that, in converting dry wood into charcoal, the greateſt part is changed into inflammable air. I have ſometimes found, that charcoal which was made with the moſt intenſe heat of a ſmith's fire, which vitrified part of a common crucible in which the charcoal was confined, and which had been continued above half an hour, did not diminiſh the air in which the focus of a burning mirror was thrown upon it; a quantity of inflammable air equal to the diminution of the common air being generated in the proceſs; whereas, at other times, I have not perceived that there was any generation of inflammable air, but a perfect diminution of common air, when the charcoal had been made with a much leſs degree of heat. This ſubject deſerves to be farther inveſtigated.

To make the preceding experiment with ſtill more accuracy, I repeated it in quickſilver; when I perceived that there was a ſmall increaſe of the quantity of air, from a generation either of fixed or inflammable air, but I ſuppoſe of the former. Thus it ſtood without any alteration a whole night, and part of the following day; when lime-water, being admitted to it, it preſently became turbid, and, after ſome time, the whole quantity of air, which was about four ounce meaſures, was diminiſhed one fifth, as before. In this caſe, I carefully weighed the piece of charcoal, which was exactly two grains, and could not find that

that it was ſenſibly diminiſhed in weight by the operation.

Air thus diminiſhed by the fumes of burning charcoal not only extinguiſhes flame, but is in the higheſt degree noxious to animals; it makes no efferveſcence with nitrous air, and is incapable of being diminiſhed any farther by the fumes of more charcoal, by a mixture of iron filings and brimſtone, or by any other cauſe of the diminution of air that I am acquainted with.

This obſervation, which reſpects all other kinds of diminiſhed air, proves that Dr. Hales was miſtaken in his notion of the abſorption of air in thoſe circumſtances in which he obſerved it. For he ſuppoſed that the remainder was, in all caſes, of the ſame nature with that which had been abſorbed, and that the operation of the ſame cauſe would not have failed to produce a farther diminution; whereas all my obſervations not only ſhew that air, which has once been fully diminiſhed by any cauſe whatever, is not only incapable of any farther diminution, either from the ſame or from any other cauſe, but that it has likewiſe acquired new properties, moſt remarkably different from thoſe which it had before, and that they are, in a great meaſure, the ſame in all the caſes. Theſe circumſtances give reaſon to ſuſpect, that the cauſe of diminution is, in reality, the ſame in all the caſes. What this cauſe is, may, perhaps, appear in the next courſe of obſervations.

VIII.

OF THE EFFECT OF THE CALCINATION OF METALS, AND OF THE EFFLUVIA OF PAINT MADE WITH WHITE-LEAD AND OIL, ON AIR.

Having been led to ſuſpect, from the experiments which I had made with charcoal, that the diminution of air in that caſe, and perhaps in other caſes alſo, was, in ſome way or other, the conſequence of its having more than its uſual quantity of phlogiſton, it occurred to me, that the calcination of metals, which are generally ſuppoſed to conſiſt of nothing but a metallic earth united to phlogiſton, would tend to aſcertain the fact, and be a kind of *experimentum crucis* in the caſe. Accordingly, I ſuſpended pieces of lead and tin in given quantities of air, in the ſame manner as I had before treated the charcoal; and throwing the focus of a burning mirror or lens upon them, in ſuch a manner as to make them fume copiouſly, I preſently perceived a diminution of the air. In the firſt trial that I made, I reduced four ounce meaſures of air to three, which is the greateſt diminution of common air that I had ever obſerved before, and which I account for, by ſuppoſing that, in other caſes, there was not only a cauſe of diminution, but cauſes of addition alſo, either of fixed or inflammable air, or ſome other permanently elaſtic matter, but that, the effect of the calcination of metals being ſimply the eſcape of phlogiſton, the cauſe of diminution was alone and uncontrouled.

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The air, which I had thus diminiſhed by calcination of lead, I transferred into another clean phial, but found that the calcination of more lead in it had no farther effect upon it. This air alſo, like that which had been infected with the fumes of charcoal, was in the higheſt degree noxious, made no effervescence with nitrous air, was no farther diminiſhed by the mixture of iron filings and brimſtone, and was not only rendered innoxious, but alſo recovered, in a great meaſure, the other properties of common air, by waſhing in water.

It might be ſuſpected that the noxious quality of the air in which lead was calcined, might be owing to ſome fumes peculiar to that metal; but I found no ſenſible difference between the properties of this air, and that in which tin was calcined.

The water over which metals are calcined acquires a yellowiſh tinge, and an exceedingly pungent ſmell and taſte, pretty much, as near as I can recollect, for I did not compare them together, like that over which brimſtone has been frequently burned. Alſo a thin and whitiſh pellicle covered both the ſurface of the water, and likewiſe the ſides of the phial in which the calcination was made, inſomuch that, without frequently agitating the water, it grew ſo opaque by this conſtantly accumulating incruſtation, that the ſun beams could not be tranſmitted through it in a quantity ſufficient to produce the calcination.

I imagined, however, that, even when this air was transferred into a clean phial, the metals were not ſo eaſily melted or calcined as they were in freſh air; for the air being once fully ſaturated with phlogiſton, may not ſo readily admit any more, though it be only

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to tranſmit it to the water. I alſo ſuſpected that metals were not eaſily melted or calcined in inflammable, fixed, or nitrous, air, or any kind of diminiſhed air. None of theſe kinds of air ſuffered any change by this operation; nor was there any precipitation of lime, when charcoal was heated in any of theſe kinds of air ſtanding in lime-water.

Query. May not water impregnated with phlogiſton from calcined metals, or by any other method, be of ſome uſe in medicine? The effect of this impregnation is exceedingly remarkable; but the principle with which it is impregnated is volatile, and entirely eſcapes in a day or two, if the ſurface of the water be expoſed to the common atmoſphere.

It ſhould ſeem that phlogiſton is retained more obſtinately by charcoal than it is by lead or tin; for when any given quantity of air is fully ſaturated with phlogiſton from charcoal, no heat that I have yet applied has been able to produce any more effect upon it; whereas, in the ſame circumſtances, lead and tin may ſtill be calcined. The air, indeed, can take no more; but the water receives it, and the ſides of the phial alſo receive an addition of incruſtation. This is a white powdery ſubſtance, and well deſerves to be examined. I ſhall endeavour to do it at my leiſure.

Lime-water never became turbid by the calcination of metals over it; but the colour, ſmell, and taſte of the water was always changed, and the ſurface of it became covered with a yellow pellicle, as before.

When this proceſs was made in quickſilver, the air was diminiſhed only one fifth; and upon water being

admitted to it, no more was abſorbed; which is an effect ſimilar to that of a mixture of nitrous and common air, which was mentioned before.

The preceding experiments on the calcination of metals ſuggeſted to me a method of explaining the cauſe of the miſchief which is known to ariſe from freſh paint, made with white lead (which I ſuppoſe is an imperfect calx of lead) and oil. To verify my hypotheſis, I firſt put a ſmall pot full of this kind of paint, and afterwards (which anſwered much better, by expoſing a greater ſurface of the paint) I daubed ſeveral pieces of paper with it, and put them under a receiver, and obſerved, that in about twenty-four hours, the air was diminiſhed between one fifth and one fourth, for I did not meaſure it very exactly. This air alſo was, as I expected to find it, in the higheſt degree, noxious; it did not efferveſce with nitrous air, it was no farther diminiſhed by a mixture of iron filings and brimſtone, and was made wholeſome by agitation in water deprived of all air.

