EXPERIMENTS

AND

OBSERVATIONS

ON DIFFERENT KINDS OF

AIR,

AND OTHER BRANCHES OF

NATURAL PHILOSOPHY,

CONNECTED WITH THE SUBJECT.

IN THREE VOLUMES;

Being the former Six Volumes abridged and methodized, with many Additions.

By JOSEPH PRIESTLEY, LL.D. F.R.S.

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BOOK III.

OBSERVATIONS RELATING TO NITROUS AIR.

PART III.

EXPERIMENTS TENDING TO ASCERTAIN THE CONSTITUENT PRINCIPLES OF NITROUS AIR.

AS the circumſtances in which any kind of air is decompoſed tend to diſcover its compoſition, I ſhall under this head recite all the obſervations I have made of this kind.

SECTION I.

Of the Diminution of nitrous Air by ſtanding in Water.

BEING deſirous of aſcertaining whether nitrous air tended, in any degree, to a ſpontaneous decompoſition by long ſtanding only, without being in contact with any ſubſtance that could be ſuppoſed to have an affinity with any conſtituent

part of it, I filled a large phial with this air, and corked it very tight, keeping it for the moſt part with its mouth immerſed in a veſſel of water, but afterwards neglected it, and ſuffered the water to evaporate. However, examining this air after about *two years*, I found that it had the ſame power of diminiſhing common air that freſh-made nitrous air has. It was alſo not more eaſily decompoſed by agitation in water; and I would obſerve in this place, that I have found great differences in the readineſs with which nitrous air is diminiſhed, and reduced to the ſtate of phlogiſticated air, by agitation in water, or by ſimply ſtanding expoſed to a conſiderable ſurface of water; a difference depending on the *water*, and the impregnations of it; but I have not given ſo much attention to theſe circumſtances as to have diſcovered the cauſe of them.

I have obſerved that if nitrous air be agitated in an open trough of water preſently after it is made, or indeed after it has been kept ſome weeks, it will be reduced to a very ſmall quantity, perhaps one twentieth of its original bulk; and it will then be wholeſome air. But I find that if it be kept a very *long time*, its conſtituent principles, as we may ſay, acquire a much firmer conſiſtence, and that then a remarkably greater proportion of it becomes firſt phlogiſticated air, and then, by agitation in water, wholeſome air.

I once

I once obſerved the changes produced in two quarts of nitrous air, one from *iron*, and the other from *copper*, made in November, 1773. On removing from Wiltſhire to Birmingham, I thought proper to put an end to this proceſs, when I found no farther change in the bulk of theſe two quantities of air; that which had been produced from iron ſtill occupying two thirds of its original dimenſions, and that from copper one half. I agitated in water a portion of each of theſe quantities of air, without producing any change in their bulk; but they were both conſiderably improved by it, ſo that when mixed with equal quantities of freſh nitrous air, the meaſures of the teſt were 1.75.

This I conſider as a pretty remarkable obſervation, as it exhibits a change in the conſtitution of a body depending upon *time* only, and which it is not yet in our power to produce by any other agent or inſtrument.

Both nitrous and inflammable air contain phlogiſton, and, as will be ſeen in its proper place, they probably contain nearly equal quantities in equal bulks; but as their properties are remarkably different, their conſtitution muſt be different alſo; the phlogiſton which enters into the compoſition of them both being combined in them in a very different manner. In ſome caſes nitrous air parts with its phlogiſton more readily than in-

flammable air, but in other respects inflammable air is the more easily decomposed of the two. The phlogiston of nitrous air immediately quits it on the contact of common air, when it is even quite cold, whereas the phlogiston of inflammable air will not leave it to join the common air except when it is very hot; but it will be seen that inflammable air parts with its phlogiston to the glass of lead in the composition of flint glass in circumstances in which nitrous air undergoes no change whatever. I kept a quantity of nitrous air in a tube of flint glass, hermetically sealed, buried in hot sand, but not sufficient to melt the glass, twenty days without any sensible change in the bulk or quality of the air. In the upper part of one tube filled in this manner there was something like small crystallizations, but they might possibly come from a small quantity of the quicksilver accidentally left in the tube. But whether nitrous air will be decomposed by quicksilver in this state of heat and confinement I did not try. Indeed I did not examine whether what I saw were properly crystallizations, or not.

I kept both nitrous air and inflammable air very hot in contact with quicksilver with liberty to expand, and did not find that either of them underwent any change. A quantity of nitrous air I exposed several hours for three days to a degree of

of heat which kept the quickſilver in a ſtate of vapour, the firſt and ſecond days to the ſame quickſilver, and the third day to freſh quickſilver; but the dimenſions of the air, and its property of affecting common air, continued the ſame. The proceſs is deſcribed in the Introduction.

The addition of ſteam of water to the nitrous air in this ſtate of heat and expanſion made no difference in the reſult of the experiment, though they continued together upon quickſilver more than two hours. The ſmall alteration that I found in the nitrous air might be aſcribed to its having been tranſferred from the trough of water to the baſon of quickſilver in a bladder. I varied the experiment by confining the nitrous air in a glaſs jar inverted in a pan of water, which I made to boil, in order that the hot ſteam might pervade the whole maſs of the air, which it effectually did, as it appeared by its having expelled a very great part of it. After the proceſs, which continued about an hour, the nitrous air had loſt nothing of its power of diminiſhing common air. On the contrary, it ſeemed, to be rather improved than to have had its virtue impaired.

SECTION II.

Of the Diminution of nitrous Air by Iron Filings and Sulphur, and by a Solution of green Vitriol.

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 ſulphur, 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

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 ſo ſtrong a ſmell as nitrous air itſelf, 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 ſulphur, though the mixture ferments with great heat, and ſwells very much in it.

To the experiments upon iron filings and ſulphur in nitrous air, I muſt add, that when a pot full of this mixture had abſorbed as much as it could of a jar of nitrous air (which is about three fourths of the whole) I put freſh nitrous air to it, and it continued to abſorb, till three or four jars full of it diſappeared; but the abſorption was exceedingly ſlow at the laſt. Alſo when I drew this pot through the water, and admitted freſh nitrous air to it, it abſorbed another jar full, and then ceaſed. But when I ſcraped off the outer ſurface of this mixture, which had been ſo long expoſed to the nitrous air, the remainder abſorbed more of the air.

When I took the top of the mixture which I had ſcraped off, and threw upon it the focus of a burning glaſs, the air in which it was confined was diminiſhed, and became quite noxious; yet when I endeavoured to get air from this matter in a jar full of quickſilver, I was able to procure little or nothing.

Nitrous air is as much diminiſhed both by iron filings, and alſo by liver of ſulphur, when confined in quickſilver, as when it is expoſed to water.

There is a remarkably quick abſorption of nitrous air by a ſolution of green vitriol, the particulars of which are as follows.

Having diſſolved a quantity of green vitriol, and put it into a phial, with its mouth inverted in a baſon of the ſame, and having admitted a quantity of nitrous air to it, I began to agitate the ſolution, in the ſame manner as in the proceſs for impregnating water with fixed air; when I obſerved that the nitrous air, in theſe circumſtances, was abſorbed much more readily than fixed air is by water. I even made a quantity of this ſolution abſorb more than ten times its bulk of nitrous air, without any very ſenſible approach to ſaturation. This ſolution became black by this proceſs; but when a ſmall part of it was viewed by the light

light of a candle, placed beyond it, it looked red. The taſte of the ſolution was acid, owing, no doubt, to the mixture of nitrous acid, which it had acquired, in conſequence of the decompoſition of the nitrous air.

When this impregnated ſolution was expoſed to the open air, large green cryſtals were formed at the bottom of the veſſel, and all the black colour intirely diſappeared. But when theſe cryſtals were formed at the bottom of a very tall veſſel, they were much blacker, and did not even become green on being expoſed afterwards to the open air, any more than thoſe which I expoſed to nitrous air itſelf on quickſilver.

The changes of colour, and all the phenomena of the cryſtals, were evidently owing to the ſpirit of nitre contained in the nitrous air, and ſet at liberty in its decompoſition. For a few drops of the acid itſelf produced the ſame effects, in all reſpects, on this ſolution.

Conceiving that the principal of theſe phenomena muſt have ariſen from the affinity between nitrous acid and iron, I agitated nitrous air in a natural chalybeate water, when it preſently became of a browniſh colour, which ſeemed to be a confirmation of my ſuppoſition.

I alſo

I alſo made another experiment in which the nitrous acid might ſhow its affinity to iron in a manner ſomewhat ſimilar to this. I firſt ſaturated a quantity of water with fixed air, then with iron, and afterwards impregnated it with nitrous air. The reſult of this experiment was, that the ſolution aſſumed a colour between green and yellow; but it did not abſorb much more nitrous air than water unimpregnated with fixed air, or with iron, would have done.

The nitrous air which I had hitherto made uſe of in theſe experiments was made from copper, but when I uſed that which was made from iron, which is an ingredient in green vitriol, the effect was not at all different. The ſolution of the vitriol abſorbed this nitrous air with the ſame rapidity as it did that which was made from copper, and the ſubſequent phenomena were alſo, in all reſpects, the ſame.

I then agitated nitrous air in ſolutions of blue and white vitriol, the former of which is known to be compoſed of copper, and the latter of zinc. The reſult was, that the colour of both theſe ſolutions became preſently very dark, the former changing into a deep green, and the latter into a kind of brown. Not more than between one half and one third of the air (which was about one fourth

of the contents of the phial I made uſe of) was abſorbed in either of theſe caſes, which is very far ſhort of the effect of the ſolution of green vitriol on the ſame kind of air.

It made no difference whether the nitrous air was procured from iron or from copper, in any of theſe experiments. For the ſolution of green vitriol, as I have obſerved, decompoſed nitrous air made from iron juſt as readily as that which was made from copper; and, on the other hand, the ſolutions of blue and white vitriol were affected in the very ſame manner by nitrous air made from copper, as by that from iron.

The ſolution of white vitriol depoſited a white and flocculent matter, and then was tranſparent like water; but, being impregnated with nitrous air, it preſently became of as dark a colour as when it had been impregnated before that depoſit was made.

Spirit of nitre dropped into the ſolution of blue or white vitriol made little or no change in their colour.

All the ſolutions of vitriol which had their colour changed by the impregnation of nitrous air recovered it again by expoſure to the common air. This was evidently effected by the eſcape of that phlogiſton, which had contributed to the deepneſs of

of their colour. To aſcertain this, I filled a phial about three fourths full of the ſolution of green vitriol, made black by the decompoſition of nitrous air, and after about a week, examining the air which had been confined with it, I found it to be ſo much phlogiſticated, that one meaſure of it and one of nitrous air occupied the ſpace of 1.92 meaſures.

Upon the whole, it ſeems that the greater effect of the ſolution of green vitriol in decompoſing nitrous air muſt be owing to the ſtronger affinity between the ſpirit of nitre and iron, than between the ſame acid and copper or zinc.

They ſeem to ſhow, however, that there is little, if any martial earth in nitrous air, at leaſt, that ſuch earth exiſting in nitrous air is not combined with phlogiſton, or in a metallic ſtate; ſince this air is decompoſed by the nitrous acid in it quitting the phlogiſton with which it was already combined, in order to unite itſelf to the iron in the ſolution, at the ſame time that the phlogiſton which entered into the nitrous air contributes to blacken the ſolution. It will, perhaps, however be thought extraordinary, that the nitrous acid ſhould have a ſtronger affinity with iron than the vitriolic, which, on this hypotheſis, it muſt, in this particular caſe, have.

This effect of the ſolution of vitriol on nitrous air helps to explain a phenomenon, which I had often

often obſerved without underſtanding it. When the water in my trough had got impregnated with various metallic ſubſtances, that which was contiguous to the nitrous air, in jars ſtanding in it, would be of a darker colour than the reſt of the water. This muſt have been in conſequence of the affinity between the phlogiſton in the nitrous air and the metallic matter diſſolved in the water, by means of ſome acid that happened to be mixed with it. At one time, when the water in my trough was particularly foul, and ſeemed diſpoſed to make a depoſit, I impregnated part of it with nitrous air, and the water, by this means, preſently became of a darker colour than before.

To determine whether the phenomena attending the impregnation of the ſolution of green vitriol with nitrous air depended, in any meaſure, upon the ſeeming *aſtringency* of that ſolution, and of chalybeate waters, I impregnated a quantity of *green tea*, which is alſo ſaid to be aſtringent, with nitrous air, but no ſenſible change of colour was produced in it.

In my firſt publications I mentioned a variety of circumſtances in which nitrous air is remarkably diminiſhed, in ſeveral of which it paſſes through a ſtate in which a candle burns in it quite naturally, and ſometimes with a much enlarged

flame,

flame, and at laſt becomes mere phlogiſticated air. In all theſe proceſſes I took it for granted (not having examined the air except when it was completely, or, at leaſt, very nearly reduced to one of the two ſtates above-mentioned) that the approximation to its final ſtate of phlogiſticated air was equable, ſo that as ſoon as it began to be diminiſhed, it alſo began to loſe its power of affecting common air. I find, however, that, with reſpect to ſeveral of the cauſes of diminution, and perhaps all of them, the air paſſes very ſuddenly from the ſtate in which it is perfect nitrous air, to the ſtate above-mentioned; but that the term at which this change takes place is various, as ſometimes two thirds, and ſometimes fourteen fifteenths of any quantity on which the experiment is made, will have diſappeared before any ſenſible change can be obſerved in the remainder. I have even ſometimes been inclined to think that its power of affecting common air has been rather increaſed than diminiſhed at the beginning of theſe proceſſes.

I imagine, therefore, that, as ſoon as either the nitrous principle, or the phlogiſton which enters into the compoſition of nitrous air, is ſeized upon by any ſubſtance which has a ſtronger affinity with either of them than they have with each other, ſo much of the other principle as was combined with it

it is precipitated, ſo that the air which remains is not at all altered from what it was, at leaſt for a conſiderable time. It will appear, however, that the ſlower the proceſs is, the greater quantity of nitrous air will be preſerved in the ſtate of phlogiſticated air, and the quicker the proceſs, the farther it will proceed before this change takes place.

I entertained the firſt ſuſpicion of my having been miſtaken in my former opinion when I was examining ſome nitrous air in which I had confined a fowl, in order to preſerve it as long as poſſible from putrefaction. For though this air was greatly diminiſhed in quantity, it affected common air quite as much as the beſt nitrous air I had ever tried.

Being deſirous of aſcertaining this fact with abſolute certainty, with reſpect to ſome one cauſe of the diminution of nitrous air, I placed a pot of *iron filings and brimſtone* in a jar of nitrous air, and let it remain there a whole day, keeping it generally warm, near the fire, the ingredients not being good of their kind, and not diſpoſed to ferment. As the diminution proceeded, I kept taking from it ſmall portions of the air, by introducing into it a ſmall jar full of water; which, being emptied within the jar, I withdrew, filled with the air from within it. Doing this occaſionally, I obſerved no change

change of the quality of the air when it was reduced to one third of its original bulk, for it retained its full power of diminiſhing common air.

The next day I found it diminiſhed to one fourth of the whole, and then a candle burned in it in a manner not to be diſtinguiſhed from the burning of a candle in common air. But it was not common air; for it was not at all diminiſhed by freſh nitrous air, and it affected common air ſo little, that one meaſure of it and one of common air occupied the ſpace of 1.85 meaſures. It had not acquired the peculiar property of fixed air, for it did not make lime water in the leaſt degree turbid, and it bore conſiderable agitation in water without being much diminiſhed.

The diminution of nitrous air by means of *ſpirit of nitre* is effected in the ſame manner; and as this diminution was made more quickly, on this account, perhaps, it proceeded much farther before I could perceive any change in it. In one experiment of this kind, I thought the change took place when the air was diminiſhed to one twelfth of the whole, but in another caſe there was no change till it was reduced to between one twelfth and an eighteenth part, when it was completely phlogiſticated. In this mode of diminution I was not able to find it in that ſtate in which a candle could burn

in it. At another time when exactly one fifteenth of the whole remained, it affected common air manifestly less than fresh nitrous air; but here again, when only one eighteenth remained it had lost all its peculiar property.

The diminution of nitrous air by the *solution of green vitriol* is effected according to the same rule. I decomposed nitrous air by exposing it to be absorbed by the solution of green vitriol till about one fourth of the original quantity remained, but it affected common air as much as it had done before any part of it was absorbed.

Such, also, is the manner in which nitrous air is diminished in a *bladder*. Nitrous air reduced in this manner from ten ounce measures to two and a half, was so much altered, that one measure of it and one of common air occupied the space of 1.75 measures. It then extinguished a candle without any appearance of a blue flame. When a little more of it was absorbed by the same process, I found the remainder all phlogisticated air, not affecting common air in the least. Till the nitrous air was reduced, in this manner, to very near one fourth, it continued unchanged.

If, however, common air be mixed with nitrous air, by which means it becomes in part phlogisticated air, the spirit of nitre will absorb the superfluous nitrous air only, and consequently leave the

remainder more diluted with phlogisticated air. I put a measure and a half of common air to two measures of nitrous air, so that one measure of this mixture and one of common air occupied the space of 1.36 measures. I then put some spirit of nitre to the mixture, and when it had absorbed one third of it, one measure of it and one of common air occupied the space of 1.8 measures.

I also introduced a piece of hot *charcoal* into a phial of nitrous air by which means one half of it was absorbed, and found that the remainder had not lost its power of diminishing common air in any sensible degree. The absorption of all kinds of air by *charcoal* is a very capital discovery of the Abbé Fontana, which he has been so obliging as to give me leave to mention.

When nitrous air has been kept a long time in water, it is known to be diminished, and in this case I suspect that it loses its virtue gradually, being impaired from the first. I have found, however, that by long keeping in perfectly stagnant water, the surface of which was exposed to the atmosphere, and without any change, except to supply the waste by evaporation, it came to the state of phlogisticated air; but by what steps in the process I omitted to observe, having taken it for granted that this was always equable.

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On the 11th of November 1773, I filled two quart bottles with fresh made nitrous air, one from iron, and the other from copper, and then set them aside, with their necks immersed in jars of water, and never agitated the air or the water in contact with it, only supplying the jars with fresh water as I perceived it was wanted. On the 29th of September 1778, I examined the state of these bottles of air, and found as follows. Of that which had been made from iron about one half was absorbed, and of that from the copper about a third; but both of them were equally and perfectly phlogisticated, making no effervescence with common air, and extinguishing a candle. That which had been made from copper did not make lime water turbid, and the same, I doubt not, would have been the case with the other, if it had been tried. I did this from a suspicion, that, since fixed air may be composed from the nitrous acid, nitrous air, in some of its changes, might, in part, assume that form. I had not given much attention to these bottles of air, but I do not think they had been at all diminished the last year, or the last year and a half.

Pyrophorus also decomposes nitrous air, and presently reduces it to the state of phlogisticated air. Having put a quantity of it into a glass jar standing inverted in quicksilver, I introduced some nitrous air to it, when the pyrophorus became instantly red

hot. What remained of the nitrous air had no effect on common air, and extinguiſhed a candle. All this change was effected at once. For though the nitrous air continued in the jar a day and two nights after it had been admitted to the pyrophorus, there was no farther change in its dimenſions.

Since pyrophorus fires pretty much alike in nitrous and in dephlogiſticated air, theſe two very different kinds of air muſt contain ſome common element; and this may be either *water*, or the *acidifying principle*. It is moſt probable that nitrous air contains both, though in a very different ſtate of combination, and in different proportions than thoſe in which they exiſt in dephlogiſticated air.

The *willow plant*, as I ſhall obſerve, abſorbs nitrous air as well as every other kind of air. What were the immediate ſtates of it I did not note, but when the air was reduced to one tenth of its bulk, I found it to be mere phlogiſticated air.

Nitrous air, as I have obſerved, is preſently decompoſed by a ſolution of green vitriol in water, which, in conſequence of it, becomes of a very dark colour; but becomes green again on being expoſed to the open air. In this and many other properties, the effect is the ſame as that of mixing a ſmall quantity of ſpirit of nitre with that ſolution. Having impregnated a quantity of the ſolution in this manner, I heated it in a glaſs phial, and found that

that it gave out half its bulk of pure nitrous air, but when it had loſt its dark colour, it gave no air at all by heat.

I have afterwards obſerved this change of colour to be a criterion of the preſence of a very ſmall quantity of the martial ſalts in water. When a very little of it has been accidently formed, in the courſe of my experiments, and mixed with the water in my trough, I have never failed to diſcover it by the dark colour of the water in thoſe jars which contained nitrous air. This change of colour muſt, as I obſerved before, have been produced by the phlogiſton of the nitrous air, the nitrous *acid* having no ſuch effect. This was alſo the caſe with a ſolution of copper in nitrous acid; but the change was not from blue to a darker colour, but into *green*.

I filled a jar, about an inch in diameter, and twelve inches long, with that ſolution of copper in ſpirit of nitre which remains after making nitrous air, and which is of a beautiful blue colour. Then inverting it in a baſon of the ſame, I introduced to it a quantity of nitrous air. After ſome time I obſerved that the air was conſiderably diminiſhed, and that all the ſurface of the liquid in contact with the air, to the depth of about a quarter of an inch, was of a beautiful green colour. This air kept diminiſhing ſome months, and the green colour of the ſolution extended two or three inches within the li-

quid. At laſt there remained only two ſevenths of the original quantity of air; and, examining it in that reduced ſtate, I found it to be mere phlogiſticated air. Had it been examined in the intermediate ſtate, it would, I doubt not, have been found to be of that kind of air in which a candle will burn. The experiment was begun on the 4th of October 1779, and the air was not examined till the 20th of July 1780.

SECTION III.

Of the Diminution of nitrous Air by the electric Spark.

THE electric ſpark taken in nitrous air, diminiſhes it to one fourth of its original quantity, which is about the quantity of its diminution by iron filings and ſulphur, and alſo by liver of ſulphur without heat. The air is alſo brought by electricity to the ſame ſtate as it is by iron filings and ſulphur, not diminiſhing common air. If the electric

electric spark be taken in it when it is confined by water tinged with archil, it is presently changed from blue to red, and that to a very great degree.

This experiment I have frequently repeated, in order to ascertain more particularly the quantity and quality of the residuum. In one experiment I reduced half an ounce measure of nitrous air, in less than half an hour, to one quarter of its bulk, but then one fourth of it was still nitrous, and the rest phlogisticated air. Receiving it in lime water, there seemed to be a slight precipitation, but this circumstance will perhaps be explained in the following experiment.

I took the electric spark in a quantity of nitrous air till there remained no more than one third of it, and this was completely phlogisticated, not affecting common air at all, and extinguishing a candle. A white matter was formed with the mercury over which the spark was taken, which made the water admitted to it extremely turbid. At that time I supposed that this white substance was mercurial nitre, consisting of nitrous acid from the decomposed nitrous air and mercury. I shall now examine it more particularly. Why there should be more than one third of this quantity of nitrous air left pure phlogisticated air, and not one fourth, as in the former experiment, I cannot tell.

In another proceſs I took the electric ſpark in a quantity of nitrous air, till it could be no more diminiſhed; when it was in proportion to its former bulk, as $10\frac{1}{2}$ to 24. Letting it ſtand all night upon the mercury, it was increaſed to the proportion of $11\frac{1}{4}$ to 24, ſeemingly by the acid uniting to the mercury, and generating more nitrous air, ſince it had that ſmell. No water appeared after the proceſs, and the water admitted to it acquired no acid taſte, but an aſtringent one, like water impregnated with nitrous air. There was a white powder formed, as in the former experiments.

Thinking to make water imbibe the acid from the nitrous air, decompoſed in this proceſs, I took the electric ſpark in it with a ſmall quantity of water over the mercury. But even this water did not acquire any acid taſte, but only an aſtringent one.

It remains to be aſcertained on what principles the nitrous air is decompoſed in this proceſs. That it is not by means of the *heat* communicated to it, may, I think, be inferred from an experiment, in which I made a quantity of nitrous air paſs through a red hot tobacco pipe, without producing any change in it. Neither does it ſeem to be the *light* of the electric ſpark, ſince I have heated pieces of crucibles in nitrous air by the ſun beams, without making any change in it.

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Upon the whole, it may be moſt probable that this change depends upon *phlogiſton*, as it is ſimilar to that which is made in nitrous air by the contact of iron, and eſpecially by heating iron in this air. There is this difference, however, in the two proceſſes; that in that with electricity the reſiduum is wholly phlogiſticated air, and the intermediate ſtate of dephlogiſticated nitrous air is not found. At leaſt I have not obſerved it.

I afterwards repeated with particular care this experiment of decompoſing nitrous air by the electric ſpark; from which it appears that, in this proceſs, though not in that of heating iron in it by the ſun beams, ſome *acid* is really depoſited from the air. But it is ſo little, that it may perhaps be ſuppoſed to have been *contained* in it, as an extraneous ſubſtance, not ſeparable from it by ſtanding in water. It is alſo to be obſerved that, in this proceſs, I was never able to produce any dephlogiſticated nitrous air. Whenever the experiment was ſuſpended, the air was always found to be either nitrous or phlogiſticated.

In one experiment of this kind, to which I gave particular attention, an ounce meaſure and a half of nitrous air was reduced to 0.4 of a meaſure by electric exploſions, taken in a veſſel of mercury, with a little pure water on the ſurface of it; an iron wire introduced through the mercury, and into the air, being uſed for the purpoſe of taking the exploſion

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at a proper distance After the greatest possible diminution of the air I could perceive no change of colour in the water, but after a few hours standing it had a purplish colour, and after a day and night, it was become of a deep red, and very turbid, evidently from the solution of the iron. The water was acid to the taste, but in the slightest degree imaginable, and I could perceive by the smell of it, that there was a little nitrous air still contained in it, though I could not perceive any redness on exposing the vessel to the open air A lighted candle being dipped into it, was immediately extinguished

There was one circumstance which I cannot easily account for in this process. After the diminution of the air would proceed no farther, I observed, as before, that, after some time, there was an increase of the quantity of it, viz. about one tenth of an ounce measure, but after this the air was gradually contracted to its former dimensions, without any farther use of electricity

I repeated this experiment with the same quantity of nitrous air, and it was diminished in about the same proportion as in the preceding But examining the water before it had been discoloured by the solution of the iron, I could only perceive some astringency in the taste of it, though it reddened a considerable quantity of water tinged blue with the juice of turnsole. There was also still something nitrous

nitrous in the residuum of this air. For being mixed in equal quantities with common air, the standard was about 1.8. It contained no dephlogisticated nitrous air, as it was not sensibly diminished by agitation in water.

Lastly, I nearly filled a phial, containing about six ounce measures of nitrous air, confined by quicksilver; and taking the electric spark within it, it was in about an hour diminished about one half, but after that very little. The quicksilver was much corroded, and a candle went out in the remainder of the air. The diminution of common air by the electric spark requires a good deal of time, but this process goes on very rapidly with nitrous air. I repeated the experiment in a tube a quarter of an inch in diameter, receiving the spark upon water tinged blue with the juice of turnsole; and the diminution was so quick, that the motion of the water up the tube was constantly sensible to the eye. The water was deeply and permanently red.

As the electric spark cannot be supposed to communicate so much phlogiston, as the heating of iron by the sun beams, we may be better able by this process to ascertain the quantity of phlogiston contained in phlogisticated air, since the residuum probably contains all the phlogiston belonging to the nitrous air from which it is made, and if we may calculate

calculate from the numbers in the former of these experiments, phlogisticated air must contain about four times as much phlogiston as an equal bulk of nitrous air; or, as appears by my former experiments, almost four times as much as inflammable air made from iron by oil of vitriol, or steam, of which one half is probably phlogiston, and the other half water.

SECTION IV.

Of the nitrous Acid produced from nitrous Air, by a Decomposition with common, or dephlogisticated Air.

IN order to complete the analysis of nitrous air, the *nitrous acid* which it is capable of forming by means of pure air, and which is a principal ingredient in its composition, ought to be more especially attended to; and it is not difficult, by means either of common or dephlogisticated air, so to decompose it, as to collect almost all the acid that it can assist in forming. And could we apply any certain *measure* to ascertain the strength of this acid,

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we might meaſure it in this as well as in any other caſe.

It is well known that when nitrous air is mixed with as much common air as it can completely phlogiſticate, it is itſelf decompoſed, and eſpecially that the acid which it aſſiſts in forming, immediately appears in the form of a red vapour, which is abſorbed by water, if the decompoſition be made over water, or imbibed by alkaline or metallic ſubſtances, if any ſuch be at hand to unite with it. Now by providing a given quantity of water to imbibe this acid, formed by a known quantity of nitrous air, it will be eaſy to aſcertain the quantity of nitrous acid transferred to the water, by finding how much nitrous air that acidulated water will yield, by the ſolution of copper, or any other metal. In this manner, at leaſt, the ſtrength of the acid may be compared with the ſtrength of any other nitrous acid in a fluid form.

The apparatus for this experiment is very ſimple, and very eaſily applied. I firſt provide a phial of a convenient ſize and form for the impregnation of the water, which ought to be pretty large, in order to decompoſe a conſiderable quantity of nitrous air at one proceſs; and having fitted a cork to it, I perforated the cork, ſo as juſt to admit the nozzle of a ſmall funnel, when it is thruſt in pretty hard. I then tie this cork in the mouth of a bladder large enough

enough to contain at least half as much air as the phial. But if I use dephlogisticated air instead of common air, for the decomposition of the nitrous air, the bladder ought to contain about five times as much as the phial.

If the process be made with common air, I convey into the bladder half as much nitrous air as the phial can contain; and taking care to dispose it so that no water, that may be in the bladder can get into the phial (which is not difficult to manage) I withdraw the funnel, and put the cork of the bladder into the phial; then pressing the bladder a little at first, in order to force a little of the nitrous air out of the bladder into the phial, the effervescence immediately begins, and of course the diminution of the air in the phial; the consequence of which is, a demand for more nitrous air from the bladder, to supply the place of that which had disappeared. This again produces a stronger effervescence; and by this means, after the redness has begun to appear in the phial, all the nitrous air will presently be drawn out of the bladder, and be decomposed within the phial; and all the acid that it forms will be imbibed by the water in the phial, or by any other substance that may be placed there for the purpose.

Before I endeavoured to ascertain, by this means, the precise quantity of nitrous acid furnished by a given

given quantity of nitrous air, I was willing to try how strongly I could impregnate a quantity of water with the nitrous acid in this manner; and I found that I could make four ounce measures and a half of water receive all the acid that could be supplied by 300 ounce measures of nitrous air; and that then the water was become quite blue. But water impregnated with the nitrous acid vapour either in this manner, or from the solution of metals in strong nitrous acid, adheres to it very slightly, and can hardly be prevented from making its escape. It is impossible to pour water thus impregnated from one vessel to another, but the copious red fumes which issue from it, even when the water is quite colourless, shew that it suffers a great loss of its acid. A quantity of the impregnated water which I got in this experiment, equal in bulk to a quantity of distilled water weighing four penny weights, being poured upon copper, yielded only one ounce measure and a quarter of nitrous air. The same quantity of the strong spirit of nitre made in the common way will yield twelve or fifteen ounce measures. In order to make my principal experiment with more accuracy, I was taught by this to think it necessary to provide such a quantity of water as would be but slightly impregnated, that the loss by evaporation might be the less in proportion.

In order to compare the quantity of nitrous acid formed by a given quantity of nitrous air, with the quantity of acid contained in an equal bulk of red nitrous vapour procured by the ſolution of biſmuth, I ſaturated a quantity of water in the method deſcribed above, with as much of the acid as was contained in a pint phial of nitrous air; and having filled the ſame phial with the red vapour, I put into it the ſame quantity of water, to imbibe that vapour; and then pouring all the water impregnated with nitrous acid from the nitrous air, and afterwards that which was impregnated by the nitrous vapour, upon equal quantities of copper, in a phial fitted with a ground ſtopper, I collected the nitrous air that they yielded, and found them to be exactly the ſame; and this was one ſeventh of the quantity of nitrous air by which the acid had been formed; ſo that, provided there was no loſs of the acid in the proceſs (and I am pretty well perſuaded that the loſs could not have been at all conſiderable) it may be inferred from this experiment, that the nitrous acid which is detained in the *ſalt* made by the ſolution of copper, is ſix times as much as can be formed by the nitrous air procured by that ſolution.

In order to aſcertain what quantity of nitrous vapour I had got in the phial that was filled by means of the ſolution of biſmuth, I filled the phial

a ſecond time in the ſame manner, as nearly as I could determine (but this eſtimate is attended with a good deal of uncertainty) and admitting water to it, I found that a little leſs than one half entered it; that proportion of its contents having been occupied by the nitrous vapour, and the remainder by the common air diffuſed through it. Conſequently, nitrous air does not form quite half ſo much nitrous acid as the ſame bulk of red nitrous vapour from the ſolution of biſmuth; ſuppoſing that as much red vapour was contained in the phial as can, at a medium, be thrown into it. But two phials will look almoſt equally red, ſo as hardly to be diſtinguiſhed by the eye, when one of them does not contain much more than half as much as the other.

The ſaturation of water with nitrous acid from nitrous air, by means of dephlogiſticated air, makes a pleaſing experiment, on account of the great quantity of nitrous air decompoſed by this means at one proceſs, the quickneſs with which the decompoſition is made, and the viſible effect of the ſudden impregnation on the water. For the ſurface of it inſtantly becomes, as it were, oily, deſcending in waves from the top to the bottom of the water, while nitrous air iſſues plentifully from the bottom and ſides of the veſſel; a moſt remarkable phenomenon, of which a full account will be given in a proper place.

SECTION V.

Of Water in nitrous Air.

THAT water enters into the compoſition of nitrous air, is not improbable, becauſe it is procured in ſo great abundance from pure water impregnated with phlogiſticated nitrous vapour, and alſo from its not being procured from copper, and other metals, except in a very diluted ſolution of the nitrous acid.

That water enters into the conſtitution of every kind of air I alſo ſuppoſed, becauſe it certainly does into that of *imflammable*, *fixed*, and *dephlogiſticated* air. That *nitrous* air contains water, I have lately found from the iron that is heated in it becoming a proper finery cinder.

I had found before, that iron heated in nitrous air acquired weight, and that what remained of the air was phlogiſticated air. Having ſince that time repeated this experiment, and afterwards heated the iron, which was by this means increaſed in weight, in inflammable air, the iron loſt its additional weight, and water was copiouſly produced, as in the ſame proceſs with finery cinder, or, as I ſometimes call it, ſcale of iron.

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As nitrous air may be deprived of its water, and become phlogisticated air by heating iron in it; I find that it undergoes the same change by being repeatedly transmitted through hot porous earthen tubes, through which I some time ago discovered that vapour will pass one way, while the air contiguous to the heated tube will pass the other.

I first tried this process with turnings of iron in the tube, by which means the iron was readily converted into finery cinder; but afterwards I found that the same change was produced in the nitrous air by the hot tube only. The two bladders which I made use of in this experiment (and by the alternate pressure of which I made the air contained in them pass through the hot tube) became *red*, just as any bladder does that is filled with nitrous air, and then exposed to the influence of the atmosphere till it becomes phlogisticated air, as may be seen in my former experiments. In this manner I now always treat the bladders in which I make experiments on air. It prevents them from putrefying, and gives them a firmness of texture similar to tanning.

That nitrous air contains water, and that this water can contribute to the formation of fixed air, is evident from the following experiment. I heated five grains of charcoal of copper in eight ounce measures of nitrous air, till it was increased to ten

ounce measures, and the charcoal had lost one grain. Examining the air, I found about one *fifth of it to be fixed air, and the remainder* phlogisticated. It seems, therefore, that nitrous air consists of water, and something that may be called the basis of nitrous acid, or that substance which, when united to dephlogisticated air, will make nitrous acid; and this seems to be pure phlogiston, since it is found, as the preceding experiment shews, in the purest inflammable air. May we not hence infer, that the nitrous is the simplest of all the acids, and perhaps the basis of all the rest?

It is evident, that more water than enters into the composition of nitrous air is necessary for the change of it into what I have called *dephlogisticated nitrous air*, because the contact of iron will not, without water, produce that change in it.

Though fixed air, as I have shewn, contains water as well as nitrous air, it cannot be deprived of it, and be decomposed, by the same means; for I have heated iron in it by a burning lens, and have also made it pass repeatedly through a hot earthen tube containing turnings of iron, without producing any change in it.

That nothing is necessary to the formation of nitrous air besides phlogisticated nitrous acid and water, is evident from the production of it by the impreg-

impregnation of pure water with phlogiſticated nitrous vapour, formed by the rapid ſolution of biſmuth; an experiment which I have mentioned before. However, to make it in a more unexceptionable manner, I interpoſed a glaſs veſſel between that in which the ſolution was made, and that in which the water to be impregnated with the phlogiſticated vapour was contained; that whatever of the ſolution was diſtilled over by the heat of the proceſs (which evidently occaſioned the dropping of a blue liquor from the end of the tube through which the vapour was delivered) might be prevented from reaching the water. In theſe circumſtances, however, when nothing but the dry phlogiſticated vapour could enter the water, it began to ſparkle, and yield nitrous air very copiouſly, as ſoon as it had acquired a blue tinge from the impregnation. This is a very ſtriking experiment to all perſons when they have ſeen it the firſt time.

Nitrous air is alſo produced by pouring a highly coloured, or phlogiſticated, nitrous acid into pure water, in which no metal or earthy matter is any way concerned.

SECTION VI.

Of the Decompoſition of nitrous Air by heating in it Iron or Charcoal.

NITROUS air ſeems to contain more water than phlogiſticated air. For when iron is heated it, which, by getting water in this proceſs, becomes finery cinder, the nitrous air is converted into phlogiſticated air, as will be ſeen more diſtinctly hereafter. Hot charcoal has a ſimilar effect upon it.

I dropped a piece of red hot charcoal into a phial of nitrous air, and immediately inverting it in a baſon of mercury, the air was preſently reduced to one fifth of the whole. Thus it continued two months, without any ſenſible change; after which I found that the air that remained unabſorbed did not affect common air, nor did the air that was emitted by the charcoal, when it was plunged in water; ſo that, in both theſe caſes, the air ſeems to be intirely deprived of its peculiar properties, and to become mere phlogiſticated air.

Having heated *iron* in nitrous air, I proceeded to heat in the ſame air a piece of *charcoal*, not long

long after it had been ſubjected to a ſtrong heat covered with ſand. The ſun not ſhining immediately after the charcoal was introduced into the veſſel of air (through the mercury, by which it was confined) part of the air was abſorbed; but on heating the charcoal the quantity was ſoon increaſed. Having continued the proceſs as long as I thought neceſſary, I examined the air, and found it to be about as much as the original quantity of nitrous air, but it was all phlogiſticated air, extinguiſhing a candle, and having no mixture of fixed air in it. At one time, when I dipped a lighted candle into it, I thought there was an appearance of its containing ſomething inflammable, but it was in the ſlighteſt degree imaginable. There was, however, evidently ſomething inflammable in the air expelled by water from this charcoal afterwards.

When I heated *charcoal of copper* in nitrous air, there was a ſmall addition to it, of perhaps a twentieth part; and being examined, it was found to contain two ſevenths of its bulk of fixed air, and the reſt was phlogiſticated. This change in four ounce meaſures of the air was produced by the loſs of between one fourth and one half of a grain of the charcoal, and the copper was evidently revived.

That fixed air ſhould be produced in this experiment, and not in the preceding, is rather extraor-

dinary. In this circumſtance it reſembles the experiment on the heating of charcoal of copper in dephlogiſticated air.

It is ſomething remarkable that heating the *ſcales of iron* in nitrous air, has the ſame effect as heating iron itſelf in it, but then much more *time* is requiſite to produce the ſame effect.

I heated a quantity of ſcales of iron in nitrous air till two ounce meaſures and ſeven tenths were reduced to one ounce meaſure and thirty five hundredth parts, and then found it to be wholly phlogiſticated air, extinguiſhing a candle, and neither affecting common air, nor being affected by freſh nitrous air. During the proceſs the ſurface of the ſcales on which the focus of the lens fell were affected in a very peculiar manner, as if they were ſuperficially melted, the parts being in a ſtate reſembling that of ebullition, which continued a long time on the ſame ſpot. No moiſture was produced in the proceſs, as is the caſe when theſe ſcales are heated in inflammable air.

When I put the ſcales which had been uſed in this experiment into diluted oil of vitriol, it became white like milk, and very little air was produced, even by means of heat. I got, however, a few bubbles; ſo that with good management I could juſt be ſure that it was not fixed air, that it did not affect nitrous air, nor was affected by it.

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SECTION VII.

Of volatile Alkali procured by Means of nitrous Air.

HAVING, for the purpoſe of producing a large quantity of that kind of nitrous air in which a candle burns with an enlarged, or with a vivid flame, filled a large jar with pieces of iron wire, and having repeatedly poured upon them a diluted ſolution of copper in the nitrous acid, at length a thick incruſtation was formed upon them; and having no occaſion to make uſe of the jar for ſeveral months, I took no notice of it till I found the jar was burſt by the ſwelling of that ſaline incruſtation.