I think it appears pretty evident, from the preceding experiments on the calcination of metals, that air is ſome way or other diminiſhed in conſequence of being highly charged with phlogiſton, and that agitation in water reſtores it, by imbibing a great part of the phlogiſtic matter. That water has a conſiderable affinity with phlogiſton, is evident from the ſtrong impregnation which it receives from it. May not plants alſo reſtore air diminiſhed by putrefaction, by abſorbing part of the phlogiſton with which it is loaded? The greater part of a dry plant, as well as of a dry animal ſubſtance, conſiſts of inflammable air, or ſomething that is capable of being converted into

into inflammable air; and it ſeems to be as probable that this phlogiſtic matter may have been imbibed by the roots and leaves of plants, and afterwards incorporated into their ſubſtance, as that it is altogether produced by the power of vegetation. May not this phlogiſtic matter be even the moſt eſſential part of the food and ſupport of both vegetable and animal bodies?

In the experiments with metals, the diminution of air ſeems to be the conſequence of nothing but a ſaturation with phlogiſton; and in all the other caſes of the diminution of air, I do not ſee but that it may be effected by the ſame means. When a vegetable or animal ſubſtance is diſſolved by putrefaction, the eſcape of the phlogiſtic matter (which, together with all its other conſtituent parts, is then let looſe from it) may be the circumſtance that produces the diminution of the air in which it putrefies. It is highly improbable that what remains after an animal body has been thoroughly diſſolved by putrefaction, ſhould yield ſo great a quantity of inflammable air, as the dried animal ſubſtance would have done. Of this I have not made an actual trial, though I have often thought of doing it, and ſtill intend to do it; but I think there can be no doubt of the reſult. Again, the iron, by its fermentation with brimſtone and water, is evidently reduced to a calx, ſo that phlogiſton muſt have eſcaped from it. Phlogiſton alſo muſt evidently be ſet looſe by the ignition of charcoal, and is not improbably the matter which flies off from paint, compoſed of white lead and oil. Laſtly, ſince ſpirit of nitre is known to have a very remarkable affinity with phlogiſton, it is far from being

being improbable that nitrous air may alſo produce the ſame effect by the ſame means.

To this hypotheſis it may be objected, that, if diminiſhed air be air ſaturated with phlogiſton, it ought to be inflammable; but this by no means follows, ſince its inflammability may depend upon ſome particular mode of combination, or degree of affinity, with which we are not acquainted. Beſides, inflammable air ſeems to conſiſt of ſome other principle, or to have ſome other conſtituent part, beſides phlogiſton and common air, as is probable from that remarkable depoſit, which, as I have obſerved, is made by inflammable air, both from iron and zinc.

It is not improbable, however, but that a greater degree of heat may inflame that air which extinguiſhes a common candle, if it could be conveniently applied. Air that is inflammable, I obſerve, extinguiſhes red hot wood; and indeed inflammable ſubſtances can only be thoſe which, in a certain degree of heat, have a leſs affinity with the phlogiſton they contain, than the air, or ſome other contiguous ſubſtance, has with it; ſo that the phlogiſton only quits one ſubſtance, with which it was before combined, and enters another, with which it may be combined in a very different manner. This ſubſtance, however, whether it be air or any thing elſe, being now fully ſaturated with phlogiſton, and not being able to take any more, in the ſame circumſtances, muſt neceſſarily extinguiſh fire, and put a ſtop to the ignition of all other bodies, that is, to the farther eſcape of phlogiſton from them.

That plants reſtore noxious air, by imbibing the phlogiſton with which it is loaded, is very agreeable to

the conjectures of Dr. Franklin, made many years ago, and expreſſed in the following extract from the laſt edition of his Letters, p. 346.

" I have been inclined to think that the fluid *fire*, " as well as the fluid *air*, is attracted by plants in " their growth, and becomes conſolidated with the " other materials of which they are formed, and " makes a great part of their ſubſtance; that, when " they come to be digeſted, and to ſuffer in the " veſſels a kind of fermentation, part of the fire, as " well as part of the air, recovers its fluid active ſtate " again, and diffuſes itſelf in the body, digeſting and " ſeparating it; that the fire ſo reproduced, by di- " geſtion and ſeparation, continually leaving the " body, its place is ſupplied by freſh quantities, " ariſing from the continual ſeparation; that what- " ever quickens the motion of the fluids in an ani- " mal quickens the ſeparation, and re-produces " more of the fire, as exerciſe; that all the fire " emitted by wood, and other combuſtibles, when " burning, exiſted in them before, in a ſolid ſtate, " being only diſcovered when ſeparating; that ſome " foſſils, as ſulphur, ſea-coal, &c. contain a great " deal of ſolid fire; and that, in ſhort, what eſcapes " and is diſſipated in the burning of bodies, beſides " water and earth, is generally the air, and fire, " that before made parts of the ſolid."

IX.

Of Air procured by means of spirit of salt.

Being very much ſtruck with the reſult of an experiment of the Hon. Mr. Cavendiſh, related Phil. Tranſ.

Tranſ. Vol. LVI. p. 157. by which, though, he ſays, he was not able to get any inflammable air from copper, by means of ſpirit of ſalt, he got a much more remarkable kind of air, *viz.* one that loſt its elaſticity by coming into contact with water, I was exceedingly deſirous of making myſelf acquainted with it. On this account, I began with making the experiment in quickſilver, which I never failed to do in any caſe in which I ſuſpected that air might either be abſorbed by water, or be in any other manner affected by it; and by this means I preſently got a much more diſtinct idea of the nature and effects of this curious ſolution.

Having put ſome copper filings into a ſmall phial, with a quantity of ſpirit of ſalt; and making the air, which was generated in great plenty, on the application of heat, aſcend into a tall glaſs veſſel full of quickſilver, and ſtanding in quickſilver, the whole produce continued a conſiderable time without any change of dimenſions. I then introduced a ſmall quantity of water to it, when about three fourths of it (the whole being about four ounce meaſures) preſently, but gradually, diſappeared, the quickſilver riſing in the veſſel. I then introduced a conſiderable quantity of water; but there was no farther diminution of the air, and the remainder I found to be inflammable.

Having frequently continued this proceſs a long time after the admiſſion of the water, I was much amuſed with obſerving the large bubbles of the newly generated air, which came through the quickſilver, the ſudden diminution of them when they came to the water, and the very ſmall bubbles which went

through the water. They made, however, a continual, though ſlow, increaſe of inflammable air.

Fixed air, being admitted to the whole produce of this air from copper, had no ſenſible effect upon it. Upon the admiſſion of water, a great part of the mixture, which, no doubt, was the moſt ſubtle kind of air from the copper, preſently diſappeared; another part, which I ſuppoſe to have been the fixed air, was abſorbed ſlowly; and in this particular caſe the very ſmall permanent reſiduum did not take fire; but it is very poſſible that it might have done ſo, if the quantity had been greater.

Lime-water being admitted to the whole produce of air from copper became white; but this I ſuſpect to have ariſen from ſome other circumſtance than the precipitation of the lime which it contained.

The ſolution of lead in the marine acid is attended with the very ſame phænomena as the ſolution of copper in the ſame acid; about three fourths of the generated air diſappearing on the contact of water, and the remainder being inflammable.

The ſolutions of iron, tin, and zinc, in the marine acid, were all attended with the ſame phænomena as the ſolutions of copper and lead, but in a leſs degree; for in iron one eighth, in tin one ſixth, and in zinc one tenth of the generated air diſappeared on the contact with water. The remainder of the air from iron, in this caſe, burned with a green, or very light blue flame.

I had always thought it ſomething extraordinary that a ſpecies of air ſhould loſe its elaſticity by the mere contact of any thing, and from the firſt ſuſpected that it muſt have been imbibed by the water that

that was admitted to it; but ſo very great a quantity of this air diſappeared upon the admiſſion of a very ſmall quantity of water, that I could not help concluding that appearances favoured the former hypotheſis. I found, however, that when I admitted a much ſmaller quantity of water, confined in a narrow glaſs tube, a part only of the air diſappeared, and that very ſlowly, and that more of it vaniſhed upon the admiſſion of more water. This obſervation put it beyond a doubt, that this air was properly imbibed by the water, which, being once fully ſaturated with it, was not capable of receiving any more. The water thus impregnated taſted very acid, even when it was much diluted with other water, through which the tube containing it was drawn. It even diſſolved iron very faſt, and generated inflammable air. This laſt obſervation, together with another which immediately follows, led me to the diſcovery of the true nature of this remarkable kind of air, as it had hitherto been called.

Happening, at one time, to uſe a good deal of copper and a ſmall quantity of ſpirit of ſalt, in the generation of this kind of air, I was ſurprized to find that air was produced long after, I could not but think that the acid muſt have been ſaturated with the metal; and I alſo found that the proportion of inflammable air to that which was abſorbed by the water continually diminiſhed, till, inſtead of being one fourth of the whole as I had firſt obſerved, it was not ſo much as one twentieth. Upon this, I concluded that this ſubtle air did not ariſe from the copper, but from the ſpirit of ſalt; and preſently making the experiment with the acid only, without any cop-

per, or metal of any kind, this air was immediately produced in as great plenty as before; ſo that this remarkable kind of air is, in fact, nothing more than the vapour, or fumes of ſpirit of ſalt, which appear to be of ſuch a nature, that they are not liable to be condenſed by cold, like the vapour of water, and other fluids. This vapour, however, ſeems to loſe its elaſticity, in ſome meaſure, gradually, unleſs it ſhould be thought to be affected by the quick-ſilver, with which it is in contact; for it was always diminiſhed, more or leſs, by ſtanding.

This elaſtic acid vapour extinguiſhes flame, and is much heavier than common air; but how much heavier, will not be eaſy to aſcertain. A cylindrical glaſs veſſel, about three fourths of an inch in diameter, and four inches deep, being filled with it, and turned upſide down, a lighted candle may be let down into it more than twenty times before it will burn at the bottom. It is pleaſing to obſerve the colour of the flame in this experiment; for both before the candle goes out, and alſo when it is firſt lighted again, it burns with a beautifully green, or rather light blue flame, ſuch as is ſeen when common ſalt is thrown into the fire.

When this elaſtic vapour is all expelled from any quantity of ſpirit of ſalt, which is eaſily perceived by the vapour being condenſed by cold, the remainder is a very weak acid, barely capable of diſſolving iron.

Being now in the poſſeſſion of a new ſubject of experiments, *viz.* an elaſtic acid vapour, in the form of a permanent air, eaſily procured, and effectually confined by glaſs and quickſilver, with which

which it did not ſeem to have any affinity; I immediately began to introduce a variety of ſubſtances to it, in order to aſcertain its peculiar properties and affinities, and alſo the properties of thoſe other bodies with reſpect to it.

Beginning with water, which, from preceding obſervations, I knew would imbibe it, and become impregnated with it; I found that 2½ grains of rain water abſorbed three ounce meaſures of this vapour, after which it was increaſed one third in its bulk, and weighed twice as much as before; ſo that this concentrated vapour ſeems to be twice as heavy as rain water. Water impregnated with it makes the ſtrongeſt ſpirit of ſalt that I have ſeen, diſſolving iron with the moſt rapidity. Conſequently, two thirds of the beſt ſpirit of ſalt is nothing more than mere phlegm or water.

Iron filings, being admitted to this vapour, were diſſolved by it pretty faſt, half of the vapour diſappearing, and the other half becoming inflammable air, not abſorbed by water. Putting chalk to it, fixed air was produced.

I had not introduced many ſubſtances to this vapour, before I diſcovered that it had an affinity with phlogiſton, ſo that it would deprive other ſubſtances of it, and form with it ſuch an union as conſtitutes inflammable air; which ſeems to ſhew, that inflammable air univerſally conſiſts of the union of ſome acid vapour with phlogiſton.

Inflammable air was produced, when to this vapour I put ſpirit of wine, oil of olives, oil of turpentine, charcoal, phoſphorus, bees-wax, and even ſulphur. This laſt obſervation, I own, ſurprized

prized me; for, the marine acid being reckoned the weakeſt of the three mineral acids, I did not think that it had been capable of diſlodging the oil of vitriol from this ſubſtance; but I found that it had the very ſame effect both upon alum and nitre; the vitriolic acid in the former caſe, and the nitrous in the latter, giving place to the ſtronger vapour of ſpirit of ſalt.

The ruſt of iron, and the precipitate of nitrous air made from copper, alſo imbibed this vapour very faſt, and the little that remained of it was inflammable air; which proves, that theſe calces contain phlogiſton. It ſeems alſo to be pretty evident, from this experiment, that the precipitate above-mentioned is a real calx of the metal, by the ſolution of which the nitrous air is generated.

As ſome remarkable circumſtances attend the abſorption of this vapour of ſpirit of ſalt, by the ſubſtances above-mentioned, I ſhall briefly mention them.

Spirit of wine abſorbs this vapour as readily as water itſelf, and is increaſed in bulk by that means. Alſo, when it is ſaturated, it diſſolves iron with as much rapidity, and ſtill continues inflammable.

Oil of olives abſorbs this vapour very ſlowly, and, at the ſame time, it turns almoſt black, and becomes glutinous. It is alſo leſs miſcible with water, and acquires a very diſagreeable ſmell. By continuing upon the ſurface of the water, it became white, and its offenſive ſmell went off in a few days.

Oil of turpentine abſorbed this vapour very faſt, turning brown, and almoſt black. No inflammable air was formed, till I raiſed more of the vapour than the

the oil was able to abſorb, and let it ſtand a conſiderable time; and ſtill the air was but weakly inflammable. The ſame was the caſe with the oil of olives, in the laſt mentioned experiment; and it ſeems to be probable, that, the longer this acid vapour had continued in contact with the oil, the more phlogiſton it would have extracted from it. It is not improbable, but that, in the intermediate ſtate, before it becomes inflammable air, it may be nearly of the nature of common air.

Bees-wax abſorbed this vapour very ſlowly. About the bigneſs of a hazel-nut of the wax being put to three ounce meaſures of the vapour, the vapour was diminiſhed one half in two days, and, upon the admiſſion of water, half of the remainder alſo diſappeared. This air was ſtrongly inflammable.

Charcoal abſorbed this vapour very faſt. About one fourth of it was rendered immiſcible in water, and was but weakly inflammable.

A ſmall bit of phoſphorus, perhaps about half a grain, ſmoked, and gave light in the vapour of ſpirit of ſalt, juſt as it would have done in common air confined. It was not ſenſibly waſted after continuing about twelve hours in that ſtate, and the bulk of the vapour was very little diminiſhed. Water being admitted to it abſorbed it as before, except about one fifth of the whole, which was but weakly inflammable.

Putting ſeveral pieces of ſulphur to this vapour, it was abſorbed but ſlowly. In about twenty-four hours about one fifth of the quantity had diſappeared; and water being admitted to the remainder, very little

more was abſorbed. The remainder was inflammable, and burned with a blue flame.

Nowithſtanding the affinity which this vapour of ſpirit of ſalt appears to have with phlogiſton, it is not capable of depriving all bodies of it. I found that dry wood, cruſts of bread, and raw fleſh, very readily imbibed this acid vapour, but did not part with any of their phlogiſton to it. All theſe ſubſtances turned very brown, after they had been ſome time expoſed to this vapour, and taſted very ſtrongly of the acid when they were taken out; but the fleſh, when waſhed in water, became very white, and the fibres eaſily ſeparated from one another, even more than they would have done if it had been boiled or roaſted.