The ſubſtance of this matter was generally red, being the calx of iron; but there was mixed with it a quantity of *green matter*, which, when broken had a ſtrong ſmell of volatile alkali. I then doubted whether this aroſe from any of the materials I have mentioned, or from ſomething elſe which had got into the jar, unknown to me. If the former were the caſe, which, however, at that time, I could hardly ſuppoſe, I thought it to be not a little remarkable; but I have ſince had another opportunity

tunity of obſerving the ſame fact; having examined a ſecond jar filled with iron wire, which had been treated in the ſame manner, and found the ſame ſtrong ſmell of volatile alkali. Alſo I now the leſs wonder at this fact, which puzzled me ſo much at the firſt, as I find, in Mr. Keir's very valuable notes to his tranſlation of Mr. Macquer's chemical dictionary, that volatile alkali has been found in many earthy ſubſtances, and amongſt others, in *ruſt of iron diſtilled.*

It ſhould ſeem that, in general, the calces of metals contain leſs phlogiſton than the metals themſelves; and for this reaſon I was originally led to conclude, that nitrous air expoſed to iron, which is evidently turned to *ruſt*, or a *calx* in it, had received phlogiſton from the metal; and I therefore termed the nitrous air that had been ſo treated *phlogiſticated nitrous air.* I now think it moſt probable that this ruſt of iron contains more phlogiſton than the iron itſelf, and that the nitrous air, in which, after this proceſs, a candle burns better than in common air, is properly termed *a dephlogiſticated nitrous air*, having parted with its phlogiſton to the iron.

Having found that iron which had long ruſted in nitrous air, gave out a ſtrong ſmell of volatile alkali, I afterwards repeatedly obſerved the ſame, and alſo ſome other curious particulars relating to the experiment.

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In all the caſes in which I had before obſerved this ſmell of volatile alkali, the iron had been expoſed to the nitrous air a very long time. But this effect is produced much ſooner by putting the iron into a weak ſolution of copper in the nitrous acid, ſuch as remains after making nitrous air, and ſuch as I now commonly uſe in order to procure dephlogiſticated nitrous air. A phial containing ſome of this iron, which had been uſed not more than once for this laſt mentioned purpoſe, having been kept cloſe corked about two months, was accidentally broken; when ſome pieces of the iron were found covered with a green ruſt, and theſe had a ſtrong ſmell of volatile alkali.

Time, however, is requiſite to produce this effect. I put ſome of this iron (which was of the turnings) moiſt, from a ſolution of copper in the nitrous acid, into a phial; and ſtopping it cloſe, left it to ſtand with its mouth immerſed in water. At that time it had no ſmell, nor had it any a month afterwards; but examining it again after two months, the ſmell of volatile alkali was pretty ſtrong.

Having, in an early period of my experiments, been much ſtruck with the change of nitrous air, in conſequence of the contact of iron, I kept ſeveral large bottles filled with ſmall pieces of iron, which I occaſionally ſupplied with nitrous air. The

The bottles ſhould be ſtrong ones, becauſe the iron in ruſting ſwells, and is apt to break the phials, though I do not know that this ever happened except when the iron was quite dry. The conſequence of expoſing nitrous air in this manner had always been, in the firſt place, a diminution of ſomething more than one third of the quantity of it, in which ſtate a candle would burn in it with an enlarged flame; and when well waſhed in water it would be farther diminiſhed about one half, when the remainder would be phlogiſticated air. I had never obſerved any *increaſe* of air in theſe bottles, though ſome of them had been uſed a longe time. But having neglected one of them about a year (though the mouth of it had always been kept immerſed in water) I caſually obſerved, that bubbles of air occaſionally iſſued from it. This ſurprized me very much, as I imagined it had not been half filled with air; and placing a veſſel to receive theſe bubbles, in about a week or ten days, I got about the quantity of an ounce meaſure. It contained no fixed air, but was all ſtrongly inflammable, and when I analized it, it appeared to be of that kind into which alkaline air is converted by heat.

It ſeems as if the phlogiſticated air which is ultimately produced in this proceſs, had been decompoſed in theſe circumſtances, ſo as to form this

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inflammable air, but on what principle this ſhould take place, I cannot imagine.

In order to make a compariſon between the inflammable air, produced from this bottle, and that which had been made from alkaline air, I exploded equal meaſures of each of them with equal quantities of the ſame dephlogiſticated air, the ſtandard of which was 0.3. When I made the exploſion with the inflammable air from alkali, the firſt diminution was to 1.2, and with that from the bottle it was 1.1. Of the former reſiduum the ſtandard was 1.1; and of the latter 0.98. Neither of them contained any portion of fixed air. On the whole, the reſemblance between theſe two kinds of air was ſo great, and the difference between them ſo ſmall, that they may be concluded to have had the ſame origin, and the ſame conſtitution.

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SECTION VIII.

General Obſervations on the conſtituent Principles of nitrous Air.

THERE is no kind of air, the conſtituent principles of which ſeemed to be more clearly aſcertained than thoſe of nitrous air, by thoſe philoſophers who admitted the doctrine of phlogiſton. We took it for granted, that it conſiſted of nitrous acid and phlogiſton. In an early period of my experiments I endeavoured to aſcertain the quantity of acid contained in it. Mr. Kirwan has done the ſame, and Mr. Cavendiſh alſo has conſidered it as containing a very concentrated nitrous acid. I had no doubt on the ſubject till I read the work of Mr. Metherie, who aſſerts that nitrous air contains no proper nitrous acid, but only one of the elements of it, the other being dephlogiſticated air, which had before been conſidered by Mr. Lavoiſier as the principle of all *acidity*.

Among other obſervations in ſupport of his aſſertion Mr. Metherie has the following. Nitrous air burned together with inflammable air, produces no nitrous acid, p. 145. Though nitrous air is obtained from

from a ſolution of mercury in nitrous acid, almoſt all the acid is found in the ſolution, p. 146. Nitrous air abſorbed by marine acid, does not make aqua regia; p. 153. He is of opinion that a ſmall portion of the nitrous acid being decompoſed, furniſhes a pure air, ſo altered that, uniting with inflammable air, it changes it into nitrous air, p. 150.

As to this *hypotheſis* of Mr. Metherie, I cannot ſay that I well underſtand it; but, being ſtruck with his *obſervations*, I began to review the experiments which I had formerly made on this kind of air, and could not recollect any of them in which it appeared that real nitrous acid was produced, except when nitrous air was combined with dephlogiſticated air, beſides the experiment in which it was decompoſed by the electric ſpark, which furniſhes a ſtrong objection to this hypotheſis. In every other view Mr. Metherie's theory appeared to me not improbable, and led me to make the following experiments, which though not deciſively in favour of it, may deſerve conſideration. They will at leaſt ſhew that, if there be all the elements of nitrous acid in nitrous air, they muſt, together with an additional portion of phlogiſton, exiſt in the phlogiſticated air that is formed in the proceſſes.

When nitrous air is decompoſed by iron, or by a mixture of iron and ſulphur, I have obſerved that the

the water over which the proceſs is made, acquires no acidity by it; but I had ſuppoſed that all the acid had been ſeized by the *iron*. Having by me a conſiderable quantity of this iron, which had been reduced to a perfect ruſt in nitrous air, and which I knew muſt have imbibed more than its weight of this air, I thought that I might get the acid out of it by diſtillation, if it contained any. But a quantity of this ruſt of iron, carefully diſtilled in an earthen retort, yielded neither nitrous air, nor nitrous acid, at leaſt in any quantity that could favour the common hypotheſis. For beſides the *air* (of which an account will be given in its proper place) ſome ounces of this ruſt yielded only a few drops of liquor, which was not ſenſibly acid to the taſte, and when poured into a little water, tinged blue with the juice of turnſole, barely turned it red. But ſo ſlight a degree of acidity as this might have come (and I have no doubt did come) from the union of nitrous air with thoſe bubbles of common air which could not be excluded from the veſſels containing the iron, in the many times in which I changed the air, when I ſupplied them from time to time with freſh nitrous air; ſo that the great maſs of nitrous acid which ought to have been depoſited in this iron muſt have been concealed in the phlogiſticated air, which is generally about one fourth of the original quantity of nitrous air.

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This brown ruſt (become ſo by expoſure to the air; which it imbibes) became black by the expulſion of its air and water, and exactly reſembled that iron ore which is found in the form of a black ſand.

I then endeavoured to decompoſe nitrous air by heating iron in it with a burning lens; and in this proceſs I ſucceeded much beyond my expectation. For the air was preſently diminiſhed in quantity, while the iron became of a darker colour, was ſometimes melted into balls, and gained conſiderable weight. It had no appearance, however, of containing any nitrous acid, and yet water tinged with the juice of turnſole introduced into the veſſel, and agitated in it after the proceſs, never changed its colour.

In the firſt experiment the original quantity of nitrous air was diminiſhed to about one third, and after this it was increaſed. But then I found that when I had got to the extreme of diminution, there began to be an addition made of inflammable air from the iron; for this was evidently ſo. It was alſo obſervable that there was either ſome pure air, or dephlogiſticated nitrous air, in what remained of this proceſs. For when a meaſure of this reſiduum was mixed with an equal meaſure of common nitrous air, they were reduced to 1.7. I ſuſpect, however, that this was owing to a quantity of dephlogiſticated nitrous air being formed by this pro-

cefs, as it is fimilar to the rufting of iron in nitrous air.

In another procefs of this kind, I reduced fix ounce meafures of this air to three, and this was wholly phlogifticated air, extinguifhing a candle. I alfo heated iron in feven ounce meafures and a half of nitrous air, till it was reduced to 3.7 meafures, when it appeared to be perfectly phlogifticated air. The iron had then gained two grains and a quarter in weight, and at this time I obferved that there was not the leaft appearance of *moifture* produced in the courfe of the experiment.

Then recollecting that, in my former experiments, Homberg's pyrophorus had taken fire in nitrous air, I made a quantity of this fubftance, and repeated the experiment with the greateft care. But when nitrous air was decompofed by this means, I faw no reafon to think that any nitrous acid had been formed. In this procefs the nitrous air is readily diminifhed to about one half of its bulk, after which nothing nitrous is found in it, but a portion of dephlogifticated nitrous air; as appears by its being much more readily abforbed by agitation in water than nitrous air is. But neither the air, nor any thing that can be expelled from the pyrophorus, difcovers any fign of containing nitrous acid.

Having faturated a quantity of pyrophorus with nitrous air in an earthen tube, which by this means became

became very hot, I admitted water to it; and distilling the pyrophorus with this water, the liquor that came from it would not change the colour of the juice of turnsole; and the air which came afterwards was of the same kind with that which is always expelled from pyrophorus after it has been burned in the open air, viz. partly fixed air, and partly inflammable.

I shall just mention some farther particulars of another experiment of this kind, though it was made long before I had the views which led to those now mentioned. Having saturated about two ounce measures of Homberg's pyrophorus with nitrous air. I observed that it had imbibed about four times its bulk of the air. This pyrophorus was of a kind that did not burn readily in the nitrous air, or I believe in common air, without external warmth. Having withdrawn it from the nitrous air, it took fire again, but with more external heat. Then putting it into an earthen retort, I expelled from it ninety ounce measures of air, a very small proportion of which was fixed air. The former portions of it extinguished a candle, and were of the standard of 1.7, and the latter of 1.55, burning with a lambent blue flame. Three days after this, being taken out of the retort (the mouth of which had been all the time immersed in mercury) a small part of it took fire. I observed it then weighed 416 grains, and the next morning it weighed 438 grains.

There is ſomething very remarkable in this ſubſtance burning equally well in two ſuch very different kinds of air as nitrous and dephlogiſticated.

In my former experiments I found that the juice of turnſole became red by being impregnated with nitrous air. I found, however, that this muſt have been owing to ſome extraneous acid that was mixed with it. For when I took nitrous air that had ſtood long in water, and transferred it into a veſſel of mercury, I impregnated with it water tinged with the juice of turnſole, without producing any change in its colour.

I have alſo obſerved that alkaline air admitted to nitrous air, made a whitiſh cloud in it. But this muſt have been owing to the ſame cauſe; the nitrous air in that experiment having been probably freſh made. For in the nitrous air which had been long confined by water, the mixture of alkaline air, did not produce the leaſt cloud. I perceived, however, that after the two kinds of air has been together a day or two, there was a ſmall quantity of a very deeply tinged blue liquor in the ſpace between the mercury and the glaſs; which ſhows that there was copper diſſolved in the nitrous air, and that it continued there after the extraneous acid was ſeparated from it. On this copper the volatile alkali had ſeized.

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Notwithſtanding theſe reſults, when the electric ſpark was taken in the very ſame nitrous air, with which the preceding experiments were made, the juice of turnſole by which it was confined, preſently became red. This circumſtance deſerves to be examined with care, as it ſeems to be deciſively in favour of the nitrous air containing all the neceſſary elements of nitrous acid.

PART IV.

OF DEPHLOGISTICATED NITROUS AIR.

SECTION I.

Of the Discovery of dephlogisticated nitrous Air, by exposing Iron to nitrous Air.

AS fixed air united to water dissolves iron, I had the curiosity to try whether fixed air alone would do it; and as I then supposed nitrous air to be of an *acid* nature, as well as fixed air, I, at the same time, exposed a large surface of iron to both the kinds; first filling two eight ounce phials with nails, and then with quicksilver, and after that displacing the quicksilver in one of the phials by fixed air, and in the other by nitrous air; then inverting them, and leaving them with their mouths immersed in basons of quicksilver.

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In these circumstances the two phials stood about two months, when no sensible change at all was produced in the fixed air, or in the iron which had been exposed to it, but a most remarkable, and most unexpected change was made in the nitrous air; and in pursuing the experiment, it was transformed into a species of air, with properties which, at the time of my first publication on this subject, I should not have hesitated to pronounce impossible, *viz.* air in which a candle burns quite naturally and freely, and which is yet in the highest degree noxious to animals, insomuch that they die the moment they are put into it; whereas, in general, animals live with little sensible inconvenience in air in which candles have burned out. Such, however, is nitrous air, after it has been long exposed to a large surface of iron.

It is not less extraordinary, that a still longer continuance of nitrous air in these circumstances (but *how long* depends upon too many, and too minute circumstances to be ascertained with exactness) makes it not only to admit a candle to burn in it, but enables it to burn with an *enlarged flame*, by another flame (extending every where to an equal distance from that of the candle, and often plainly distinguishable from it) adhering to it. Sometimes I have perceived the flame of the candle, in these circumstances, to be twice as large as it is naturally, and sometimes not less than five or six times larger;

and yet without any thing like an *explosion*, as in the firing of the weakest inflammable air.

Nor is the farther progress in the transmutation of nitrous air, in these circumstances, less remarkable. For when it has been brought to the state last mentioned, the agitation of it in fresh water almost instantly takes off that peculiar kind of inflammability, so that it extinguishes a candle, retaining its noxious quality. It also sometimes retains its power of diminishing common air in a very great degree.

Nitrous air treated in the manner above mentioned, is diminished about one fourth by standing in quicksilver; and water admitted to it will absorb about half the remainder; but if water only, and no quicksilver, be used from the beginning, the nitrous air will be diminished much faster and farther; so that not more than one fourth, one sixth, or one tenth of the original quantity will remain.

The water which has imbibed this nitrous air exposed to iron is remarkably green, also the phial containing it becomes deeply, and, I believe, indelibly tinged with green; and if the water be put into another vessel, it presently deposits a considerable quantity of matter, which when dry appears to be the earth or ochre of iron.

Instead of using *iron*, I introduced a pot of *liver of sulphur* into a jar of nitrous air, and presently found, that what I had before done by means of iron

iron in ſix weeks, or two months, I could do by liver of ſulphur in leſs than twenty four hours, eſpecially when the proceſs was kept warm.

When the proceſs is in water with iron, the time in which the diminution is accompliſhed is exceedingly various; being ſometimes completed in a few days, whereas at other times it has acquired a week or a fortnight. Some kinds of iron alſo produced this effect much ſooner than others, but on what circumſtances this difference depends I do not know.

SECTION II.

Of dephlogisticated nitrous Air, produced by the Solution of Metals in nitrous Acid.

THE solution of iron in spirit of nitre is known to produce nitrous air; but when all the nitrous air is produced in this manner, without foreign heat, if a candle be applied to the solution, more air will be procured; and this will be possessed of the peculiar kind of inflammability mentioned in the preceding section. But I first met with this air by dissolving iron in water, first impregnated with vitriolic acid air, and then with nitrous vapour, as will be mentioned in its proper place. The first produce of this solution was nitrous air; but applying the flame of a candle, when no more air could be got without it, air was produced in which a candle burned with a natural flame.

It was upon observing this, that I tried the same experiment after the termination of the common solution of iron in spirit of nitre; when, by this means, I got, by a direct process, such a kind of air as I had brought nitrous air to be by exposing it to iron, or liver of sulphur; for on the first trial a candle

burned in it with a much enlarged flame. At another time the application of a candle to air produced in this manner, was attended with a real, though not a loud *explosion*; and immediately after this, a greenish coloured flame descended from the top to the bottom of the vessel in which the air was contained. In the next produce of air from the same process the flame descended *blue* and very rapid, from the top to the bottom of the vessel.

When nitrous air is produced from iron, the quality of it, I doubt not, is always the same, though there is a case in which I do not wonder that some persons have been deceived; having got phlogisticated air when they expected nitrous; not considering, that exposure to a large surface of iron decomposes nitrous air, as my former experiments shew; changing it at first into a species of air in which a candle will burn, and then into phlogisticated air. This process, however, required a considerable time; but the following experiment shews that this effect may be produced very soon.

I filled an eight ounce phial with small nails, then with water, into which I put a very small quantity of nitrous acid, just enough to make it produce air; and then some was yielded in which a candle went out. Also this acid water poured off from the iron, gave a considerable quantity of air, in the heat

heat of boiling water, and this was all phlogisticated air.

Using more spirit of nitre, I observed, that, though the production of air was pretty copious, as was manifest by the bubbles formed at the bottom of the phial, and rising to the top, there was no increase of the whole quantity of air in the phial. Examining the air in this state of the process, I found that it had very little power of diminishing common air, and that a candle burned in it with a vivid flame, which is the intermediate state of this air, before it becomes phlogisticated air. I imagine, therefore, that in all these cases, a proper nitrous air is first produced, and that it is afterwards, by means of the iron to which it is exposed, changed into that species of air in which a candle can burn, and lastly into phlogisticated air, which extinguishes a candle.

I have also procured this kind of air in a direct process by the solution of *zinc* and *tin*.

I completely saturated a quantity of spirit of nitre with zinc. This solution, while it was hot was fluid, but when it was cold it resembled a whitish jelly. Distilling this jelly to dryness, in a glass vessel, and receiving the air that came from it in three parts, I observed that the first was the common air in the phial, very little phlogisticated; which

which ſhews that, after the common ſolution of this metal, without additional heat, very little phlogiſton remains in the ſolution. The ſecond portion was twice as good as common air, and the third was as pure as any that I had ever met with.

At another time that I repeated this experiment, I received the air in more portions; but the general reſult was the ſame, except that between the phlogiſticated and the pure air, there was a quantity of that kind of nitrous air in which a candle burns with an enlarged flame. That it was preciſely the ſame kind of air that was procured by the direct ſolution of zinc deſcribed above was evident, becauſe after that *vapour* (which is always the cauſe of this peculiar phenomenon) was waſhed out of it, it neither affected common air, nor was affected by nitrous air; being the ſame thing with that which I have termed *phlogiſticated air*, and which is alway the baſis of this peculiar kind of nitrous air.

This remarkable fact ſeems to prove that this ſpecies of air contains leſs phlogiſton, in proportion to its bulk, than phlogiſticated air, and conſequently that the vapour which is the cauſe of this peculiar property, and which is readily imbibed by water, partakes chiefly of the nature of the nitrous acid. For, in all theſe proceſſes, every produce of air has

leſs

leſs and leſs phlogiſton, till we came to the laſt, which contains the leaſt poſſible. Alſo this kind of air coming between the phlogiſticated and the pure air ſeems to ſhew that, if that which conſtitutes the enlarged flame be phlogiſton contained in the air itſelf (and indeed it can hardly admit of any other ſuppoſition) it muſt be in a ſtate not perfectly combined with that nitrous acid vapour. We ſee alſo that this diffuſion of phlogiſton, and the looſening of its connexion with the other conſtituent parts of the air is that ſtate of it which immediately precedes its total diſappearing. For immediately after this kind of air, we find the next produce to have no phlogiſton at all.

Leſt this intervention of the dephlogiſticated nitrous air between the other kinds of air, and the pureſt of all, ſhould have been occaſioned by ſome accident, I carefully repeated the experiment, and found it in the ſame place a ſecond time. Having diſſolved a quantity of zinc in a quantity of one fourth ſpirit of nitre and three fourths water, diſtilling the ſolution to dryneſs, and receiving the produce in four portions, the firſt was only the common air expelled from the phial, and not ſenſibly injured; the ſecond was *dephlogiſticated nitrous air* (as we may call that kind of nitrous air in which a candle burns with an enlarged flame) the third was

was dephlogisticated air, about four times as pure as common air; and the fourth was as pure as any that I have ever examined.

In order to ascertain the quantity of air produced by the solution of zinc in spirit of nitre, in a more satisfactory manner than I had been able to do at first, I suspended a lump of zinc within the mouth of a large phial, in the bottom of which was a quantity of strong spirit of nitre; and when I had placed them under the receiver of an air-pump, I carefully exhausted the air, and then, by means of the apparatus for a collar of leathers, I let down the zinc into the acid. Immediately upon this there was a most prodigious effervescence; but, the vessel into which it was made being very large, no part of the fluid escaped out of it.

By this means I got a very considerable quantity of air; and when I examined it, it appeared to be the very same thing with nitrous air exposed to iron or liver of sulphur, and such as I had got in the last-mentioned experiment by means of heat from the direct solution of iron; and the great heat attending this effervescence might answer the same purpose as the foreign heat of the candle in the case of the iron, viz. expelling the acid with more force, and throwing it into a state into which it was not strictly combined with phlogiston.

The

The air thus procured diminiſhed common air about half as much as nitrous air would have done; owing, I ſuppoſe, to half of it being properly nitrous air, and the remainder *nitrous vapour* uncombined with phlogiſton, and diffuſed through the nitrous air. One third of this air was readily abſorbed by water, which is another circumſtance in favour of its containing an acid vapour; and a candle burned in it with an enlarged flame, almoſt twice as large as its own proper flame, very vivid, and ſurrounded with another flame of a blue colour. With a very ſlight agitation in water, this air extinguiſhed a candle; the water having, by this means, probably, abſorbed the acid vapour that was diffuſed through it.

In order to aſcertain the *quantity* as well as the quality of the air produced in this manner, I diſſolved four penny-weights of zinc in very ſtrong ſpirit of nitre, contained in a glaſs phial with a ground ſtopper, when I got about eight ounce-meaſures of air, which I took at twice; but neither part of this produce either affected common air, or was affected by nitrous air; and though part of it was abſorbed by water, it did not make lime water turbid. Alſo no candle would burn in it. But I did not examine this air till about two hours after it was generated, when a conſiderable part of it was abſorbed by water. Had I made a trial of

it

it immediately after it was produced, and before any part of it had been abſorbed by water, I make no doubt but a candle would have burned in it. For when I repeated the experiment, and tried both parts of the produce immediately, a candle burned with an enlarged flame in the former, and with a natural flame in the latter.

At other times, however, I have got nothing of the peculiar kind of air above-mentioned, but only ſtrong nitrous air from ſeemingly the ſame proceſs; and I was not able to determine on what circumſtances the different reſults depended. I once thought that I had got this dephlogiſticated nitrous air, when I uſed a very large quantity of ſpirit of nitre, much more than was neceſſary to diſſolve the zinc, but at other times this expedient failed me. When I had taken the air at different times, I found the laſt produce more nitrous than the firſt. But I once divided the produce into five parts, and found all of them nearly equally, and all very ſtrongly nitrous.

It will be ſeen that in the ſolution of *tin*, which I find to contain very little phlogiſton, this air is always produced, as will appear when I come to recite the phenomena attending the ſolution of this metal in the nitrous acid, among other metals that yield dephlogiſticated air.

When, after all the uncertainty of the preceding trials, I uſed only a very weak ſpirit of nitre in the ſolution of zinc (with a view to produce air in about the ſame quantity as it had been yielded by means of the water impregnated firſt with vitriolic acid air and then with nitrous vapour) I got no other than this kind of air in which a candle burned with an enlarged flame; and the air was of the very ſame kind from the beginning to the end of the proceſs.

I obſerved that when I had juſt diſſolved all the metals in ſpirit of nitre, *tin* yielded nitrous air as well as the reſt. But had I proſecuted the experiment, I ſhould have found that the proper nitrous air which it yields is in very ſmall quantity, and that the greateſt part of the produce is that kind of nitrous air in which a candle burns with an enlarged flame. This metal alſo yields no fixed air; but ſeveral of the circumſtances attending this ſolution, being pretty remarkable, and I having varied then pretty much, it may not be amiſs to recite ſome of them.

Tin did not diſſolve ſo as to yield air in ſtrong ſpirit of nitre, but was affected by it in the manner that Mr. Macquer deſcribes. With an equal quantity of water and ſpirit of nitre, it diſſolved with great violence, and five penny-weights ſix grains yielded about fifteen ounce meaſures of air, about one

one fifth of which was abſorbed by water, being the nitrous vapour, ſo often mentioned above, diffuſed through the air. Before this part was waſhed out, a candle burned in the air naturally; but when this part had been abſorbed by the water, a candle went out in it. The former part of the produce, which was three fourths of the whole, was but ſlightly nitrous; for two meaſures of common air and one of this occupied the ſpace of two meaſures and a half; and the laſt part was proper phlogiſticated air, neither affecting common air, nor being affected by nitrous air. Had I taken the produce in ſmall portions, the firſt produce would, I doubt not, have been proper nitrous air, as I had firſt found. I particularly obſerved that no part of this air, though readily abſorbed by water, made lime water turbid, ſo that it contained no fixed air.

After this, diſtilling the reſiduum to dryneſs, in a glaſs phial with a ground ſtopper, I got about twelve ounce meaſures of air, in four parts; of which a portion of each, and eſpecially at the firſt, was readily abſorbed by water, but without making lime water turbid. When this ingredient was waſhed out, the air which had lodged within the phial was phlogiſticated. Then the firſt portion of the proper produce of the materials was com-

mon air, the ſecond twice as good, and the remaining portions were exceedingly pure.

Putting more ſpirit of nitre upon the reſiduum, I got only about twice its bulk of air, and part of this was fixed air; and in all reſpects this proceſs ſeemed to reſemble that with flint. After this I weighed the reſiduum, and found it to be five pennyweights two grains.

The phenomena attending the ſolution of *tin* being ſo very remarkable, I repeated it ſeveral times, with a conſiderable variation of circumſtances; and the following appeared to me to be worth reciting. I diſſolved a quantity of tin in a quantity of ſpirit of nitre, diluted with an equal quantity of water; when the ſolution was at firſt very ſlow, but afterwards very rapid. In the courſe of the proceſs I ſeveral times took a ſmall quantity of the air to lime water, but it was not made in the leaſt degree turbid, though a ſmall part of the air was abſorbed by it.

I received the whole produce in four parts, and found that the firſt was ſo far nitrous, that two meaſures of common air and one of this occupied the ſpace of two meaſures and a half; the ſecond was a little more nitrous; in the third the ſame meaſures occupied the ſpace of two meaſures and a quarter; but in the laſt the air was leſs nitrous, the

the ſame mixture occupying the ſpace of two meaſures and three fourths. Towards the middle of the proceſs, the air was very white, and continued ſo a long time. N. B. In the firſt of the portions of air above-mentioned a candle burned very bright, and with a crackling noiſe; in the ſecond and third it burned with a large flame, and very ſilently, a blue flame being very diſtinctly perceived ſurrounding the central white flame. The fourth extinguiſhed a candle.

When I could get no more air without additional heat, I applied the flame of a candle, and afterwards of two candles; and then took about half as much air as I had got before, in ſix portions; of which the firſt was nitrous, in about the ſame degree as the laſt part of the former produce; the ſecond was ſo much nitrous, that two meaſures of common air and one of this occupied the ſpace of two meaſures and one ſixth; the third hardly affected common air at all; the fourth was twice as good as common air; and the fifth and ſixth portions, which were four times the quantity of the preceding, were highly dephlogiſticated. N. B. The third, fourth, and fifth portions were exceedingly turbid; but the laſt was quite clear, coming very ſlowly.

In theſe two proceſſes, with and without foreign heat, we ſee a pretty regular gradation from the moſt impure to the moſt pure kind of air.

SECTION III.

Of dephlogisticated nitrous Air produced in the Diminution of nitrous Air by Iron Filings and Sulphur, and also by Liver of Sulphur.

THE first remarkable diminution of nitrous air that I observed was occasioned by the fermentation of iron filings and sulphur, made into a paste with water. This process is attended with much heat, the diminution of the air is exceedingly rapid, and whenever I examined the air that remained, it always appeared to be simply phlogisticated air, neither affecting common air, nor being affected by nitrous air, and extinguishing a candle. And in this last-mentioned property it differed from nitrous air diminished by iron only, or by liver of sulphur, in which a candle burned with an enlarged flame before it was agitated in water. I have since, however, observed that nitrous air diminished by iron filings and sulphur does not really differ from that which is diminished by the other processes; but that this process being made in a large quantity of water, either the superfluous nitrous acid vapour, the superfluous phlogiston,

giſton, or both, were always abſorbed before the experiment was made. This I diſcovered by repeating the proceſs with more care and attention, in the following manner.

Having introduced a pot of iron filings and ſulphur into a large jar of nitrous air, I examined the ſtate of the air in all the ſtages of its diminution, from the time that the fermentation began till it could be diminiſhed no more by that proceſs. In order to get a ſmall quantity of the air, without moving the jar, or diſturbing the apparatus contained in it, I faſtened a ſmall phial, or a piece of glaſs tube, to the end of a ſtiff wire; and filling it with water, I put it up into the veſſel, with its mouth downwards; when, the water running out, it would neceſſarily be filled with the air of the jar, which I could then with the ſame eaſe withdraw, and examine.

Proceeding in this manner, I found that, in the laſt ſtage of the diminution of this air, and not before, a candle burned in it with an enlarged flame. This proceſs, therefore, exactly reſembles that with iron only or liver of ſulphur, only that in this caſe the air muſt be examined very ſoon, before the water can have had an opportunity to act upon it. For when I tried part of the ſame air the next morning (when, without any agitation, part of it had been abſorbed by water) it extin-

guiſhed a candle; having, in the mean time, become mere phlogiſticated air.

I transferred another quantity of nitrous air from quickſilver, in which the proceſs was made, to lime water, but it did not make it in the leaſt turbid. Indeed, but little of this air was abſorbed by water, and a candle burned in it only naturally, without any enlargement of the flame. It had been diminiſhed to about one third of its bulk in the quickſilver.

I ſhall now proceed to note other phenomena attending this proceſs, which the philoſophical reader will eaſily perceive may be of great uſe in the analyſis of the ſpecies of air, as well as help to explain the proceſs itſelf. In order to determine whether the *acid*, or *phlogiſton*, of the nitrous air (for I then thought it contained both) had been ſeized upon in this proceſs, I made it over a quantity of very pure water; thinking that, if it ſhould acquire any acidity, it would ſhew that the phlogiſton only had been ſeized; but that if it ſhould not have become acid, it would appear that the decompoſition had been effected by the acid vapour having been ſeized, and the phlogiſton leſt. And the reſult ſeems to determine this queſtion in favour of the latter ſuppoſition. For when the proceſs was over, I examined the water with the greateſt attention, and found not the leaſt appearance of acidity in

in it. It did not even turn the juice of turnſole red.

In the experiment above recited, concerning the diminution of nitrous air by iron filings and ſulphur over a quantity of pure water, I firſt obſerved a pretty large depoſit of a brownifh kind of matter, from the water over which this proceſs had been made. If this be really the *ochre of the iron* employed in it, it will ſhew that the earth of the metal had been volatilized by the heat of the fermentation.

That *liver of ſulphur* alſo decompoſes nitrous air by ſeizing upon its acid vapour ſeems to be proved by the following experiment. I put ſome pieces of liver of ſulphur to two quantities of nitrous air confined by quickſilver, and I obſerved that, in about ten hours, between one third and one half of the air in each of them was abſorbed. The next day I admitted water to one of them, but no part of the air was abſorbed, and after it had paſſed ſeveral times through water, a candle burned in it with an enlarged flame, crackling very much. Alſo a candle burned in the very ſame manner in the air contained in the other veſſel, which I had not made to paſs through water.

The water over which the decompoſition is made does not acquire the leaſt acidity, not even diſcoverable by the juice of turnſole. In this caſe, alſo, there

there was an *earthy precipitate*, exactly as in the proceſs with the iron filings and ſulphur.

I had at one time ſome ſuſpicion, that, as part of this kind of air is readily abſorbed by water, exactly like fixed air, it might be, in part, real fixed air, and precipitate lime in lime water. But in the preceding experiments, as well as upon other occaſions, I particularly attended to this circumſtance, without ever obſerving it to have any ſuch effect.

I endeavoured to decompoſe nitrous air by means of ſulphur only; making uſe of a burning mirror and quickſilver for that purpoſe, which, however, is not an eaſy operation.

By this means, continuing the proceſs a long time, I made the ſulphur not only melt, but alſo become very black, and ſmoke very much in the nitrous air; and, in conſequence of it, the air was conſiderably diminiſhed, and had loſt its power of diminiſhing common air in ſome degree. Had the operation been continued much longer, the diminution would, no doubt, have been more complete, and the nitrous air have become nothing more than phlogiſticated air, as in other ſimilar proceſſes. That this diminution alſo was effected by means of the abſorption of the nitrous vapour was pretty evident, becauſe the mercury was not ſenſibly affected by it. For if that had been the caſe, there would

would have been an acceſſion of more nitrous air from that ſolution.

I have mentioned a caſe, in which nitrous air, after having been expoſed to iron, became not only partially inflammable, admitting a candle to burn in it with an enlarged flame, but was even fired with an exploſion, like inflammable air from metals by oil of vitriol. I have ſince met with a more remarkable fact of the kind.

At the latter end of September 1778, I had put a pot of iron filings and ſulphur into a jar of nitrous air, which, in the courſe of ſeveral days, was diminiſhed by it in the uſual proportion. From that time till the beginning of December, it had continued without any change that I had perceived; but about that time, imagining it was increaſed in bulk, I took exact notice of the dimenſions of it, and preſently found that the quantity was certainly increaſing. Upon the whole, I concluded that it had increaſed about one ſixth of its bulk, from the ſtate of its greateſt diminution. On the 11th of December I examined it, and found it to be proper inflammable air, being fired with many exploſions when tried in the uſual manner, but they were not ſo vigorous as thoſe with freſh made inflammable air from iron and oil of vitriol.

After this, on the 12th of December, I put a pot of iron filings and ſulphur to another quantity of

nitrous air, and on the 4th of February following it had increaſed in bulk about one third, and then burned with exploſions like the former. But a quantity of nitrous air expoſed to the effluvium of liver of ſulphur, the very ſame time, never increaſed at all after the period of its utmoſt diminution and was mere phlogiſticated air.

SECTION IV.

Of dephlogiſticated nitrous Air, procured by Iron, and a Solution of Copper, in nitrous Acid.

AFTER many trials, I ſucceeded in a method of producing this air, when I expected quite a different reſult. As iron will precipitate copper from a ſolution of it in ſpirit of nitre, and I had obſerved air to be generated in this proceſs; I imagined that by putting iron into that ſolution of copper in ſpirit of nitre, I ſhould get more nitrous

nitrous air, and with leſs expence. But inſtead of this, I procured what I ſhould much more have wiſhed for, viz. this new ſpecies of nitrous air. But before I hit upon this, I ſucceeded in the method of changing *freſh nitrous air into this ſpecies of it, in a* remarkably ſhort ſpace of time.

Having at hand a phial filled with nails, which had often been employed in diminiſhing nitrous air, I filled it up with a diluted ſolution of copper in ſpirit of nitre, and left it all night. I then diſplaced the liquor by nitrous air, and in about two hours the whole quantity was diminiſhed one half, and a candle burned in the remainder with an enlarged flame.

In this experiment I collected no air from the iron itſelf; but now, with the view that I have mentioned above, I filled the phial containing the nails with the ſolution of copper in ſpirit of nitre, and inverting it, the next morning I found the phial full of air, and it was not proper nitrous air, as I had expected, but that ſpecies of it in which a candle burns. In this caſe it burned quite naturally, and without any enlarged flame.

The quantity of this peculiar ſpecies of nitrous air that may be procured in this manner, and from the ſame materials, viz. without changing either the iron or the ſolution of copper, is aſtoniſhing. I made however, a pretty full trial of it, in a jar which I had

I had thruſt full of long pieces of iron wire, on purpoſe to expoſe as much of the ſurface of the iron as I poſſibly could, to any kind of air that I ſhould afterwards put into it. This jar, which held about a pint and a half, I filled with the diluted ſolution of copper, and inverted it in a baſon of the ſame ſolution; when, at the firſt, the jar was quite filled with this air in a few hours.

However, having got as much of this air as I wanted, I obſerved that more air was ſtill generated; and inverting the jar every day, after I had filled it with the ſame ſolution of copper that had been expelled by the generated air the preceding day, it never failed to produce a jar full of that air every day for at leaſt a fortnight, beſides what eſcaped towards the beginning of the proceſs from the mouth of the jar, as ſoon as it was full, and which I never collected.

I had imagined that, in time, the quality of this air would change, but to the very laſt it yielded air of the ſame kind in which a candle burned, not only naturally, but in a very vivid manner. If, however, I ſuffered the air to continue long in the jar, I always found it to be phlogiſticated air; which, indeed, is the ſtate to which this ſpecies of air is always reduced by long expoſure in the ſame circumſtances in which it is generated; the water either

abſorbing

abſorbing the dephlogiſticated vapour, as it may be called, or this vapour getting ſaturated with phlogiſton.

It was ſomething remarkable, that a phial of nails which had often been uſed to diminiſh nitrous air, when filled with water only, yielded phlogiſticated air. I at firſt filled this phial with inflammable air, and had obſerved a conſtant addition to it. It required, however, the warmth of a fire, when the phial was filled with water, to make it produce any conſiderable quantity of phlogiſticated air. Theſe nails had probably a quantity of nitrous vapour mixed with their ruſt, which might continue to act upon them; and this air, in its naſcent ſtate, might be nitrous, but afterwards become phlogiſticated, according to the uſual courſe of this proceſs.

After I had written the above, I produced this kind of air much more readily than in the method there deſcribed, viz. by applying *heat* to the veſſel in which it is produced. But I ſhall firſt obſerve that, immediately after producing a quantity of this air, in the manner before directed, I filled the veſſel up with water inſtead of the ſolution of copper; and, in about two days, the veſſel (which was a phial, containing near a quart) produced about three ounce meaſures of air, and it was phlogiſticated air, extinguiſhing a candle.

Then

Then, pouring out the water, I filled it again with the ſolution of copper, and put into a pan of water, which I heated till it boiled; when, by means of a cork and bent glaſs tube, &c. I received from it about a quart of air, the firſt part of which was phlogiſticated (owing perhaps to its having been in a ſtate of yielding that air immediately before this proceſs) but afterwards it was a proper dephlogiſticated nitrous air, admitting a candle to burn in it quite naturally.

My original proceſs for procuring dephlogiſticated nitrous air requiring much time, I now always procure it by diſſolving turnings of iron in a dilute ſolution of copper in the nitrous acid (the ſame that remains from making nitrous air) mixing with it again about an equal quantity of water. Without this precaution, though the iron will at firſt be acted upon very ſlowly, yet the mixture will at length grow ſo hot, as actually to boil, and the proceſs will be exceedingly troubleſome. It will be neceſſary, however, previouſly to heat the ſolution of copper, in order to expel from it all ſuperfluous nitrous acid, though the keeping in it pieces of copper might anſwer the ſame purpoſe. Without one or other of theſe precautions, part of the air procured by diſſolving iron in it will be proper nitrous air; but with this precaution, the whole of it will be ſuch as

nitrous

nitrous air is reduced to by expoſing common nitrous air to iron, that is, one third of it will generally be abſorbed by water, and the remainder will be proper phlogiſticated air.

SECTION V.

Of the Separation of the pure dephlogiſticated nitrous Air from the phlogiſticated Air with which it was mixed, and the Properties of it.

HAVING, in the manner mentioned above, procured a mixture of dephlogiſticated nitrous air and phlogiſticated air, the former will be rendered pure by agitating the mixture in water, which will imbibe it, and leave the phlogiſticated part unabſorbed.

A given quantity of water, I find, will take up about one half of its bulk of this dephlogiſticated nitrous air; and it does it almoſt as readily as water imbibes fixed air. The water thus ſaturated with

the air muſt be heated, and then the dephlogiſticated nitrous air will be expelled pure, and may be received in veſſels containing mercury. I obſerved, however, that as this kind of air much reſembles fixed air, in its properties of being imbibed by water, and in being expelled from it again by heat, it does ſo alſo in this farther property, that all the air which has been actually incorporated with the water, will not be imbibed by water again. But the proportion of this part is much greater in this kind of air than in fixed air. For in this caſe I have found it to be as one to ten; whereas in fixed air it is as about one to thirty or forty. This reſiduum being examined, was found to be air partially phlogiſticated, the ſtandard of it being about 1.4, ſo that a candle will juſt burn in it; but the quality of this reſiduum is not always the ſame. The reſiduum of fixed air is generally much purer, viz. of the ſtandard of 1.0.

I obſerved, that when water fully impregnated with this dephlogiſticated nitrous air is expoſed to the atmoſphere, it very ſoon parts with it, much ſooner than water impregnated with fixed air.

I mixed pure deplogiſticated nitrous air with all the other kinds of air that I have made, but it did not unite with any of them; and no ſenſible change was made in *it* by any of them, or by any of them in it; except that dephlogiſticated air was injured, being

reduced from the ſtandard of 0.3 to 0.8. But this was, no doubt, by the addition of that part of this air which is not miſcible with water, and is conſiderably phlogiſticated.

In this pure kind of dephlogiſticated nitrous air a candle burned better than in common air, but not ſo well as I have frequently known it to do, as it is originally produced mixed with phlogiſticated air; which ſeems to ſhew that ſome change is made in it by being incorporated with water. When it was mixed with inflammable air, they were exploded together with violence, very much like a mixture of inflammable and dephlogiſticated air; but a mouſe died in it as quickly as in fixed air.

That it is a mixture of pure nitrous air that is the cauſe of this thin blue flame with which the central flame in my former experiments is ſometimes ſurrounded, is, I think, evident from the following experiment. Having a quantity of this air in which a candle burned with a ſtrong and bright flame, I mixed nitrous air with it, and then the candle burned in it with the uſual enlarged flame, a bluiſh flame ſurrounding the former.

In order to aſcertain more exactly the degree of purity to which I could bring this air, I impregnated a quantity of ſnow water with it, and expelling it again by heat, I found that only one ſixth of what had been expelled from the water, would not be imbibed by water again; ſo that, in this method,

it may be procured in a ſtate of conſiderable purity.

Having a quantity of air in this ſtate, and having found that a candle burned very well in it, I put a mouſe into it; but it would have died very ſoon if I had not withdrawn it. This was on the 17th of March, 1781. But on the 21ſt of the ſame month, I put another mouſe into the very ſame air, and was ſurprized to find that it continued perfectly at its eaſe five minutes. To be quite ſure with reſpect to the other properties of this air, I withdrew the mouſe while it was quite vigorous, and found that a candle burned very well in it, but it was not in the leaſt affected by nitrous air. In this very ſingular caſe, nitrous air fails to be a teſt of the reſpirability of air. The air made uſe of in this experiment had been kept in a cup of mercury, but ſome water was in the veſſel along with it, and this water had imbibed a little of the air. This, however, was the only caſe in which I have found a mouſe to live in this kind of air, and therefore, though I cannot account for it, I do not lay much ſtreſs upon it.

I then expelled more of this air, from ſome water that had been impregnated with it on the 17th day of March, but a mouſe died in it, and almoſt as ſoon as it would have done in any other kind of noxious air.

It was natural to ſuſpect, that there might be ſomething *acid* in this air. But though it was readily

dily abſorbed by water tinged blue with the juice of turnſole, it made no change in its colour. Neither was there any thing alkaline in it. For when the ſlighteſt tinge of red was given to this liquor by an acid, the blue colour was never in the leaſt degree reſtored by its impregnation with this kind of air.

The ſame thing may indeed be inferred from its not incorporating with either acid or alkaline air. In the experiment with the latter it appeared that this dephlogiſticated nitrous air is lighter than common air. For mixing it with alkaline air, and then putting acid air to it, the white cloud formed by their union was almoſt wholly in the lower part of the veſſel. But it is not ſo much lighter as inflammable air is found to be by the ſame experiment. For in this caſe the white cloud never reaches the top of the veſſel; where the inflammable air appears to remain intirely unmixed with the alkaline air.

Thinking that, though dephlogiſticated air ready formed might not affect, or be affected by, dephlogiſticated nitrous air, it poſſibly might in its firſt formation; to try this, I revived mercury from red precipitate in this air. But the operation proceeded juſt as it would have done in vacuo, the mercury being revived very readily, and dephlogiſticated air produced, without affecting, or being affected by, the dephlogiſticated nitrous air.

No leſs remarkable was the reſult of heating malleable iron in this air. For the bulk of the air was increaſed, and yet afterwards no part of it was imbibed by water. It was all phlogiſticated air, without any mixture of fixed air. It ſeems, therefore, as if this kind of air wanted nothing but phlogiſton to make it phlogiſticated air, and nothing but the principle of *heat*, to make it dephlogiſticated air.

This experiment happening to draw my particular attention, I repeated it very often, and always had the ſame reſult, though with variations, depending on the quantity of the air, and the heat I was able to apply. If this was ſufficient to melt the bits of iron, the effect was produced very quickly. As the particulars may be of uſe to future experimenters, I ſhall recite ſome of them.

Of three ounce meaſures and a half of dephlogiſticated nitrous air, two ounce meaſures remained unabſorbed by water after heating malleable iron in it. During the proceſs, the veſſel had been full of whitiſh fumes. What remained of the air was of the ſtandard of 1.7, and extinguiſhed a candle.

Throwing the focus of the lens about five minutes on ſome filings of iron in this kind of air, the quantity was increaſed one tenth. Afterwards water abſorbed about one ſixth of it, and the remainder extinguiſhed a candle, being of the ſtandard of 1.8.

I repeated

I repeated the experiment with caſt iron turnings, and had the ſame reſult. About one fifth was added to the quantity, water abſorbed about the ſame proportion of it, and the remainder was of the ſtandard of 1.85. Some little common air may be ſuppoſed to have been introduced into the veſſel along with the turnings or ſhavings of iron, but this could not be much; and in all theſe experiments the addition produced in the proceſs was a very ſmall part of what remained unabſorbed by water; ſo that there can be no doubt but that this operation produces a real change in the quality of the air, rendering it almoſt wholly phlogiſticated.

In order to produce this effect, the heat muſt be very conſiderable, or the pieces of iron very ſmall, ſo that they muſt at leaſt be red hot. For when I uſed whole caſt iron nails, on which I ſuppoſe the heat of my lens could not make ſufficient impreſſion, the effect of applying it an hour and a half, and with a very good ſun, was very inconſiderable; the proportion of that part of the air which was immiſcible with water, being only increaſed in the proportion of fifty to twenty ſeven.

But the circumſtance to which I happened to give the greateſt attention with reſpect to this kind of air, was the change that is made in it by *heat*, or *light*. For ſuch muſt be the agents, when bits of clean cruci-

bles, or retorts, are heated by a burning lens in any kind of air. The consequence of this process always was both an increase of that part of the air which will not unite with water, and also rendering it purer than before, approaching nearer to the state of atmospherical air; exactly like the result of taking the electric spark in fixed air. I frequently stopped in the middle of this process, and always found these effects to be more considerable the longer I continued it. I shall give the particulars of those experiments only, on which I place the greatest dependence.

Having impregnated a quantity of boiled water with dephlogisticated nitrous air, I found that the air expelled from it was again absorbed by water, in the proportion of 0.7 to 10.1, and the standard of this residuum was 1.7. I then heated in it pieces of crucibles which had been just before made red hot, introducing them into the air by means of a forceps; and after heating them an hour and a half, in a very hot sun, found that a portion of the air was then unabsorbed by water, in the proportion of four to seven, and the standard of the residuum was 1.55. I resumed the experiment with what remained of the air, and continued it half an hour longer, when the proportion of the part that was not absorbed by water, was to the other as twenty six to forty, and the standard of this residuum was 1.45. No moisture ever

ever appeared in the inſide of the veſſel, and there was no ſenſible change in the quantity of the air.

In a portion of the ſame dephlogiſticated nitrous air that was uſed in the former experiment, I heated bits of crucibles, which, after being dipped in water, had only been dried, though they had been made very hot in the ſun. In the courſe of this proceſs, much water appeared in the veſſel, expelled no doubt from the earthen ware; and after the proceſs had been continued an hour and a half, the part of the air that was unabſorbed by water, was to the reſt as 4.5 to 7, and the ſtandard of it was 1.45.

Examining a quantity of dephlogiſticated nitrous air, in a portion of which I was about to heat bits of dry crucibles over mercury, I found that by agitation in water 80.5 meaſures of it was reduced to 3.5 meaſures, and the ſtandard of this reſiduum was 1.58. But of that portion of the ſame air in which bits of crucibles had been heated about an hour in all, ſixty three meaſures were only reduced to twenty one, and the ſtandard of this reſiduum was 1.38. On this occaſion I obſerved that the inſide of the glaſs veſſel was pretty thickly coated with a black matter. Indeed, in the former experiments, it had always acquired a dark incruſtation; but I did not know whether it might not have come from the earthen ware. I think it highly probable, that it

was

was mercury ſuper-phlogiſticated, as every other black incruſtation in proceſſes in which mercury was uſed has been. It is probable, however, that ſome phlogiſticated matter paſſed from the air to the mercury, which would of courſe render the reſiduum more phlogiſticated, though I do not perceive how it ſhould follow that it ſhould thereby render it leſs miſcible with water. But whatever proportion of phlogiſton be neceſſary to make any kind of air miſcible with water, any *change* in that proportion may make it leſs miſcible with it.

In theſe reſpects this change in the conſtitution of dephlogiſticated nitrous air, very much reſembles that which is made in fixed air, both by the electric ſpark, and alſo by heating bits of crucibles in it; an account of which has been given before.

I endeavoured to aſcertain whether this change in the conſtitution of dephlogiſticated nitrous air was produced by mere *heat*, without light, by heating the air in earthen tubes or retorts, when their mouths were immerſed in mercury; but, to my great diſappointment, I found that all kinds of air heated in this manner, came out common air, or rather air a little phlogiſticated. In fact, the earthen veſſels, in that degree of heat to which I expoſed them, muſt have been rendered pervious to air, though they were glazed inſide and outſide, and were perfectly air tight both before and after the experiment.

But

But the moſt effectual change that I was able to make in this kind of air, was bringing it to a ſtate in which I doubt not it was reſpirable, by means of the electric ſpark. Taking this ſpark, or making ſmall exploſions, in 0.4 of an ounce meaſure of dephlogiſticated nitrous air, it was rendered wholly immiſcible with water, and of the ſtandard of 1.45.

From this experiment it ſhould ſeem, that there is in this ſpecies of air ſomething that is capable of being converted into pure air. This, indeed, is ſufficiently indicated both by a candle burning perfectly well in it, and alſo by pyrophorus ſpontaneouſly kindling in the nitrous air from which it is formed. A judicious proſecution of theſe experiments may therefore throw light upon the nature of both the kinds of air, and of combuſtion alſo.

This kind of air, however, though a candle burns ſo well in it, will not kindle pyrophorus. I put a ſmall cupful of the pyrophorus into a quantity of this kind of air, and let it remain there a conſiderable time, without perceiving any ſenſible effect; but when I took it out, and put it into a veſſel containing nitrous air, it took fire immediately.

In the courſe of theſe experiments with the ſun, I obſerved a remarkable ſource of fallacy, with reſpect to the increaſe of the quantity of air confined by mercury, when there is ſo much moiſture in the inſide

inſide as to be ſubject to ſudden dilations and compreſſions. For a conſiderable quantity of common air would get into the inſide of the veſſel, when there was the depth of an inch of mercury on the outſide of it, and of two or three inches within. In theſe circumſtances I have ſeen more than an ounce meaſure of the external air gain admiſſion in leſs than one minute. This muſt have been occaſioned by the mercury never being in perfect contact with glaſs; ſo that when the mercury was in a ſtate of undulation, the air that was confined between it and the glaſs was continually protruded, and more air from the atmoſphere was forced into its place, by the ſame preſſure which ſupported the column of mercury within the glaſs. This effect I prevented by having a quantity of water upon the mercury, on the outſide of the veſſel. For this would be in perfect contact with the glaſs; and in this caſe I never found either air or water to get into the veſſel to diſturb my experiment.

I took the electric ſpark in a quantity of *dephlogiſticated nitrous air*, till, without any perceivable change in its quantity, it was become immiſcible with water. Then, admitting diſtilled water to it, there was no depoſit made, though I could perceive a white incruſtation quite round the inſide of the glaſs veſſel, contiguous to the mercury by which the air was confined. But admitting to it the water in

in my trough, which happened to have mixed with it various ſolutions of iron, there was a copious white precipitate. This ſubſtance I had got once before, and found that by expoſure to a red heat it became of a brown colour, and adhered firmly to the pieces of earthen ware on which it was heated, thereby appearing to be ochre. The whiteneſs may have been occaſioned by the mercury.

SECTION VI.

Of Iron that has been uſed to diminiſh nitrous Air.

WHEN the iron nails or wires, which I have uſed to diminiſh nitrous air, had done their office, I laid them aſide, not ſuſpecting that they could be of any other philoſophical uſe; but after having lain expoſed to the open air almoſt a fortnight, having, for ſome other purpoſe, put ſome of them into a veſſel containing common air, ſtanding inverted, and immerſed in water, I was ſurprized to obſerve that the air in which they were confined was diminiſhed. The diminution proceeded

ceeded ſo faſt, that the proceſs was completed in about twenty four hours; for in that time the air was diminiſhed about one fifth, ſo that it made no effervescence with nitrous air, and was therefore, no doubt, highly noxious, like air diminiſhed by any other proceſs.

This experiment I repeated a great number of times, with the ſame phials, filled with nails or wires that have been ſuffered to ruſt in nitrous air, but their power of diminiſhing common air grows leſs and leſs continually. How long it will be before it is quite exhauſted I cannot tell. This diminution of air I conclude muſt ariſe from the phlogiſton, with which the iron was overcharged by extracting it from the nitrous air, which it thereby left dephlogiſticated. With this addition of phlogiſton it would powerfully attract the dephlogiſticated air in the atmoſphere, and conſequently leave the remainder phlogiſticated.

Having a quantity of iron that was reduced to a perfect *ruſt* in theſe proceſſes, I had the curioſity to heat it in an earthen crucible, in order to examine the *air* that might be expelled from it, and the reſult was as follows. From ten ounces of this ruſt, I got about 180 ounce meaſures of air, mixed at firſt with much ſteam. The firſt ſixty ounce meaſures were chiefly fixed air, with a phlogiſticated reſiduum, of the ſtandard of 1.7. The remainder

mainder came much more ſlowly, requiring about four hours continuance of heat. This contained no mixture of fixed air, and was ſo pure, that a candle burned in it pretty well, being of the ſtandard of 1.32. The matter was reduced to one maſs, like iron, ſtrongly attached by the magnet. But, indeed, the ruſt itſelf had been ſo before the proceſs.

It ſeems probable, that the dephlogiſticated nitrous vapour is contained in this ruſt, and that with the addition of the principle of heat it becomes proper dephlogiſticated air. Or this dephlogiſticated air might be that which by long expoſure to the atmoſphere it had extracted from it. It has been ſeen that that iron which has been expoſed to nitrous air will diminiſh common air, and leave it phlogiſticated.

In order to compare this kind of ruſt of iron with that which is formed by expoſure to the atmoſphere, I took four ounces of the latter; and, by the ſame treatment, got from it 180 ounce meaſures of air, about one half of which was fixed air, with a reſiduum of the ſtandard of 1.7; but the bulk of it was inflammable, burning with a lambent blue flame.

To a quantity of this ruſt I put oil of vitriol with only two equal parts of water, and obſerved that it was but weakly acted upon, and that much heat was neceſ-

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ſary to effect the ſolution of it. During the proceſs much *black matter* was formed, and the inflammable air produced had an offenſive ſmell. In theſe reſpects this iron ſeemed to approach to the nature of *caſt iron*; but when it was melted by a burning lens in the open air, the parts of it were not diſperſed like thoſe of caſt iron.

As yet I have ſeen no iron ſo far affected by this proceſs, though it is in a ſtate that eaſily crumbles to powder, as not to be able to decompoſe more nitrous air, and almoſt as readily as freſh iron.

I ſhall juſt mention one experiment in which this ruſted iron, and freſh iron, were made uſe of. The nitrous air had been put to each of them on the 27th of Auguſt, and they were both examined on the 18th of October; when, in both caſes, the air was found to be diminiſhed to about one third, and a candle burned with equal brilliancy in both. Water then abſorbed about one half of each of them, and the remainder extinguiſhed a candle. Neither of them had in the ſmalleſt degree the property of freſh nitrous air.

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SECTION VII.

Obſervations more particularly relating to the Conſtitution of dephlogiſticated nitrous Air.

MY conjectures concerning the conſtitution of this kind of air will be found to have been various. But having diſcovered it at one time in a continued proceſs, of the ſolution of zinc in ſpirit of nitre, always to come between the phlogiſticated and dephlogiſticated air, I ſuppoſed it to contain leſs phlogiſton than phlogiſticated air, and more than the dephlogiſticated air; and therefore as it has ſome properties of a genuine dephlogiſticated air, I am inclined, on the whole, to call it *a dephlogiſticated nitrous air.*

Since a candle burns in this kind of air, and an animal dies in it, I think we are authorized to ſay, that it is of ſuch a conſtitution, as to be capable of receiving phlogiſton in a very great degree of heat, perhaps not ſhort of a red heat, but not in that degree which is compatible with animal life. It is well known that many ſubſtances in chemiſtry can act upon one another, when they are hot, which do not at all affect one another when

they are cold. This, therefore, may be the caſe with this kind of air, and ſubſtances containing phlogiſton.

That this ſpecies of air is really a dephlogiſticated nitrous air, and not a phlogiſticated air, as I had originally ſuppoſed, is more evident from its being produced from nitrous air by the *ſcales of iron*, which fly off when it is hammered, and which are iron partly reduced to a calx. I filled a phial with theſe ſcales, and then filling it up with mercury, diſlodged the mercury with nitrous air. In this ſtate the phial continued near three weeks; when I found the air diminiſhed, I did not note how much, but a candle burned in the remainder juſt as if the nitrous air had been expoſed to iron. Theſe ſcales wanting much phlogiſton to make them iron, muſt be in a ſtate more ready to receive than to give phlogiſton to nitrous air.

Water is abſolutely neceſſary to this decompoſition of nitrous air, when it is effected by iron. I had decompoſed it before, either by previouſly filling the veſſels that were to contain the nitrous air with *water*, or with *mercury*, though it had always required much longer time when mercury was uſed. The reaſon of this difference I did not then know; but I now perceive that it was owing to the ſmall quantity of *moiſture* in the veſſel containing the mercury.

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Suſpecting the influence of *water* in this proceſs, I procured a number of very clean ſmall needles, and having made a phial, and alſo a proper quantity of mercury, perfectly clean and dry, I put the needles into the phial; and filling it up with mercury, introduced the nitrous air. This phial continued in the ſame ſtate ſix or eight months, without the leaſt ſenſible change in the ſpace occupied by the air. I therefore concluded that no change had taken place in it, and therefore introduced a few drops of water; after which I perceived, in a day or two, that the air was diminiſhed, and the diminution proceeded till about one third of the air had diſappeared. In this ſtate the phial continued ſeveral months more; when, obſerving no farther change, I examined it, and found that a candle would not burn in it, which I had expected it would have done; as I imagined there was not water ſufficient to imbibe that part of the air which had enabled a candle to burn in it; and how this part of the air ſhould diſappear, or be converted into phlogiſticated air, without water, I could not tell. In another experiment the reſult was ſeemingly different.

On the 26th of May, 1782, I examined a quantity of nitrous air, which had been confined by mercury with iron ſhavings from the 27th of Auguſt preceding, when I found one half of it abſorbed,

and a candle burned in the remainder better than in common air, though a mouſe died in it; and yet this air had remained ſeveral months in the ſame ſtate, with reſpect to quantity, in which it was when I examined it; ſo that I concluded it would never have extinguiſhed a candle, in conſequence of any longer continuance in that ſtate. In this caſe, however, there was no ſenſible quantity of water at all; whereas in the other there were a few drops. Water, therefore, may contribute to this change in the air, independently of its *abſorbing* any part of it. When I agitated this air in water, ſomething more than one half was abſorbed, and the remainder extinguiſhed a candle.

But, perhaps, the more probable inference from this experiment may be that, in time, all the dephlogiſticated nitrous air is imbibed by the iron, ſo that nothing beſides the phlogiſticated air will be left in the veſſel; though there was no appearance of this being the caſe in the experiment with the needles; as the air did not continue to diminiſh longer than uſual, and there remained two thirds of the original quantity of phlogiſticated air. This, therefore, rather looks like a change of the dephlogiſticated nitrous air into phlogiſticated air, by the addition of phlogiſton from the iron.

Another experiment I made with a view to aſcertain the degree of diminution at which the nitrous air

air begins to be decompoſed. On the 14th of May, I put twenty four ounce meaſures of nitrous air into a quart bottle, previouſly filled with pieces of iron, that had before been ruſted in this proceſs. On the 18th there were only ſeventeen ounce meaſures of air, and it was not at all changed; on the 21ſt there were thirteen ounce meaſures, and on the 31ſt four ounce meaſures, of which half was then abſorbed by water, and the remainder was phlogiſticated air. This was the greateſt diminution that I remember to have obſerved in this proceſs. But having neglected it from the 21ſt to the 31ſt, I did not find the time at which the decompoſition firſt took place; but in general I believe it is when about one fourth of the quantity is remaining, and that the change takes place pretty ſuddenly, for I do not remember ever to have found it in a middle ſtate.

BOOK IV.

OF DEPHLOGISTICATED AIR.

PART I.

OF THE SOURCES OF DEPHLOGISTICATED AIR.

SECTION I.

An Account of the Discovery of dephlogisticated Air, and its general Properties.

THE contents of this section will furnish a very striking illustration of the truth of a remark, which I have more than once made in my philosophical writings, and which can hardly be too often repeated, as it tends greatly to encourage philosophical investigations; viz. that more is owing to what we call *chance*, that is, philosophically speaking,

ing, to the obſervation of *events ariſing from unknown cauſes*, than to any proper *deſign*, or preconceived *theory* in this buſineſs. This does not appear in the works of thoſe who write *ſynthetically* upon theſe ſubjects; but would, I doubt not, appear very ſtrikingly in thoſe who are the moſt celebrated for their philoſophical acumen, did they write *analytically* and ingenuouſly.

For my own part, I will frankly acknowledge, that, at the commencement of the experiments recited in this ſection, I was ſo far from having formed any hypotheſis that led to the diſcoveries I made in purſuing them, that they would have appeared very improbable to me had I been told of them; and when the deciſive facts did at length obtrude themſelves upon my notice, it was very ſlowly, and with great heſitation, that I yielded to the evidence of my ſenſes. And yet, when I re-conſider the matter, and compare my laſt diſcoveries relating to the conſtitution of the atmoſphere with the firſt, I ſee the cloſeſt and the eaſieſt connexion between them, ſo as to wonder that I ſhould not have been led immediately from the one to the other. That this was not the caſe, I attribute to the force of prejudice, which, unknown to ourſelves, biaſſes not only our *judgments*, properly ſo called, but even the perceptions of our ſenſes; for we may take a maxim ſo ſtrongly for granted, that

the plaineſt evidence of ſenſe will not entirely change, and oſten hardly modify, our perſuaſions; and the more ingenious a man is, the more effectually he is entangled in his errors; his ingenuity only helping him to deceive himſelf, by evading the force of truth.

There are, I believe, very few maxims in philoſophy that have laid firmer hold upon the mind, than that air, meaning atmoſpherical air (free from various foreign matters, which were always ſuppoſed to be diſſolved, and intermixed with it) is *a ſimple elementary ſubſtance*, indeſtructible, and unalterable, at leaſt as much ſo as water is ſuppoſed to be. In the courſe of my inquiries, I was, however, ſoon ſatisfied that atmoſpherical air is not an unalterable thing; for that, according to my firſt hypotheſis, the phlogiſton with which it becomes loaded from bodies burning in it, and animals breathing it, and various other chemical proceſſes, ſo far alters and depraves it, as to render it altogether unfit for inflammation, reſpiration, and other purpoſes to which it is ſubſervient; and I had diſcovered that agitation in water, the proceſs of vegetation, and probably other natural proceſſes, reſtore it to its original purity. But I own I had no idea of the poſſibility of going any farther in this way, and thereby procuring air purer than the beſt common air. I might, indeed, have naturally imagined that ſuch would be air that ſhould contain

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leſs phlogiſton than the air of the atmoſphere; but I had no idea that ſuch a compoſition was poſſible.

In my firſt publication on the ſubject of air, I mentioned that which I had got from *nitre*, and the account I then gave of it demonſtrates it to have been dephlogiſticated air, but I had not purſued that experiment, nor was it of any uſe to me in the following courſe. It may be worth while, however, to recite what I then obſerved, which was as follows.

All the kinds of factitious air, I then obſerved, on which I had made the experiment, were highly noxious, except that which is extracted from nitre, or alum; but in this even a candle burned juſt as in common air. In one quantity which I got from nitre 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.

The air was extracted from theſe ſubſtances by heating them in a gun barrel, which was much corroded and ſoon ſpoiled by the experiment. What effect this circumſtance had upon the air I did not conſider.

At the time of my firſt publication, I was not poſſeſſed of a *burning lens* of any conſiderable force; and for want of one, I could not poſſibly make many of the experiments that I had projected, and which,

which, in theory, appeared very promiſing. I had, indeed, a *mirror* of force ſufficient for my purpoſe. But the nature of this inſtrument is ſuch, that it cannot be applied, with effect, except upon ſubſtances that are capable of being ſuſpended, or reſting on a very ſlender ſupport. It cannot be directed at all upon any ſubſtance in the form of *powder*, nor hardly upon any thing that requires to be put into a veſſel of quickſilver; which appears to me to be the moſt accurate method of extracting air from a great variety of ſubſtances, as was explained in the introduction to this work. But having afterwards procured a lens of twelve inches diameter, and twenty inches focal diſtance, I proceeded with great alacrity to examine, by the help of it, what kind of air a great variety of ſubſtances, natural and factitious, would yield, putting them into the veſſels repreſented fig. *a*, Pl. IV. which I filled with quickſilver, and kept inverted in a baſon of the ſame. Mr. Warltire, a good chemiſt, and lecturer in natural philoſophy, happening to be at that time in Calne, I explained my views to him, and was furniſhed by him with many ſubſtances, which I could not otherwiſe have procured.

With this apparatus, after a variety of other experiments, an account of which will be found in its proper place, on the 1ſt of Auguſt, 1774, I endeavoured to extract air from *mercurius calcinatus per ſe*;

ſe; and I preſently found that, by means of this lens, air was expelled from it very readily. Having got about three or four times as much as the bulk of my materials, I admitted water to it, and found that it was not imbibed by it. But what ſurprized me more than I can well expreſs, was, that a candle burned in this air with a remarkably vigorous flame, very much like that enlarged flame with which a candle burns in nitrous air, expoſed to iron or liver of ſulphur; but as I had got nothing like this remarkable appearance from any kind of air beſides this particular modification of nitrous air, and I knew no nitrous acid was uſed in the preparation of *mercurius calcinatus*, I was utterly at a loſs how to account for it.

In this caſe, alſo, though I did not give ſufficient attention to the circumſtance at that time, the flame of the candle, beſides being larger, burned with more ſplendor and heat than in that ſpecies of nitrous air; and a piece of red hot wood ſparkled in it, exactly like paper dipped in a ſolution of nitre, and it conſumed very faſt; an experiment which I had never thought of trying with dephlogiſticated nitrous air.

At the ſame time that I made the above-mentioned experiment, I extracted a quantity of air, with the very ſame property, from the common *red precipitate*, which being produced by a ſolution of mercury in ſpirit of nitre, made me conclude that this

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peculiar property, being ſimilar to that of the modification of nitrous air above mentioned, depended upon ſomething being communicated to it by the nitrous acid; and ſince the *mercurius calcinatus* is produced by expoſing mercury to a certain degree of heat, where common air has acceſs to it, I likewiſe concluded that this ſubſtance had collected ſomething of *nitre*, in that ſtate of heat, from the atmoſphere.

This, however, appearing to me much more extraordinary than it ought to have done, I entertained ſome ſuſpicion that the *mercurius calcinatus*, on which I had made my experiments, being bought at a common apothecary's, might, in fact, be nothing more than red precipitate; though, had I been any thing of a practical chymiſt, I could not have entertained any ſuch ſuſpicion. However, mentioning this ſuſpicion to Mr. Warltire, he furniſhed me with ſome that he had kept for a ſpecimen of the preparation, and which, he told me, he could warrant to be genuine. This being treated in the ſame manner as the former, only by a longer continuance of heat, I extracted much more air from it than from the other.

This experiment might have ſatisfied any moderate ſceptic; but, however, being at Paris in the October following, and knowing that there were ſeveral very eminent chymiſts in that place, I did not

not omit the opportunity, by means of my friend Mr. Magellan, to get an ounce of *mercurius calcinatus* prepared by Mr. Cadet, of the genuineneſs of which there could not poſſibly be any ſuſpicion; and at the ſame time, I frequently mentioned my ſurpriſe at the kind of air which I had got from this preparation to Mr. Lavoiſier, Mr. le Roy, and ſeveral other philoſophers, who honoured me with their notice in that city; and who, I dare ſay, cannot fail to recollect the circumſtance.

At the ſame time, I had no ſuſpicion that the air which I had got from the *mercurius calcinatus* was even wholeſome, ſo far was I from knowing what it was that I had really found; taking it for granted, that it was nothing more than ſuch kind of air as I had brought nitrous air to be by the proceſſes above mentioned; and in this air I have obſerved that a candle would burn ſometimes quite naturally, and ſometimes with a beautiful enlarged flame, and yet remain perfectly noxious.

At the ſame time that I had got the air above mentioned from *mercurius calcinatus* and the red precipitate, I had got the ſame kind from *red lead* or *minium*. In this proceſs, that part of the minium on which the focus of the lens had fallen, turned yellow. One third of the air, in this experiment, was readily abſorbed by water, but, in the remainder,

der, a candle burned very ſtrongly, and with a crackling noiſe.

This experiment with *red lead* confirmed me more in my ſuſpicion, that the *mercurius calcinatus* muſt have got the property of yielding this kind of air from the atmoſphere, the proceſs by which that preparation, and this of red lead is made, being ſimilar. As I never make the leaſt ſecret of any thing that I obſerve, I mentioned this experiment alſo, as well as thoſe with the *mercurius calcinatus*, and the red precipitate, to all my philoſophical acquaintance at Paris, and elſewhere; having no idea at that time, to what theſe remarkable facts would lead.

Preſently after my return from abroad, I went to work upon the *mercurius calcinatus*, which I had procured from Mr. Cadet; and, with a very moderate degree of heat, I got from ſome of it, an ounce meaſure of air, which I obſerved to be not readily imbibed, either by the ſubſtance itſelf from which it had been expelled (for I ſuffered them to continue a long time together before I transferred the air to any other place) or by water, in which I ſuffered this air to ſtand a conſiderable time before I made any experiment upon it.

In this air, as I had expected, a candle burned with a vivid flame; but what I obſerved new at this time (Nov. 19) and which ſurprized me no

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leſs than the fact I had diſcovered before, was, that, whereas a few moments agitation in water will deprive the modified nitrous air of its property of admitting a candle to burn in it; yet, after more than ten times as much agitation as would be ſufficient to produce this alteration in the nitrous air, no ſenſible change was produced in this. A candle ſtill burned in it with a ſtrong flame; and it did not, in the leaſt, diminiſh common air, which I had obſerved that nitrous air, in this ſtate, in ſome meaſure, does.

But I was much more ſurprized, when, after two days, in which this air had continued in contact with water (by which it was diminiſhed about one twentieth of its bulk) I agitated it violently in water about five minutes, and found that a candle ſtill burned in it as well as in common air. The ſame degree of agitation would have made phlogiſticated nitrous air fit for reſpiration indeed, but it would certainly have extinguiſhed a candle.

Theſe facts fully convinced me, that there muſt be a very material difference between the conſtitution of the air from *mercurius calcinatus*, and that of dephlogiſticated nitrous air, notwithſtanding their reſemblance in ſome particulars. But though I did not doubt that the air from *mercurius calcinatus* was fit for reſpiration, after being agitated in water, as every kind of air without exception, on which I had tried

tried the experiment, had been, I ſtill did not ſuſpect that it was reſpirable in the firſt inſtance; ſo far was I from having any idea of this air being, what it really was, much ſuperior, in this reſpect, to the air of the atmoſphere.

In this ignorance of the real nature of this kind of air, I continued from this time (November) to the 1ſt of March following; having, in the mean time, been intent upon my experiments on the vitriolic acid air, and the various modifications of air produced by ſpirit of nitre. But in the courſe of this month, I not only aſcertained the nature of this kind of air, though very gradually, but was led by it, as I then thought, to the complete diſcovery of the conſtitution of the air we breathe.

Till this firſt of March, 1775, I had ſo little ſuſpicion of the air from *mercurius calcinatus*, &c. being wholeſome, that I had not even thought of applying to it the teſt of nitrous air; but thinking (as my reader muſt imagine I frequently muſt have done) on the candle burning in it after long agitation in water, it occurred to me at laſt to make the experiment; and putting one meaſure of nitrous air to two meaſures of this air, I found, not only that it was diminiſhed, but that it was diminiſhed quite as much as common air, and that the redneſs of the mixture was likewiſe equal to that of a ſimilar mixture of nitrous and common air.

After

After this I had no doubt but that the air from *mercurius calcinatus* was fit for respiration, and that it had all the other properties of genuine common air. But I did not take notice of what I might have observed, if I had not been so fully possessed by the notion of there being no air better than common air, that the redness was really deeper, and the diminution something greater than common air would have admitted.

I now concluded that all the constituent parts of the air were equally, and in their proper proportion, imbibed in the preparation of this substance, and also in the process of making red lead. For at the same time that I made the above mentioned experiment on the air from *mercurius calcinatus*, I likewise observed that the air which I had extracted from red lead, after the fixed air was washed out of it, was of the same nature, being diminished by nitrous air like common air; but, at the same time, I was puzzled to find that air from the red precipitate was diminished in the same manner, though the process for making this substance is quite different from that of making the two others. But to this circumstance I happened not to give much attention.

I wish my reader be not quite tired with the frequent repetition of the word *surprize*, and others of similar import; but I must go on in that style a little

tle longer. For the next day I was more ſurprized than ever I had been before, with finding that, after the above mentioned mixture of nitrous air and the air from *mercurius calcinatus*, had ſtood all night (in which time the whole diminution muſt have taken place; and, conſequently, had it been common air, it muſt have been made perfectly noxious, and intirely unfit for reſpiration or inflammation) a candle burned in it, and even better than in common air.

I cannot, at this diſtance of time, recollect what it was that I had in view in making this experiment; but I know I had no expectation of the real iſſue of it. Having acquired a conſiderable degree of readineſs in making experiments of this kind, a very ſlight and evaneſcent motive would be ſufficient to induce me to do it. If, however, I had not happened, for ſome other purpoſe, to have had a lighted candle before me, I ſhould probably never have made the trial; and the whole train of my future experiments relating to this kind of air might have been prevented.

Still, however, having no conception of the real cauſe of this phenomenon, I conſidered it as ſomething very extraordinary; but as a property that was peculiar to air extracted from theſe ſubſtances, and *adventitious*; and I always ſpoke of the air to my acquaintance as being ſubſtantially the ſame thing

thing with common air. I particularly remember my telling Dr. Price, that I was myſelf perfectly ſatisfied of its being common air, as it appeared to be ſo by the teſt of nitrous air; though, for the ſatisfaction of others, I wanted a mouſe to make the proof quite complete.

On the 8th of this month I procured a mouſe, and put it into a glaſs veſſel, containing two ounce meaſures of the air from mercurius calcinatus. Had it been common air, a full grown mouſe, as this was, would have lived in it about a quarter of an hour. In this air, however, my mouſe lived a full half hour; and though it was taken out ſeemingly dead, it appeared to have been only exceedingly chilled; for, upon being held to the fire, it preſently revived, and appeared not to have received any harm from the experiment.

By this I was confirmed in my concluſion, that the air extracted from *mercurius calcinatus*, &c. was *at leaſt as good* as common air; but I did not certainly conclude that it was any *better*; becauſe, though one mouſe would live only a quarter of an hour in a given quantity of air, I knew it was not impoſſible but that another mouſe might have lived in it half an hour; ſo little accuracy is there in this method of aſcertaining the goodneſs of air: and indeed I have never had recourſe to it for my own ſatisfaction, ſince the diſcovery of that moſt ready,

accurate, and elegant teſt that nitrous air furniſhes. But in this caſe I had a view to publiſhing the moſt generally-ſatisfactory account of my experiments that the nature of the thing would admit of.

This experiment with the mouſe, when I had reflected upon it ſome time, gave me ſo much ſuſpicion that the air into which I had put it was better than common air, that I was induced, the day after, to apply the teſt of nitrous air to a ſmall part of that very quantity of air which the mouſe had breathed ſo long; ſo that, had it been common air, I was ſatisfied it muſt have been very nearly, if not altogether, as noxious as poſſible, ſo as not to be affected by nitrous air; when, to my ſurprize again, I found that though it had been breathed ſo long, it was ſtill better than common air. For after mixing it with nitrous air, in the uſual proportion of two to one, it was diminiſhed in the proportion of four and a half to three and a half; that is, the nitrous air had made it two ninths leſs than before, and this in a very ſhort ſpace of time; whereas I had never found that, in the longeſt time, any common air was reduced more than one fifth of its bulk by any proportion of nitrous air, nor more than one fourth by any phlogiſtic proceſs whatever. Thinking of this extraordinary fact upon my pillow, the next morning I put another meaſure of nitrous air to the ſame mixture, and, to my utter aſtoniſhment, found that it

it was farther diminiſhed to almoſt one half of its original quantity. I then put a third meaſure to it; but this did not diminiſh it any farther; but, however, left it one meaſure leſs than it was even after the mouſe had been taken out of it.

Being now fully ſatisfied that this air, even after the mouſe had breathed it half an hour, was much better than common air; and having a quantity of it ſtill left, ſufficient for the experiment, viz. an ounce meaſure and a half, I put the mouſe into it; when I obſerved that it ſeemed to feel no ſhock upon being put into it, evident ſigns of which would have been viſible, if the air had not been very wholeſome; but that it remained perfectly at its eaſe another full half hour, when I took it out quite lively and vigorous. Meaſuring the air the next day, I found it to be reduced from one and a half to two thirds of an ounce meaſure. And after this, if I remember well (for in my *regiſter* of the day I only find it noted, that it was *conſiderably diminiſhed* by nitrous air) it was nearly as good as common air. It was evident, indeed, from the mouſe having been taken out quite vigorous, that the air could not have been rendered very noxious.

For my farther ſatisfaction I procured another mouſe, and putting it into leſs than two ounce meaſures of air extracted from *mercurius calcinatus* and

air from red precipitate (which, having found them to be of the ſame quality, I had mixed together) it lived three quarters of an hour. But not having had the precaution to ſet the veſſel in a warm place, I ſuſpect that the mouſe died of cold. However, as it had lived three times as long as it could probably have lived in the ſame quantity of common air, and I did not expect much accuracy from this kind of teſt, I did not think it neceſſary to make any more experiments with mice.

Being now fully ſatisfied of the ſuperior goodneſs of this kind of air, I proceeded to meaſure that degree of purity, with as much accuracy as I could, by the teſt of nitrous air; and I began with putting one meaſure of nitrous air to two meaſures of this air; as if I had been examining common air; and now I obſerved that the diminution was evidently greater than common air would have ſuffered by the ſame treatment. A ſecond meaſure of nitrous air reduced it to two thirds of its original quantity, and a third meaſure to one half. Suſpecting that the diminution could not proceed much farther, I then added only half a meaſure of nitrous air. By this it was diminiſhed ſtill more, but not much, and another half meaſure made it more than half of its original quantity; ſo that, in this caſe, two meaſures of this air took more than two meaſures of nitrous

trous air, and yet remained leſs than half of what it was. Five meaſures brought it pretty exactly to its original dimenſions.

At the ſame time, air from the *red precipitate* was diminiſhed in the ſame proportion as that from *mercurius calcinatus*, five meaſures of nitrous air being received by two meaſures of this without any increaſe of dimenſions. Now as common air takes about one half of its bulk of nitrous air, before it begins to receive any addition to its dimenſions from more nitrous air, and this air took more than four half meaſures before it ceaſed to be diminiſhed by more nitrous air, and even five half meaſures made no addition to its original dimenſions, I concluded that i twas between four and five times as good as common air. It will be ſeen that I have ſince procured air purer than this.

SECTION II.

Of the Production of dephlogiſticated Air, by Means of Spirit of Nitre and the Calx of Lead.

BEING now fully ſatisfied with reſpect to the *nature* of this new ſpecies of air, viz. that, being capable of taking more phlogiſton from nitrous air, it therefore originally contains leſs of this principle; my next inquiry was, by what means it comes to be ſo pure, or philoſophically ſpeaking, to be ſo much *dephlogiſticated*; and ſince the red lead yields the ſame kind of air with *mercurius calcinatus*, though mixed with fixed air, and is a much cheaper material, I proceeded to examine all the preparations of lead, made by heat in the open air, to ſee what kind of air they would yield, beginning with the *grey calx*, and ending with *litharge*.

The red lead which I uſed for this purpoſe yielded a conſiderable quantity of dephlogiſticated air, and very little fixed air. In this experiment two meaſures of nitrous air being put to one meaſure of this air, reduced it to one third of what it was at firſt,

firſt, and nearly three times its bulk of nitrous air made very little addition to its original dimenſions; ſo that this air was exceedingly pure, and better than any that I had procured before.

One quantity of red lead, which I procured, had a yellowiſh caſt, and had in it ſeveral pieces intirely yellow. I tried it with the burning lens in quick-ſilver, and found that it yielded very little air, and with great difficulty; requiring the application of a very intenſe heat. With an equal quantity of nitrous air, a part of this air was reduced to one half of its original bulk, and three meaſures and a half ſaturated it. The air, therefore, was very pure, and the quantity that it yielded being very ſmall, it proved to be in a very favourable ſtate for aſcertaining on what circumſtances its acquiring this air depended.