When I put a piece of ſaltpetre to this vapour, it was preſently ſurrounded with a white fume, which ſoon filled the whole veſſel, exactly like the fume which burſts from the bubbles of nitrous air, when it is generated by a vigorous fermentation, and ſuch as is ſeen when nitrous air is mixed with this vapour of ſpirit of ſalt. In about a minute, the whole quantity of vapour was abſorbed, except a very ſmall quantity, which might be the common air that had lodged upon the ſurface of the ſpirit of ſalt within the phial.

A piece of alum expoſed to this vapour turned yellow, abſorbed it as faſt as the ſaltpetre had done, and was reduced by it to the form of a powder. The ſurface both of the nitre and alum was, I doubt not, changed into common ſalt, by this proceſs. Common ſalt, as might be expected, had no effect whatever on this vapour.

From

From conſidering the affinity which this vapour has with phlogiſton, I was induced to try the effect of a mixture of it with nitrous air. Accordingly, to two parts of this vapour, I put one part of nitrous air, and, in about twenty-four hours, the whole was diminiſhed to ſomething leſs than the original quantity of the vapour, and was no farther diminiſhed by the admiſſion of water. Holding the flame of a candle over this air, the lower part of it burned green, but there was no ſenſible exploſion. At different times I collected 2¾ ounce meaſures of this mixture of air; but, upon agitating it in rain-water, it was preſently diminiſhed to 1½ ounce meaſures. In this ſtate it effervеſced with nitrous air, and was conſiderably diminiſhed by it, but not ſo much as common air. Some allowance, no doubt, muſt be made for the ſmall quantities of common air, which lodged on the top of my phials, when I raiſed the fume from the ſpirit of ſalt; but, from the precautions that I made uſe of, I think that very little is to be allowed to this circumſtance; and, upon the whole, I am of opinion, that this experiment is an approach to the generation of common air, or air fit for reſpiration.

I had alſo imagined, that if air diminiſhed by the proceſſes above-mentioned was affected in this manner, in conſequence of its being ſaturated with phlogiſton, a mixture of this vapour might imbibe that phlogiſton, and render it wholeſome again; but I put about one fourth of this vapour to a quantity of air in which metals had been calcined, without making any ſenſible alteration in it. I do not, however, infer from this, that air is not diminiſhed by means of phlogiſton, ſince the air, like ſome other ſubſtances,

may hold the phlogiſton too faſt, to be deprived of it by this acid vapour.

I ſhall conclude my account of theſe experiments with obſerving, that the electric ſpark is viſible in the vapour of ſpirit of ſalt, exactly as it is in common air; and though I kept making this ſpark a conſiderable time in a quantity of it, I did not perceive that any ſenſible alteration was made in it. A little inflammable air was produced, but not more than might have come from the two iron nails which I made uſe of in taking the ſparks.

X.

Miscellaneous Observations.

Many of the preceding obſervations relating to the vinous and putrefactive fermentations, I had the curioſity to endeavour to aſcertain in what manner the air would be affected by the acetous fermentation. For this purpoſe I incloſed a phial full of ſmall beer in a jar ſtanding in water, and obſerved that during the firſt two or three days there was an increaſe of the air in the jar, but from that time it gradually decreaſed, till at length there appeared to be a diminution of about $\frac{1}{10}$ of the whole quantity. During this time the whole ſurface of it was gradually covered with a ſcum, beautifully corrugated. After this there was an increaſe of the air till there was more than the original quantity; but this muſt have been fixed air, not incorporated with the reſt of the maſs; for, withdrawing the beer, which I found to be ſour, after it had ſtood 18 or 20 days under the jar, and paſſing

paſſing the air ſeveral times through cold water, the original quantity was diminiſhed about $\frac{1}{9}$. In the remainder a candle would not burn, and a mouſe would have died preſently. The ſmell of this air was exceedingly pungent, but different from that of the putrid effluvium. A mouſe lived perfectly well in this air, thus affected with the acetous fermentation; after it had ſtood ſeveral days mixed with four times the quantity of fixed air.

All the kinds of factitious air on which I have yet made the experiment are highly noxious to animals, except that which is extracted from ſalt-petre, or alum; but in this even a candle burned juſt as in common air. In one quantity which I got from ſalt-petre a candle not only burned, but the flame was increaſed, and ſomething was heard like a hiſſing, ſimilar to the decrepitation of nitre in an open fire. This experiment was made when the air was freſh made, and while it probably contained ſome particles of nitre, which would have been depoſited afterwards. The air was extracted from theſe ſubſtances by putting them into a gun barrel, which was much corroded and ſoon ſpoiled by the experiment. What effect this circumſtance may have had upon the air I have not conſidered.

November 6, 1772, I had the curioſity to examine the ſtate of a quantity of this air, which had been extracted from ſalt-petre above a year, and which at firſt was perfectly wholeſome; when, to my very great ſurprize, I found that it was become, in the higheſt degree, noxious. It made no effervescence with nitrous air, and a mouſe died the moment it was put into it. I had not, however, waſhed it in rain water quite ten minutes

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(and perhaps leſs time would have been ſufficient) when I found, upon trial, that it was reſtored to its former perfectly wholeſome ſtate. It effervesced with nitrous air as much as the beſt common air ever does, and even a candle burned in it very well, which I had never before obſerved of any kind of noxious air meliorated by agitation in water. This ſeries of facts, relating to air extracted from nitre, appear to me to be very extraordinary and important, and, in able hands, may lead to conſiderable diſcoveries.

There are many ſubſtances which impregnate the air in a very remarkable manner, but without making it noxious to animals. Among other things I tried volatile alkaline ſalts, and camphire, the latter of which I melted with a burning glaſs, in air incloſed in a phial. The mouſe which was put into this air ſneezed and coughed very much, eſpecially after it was taken out; but it preſently recovered, and did not appear to have been ſenſibly injured.

Having made ſeveral experiments with a mixture of iron filings and brimſtone, kneaded to a paſte with water, I had the curioſity to try what would be the effect of ſubſtituting braſs duſt in the place of the iron filings. The reſult was, that when this mixture had ſtood about three weeks, in a given quantity of air, it had turned black, but was not increaſed in bulk. The air alſo was neither ſenſibly increaſed nor decreaſed, but the nature of it was changed, for it extinguiſhed flame, it would have killed a mouſe preſently, and was not reſtored by fixed air, which had been mixed with it ſeveral days.

I have

I have frequently mentioned my having, at one time, expoſed equal quantities of different kinds of air in jars ſtanding in boiled water. The common air in this experiment was diminiſhed four ſevenths, and the remainder extinguiſhed flame. This experiment demonſtrates that water does not abſorb air equally, but that it decompoſes it, taking one part, and leaving the reſt. To be quite ſure of this fact, I agitated a quantity of common air in boiled water, and when I had reduced it from eleven ounce meaſures to ſeven, I found that it extinguiſhed a candle, but a mouſe lived in it very well. At another time a candle barely went out when the air was diminiſhed one third, and at other times I have found this effect take place at other very different degrees of diminution. This difference I attribute to the differences in the ſtate of the water with reſpect to the air contained in it; for ſometimes it had ſtood longer than at other times before I made uſe of it. I alſo uſed diſtilled water, rain water, and water out of which the air had been pumped, promiſcuouſly with rain water. I even doubt not but that, in a certain ſtate of the water, there might be no ſenſible difference in the bulk of the agitated air, and yet at the end of the proceſs it would extinguiſh a candle, air being ſupplied from the water in the place of that part of the common air which had been abſorbed.

It is certainly a little extraordinary that the very ſame proceſs ſhould ſo far mend putrid air, as to reduce it to the ſtandard of air in which candles have burned out; and yet that it ſhould ſo far injure common and wholeſome air, as to reduce it to about the

the ſame ſtandard: but ſo the fact certainly is. If air extinguiſh flame in conſequence of its being previouſly ſaturated with phlogiſton, it muſt, in this caſe, have been transferred from the water to the air.