My object now was to bring this red lead, which yielded very little air, to that ſtate in which other red lead had yielded a conſiderable quantity; and taking it, in a manner, for granted, in conſequence of the reaſoning intimated above, that red lead muſt imbibe from the atmoſphere ſome kind of *acid*, in order to acquire that property, I took three ſeparate half ounces of this freſh made red lead, and moiſtened them till they made a kind of paſte, with each of the three mineral acids, viz. the vitriolic, the marine, and the nitrous; and as I intended to make the

the experiment in a gun barrel, left the iron ſhould be too much affected by them, I dried all theſe mixtures, till they were perfectly hard; then pulverizing them, I put them ſeparately into my gun barrel, filled up to the mouth with pounded flint, which I had found by trial to yield little, or no air when treated in this manner. I had alſo found that no quantity of air, ſufficient to make an experiment, could be procured from an equal quantity of this red lead by this proceſs.

Thoſe portions of the red lead which had been moiſtened with the vitriolic and marine acids became white; but that which had been moiſtened with the nitrous acid, had acquired a deep brown colour. The mixtures with the nitrous and marine acids dried pretty readily, but that with the vitriolic acid was never perfectly dry; but a great part of it remained in the form of a ſoftiſh paſte.

Neither the vitriolic nor the marine acid mixtures gave the leaſt air when treated in the manner above-mentioned; but the moment that the compoſition into which the *nitrous* acid had entered became warm, air began to be produced; and I received the produce in quickſilver. About one ounce meaſure was quite tranſparent, but preſently after it became exceedingly red; and being ſatiſfied that this redneſs was owing to the nitrous acid vapour having diſſolved the quickſilver, I took no

more than two ounce meaſures in this way, but received all the remainder, which was almoſt two pints, in water. Far the greateſt part of this was fixed air, being readily abſorbed by water, and extinguiſhing a candle. There was, however, a conſiderable reſiduum, in which the flame of a candle burned with a crackling noiſe, from which I concluded that it was true dephlogiſticated air.

In this experiment I had moiſtened the red lead with ſpirit of nitre ſeveral times, and had dried it again. When I repeated the experiment, I moiſtened it only once with the ſame acid, when I got from it not quite a pint of air; but it was almoſt all of the dephlogiſticated kind, about five times as pure as common air. N. B. All the acids made a violent effervescence with the red lead.

Though there was a difference in the reſult of theſe experiments, I was now convinced that it was the nitrous acid which the red lead had acquired from the air, and which had enabled it to yield the dephlogiſticated air, agreeably to my original conjecture. Finding alſo, as will be ſeen in the following ſection, that the ſame kind of air is produced by moiſtening with the ſpirit of nitre any kind of earth that is free from phlogiſton, and treating it as I had done the red lead in the laſt-mentioned experiment, there remained at that time no doubt in my mind, but that *atmoſpherical air*,

or

or the thing that we breathe, *consists of the nitrous acid and earth*, with so much phlogiston as is necessary to its elasticity; and likewise so much more as is required to bring it from its state of perfect purity to the mean condition in which we find it.

From the following experiments it will appear that the quantity of dephlogisticated air depends upon the quantity of the spirit of nitre made use of in the process, the quantity of fixed air being nearly the same in all the cases.

From an ounce of red lead, heated in a gun barrel, I got about an ounce measure of air, which all together was worse than common air; an effect which I attribute, in a great measure, to phlogiston discharged from the iron. The production of air in this case was very slow.

From an ounce measure of the same red lead, diluted with half spirit of nitre and half water, I got twelve ounce measures of air, the last produce of which was highly nitrous. Half of this quantity was absorbed by water, and the remainder was twice as good as common air.

From an ounce of the same red lead, diluted with the same spirit of nitre, without water, I got, by the same treatment, about thirty ounce measures of air, about one eighth of which was absorbed by water, while the rest was highly dephlogisticated.

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From the ſame quantity of red lead, moiſtened with twice the quantity of the ſame ſpirit of nitre, I got about ſixty ounce meaſures of air, a very ſmall part of which was abſorbed by water, and the reſt was as highly dephlogiſticated as that in the laſt experiment.

The produce of air was quicker, with the ſame degree of heat, in proportion as the quantity produced was greater; and in the laſt proceſs the air was very red in the inſide of the veſſel that received it, for a conſiderable time.

For this purpoſe I tried, with ſucceſs, *flowers of zinc*, *chalk*, *quick lime*, *ſlacked lime*, *tobacco pipe clay*, *flint* and *Muſcovy talck*, with other ſimilar ſubſtances, which will be found to comprize almoſt all the kinds of earth that are eſſentially diſtinct from each other, according to their chemical properties.

I was the more confirmed in my idea of ſpirit of nitre and earth conſtituting reſpirable air, by finding, that when any of theſe matters, on which I had tried the experiment, had been treated in the manner above-mentioned, and they had thereby yielded all the air that could be extracted from them by this proceſs; yet when they had been moiſtened with freſh ſpirit of nitre, and were treated in the ſame manner as before, they would yield as much dephlogiſticated air as at the firſt. This

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may be repeated till all the earthy matter be exhausted. It will be sufficient to recite one or two facts of this kind from my register.

April 18, I took the remains of the fresh made red lead, out of which a great quantity of dephlogisticated air had been extracted, and moistening about three quarters of an ounce of it a second time with spirit of nitre, I got from it about two pints of air, all of which was nearly six times as pure as common air. This air was generated very fast, and the glass tube through which it was transmitted was filled with red fumes; the nitrous acid, I suppose, prevailing in the composition of the air, but being absorbed by the water in which it was afterwards received.

In this, and many other processes, my reader will find a great variety in the purity of the air procured from the same substances. But this will not be wondered at, if it be considered that a small quantity of phlogistic matter, accidentally mixing with the ingredients for the composition of this air, depraves it. It will also be unavoidably depraved, in some measure, if the experiment be made in a gun barrel, which I commonly made use of, when, as was generally the case, it was sufficiently exact for my purpose, on account of its being the easiest, and in many respects, the most commodious instrument.

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The reaſon of this is, that if the produce of air be not very rapid, there will be time for the phlogiſton to be diſengaged from the iron itſelf, and to mix with the air. Accordingly I have ſeldom failed to find, that when I endeavoured to get all the air I poſſibly could from any quantity of materials, and received the produce at different times (as for my ſatisfaction I generally did) the laſt was inferior in purity to that which came firſt. Not unfrequently it was phlogiſticated air; that is, air ſo charged with phlogiſton, as to be perfectly noxious. If, therefore, any perſon ſhall propoſe to make dephlogiſticated air, in large quantities, he ſhould have an apparatus appropriated to that purpoſe; and the greateſt care ſhould be taken to keep the inſtruments as clear as poſſible from all phlogiſtic matter, which is the very bane of purity with reſpect to air, they being exactly *plus* and *minus* to each other.

In theſe experiments I was led into an error by not giving ſufficient heat to the mixture of red lead and vitriolic acid, or rather by uſing a gun barrel. Had I been then in poſſeſſion of the excellent *earthen tubes* with which Mr. Wedgwood now furniſhes me, I ſhould have got as pure dephlogiſticated air from the vitriolic, as the nitrous acid.

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SECTION III.

A more particular Account of ſome Proceſſes for the Production of dephlogiſticated Air, in order to determine what Kind of Earth was moſt proper to mix with nitrous Acid for the Purpoſe.

HAVING ſeen ſufficient reaſon, as I at firſt thought, to conclude that reſpirable air conſiſts of nitrous acid and earth, my object, in all this courſe of experiments, was ſimply to find *what kind of earth* was moſt proper for this purpoſe, or which had the moſt aptneſs to form this peculiar union with the nitrous acid. Upon the whole, I think it will appear that the *metallic earths*, if they be free from phlogiſton, are the moſt proper, and next to them the *calcareous* earths; but that a very great difference in the production of this kind of air depends upon a variety of circumſtances in which the experiments are made.

The pureſt air that I at that time procured was from *flowers of zinc*, moiſtened, as in the other proceſſes, with ſpirit of nitre, and put into a glaſs phial, with a ground-ſtopper and tube. At firſt I deſpaired of getting any air at all from the proceſs;

ceſs; but at length it came in a prodigious torrent, and was ſo cloudy, that the burſting of every bubble, after it had paſſed through the water, reſembled the burſting of a bag of flour. The tube through which it was tranſmitted was exceedingly red, and in ſome degree, the inſide of the receiver too, as might be perceived amidſt the thick cloud that filled it. This cloudineſs of the newly-generated air, I have often perceived in the proceſs with red lead, but never in ſo great a degree as in this caſe.

The quantity of air procured was nearly three pints, from about half an ounce meaſure of the flowers of zinc; and it was ſo highly dephlogiſticated, that it took three times its bulk of nitrous air before its dimenſions were increaſed. When it had got only twice its bulk of nitrous air, it was reduced to leſs than one fifth of its original quantity. The laſt produce came very ſlowly, and was not quite ſo pure. The flowers of zinc, which I uſed in this experiment, formed a very hard and brittle ſubſtance, when mixed with ſpirit of nitre, and dried. After the proceſs it ſwelled, and broke the phial into many pieces.

Next to the metallic earths of lead and zinc, I found the *calcareous* earths the moſt proper for the production of dephlogiſticated air; but I had no opportunity of trying any great variety of them.

The beſt that I did try was *chalk.* Having ſaturated half an ounce of it with diluted ſpirit of nitre, and dried it, I got from it, in a gun barrel, more than a pint of air, which was highly dephlogiſticated. I began to receive this produce in quickſilver, the conſequence of which was, that the nitrous acid, coming over in the form of vapour, diſſolved the quickſilver, and made nitrous air; but a cruſt being formed upon the ſurface of it, prevented the ſolution of more, and the air continued red a long time.

From another ounce meaſure of chalk, treated in the ſame manner, I got about a quart of air. What I took firſt was conſiderably nitrous, two meaſures of common air and one of this, occupying the ſpace of two meaſures and a half. The ſecond pint was dephlogiſticated; ſo that two meaſures of it, and five of nitrous air, occupied the ſpace of two meaſures. The laſt was leſs dephlogiſticated, being about one half better than common air. At this time the air was generated with prodigious rapidity; the glaſs tube through which it was tranſmitted was exceedingly red; and when, in changing the veſſels, ſome of the vapour eſcaped into the air, it had the reddeſt appearance of any thing that I had ever ſeen of the kind.

Having ſaturated half an ounce of exceedingly good *quick-lime* with diluted ſpirit of nitre, dried it,

it, and put it into a gun barrel, I got from it about a pint and half of air, the firſt part of which was ſo far dephlogiſticated, that it required an equal meaſure of nitrous air to ſaturate it. The ſecond was no better than common air, and the third was equal to the firſt. In this proceſs the air was produced very irregularly, ſometimes coming in great quantities, and at other times the water would ruſh back into the tube.

I repeated the experiment with quick-lime, in a glaſs phial and tube, when the whole quantity was ſo pure, that it required twice its bulk of nitrous air to ſaturate it. The produce of air, in this experiment, was as irregular as in the preceding.

It is ſufficiently evident from theſe experiments, that dephlogiſticated air is produced by means of all kinds of earth mixed with ſpirit of nitre, only that a greater quantity of air is produced from ſome than from others; the advantage in this reſpect being on the ſide of the metallic and calcareous earths. The reaſon, though I was not then aware of it is, that thoſe earths, by their affinity to the nitrous acid, retain it till it can be acted upon by the fire. For in all caſes it is the *acid* only, and not the *earth*, from which the air comes.

I would obſerve, that this proceſs ſeems to furniſh a pretty accurate teſt, perhaps the moſt accurate hitherto known, of the preſence of phlogiſton in

bodies. Nitrous air contains ſo much phlogiſton, that I think it cannot be produced unleſs the materials themſelves contain it in a very conſiderable degree. Whereas, if the air be highly dephlogiſticated, I think it may be conſidered as the moſt ſatisfactory proof we are yet acquainted with, that the ſubſtance contains no phlogiſton at all.

I had at this time made trial of no more than two of the metallic earths, viz. thoſe of *lead* and *zinc*. The reaſon why I did not proceed any farther in this way was, that I knew not how to procure the earths of the ſeveral metals in a ſtate tolerably free from phlogiſton, without which I was well aware they would have yielded nitrous or phlogiſticated air, and not dephlogiſticated.

But I afterwards hit upon a method of coming at the pure earth of all the metals, with the leaſt trouble poſſible, alſo of determining the quantity of phlogiſton they each contain, with the quantity of pure earth which remains after all the phlogiſton is expelled, and the quality of it, with reſpect to what I then thought its *convertibility into air*. This is effected by diſſolving the metals completely, and diſtilling the ſolution to dryneſs in a glaſs veſſel, then employing more ſpirit of nitre, till as much as I pleaſe of it be converted into air. If much phlogiſton do not adhere to the calx of the metal, pure air will be procured at the very firſt diſtillation; but if the earth be very impure,

pure, by containing much phlogiſton, the whole of the firſt produce will be nitrous, or phlogiſticated air, and pure air may not be procured till the ſecond or third proceſs. It was from *copper* that I firſt procured air in this manner, as will be related preſently when I come to treat of air from that metal. But I chuſe, for the ſake of better order, to begin with my accounts of the more *perfect metals*, before I mention the more imperfect ones, and I ſhall relate all the appearances, though ſome of them are not to the preſent purpoſe.

I diſſolved a quantity of *gold* in *aqua regia*, and found that it had loſt four grains in weight. During this ſolution it yielded an ounce meaſure of air, mixed with a good deal of acid vapour; for when lime water was admitted to it, though it did not become in the leaſt degree turbid, it abſorbed more than half of the air. After this I diſtilled the ſolution to dryneſs, and got from it about half an ounce meaſure of air, half of which, judging by the ſame appearances, was acid vapour, and the remainder pure dephlogiſticated air. A browniſh powder remained, which, being collected as carefully as I could, weighed three grains; ſo that the air above-mentioned had been produced from one grain of gold only, and probably ſomething leſs. The powder that remained was pure gold, being ſoluble in *aqua regia*, as I found ſome months afterwards.

This ſolution of gold was made in the common *aqua regia*, conſiſting of one fourth of marine acid and three fourths of ſpirit of nitre; and it was remarkable that when I made another ſolution of gold in that kind of *aqua regia* which I made by impregnating marine acid with nitrous vapour, (which is a much more powerful menſtruum for gold than the common *aqua regia*, yet containing leſs ſpirit of nitre) ſo great a quantity of the air procured from the ſolution was not dephlogiſticated. The particulars of this proceſs were as follows.

I diſſolved a pennyweight of gold in this new, but more powerful kind of *aqua regia*; and diſtilling the ſolution to dryneſs, in a glaſs phial, with a ground ſtopper, I got from it about eight ounce meaſures of air, which did not make lime water turbid, but was ſo much abſorbed by water, that not more than one fourth of an ounce meaſure was left; and this was not better than common air, but it might be a little depraved by ſome part of the acid vapour ſtill remaining in it. When this proceſs approached to dryneſs, the recipient was filled with an exceedingly thick and white cloud. In this experiment alſo the reſiduum was intirely diſſolved by freſh *aqua regia*.

In ſome of theſe experiments I imagined that all the reſiduum was not wholly diſſolved by *aqua*

regia, but the quantity undissolved was exceedingly small, and might be some impurity in the gold that I made use of. However, that a real *calx of gold* was produced in these experiments, is, I think, probable, from the *tinge* given to the glass in which the process was made, which, when the heat was considerable, was partly purple, and partly a dark gold colour, which no acid would touch. But I must observe, that a very slight tinge of the same colour I once observed to be given to a glass vessel of the same kind, in which *nitre* only had been melted. I am not, therefore, absolutely sure that, in the former case, the tinge was given by the gold, though I think it by much the most probable supposition.

From four pennyweights six grains of *silver* dissolved in the nitrous acid, diluted with one third part of water, I got five ounce measures of nitrous air; and removing the vessel to a sand heat, I got a little very pure air, without any mixture of fixed air; but the vessel breaking, I cannot tell how much more might have been procured.

I poured diluted spirit of nitre upon *quicksilver*, till I had got about three ounce measures of nitrous air without the application of heat; when pouring out the undissolved quicksilver, I put the solution, together with the crystals (a third part of the vessel being filled with them) into a sand heat; when I

got firſt forty ounce meaſures of nitrous air, and immediately after that about the ſame quantity of the pureſt dephlogiſticated air. During the rapid production of this air, it was exceedingly white, and the tube through which it was conveyed, was very red with the redundant nitrous vapour. Towards the end of the proceſs the tube was tranſparent, and colourleſs, though the air burſt in white clouds within the recipient.

The firſt time that I procured dephlogiſticated air from *copper*, was from the calx of that metal, which remained after I had diſſolved a quantity of it in oil of vitriol in order to procure vitriolic acid air, and of which I had a pretty large quantity; having generally preferred this metal for that purpoſe. This ſubſtance, when it was well dried, was white; but on putting ſpirit of nitre upon it, and drying it again, it became green. From this ſubſtance, thus prepared, making uſe of a gun barrel, I got about twenty times its bulk of air, of which about one half was fixed air, being readily abſorbed by lime water, and making it turbid; and the reſiduum of the firſt portion was nitrous air, but that of the laſt dephlogiſticated.

In this manner I procured dephlogiſticated air from a calx of copper *previouſly formed*, pouring ſpirit of nitre upon it, as I had done before on red lead, and the flowers of zinc; but I afterwards effected

fected the ſame thing in an eaſier manner, viz. by diſtilling to dryneſs the very ſolution of copper from which I had juſt before produced nitrous air; which I at firſt did with a view to aſcertain whether there was any *fixed air* in that ſolution. It is well known that the ſolution of copper in ſpirit of nitre yields only nitrous air; but ſome conſiderations had led me to ſuſpect that there might be fixed air in that ſolution, diſcoverable by a boiling heat, in the manner in which I had applied it, in order to expel fixed air from water and various other fluids.

With this view I filled a large phial with this ſolution, having a conſiderable quantity by me, remaining from moſt of my experiments on nitrous air; having made it a rule, as much as poſſible, to throw nothing away, if I could make room for it. No fixed air, however, was procured from this ſolution in the manner that I had expected; but continuing to make it boil, and ſupplying it with freſh liquor, as the preceding evaporated, I at length got into the phial all the ſolid materials of a very large quantity of that ſolution; and when it approached to dryneſs, air came from it in very great abundance. I might have caught, I believe ſeveral quarts. A conſiderable quantity I did collect, and found about half of it to be fixed air; and the reſiduum dephlogiſticated. The air was exceedingly cloudy,

cloudy, and mixed with much red vapour. After the proceſs there remained a black powder.

To this *black powder* I applied more ſpirit of nitre, when it preſently became very hot, and fumed; and being dried again aſſumed a green colour, attracting much moiſture from the air. Being again made thoroughly dry, I put it again into a gun barrel, and with a ſtrong heat, got from it about two pints of air, which I received in three portions, and obſerved that one half of each was fixed air, the reſiduum of the firſt and laſt nitrous, and that of the middle dephlogiſticated. That the laſt reſiduum was nitrous, I attribute to the gun barrel, eſpecially as the air came very ſlowly. After this proceſs very little of the black powder remained.

Having got a quantity of nitrous air from iron, in the uſual manner, that is, in a phial with a ground ſtopper and tube, where the common air had no acceſs to it, I immediately took the reſiduum, without giving it an opportunity of getting any thing by communication with the external air; and, in a red ſand heat got from it about the ſame quantity of air that it had before yielded of nitrous air. One half of this was fixed air, precipitating lime in lime water, and being readily abſorbed by water, and the remainder was altogether nitrous.

It is evident from this experiment that iron contains more phlogiſton than copper. The cork, indeed,

deed, which I made uſe of in extracting this air, was evidently corroded in the experiment, and might contribute a little, both to the fixed and the nitrous air; but the ſame cork had been uſed before in the experiment with the copper, which, notwithſtanding this circumſtance, had yielded the pureſt air.

Being determined, if poſſible, to *exhauſt* the phlogiſton in the calx of iron, by the addition of more ſpirit of nitre, I repeated the proceſs; and uſing now a glaſs phial with a ground ſtopper, in a ſand-heat, as before, I got three ounce meaſures of air, which was highly dephlogiſticated. A ſmall part of this air, however, was evidently fixed air, making lime water turbid.

The bulk of the reſiduum was ſtill of the ſame dark brown colour with the common ruſt of iron; but towards the neck of the phial a part had been ſublimed of a light red colour, and above that again was a powder of an exceedingly beautiful yellow, to appearance exactly like ſulphur.

I have not made trial of all the *ſemi-metals*; having hitherto confined my experiments to *zinc*, *biſmuth* and *antimony*, all of which yield a conſiderable quantity of pure air by a direct proceſs, biſmuth being not much inferior to zinc in this reſpect; but I have not made any accurate compariſon of them.

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Putting the ſalt formed by the ſpirit of nitre and biſmuth into a glaſs veſſel, and diſtilling to dryneſs, with a ſand-heat, I procured from it about twenty times its bulk of air, in the firſt part of which there was ſome fixed air; but all the reſiduum was pure dephlogiſticated air, eſpecially at the laſt.

Having poured a quantity of ſtrong ſpirit of nitre upon ſome powder, and ſmall fragments of *antimony*, the whole was reduced, after ſome days, to a whitiſh ſubſtance, of a pretty thick conſiſtence, without yielding any air. A quantity of this ſolution I put into a glaſs veſſel, and with a very ſtrong ſand-heat I got from it about ten times its bulk of air, about one third of which was fixed air, being readily abſorbed by water, and making it turbid, and the remainder was dephlogiſticated. Towards the end of the proceſs the proportion of the fixed air was, as is uſual, very ſmall, and the dephlogiſticated air was of the pureſt kind.

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SECTION IV.

Of the Production of dephlogisticated Air from the vitriolic Acid and Metals.

AS I have been, perhaps, more than any other person, indebted to what are commonly called *accidents* (I mean with respect to *us*; for, in the general plan of nature, and with respect to that great Being who conducts and appoints every thing, there cannot be any such thing as accident) so have I been very often prevented by other accidents from making valuable discoveries, to which I had made near approaches. This was remarkably the case with respect to the production of dephlogisticated air from substances containing the vitriolic acid. For had I, in what I improperly supposed to be an *experimentum crucis*, made use of the calx of perhaps any other metal besides *lead*, on which the vitriolic acid has no proper action, I could not have failed to hit upon what the better genius of Mr. Landriani brought him acquainted with. Having, as I observed before, got a quantity of red lead which was in a state to give little or no air of itself, I got pure air

air from it, in great abundance, by means of the nitrous acid, but none at all by means of the vitriolic or marine acid. I therefore concluded, that the nitrous acid, and not either of the other mineral acids, enters into the composition of dephlogisticated, or atmospherical air.

It was not till after I had made the experiments before recited on manganese, and other mineral substances, that I thought of subjecting *green vitriol*, and other saline matters, to the same trial. It is true I had tried them before; but the method was not adequate to the purpose. And though I had even got a small quantity of air considerably better than common air from *Roman vitriol*, I had concluded that "there must certainly have been some "nitrous acid in that Roman vitriol." In this case, therefore, as in the experiments with *nitre* and *alum*, I had made a discovery without being sensible of the value of it, or indeed understanding it. Nor, when I resumed my experiments on vitriol, had I any expectation of getting from it any thing besides fixed air and water. However, having every thing at hand, a very slight motive was sufficient to induce me to include this among other articles destined for the same process.

In this manner, therefore, without expecting the actual result, on the 24th of November 1777, I put an ounce of green vitriol into a glass vessel, and with

with a ſand heat got from it, at firſt, after the common air was expelled, and the vapour of the water combined with it was come over, a little fixed air; then, after ſome interval, a large quantity of vitriolic acid air; the reſiduum of which was at firſt hardly perceivable, but was afterwards conſiderable, and chiefly fixed air. When the reſiduum was ſtill more conſiderable, I found that it was diminiſhed by nitrous air. At length it had no mixture of vitriolic acid air, but was very turbid, and appeared to be pure dephlogiſticated air, except that, at the laſt, it was not quite ſo pure as before. Of this dephlogiſticated air, I collected ten ounce meaſures. What remained in the glaſs veſſel was ſix pennyweights, of a purpliſh coloured ochre.

Notwithſtanding this evident production of a conſiderable quantity of dephlogiſticated air from green vitriol, which is a combination of iron and the vitriolic acid, I ſtill ſuſpected, as in the caſe of the Roman vitriol mentioned before, that, by expoſure to the common atmoſphere, or in ſome other unknown manner, this vitriol, which had been bought at a common ſhop, might have got ſome mixture of ſpirit of nitre. I therefore made a quantity of vitriol myſelf, by diſſolving iron filings in oil of vitriol, diluted with water. This vitriol, treated as the former had been, yielded air of all the ſame kinds, and in the ſame proportions, as in the preceding

ceding experiment; the dephlogiſticated air, as then, being very turbid, and exceedingly pure. The firſt air that came over was the common air in the veſſel, a little phlogiſticated. A very ſmall quantity of fixed air was ſtill obſerved in the reſiduum of the vitriolic acid air, but none after the dephlogiſticated air was procured.

In making the vitriol for all the above mentioned experiments, I had taken care that the cryſtals ſhould be formed at the bottom of a deep glaſs veſſel, ſo as to have no viſible communication with the external air; and I had alſo covered the veſſel as carefully as I could during the proceſs, and had ſpent as little time as poſſible in conveying the vitriol from the veſſel in which it was formed into that in which it was to be diſtilled. I determined, however, to avoid the ſmall objection to which this trifling expoſure to the air was liable, and therefore next made the diſtillation in the ſame retort in which the ſolution had been made, and in continuation of the ſame proceſs, ſo that all communication with the external air was moſt effectually precluded.

For this purpoſe I diſſolved ſix pennyweights four grains of iron in diluted Newman's oil of vitriol, and diſtilling to dryneſs in a retort with a long neck, I got from it, after the common air was expelled, a ſmall quantity of fixed air, a prodigious quantity of vitriolic acid air, and likewiſe about twenty

twenty two ounce meaſures of the pureſt dephlogiſticated air.

Being now ſufficiently ſatisfied that pure oil of vitriol would always yield dephlogiſticated air with iron, it only remained to try whether the ochre remaining from the former experiment, from which air had been procured, would yield more air with more oil of vitriol, which is the caſe with red lead and ſpirit of nitre.

Accordingly, I put more oil of vitriol to this reſiduum (obſerving that it became very hot by this mixture, as red lead does with ſpirit of nitre) and then, with a red heat, in a glaſs retort, it yielded a quantity of vitriolic acid air, no fixed air, but twenty four ounce meaſures of dephlogiſticated air; when, the retort being melted, a good deal of the air was neceſſarily loſt; for the produce of air had not begun to ſlacken when this accident happened; and removing the retort from the fire, I found only about half of the matter turned red, while the remainder was white. From this circumſtance I concluded, that before I had not got more than half the air that it would have yielded. Reſuming the proceſs in a gun barrel, I actually got about as much air as I had done before.

I had not now the leaſt doubt remaining but that the acid of vitriol, at leaſt with iron, is capable of properly *generating* dephlogiſticated air, as well as

the acid of nitre with lead, or any other ſubſtance whatever.

To complete my experiments on the vitriolic acid and iron, I took half an ounce of the common ruſt of iron, ſuch as is uſed by apothecaries; and pouring upon it a quantity of that acid, obſerved that it imbibed it very eagerly, and became of a dark and almoſt a black colour. Then uſing a gun barrel, I got from it two or three pints of air, all of which was fixed air, but with a large reſiduum, about a third of the whole, phlogiſticated air.

As the common ruſt of iron contains a good deal of phlogiſton, I did not expect any better reſult from this experiment. But having, in ſome meaſure, purified it by this proceſs, I put more oil of vitriol to what remained of the ruſt of iron, and then I got from it only a little fixed air, and ſixteen ounce meaſures of dephlogiſticated air.

Having got an indiſputable production of pure air from *iron*, by means of the vitriolic acid, it was natural for me to proceed to ſimilar experiments on *other metals*, with the ſame acid. And, in the firſt place, I made the proper trials with the two remaining kinds of vitriol, the *blue*, into which copper enters, and the *white*, which is compoſed of zinc; and having now no doubt remaining with reſpect to the purity of the vitriolic acid which enters into the compoſition of theſe kinds of vitriol, I contented myſelf

myſelf with ſpecimens bought at the ſhops, and did not think it neceſſary to take the trouble to compoſe them myſelf.

From about half an ounce of blue vitriol, in a glaſs veſſel, I got a little fixed air, and one ounce meaſure of dephlogiſticated air. The veſſel breaking, I put the materials into a gun barrel, and then got from them about twenty five ounce meaſures of dephlogiſticated air, with hardly any more fixed air. The greateſt part of this air was very turbid.

To finiſh my experiments on the three vitriols, I took an ounce of calcined *white vitriol*, and, with a gun barrel, I got from it a great quantity of vitriolic acid air, ſome fixed air, with five ounce meaſures of dephlogiſticated air. At another time, from one ounce of this kind of vitriol, but uncalcined, I got only about two ounce meaſures of air, part of which was fixed air, and part dephlogiſticated air; not reckoning a great quantity of vitriolic acid air, which came, as uſual, before the dephlogiſticated air.

I did not think it of much conſequence to my purpoſe to go through all the metals with this proceſs, and therefore only made a trial of ſuch as I happened to have at hand.

I diſſolved an ounce of quickſilver, purified by agitation in water, in pure vitriolic acid, in a green glaſs retort. During the diſtillation to dryneſs the retort broke; but collecting the materials as well

as I could (in which perhaps one tenth of the whole might be loſt) I put them into a freſh retort, and, expoſing them to a red heat, got from them a great quantity of vitriolic acid air, a good deal of fixed air, and about fifty ounce meaſures of dephlogiſticated air.

During the proceſs the ſolution boiled violently in the form of a red liquor, while the upper part of the retort was coated with a whitiſh ſort of matter. As the heat reached this coating, it alſo became red; and during the whole proceſs that which evaporated was collected on the ſides of the retort, and then deſcended to the bottom, like drops of blood, or red ink, ſo as to make a very pleaſing appearance. After the proceſs, a very little reddiſh matter remained at the bottom and on the ſides of the retort, which, as well as that which was collected at the neck of the retort, became white when it was cold. Very little of the quickſilver was revived.

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SECTION V.

Dephlogisticated Air procured from other Substances, containing vitriolic Acid.

FROM an ounce of calcined *alum*, prepared some months before, I got about six ounce measures of air, all quite as good as, or better than, common air, and without any fixed air in it. The process was in a gun barrel, and the residuum of the alum was very hard. This I moistened with oil of vitriol, still keeping it hard and dry; and, in a gun barrel, it yielded again two or three ounce measures of air, chiefly fixed air, and at last some that was about as good as common air. After this it was remarkable that this matter absorbed air, perhaps about an ounce measure in all. This I observed twice, and it may be worth while to investigate this circumstance a little farther.

I afterwards made one experiment in order to ascertain the quantity of this pure air, that a given quantity of alum could be made to yield. For this purpose I put one ounce and fourteen pennyweights

of calcined alum into an earthen retort, and by means of a reverboratory furnace I extracted from it one hundred ounce meafures of air, a fmall part of which was fixed air, and the reft fo pure, that with two equal quantities of nitrous air, the meafures of the teft were 1.0.

The water in which this air was received was ftrongly impregnated with vitriolic acid air. This air containing much phlogifton, and in a ftate in which it can be imparted to air, was, no doubt, the reafon why the air in this cafe was not fo pure as that which is obtained from nitre. Otherwife this would be the cheapeft and beft method of procuring dephlogifticated air.

Collecting what remained of the alum, it weighed fifteen pennyweights, and ftill had the tafte of alum, though not very ftrongly. More heat would probably have expelled all the acid, and confequently would have procured more dephlogifticated air. Alfo had the air been quite pure, it would, no doubt, have been much more in quantity. If the weight of the alum, and of the refiduum after this procefs be compared, it will be found that this one hundred ounce meafures of air occafioned the lofs of only nineteen pennyweights, that is, not quite an ounce of the alum, and fince the one hundred ounce meafures of dephlogifticated air would weigh about fixty

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ſix grains, the weight of the vitriolic acid air, with which the water was impregnated, may be eſtimated at ſixteen pennyweights ſix grains.

Another time I got ſixty ounce meaſures of air from an ounce of alum, which is in about the ſame proportion as in the former experiment. But it had been ſo well calcined previous to this proceſs, that ſome of the air had probably been expelled in that operation; and ſtill what remained taſted very ſenſibly of alum. This air being examined, the meaſures of the teſt, with two equal quantities of nitrous air, were 1.4. There was hardly the leaſt ſenſible quantity of fixed air produced.

It is eaſy to conceive, that till any ſubſtance be completely dephlogiſticated, it cannot yield dephlogiſticated air; and it is ſomething remarkable, that a *red colour* ſhould be the criterion of dephlogiſtication, both in the calx of iron, and of mercury. Accordingly, when mercury is diſſolved in ſpirit of nitre, the produce is pure nitrous air, not only during the ſolution itſelf, but alſo during the application of heat to the yellow concrete maſs, that is formed by the evaporation of the ſolution; and no dephlogiſticated air is produced till the red precipitate is completely formed. But the action of heat upon this red ſubſtance is always followed by the production of pure air, as much as from the precipitate per ſe.

It is alſo evident from this experiment, that the air produced in it does not come from the atmoſphere, which has been conjectured with reſpect to ſome of the proceſſes for procuring dephlogiſticated air; but muſt have been contained in the ingredients, viz. ſpirit of nitre.

To half an ounce of *quick lime*, I put oil of vitriol till it weighed one ounce four pennyweights, when it made a hard maſs. This I pounded, and putting it into a gun barrel, I got from it, in all, about ten ounce meaſures of air, the greateſt part of which was fixed air; but towards the laſt, when the heat was as great as I could make it, in a common fire, urged with a pair of bellows, the reſiduum was as good as common air, or rather better. This air came over very turbid.

I concluded from many experiments on ſubſtances containing marine acid, that it differs eſſentially from both the vitriolic and nitrous in this, that it cannot, by any combination whatever, be made to yield dephlogiſticated air, at leaſt with the degree of heat that I was able to apply. But other perſons have procured it by means of this acid, as Mr. Landriani, who firſt ſucceeded in this proceſs, and then Mr. Bertholet. I have now procured this air by means of the *acetous acid*, and though the fact is but of little conſequence, and the expe-

experiment was made with another view, it may not be amiſs to recite the particulars of it.

Having mixed half an ounce of *radical vinegar* with two ounces of calcined whiting, I got from it, by heat, in an earthen retort, 350 ounce meaſures of air, of which about one third was fixed air, but more than this in the firſt portions, and leſs in the laſt. The ſtandard of the reſiduum in the firſt portions was 1.66, in the ſecond 1.42, and in the third 1.38; which was very near the purity of common air. The whiting then weighed 760 grains. I then put a quarter of an ounce more radical vinegar to it, and without taking any account of the quality of the air, only obſerved that I got 120 ounce meaſures from it, and that the reſiduum weighed 730 grains. After this I put to it a quarter of an ounce more of the ſame acid, and got from it 140 ounce meaſures of air, of which the laſt portion had in it no fixed air, and was conſiderably better than common air; but of the exact ſtandard of it I find no note. The whiting then weighed 489 grains. The air had always been very turbid, which accounts for the continual loſs of weight in theſe proceſſes.

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SECTION VI.

Of the Extraction of dephlogisticated Air from several Mineral Substances.

WITH no other view than to try what kind, and proportion of air, different substances would give in a red heat, I entered upon the examination of *manganese*. Of this I procured a quantity finely pounded; and from an ounce of it I got, in a red sand heat, forty ounce measures of air, part of which, in every portion, was fixed air, and at first almost wholly so; but four fifths of the last was the purest dephlogisticated air. Even the first that came over, which was the common air in the vessel, was not in the least phlogisticated. The manganese had lost one pennyweight and a half of its weight, and was not to be distinguished in colour (which was black) from what it had been before. A considerable quantity of water came over during this process.

That manganese should give *fixed air* did not at all surprize me; since there are few earthy substances that do not contain more or less of it; but I did not at all expect the dephlogisticated air; as,

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before I had imagined that the nitrous acid was neceſſary to the production of it, or at leaſt the influence of the atmoſphere, which I ſuppoſed might depoſit the acid that entered into its compoſition, and which I concluded to be the nitrous. On the contrary here was pure air from a ſubſtance which, for any thing that appeared, had always been in the bowels of the earth, and never had had any communication with the external air; and yet it exactly reſembled *red lead*, both in yielding fixed air, and dephlogiſticated air; and it is known that red lead, like the *precipitate per ſe* cannot be made but in contact with the open air.

I next made trial of ſome *lapis calaminaris*, firſt pounding it very finely, then putting an ounce of it into one of the ſmall long necked* retorts already mentioned, and with a red hot ſand heat I got from it 306 ounce meaſures of air; and making allowance for the loſs of air in changing the veſſels in which I received it, &c. I believe I may ſtate the whole produce at 316 ounce meaſures, the whole of which was fixed air, except four ounce meaſures; and, what I did not at all expect, the reſiduum, after the fixed air had been extracted from it by water, appeared to be nearly as good as common air. For one meaſure of it and one of nitrous air occupied the ſpace of one meaſure and

and a quarter. Had the proper reſiduum of fixed air been well extracted, the remainder would probably have been dephlogiſticated air. What remained of the lapis calaminaris weighed thirteen pennyweights ſix grains, and had a lighter colour than before. This produce of air I took at different times, but the reſiduum of the laſt portion was but little better than that of the firſt.

Spirit of nitre made no effervescence, and produced no heat, in mixing with the lapis calaminaris. It alſo made no change of colour in it, and a very little of the acid was ſufficient to make it ſenſibly moiſt; in all which reſpects it differs very remarkably both from red lead, and from manganeſe.

I next proceeded to the examination of the mineral ſubſtance called *wolfram*, from the mines of Cornwall. This I pounded, eſpecially the black part of it, and treating it, in all reſpects, as I had done the lapis calaminaris, I procured from an ounce of it, not more than about an ounce meaſure of air, a little of which was fixed air, but the remainder was about the ſtandard of common air. It required a great and long continued heat to extract this air, and I had nearly deſiſted from the proceſs before any of it came. After the proceſs the wolfram was, to all appearance, the ſame as before. Perhaps a greater degree of

of heat, in veſſels proper for ſuſtaining it, would have produced a greater quantity of air.

Theſe experiments ſuggeſted to me that, poſſibly, the expulſion of dephlogiſticated air from theſe, and other mineral ſubſtances, might aſſiſt in ſuſtaining ſubterraneous fires. For phlogiſton ſet looſe in the diſſolution of all bodies by ignition muſt be received by ſome other ſubſtance, as it is not a thing that, as far as we know, can exiſt, except in combination with other ſubſtances; and we do not know of any thing that can combine with it ſo readily as *air*; and therefore we find that nothing can burn but in contact with air, and with change of air.

When, indeed, phlogiſton is ſet looſe in the putrefactive proceſs, air is not abſolutely neceſſary. For, in that caſe, it may be communicated to water, and probably to other ſubſtances fluid or ſolid. It does not, therefore, certainly follow, that there can be no combuſtion without air, though it be probable; becauſe phlogiſton may be able to eſcape without the help of air in one way, though not in another. The ſolution of the phenomena of ſubterraneous fires would certainly, however, be much eaſier on the ſuppoſition of their ſupplying their own *pabulum*, by means of dephlogiſticated air, contained in ſubſtances expoſed to their heat.

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I therefore desired Mr. Landriani, who, being in Italy, had a good opportunity of making inquiries on the subject, to inform me whether any of those substances, and particularly *manganese*, be found in their volcanos; and his answer makes it rather probable that those fires are, in part, sustained by this means. The extract of his letter, translated from the Italian, is as follows.

" With respect to what you desire to be inform-
" ed of, concerning the *volcanic productions*, there
" is found in the zolfatara of Pozzuolo a great
" quantity of *martial vitriol*; but I do not know
" that there is any manganese, or lapis calaminaris,
" found there. The Abbé Fortis, who has lately
" examined the extinguished volcano of Verona,
" assures me that, besides *martial vitriol*, he has
" found a quantity of manganese there. Sig.
" Volta, having repeated the experiments that I
" communicated to him, has lately informed me,
" that he has found dephlogisticated air in calcined
" *roche alum*, a substance which is found in great
" quantities in all volcanos; so that it is out of
" doubt, that subterraneous fires are continually
" fed with dephlogisticated air, dislodged from
" substances proper for supplying it."

It is very probable, that other mineral substances may contain dephlogisticated air as well as these; and

and it is certainly very well worth while to add this proceſs to the chemical analyſes of them. Whether the ſubſtance be converted into air, or whether it contain the air, in a condenſed or combined ſtate, like fixed air in chalk, it is ſtill of importance to know what kind of air they may be made to yield by heat; and in time we may be able to aſcertain the true origin of ſuch air.

PART

PART II.

OF THE PROPERTIES AND USES OF DEPHLOGISTICATED AIR.

SECTION I.

Of Combuſtion and Reſpiration in dephlogiſticated Air.