To a quantity of common air, thus diminiſhed by agitation in water, till it extinguiſhed a candle, I put a plant, but it did not ſo far reſtore it as that a candle would burn in it again; which to me appeared not a little extraordinary, as it did not ſeem to be in a worſe ſtate than air in which candles had burned out, and which had never failed to be reſtored by the ſame means. I had no better ſucceſs with a quantity of permanent air; which I had collected from my pump water. Indeed theſe experiments were begun before I was acquainted with that property of nitrous air, which makes it ſo accurate a meaſure of the goodneſs of other kinds of air; and it might perhaps be rather too late in the year when I made the experiments. Having neglected theſe two jars of air, the plants died and putrefied in both of them; and then I found the air in them both to be highly noxious, and to make no effervescence with nitrous air.

I found that a pint of my pump water contains about one fourth of an ounce meaſure of air, one half of which was afterwards abſorbed by ſtanding in freſh pump water. A candle would not burn in the air, but a mouſe lived in it very well. Upon the whole, it ſeemed to be in about the ſame ſtate as air in which a candle had burned out.

I once imagined that, by mere ſtagnation, air might become unfit for reſpiration, or at leaſt for the burning of candles; but if this be the caſe, and the change be produced gradually, it muſt require a long time for the purpoſe. For on the 22d of September 1772, I examined a quantity of common air, which had been kept in a phial, without agitation, from May 1771, and found it to be in no reſpect worſe than freſh air, even by the teſt of the nitrous air.

The cryſtallization of nitre makes no ſenſible alteration in the air in which the proceſs is made. For this purpoſe I diſſolved as much nitre as a quantity of hot water would contain, and let it cool under a receiver, ſtanding in water.

November 6, 1772, a quantity of inflammable air, which, by long keeping, had come to extinguiſh flame, I obſerved to ſmell very much like common air in which a mixture of iron filings and brimſtone had ſtood. It was not, however, quite ſo ſtrong, but it was equally noxious.

Biſmuth and nickel are diſſolved in the marine acid with the application of a conſiderable degree of heat; but little or no air is got from either of them; but, what I thought a little remarkable, both of them ſmelled very much like Harrowgate water. This ſmell I have met with ſeveral times in the courſe of my experiments, and in proceſſes very different from one another.

As I generally made uſe of mice in the experiments which relate to reſpiration, and ſome perſons may chuſe to repeat them after me, and purſue them farther than I have done; it may be

of uſe to them to be informed, that I kept them without any difficulty in glaſs receivers, open at the top and bottom, and having a quantity of paper, or tow, in the inſide, which ſhould be changed every three or four days; when it will be moſt convenient alſo to change the veſſel, and waſh it. But they muſt be kept in a pretty exact temperature, for either much heat or much cold kills them preſently. The place in which I have generally kept them is a ſhelf over the kitchin fire place, where, as it is uſual in Yorkſhire, the fire never goes out; ſo that the heat varies very little; and I find it to be at a medium about 70 degrees of Fahrenheit's thermometer. When they had been made to paſs through the water, as they neceſſarily muſt be, in order to a change of air, they require, and will bear a very conſiderable degree of heat, to warm and dry them.

I found, to my great ſurprize, in the courſe of theſe experiments, that mice will live intirely without water; for though I have kept ſome of them for three or four months, and have offered them water ſeveral times, they would never taſte it; and yet they continued in perfect health and vigour. Two or three of them will live very peaceably together in the ſame veſſel; though I had one inſtance of one mouſe tearing another almoſt in pieces, though there was plenty of proviſions for both of them.

The apparatus with which the principal of the preceding experiments were made is exceedingly ſimple, and cheap. The drawing annexed (Tab. IX.) exhibits a view of every thing that is moſt important in it.

A is

A is an oblong trough, about eight inches deep, kept nearly full of water, and B, B are jars ſtanding in it, about ten inches long, and two and a half wide; ſuch as I have generally uſed for electrical batteries.

C, C are flat ſtones, ſunk about an inch, or half an inch, under the water, on which veſſels of any kind may be conveniently placed, during a courſe of experiments.

D, D are pots nearly full of water, in which jars or phials, containing any kind of air, to which plants or any other ſubſtances may be expoſed, and having their mouths immerſed in water; ſo that the air in the inſide can have no communication with the external air.

E is a ſmall glaſs veſſel, of a convenient ſize for putting a mouſe into it, in order to try the wholeſomeneſs of any kind of air that it may contain.

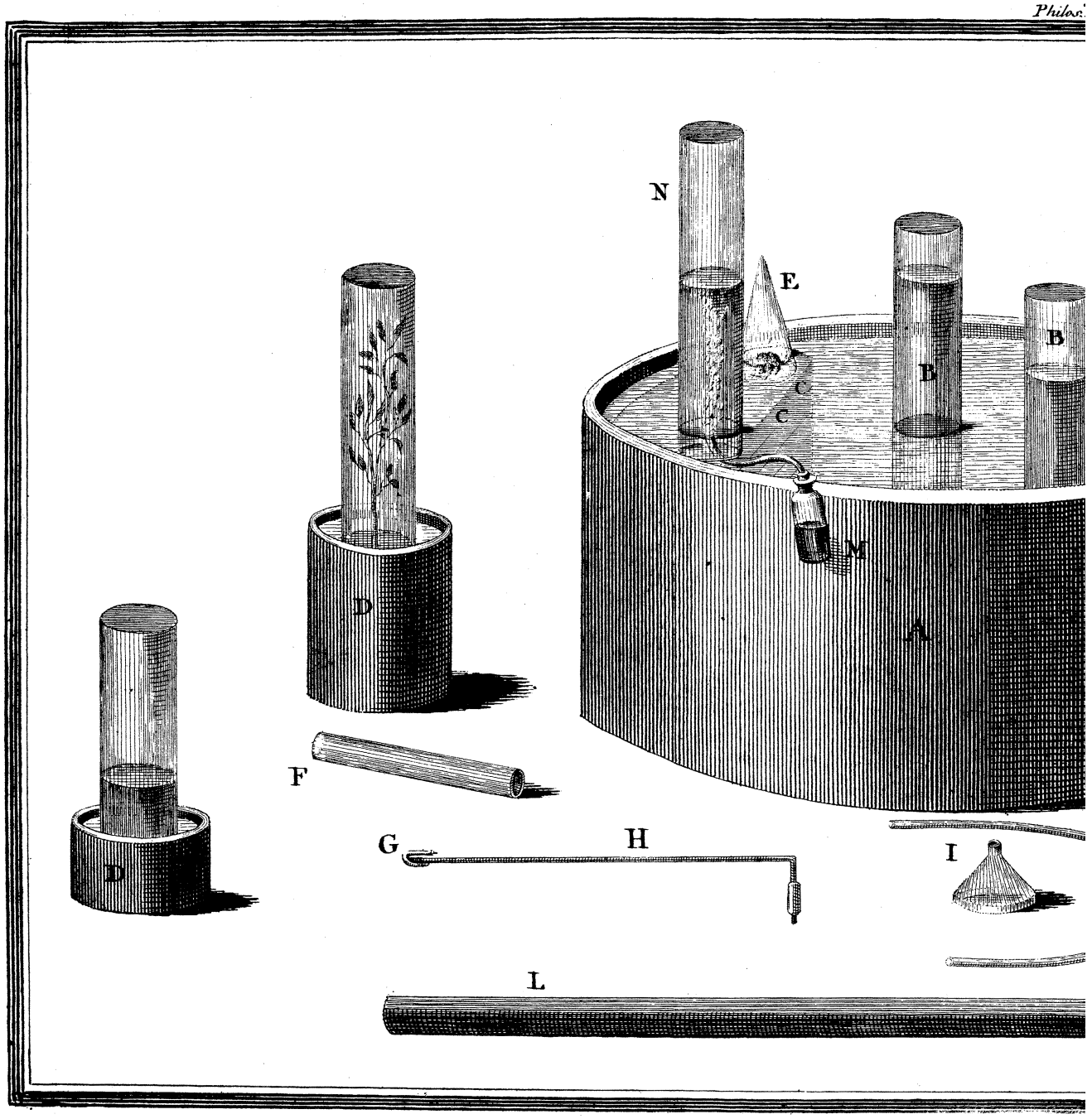
F is a cylindrical glaſs veſſel, five inches in length, and one in diameter, very proper for trying whether any kind of air will admit a candle to burn in it. For this purpoſe a bit of wax candle, G, may be faſtened to the end of a wire, H, and turned up in ſuch a manner as to be let down into the veſſel with the flame upwards. The veſſel ſhould be kept carefully covered till the moment that the candle is admitted to it. In this manner I have frequently extinguiſhed a candle above twenty times in one of theſe veſſels full of air, though it is impoſſible to dip the candle into it, without giving the external air an opportunity of mixing with it, more or leſs.