I Have not made many experiments on the mixture of dephlogiſticated air with the other kinds of air, becauſe the analogy which it bears to common air is ſo great, that I think any perſon may know before-hand, what the reſult of ſuch experiments would be. It is pleaſing, however, to obſerve how readily and perfectly dephlogiſticated air mixes with phlogiſticated air, or air injured by reſpiration, putrefaction, &c. each tempering the other; ſo that the purity of the mixture may be accurately known from the quantity and quality of the

the two kinds of air before mixture. Thus, if one measure of perfectly noxious air be put to one measure of air that is exactly twice as good as common air, the mixture will be about the standard of common air.

I observed also, in making this experiment, that after mixing one measure of each of these kinds of air, they made exactly two measures; so that there was neither any increase nor diminution of quantity in consequence of the mixture, as is the effect of mixing nitrous air with either common or dephlogisticated air.

It may hence be inferred, that a quantity of very pure air would agreeably qualify the noxious air of a room in which much company should be confined, and which should be so situated, that it could not be conveniently ventilated; so that from being offensive and unwholesome, it would almost instantly become sweet and wholesome. This air might be brought into the room in casks; or a laboratory might be constructed for generating the air, and throwing it into the room as fast as it should be produced. This pure air would be sufficiently cheap for the purpose of many assemblies, and a very little ingenuity would be sufficient to reduce the scheme into practice.

My reader will not wonder, that, after having ascertained the superior goodness of dephlogisticated

air by mice living in it, and the other tests above-mentioned, I should have the curiosity to taste it myself. I have gratified that curiosity, by breathing it, drawing it through a glass syphon, and, by this means, I reduced a large jar full of it to the standard of common air. The feeling of it to my lungs was not sensibly different from that of common air; but I fancied that my breast felt peculiarly light and easy for some time afterwards. Who can tell but that, in time, this pure air may become a fashionable article in luxury. Hitherto only two mice and myself have had the privilege of breathing it.

Some of my friends have expressed a little doubt about the certainty of the test of nitrous air, as a measure of the wholesomeness of respirable air in general, and of dephlogisticated air in particular. To this I can only say, that every thing I have yet observed leads me to depend upon the accuracy of this test, with respect to dephlogisticated as well as common air; and, according to what I should think to be the fairest method of computation, dephlogisticated air serves even longer for respiration than, from this method of examining it, I should have conjectured *a priori*.

The most natural method, as I should think, for estimating the purity of air, and thereby judging of the *time* that any given quantity of it would suffice

ſuffice for the purpoſe of reſpiration, would be to find the quantity of phlogiſton that is required to ſaturate it, or which comes to the ſame thing, the quantity of nitrous air that is required to bring it to the ſtate of perfectly phlogiſticated air. But a mouſe will live much longer in a given quantity of dephlogiſticated air, than in this proportion, with reſpect to common air; owing, I ſuppoſe, to the animal not throwing out equal quantities of phlogiſton in equal times, but much leſs at the laſt, when the vital powers are languid, than at the firſt.

I have a glaſs veſſel which I have made uſe of in all my experiments with mice, from the begining of my reſearches into this ſubject, a conſiderable time before I had diſcovered nitrous air. In this veſſel, which was a tall beer glaſs, and which holds about two ounce meaſures of air when a mouſe of a middle ſize is confined in it, I never knew it to live longer than half an hour, and in general they have not ſurvived twenty minutes. Suppoſing, however, the full time for a mouſe's breathing the common air contained in this veſſel to be half an hour, I ſhould not have expected that a ſpecies of air which required only *four times* as much nitrous air to ſaturate it, would ſuffice for the reſpiration of the ſame animal more than four times as long; and therefore that in this

veſſel of dephlogiſticated air, which at a medium requires about that proportion of nitrous air to ſaturate it, a mouſe might live about two hours. But I believe that mice in general will live conſiderably longer in that quantity of dephlogiſticated air.

I lately put a young mouſe into that very veſſel, filled with dephlogiſticated air, ſuch as when mixed with two equal quantities of nitrous air, the meaſures of the teſt were 0.55. It continued there near three full hours; and being taken out alive, the air was found to be ſo far from being phlogiſticated, that it was ſtill conſiderably better than common air; for, with an equal quantity of nitrous air, the meaſures of the teſt were 1.05. Perhaps this mouſe being languid, in conſequence of its confinement, did not phlogiſticate the air ſo faſt as it would have done had it been more vigorous. But then this may in general be expected to be the caſe with all mice, in the ſame ſituation.

I once obſerved, that I could never make mice live in dephlogiſticated air till they had completely phlogiſticated it, and I could not, at the time of writing that article, aſſign any ſufficient reaſon for the fact. Being unwilling to leave this difficulty unſolved, I repeated the experiment; and putting a vigorous mouſe into about ten ounce meaſures of this air, it continued ſome hours ſeemingly at its eaſe,

eaſe, but died while the air was ſo pure, that, with two equal quantities of nitrous air, the meaſures of the teſt were conſiderably leſs than 1.0.

I then put another young mouſe into the remainder of the air, and it alſo continued at its eaſe two or three hours; but then ſeemed to be expiring, its reſpiration being very languid, and ſo ſlow, that I ſeveral times concluded it was abſolutely dead. I did not at firſt ſuſpect that it could be affected by *cold*, when other mice lived very well in a wire cage in the ſame room; for it had ſoon become quite dry after paſſing through the water, and had never ſhewn any ſign of uneaſineſs. But bringing it near the fire, when the heat was about eighty or ninety degrees (though I removed it occaſionally when it ſeemed to be uneaſy on that account) it lived ſeveral hours longer, and when it died the air was as completely phlogiſticated as common air is generally found to be when mice have died in it.

This experiment fully ſatisfied me, that it was nothing in the dephlogiſticated air itſelf that was the reaſon that mice could not live in it. I obſerved before, that a mouſe is a tender animal, and after paſſing through water, requires a conſiderable degree of warmth; but I did not ſuſpect that it required ſo much, and ſuch a continuance of it, as I found in this particular caſe.

I eaſily conjectured that inflammable air would explode with more violence, and a louder report, by the help of dephlogiſticated than of common air; but the effect far exceeded my expectations, and it has never failed to ſurprize every perſon before whom I have made the experiment.

Inflammable air requires about two thirds of common air to make it explode to the greateſt advantage; and if a phial, containing about an ounce meaſure and a half, be uſed for the experiment, the exploſion with common air will be ſo ſmall, as not to be heard farther than, perhaps, fifty or ſixty yards; but with little more than one third of highly dephlogiſticated air, and the reſt inflammable air, in the ſame phial, the report will be almoſt as loud as that of a ſmall piſtol; being, to judge by the ear, not leſs than forty or fifty times as loud as with common air.

The orifice of the phial in which this experiment is made, ſhould not much exceed a quarter of an inch, and the phial ſhould be a very ſtrong one; otherwiſe it will certainly burſt with the exploſion. The repercuſſion is very conſiderable; and the heat produced by the exploſion very ſenſible to the hand that holds it. I have ſometimes amuſed myſelf with carrying in my pocket, phials thus charged with a mixture of dephlogiſticated and inflammable air, confined either with common corks or ground

ground ſtoppers, and I have perceived no difference in the exploſion, after keeping them a long time, and carrying them to any diſtance.

The dipping of a lighted candle into a jar filled with dephlogiſticated air is alone a very beautiful experiment. The ſtrength and vivacity of the flame is ſtriking, and the heat produced by the flame in theſe circumſtances is alſo remarkably great. But this experiment is more pleaſing, when the air is only little more than twice as good as common air; for when it is highly dephlogiſticated, the candle burns with a crackling noiſe, as if it was full of ſome combuſtible matter.

It may be inferred, from the very great exploſions made in dephlogiſticated air, that, were it poſſible to fire gun-powder in it, leſs than a tenth part of the charge, in all caſes, would ſuffice; the force of an exploſion in this kind of air, far exceeding what might have been expected from the purity of it, as ſhewn in other kinds of trial. But I do not ſee how it is poſſible to make this application of it. I ſhould not, however, think it difficult to confine gun-powder in bladders, with the interſtices of the grains filled with this, inſtead of common air; and ſuch bladders of gun-powder might, perhaps, be uſed in mines, or for blowing up rocks, in digging for metals, &c.

Nothing, however, would be eaſier than to augment the force of fire to a prodigious degree, by blowing it with dephlogiſticated air inſtead of common air. This I have tried, in the preſence of my friend Mr. Magellan, by filling a bladder with it, and puffing it, through a ſmall glaſs tube, upon a piece of lighted wood; but it would be very eaſy to ſupply a pair of bellows with it from a large reſervoir.

Poſſibly much greater things might be effected by chemiſts, in a variety of reſpects, with the prodigious heat which this air may be the means of affording them. I had no ſooner mentioned the diſcovery of this kind of air to my friend Mr. Michell, than this uſe of it occurred to him. He obſerved that poſſibly *platina* might be melted by means of it. Theſe conjectures have been abundantly verified by the experiments of Mr. Lavoiſier and others.

From the greater ſtrength and vivacity of the flame of a candle, in this pure air, it may be conjectured, that it might be peculiarly ſalutary to the lungs in certain morbid caſes, when the common air would not be ſufficient to carry off the phlogiſtic putrid effluvium faſt enough. But, perhaps, we may alſo infer from theſe experiments, that though pure dephlogiſticated air might be very uſeful as a *medicine*, it might not be ſo proper for us in the uſual

uſual healthy ſtate of the body; for, as a candle burns out much faſter in dephlogiſticated than in common air, ſo we might, as may be ſaid, *live out too faſt*, and the animal powers be too ſoon exhauſted in this pure kind of air. A moraliſt, at leaſt, may ſay, that the air which nature has provided for us is as good as we deſerve.

Whether the air of the atmoſphere was, in remote times, or will be in future time, better or worſe than it is at preſent, is a curious ſpeculation; but I have no theory to enable me to throw any light upon it. Philoſophers, in future time, may eaſily determine, by comparing their obſervations with mine, whether the air in general preſerves the very ſame degree of purity, or whether it becomes more or leſs fit for reſpiration in a courſe of time; and alſo, whether the changes to which it may be ſubject are *equable*, or otherwiſe; and by this means may acquire *data*, by which to judge both of the paſt and future ſtate of the atmoſphere. But no obſervations of this kind having been made in former times, all that any perſon could now advance on this ſubject would be little more than random conjecture. If we might be allowed to form any judgment from the length of human life in different ages, which ſeems to be the only *datum* that is left us for this purpoſe, we may conclude that,

that, in general, the air of the atmosphere has, for many ages, preserved the same degree of purity. This *datum*, however, is by no means sufficient for an accurate solution of the problem.

SECTION II.

Of the very great Purity of some dephlogisticated Air.

IT appears from my first observations on the properties of dephlogisticated air, that, in general, that when two equal measures of nitrous air are mixed with one measure of it, the whole is reduced to half a measure, and sometimes, when I thought it peculiarly pure, to one sixth of a measure. I have since, on one particular occasion, produced this kind of air in a state of so much greater purity as appeared very extraordinary to myself, and I doubt not will be thought so by others.

Having, for a purpose that has been mentioned in the account of my observations on fixed air, kept a solution of mercury in spirit of nitre for several months,

months, in a phial with a ground ſtopper, I put it into a retort with a long neck, and, in a ſand heat, received in the firſt place, the nitrous air it yielded, and then without removing the retort from the fire, the dephlogiſticated air. Uſing both the nitrous and dephlogiſticated air of the ſame produce, I obſerved that two meaſures of the former and one of the latter mixed together, occupied, after the effervescence was over, the ſpace of no more than three hundred parts of a meaſure.

It was impoſſible for me to be miſtaken with reſpect to this remarkable fact; for the tube in which I meaſured the reſiduum was ſo long, in proportion to the capacity of the phial which I uſed as a meaſure, that a hundredth part of a meaſure exceeded the eighth of an inch. Repeating the experiment, I found that two meaſures of nitrous air were rather more than ſufficient to ſaturate one meaſure of the dephlogiſticated air; ſo that, poſſibly, had the former experiment been made with more circumſpection, the diminution, extraordinary as it was, would have been ſomewhat greater. Indeed it cannot be ſuppoſed, that *exactly* two meaſures of nitrous air ſhould be the preciſe quantity that would produce the greateſt diminution. It ſhould alſo be conſidered, that a ſmall portion of air might be yielded by the water in which the experiment was made.

Upon

Upon the whole, therefore, I am inclined to think that, were it poſſible to make both the nitrous and dephlogiſticated air in the greateſt purity, and then to mix them in ſome exact proportion, the aerial form of them both would be entirely deſtroyed, the whole quantity ſeeming to diſappear, as in the mixture of alkaline and acid air. But whereas a white ſaline ſubſtance is the immediate viſible reſult of this mixture, there is no viſible produce from the other, the whole, whatever it be, being diſſolved in the water; ſo that, this would probably be the more ſtriking phenomenon of the two; and the mixture of acid and alkaline air never fails to excite a good deal of aſtoniſhment, eſpecially when they are previouſly made, and contained in ſeparate veſſels, and then ſuddenly mixed together, by transferring them from one veſſel to another in a trough of quickſilver.

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SECTION III.

Of procuring dephlogisticated Air in large Quantities, and especially from Nitre.

IMAGINING that the discovery of dephlogisticated air might prove a very useful one, if a quantity of it could be made very *cheap*, as it would be easy enough to contrive methods, by which it might be breathed, provided it should be thought adviseable, in certain morbid states of the lungs and animal system; I have had various schemes for producing it in large quantities, by cheap processes; but none that I have tried have as yet answered. I do not, however, despair; and some person of a happier invention, giving that attention to the subject which it seems to deserve, may be more fortunate.

The cheapest method of procuring dephlogisticated air in large quantities that we are yet acquainted with, is to extract it from nitre by means of heat; and in general no more has been got from an ounce of nitre than about 100 ounce measures of air. The reason of which has been, that, in order to get it pure, *glass vessels* have been used; and after a part of

of the acid has been expelled, the remaining alkali has diſſolved the glaſs, and the proceſs has of courſe terminated. But by means of Mr. Wedgwood's excellent earthen retorts, I have been able to get ſuch quantities of this air from nitre, as to myſelf, and all my friends, appeared very extraordinary.

From two ounces of nitre, in an earthen retort, and with an intenſe white heat, raiſed by ſuch a furnace as Dr. Black has conſtructed, I got at firſt five hundred ounce meaſures of air, the whole conſiderably dephlogiſticated, and with very little fixed air. With the firſt produce the meaſures of the teſt were 0.7; but the laſt part of the produce came ſlowly, and the meaſures were 1.3, which ſhews that the air was conſiderably injured by ſomething in the retort. For the air procured by this proceſs in glaſs is more pure, the meaſures of the teſt being generally leſs than 0.5.

What remained in the retort was a dark green or blue ſubſtance, extremely acrid to the taſte, and deliqueſcent, weighing, when it was taken out of the retort, about eighteen pennyweights. The air would have weighed about thirteen pennyweights, and the water of cryſtallization, which is eighteen parts in 110 of the water, ſix pennyweights and a half, ſo that three pennyweights remain to be accounted for of the two ounces. This laſt was, in part, an *acid vapour* diffuſed through the air, and not

not incorporated with it; for whenever I emptied any of the veſſels of this air, I perceived a very pungent ſmell. Part of the loſs alſo was the *white cloud* with which the air was often filled when it was produced.

From the ſame quantity of *purified nitre*, in one of Mr. Wedgwood's retorts, I got 787 ounce meaſures. The air procured in this manner is never of the pureſt kind; and the ſtandard of this, from the firſt to the laſt, with two equal meaſures of nitrous air, was only 1.25. I conducted this proceſs ſo that the air was very little turbid, and yet there was ſomething acid diſcovered by the ſmell in every portion of it. In another proceſs I got 796 ounce meaſures of air, and the retort was diſſolved before the proceſs was completed. But the largeſt quantity that I ever got from two ounces of nitre was 812 ounce meaſures, and this was purer than any I had got before in this way, the ſtandard of the greateſt part of it being 0.95, though at the very laſt 1.5. In all caſes I have obſerved, that the purer the air is, the greater is the quantity. For it can only be rendered impure by ſome phlogiſtic matter uniting with the pureſt part of it, and forming, in many caſes, as I am inclined to think, phlogiſticated air. In this experiment the loſs in the nitre was 531 grains. The air would have weighed ſomething more than 487 grains, ſo that more than one half

of the weight of the nitre was reduced to the form of air; and there remained thirty four grains for the water of cryſtallization, &c.

In theſe experiments it was a matter of ſome conſequence to determine whether any *acid* remained in the nitre after all the air was come over; and as the vitriolic acid has a ſtronger affinity with the baſe of the nitre, there appeared to be no better method of determining this, than to put oil of vitriol to it, and endeavour to make nitrous acid. Accordingly, in the former proceſſes, I had always been able to get nitrous acid, or nitrous air, from what remained of the nitre, after the dephlogiſticated air was expelled from it. Thus from half an ounce of the ſaline reſiduum which remained from one of the preceding proceſſes, I got nine ounce meaſures of nitrous air.

To theſe circumſtances I gave particular attention in my future experiments. To that which remained of the laſt mentioned experiment, in which I procured 812 meaſures of air from two ounces of nitre, I put oil of vitriol and water. But though it was expoſed to heat till the water was diſtilled over, I did not get half an ounce meaſure of air more than the veſſel contained; and this was dephlogiſticated. Conſequently, all the nitrous acid had been expelled from this nitre.

The

The colour of the alkaline matter to which the nitre was reduced in thefe earthen retorts, was a deep green, owing, perhaps, to fomething of the earth in the retort; for in a porcelain veffel, I, at one time, had it perfectly white.

At Mr. Watt's requeft, I endeavoured to afcertain the quantity of acid that was expelled from nitre, in procuring the dephlogifticated air from it. To do this, I put two ounces of purified nitre into a glafs retort, and receiving the air in 300 ounce meafures of water, only filled each recipient half full, and agitated the air very much in the water, in order to make the water imbibe as much as poffible of the acid that it contained. However, notwithftanding this agitation, every veffel of the air retained a ftrong fmell of the acid.

I afterwards examined a portion of this water, by expelling air from it, and did not find it to contain more air than water generally does, but what I did get from it was confiderably phlogifticated.

SECTION IV.

Of the White Matter depoſited in the Production of dephlogiſticated Air.

IN the rapid production of all kinds of air from earthy materials, I have frequently obſerved that there is a quantity of ſuperfluous *white matter* depoſited in the cold water in which it is received. This earth ſeems to have been held in ſolution in the air while it was hot, becauſe it was then quite tranſparent, and did not become turbid till it was cool; and this was one reaſon why I once thought that an earth is the proper baſis of all ſuch kinds of air. For if ſome earth be certainly held in a proper ſolution, ſo as to make a conſtituent part of the air while hot, as its tranſparency ſeems to prove, and it be only depoſited by *cold*, ſome of the earth muſt, I thought, be retained by it in every degree of heat, and therefore in the temperature of the atmoſphere. And perhaps no degree of cold can deprive it of all the earth that it contains. If it ſhould, I ſhould have imagined that, as nothing but the *acid principle* would remain, it would then, like any other *acid*

air, become liable to be immediately abſorbed by water.

This earthy matter when incorporated with the air, I imagined to be then *the ſame thing*, from whatever ſubſtance the air had been produced, being diveſted of every thing that was peculiar to the ſubſtance from which it had been expelled; juſt as the *acidifying principle*, in the compoſition of dephlogiſticated air, is probably the ſame thing, whether the air had been produced from materials containing ſpirit of nitre, or oil of vitriol. If this reaſoning be true, we ſhould, I thought, be in poſſeſſion of a method of obtaining a truly *primitive earth*, or an *earthy principle* common to all earths, and all metallic calces whatſoever, ſince dephlogiſticated air may, as I had ſhewn, be produced from them all. The following obſervations, however, may perhaps lead to a contrary concluſion, or, that earth depoſited from dephlogiſticated air produced from different materials, has not, in all reſpects, the ſame properties.

Having collected ſome of the white powder diffuſed through a quantity of dephlogiſticated air, procured from minium and ſpirit of nitre, I obſerved that when it was dry, it was of a grey colour, and that it was not, at leaſt immediately, affected by ſpirit of ſalt. When it was heated in a glaſs tube, by means of the flame of a candle and a blowpipe, it fumed copiouſly, and covered the inſide of

the tube with a white ſubſtance; that which was not ſublimed becoming black. When it was laid upon a red hot iron, it ſmoked very much, and became of a brown colour. But in none of its forms was it quickly affected by ſpirit of ſalt, though after twelve hours this acid did acquire an orange colour, both from the black and the brown matter.

At the ſame time I had by me a quantity of white matter which, as I believe, had been collected in a ſimilar manner, when I procured dephlogiſticated air from *red precipitate*; but having loſt the label, I could not be abſolutely certain. This matter was perfectly white when dry, and bore a red heat without any ſenſible change, nor was it affected by ſpirit of ſalt.

I once collected a quantity of this ſubſtance, and found that it continued in the form of a white powder. It made no effervescence with ſpirit of nitre, but ſeemed to be in part, though not wholly, diſſolved in it.

PART

PART III.

MISCELLANEOUS OBSERVATIONS RELATING TO DEPHLOGISTICATED AIR.

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1. *Of the Detonation of Nitre.*

THE discovery of dephlogisticated air throws great light on many very important facts in chemistry, but upon none more than upon that very difficult and striking one, of the *detonation of nitre*, concerning which the most improbable conjectures had been advanced by the most eminent philosophers and chemists. This detonation is the sudden inflammation produced by the contact of various substances containing phlogiston and nitre, when either of them is red hot. The hypothesis that has been thought the most satisfactory is that of

Mr. Macquer, who ſuppoſes that, in theſe circumſtances, an union is formed between the pure nitrous acid and phlogiſton, ſimilar to that which is formed between the vitriolic acid and phlogiſton in the compoſition of ſulphur. He therefore ſuppoſes that, in this caſe, *a nitrous ſulphur* is formed, and that this ſubſtance is of ſo inflammable a nature, that it cannot exiſt a moment without actual ignition.

But I would obſerve that, ſuppoſing this hypothetical nitrous ſulphur to be actually formed, yet if it reſemble other combuſtible ſubſtances, the vitriolic ſulphur for inſtance, or any other whatever, in a property that is common to them all without exception, it cannot be inflamed but in contact with air; which, according to concluſions clearly drawn from my experiments, and all other obſervations, is ſaturated with phlogiſton by the proceſs, and when ſaturated can take no more, let the ſubſtance that is heated in it be ever ſo combuſtible; and conſequently, in thoſe circumſtances, all inflammation muſt be impoſſible. Whereas Mr. Macquer acknowledges, that this nitrous ſulphur is capable of the moſt violent inflammation in the cloſeſt veſſels, where there is no acceſs of air, and it is well known that compoſitions of gunpowder are made to burn even under water.

Now the doctrine of dephlogiſticated air ſupplies the eaſieſt ſolution imaginable of this very difficult phenomenon,

phenomenon. For it appears that the nitrous acid cannot be heated to a certain degree, without producing dephlogisticated air; by the help of which all combustible substances burn with the greatest violence, much more than they can be made to burn with in common air. Here then, I suppose that the moment the acid of nitre, contained in the nitre, and the earth of the coal, for example, thrown into it become red hot, in contact with each other, dephlogisticated air is produced; and in this air the remainder of the charcoal, being likewise red hot, burns with the violence that is observable in the experiment; while, at the same time, other portions of the nitrous acid are forming more dephlogisticated air; and thus the detonation continues, till all the charcoal, or all the nitre, is consumed; the acid not being *lost*, as some chemists express it, but entering into the composition either of the dephlogisticated air, or of some other kind of air, that may be generated in the process.

Let any person but attend to the phenomena of the detonation of charcoal in nitre, and that of the dipping a piece of hot charcoal into a jar of dephlogisticated air, and I think it will be impossible for him not to conclude that the appearances are the very same, and must have the same cause.

There is the ſame intenſe incandeſcence, and the ſame rapid conſumption of the charcoal in both caſes; and this is evidently owing to the eagerneſs, as I may ſay, with which this ſpecies of air, the moſt free from phlogiſton itſelf, ſeizes upon the phlogiſton of other bodies, in a ſufficient degree of heat. Such appearances cannot be produced in common air, which, being more than half ſaturated with phlogiſton already, can take but little more; and therefore, to produce an appearance any thing reſembling them, we are obliged to ſupply the fire with a current of freſh air thrown into it by bellows. But ſupplying a fire in the ſame manner with a current of dephlogiſticated air, which I have ſometimes done, has a moſt aſtoniſhing effect of the ſame kind.

I ſhall conclude this article concerning detonation, with an account of a very ſtriking experiment that I made with Homberg's pyrophorus. I put a quantity of it into one of the ſmall jars which I uſe for experiments on air in quickſilver; then, filling up the veſſel with quickſilver, I inverted it in a baſon of the ſame, and threw up dephlogiſticated air at different times. It always occaſioned a ſudden and vehement accenſion, like the flaſhing of gunpowder, and the air was greatly diminiſhed, as might have been foreſeen.

2. *Of*

2. *Of the Formation of Precipitate per se.*

Precipitate per se is much more easily procured in dephlogisticated than in common air, and probably not at all in phlogisticated air; this air not being capable of taking any phlogiston from mercury, without which the calx cannot be formed.

I exposed equal quantities of the same quicksilver, in equal glass tubes, of about two feet and a half long, and an inch and a half in diameter, but narrower towards the top, to a sand heat, for one day, one of the tubes containing phlogisticated, and the other dephlogisticated air, both hermetically sealed. In the result, the mercury in the tube containing the dephlogisticated air was completely covered with a coating of precipitate per se; but the mercury in the other tube was not sensibly altered. When the process had been resumed, and continued four days, I opened the tubes, and found the dephlogisticated air something worse than it had been, but by no means so much so as I had expected; but the phlogisticated air was not at all altered. The quantity of precipitate in the dephlogisticated air was trifling.

I then repeated the same experiments with common air, but in two days no precipitate was formed. With more time there probably would have

been ſome. But this was ſufficient for my purpoſe, viz. to aſcertain the difference that would be produced by different kinds of air in this proceſs, according to the quantity of phlogiſton which they contained.

Perhaps the *precipitate per ſe* may, by this means, be made in much leſs time, and with much leſs expence, than it now is.

3. *Of the ruſting of Metals in Air.*

It is generally thought, I believe, that metals expoſed to the open air are corroded, and contract ruſt, by means of ſome *acid vapour* contained in it. I thought it poſſible, however, that very pure air might have ſuch an affinity with phlogiſton, as to deprive ſome metals of it, without the aid of any acid. To try this, I filled an eight ounce phial with very dry clean nails, and then with quickſilver, which I diſplaced by very pure and dry dephlogiſticated air, and left it inverted in a baſon of quickſilver on the 13th of April, 1778. On the 26th of January, 1779, I found that one tenth of the whole quantity of air was gone, the quickſilver having riſen ſo high in the phial. I therefore took it for granted, though I could not perceive any ruſt on the nails, that my conjecture was well founded; that the air has been diminiſhed by means

means of phlogiſton from the iron, and that in *time*, if the quantity ſhould be ſufficient, the ruſt will be apparent. This air being examined on the 20th of July, 1780, was found not to be diminiſhed any farther. It was alſo a little worſe than it had been when it was put into the phial, and the nails were very clean and without ruſt.

4. *Of the Formation of Nitre.*

Before the diſcovery of dephlogiſticated air it was generally ſuppoſed by chemiſts, that nitrous acid, by which common nitre is formed, exiſts in the atmoſphere as an *extraneous ſubſtance*, like water, and a variety of other ſubſtances, which float in it, in the form of effluvia; but ſince there is no place in which nitre may not be made, it may, with more probability be ſuppoſed, that nitre is formed by a real *decompoſition of the air itſelf*, the *baſes* that are preſented to it having, in ſuch circumſtances, a near affinity with pure air, which is the principal ingredient in it.

BOOK

BOOK V.

OBSERVATIONS ON PHLOGISTICATED AIR*.

PART I.

PROCESSES BY WHICH GOOD AIR IS NOT INJURED, AND NOXIOUS AIR NOT RESTORED.

ONE of the firſt objects of my experiments on *atmoſpherical air*, after ſome relating to *fixed air*, was the cauſe of the change made in its properties by a variety of proceſſes, which, becauſe I firſt concluded that the principal agent was phlo-

* It will be ſeen, that the reaſon why I called the ſpecies of air, which is the ſubject of this book, *phlogiſticated*, was my ſuppoſition of its being atmoſpherical air, affected by phlogiſton. But whether this be the caſe, or the air of the atmoſphere conſiſt of two diſtinct parts, and ſubſtances containing phlogiſton, attract one of theſe, viz. the *dephlogiſticated*, and leave the other, this other part may ſtill be called *phlogiſticated air*, ſo that no miſtake will be occaſioned by the uſe of the term.

giſton,

giſton, I termed *phlogiſtic.* But before I relate any of them, it may not be amiſs to recite the circumſtances in which air is not changed, notwithſtanding it either had been apprehended, or might naturally be imagined to be ſo, in them.

SECTION I.

Of Air not being ſenſibly injured by Heat, or by offenſive putrid Subſtances.

IT had been advanced by the Count de Saluzes, that air is injured by *heat*, and reſtored by *cold*, eſpecially by froſt. But though I made the experiments with all the attention of which I was capable, I did not find theſe to be the effect of mere heat or cold.

It has been obſerved both by myſelf and others, that air exceedingly offenſive to the noſtrils, is not always properly *phlogiſticated*, ſo as to be diſtinguiſhed by the teſt of nitrous air. For though it may be true that phlogiſton is the thing that conſtitutes *ſmell*,

ſmell, or at leaſt that it is in ſome manner eſſential to it, that phlogiſton which ſenſibly affects the olfactory nerves, may be attached to ſomething that is only *diffuſed* through the air, and not properly *incorporated* with it. For when this air, ſo exceedingly offenſive to the noſtrils, is made to paſs through a body of water, this phlogiſton is entirely ſeparated from it, and leaves the air through which it was diffuſed, and which it had ſeemed to contaminate, quite pure and inoffenſive.

In order to make full proof of the truth of this obſervation, and alſo with the farther view of trying whether the quantity of phlogiſton contained in an animal ſubſtance might be ſo far exhauſted by putrefying in quickſilver, as to be unable to phlogiſticate common air, I confined a large piece of the tendon of a neck of veal, and likewiſe a whole mouſe, in ſeparate veſſels of quickſilver, ſome time in September, 1779; and when they had yielded all the air that I could perceive they would yield, and of which an account has already been given, I took what remained of them both in the April following, and putting them into a jar of common air, containing about ſeven ounce meaſures, I examined this air after two days, and did not find it ſenſibly injured, though the ſubſtances were very offenſive to the noſtrils. After keeping them, however, in the

the ſame jar about two months longer, I found the air to be phlogiſticated.

Notwithſtanding this, I make no doubt but that in *length of time* theſe ſubſtances would have loſt all their power of phlogiſticating air. But whether this property, or that of yielding an offenſive ſmell, would have gone firſt, I had no opportunity of obſerving, in conſequence of removing my habitation, by which I was obliged to put an end to the proceſs. It appears, however, ſufficiently, that very much of the power of theſe putrefying ſubſtances to phlogiſticate air was gone before they ceaſed to be offenſive, though it is probable they were not ſo highly offenſive as they had been before.

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SECTION II.

Of the Air that has been ſuppoſed to come through the Pores of the Skin, and of the Effects of the Perſpiration of the Body.

IT cannot be thought extraordinary, that when it has been imagined that air is extracted from the moſt compact bodies, as gold, by means of the air pump, it ſhould be thought to iſſue from the human ſkin. It was alſo very natural to imagine, that ſince *reſpiration* injures and phlogiſticates air, the *perſpiration* of the body, ſenſible and inſenſible, ſhould do the ſame; and they who ſuppoſe that phlogiſton converts common air into fixed air, muſt of courſe imagine, that the air contiguous to the ſkin is continually undergoing this change. Dr. Ingenhouſz aſſerts the former, and Mr. Cruikſhank, after Sig. Moſcati, the latter. On both theſe ſubjects I ſhall make ſome animadverſions, and likewiſe a few experiments which I think will be deemed concluſive, on the ſubject of perſpiration, and ſufficient to confirm what I have advanced with reſpect to it.

Dr. Ingenhouſz not only ſuppoſes that air is continually iſſuing from the human ſkin; but he took pains

pains to collect it, in a conſiderable variety of circumſtances, of which he has given a particular account. This I took the liberty to tell him I had no doubt was a deception; the air that he found not having come from the *ſkin*, but from the water in which it was plunged; and both the quality of the air that he found, and the circumſtances in which he procured it, left me no doubt upon the ſubject. It was juſt that mixture of fixed air and partially phlogiſticated air, that pump water, which he recommends for the purpoſe, generally abounds with. The bubbles of air riſing and ſwelling at the ſame part of the ſkin is by no means any proof that the air came from the ſkin; for that is always the caſe with air iſſuing from water, the air bubbles never riſing within the water itſelf, but always from ſome other body immerſed in it. All the phenomena he has deſcribed may be ſeen with a piece of metal, or glaſs, plunged in water containing air, in an exhauſted receiver; in which caſe it is eaſily ſhown, that the air does not come from the pores of the metal, or of the glaſs, but from the water itſelf. For if the water contain no air, and the ſurfaces of the metal and of the glaſs be carefully wiped, that appearance cannot be produced.

He ſays that water exhauſted of its air is not proper for this experiment, becauſe it readily abſorbs

all the air as faſt as it iſſues from the ſkin. But if the experiment be made in water at all, this muſt be the only unexceptionable manner of making it; and water by no means abſorbs any kind of air ſo faſt as he deſcribes this to iſſue from the ſkin, and eſpecially ſuch a kind of air as he deſcribes, a great proportion of which is air partially phlogiſticated. It requires a long time before water, in a quieſcent ſtate, will take up any ſenſible quantity of ſuch air as this. Beſides, there is nothing that we know of the human frame, that would lead any perſon to ſuſpect that air ever iſſues from the ſkin. Where are the *air veſſels* for that purpoſe? and what is their origin, or connexion with other parts of the ſyſtem? The preſent ſtate of anatomy indicates nothing on this ſubject.

To ſatisfy my friend, not myſelf, I told him I would make an experiment, which I did not doubt would convince him of his miſtake in this reſpect; I did it in the following manner. I boiled a quantity of rain water, in order to expel from it all the air it might contain, and then ſat with my naked arm plunged in a veſſel filled with it, after carefully wiping off, under water, all the bubbles of air that adhered to it. But though I continued to ſit in this manner a full half hour, not a ſingle bubble of air made its appearance afterwards.

After

After this I need not ſay any thing to my friend's ingenious obſervations on the air which he took the pains to collect from the ſkins of old and young perſons, and his laudable endeavours to remove a popular prejudice concerning the unwholeſomeneſs of the former, and the wholeſomeneſs of the latter kind of air.

Mr. Cruikſhank's experiments, if they could be depended upon, would both prove that fixed air is compoſed of common air and phlogiſton, and that the perſpiration of animal bodies, in a healthy ſtate, has the ſame effect upon air that breathing it has, viz. phlogiſticating it, and making it noxious, which is contrary to the experiments of which I ſhall preſently give an account; by which it appears that the air under my arm-pits, and near other parts of my body, was never leſs pure than the external air. The Abbé Fontana alſo told me, that he had always found the ſame reſult in experiments made upon himſelf. But Mr. Cruikſhank ſays (in the ſecond edition of his *Letter to Mr. Clare*, printed in Mr. Clare's Treatiſe on Abſceſſes) that, after he had confined his leg in a glaſs veſſel, ſo as to prevent all communication with the external air, lime water poured into it immediately afterwards, came out a little turbid. But this he would probably have found to be the caſe with a

ſmall quantity of lime water, poured into, and out of, any veſſel of the ſame ſize, on account of the great ſurface of the fluid that muſt, in thoſe circumſtances, have been expoſed to the common atmoſphere; in conſequence of which it is always known to attract fixed air.

However, partly to examine this matter more thoroughly, and with a variation that I had thought of, I repeated the experiments on my own perſpiration in various ways, and they all confirmed what I advanced before, viz. that the perſpirable matter has no ſuch effect upon the air, but leaves it as wholeſome, that is, as fit for reſpiration, as ever, judging by the teſt of nitrous air, which, however, Mr. Cruikſhank does not ſay that he ever applied in this caſe.

Purſuing his ſteps, I faſtened a moiſt ox's bladder, containing about a quart of air, cloſe about my ancle, ſo that my foot, clean waſhed and warm, as his was, was expoſed to it; and I ſat near the fire, ſo as to keep my foot properly warm a full hour. After this I carefully withdrew my foot from the bladder, without changing the air; and applying the teſt of nitrous air, the air in the bladder appeared to be of the ſame degree of purity with the external air; the meaſures of the teſt, applied in the ſame manner to both, being 1.26. I

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alſo admitted part of this air to lime water, and obſerved that it did not make it in the ſmalleſt degree turbid.

Willing to give more time to this experiment, that the opportunity of this perſpiration phlogiſticating the air might be the greater, I once more faſtened the bladder about my foot, juſt before I went to bed, and ſlept with it all night, keeping myſelf ſufficiently warm, from eleven to half paſt ſix in the morning, when the bladder was quite dry. However, carefully moiſtening it, and eſpecially where it was faſtened to my ankle, I withdrew my foot, without changing the air, and immediately examined it. The quantity contained in the bladder was forty ounce meaſures. It did not affect lime water, and with reſpect to purity was of the ſame ſtandard with common air; the meaſures of the teſt with the nitrous air I happened to make uſe of, being in both caſes 1.3.

I cannot therefore but ſee reaſon to conclude, as I did before, that it is only *reſpiration*, and not the *perſpiration* of the body that injures common air.

I have ſat an hour with my arm in a trough of very warm water, and my warm hand in a glaſs jar placed with its mouth in the water (my hand, of courſe, perſpiring, though inſenſibly, all that time) but when I examined the air within the

jar immediately afterwards, it appeared not to have been the leaſt injured by the proceſs.

But what I expected to produce a much more ſenſible change in the air was the perſpiration under the arm-pits, after walking, or uſing much exerciſe. For this purpoſe, I have ſometimes taken phials of warm water, and poured it out, when I had introduced my hand as carefully as poſſible into the place; but at other times I have put open phials, with perforations in the bottoms, and alſo open glaſs tubes, three or four inches long, the orifices of which were ſuch as that I could eaſily cover them with my thumb or finger. This appearing to be the faireſt method of all, I made the greateſt uſe of it. For the air within the open tube muſt certainly, in the courſe of an hour or two, become of the ſame quality with the air on the outſide of it. In theſe trials alſo, I have preferred *walking* to any other kind of exerciſe, though I have tried ſeveral methods; becauſe, in walking, little or no motion is given to the air about the arm; and it is very eaſy to introduce one's hand, and, covering both the ends of the tube at the ſame time, to be quite ſure that the air within the tube is in that ſtate to which the perſpiration of the body had reduced it. But ſtill, after walking a long time, and making myſelf purpoſely as hot as poſſible, I have never found the air within the

tubes

tubes in the leaſt degree worſe than the external air; but, as I have ſometimes thought, a little better.

The experiment of this kind that I made with the moſt care was in pretty hot weather, on the 4th of June, 1778. I put ſuch tubes as I have mentioned above under each of my arms, and after firſt working with a ſpade, and then walking about three miles, in which exerciſes I purpoſely made myſelf exceedingly hot, I withdrew the tubes with as much care as a good deal of experience had taught me, and I found that one meaſure of this air and one of nitrous air occupied the ſpace of 1.267 meaſures; when the ſame experiment being made with the beſt external air on the ſame day, the meaſures were 1.28. Every circumſtance in the application of the teſt was, as near as I could make it, the very ſame in both caſes.

SECTION III.

Of Air which has been exposed to Steam, and of that which is found in the hollow Parts of some Plants.

VERY early in the course of my observations concerning air, I found that the agitation of any kind of noxious air in *water* purified it to a certain degree, as also that the agitation of pure air in water depraved it so much, as to bring it to about the same standard, viz. that in which a candle just goes out. It might, therefore, be thought, that *steam*, or the vapour of water, intimately diffused through a quantity of noxious air, would much sooner imbibe the phlogiston with which it was charged; and several persons have even thought that the melioration of air by vegetation may be owing to the exhalation of moisture from plants in a vegetating state. I was very willing to adopt that idea myself, in preference to my own, which was that plants imbibe the phlogiston with which the air is overcharged *into their substance*, and convert it into their proper nourishment. But when I tried the effects of steam on phlogisticated air, with as much attention as I could give

give to the experiments, I never found that it was at all mended by the proceſs.

I firſt took a quantity of air that had been phlogiſticated by a mixture of iron filings and ſulphur, and introducing into it the end of a glaſs tube, communicating with a phial, which I had filled with water, I kept it in a boiling heat, about a quarter of an hour, in which time the ſteam had effectually pervaded the maſs of air, having made the jar in which it was contained thoroughly hot, and having expelled three fourths of it. But what remained of this air was no more diminiſhed by nitrous air than it had been before.

Afterwards I ſeveral times filled jars with air phlogiſticated with nitrous air, and alſo by other means; and placing them, inverted, in pans of water, made the water boil a long time, till a great part of the air was expelled by the ſteam, but I never found the air ſo expoſed to ſteam to be at all mended by it. Common air was always ſenſibly injured by this proceſs, as might have been expected from my former experiments.

I am willing to think, however, from the obſervation of Mr. Arden, an intelligent lecturer in natural philoſophy, who firſt mentioned the obſervation to me, as his own, that ſteam, or the vapour of water, may unite with ſomething or other that makes air offenſive, and help to ſweeten it; or, at

leaſt,

leaſt, that throwing a quantity of ſteam into a room in which the air is offenſive, may promote a change in the air, ſo as to be an eaſy and valuable remedy in ſuch caſes. He mentioned to me ſeveral experiments of his own, as well as the obſervations of other perſons, that make it very probable.

Having examined the ſtate of the air confined in the *bladders of fiſhes*, I had the curioſity to examine that which was confined in certain hollow places in plants, where it has no viſible acceſs to the external air. For this purpoſe I tried the air contained in the pods of the *bladder ſena*, and alſo in the ſtalks of *onions*, but did not find that in either of theſe caſes, it was at all ſenſibly different from the common air; being diminiſhed by nitrous air in the ſame proportion.

PART

PART II.

OF PROCESSES BY WHICH AIR IS RENDERED UNFIT FOR RESPIRATION OR COMBUSTION.

SECTION I.

Of Air exposed to a Mixture of Sulphur and Filings of Iron, to Liver of Sulphur and Pyrophorus.

READING in Dr. Hales's account of his experiments, that there was a great diminution of the quantity of air in which had been exposed, a mixture of powdered sulphur and filings of iron, made into a paste with water, I repeated the experiment, and found the diminution greater than I had expected. This diminution of air is made as effectually, and as expeditiously, in quicksilver as in water;

water; and it may be meaſured with the greateſt accuracy, becauſe there is neither any previous expanſion, or increaſe, of the quantity of air, and becauſe it is ſome time before this proceſs begins to have any ſenſible effect. This diminution of air is various; but I have generally found it to be between one fifth and one fourth of the whole.

Air thus diminiſhed is not heavier, but rather lighter than common air.

I conclude that 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 once 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

very offenſively; and when it was cold, it was brown, like the ruſt of iron.

Air diminiſhed by this mixture of iron filings and ſulphur, is exceedingly noxious to animals, and I have not perceived that it grows any better by keeping in water. The ſmell of it is at firſt very pungent and offenſive, which muſt be owing to a quantity of vitriolic acid air generated in the proceſs.

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 ſulphur 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. 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.

Common

Common air, I find, is diminiſhed, and rendered noxious, by *liver of ſulphur*. The diminution in this caſe was one fifth of the whole, and afterwards, as in other ſimilar caſes, it made no effervescence with nitrous air.

I found alſo, after Dr. Hales, that air is diminiſhed by *Homberg's pyrophorus*.

SECTION II.

Of Air infected with the Fumes of burning Charcoal.

AIR infected with the fumes of burning charcoal is well known to be noxious; and 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 burn-

ing. Mr. Cavendiſh alſo obſerved, that there had been a generation of fixed air in this proceſs, but that it was abſorbed by ſoap leys. This experiment I alſo repeated, with a ſmall variation of circumſtances, and with nearly the ſame reſult.

I alſo endeavoured to aſcertain in what manner air is affected with the fumes of charcoal, 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.

If, inſtead of pure water, I uſed *lime water* in this proceſs, it never failed to become turbid by the precipitation of the lime, which could only be occaſioned by fixed air, either diſcharged from the charcoal, or formed by ſomething eſcaping from it, and ſomething in the common air.

There was the ſame precipitation of lime, in this proceſs, 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 had obſerved before, that, in converting dry

dry wood into charcoal, the greateſt part is changed into inflammable air.

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, probably from a generation of inflammable air. 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.

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 ſulphur, 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 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.

SECTION III.

Of the Effect of the Calcination of Metals.

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

cination 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, ſo 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. It appears, however, from the experiments of Mr. Lavoiſier, that the dephlogiſticated part of common air is imbibed in this proceſs, and from my own later experiments, that the phlogiſton of the metals uniting with another part of it makes fixed air.

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

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(or at leaſt the attempt to make a farther calcination) 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, and was no farther diminiſhed by the mixture of iron filings and ſulphur.

It might be ſuſpected that the noxious quality of 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 ſulphur 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.

Lime-water never became turbid by the calcination of metals over it, the calx immediately ſeizing the fixed air that was formed, in preference to the lime in the water; but the colour, ſmell, and taſte

of the water were always changed, and the ſurface of it became covered with a yellow pellicle, as before.

When I was making the experiments on the extraction of inflammable air from iron, I found, that if the quantity of air was conſiderable, the throwing of the focus of the burning lens upon iron-filings incloſed in it, had no other effect than to diminiſh the air, and make it noxious; which it did to as great a degree as the calcination of lead or tin had done: for after this it made no effervescence, and was no farther diminiſhed, by nitrous air. After this, I make no doubt, but that if the proceſs had been continued a ſufficient time, there would have been an increaſe of the quantity of air, by the production of inflammable air; but the firſt effect of the diſcharge of phlogiſton from the iron was the phlogiſticating and diminiſhing of the common air.

I even found that air would be injured by having iron confined in it for a conſiderable time. To try this, I filled a phial, containing common air, full of nails, the 18th of December, 1773, and left it inverted in a veſſel of water till the 2d of March, 1775, when I found that it was diminiſhed one-fifth of its bulk, and was not in the leaſt diminiſhed by a mixture of nitrous air; ſo that it muſt have been perfectly noxious.

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SECTION IV.

Of Air in which Candles have burned.

THE diminution of the quantity of air in which a candle, or ſulphur, has burned out, is various; but I imagine that, at a medium, it may be about one fifteenth, or one ſixteenth of the whole; which is one third as much as by animal or vegetable ſubſtances putrefying in it, by the calcination of metals, or by any of the other cauſes of the complete diminution of air, which will be mentioned hereafter.

I firſt thought, that flame diſpoſed 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, ether, and every other ſubſtance which I have yet tried, except ſulphur, 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 ſulphur be burned in the ſame circumſtances, the lime-water continues tranſparent, but ſtill there may have been the ſame precipitation, or formation of fixed 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 burniug ſulphur 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 ſulphur 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 ſulphur 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 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 through cold water. If the air has ſtood in quick-ſilver, 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 ſulphur, 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, is rather lighter than common air.

SECTION V.

Of Animal Subſtances putrefying in Air.

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 in about eight or ten days, if the weather be pretty warm, it will be found to be diminiſhed one ſixth, or one fifth 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.

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

came 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 one twentieth. After this, it stood two days on 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 one sixth. 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.

The air that is discharged from putrefying substances seems, in some cases, to be chiefly fixed air, with the addition of some other effluvium, which has the power of diminishing common air. The resemblance

resemblance between the true putrid effluvium and fixed air in the following experiment (which was as decisive as I could 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 quicksilver, 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. In general, however, I found some portion of the air from putrefying animal substances to be inflammable; and and it was probably the same in this case.

Putrid cabbage, green or boiled, infects the air in the very same manner as putrid animal substances. Air thus tainted is equally contracted in its dimensions,

ſions, 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.

SECTION VI.

Of the Effect of the Calces of Copper and Iron, and alſo of Mercury on Air.

SEVERAL properties of *metallic calces* may be diſcovered by their expoſure to the common air. I have made ſome obſervations which may be pleaſing and ſatisfactory with reſpect to thoſe of *copper* and *iron*. They prove that the blue colour acquired by the former, and the red colour acquired by the latter, are owing to the dephlogiſtication of them. For theſe colours cannot be aſſumed by them but in the open air, and the air to which they are expoſed is more or leſs phlogiſticated by this means.

I diſſolved copper in a ſolution of ſal-ammoniac, and confined the ſolution in a phial with a ground ſtopper.

ſtopper. After a day or two, when the ſolution was become thoroughly blue, I examined the air within the phial, and found it to be conſiderably worſe than it had been. For one meaſure of it and one of nitrous air occupied the ſpace of 1.33 meaſures; when the common air at the ſame time was diminiſhed by the nitrous air ſo much, that the ſame quantities occupied the ſpace of little more than 1.1 meaſure. At another time I covered a phial containing a quantity of this ſolution with a ſmall jar ſtanding in a trough of water, and found, after a few days, though not more than half the ſolution, beginning from the top, had turned blue, that the air to which it had been expoſed was almoſt completely phlogiſticated.

Pouring a diluted ſolution of pearl aſhes into a diluted ſolution of *green vitriol* with a funnel, that the common air within the phial might mix as little as poſſible with the open air, the precipitate was at firſt of a light blue; but by expoſure to the air it became firſt of a deep indigo blue, and then a red.

Covering a quantity of this blue precipitate contained in a glaſs cup, with a glaſs jar ſtanding in water, I obſerved that, after two or three days, all the ſurface of the precipitate, though covered with water, was become red. When I ſtirred it up, all below the ſurface was as blue as ever. In this

ſtate

ſtate I examined the air, and found it ſenſibly phlogiſticated, though not to a great degree.

Having made another blue precipitate of iron, I poured it into a ſmall retort, and turning it every way, to give all the inſide a coating of it, I expoſed it to the heat of the fire, till it was become partially red (for I did not perceive it would become wholly ſo) and, examining the air in the inſide, I found that one meaſure of it and one of nitrous air occupied the ſpace of 1.3 meaſures; when the ſame quantities of common air and the ſame of nitrous air occupied the ſpace of 1.24 meaſures.

Laſtly, to give the calx of iron more time to affect the air, I made the mixture in a phial which I left half full of air; and in a few days the ſurface of the water was covered with a red pellicle, and ſome time afterwards the ſurface alſo of the precipitate at the bottom of the phial, which had been of a deep blue, was become red. After waiting three weeks, I examined the air, and found it ſo much phlogiſticated, that one meaſure of it and one of nitrous air occupied the ſpace of 1.55 meaſures.

Having alſo coated the inſide of a glaſs tube with the green precipitate, I let it ſtand near three weeks with its orifice immerſed in water, in which time it had become nearly red; and then examin-

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ing the air, I found no fixed air in it (which might have been ſuſpected to come from the pearl aſhes eſpecially; and thus to have injured the air, without any proper phlogiſtication) and one meaſure of it and one of nitrous air occupied the ſpace of 1.45 meaſures. In this experiment, therefore, there was a proper phlogiſtication of the common air, without its receiving any thing from the alkaline ſalts.

It is not a little remarkable, that this change of colour will take place though the precipitate be covered with a large body of water. I have found it when it was covered to the depth of eleven inches, which is that of the trough in which I uſually make my experiments. It was at firſt all blue, the next day I found the ſurface completely red, when the bottom was as deep a blue as ever. This reſembles the property of *ſerum* in my experiments on *blood*. For as that liquor admits phlogiſton to paſs from the blood to the air, ſo water permits phlogiſton to paſs from this precipitated calx to the air, and, as appears from later experiments, permits the calx to become ſaturated with dephlogiſticated air.

The reſult of theſe experiments will be different according to the degree of ſaturation in the ſolution, and perhaps according to other circumſtances.

At the ſame time that I got the deep blue precipitate, with which I made ſeveral of the experiments above-mentioned, I mixed a quantity of the ſaturated

ſaturated ſolutions, both of the vitriol and of the pearl aſhes, in an open jar, and the whole became red at once, without my being able to perceive any previous blue colour at all. Sometimes the precipitate will be white, or grey, eſpecially when the ſolution of the iron is poured into that of the alkali. In this caſe the firſt change is to a very light blue, then to a deeper blue, and laſtly to a red.

In the experiments above-mentioned the air became phlogiſticated in conſequence of the liquor to which it was expoſed *acquiring* colour; whereas in the following it was injured at the ſame time that the liquor *loſt* its colour. I took a quantity of ſpirit of ſalt made yellow by various impregnations, and then made it colourleſs by liver of ſulphur. After this I inverted the phial with common air in it, and let it ſtand about a week, obſerving that in two days it had recovered its original yellow colour; and the air appeared to be ſo much injured, that one meaſure of it and one of nitrous air occupied the ſpace of 1.9 meaſures. The phlogiſton that produced this effect came probably from the liver of ſulphur.

I accidentally met with a much better method of making the experiment with the calx, by means of lime water, than that mentioned above.

The trough in which I make my experiments on air being at one time very foul, with various metallic

metallic ſolutions, eſpecially in conſequence of having diſſolved iron in ſpirit of nitre, for the production of nitrous air, and other purpoſes, and it not having been convenient to change the water, I continued to uſe it in that ſtate; when caſually pouring a little lime water into it, I obſerved that a precipitate of a very deep blue colour was formed. It was ſo beautiful, that, having been obliged to leave my experiments, for the ſake of a ſmall excurſion to Bath, before I ſaw any farther into it, I remember telling a friend whom I met there, that I thought it poſſible that I had accidentally diſcovered a new and cheap method of making Pruſſian blue. However my dream of a diſcovery vaniſhed on my return home, when I obſerved the bottom of my trough covered with a very lively *red.* But when I turned it up, I found the red was only ſuperficial, and that the precipitate underneath was of as deep a blue as ever.

I then repeated the experiment in ſmall jars, phials, &c. and was much better pleaſed with the reſult than when I had made uſe of a ſolution of alkali in order to make the precipitate. Here the lime is ſeized by the acid, as it was before by the alkali; and in both caſes the calx of the iron is ſet at liberty, and depoſited in a phlogiſticated ſtate. But it readily parts with its phlogiſton if pure air be at hand, even though ſeparated from it

it by a body of water, of, I believe, any depth. Thefe experiments fhew that water, though capable of receiving phlogifton, is not capable of retaining it in the prefence of air, which appears to have a much ftronger affinity with it.

It may not be improper to mention again in this place, in fpeaking of the effect of metals on air, that iron that has been fuffered to ruft in nitrous air diminifhes common air very faft.

In a fubfequent part of this work I fhall fhow that there is no air in quickfilver; as has generally been imagined, and that all the air which is difcovered in boiling it in a glafs tube, is only that which had been concealed, and compreffed, between the quickfilver and the glafs. Having for this purpofe collected a fmall quantity of this air, I found it to be common air, being diminifhed by nitrous air. But the quantity being fmall, and not having applied a very accurate meafure, I afterwards repeated that experiment with more precaution, and find fuch air to be in fome degree phlogifticated.

I firft filled a tall thin tube, about an inch in diameter, with quickfilver; and, expofing the upper part of it to a degree of heat that converted it into vapour, in the manner repreefented Pl. III. fig. 4, and confequently effectually fetting at liberty all the air that was confined between the quickfilver

and the glaſs, I collected and examined that air, and found it not to be diminiſhed by nitrous ſo much as common air is.

I then repeated the experiment by throwing up a quantity of common air, and expoſing it to heat mixed with the vapour of quickſilver, and let it continue in that ſtate four or five hours. After this I perceived that the air was conſiderably diminiſhed in bulk; and, examining it, I found that one meaſure of it and one of nitrous air occupied the ſpace of 1.66 meaſures. The air, therefore, in the former experiment, not having been pure air, is no proof of its having been incorporated with the quickſilver; ſince common air mixed with it, in the ſtate of vapour, receives phlogiſton from it. This proves that mercury, like other metals, imbibes pure air, and by this means would in time become *precipitate per ſe.*

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SECTION VII.

Of the Effect of Oils, &c. on Air.

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 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, in the higheſt degree noxious. It did not effervesce with nitrous air, it was no farther diminiſhed by a mixture of iron filings and ſulphur, and was made wholeſome by agitation in water deprived of all air.

Air is diminiſhed by a cement made with one half common coarſe turpentine, and half bees-wax. This was the reſult of a very caſual obſervation.

Having, in an air-pump of Mr. Smeaton's conſtruction, cloſed that end of the ſyphon-gage, which is expoſed to the outward air, with this cement (which I knew would make it perfectly air-tight) inſtead of ſealing it hermetically; I obſerved that, in a courſe of time, the quickſilver in that leg kept continually riſing, ſo that the meaſures I marked upon it were of no uſe to me; and when I opened that end of the tube, and cloſed it again, the ſame conſequence always took place. At length, ſuſpecting that this effect muſt have ariſen from the bit of *cement* diminiſhing the air to which it was expoſed, I covered all the inſide of a glaſs tube with it, and one end of it being quite cloſed with the cement, I ſet it perpendicular, with its open end immerſed in a baſon of quickſilver; and was preſently ſatisfied that my conjecture was well founded; for, in a few days, the quickſilver roſe ſo much within the tube, that the air in the inſide appeared to be diminiſhed about one ſixth.

To change this air I filled the tube with quickſilver, and pouring it out again, I replaced the tube in its former ſituation; when the air was diminiſhed again, but not ſo faſt as before. The ſame lining of cement diminiſhed the air a third time. How long it will retain this power I cannot tell. This cement had been made ſeveral months before I made

made this experiment with it. I muſt obſerve, however, that another quantity of this kind of cement, made with a finer and more liquid turpentine, had not the power of diminiſhing air, except in a very ſmall proportion. Alſo the common red cement has this property in the ſame ſmall degree. Common air, however, which had been confined in a glaſs veſſel lined with this cement about a month, was ſo far injured that a candle would not burn in it. In a longer time it would, I doubt not, have become thoroughly noxious.

I before obſerved that air, which had been expoſed to the effluvia of the common *red cement*, was injured by it. For this purpoſe, I had covered all the inſide of a phial with it, and placed it, inverted, in a veſſel of water. Since that time I have repeated, or rather continued the experiment, letting the ſame veſſel ſtand about nine months in that ſituation; when, upon examining it, I found it was diminiſhed one fifth of its bulk, and that it was not at all affected by nitrous air.

Having had ſo much to do with *red lead*, in the courſe of my late experiments, I had the curioſity to try what would be the effect of that proceſs by which the *grey calx of lead* is converted into red lead. The ſimple calcination of lead, I knew to be the diſcharge of phlogiſton, by which the air contiguous to it is diminiſhed; and, as far as I

have had an opportunity of obſerving, the continuance of the ſame proceſs, by which the grey calx is converted into red lead, is of the ſame nature, viz. a farther diſcharge of phlogiſton, and conſequently a diminution of the air. For I threw the focus of a burning lens upon a ſmall quantity of the grey calx of lead, placed under a receiver ſtanding in water; and though this operation did not make it abſolutely red lead, it gave it a reddiſh tinge; and I found, by the teſt of nitrous air, that the air in which the experiment was performed was conſiderably injured, being not nearly ſo much affected by nitrous air as common air is. In fact, the calx of lead imbibes dephlogiſticated air from the atmoſphere, as does the vapour of mercury in the experiment above-mentioned.

Spirit of wine, which affects nitrous air in a manner ſimilar to the effect that oil of turpentine has upon it, affects common air alſo in a ſimilar manner, and in both caſes alſo in a much leſs degree. When I agitated a quantity of common air in ſpirit of wine, it was not much injured, but was ſenſibly affected, as appeared by its not being quite ſo much diminiſhed by nitrous air as it would have been before.

Though *ether* be an oil, I do not find that it has any power of parting with its phlogiſton to common air, ſo as ſenſibly to injure it. To a phial half filled

filled with water I put a ſmall quantity of ether, and then inverted the phial in a baſon of water, ſo that the air within the phial was expoſed to the influence of the ether. Things being thus circumſtanced, I obſerved that, after ſome time, the air was diminiſhed about one third; but being examined after it had continued a month in this ſituation, and all the ether had diſappeared, the air was not ſenſibly changed. It was diminiſhed as much as before by nitrous air, and was not in the leaſt degree inflammable. The reaſon of its apparent diminution could only be this, that the firſt effect of ether, as I obſerved a long time ago, is to double the bulk of any kind of air into which it is introduced; but that, in all theſe caſes, part of the increaſe always diſappears, probably by the abſorption of part of the vapour of the ether diffuſed through it.

SECTION VIII.

Of the Power of Oil of Turpentine, both to phlogisticate and absorb Air.

I Have obſerved that *oil of turpentine* has the power not only of diminiſhing, and phlogiſticating, but alſo of actually *abſorbing* a very great proportion of common air, in a manner that is very remarkable.

The firſt time that I expoſed common air to the influence of oil of turpentine (which was about the ſame time that I was trying its effects upon nitrous air, and which I was led to do in conſequence of obſerving its remarkable power of decompoſing that kind of air) I found that when I placed a phial of oil of turpentine with its mouth in a baſon of the ſame, leaving about one fourth of the contents of the phial of common air, it was very conſiderably diminiſhed, and without any agitation, in a few hours, ſo that it extinguiſhed a candle. It had a ſtill greater effect in diminiſhing dephlogiſticated air. At another time oil of turpentine, applied in the *ſame* manner, abſorbed more than three fourths of a quantity of common air; and ſome oil of turpentine,

pentine, which had before imbibed a very large quantity of nitrous air, alſo diminiſhed common air, but not in ſo great a degree.

My reader will not wonder that I was exceedingly ſurprized at this power of oil of turpentine to abſorb ſo great a proportion of common air, in the above-mentioned experiment, when he recollects that in no former caſe, had I ever found that common air was diminiſhed more than *one fourth* by any phlogiſtic proceſs. But that oil of turpentine would do much more than this, I had repeated proof. I have one memorandum of a quantity of oil of turpentine abſorbing five ſixths of a quantity of common air. The reſiduum in this experiment I examined very particularly, and found it to be, in all reſpects, the ſame thing that I have denominated *phlogiſticated air*; for it did not make lime water turbid, was not affected by nitrous air, and extinguiſhed a candle. At the ſame time four fifths of a quantity of dephlogiſticated air was abſorbed by the ſame fluid, and the remainder extinguiſhed a candle; and, I doubt not, would have appeared, if it had been examined, to have been completely phlogiſticated.

After this I was exceedingly puzzled to find that I could not make a quantity of oil of turpentine abſorb more than one fourth of the common air to which it was expoſed. Even *long time*, and *agitation*, had

had no more effect; and yet this was of the very ſame oil of turpentine that I had made uſe of before. But reflecting on the prodigious difference in theſe reſults, it occurred to me that, poſſibly, the oil of turpentine, in the ſtate in which I firſt made uſe of it, might have been deprived of all air; and being, perhaps, capable not only of imparting phlogiſton to air, and thereby of diminiſhing it in the uſual proportion, but alſo of *imbibing* the air in *ſubſtance*, it had, in the former caſe, produced both theſe effects; whereas having ſince that time been much expoſed to the open air, it had become ſaturated with it, and therefore could have no effect of imbibing, but only of phlogiſticating air, and conſequently of diminiſhing it one fourth, which was the utmoſt extent of other purely phlogiſtic proceſſes.

In order to aſcertain this, I placed a quantity of the ſame oil of turpentine under the receiver of an air-pump; and, exhauſting it, preſently found that it diſcharged air in great plenty; and immediately after this expoſing a quantity of common air to it, in the manner mentioned above, I found that, in the ſpace of a day, one half of it was abſorbed.

In order to try whether *other eſſential oils* had the property of phlogiſticating common air, as well as oil of turpentine, I careſully took out the corks of three phials, two of which contained oil of mint, and the other oil of cinnamon; and found that pieces of red-hot

hot wood were inſtantly extinguiſhed in them all; ſo that I was fully ſatisfied that the air was phlogiſticated, without conveying it into other phials through water, which would have been attended with the loſs of the oils.

Being employed at the ſame time in making experiments on *cauſtic alkali*, and other ſubſtances, to aſcertain their effects on ſeveral kinds of air, I placed under the ſame receiver of the air-pump a quantity of *oil of turpentine*, of *cauſtic alkali*, and of *diſtilled water*; and found that they all contained a good deal of air: and then, nearly filling equal phials with each of theſe fluids, and inverting them in baſons of the ſame, ſo that equal quantities of common air were left in all the phials, expoſed to equal ſurfaces, and equal quantities of the ſeveral fluids; I obſerved that the oil of turpentine immediately and vigorouſly abſorbed the air, ſo that in a few hours it had imbibed about one half of the whole quantity expoſed to it, while the water had imbibed but very little, and the cauſtic alkali none at all that I could perceive.

SECTION IX.

Of the Effect of Spirit of Nitre on Air.

I have ſufficient proof, that the nitrous acid, both when combined, as it generally is, with water, and likewiſe when exhibited in the form of vapour, or air, is ſo loaded with phlogiſton, as to be capable of diminiſhing both the common air, and the nitrous air, which are expoſed to it. This I thought to be a fact of a pretty extraordinary kind; it appeared ſo much ſo to me, that I had expected the very contrary effect from the experiments that I made upon it; having imagined that, ſince the nitrous acid conſtitutes the pureſt of all the kinds of air, common air wanted nothing more than a greater proportion of this acid to become dephlogiſtcated air; and thus I was in hopes of being maſter of a proceſs, by which I could not only reſtore vitiated air to its original purity, but even improve the purity of common air itſelf.

But when I had got my phials with ground-ſtoppers, I introduced the tube of one of them, into

which I had put ſome ſtrong ſpirit of nitre, under the edge of a ſmall jar, filled with air that had been injured by putrefaction about a year before; and making it boil, forced the hot fumes to riſe into, and mix with it, for a conſiderable time, till the acid ſeemed to be almoſt expelled from it; but I could not perceive that the air was ſenſibly altered by this operation. It was no more diminiſhed by nitrous air than it had been before.

Then it occurred to me that I had, at that very time, a very good opportunity of aſcertaining the effect of the fumes of ſpirit of nitre on common air, by means of a quantity of ſtrong ſmoking ſpirit of nitre, contained in a large phial, one fourth part of which was full, and which had not been opened for half a year; ſo that all the incloſed air, which was three fourths of the contents of the phial, had been expoſed to the vapour of the ſpirit of nitre all that time.

By loſing the ſpirit of nitre, I might have tranſferred this air into another veſſel; without any mixture of common air; but not chuſing to do that, I poured it into another phial, by which means I got a mixture, three-fourths of which was the air from the phial, and one-fourth atmoſpherical air, for which I had to make an allowance. Examining this mixture by the teſt of nitrous air, I found that

two

two meaſures of it, and one of nitrous air, occupied the ſpace of two meaſures and a half, and a candle would not burn in it; ſo that the ſpirit of nitre muſt, as I then thought, have imparted phlogiſton to the air which had been expoſed to it, and in ſo great a degree, that it had become almoſt perfectly noxious; as may be eaſily concluded, by allowing for the mixture of common and wholeſome air with it in this experiment.

That *acid* fumes, as ſuch, have not this effect upon common air, I, at the ſame time, aſcertained, by making the ſame experiment on the air of the phial which had contained the ſtrongeſt ſpirit of ſalt; and, I believe, for a longer time. This, however, was in all reſpects as good as common air.

Afterwards, when I had contrived to fumigate different kinds of air, with the vapour of ſpirit of nitre, by a ſingular kind of proceſs which will be mentioned below, I found that it had no effect whatever upon common air; for in this caſe, I believe it contained very little phlogiſton.

I have obſerved above, that common air is phlogiſticated by continuing a conſiderable time involved in the red vapour of ſpirit of nitre. This, contrary to my expectation, I alſo found to be the caſe with the colourleſs, or inviſible vapour of ſpirit of nitre, after all the colouring phlogiſtic matter had been

driven out of it. Air that had continued only two days in a phial with a glaſs ſtopper, which contained ſome of this colourleſs acid, was ſenſibly leſs affected by nitrous air than common air was; and the air that had been confined in the ſame glaſs tube in which ſome of the colourleſs nitrous acid had been placed in the ſand furnace only two days, though the heat had been ſo ſmall as to have produced no change of colour in the acid, was ſo much phlogiſticated, that one meaſure of it, and one of nitrous air occupied the ſpace of 1.81 meaſures.

In order to compare the effects of the ſame ſpirit of nitre on common and on dephlogiſticated air, I once, for about a fortnight, expoſed two half-ounce phials, nearly filled with the ſtrongeſt brown ſpirit of nitre, one of them to a quantity of common air, and the other to about an equal quantity of dephlogiſticated air; and I obſerved that the air in each of theſe caſes was diminiſhed in about the ſame proportion, and that the dephlogiſticated air was conſiderably injured. The ſurface of the ſpirit of nitre in each of the phials was become much lighter-coloured than before, and the brown colour had, throughout, intirely diſappeared; owing, as I then thought, to its having parted with its phlogiſton; and what I thought ſomething remarkable, clouds of ſome whitiſh matter, not much unlike flowers of zinc, floated on the

ſurface

ſurface of the ſpirit of nitre in both the phials. The quantity of acid was ſenſibly diminiſhed in both; and carefully pouring off the upper and lighter-coloured part of the acid in both the phials, I found that they yielded nitrous air in nearly the ſame proportion, which was as *four* to *nine*, of what they had done before; while the ſtronger part of the acid, at the bottom of the phials, yielded nitrous air in the proportion of *ſeven* to *nine* of what they had done before.

I have found air to be diminiſhed by the effluvia of *nitrous ether*. For the air which had been confined about a week, in a bottle in which a quantity of nitrous ether had been kept, was ſo much injured, that two meaſures of it, and one of nitrous air, occupied the ſpace of two meaſures and a half. As I let a good deal of common air into the phial, at the ſame time that (not chuſing to loſe it) I poured the ether out of it into another phial, I conclude that the air in the phial was almoſt perfectly noxious.

I alſo found that air was injured by being expoſed to freſh melted nitre. I had been led to make this experiment by obſerving that nitre, when it is fuſed by heat, yields air. Seeing this, I had the curioſity to try whether it would recover the air it had loſt by being expoſed to the common air, and at the ſame time to obſerve what effect this expoſure would have upon the common air, in order to judge what

it

it was that nitre, in thoſe circumſtances, took from the air. And I find that common air, expoſed to nitre in theſe circumſtances, is a little injured, but with ſuch circumſtances attending the *proof* of it, as I had never obſerved before, and which I cannot well account for. The facts were as follow.

I melted about an ounce of nitre in a crucible, and immediately placed it under a receiver ſtanding in water, where it preſently became ſolid. The next morning I examined the air in which it had ſtood, and found it to be not ſo good as common air.

Afterwards I melted ſome nitre in a glaſs-phial, and the veſſel being broken by the expanſion of the nitre in cooling, I expoſed the nitre to a quantity of air confined by water, ſo that the common air had acceſs to it on all ſides; whereas, in the former experiment, it had been contiguous to it at its ſurface only. After about a week I examined this air, by the teſt of nitrous air, and found it to be conſiderably worſe than common air; two meaſures of it, and one of nitrous air, occupying the ſpace of two meaſures only: whereas the mixture of common air made at the ſame time, and with part of the ſame quantity of nitrous air, was diminiſhed as much as uſual. I did not carry this experiment any farther, as I did with another quantity of common air, which had likewiſe been expoſed about a week to melted nitre in the ſame circumſtances.

Two meaſures of this air, and one of nitrous, at firſt occupied the ſpace of little more than two meaſures; but it kept continually approaching to the degree of diminution of a mixture of common air made at the ſame time, till after four days the difference between them was very ſmall. Whether after a longer time theſe two mixtures would have been reduced to the very ſame dimenſions, I cannot tell. I made no more experiments of this kind, nor have I, in any other reſpect, purſued this ſingular kind of fact any farther.

I obſerved that the vapour of marine acid does not injure air, and I had the faireſt opportunity of trying this; when I had expoſed a quantity of this acid, in a glaſs tube hermetically ſealed, during ſeveral months, to a ſand heat. For the air within this tube, being examined ſeveral months after it had been expoſed to that heat, was found to be not at all injured. The air which had been confined along with vinegar in the ſame manner, and the ſame ſpace of time, was ſo far injured, that, with an equal quantity of nitrous air, the meaſures of the teſt were 1.44.

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SECTION X.

Air injured by the Effluvium of Water fresh distilled.

NOTWITHSTANDING it has been a maxim with chemists, that water contracts no union with phlogiston, it is acknowledged that water fresh distilled acquires something of an empyreumatic nature, which gives it an unpleasant flavour, and which goes off by exposure to the open air. That this volatile principle is phlogiston I thought that I had ascertained by exposing air to the influence of it.

I took water fresh distilled in copper, and filled a phial about half full of it, and examining the air within the phial about a month afterwards, I found it so much phlogisticated, that one measure of it and one of nitrous air occupied the space of 1.32 measures; when, with the same nitrous air and common air, the same measures were 1.22.

In this case it might be suspected that the phlogiston came from the copper. But at the same time I made a similar experiment, with a similar result, on water distilled in glass. In this case there was more air, and a smaller quantity of water in the phial, but the time of exposure was nearly the same;

and with this air the meaſures of the teſt were 1.26. It is probable that with more water, more time, and leſs air, the reſult would have been more conſiderably in favour of the water having acquired phlogiſton in the act of evaporation, without any communication with ſubſtances that are thought to contain it. This experiment, however, is ſufficiently ſimilar to the others I have recited, in which mere *heat* had the ſame effect as the communication of phlogiſton. However, the true explanation of theſe appearances is, that water freſh diſtilled, is diſpoſed to imbibe pure air, which, accordingly, may be expelled from water that has been long expoſed to it.

To theſe obſervations on the phlogiſtication, as I have called it, of air by means of water, I ſhall add others in which phlogiſticated air was, in ſome degree, purified by agitation in it; which, though I did not ſuſpect it at the time, muſt have been owing to purer air from the water, mixing with the more impure air agitated in it.

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

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 sulphur, 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 has been given in its proper place.

If this process be made in water deprived of air, either by the air-pump, by boiling, or 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 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 indi-

cated by the diſagreeable ſmell which attended the operation.

I ſhall conclude this ſection with obſerving, that I have found a remarkable difference in different kinds of water, with reſpect to their effect on common air agitated in them, and which I am not yet able to account for. If I agitated common air in the water of a deep well, near my houſe in Calne, which is hard, but clear and ſweet, a candle would not burn in it after three minutes. The ſame was the caſe with the rain-water, which I got from the roof of my houſe. But in diſtilled water, or the water of a ſpring-well near the houſe, I agitated the air about twenty minutes, before it would be ſo much injured, It may be worth while to make farther experiments with reſpect to this property of water.

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SECTION XI.

Of the Effluvia of Flowers in Air.

THE action of a plant considered as simply *vegitating* in air is a thing quite different from the effect that the *exhalation of the flowers*, and perhaps other particular parts of the plant, may have upon it. *Smell*, the old chemists said, was an indication of phlogiston, and I find that the most delicate flowers injure the air much more than I had imagined. Nothing is sweeter than a *rose*, and yet the fragrant effluvia of it is far from being favourable to the air in which it is confined.

On the 25th of June I confined a full blown red rose in about four ounce measures of common air, having covered it with a small glass jar standing in water; and I observed that, the next day, the air was so much injured, that one measure of it, and another of nitrous air occupied the space of 1.75 measures; so that I doubt not that any animal would have expired immediately on being put into it. The day following the measures of the test were 1.9, and

and the day after ſomething more. Notwithſtanding this, when the roſe was withdrawn, it did not ſeem to have loſt any thing of its agreeable fragrance.

SECTION XII.

Of the Effect of the electric Spark on Air.

AIR is diminiſhed and rendered noxious by the *electric ſpark*, though I had no expectation of this event when I made the experiment.

The experiments which I made with electricity were ſolely intended to aſcertain what has often been attempted, but, as far as I know, had never been fully accompliſhed, viz. to change the blue colour of liquors, tinged with vegetable juices, red.

For this purpoſe I made uſe of a glaſs tube, about one tenth of an inch diameter in the inſide, as in Pl. II. fig. 16. In one end of this I cemented a piece of wire *b*, on which I put a braſs ball. The lower part from *a* was filled with water tinged blue, or rather purple, with the juice of turnſole,

or

or archil. This is easily done by an air-pump, the tube being set in a vessel of the tinged water.

Things being thus prepared, I perceived that, after I had taken the electric spark, between the wire *b*, and the liquor at *a*, about a minute, the upper part of it began to look red, and in about two minutes it was very manifestly so; and the red part, which was about a quarter of an inch in length, did not readily mix with the rest of the liquor. I observed also, that if the tube lay inclined while I took the sparks, the redness extended twice as far on the lower side as on the upper.

The most important, though the least expected observation, however, was that, in proportion as the liquor became red, it advanced nearer to the wire, so that the space of air in which the sparks were taken was diminished; and at length I found that the diminution was about one fifth of the whole space; after which more electrifying produced no sensible effect.

To determine whether the cause of the change of colour was in the *air*, or in the *electric matter*, I expanded the air which had been diminished in the tube by means of an air-pump, till it expelled all the liquor, and admitted fresh blue liquor into its place; but after that, electricity produced no sensible effect, either on the air, or on the liquor; so

ſo that, as I then thought, it was evident, that the electric matter had decompoſed the air, and had made it depoſit ſomething that was of an acid nature. But Mr. Cavendiſh has proved that nitrous acid was formed in this proceſs by an union of the dephlogiſticated and phlogiſticated air in common air.

In order to determine whether the *wire* had contributed any thing to this effect, I uſed wires of different metals, iron, copper, braſs, and ſilver; but the reſult was the very ſame with them all.

It was alſo the ſame when, by means of a bent glaſs tube, I made the electric ſpark without any wire at all, in the following manner. Each leg of the tube, Pl. II. fig. 19, ſtood in a baſon of quickſilver; which, by means of an air-pump, was made to aſcend as high as *a*, *a*, in each leg, while the ſpace between *a* and *b* in each contained the blue liquor, and the ſpace between *b* and *b* contained common air. Things being thus diſpoſed, I made the electric ſpark perform the circuit from one leg to the other, paſſing from the liquor in one leg of the tube to the liquor in the other leg, through the ſpace of air. The effect was, that the liquor, in both the legs, became red, and the ſpace of air between them was contracted, as before.

Air

Air thus diminiſhed by electricity makes no effervescence with, and is no farther diminiſhed by, a mixture of nitrous air; ſo that it muſt have been in the higheſt degree noxious, exactly like air diminiſhed by any other proceſs.

I took the electric ſpark in common air confined by quickſilver; and then, admitting to it water tinged blue with the juice of turnſole, it became red in the ſpace of a day and two nights, but the colour did not change preſently. Alſo, after this the diminution was greater than it had been before.

Having taken the electric ſpark in common air upon quickſilver, as before, it was preſently diminiſhed as uſual; and the next day without any farther electrification, the diminution was more conſiderable. The third day I admitted to it the juice of turnſole, and in about an hour it appeared to be red at the top, but was not ſenſibly diminiſhed more than before. In leſs than a day it became wholly red, and then no farther diminution was apparent.

I took a quantity of water which had been made blue with the juice of turnſole, and which had been made ſeemingly red with the electric ſpark, taken in the common air over it; but, on mixing all the parts of it together, it reſumed its blue colour (the

(the blue colouring matter having only ſubſided to the bottom) ſo that alteration in the conſtitution of this liquor by this proceſs, though manifeſt to the eye, is not, in fact, ſo very conſiderable. It is evident, however, from the preceding obſervations, that it could not be the mere *concuſſion* given to the air by the ſpark, or ſhock, that had this effect upon it; becauſe when the air was completely diminiſhed, the ſpark or ſhock had no effect, and the liquor turned red when it was admitted to the air a long time after the operation of the electric ſpark upon it, while it was confined by quick-ſilver.

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SECTION XIII.

Of the Effect of putrid Marshes on Air.

TO these sections on the subject of diminished, and noxious air, or as it might have been called *phlogisticated air*, I shall subjoin a letter which I addressed to Sir John Pringle, on the noxious quality of the effluvia of putrid marshes, and which was read at a meeting of the Royal Society, December 16, 1773.

This letter, which is printed in the Philosophical Transactions, vol. 74, p. 90, is immediately followed by another paper, to which I would refer my reader. It was written by Dr. Price, who has so greatly distinguished himself, and done such eminent service to his country, and to mankind, by his calculations relating to the probabilities of human life, and was suggested by his hearing this letter read at the Royal Society. It contains a confirmation of my observations on the noxious effects of stagnant waters, by deductions from Mr. Muret's account of the Bills of Mortality for a parish situated among marshes, in the district of Vaud, belonging to the Canton of Bern in Switzerland.

To

To Sir JOHN PRINGLE, Baronet.

DEAR SIR,

Having purſued my experiments on different kinds of air conſiderably farther, in ſeveral reſpects, than I had done, when I preſented the laſt account of them to the Royal Society; and being encouraged by the favourable notice which the Society has been pleaſed to take of them, I ſhall continue my communications on this ſubject. But, without waiting for the reſult of a variety of proceſſes, which I have now going on, or of other experiments, which I propoſe to make, I ſhall, from time to time, communicate ſuch detached articles, as I ſhall have given the moſt attention to, and with reſpect to which, I ſhall have been the moſt ſucceſsful in my enquiries.

Since the publication of my papers, I have read two treatiſes, written by Dr. Alexander, of Edinburgh, and am exceedingly pleaſed with the ſpirit of philoſophical inquiry which they diſcover. They appear to me to contain many new, curious, and valuable obſervations; but one of the *concluſions*, which he draws from his experiments, I am ſatiſfied, from my own obſervations, is ill founded, and from

from the nature of it, muſt be dangerous. I mean his maintaining, that there is nothing to be apprehended from the neighbourhood of putrid marſhes.

I was particularly ſurpriſed, to meet with ſuch an opinion as this, in a book inſcribed to yourſelf, who have ſo clearly explained the great miſchief of ſuch a ſituation, in your excellent treatiſe *on the diſeaſes of the army*. On this account, I have thought it not improper to addreſs to you the following obſervations and experiments, which I think clearly demonſtrate the fallacy of Dr. Alexander's reaſoning, indiſputably eſtabliſh your doctrine, and indeed juſtify the apprehenſions of all mankind in this caſe.