I is a funnel of glaſs or tin, which is neceſſary for transferring air into veſſels which have narrow mouths.

K is a glaſs ſyphon, which is very uſeful for drawing air out of a veſſel which has its mouth immerſed in water, and thereby raiſing the water to whatever height may be moſt convenient. I do not think it by any means ſafe to depend upon a valve at the top of a veſſel, which Dr. Hales very often made uſe of; for, ſince my firſt diſappointments, I have never thought the communication between the external and internal air ſufficiently cut off, unleſs glaſs, or a body of water, or, in ſome caſes, quickſilver, have intervened between them.

L is a piece of a gun barrel, cloſed at one end, having the ſtem of a tobacco-pipe luted to the other. To the end of this pipe I ſometimes faſtened a flaccid bladder, in order to receive the air diſcharged from the ſubſtance contained in the barrel; but, when the air was generated ſlowly, I commonly contrived to put this end of the pipe under a veſſel full of water, and ſtanding with its mouth inverted in another veſſel of water, that the new air might have a more perfect ſeparation from the external air than a bladder could make.

M is a ſmall phial containing ſome mixture that will generate air. This air paſſes through a bent glaſs tube inſerted into the cork at one end, and going under the edge of the jar N at the other; the jar being placed with part of its mouth projecting beyond the flat ſtones C C for that purpoſe.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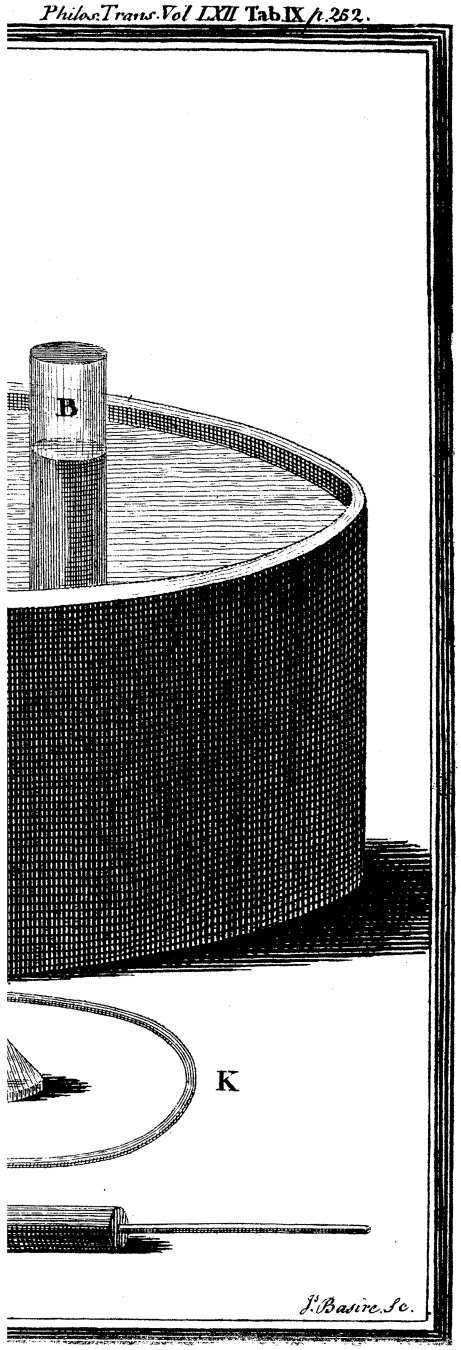
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AN APPENDIX,

Containing an account of ſome experiments made by Mr. Hey, which prove that there is no oil of vitriol in water impregnated with fixed air extracted from chalk by oil of vitriol; and alſo a letter from Mr. Hey, to Dr. Prieſtley, concerning the effects of fixed air applied by way of clyſter.

EXPERIMENTS TO PROVE THAT THERE IS NO OIL OF VITRIOL IN WATER IMPREGNATED WITH FIXED AIR.

It having been ſuggeſted, that air ariſing from a fermenting mixture of chalk and oil of vitriol might carry up with it a ſmall portion of the vitriolic acid, rendered volatile by the act of fermentation; I made the following experiments, in order to diſcover whether the acidulous taſte, which water impregnated with ſuch air affords, was owing to the preſence of any acid, or only to the fixed air it had abſorbed.

EXPERIMENT I.

I mixed a tea-ſpoonful of ſyrup of violets with an ounce of diſtilled water, ſaturated with fixed air procured from chalk by means of the vitriolic acid; but neither upon the firſt mixture, nor after

ſtanding 24 hours, was the colour of the ſyrup at all changed, except by its ſimple dilution.

Experiment II.

A portion of the ſame diſtilled water, unimpregnated with fixed air, was mixed with the ſyrup in the ſame proportion: not the leaſt difference in colour could be perceived betwixt this and the above mentioned mixture.

Experiment III.

One drop of oil of vitriol being mixed with a pint of the ſame diſtilled water, an ounce of this water was mixed with a tea-ſpoonful of the ſyrup. This mixture was very diſtinguiſhable in colour from the two former, having a purpliſh caſt, which the others wanted.

Experiment IV.

The diſtilled water impregnated with ſo ſmall a quantity of vitriolic acid having a more agreeable taſte than when alone, and yet manifeſting the preſence of an acid by means of the ſyrup of violets; I ſubjected it to ſome other teſts of acidity. It formed curds when agitated with ſoap, lathered with difficulty, and very imperfectly; but not the leaſt ebullition could be diſcovered upon dropping in ſpirit of ſal ammoniac, or ſolution of ſalt of tartar, though I had taken care to render the latter free from cauſticity by impregnating it with fixed air.

Ex-

Experiment V.

The diſtilled water ſaturated with fixed air neither effervеſced, nor ſhewed any clouds, when mixed with the fixed or volatile alkali.

Experiment VI.

No curd was formed by pouring this water upon an equal quantity of milk, and boiling them together.

Experiment VII.

When agitated with ſoap, this water produced curds, and lathered with ſome difficulty; but not ſo much as the diſtilled water mixed with vitriolic acid in the very ſmall proportion above-mentioned. The ſame diſtilled water without any impregnation of fixed air lathered with ſoap without the leaſt previous curdling. River water, and a pleaſant pump water not remarkably hard, were compared with theſe. The former produced curds before it lathered, but not quite in ſo great a quantity as the diſtilled water impregnated with fixed air: the latter cauſed a ſtronger curd than any of the others above-mentioned.

Experiment VIII.