I think it probable enough, that putrid matter, as Dr. Alexander has endeavoured to prove, will preſerve other ſubſtances from putrefaction; becauſe, being already ſaturated with the putrid effluvium, it cannot readily take any more; but Dr. Alexander was not aware, that air thus loaded with putrid effluvium is exceedingly noxious when taken into the lungs. I have lately, however, had an opportunity of fully aſcertaining how very noxious ſuch air is.

Happening to uſe at Calne, a much larger trough of water, for the purpoſe of my experiments, than I had done at Leeds, and not having freſh water ſo near at hand as I had there, I neglected to change

it, till it turned black, and became offenſive, but by no means to ſuch a degree, as to deter me from making uſe of it. In this ſtate of the water, I obſerved bubbles of air to riſe from it, and eſpecially in one place, to which ſome ſhelves, that I had in it, directed them; and having ſet an inverted glaſs veſſel to catch them, in a few days I collected a conſiderable quantity of this air, which iſſued ſpontaneouſly from the putrid water; and putting nitrous air to it, I found that no change of colour or diminution enſued, ſo that it muſt have been, in the higheſt degree, noxious. I repeated the ſame experiment ſeveral times afterwards, and always with the ſame reſult.

After this, I had the curioſity to try how wholeſome air would be affected by this water; when, to my real ſurpriſe, I found, that after only one minute's agitation in it, a candle would not burn in it; and, after three or four minutes, it was in the ſame ſtate with the air, which had iſſued ſpontaneouſly from the ſame water.

I alſo found that common air, confined in a glaſs veſſel, in *contact* only with this water, and without any agitation, would not admit a candle to burn in it after two days.

Theſe facts certainly demonſtrate, that air which either ariſes from ſtagnant and putrid water, or which has been for ſome time in contact with it, muſt

muſt be very unfit for reſpiration; and yet Dr. Alexander's opinion is rendered ſo plauſible by his experiments, that it is very poſſible that many perſons may be rendered ſecure, and thoughtleſs of danger, in a ſituation in which they muſt neceſſarily breathe it. On this account, I have thought it right to make this communication as early as I conveniently could; and as Dr. Alexander appears to be an ingenuous and benevolent man, I doubt not but he will thank me for it.

I am, &c.

In all the preceding experiments, there is a remarkable *diminution* of common, or reſpirable air, in proportion to which it is always rendered unfit for reſpiration, indiſpoſed to effervesce with nitrous air, and incapable of farther diminution from any other cauſe.

All theſe proceſſes, I obſerved, agree in this one circumſtance, and I believe in no other, that the principle which the chemiſts call *phlogiſton* is ſet looſe; and therefore I concluded that the diminution of the air was, in ſome way or other, the conſequence of the air becoming overcharged with phlogiſton, and that water, and growing vegetables, tend to reſtore this air to a ſtate fit for reſpiration, by imbibing the ſuperfluous phlogiſton.

For this reaſon I ſaid, in the firſt account of theſe experiments, if it was thought convenient to intro-

duce a new term (or rather make a new application of a term already in uſe among chemiſts) it might not be amiſs to call air that has been diminiſhed, and made noxious by any of the proceſſes above mentioned, or others ſimilar to them, by the common appellation of *phlogiſticated air.*

Mr. Lavoiſier, however, has proved that in all theſe *phlogeſtic proceſſes*, as I termed them, of air, the purer, or dephlogiſticated part of it, is abſorbed, though he does not admit that any thing is emitted. In what I ſhall advance on the ſubject of phlogiſton, reaſons will be given for concluding that both thoſe things take place at the ſame, dephlogiſticated air uniting with the phlogiſton emitted from the ſubſtance to which the air is expoſed, and forming with it either nitrous acid, or fixed air.

PART

PART III.

MISCELLANEOUS OBSERVATIONS RELATING TO PHLOGISTICATED AIR.

SECTION I.

1. *Of the Purity of Air in different Circumſtances.*

WHEN I firſt diſcovered the property of nitrous air as a teſt of the wholeſomeneſs of common air, I flattered myſelf that it might be of conſiderable practical uſe, and particularly that the air of diſtant places and countries might be brought and examined together, with great eaſe and ſatisfaction; but I own that hitherto I have rather been diſappointed in my expectations from it. My own obſervations have not, indeed, been many; but according to them the difference of the open air in different places, as indicated by a mixture of nitrous air, is generally inconſiderable; and

I have reaſon to think that when very unwholeſome air is conveyed to a great diſtance, and much time elapſes before it is tried, it approaches, by ſome means or other, to the ſtate of wholeſome air. At leaſt ſuch I found to be the worſt air that has at any time been ſent to me into Wiltſhire from diſtant manufacturing towns and workſhops, &c. in them, where the air was thought to be peculiarly unwholeſome. I am ſatisfied, however, from my own obſervations, that air may be very offenſive to the noſtrils, probably hurtful to the lungs (and perhaps alſo in conſequence of the preſence of phlogiſtic matter in it) without the phlogiſton being ſo far *incorporated with it*, as to be diſcoverable by the mixture of nitrous air.

I gave ſeveral of my friends the trouble to ſend me air from diſtant places, eſpecially, from manufacturing towns, and the worſt they could find to be actually breathed by the manufacturers, ſuch as is known to be exceedingly offenſive to thoſe who viſit them; but when I examined thoſe ſpecimens of air in Wiltſhire, the difference between them and the very beſt air in this county, which is eſteemed to be very good, as alſo the difference between them and ſpecimens of the beſt air in the counties in which thoſe manufacturing towns are ſituated, was very trifling.

Mr.

Mr. Boulton of Birmingham was ſo obliging as to ſend me a great variety of ſpecimens of air from that manufacturing town, along with an account of his own examination of them by the teſt of nitrous air. I ſhall only note his account of four of the ſpecimens, including the beſt and the worſt, and reducing his numbers to my own.

	Meaſures.
The air in a garden near the new church	1.39
The bottom of the old church ſteps, very low and cloſe - - -	1.45
The middle of Mr. Taylor's manufactory	
The horn button manufactory - -	

When I examined them myſelf, on the 12th of December, 1777, the former was as nearly as poſſible the ſame with the air of pretty high ground in Wiltſhire; ſo that the difference between the worſt air in the manufactories at Birmingham, and very good air was .06. On the 3d of July following, I examined the remainder of the ſame ſpecimens of air again, and found the difference between them and good air to be .02; and at the end of October it was only .01.

Dr. Percival alſo was ſo good as to ſend me ſeveral ſpecimens of air from Mancheſter, and one from his country houſe at Hart-hill, about three miles from Mancheſter, the higheſt and healthieſt ſituation in that part of the country. The air of

this place was nearly the ſame with that of Wiltſhire; and when I examined the ſpecimens he ſent me on the 3d of July, 1778, the meaſures of the teſt for this air were 1.27, of the air from a weaving ſhop in Mancheſter 1.305, and of the market place 1.295. The difference therefore between the former and pure air was only .035, and of the latter only .025.

The worſt air that I have yet found breathed by men, and that was ſent from a diſtance, was from a coal-pit in the neighbourhood of Briſtol. For the difference between good air and that which was taken in the ſhaft of the pit ten yards below the mouth was .07, and between the ſame and that which was taken where the men were at work was .21.

Mr. William Vaughan took the trouble to procure me a ſpecimen of air from a calico printing houſe, which was exceedingly offenſive, and I have no doubt of its having been taken very properly, and having been well ſecured from all communication with the external air; and yet when I examined it in Wiltſhire the difference between it and good common air was only .02.

Mr. S. Vaughan, ſenior, on his paſſage from Jamaica, brought me two bottles of air, one from the hold of the ſhip, intolerably offenſive, the other the freſh air above deck about 30′ N. but the difference

ference between these specimens of air, and the air of Wiltshire, was quite inconsiderable.

I have frequently taken the open air in the most exposed places in this country at *different times of the year*, and in different states of the *weather*, &c. but never found the difference so great, as the inaccuracy arising from the method of making the trial might easily amount to, or exceed.

2. *Of the State of the Air in Hot-houses.*

There is generally a sense of oppression, or difficult respiration, felt on entering a *hot-house*, which seems to proceed from something different from mere heat; for we feel nothing of that sensation in an equally warm, well aired room; but my observations on this kind of air would not have indicated any such thing. On the 3d of June, 1778, I took the air in three several hot-houses adjoining to each other, but having different degrees of heat, and found that one measure of that air and one of nitrous air occupied the space of 1.29 measures; when the result of the same experiment with the external air, taken at the same time, was 1.27, a difference certainly very inconsiderable.

3. *Of the State of the Air in Dining-Rooms.*

Large and *lofty rooms* are generally preferable to ſmall and low ones. But this is only the caſe when the ſame company confine themſelves in it the ſame ſpace of time, with the doors, &c. ſhut; for, having more air to breathe, it will certainly require more time to contaminate it. But when the company is large, or proceſſes are going on that will effectually contaminate the air (as many candles burning in the room, hot victuals, continuing a long time upon the table, &c.) a ſmall room is much preferable, unleſs there be an opening in the top of the large room, that will eaſily promote a change of air in it. Becauſe the occaſional opening of the door in a ſmall room will generally produce a ſufficient change of a great part of the air; whereas the height of the door bearing but a ſmall proportion to the height of a large and well proportioned dining room, the opening of the door, or even its continuing open, has very little effect. The extreme offenſiveneſs of the air in theſe circumſtances is not perceived by perſons who ſit in it from the beginning, but it is immediately perceived by perſons who ſtep out of the room, and return to it,

Dining one time in a company of not more than eight or ten perſons, in a large and very lofty room; and being called out preſently after the cloth was removed, I was much ſtruck with the offenſiveneſs of the air on my return; and being willing to aſcertain the *degree* in which it was injured, I took occaſion on ſome pretence or other, to pour the water from one full decanter into another, and putting in the ſtopper, ſaw that nobody opened it till the company ſeparated. I then took the decanter into my laboratory, and examined the air at my leiſure; when it appeared to be much contaminated. For one meaſure of this air, and one of nitrous air, occupied the ſpace of one 1.31 meaſures; when the ſame experiment being made with the air of a well ventulated room in the ſame houſe, the meaſures were 1.25. At the ſame time I breathed a quantity of air till it juſt extinguiſhed a candle, and found that the meaſures were 1.43. So that, had the air of the dining room received a little more than twice as much more phlogiſtic matter, as it was charged with by the breathing of theſe eight or ten perſons, the effluvia of the victuals, &c. a candle would not have burned in the room. I would adviſe, therefore, that when ſuch large dining rooms are built, proviſion be made for letting out the vitiated air at the top of them. For breathing ſuch contaminated air ſo long a time as it is now the

the cuſtom to do, at and after dinner, muſt be very hurtful. Otherwiſe if it were not inconvenient on other accounts, it would be better to have the dinner in one room, and the deſert in another.

SECTION II.

Of the Manner in which Air is phlogiſticated by Means of inflammable Air.

HAVING been led by various experiments to expect, and even to believe, that common air is uſually phlogiſticated by actually decompoſing a ſmall quantity of inflammable air, admitted to it in its naſcent ſtate (notwithſtanding large quantities of inflammable air ready formed have no ſenſible effect upon it) I wiſhed to aſcertain ſo extraordinary a fact, by ſome experiments of a more deciſive nature, and with that view I made the following.

Firſt I took a pot of iron filings and ſulphur, which I had found to have been in a ſtate of yielding inflammable air in water about three months, and which

which I therefore presumed would continue for some time in the same state. This pot being introduced to a quantity of common air made no addition to it, but diminished it, and phlogisticated it as usual.

I then took a quantity of this mixture, which had yielded inflammable air many months, in a vessel of water. On the 22d of September I introduced some common air into the vessel in which it was contained, and on the 26th of October I observed that, though this mixture, now covered with water, had thrown up bubbles of air, which mixed with the common air on the surface of the water, that air was sensibly diminished, though not more than one tenth in all, and being examined was found to be phlogisticated, and to have nothing inflammable in it. At the same time a quantity of dephlogisticated air, exposed in a similar manner, was diminished from eleven and a half to one third; and from having been very pure, the measures of the test, with two equal quantities of nitrous air, were now 1.24, A candle burned in it better than in common air, but there was nothing inflammable in it.

But the most decisive experiment that I made of this kind was the following. A quantity of iron filings and surphur were mixed, and put into a phial filled up with water, on the 24th of June 1779, and on the 25th of July following it had yielded a quantity of inflammable air, which was then all taken out; and

and on the 22d of September more inflammable air was produced, about two ounce meafures in all. Having by this means fatisfied myfelf that this mixture was in a ftate of yielding inflammable air, I introduced to it a quantity of common air (the phial having been always kept inverted in a bafon of water) and on the 26th of October I found the common air very confiderably diminifhed. This air then being thrown out, the mixture was kept in the phial, now filled with water. In thefe circumftances, it continued to yield air, and when an ounce meafure and a half was produced, which was on the 24th of March 1780, I examined it, and found it to be ftrongly inflammable. There could therefore be no doubt but that the common air in this experiment had been diminifhed and phlogifticated by an addition of inflammable air in its nafcent ftate, or rather after it was completely, though but *newly formed.*

I do not by any means infer that, becaufe common air was diminifhed in this cafe, in confequence of an acceffion of inflammable air in its nafcent ftate, that it is never diminifhed in any other manner; but perhaps it will be found that all the fubftances which we know to phlogifticate common air are likewife capable of yielding inflammable air, if not in the temperature of the atmofphere, at leaft by means of *heat*, or in fome other procefs. This I found to be the cafe with metals, and

and it has been ſeen to be ſo, in one inſtance, with liver of ſulphur, and it is remarkably the caſe with all putrefactive ſubſtances. Vaults containing human excrements are often found to abound with inflammable air, and they, like other putrefactive ſubſtances, diminiſh common air.

The preceding experiments on the phlogiſtication of common air by means of inflammable air, led me to try whether a degree of heat ſhort of ignition would not make a quantity of inflammable air, ready formed, part with its phlogiſton to common air. For this purpoſe I mixed two thirds of common air with one third of inflammable air, and I kept them as hot as I could do without melting the glaſs veſſel in which they were contained ſeveral hours; but afterwards they occupied the ſame dimenſions as before. This air was confined in a glaſs jar, the upper part of which was ſurrounded with hot coals, by means of the inſtrument Pl. III. fig. 4.

I then tried what *length of time* would effect in this caſe; but though I always found a very ſenſible, and ſometimes a conſiderable diminution, the phlogiſtication was never completed, and the progreſs of it always ſtopped, without ever proceeding farther in any length of time afterwards. The following facts in proof of this it may be juſt worth while to recite.

A mixture

A mixture of one third inflammable air and two thirds common air, which had been confined by water from the 30th of June 1779, was diminiſhed the 18th of March 1780 one twelfth, and burned with a lambent blue flame.

A mixture of one third inflammable air and two thirds common air, from ſome time in the month of June 1779, was diminiſhed one thirteenth on the 20th of July 1780; but it had been diminiſhed nearly as much on the 5th of October preceding. It burned with a conſiderable exploſion.

Another quantity, one third inflammable air from marſhes, and two thirds common air, from the ſame time, was diminiſhed one tenth; but on the 18th of March preceding it was diminiſhed one twelfth, and then burned with a blue flame. On the 20th of July 1780, it burned in the very ſame manner.

Exactly ſimilar to theſe were the experiments that I made with various mixtures of dephlogiſticated air and inflammable air. Air one half inflammable and one half dephlogiſticated, mixed in June 1779, was on the 20th of July 1780 diminiſhed one fifth; but it had been diminiſhed two thirds of this the 4th of October preceding. It burned with a conſiderable exploſion.

Another mixture of the ſame kind, and made at the ſame time, was, on the 3d of July 1779, diminiſhed

niſhed two eleventhſ, and on the firſt of January 1780, it was diminiſhed two fifteenths more. On the 20th of July 1780, it was in the very ſame ſtate, and burned with a very conſiderable exploſion.

SECTION III.

Of the Effect of Water on phlogiſticated Air.

IN the infancy of my experiments I concluded, that all kinds of air were brought by agitation in water to the ſame ſtate, the pureſt air being partially phlogiſticated, and air completely phlogiſticated being thereby made purer; inflammable air alſo loſing its inflammability, and all of them being brought into ſuch a ſtate, as that a candle would juſt go out in them. This inference I made from all the kinds of air with which I was then acquainted, and which did not require to be confined by mercury, being brought to that ſtate by agitation in a trough of water, the ſurface of which was expoſed to the open air; never imagining that when the air

in my jar was ſeparated from the common air by a body of water, generally about twelve inches in depth (adding that within to that without the jar) they could have any influence on each other. I have, however, been long convinced that, improbable as it then appeared to me, this is actually the caſe, though a fact ſo remarkable well deſerves to be farther attended to.

As I have no where recited any experiments which prove that this change in the ſtate of the air, agitated in water, depends upon that water having ſome communication with the atmoſphere, I ſhall do it here, after an account of ſome other experiments, from which it is equally evident that, notwithſtanding every poſſible precaution, the air will be changed if there be that communication.

I agitated, a quarter of an hour, about three ounce meaſures of air, phlogiſticated with nitrous air, in a veſſel containing twenty ounces of water, which had been boiled ſeveral hours, and while it was very warm; after which it was diminiſhed one ſixth, and was ſo much improved, that the ſtandard of it was 1.74. The next day I agitated the remainder of it another quarter of an hour in the water which had been boiled at the ſame time, when it was diminiſhed about one tenth, and the ſtandard of it was 1.57.

I alſo

I alſo agitated the ſame quantity of air, phlogiſticated with iron filings and ſulphur, in the ſame manner twenty minutes, when it was diminiſhed one ſeventh, and the ſtandard of it was 1.66.

I afterwards expelled a little air from the water in which this air had been agitated; and from that with which the firſt of theſe experiments was made, I got air, the ſtandard of which was 1.66; and from the water in which the ſecond experiment was made, air of the ſtandard of 1.33. In this a candle would have burned. It is evident, from the diminution of the air in theſe experiments, that part of it is abſorbed by the water. But it is no leſs evident, as will appear by the next experiment, that the place of ſome part of the air which had been abſorbed was ſupplied by a purer air, which the water had imbibed from the atmoſphere. That water does imbibe the purer part of atmoſpherical air, in preference to that which is impure, is evident from the air that is expelled from it by heat. For if the water be clear, and free from any thing that is putreſcent, the air expelled from it is generally of the ſtandard of 1.0; whereas, that of the atmoſphere, when the nitrous air is the pureſt, is about 1.2.

But the experiment which deciſively proved that my former concluſion was fallacious was the following. After expelling all air, by boiling, from a

quantity of water, I put to it, in ſeparate phials, air that had been phlogiſticated with iron filings and ſulphur; and corking them very well, I left them with their mouths in water, and occaſionally agitated them. After about two months I examined them, and I found both the air that was confined by the water, and alſo that air which by heat I expelled from the water, completely phlogiſticated. The ſame was the reſult of a ſimilar experiment, in which the air had been expelled from the water by the air-pump.

BOOK

BOOK VI.

OF SOME KINDS OF AIR THAT ARE READILY ABSORBED BY WATER.

PART I.

OF MARINE ACID AIR.

SECTION I.

The History of the Discovery of this Kind of Air.

BEING very much struck with the result of an experiment of Mr. Cavendish's, related Phil. 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 ac-

quainted 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 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.

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.

The

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

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 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 at firſt 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.

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 copper, 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, and therefore may be very properly called an *acid air*, or more restrictively, the *marine acid air*.

When this air is all expelled from any quantity of spirit of salt, which is easily perceived by the subsequent

ſequent vapour being condenſed by cold, the remainder is a very weak acid, barely capable of diſſolving iron.

In my firſt experiments on this ſpecies of air, I procured it from ſpirit of ſalt, but I afterwards, hit upon a much leſs expenſive method of getting it, by having recourſe to the proceſs by which the ſpirit of ſalt is itſelf originally made. For this purpoſe I fill a ſmall phial with common ſalt, pour upon it a ſmall quantity of concentrated oil of vitriol, and receive the fumes emitted by it in a veſſel previouſly filled with quickſilver, and ſtanding in a baſon of quickſilver, in which it appears in the form of a perfectly *tranſparent air*, being preciſely the ſame thing with that which I had before expelled from the ſpirit of ſalt.

This method of procuring acid air is the more convenient, as a phial, once prepared in this manner, will ſuffice, for common experiments, many weeks; eſpecially if a little more oil of vitriol be occaſionally put to it. It only requires a little more heat at the laſt than at the firſt. Indeed, at the firſt, the heat of a perſon's hand will often be ſufficient to make it throw out the vapour. In warm weather it will even keep ſmoaking many days without the application of any other heat.

On this account it ſhould be placed where there are no inſtruments, or any thing of metal, that can be corroded by this acid vapour. It is from dear-

bought experience that I give this advice. It may eaſily be perceived when this phial is throwing out this acid vapour, as it always appears, in the open air, in the form of a light cloud; owing, I ſuppoſe, to the acid attracting to itſelf, and uniting with, the moiſture that is in the common atmoſphere.

SECTION II.

The Effect of Marine Acid Air on Subſtances containing Phlogiſton.

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 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.

Iron filings, being admitted to this air, were diſſolved by it pretty faſt; half of the air, diſappearing, and the other half becoming inflammable air, not abſorbed

abſorbed by water. Putting chalk to it, fixed air was produced.

I had not introduced many ſubſtances to this air, before I diſcovered that by means of it, inflammable air was produced from moſt ſubſtances that contain phlogiſton.

Inflammable air was produced, when to this acid air I put ſpirit of wine, oil of olives, oil of turpentine, charcoal, phoſphorus, bees-wax, and even ſulphur. This laſt obſervation, I own, ſurprized 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 air very faſt, and the little that remained of it was inflammable air; which proves, that theſe calces contain phlogiſton.

As ſome remarkable circumſtances attend the abſorption of this acid air, by the ſubſtances abovementioned, I ſhall briefly mention them.

Spirit of wine abſorbs this air as readily as water itſelf, and is increaſed in bulk by that means. Alſo, when

when it is ſaturated, it diſſolves iron with as much rapidity, and ſtill continues inflammable.

Oil of olives abſorbs this air 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 air very faſt, turning brown, and almoſt black. No inflammable air was formed, till I raiſed more of the acid air than 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 air had continued in contact with the oil, the more phlogiſton it would have extracted from it.

Bees-wax abſorbed this air very ſlowly. About the bigneſs of a hazel nut of the wax being put to three ounce meaſures of the acid air, the air 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 air very faſt. About one fourth of it was rendered immiſcible in water, and was but weakly inflammable.

A ſmall

A ſmall bit of *phoſphorus*, perhaps about half a grain, ſmoked, and gave light in the acid air, 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 air was very little diminiſhed. Water being admitted to it abſorbed it as before, except about one fifth of the whole. It was but weakly inflammable.

Putting ſeveral pieces of *ſulphur* to this air, 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.

Notwithſtanding the affinity which this acid air appears to have with phlogiſton, it did not at firſt appear that it was capable of depriving all bodies of it. I found that dry wood, cruſts of bread, and raw fleſh, very readily imbibed this air, 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 air, 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,

other, even more than they would have done if it had been boiled or roaſted.

But I afterwards obſerved that, by giving it more time, it will extract phlogiſton from ſubſtances from which I at firſt concluded that it was not able to do it, as from dry wood, cruſts of bread not burned, and dry fleſh. As there was ſomething peculiar to itſelf in the proceſs or reſult of each of theſe experiments, it may not be improper to mention them diſtinctly.

Pieces of dry *cork wood* being put to the acid air, a ſmall quantity remained not imbibed by water, and was inflammable.

Very dry pieces of *oak*, being expoſed to this air a day and a night, after imbibing a conſiderable quantity of it, produced air which was inflammable indeed, but in the ſlighteſt degree imaginable. It ſeemed to be very nearly in the ſtate of common air.

A piece of *ivory* imbibed the acid vapour very ſlowly. In a day and a night, however, about half an ounce meaſure of permanent air was produced, and it was pretty ſtrongly inflammable. The ivory was not diſcoloured, but was rendered ſuperficially ſoft and clammy, taſting very acid.

Pieces of *beef*, roaſted, and made quite dry, but not burned, abſorbed the acid vapour ſlowly; and when

when it had continued in this ſituation all night, from five ounce meaſures of the air, half a meaſure was permanent, and pretty ſtrongly inflammable. This experiment ſucceeded a ſecond time exactly in the ſame manner; but when I uſed pieces of white dry *chicken fleſh*, though I allowed the ſame time, and in other reſpects the proceſs ſeemed to go on in the ſame manner, I could not perceive that any part of the remaining air was inflammable.

That inflammable air is produced from *charcoal* in acid air I obſerved before. I afterwards found that it may likewiſe be procured from *pit-coal*, without being charred.

Inflammable air I had alſo obſerved to ariſe from the expoſure of ſpirit of wine, and various *oily* ſubſtances, to the vapour of ſpirit of ſalt. I have ſince made others of a ſimilar nature, and as peculiar circumſtances attended ſome of theſe experiments, I ſhall recite them more at large.

Eſſential oil of mint abſorbed this air pretty faſt, and preſently became of a deep brown colour. When it was taken out of this air it was of the conſiſtence of treacle, and ſunk in water, ſmelling differently from what it did before; but ſtill the ſmell of the mint was predominant. Very little or none of the air was fixed, ſo as to become inflammable;

mable; but more time would probably have produced this effect.

Oil of turpentine was alſo much thickened, and became of a deep brown colour, by being ſaturated with acid air.

Ether abſorbed acid air very faſt, and became firſt of a turbid white, and then of a yellow and brown colour. In one night a conſiderable quantity of permanent air was produced, and it was ſtrongly inflammable.

Having, at one time, fully ſaturated a quantity of ether with acid air, I admitted bubbles of common air to it, through the quickſilver, by which it was confined, and obſerved that white fumes were made in it, at the entrance of every bubble, for a conſiderable time.

At another time, having fully ſaturated a ſmall quantity of ether with acid air, and having left the phial in which it was contained nearly full of the air, and inverted, it was by ſome accident overturned; when, inſtantly, the whole room was filled with a viſible fume, like a white cloud, which had very much the ſmell of ether, but peculiarly offenſive. Opening the door and window of the room, this light cloud filled a long paſſage, and another room. In the mean time the ether was ſeemingly all vaniſhed, but ſome time after the ſurface of the quick-

quickſilver in which the experiment had been made was covered with a liquor that taſted very acid; ariſing, probably, from the moiſture in the atmoſphere attracted by the acid vapour with which the ether had been impregnated.

This viſible cloud I attribute to the union of the moiſture in the atmoſphere with the compound of the acid air and ether. I have ſince ſaturated other quantities of ether with acid air, and found it to be exceedingly volatile, and inflammable. Its exhalation was alſo viſible, but not in ſo great a degree as in the caſe above-mentioned.

Camphor was preſently reduced to a fluid ſtate by imbibing acid air, but there ſeemed to be ſomething of a whitiſh ſediment in it. After continuing two days in this ſituation I admitted water to it; immediately upon which the camphor reſumed its former ſolid ſtate, and to appearance was the very ſame ſubſtance that it had been before; but the taſte of it was acid, and a very ſmall part of the air was permanent, and ſlightly inflammable.

As the acid air ſeemed to have a near affinity with phlogiſton, I expected that the fumes of *liver of ſulphur*, which chemiſts agree to be phlogiſtic, would have united with it, ſo as to form inflammable air; but I was diſappointed in that expectation. This ſubſtance imbibed half of the acid air to which it was intro-

duced; one fourth of the remainder, after ſtanding one day in quickſilver, was imbibed by water, and what was left extinguiſhed a candle. This experiment, however, ſeemed to prove that acid air and phlogiſton may form a permanent kind of air that is not inflammable.

I afterwards made another experiment of the ſame kind, rather more deciſive than the former. I put ſeveral pieces of liver of ſulphur to a quantity of marine acid air; when I obſerved that it preſently began to be abſorbed, and it continued in that ſtate till one-half of the whole had diſappeared. By this time the liver of ſulphur, which had been of a greeniſh or yellowiſh colour, became white. Afterwards more liver of ſulphur abſorbed more of this air; but after two days the pieces began to diſſolve, and at length they became one liquid maſs, the air ſtill diminiſhing very gradually. In this ſtate I admitted water to the air; but by this very little more of it was abſorbed; and that which remained was about one-fourth of the original quantity, and extinguiſhed a candle. The whole proceſs was three days.

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SECTION III.

Miſcellaneous Obſervations relating to marine acid Air.

BEING deſirous of aſcertaining whether the marine acid air would combine with the ſame ſubſtances that the marine acid diſſolved, I made the trial with the flowers of zinc and red lead; and found that both theſe ſubſtances abſorbed a very great quantity of that air. I therefore conclude that whether the marine acid be combined with water, or not, it has the ſame affinity with theſe earthy ſubſtances.

I found that two grains and a half of rain-water abſorbed three ounce meaſures of this air, 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.

The following experiments are thoſe in which the *ſtronger acids* were made uſe of, and therefore they may aſſiſt us farther to aſcertain their affinities with

 certain

certain ſubſtances, with reſpect to this marine acid in the form of air. I put a quantity of ſtrong concentrated *oil of vitriol* to acid air, but it was not at all affected by it in a day and a night. In order to try whether it would not have more power in a more condenſed ſtate, I compreſſed it with an additional atmoſphere; but upon taking off this preſſure, the air expanded again, and appeared to be not at all diminiſhed. I alſo put a quantity of ſtrong *ſpirit of nitre* to it without any ſenſible effect. We may conclude, therefore, that the marine acid, in this form of air, is not able to diſlodge the other acids from their union with water.

Blue vitriol, which is formed by the union of the vitriolic acid with copper, turned to a dark green the moment it was put to the acid air, which it abſorbed, though ſlowly. Two pieces, as big as ſmall nuts, abſorbed three ounce meaſures of the air in about half an hour. The green colour was very ſuperficial; for it was eaſily wiped or waſhed off.

Green copperas turned to a deeper green upon being put into this acid air, which it abſorbed ſlowly. *White copperas* abſorbed this air very faſt, and was diſſolved in it.

Sal ammoniac, being the union of ſpirit of ſalt with volatile alkali, was no more affected with this acid air than, as I have obſerved before, common ſalt was.

When

When I put a piece of *nitre* to this air 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 acid air. In about a minute, the whole quantity of air was absorbed, except a very little, which might be the common air that had lodged upon the surface of the spirit of salt within the phial. I have since repeated the experiment; but the result was nothing more than might have been predicted; for the nitrous acid, dislodged from its base by the marine, had dissolved some of the quicksilver, and formed nitrous air, occupying one-half of the whole space that had been filled by the marine acid air.

A piece of *alum* exposed to this air turned yellow, absorbed it as fast as the nitre had done, and was reduced by it to the form of a powder. It had the same effect on borax. Common salt, as might be expected, had no effect whatever on this marine acid air.

Fine white *sugar* absorbed this air slowly, was thoroughly penetrated with it, became of a deep brown colour, and acquired a smell that was peculiarly pungent.

A piece of *quick-lime* being put to about twelve or fourteen ounce measures of this acid air, and continu-

ing in that ſituation about two days, there remained one ounce meaſure of air that was not abſorbed by water, and it was very ſtrongly inflammable, as much ſo as a mixture of half inflammable and half common air. Very particular care was taken that no common air mixed with the acid air in this proceſs. At another time, from about half the quantity of acid air above mentioned, with much leſs quick-lime, and in the ſpace of one day, I got half an ounce meaſure of air that was inflammable in a ſlight degree only. This experiment proves that ſome part of the phlogiſton which eſcapes from the fuel, in contact with which the lime is burned, adheres to it. But I am very far from thinking that the cauſticity of quick-lime is at all owing to this circumſtance.

The remaining experiments, in which this acid air was principally concerned, are of a miſcellaneous nature.

I put a piece of dry *ice* to a quantity of this acid air, taking it with a forceps, which, as well as the air itſelf, and the quickſilver by which it had been confined, had been expoſed to the open air for an hour, in a pretty ſtrong froſt. The moment it touched the air it was diſſolved as faſt as it would have been by being thrown into a hot fire, and the air was preſently imbibed. Putting freſh pieces of ice to that which was diſſolved before, they were alſo diſſolved immediately, and the water thus procured

procured did not freeze again, though it was exposed a whole night, in a very intense frost.

Flies and spiders die in acid air, but not so quickly as in nitrous air. This surprized me very much; as I had imagined that nothing could be more speedily fatal to all animal life than this pure acid vapour.

Marine acid air 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 beautiful green, or rather light-blue flame, such as is seen when common salt is thrown into the fire.

I shall conclude my account of these experiments with observing, that the electric spark is visible in this acid air, 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.

At another time, having made about fifty electric explosions of a common jar, in a small quantity of the *marine acid air*, confined in a glass-syphon by quicksilver, I observed that it was a little diminished, and that a small part of the inside of the glass, next to the quicksilver, was tinged white. Water admitted to this air absorbed so much of it, that no experiment could be made on the remainder.

PART

PART II.

OF VITRIOLIC ACID AIR.

SECTION I.

The History of the Discovery.

I Had no sooner exhibited the *marine acid* in the form of air, than it occurred to me that it might be possible to exhibit the other acids also in the same elegant manner, divested of the water with which they had hitherto been combined, and which must necessarily have been a great obstruction to the discovery of their real natures and affinities; but not being a practical chemist, and living in the country, where I had no access to any person of that profession (and indeed not being sufficiently able to explain my wants) I met with many hindrances in the prosecution of my inquiries into this subject.

My first scheme was to endeavour to get the *vitriolic* acid in the form of air, thinking that it

would probably be eaſy to confine it by quickſilver; for as to the nitrous acid, its affinity with quickſilver is ſo great, that I deſpaired of being able to confine it to any purpoſe. I therefore wrote to my friend Mr. Lane, to procure me a quantity of *volatile vitriolic acid*, which is the common vitriolic acid combined with phlogiſton, at the time that I was intent upon the proſecution of my former experiments; but by ſome miſtake of my meaning, a different thing from what I intended was ſent me.

Seeing Mr. Lane the winter following, he told me that if I would only heat any oily or greaſy matter with oil of vitriol, I ſhould certainly make it the very thing I wanted, viz. the *volatile*, or *ſulphureous vitriolic acid*; and accordingly I meant to have proceeded upon this hint, but was prevented from purſuing it, by a variety of engagements.

Some time after this, I was in company with Lord Shelburne at the ſeat of Monſ. Trudaine, at Montigny in France; where, with that generous and liberal ſpirit by which that nobleman is diſtinguiſhed, he has a complete apparatus of philoſophical inſtruments, with every other convenience and aſſiſtance for purſuing ſuch philoſophical inquiries as any of his numerous gueſts ſhall chuſe to entertain themſelves with. In this agreeable retreat

treat I met with that eminent philoſopher and chemiſt, Monſ. Montigni, Member of the Royal Academy of Sciences; and converſing with him upon this ſubject, he propoſed our trying to convert oil of vitriol into vapour, by boiling it with a pan of charcoal in a cracked phial. This ſcheme not anſwering our purpoſe, he next propoſed our heating it together with oil of turpentine. Accordingly we went to work upon it, and ſoon produced a quantity of ſome kind of air confined by quickſilver; but our recipient being overturned by the ſuddenneſs of the production of air, we were not able to catch any more than the firſt produce, which was little elſe than the common air which had lodged on the ſurface of the liquor, and which appeared to be a little phlogiſticated, by its not being much affected by a mixture of nitrous air.

Having no opportunity of repeating the experiment at that time, I did nothing with a view to it till my return to England; when, on the 26th of November, 1774, I reſumed the operation, beginning with *olive oil*, and by the help of a more convenient kind of glaſs veſſel, repreſented fig. *a*, Pl. IV. which I had procured for theſe and other ſimilar purpoſes, I found very little difficulty in the proſecution of the experiments.

As I wiſh that my reader may enjoy the benefit of my experience, I would caution him, if he chuſe

to

to repeat the experiments, not to put too much oil, or any other ſimilar ſubſtance, to the oil of vitriol, in order to produce this air. I began with uſing about one fifth part of common oil, leaving ſpace enough, as I thought, in the phial, for the ebullition that might be occaſioned by the production of air. But as ſoon as the veſſel was heated to a certain degree, the production of air was exceedingly rapid; and though I withdrew the candle which I had applied to it for that purpoſe, the ebullition continued to increaſe, till, the capacity of the tube not being ſufficient for the tranſmiſſion of the generated air, the cork was driven out of the phial, and all the contents of it exploded.

After this I only ſlightly covered the ſpirit of vitriol in the phial with olive oil, and then the phenomena were ſimilar to thoſe in the former experiment, at the ſame time that the proceſs was more manageable; for, by applying or withdrawing the candle, as I ſaw occaſion, I got what quantity of air I pleaſed; and removing the phial, in this ſtate of ebullition, from one veſſel to another, I filled ſeveral of them with this new ſpecies of air, as eaſily as I had been uſed to do it with the marine acid air; and the whole proceſs was as pleaſing and as elegant. Indeed, this manner of producing air from ſubſtances contained in ſmall phials, and receiving the produce in quickſilver, when it is of

ſuch

ſuch a nature that it cannot be confined by water, has never failed to ſtrike every perſon to whom I have ſhewed it.

The moment that I ſaw the acid of vitriol aſſume the form of air by the addition of phlogiſton, I concluded that the marine acid alſo muſt have been capable of being exhibited in the ſame manner, by means of the phlogiſton which it naturally contains, and which is inſeparable from it; and moreover, that, probably, ſome portion of phlogiſton may be neceſſary to the volatility and elaſticity of all ſubſtances whatever; ſo that the marine acid air may not be preciſely what I had before imagined, viz. the *pure marine acid in the form of air*; but that, though it is by this means exhibited free from water, which, in a variety of reſpects, modifies and reſtrains its action upon various bodies, it is ſtill combined with a portion of phlogiſton. Since, however, all the bodies with which we are acquainted are, in ſome degree, elaſtic, being capable, at leaſt, of being condenſed by cold, and dilated by heat, it may not be poſſible to ſeparate this principle intirely from any ſubſtance in nature; and therefore, in a ſenſe ſufficiently near the truth, it may ſtill be ſaid that the marine acid air is nothing but the marine acid; the phlogiſton it contains being ſo ſmall, as not to be diſcoverable by any of the uſual teſts of its preſence.

Before

Before any air is produced from the mixture of inflammable matter and oil of vitriol, the whole quantity becomes very black; and a quantity of this ſpirit, thus impregnated with phlogiſton, will yield many times more air than an equal quantity of the ſtrongeſt ſpirit of ſalt; but I never meaſured it with any exactneſs.

When the vitriolic acid air is produced in great plenty, the top of the phial in which it is generated is generally filled with white vapours. This air has alſo the ſame appearance as it is tranſmitted through the glaſs tube, and it is ſometimes diſcoverable in the recipient.

SECTION II.

Of Vitriolic Acid Air from Metals, and other Substances containing Phlogiston.

I Got no air from the oil of vitriol by any application of heat. But in attempting to procure it, I got it by means of *mercury*, in a manner that I little expected, and I paid pretty dearly for the discovery it occasioned. Despairing to get any air from the longer application of my candles, I withdrew them; but before I could disengage the phial from the vessel of quicksilver, a little of it passed through the tube into the hot acid; when, instantly, it was all filled with dense white fumes, a prodigious quantity of air was generated, the tube through which it was transmitted was broken into many pieces (I suppose by the heat that was suddenly produced) and part of the hot acid being spilled upon my hand, burned it terribly, so that the effect of it is visible to this day. The inside of the phial was coated with a white saline substance, and the smell that issued from it was extremely suffocating.

This accident taught me what I am surprized I should not have suspected before, viz. that some

metals will part with their phlogiſton to hot oil of vitriol, and thereby convert it into a permanent elaſtic air, producing the very ſame effect with oil, charcoal, or any other inflammable ſubſtance.

Not diſcouraged by the diſagreeable accident above-mentioned, the next day I put a little *quickſilver* into the phial with the ground ſtopper and tube, along with the oil of vitriol; when, long before it was boiling hot, air iſſued plentifully from it; and being received in a veſſel of quickſilver, appeared to be genuine vitriolic acid air, exactly like that which I had procured before; being readily imbibed by water, and extinguiſhing a candle in the ſame manner as the other had done. A white ſalt was formed; but what I thought a little remarkable, was, that, whereas in all the former experiments the oil of vitriol turned black before it yielded any air; this was not the caſe here; for it continued colourleſs and tranſparent during the whole proceſs.

After this I repeated the experiment with ſeveral other metals; but with a conſiderable variety in the reſults.

Putting pieces of *iron wire* into the oil of vitriol, a very ſmall quantity of air was produced without heat; but this ſoon ceaſing, I applied the candle, when, with a degree of heat, ſeemingly greater than that at which the air had riſen from the quickſilver in the ſame circumſtances, air was produced in great

plenty. When I had got about three ounce meaſures of it, I admitted water to it, and about four fifths of the whole was preſently abſorbed. The remainder was inflammable, burning very red.

Had the oil of vitriol been more concentrated, or had I continued the proceſs longer, a greater proportion of the air would probably have been acid, and leſs of it inflammable. In this experiment the oil of vitriol became very opaque, being of a deep grey colour. The iron which had undergone this proceſs, and which I had laid aſide without any expectation, was, in a few days, covered with a whitiſh duſt; and after it had been wiped clean, was covered again with the ſame matter. It is very much unlike the ruſting of iron in other circumſtances.