Apprehending that the fixed air in the diſtilled water occaſioned the coagulation, or ſeparation of the oily part of the ſoap, only by deſtroying the cauſticity of the *lixivium*, and thereby rendering the

union leſs perfect betwixt that and the tallow, and not by the preſence of any acid; I impregnated a freſh parcel of the ſame diſtilled water with fixed air, which had paſſed through half a yard of a wide barometer tube filled with ſalt of tartar; but this water cauſed the ſame curdling with ſoap as the former had done, and appeared in every reſpect to be exactly the ſame.

Experiment IX.

Diſtilled water ſaturated with fixed air formed a white cloud and precipitation, upon being mixed with a ſolution of *ſaccharum ſaturni*. I found likewiſe, that fixed air, after paſſing through the tube filled with alkaline ſalt, upon being let into a phial containing a ſolution of the metallic ſalt in diſtilled water, cauſed a perfect ſeparation of the lead, in form of a white powder; for the water, after this precipitation, ſhewed no cloudineſs upon a freſh mixture of the ſubſtances which had before rendered it opaque.

A Letter

A Letter from Mr. HEY to Dr. PRIESTLEY, concerning the Effects of fixed Air applied by way of Clyſter.

Leeds, Feb. 15th, 1772.

Reverend Sir,

Having lately experienced the good effects of fixed air in a putrid fever, applied in a manner, I believe, not heretofore made uſe of, I thought it proper to inform you of the agreeable event, as the method of applying this powerful corrector of putrefaction took its riſe principally from your obſervations and experiments on factitious air; and now, at your requeſt, I ſend the particulars of the caſe I mentioned to you, as far as concerns the adminiſtration of this remedy.

January 8, 1772, Mr. Lightbowne, a young gentleman who lives with me, was ſeized with a fever, which, after continuing about ten days, began to be attended with thoſe ſymptoms that indicate a putreſcent ſtate of the fluids.

18th, His tongue was black in the morning when I firſt viſited him, but the blackneſs went off in the day-time upon drinking: He had begun to doze much the preceding day, and now he took little notice of thoſe that were about him: His belly was looſe, and had been ſo for ſome days: his pulſe beat 110 ſtrokes in a minute, and was rather low: he was ordered to take twenty five grains of Peruvian bark with five of tormentill root in powder every four hours, and to uſe red wine and water cold as his common drink.

19th, I was called to viſit him early in the morning, on account of a bleeding at the noſe which had come on: he loſt about eight ounces of blood, which was of a looſe texture: the hæmorrhage was ſuppreſſed, though not without ſome difficulty, by means of tents made of ſoft lint, dipped in cold water ſtrongly impregnated with tincture of iron, which were introduced within the noſtrils quite through to their poſterior apertures; a method which has never yet failed me in like caſes. His tongue was now covered with a thick black pellicle, which was not diminiſhed by drinking: his teeth were furred with the ſame kind of ſordid matter, and even the roof of his mouth and fauces were not free from it: his looſeneſs and ſtupor continued, and he was almoſt inceſſantly muttering to himſelf: he took this day a ſcruple of the Peruvian bark with ten grains of tormentill every two or three hours: a ſtarch clyſter containing a drachm of the compound powder of bole, without opium, was given morning and evening: a window was ſet open in his room, though it was a ſevere froſt, and the floor was frequently ſprinkled with vinegar.

20th, He continued nearly in the ſame ſtate: when rouzed from his dozing, he generally gave a ſenſible anſwer to the queſtions aſked him; but he immediately relapſed, and repeated his muttering. His ſkin was dry, and harſh, but without *petechiæ*. He ſometimes voided his urine and *fæces* into the bed, but generally had ſenſe enough to aſk for the bed-pan: as he now nauſeated the bark in ſubſtance, it was exchanged for Huxham's tincture,

tincture, of which he took a table-ſpoonful every two hours in a cup-full of cold water: he drank ſometimes a little of the tincture of roſes, but his common liquors were red wine and water, or rice water and brandy acidulated with elixir of vitriol: before drinking, he was commonly requeſted to rinſe his mouth with water to which a little honey and vinegar had been added. His looſeneſs rather increaſed, and the ſtools were watery, black, and fœtid: It was judged neceſſary to moderate this diſcharge, which ſeemed to ſink him, by mixing a drachm of the *theriaca Andromachi* with each clyſter.

21ſt. The ſame putrid ſymptoms remained, and a *ſubſultus tendinum* came on: his ſtools were more fœtid; and ſo hot, that the nurſe aſſured me ſhe could not apply her hand to the bed-pan, immediately after they were diſcharged, without feeling pain on this account: The medicine and clyſters were repeated.

Reflecting upon the diſagreeable neceſſity we ſeemed to lie under of confining this putrid matter in the inteſtines, leſt the evacuation ſhould deſtroy the *vis vitæ* before there was time to correct its bad quality, and overcome its bad effects, by the means we were uſing; I conſidered, that, if this putrid ferment could be more immediately corrected, a ſtop would probably be put to the flux, which ſeemed to ariſe from, or at leaſt to be encreaſed by it; and the *fomes* of the diſeaſe would likewiſe be in a great meaſure removed. I thought nothing was ſo likely to effect this, as the introduction of fixed air into the alimentary canal,

which, from the experiments of Dr. Macbride, and thoſe you have made ſince his publication, appears to be the moſt powerful corrector of putrefaction hitherto known. I recollected what you had recommended to me as deſerving to be tried in putrid diſeaſes, I mean, the injection of this kind of air by way of clyſter, and judged that in the preſent caſe ſuch a method was clearly indicated.

The next morning I mentioned my reflections to Dr. Hird and Dr. Crowther, who kindly attended this young gentleman at my requeſt, and propoſed the following method of treatment, which, with their approbation, was immediately entered upon. We firſt gave him five grains of ipecacoanha, to evacuate in the moſt eaſy manner part of the putrid *colluvies*: he was then allowed to drink freely of briſk orange-wine, which contained a good deal of fixed air, yet had not loſt its ſweetneſs: the tincture of bark was continued as before; and the water, which he drank along with it, was impregnated with fixed air from the atmoſphere of a large vat of fermenting wort, in the manner I had learned from you: inſtead of the aſtringent, air alone was injected, collected from a fermenting mixture of chalk and oil of vitriol: he drank a bottle of orange-wine in the courſe of this day, but refuſed any other liquor except water and his medicine: two bladders full of air were thrown up in the afternoon.

23d. His ſtools were leſs frequent; their heat likewiſe and peculiar *fœtor* were conſiderably diminiſhed: his muttering was much abated, and the *ſubſultus tendinum* had left him. Finding that part of the air was rejected when given with a bladder in the

the uſual way, I contrived a method of injecting it which was not ſo liable to this inconvenience. I took the flexible tube of that inſtrument which is uſed for throwing up the fume of tobacco, and tied a ſmall bladder to the end of it that is connected with the box made for receiving the tobacco, which I had previouſly taken off from the tube: I then put ſome bits of chalk into a ſix ounce phial until it was half filled; upon theſe I poured ſuch a quantity of oil of vitriol as I thought capable of ſaturating the chalk, and immediately tied the bladder, which I had fixed to the tube, round the neck of the phial: the clyſter pipe, which was faſtened to the other end of the tube, was introduced into the *anus* before the oil of vitriol was poured upon the chalk. By this method the air paſſed gradually into the inteſtines as it was generated; the rejection of it was in a great meaſure prevented; and the inconvenience of keeping the patient uncovered during the operation was avoided.

24th, He was ſo much better, that there ſeemed to be no neceſſity for repeating the clyſters: the other means were continued. The window of his room was now kept ſhut.

25th, All the ſymptoms of putreſcency had left him; his tongue and teeth were clean; there remained no unnatural blackneſs or *fœtor* in his ſtools, which had now regained their proper conſiſtence; his dozing and muttering were gone off; and the diſagreeable odour of his breath and perſpiration was no longer perceived. He took nouriſhment to-day, with pleaſure; and, in the afternoon, ſat up an hour in his chair.

His

His fever, however, did not immediately leave him; but this we attributed to his having caught cold from being incautiouſly uncovered, when the window was open, and the weather extremely ſevere; for a cough, which had troubled him in ſome degree from the beginning, increaſed, and he became likewiſe very hoarſe for ſeveral days, his pulſe, at the ſame time, growing quicker: but theſe complaints alſo went off, and he recovered, without any return of the bad ſymtoms above-mentioned.

I am, Reverend Sir,

Your obliged humble ſervant,

Wm Hey.

P. S.

October 29, 1772.

Fevers of the putrid kind have been ſo rare in this town, and in its neighbourhood, ſince the commencement of the preſent year, that I have not had an opportunity of trying again the effects of fixed air, given by way of clyſter, in any caſe exactly ſimilar to Mr. Lightbowne's. I have twice given water ſaturated with fixed air in a fever of the putreſcent kind, and it agreed very well with the patients. To one of them the aërial clyſters were adminiſtred, on account of a looſeneſs, which attended the fever, though the ſtools were not black, nor remarkably hot or fœtid.

Theſe

Theſe clyſters did not remove the looſeneſs, though there was often a greater interval than uſual betwixt the evacuations, after the injection of them. The patient never complained of any uneaſy diſtention of the belly from the air thrown up, which, indeed, is not to be wondered at, conſidering how readily this kind of air is abſorbed by aqueous and other fluids, for which ſufficient time was given, by the gradual manner of injecting it. Both thoſe patients recovered, though the uſe of fixed air did not produce a criſis before the period on which ſuch fevers uſually terminate. They had neither of them the opportunity of drinking ſuch wine as Mr. Lightbowne took after the uſe of fixed air was entered upon; and this, probably, was ſome diſadvantage to them.

I find the methods of procuring fixed air, and impregnating water with it, which you have publiſhed, are preferable to thoſe I made uſe of in Mr. Lightbowne's caſe.

The flexible tube uſed for conveying the fume of tobacco into the inteſtines, I find to be a very convenient inſtrument in this caſe, by the method before-mentioned (only adding water to the chalk, before the oil of vitriol is inſtilled, as you direct): the injection of air may be continued at pleaſure, without any other inconvenience to the patient, than what may ariſe from his continuing in one poſition during the operation, which ſcarcely deſerves to be mentioned, or from the continuance of the clyſter-pipe within the anus, which is but trifling, if it be not ſhaken much, or puſhed againſt the rectum.

When I ſaid in my letter, that fixed air appeared to be the greateſt corrector of putrefaction hitherto

known, your philoſophical reſearches had not then made you acquainted with that moſt remarkably antiſeptic property of nitrous air. Since you favoured me with a view of ſome aſtoniſhing proofs of this, I have conceived hopes, that this kind of air may likewiſe be applied medicinally to great advantage.

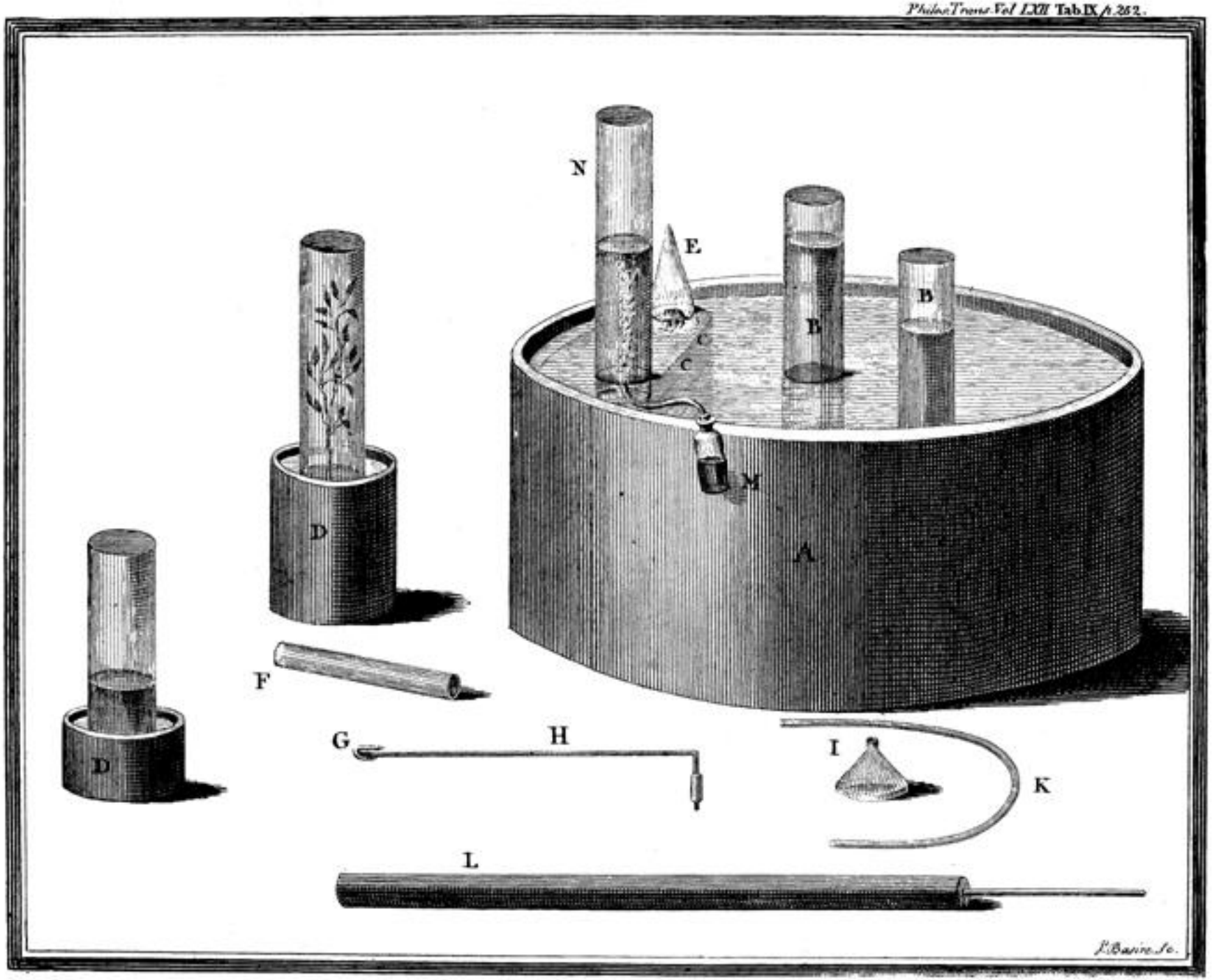
W. H.

A Correction.

Upon re-examining Dr. Hales's account of his experiments to meaſure the diminution of air by reſpiration (Statical Eſſays, Vol. I. p. 238, 4th edition), I find an error of the preſs, of $\frac{3}{13}$ for $\frac{1}{13}$; ſo that the diminution of air by reſpiration, though very various, is, I believe, always conſiderably leſs than by putrefaction, or ſeveral other cauſes of diminution. But though I have mentioned this diminution as equal to ſeveral others, nothing material depends upon it; the quality of the air thus diminiſhed being, in all reſpects, the ſame, notwithſtanding the cauſe of increaſe (which, as I have obſerved, in this and other caſes, co-operates with the cauſe of diminution) be greater than I had ſuppoſed.

I did not endeavour to meaſure the quantity of the diminution of air by reſpiration, as I did that by other cauſes; becauſe I imagined that it had been done ſufficiently by others, and eſpecially by Dr. Hales.

XX. *An*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