About one third of the produce of air from *zinc*, was acid, and the remainder inflammable. Indeed it was evident that the acid had a conſiderable effect upon the zinc before the application of the candle, ſmall bubbles of air continually riſing from it. The oil of vitriol, which had been uſed in this proceſs, after a long time, depoſited a white matter, which I ſuppoſe to be the *flowers of zinc*.

Copper, treated in the ſame manner, yielded air very freely, with about the ſame degree of heat that quickſilver had required, and the air continued to be generated with very little application of more heat. The whole produce was vitriolic acid air, and no

part

part of it inflammable. The oil of vitriol remained a long time turbid, but at length depoſited a brownifh matter.

The ſolution of *ſilver* in the ſame manner, had the very ſame reſult, all the air being acid, and no part of it inflammable. The oil of vitriol acquired a kind of orange-colour, and depoſited nothing.

With a very great degree of heat *lead* yielded a little air, which was wholly acid, and had nothing inflammable in it.

Gold yielded no air at all in this treatment; but the oil of vitriol acquired the ſame orange-colour that it had when the ſilver had been heated in it.

Neither had this treatment of *platina* any ſenſible effect. What I made uſe of was ſome which I had been favoured with from Dr. Irving, carefully purged from iron.

In moſt of theſe proceſſes, air ſeems to iſſue from the ſubſtances immediately upon the application of heat, and ſometimes without it: and this firſt produce of air forms bubbles, which continue ſome time on the ſurface of the liquor. But it ſeems to be nothing more than the common air which had adhered to the ſurfaces of thoſe ſubſtances, or had been confined in the little cavities near the ſurface, when they happened to be rough. For this ſeeming production of air ſoon ceaſes, and no more is produced without a much greater degree of heat; and

and when the genuine acid air begins to rise, bubbles formed by it break instantly, like bubbles of air in spirit of wine, and there is nothing like froth on the surface of the oil of vitriol.

As sulphur is formed by the union of phlogiston with oil of vitriol highly concentrated and very hot, I imagined that by heating substances containing phlogiston in vitriolic acid air, I could not fail to to produce sulphur; but I tried charcoal in this manner without the effect that I had expected from it The heat of a burning lens thrown upon it in this acid air, only made it throw out that quantity of the air, which, as I have observed before, is condensed upon its surface, or imbibed by it. The air that was unabsorbed after this operation was in part fixed, and in part inflammable, having come from the charcoal.

When I endeavoured to procure this air by the same process from *ether*, about one half of the produce was permanent and inflammable. The oil of vitriol became perfectly black by this means, as in heating other phlogistic matters in it. Afterwards, heating the same oil of vitriol and ether, about one fourth of the produce only was inflammable; and had I continued to use the same mixture, the produce would probably have been less and less inflammable, and more purely acid, every experiment.

I once had a quantity of concentrated acid of vinegar, from which I expelled air by heat, and found it very much, if not in all respects to resemble vitriolic acid air, and being informed that oil of vitriol was used in the preparation of it, I imagine that it was vitriolic acid air only, especially as I have not found any other vegetable acid liquor that will yield air; at least in a quantity sufficient for any experiments. I tried *radical vinegar of the crystals of verdegris rectified*, which was recommended to me, and made for me, by Mr. Woulfe, and also concentrated acid made from *sal diureticus*, by Mr. Godfrey; but neither of these acid liquors, though the smell of them was extremely pungent, yielded any air by heat.

The common air expelled from the phial by the steam of this vinegar, mixed with whatever acid vapour might come over along with it, I examined, after letting it rest upon quicksilver a whole night, and I found it not to differ from common air.

When, however, I tried this experiment with air that had lodged on the surface of oil of vitriol, into which I had put some *sal diureticus*, and which did yield a little air, the common air did appear to be injured by the mixture, as in the experiment of the mixture of common and vegetable acid air. But then oil of vitriol being employed in this

this experiment, as well as in the preparation of the concentrated vinegar above-mentioned, it is liable to the ſame objection; the acid of vitriol being, perhaps, volatilized by ſome ſmall portion of phlogiſton.

SECTION III.

Of Water impregnated with Vitriolic Acid Air.

WATER being admitted to the vitriolic acid air abſorbed it about as readily as the marine acid air; and by its union with it muſt have formed the volatile or ſulphureous acid of vitriol. Indeed the reſult of this combination was ſo obvious, that I did not think it neceſſary to make the experiment.

I often conſidered what can make the very great difference between the common vitriolic acid, and water fully impregnated with the vitriolic acid air. To judge *a priori*, from the analogy of other acids, one would imagine that there could be no great dif-

ſerence between them. Water impregnated with marine acid air is, in all reſpects, the very ſame thing with the common ſpirit of ſalt, except that this acid may be made conſiderably ſtronger in this manner than any ſpirit of ſalt made in the common way, and that it has generally leſs colour. Alſo water impregnated with nitrous acid vapour is the ſame thing with common ſpirit of nitre, except that the acid thus communicated is more volatile, and the colour is changed. But water impregnated with *vitriolic acid air* differs moſt remarkably from oil of vitriol. Its acidity is now become trifling to what it was; and from being the moſt fixed, and the ſtrongeſt, it is now become the weakeſt, and the moſt volatile of all acids; the ſmell of it being intolerably pungent, and almoſt the whole of it evaporating when it is expoſed to the open air.

This great difference ſeems, however, to be wholly occaſioned by the phlogiſton ſuperadded to the vitriolic acid. This principle united to the acid in a manner neutralizes it, forming a kind of ſulphur, in which the acidity is, in a great meaſure loſt. To the phlogiſton, alſo, which it has now acquired is owing its extreme volatility, to both which properties phlogiſton is known in other caſes to contribute. The different manner in which the vitriolic and nitrous acids combine with phlogiſton,

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and the various results from these combinations, may furnish much matter for speculation and experimental inquiry, for which we now seem to be excellently well prepared.

For a long time I concluded that water impregnated with vitriolic acid air was intirely incapable of dissolving any metal, so as to yield inflammable air; but at length I succeeded in this experiment; which convinced me that the acid is not essentially altered in this process. For having completely saturated a quantity of water with vitriolic acid air, I let it continue upon the quicksilver in which the process had been made, with a considerable quantity of superabundant acid air upon it, for about ten days; both to produce a complete saturation, and likewise to make, if possible, a more perfect combination of the acid with the water. Then, decanting it as gently as possible, and leaving it in a phial with a small perforation in the cork, that the more volatile part might escape gradually, I poured a quantity of the remainder upon some filings of zinc; upon which, with the application of heat, a considerable quantity of air was produced, and this was all strongly inflammable.

The quantity of *acid* in water impregnated with vitriolic acid air, may, in some measure, be compared with the quantity of acid in the oil of vitriol

from which it is expelled, by the method which I made uſe of in the following experiment. I put to a quantity of oil of vitriol rather more copper than it could diſſolve; and obſerved that air was expelled till nothing fluid remained in the phial, and by this time the volatile acid which it had yielded had completely ſaturated eight or ten times its bulk of water. The reſiduum was blue vitriol, containing the crude oil of vitriol united to the copper.

Now if the quantity of this oil of vitriol be eſtimated, and this experiment be repeated with a little more care, the exact proportion of acid in the oil of vitriol, and in water fully impregnated with vitriolic acid air, may be determined. When the ſalt above mentioned was waſhed in water, a blackiſh matter was ſeparated from it; but all the reſt was pure vitriol, of a light green colour, but which became white, or grey, when it was dried by the fire.

The greater degree of acidity in water impregnated with marine acid air, and its greater power of diſſolving metals, ſo as to produce inflammable air, will not be wondered at, when it is conſidered how much more *marine* acid air, than of *vitriolic* acid air, the ſame quantity of water will imbibe. In order to aſcertain this, I took two ſmall glaſs tubes, cloſed at one end, and put into each of them, as

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nearly as poſſible, two grains of rain water, out of which the air had been pumped; nearly filling them, but ſo that there might be room enough for the water to expand with the vapour they might imbibe, and then introduced them into jars filled with each of the kinds of air, ſtanding in quickſilver. I then obſerved that the water in the marine acid air imbibed it very faſt, and diminiſhed the bulk of it, in all, one ounce meaſure and three quarters, whereas the water in the vitriolic acid air imbibed it very ſlowly, and, in all, not more than occupied the ſpace of three pennyweights and ſixteen grains of water; ſo that water imbibes about ten times as much marine acid air, as of vitriolic acid air.

The water ſaturated with marine acid air at this time was, as far as I could perceive, quite colourleſs, and it was ſo ſtrongly impregnated, that the warmth of my hand only made it abſolutely boil by the emiſſion of its air, and it ſmoked copiouſly.

Conſidering the much greater quantity of marine acid air, than of vitriolic acid air, that a quantity of water will take up, I was rather ſurprized to find that when water was ſaturated with the latter, it could not be made to imbibe a greater quantity of the former than I perceived it to do; though I was not ſurprized to find that when water was fully impregnated with the former, it ſhould take but a little more of the latter, in the following experiments.

Putting vitriolic acid air to water fully ſaturated with marine acid air, it imbibed but little, and that very ſlowly; in all, about four times its bulk. It was obſervable, however, that the vitriolic acid air intirely diſcharged the ſtraw colour of the marine acid, and gave it a blueiſh tinge.

Having ſaturated a quantity of water with vitriolic acid air, it would not afterwards take marine acid air, except in a ſmall quantity, and very ſlowly. N. B. The bubbles of marine acid air, which burſt in the ſuperabundant vitriolic acid air, which lodged on the ſurface of the water, emitted a white fume.

It is remarkable that water impregnated with vitriolic acid air retains all its air when it is frozen, though every other kind of air (if the liquor containing it can be frozen at all) is ſeparated from it in the act of freezing. I have now farther obſerved, that this ice ſinks in the liquor from which it is frozen, in which it reſembles the ice of oil. This is a fact which I barely mention, without having any theory to account for it.

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SECTION IV.

Various Properties of vitriolic Acid Air.

1. *Vitriolic Acid Air and marine Acid Air compared.*

VITRIOLIC acid air is equally tranſparent with marine acid air, and ſeems to have no more affinity with quickſilver; for when confined by quickſilver, the dimenſions of it are not liable to any variation, excepting by heat and cold, juſt like common air; provided there be no moiſture in the recipient, or in the quickſilver. As the reſemblance between theſe two acid airs was ſo great, it was natural for me to have a view to the experiments I had made with the marine acid air, in conducting thoſe that relate to the vitriolic acid, which the reader will eaſily perceive.

Like the marine acid air, this vitriolic acid air extinguiſhes a candle, but without any peculiar appearance in the colour of the flame, as it goes out, or as it is lighted again, which is obſervable when the experiment is made with the marine acid air. Vitriolic acid air is alſo heavier than common air; for a candle being let down into a veſſel filled with it,

it, was extinguiſhed many times ſucceſſively, and even after it had ſtood a full hour with its mouth expoſed to the common air.

This acid air bears to be expoſed to cold, without any greater diminution of its bulk than common air is ſubject to in the ſame circumſtances; which appears to me to be a ſufficiently proper criterion to diſtinguiſh *air* from *vapour*. In a certain degree of heat, indeed, even *water* may be exhibited in the form of air; but it is a degree of heat that far exceeds what is uſual in our atmoſphere; and in other caſes terms are applied to very great uſe, for the diſtinction of bodies, which, if examined with ſtrictneſs, would be found ultimately to run into one another, the difference between them being in *degree* rather than in *kind:* but a very *great difference* in degree affords a ſufficient foundation for a difference in appellation.

2. *The Mixture of vitriolic Acid Air and Alkaline Air.*

The phenomena which attend the mixing of alkaline air with the marine acid air, were ſo ſtriking, that I had not been many hours in poſſeſſion of the vitriolic acid air without trying whether the effect of the ſame mixture with this acid air would not make a ſimilar appearance, and the experiment

fully anſwered my expectations. A like beautiful white cloud was formed the moment that theſe two kinds of air came into contact, the quantity of air was diminiſhed as faſt as the alkaline air was admitted, and the quickſilver roſe almoſt to the top of the receiver.

I obſerved alſo, that when I put the alkaline air to the vitriolic acid air, the white cloud roſe immediately to the top of the veſſel, as in the experiment with the marine acid air; which proves that the alkaline air is, in both caſes, the lighter of the two. In both caſes alſo, if the alkaline air be produced firſt, the acid air being admitted to it, forms a cloud which reſts upon the quickſilver; never extending beyond a very ſmall ſpace, and riſing only as the quickſilver riſes. The ſubſtance that is formed by the union of the alkaline air with the vitriolic acid air, muſt neceſſarily be the *vitriolic ſal ammoniac*; but I made no experiment to aſcertain it. The quantity of this ſalt with which my receivers are coated in theſe experiments is readily diſſolved in water, as in the experiments with the marine acid air. This, however, it will be ſeen, is not the caſe with the ſalt that is formed by another of the acid airs with alkaline air.

There was frequently, however, the appearance of ſulphur produced upon the mixture of alkaline air with vitriolic acid air; for the inſide of the tube would

would be covered with a perfectly yellow matter. But this colour goes off in time, and nothing but a white ſaline ſubſtance remains. This yellow appearance I firſt obſerved when I had produced the vitriolic acid air from ether; but afterwards I found the ſame effect when it was produced from charcoal, and ſtill more remarkably when it had been produced from copper. Why this yellow colour ſhould not be permanent, I do not underſtand.

Being willing to try whether, by making repeated mixtures of theſe two kinds of air, I could not fix this colour, and collect a quantity of the ſubſtance in which it inheres, I filled the ſame jar alternately with each of theſe kinds of air, till all the inſide of the veſſel ſeemed to have got a good coating of this ſubſtance. But, being expoſed to the open air, the colour in this caſe alſo ſoon diſappeared, and the ſaline ſubſtance with which all the inſide of the tube was uniformly coated, became perfectly white.

3. *Vitriolic Acid Air mixed with other Kinds of Air.*

The mixture of other kinds of air with vitriolic acid air produced no remarkable appearance whatever. When, however, I had put a quantity of this acid air to a quantity of common air, in order to obſerve whether the former might not part with ſome

ſome of its phlogiſton to the latter, though I perceived no immediate diminution of the bulk of air, as in the mixture of nitrous and common air; yet when they had continued together two days, and water being admitted to the mixture had abſorbed the acid air, the common air which remained appeared, by the teſt of nitrous air, to be conſiderably injured; ſo that the phlogiſton in the vitriolic acid air muſt have united to ſome part of the pure air contained in the common air, which is an effect that is not produced by the marine acid air when mixed with common air. In reality, the vitriolic acid air muſt have attracted dephlogiſticated air, from the atmoſphere, and have thereby formed the common vitriolic acid. What effect the vitriolic acid air would have had upon other kinds of air, had they continued together a longer time, I cannot tell.

A quantity of this acid air mixed with inflammable air ſtood ſome hours; but when water had been admitted to them, I could not perceive either that the quantity of inflammable air was altered, or that its inflammability was in the leaſt impaired.

I once put equal quantities of marine and vitriolic acid air into the ſame receiver, and obſerved that they mixed without exhibiting any appearance whatever; and when alkaline air was admitted to them, the appearance was the ſame as if it had been admitted to either of them ſingly, the white

cloud rising instantly to the top of the vessel. Had I, after the experiment, examined the *salts* which adhered to different parts of the inside of the vessel, I might perhaps have discovered which of the two acid airs was specifically lighter than the other; but I suspect that they were intimately mixed, and therefore that the salt was a mixture of the common and the vitriolic sal ammoniac.

4. *Substances containing Phlogiston exposed to vitriolic Acid Air.*

I thought it rather extraordinary, that whereas the marine acid, which is reckoned the weakest of all the three mineral acids, should, when exhibited in the form of air, be able to dislodge both the vitriolic and the nitrous acids from several of their bases; yet that this vitriolic acid, which is reckoned the strongest of the three, when seemingly exhibited to equal advantage, by being divested of the water with which it is usually combined, should not, in any instance in which I made the experiment, dislodge either of the other acids from any basis with which they were united. *Nitre*, *common salt*, and *sal ammoniac*, were all introduced to this air, without either affecting it, or being affected by it.

Oils imbibe vitriolic acid air, and pretty rapidly, in proportion to their tenuity; though they cannot be made to imbibe so much of it as *water* can

can. Being willing to ſee the utmoſt effect of the impregnation of ſeveral kinds of oil with this acid air, I took about equal quantities of oil of turpentine, olive oil, and whale oil, and gave each of them an opportunity of being fully ſaturated; having kept them conſtantly ſupplied with freſh air, when they had imbibed a former quantity, and frequently diſcharging that part of the air which they could not imbibe, and beginning the proceſs a new.

Both the whale oil and the olive-oil imbibed this air very ſlowly, being ſeveral hours in taking their bulk of it; but whereas the firſt effect of the impregnation of the whale-oil was imparting to it a reddiſh colour, the olive-oil became quite colourleſs. In all, they both imbibed ſix or eight times their bulk of this air.

In this manner they continued in glaſs jars confined by quickſilver about a month, when I obſerved that the oil of turpentine was become of a beautiful amber colour, the olive-oil was darker, and the whale-oil darker ſtill; and they all ſmelled extremely pungent, by the eſcape of the acid air from them. Their tenacity was not ſeemingly increaſed.

Vitriolic ether imbibes vitriolic acid air as readily as water imbibes it. The ether, however, was ſoon ſaturated with it, and afterwards was, to all appearance, both as tranſparent, and as inflammable as before.

A piece

A piece of *phosphorus* remained a day and two nights in vitriolic acid air, without sensibly affecting it. It gave no light in this air; but the upper surface of it turned black, and the surface of the quicksilver on which it lay, had a deep yellow or blackish kind of scum upon it, as if it had been in part dissolved by the acid.

A piece of *liver of sulphur*, in three days, absorbed the whole of a quantity of this kind of air, without sensibly affecting the colour or appearance of the liver of sulphur.

Charcoal, which forms inflammable air, by being introduced to marine acid air, only absorbs the vitriolic acid air; which, however, it does pretty rapidly, and acquires a pungent smell from being exposed to it, without producing any other effect that I could perceive. I made several pieces of charcoal imbibe as much of this acid air as they could; but, after this, fresh pieces absorbed the remainder, so that the air had only been, as it were, condensed on its surface. This I have observed to be the case with alkaline air, and in some experiments with other kinds of air which cannot be confined but by quicksilver; and I do not clearly understand it. The charcoal, in this experiment, was made very dry, or it might have been suspected that the moisture adhering to it had absorbed the air.

Vitriolic

Vitriolic acid air diſſolved *camphor* pretty readily, and reduced it to a tranſparent liquor. Water being admitted to it, the camphor re-aſſumed its natural ſolid form, but ſeemed to have acquired an acidity in its taſte

Iron is readily diſſolved in marine acid air, but it is not at all affected by the vitriolic acid air; though, when combined with water, it is ſo powerful a menſtruum for iron. But this, indeed, is the leſs extraordinary, as this acid ceaſes to affect iron when it is ſtrongly concentrated. I kept a number of iron nails in vitriolic acid air two days, without any ſenſible effect either upon the air, or the nails. There was no appearance of their being in the leaſt corroded.

I have noticed a very remarkable effect of alkaline air upon alum, rendering it white and opake, as if it had been calcined, but without altering its figure. The ſame, to appearance, is the effect of vitriolic acid air upon *borax*. This ſubſtance abſorbed a pretty large quantity of this air in two days. What remained of the air extinguiſhed a candle. But this effect was probably owing to a ſmall proportion of fixed air that was produced at the ſame time with the vitriolic acid air. I repeated this experiment with borax, and let the proceſs continue three days, when the effect was preciſely the ſame as before, the borax retaining its form, but be-

ing rendered white and opake. The acid air had, no doubt, ſeized upon the water which enters into its compoſition, as I conjecture to be the caſe with reſpect to alkaline air and alum.

As it is well known that the common vitriolic acid is changed into volatile or ſulphureous acid of vitriol by fumes of charcoal, if the veſſel in which it is heated has a crack in it, through which the fumes can have acceſs to the acid, I had the curioſity to try whether the ſame effect would not be produced by heating the charcoal in the acid. Accordingly I put ſome bits of *charcoal* into my phial, inſtead of the oil, or other inflammable matter, which I had uſed before; and, applying the flame of a candle, I preſently found that the vitriolic acid air was produced as well as in the former proceſs, and in ſeveral reſpects more conveniently, the production of air being more equable; whereby the diſagreeable effect of a ſudden exploſion is avoided.

It is neceſſary, however, that the charcoal be very well burned, ſo that all air be expelled from it; otherwiſe, there will be a mixture of fixed or inflammable air along with the acid air, eſpecially when a conſiderable degree of heat is applied to produce the air. Having often got vitriolic acid air from charcoal on account of the eaſy and equable production of it in this manner, I ſeveral times obſerved that there was a conſiderable reſiduum after

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it had been expoſed to water: and once I found that the reſiduum made lime-water turbid; a ſure ſign of its containing fixed air.

SECTION V.

Of taking the Electric Spark in Vitriolic Acid Air.

ONE of the moſt remarkable obſervations that I have made on vitriolic acid air was, that when the electric ſpark is taken in it, the inſide of the glaſs tube in which it is confined is covered with a blackiſh ſubſtance; ſo that the inſide of the glaſs through which the exploſion paſſed was uniformly covered with a blackiſh matter, and nothing could be ſeen through it, and the air ſeemed to be rather increaſed than diminiſhed. Water being admitted to it, left ſo little of it unabſorbed, that it could not be examined. Part of the blackiſh matter was waſhed off by the water.

I have ſince given more attention to this curious circumſtance, and have been happy enough to aſcer-

tain ſeveral things of conſiderable importance relating to it, though they are not ſufficient for a complete theory of it.

My firſt wiſh was to collect a quantity of this matter, enough for a chemical examination of it; and for this purpoſe I applied to my friend Mr. Vaughan, who favoured me with his aſſiſtance, and the uſe of his large machine, made by Mr. Nairne, on the ſame plan with what which was made for the Grand Duke of Tuſcany, deſcribed in the Philoſophical Tranſactions. But though we gave a good deal of time to this buſineſs, we did not get a quantity ſufficient for my purpoſe.

I ſaw reaſon, however, to conclude, that the whole of the vitriolic acid air is convertible into this black matter, but not by means of any union which it forms with the electric fluid; but as it ſhould ſeem, in conſequence of the *concuſſion* given to it by the exploſion; and that, if it be the calx of the metal which ſupplied the phlogiſton, it is not to be diſtinguiſhed from what metal, or indeed from what ſubſtance, of any other kind, the air had been extracted. Theſe particulars will be ſufficiently evinced in the following experiments.

I made 120 exploſions of a common jar in about a quarter of an ounce meaſure of vitriolic acid air from

from copper; by which I found that the bulk of it was diminiſhed about one third, and the remainder was ſeemingly not changed, being all abſorbed by water. In the courſe of this proceſs the air was carefully transferred three times from one veſſel to another; and the laſt veſſel in which the exploſions were made in it was, to all appearance, as black as the firſt; ſo that this air ſeems to be all convertible into this black matter, whatever it be.

Thinking this diminution of the vitriolic acid air might poſſibly ariſe from its abſorption by the cement, with which the glaſs tubes employed in the laſt experiment were cloſed, I repeated it, with the variation only of getting the air from quickſilver, in a *glaſs ſyphon*, confined by quickſilver; and obſerved that it was diminiſhed, by the ſame number of exploſions, in about the ſame proportion as before.

That this matter comes from the vitriolic acid air and not from any combination of the electric matter with it, will, I think, appear from the following experiment. I took the ſimple electric ſpark from a prime conductor of a moderate ſize, for the ſpace of five minutes, without interruption, in a quantity of vitriolic acid air, without producing any change in the inſide of the glaſs; when, immediately after, making in it only two exploſions of a common jar, each of which might be produced in leſs than a quarter of a minute; with the ſame ma-

chine, in the ſame ſtate, the whole of the inſide of the tube was completely covered with a black matter. Now, had the electric matter formed any union with the air, and this black matter had been the reſult of that combination, all the difference that could have ariſen from the ſimple *ſpark*, or the *exploſion*, could only have been a more gradual, or a more *ſudden* formation of that matter.

Had I uſed tubes ſmall enough, or a prime conductor large enough, I ſhould, no doubt, have produced this blackneſs with the ſparks only. On the other hand, when I had filled a large phial, about an inch and a half wide, with this air, I found that even the exploſion of a very large jar, containing more than two feet of coated ſurface, had no effect upon it; from which it ſhould ſeem that, in theſe caſes, the force of the ſhock was not able to give the whole quantity of air ſuch a concuſſion as was neceſſary to decompoſe any part of it.

At one time I had little doubt, but that the matter formed by theſe exploſions in vitriolic acid air would be different, according to the calx of the metal, or other ſubſtance which had ſupplied the phlogiſton of it; imagining that, together with phlogiſton, ſome of the earthy part of the ſubſtance had alſo entered into the compoſition of the air, and re-appeared in this decompoſition of it, in the form of a powder. But, upon the whole, I did not find that

that this conjecture was verified by the facts, which I shall here recite.

I had generally made use of *copper*; but when I got a quantity of this air from *quicksilver*, in the experiment mentioned above, and also when afterwards, I, for this very purpose, procured this air from almost all the kinds of substance from which it can be procured, the electric explosion taken in it produced the same effect, in clouding the tube with the black matter, as it had done in the air procured from copper. As some of these experiments were attended with peculiar circumstances, I shall briefly mention them.

I was most embarrassed when I endeavoured to get vitriolic acid air from *lead*, putting a quantity of leaden shot into a phial containing oil of vitriol, and applying only the usual degree of heat, a considerable quantity of air was produced; but afterwards, though the heat was increased till the acid boiled, no more air could be got. I imagine, therefore, that, in this case, the phlogiston had, in fact, been supplied by something that had adhered to the shot. However, in the air that was so produced, I took the electric explosion, and in the first quantity that I tried, a whitish matter was produced, almost covering the inside of the tube. But in the succeeding experiments with air procured from the same shot, or from something adhering to it, there

was leſs of the whitiſh matter; and at laſt nothing but black matter was produced, as in all the other experiments. Water being admitted to this air, there remained a conſiderable reſiduum, which was very ſlightly inflammable.

Vitriolic acid air is eaſily procured from *ſpirit of wine*, the mixture becoming black before any air is yielded. The electric exploſion taken in this air alſo produced the black matter,

If any of my experiments be capable of throwing light upon the ſubject of this black matter, it ſhould ſeem to be thoſe which I made with *ether*, by the help of which this air is as eaſily procured as by any other ſubſtance containing phlogiſton. In the air procured by ether the electric exploſion tinged the glaſs very black; I think more ſo than I had obſerved in any other experiment of the kind; and when water had abſorbed what it could of this air, there was a reſiduum in which a candle burned with a lambent blue flame. But what was moſt remarkable in this experiment was, that beſides the oil of vitriol becoming very black during the proceſs, a black ſubſtance, and of a thick conſiſtence, was formed, which ſwam on the ſurface of the acid. I collected a quantity of it, but I have not examined it with ſufficient attention. It may eaſily be procured by boiling ether in the vitriolic acid.

The electric ſpark, or exploſion, taken in *common air*, confined by quickſilver, in a glaſs tube, covers the inſide of the tube with a black matter, which, when heated, appears to be pure quickſilver. This, therefore, may be the caſe with the black matter into which I ſuppoſed the *vitriolic acid air* to be converted by the ſame proceſs, though the effect was much more remarkable in that than in common air. The *exploſion* will often produce the diminution of the common air, in half the time that the *ſimple ſparks* will do it, the machine giving the ſame quantity of fire in the ſame time. Alſo the blackneſs of the tube is much ſooner produced by the ſhocks than by the ſparks. When the tube conſiderably exceeds three tenths of an inch in diameter, it will ſometimes become very black without there being any ſenſible diminution of the quantity of air.

I have ſince found that this black matter is *mercury ſuperphlogiſticated*, the vapour of this metal, mixing with the air in all temperatures. This will be evident from the experiments under the article *mercury*.

SEC-

SECTION VI.

Of the Convertibility of vitriolic Acid Air into vitriolic Acid.

THE volatile vitriolic acid, though produced from the fixed vitriolic acid, is very considerably different from it, especially as it may be dislodged from its basis by the vitriolic acid, just as other weaker acids are dislodged by those that are thence called the stronger. But that volatile vitriolic acid is capable, however, of being brought back to the state of the common vitriolic acid, and becoming the same thing that it originally was, several experiments shew. I had found that it was capable of dissolving iron and zinc, and of producing inflammable air, which is the property of oil of vitriol; but I had a more decisive proof of the same thing when, to water saturated with vitriolic acid air, I had, for another purpose, put some earth of alum till it was saturated. For, after six months, in which this solution had been exposed in an open phial, and one third of it was evaporated, I observed many transparent crystals formed

ed at the bottom of the phial, as well as an incrustation on the sides of the phial above the surface of the liquor. These crystals were all triangular, of a considerable thickness, connected with each other, and when examined appeared to be *alum*, which is known to be the saline substance formed by the same earth, and the proper vitriolic acid. But the following experiments, in which it will appear that real sulphur is formed by means of the volatile vitriolic acid, exhibit a much more remarkable fact, and is another proof of the mutual convertibility of these acids into one another.

Having exposed various liquid substances to a continued heat in a sand furnace, among others I placed in it a glass tube, about an inch in diameter at the bottom, tapering to a point at the top, about two feet and a half long, closed hermetically; when I had put into it about an ounce measure of distilled water strongly impregnated with vitriolic acid air, with nothing more than a random expectation of some change or other taking place in it. This was on the 9th of September 1777, but the result was much more curious than I could possibly have imagined *a priori*. I shall note the appearances as I observed them, at the several intervals in which I examined this tube.

On the 30th of the same month this impregnated water, which continued transparent to the end

end of the proceſs, had depoſited a ſmall quantity of black powder; and alſo a bit of matter exactly like *ſulphur* about one eighth of an inch in diameter lay among it. Small pieces of the ſame matter floated on the ſurface of the liquor, and ſtreaks of the ſame coated part of the inſide of the tube an inch above the liquor. From the top of the tube to within about eight inches above the liquor, were beautiful white cryſtallizations, like *ſpicula*, diſpoſed irregularly, but generally in the form of ſtars, the glaſs being perfectly tranſparent between them.

In this ſtate the tube continued, the cryſtallizations increaſing, and ſeveral times changing their places, to the 20th of January following, when an end was put to the proceſs. Excepting, however, a place of a few ſquare inches near the ſurface of the liquor, all the lower half of the tube was quite free from them, but from thence to the top it was pretty thick and equally covered, exhibiting a moſt pleaſing appearance.

In order to obſerve the *time*, and the *manner* of the formation of theſe cryſtals, in a greater variety of circumſtances, I placed in the ſand furnace at different times, a ſtrong glaſs tube about nine inches long, and a quarter of an inch in diameter, which I ſunk pretty deep in the ſand, in order to give it a greater degree of heat; and alſo two tubes about

about four feet long, one of them half an inch, and the other a quarter of an inch in diameter, putting into the ſhort tube a quantity of the impregnated water about an inch in length, and into the long tubes two inches and a half.

The ſhort tube had been put into the ſand on the 11th of Auguſt, and on the 30th of September following the liquor was tranſparent, but the top and part of the middle of the tube had many white ſtars like cryſtallizations.

Of the long tubes the ſmaller had begun to have cryſtallizations, about one third from the bottom in about a fortnight, and the wider in about a month. When they were examined on the 19th of January, 1778, the large tube had more cryſtallizations than the ſmaller, the greateſt quantity of them about five inches above the ſurface of the liquor, but they were all on one ſide of the tube, and there were others about ſix inches above theſe. There were alſo very many between the ſurface of the liquor and two inches above it. The ſmaller tube had no cryſtals near the ſurface of the liquor, but a good many about five inches above it, and the greateſt quantity was about eighteen inches above it. Neither of theſe tubes had any cryſtals in two thirds of the upper part of them.

Applying the flame of a candle with a blowpipe to the ſmaller of the long tubes above-mentioned,

tioned, the glaſs was preſſed violently inwards; ſo that it was evident there was a decreaſe of elaſtic matter within the tube, which therefore probably entered into the cryſtals. If any part of the liquid touched the hot glaſs, a denſe white fume was excited, exactly like that from the oil of vitriol. Taking off one half of the tube, and then opening it under water, it was half filled with water, and the air within in it was completely phlogiſticated, which agrees with my former obſervations, of the vitriolic acid air imparting phlogiſton to common air, or rather of its imbibing dephlogiſticated air, and thereby forming vitriolic acid.

When I heated the dry cryſtals, the ſame white cloud was raiſed, and the cryſtals were by this means diſperſed into a kind of duſt, that incruſts the glaſs. For I applied the heat on the outſide of the tube.

The liquor itſelf was ſtill extremely acid, and the ſmell of it very pungent; ſo that, probably, only a ſmall part of the vitriolic acid air with which it was impregnated had entered into theſe cryſtals, numerous as they were.

The cryſtals were eaſily ſhaken off from the ſide of the tube, when it was waſhed with the liquor, and they continued undiſſolved in it.

The preceding obſervations were made preſently after the tubes in which the cryſtals were formed were

were taken from the ſand furnace; and in this ſtate they continued near a year, in the courſe of which I had ſhewed them to ſeveral of my chemical friends, who expreſſed much ſurprize at the ſight of them. At length I opened the tube that contained the greateſt quantity of theſe cryſtals, firſt obſerving that, when I ſoftened the glaſs, it was ſtill preſſed inwards.

The cryſtals, I found, were not diſſolved in ſpirit of ſalt, and when they had been waſhed, and dried, they had the colour, and ſmell of *ſulphur*; and being laid on a hot iron burned with a blue flame, ſo as to leave no doubt of the identity of the ſubſtances.

To form this ſulphur, I conjecture, that the volatile acid, in this expanded and confined ſtate, uniting with the dephlogiſticated air in the tube, had firſt formed vitriolic acid; and that this, uniting with phlogiſton, formed the ſulphur. The fact is certainly a remarkable one.

Having obſerved theſe curious effects of the impregnation of water with vitriolic acid air, I expoſed to the ſame heat, in ſimilar circumſtances, ſpirit of wine, and oil of turpentine, ſaturated with the ſame kind of air.

The impregnated ſpirit of wine, after being expoſed to this heat about a fortnight was tranſparent, but had many ſlender cryſtals in it, and the greater part of the tube had a thick and whitiſh incruſta-

tion, beginning about three inches above the ſurface of the liquor, and extending about twelve inches, but was thickeſt in the middle.

A ſhort tube, containing a quantity of the ſame impregnated ſpirit of wine, had no incruſtation, but many more cryſtals, in the form of *ſpicula* which ſettled to the bottom of the liquor. Another tube of the ſame length had ſimilar *ſpicula*, and near the top a conſiderable incruſtation not ſpiculine.

The oil of turpentine impregnated with vitriolic acid air, and expoſed to the heat in this manner, from being of a light amber colour, became of a deep brown. The tube in which it was contained was only eighteen inches long, and the upper half of it was covered on one ſide with white incruſtations not ſpiculine.

Whale oil impregnated with this air, from being brown, had probably become almoſt black. For the tube was broke, but had a very black incruſtation towards the bottom, eſpecially near the ſurface of the liquor.

I alſo expoſed to the ſame heat tubes containing vitriolic acid air only, having firſt filled them with quickſilver, then with this kind of air, and afterwards ſealing them hermetically with a blow-pipe; and the reſult was ſimilar to thoſe in which the impregnations were concerned.

One

One tube of this kind that had been buried in the hot ſand on the 11th of Auguſt, being examined on the 30th of September, was found in the following ſtate. The upper part of the tube was half covered with white cryſtals, but much ſmaller than thoſe in the tubes containing the water impregnated with this air.

Another tube containing the ſame kind of air, which had been buried in the ſand a longer time, was found quite covered with white cryſtals, and a ſmall part of the tube was black, probably from ſome external accidental cauſe. The end of this tube being broke under quickſilver, it filled one third of it, and water abſorbed all that remained of the air, except a very ſmall bubble. This water had the ſmell of water impregnated with vitriolic acid air.

I have ſeveral times repeated this experiment, and have never failed to find the inſide of the tubes that had been filled with vitriolic acid air coated with this white matter; but it is ſo exceedingly ſlight, that I cannot make many obſervations upon it. I am rather ſurprized to find that it does not ſeem to be *ſulphur*, which is formed from the heating of water impregnated with the ſame kind of air. For ſpirit of ſalt ſeems to diſſolve it all. At leaſt the tube is waſhed perfectly clean with it,

and I could not diſcern any thing in that acid afterwards. But this may be owing to the very ſmall quantity of it, though it be ſpread on ſo great a ſurface, and to the extreme minuteneſs of the particles of which it conſiſts.

PART

PART III.

OF FLUOR ACID AIR.

SECTION I.

The Discovery of the Fluor Acid Air, and the Impregnation of Water with it.

THE philosophical part of the world have, of late, been highly gratified by the discovery of *a new mineral acid*, contained in a substance which chemists distinguish by the name of *fluor*; but many of my readers will understand me better, when I inform them, that it is of that species of substance, which, with us, is called *the Derbyshire spar*; and of which, at present, vases and other ornaments for chimnies, are usually made. The acid is expelled from this substance by oil of vitriol, and has peculiar properties, as remarkable as any of the three other mineral acids which we were acquainted with before.

This curious discovery was made by Mr. Scheele, a Swede; from which circumstance the acid is often distinguished by the name of the *Swedish acid.* His method of operating upon this substance, and likewise that of all who have succeeded him in the inquiry, was to distil it in glass vessels, as in the process of making spirit of nitre from salt-petre; and the most remarkable facts that have been observed concerning it are, that the vessels in which the distillation is made are apt to be corroded; so that holes will be made quite through them; and that when there is water in the recipient, the surface of it will be covered with a crust, of a friable stony matter.

This crust, which I shall distinguish by the name of the *fluor crust*, Mr. Scheele supposed to be *quartz*; and therefore concluded that this acid and water were the constituent parts of that fossil. On the other hand, Mr. Boulanger, who has taken a great deal of pains with this subject, is of opinion that this new acid is only the *acid of salt*, combined with an earthy substance. For this opinion he advances various reasons; but does not pretend to be able to produce any decisive proof. The result of my own experiments, I think, prove, that the fluor contains a peculiar acid.

As soon as I had exhibited one of the acids in the form of *air*, I had no doubt but that all the acids

acids might be exhibited in the ſame manner, and this among the reſt; but I imagined that I ſhould find great difficulty in procuring the foſſil that contains it; ſuppoſing that it had only been found in Sweden; and I ſhould probably have continued in this incapacity for making the following experiments, had I not been relieved by Mr. Woulfe, who, upon my inquiry concerning it, not only explained to me what the ſubſtance was, but immediately furniſhed me with a quantity of ſeveral kinds of it, ſufficient for my purpoſe. That with which my firſt experiments were made, was that which he called *the white phoſphoric ſpar, from Saxony*; but afterwards I made uſe of the Derbyſhire ſpar; and the pieces that I had by me were partly white, or yellowiſh, and partly purple.

All my advantage in the inveſtigation of this ſubject, has ariſen from my peculiar manner of conducting the experiments. For, by exhibiting the acid in the form of air, free from moiſture, I had an opportunity of examining its nature and affinities with the greateſt eaſe and certainty. In this manner alſo, this ſpecies of air exhibits a variety of ſtriking phenomena, which cannot be produced in any other manner of operating upon it.

When I began theſe experiments, I followed the directions given by thoſe who had gone before me in the inveſtigation of this ſubject, and who

had procured the acid in the common method of diſtillation, pounding the fluor (which I afterwards found not to be neceſſary) and pouring oil of vitriol upon it. This I did in a phial, to which was fitted a ground-ſtopper and tube, and immediately found, that, at firſt, without any heat, and afterwards with a very ſmall degree of it, air was produced in great plenty, perfectly tranſparent, and confined by quick-ſilver, like the other acid airs. The vapour, as it iſſued out of the tube into the open air, formed a permanent white cloud; no doubt, by attaching to itſelf the water that floated in the atmoſphere, and the ſmell of it was extremely pungent.

I had no ſooner produced this new kind of air, but I was eager to ſee the effect it would have on *water*, and to produce the ſtony cruſt formed by their union, as deſcribed by Mr. Scheele; and I was not diſappointed in my expectations. The moment the water came into contact with this air, the ſurface of it became white and opake, by a *ſtony film*, which, forming a ſeparation between the air above, and the water below it, conſiderably retarded the aſcent of the water, till the air, inſinuating itſelf through the pores and cracks of this cruſt, the water neceſſarily roſe as the air diminiſhed, and breaking the cruſt, preſented a new ſurface of water, which, like the former, was inſtantly covered with a freſh cruſt. Thus was one ſtony incruſtation

tion formed after another, till every particle of the air was united to the water, and the different films being collected, and dried, formed a white powdery ſubſtance, generally a little acid to the taſte; but when waſhed in much pure water, was perfectly inſipid.

Few philoſophical experiments exhibit a more pleaſing appearance than this, which can only be made, by firſt producing the air confined by quick-ſilver, and then admitting a large body of water to it. Moſt perſons to whom I have ſhewn the experiment have been exceedingly ſtruck with it. It is exhibited to the moſt advantage, when the veſſel that contains the air is pretty wide, by which I mean about an inch in diameter. In this caſe the cruſt will often crack in the middle, and a ſmall jet of water ruſhing through the fiſſure, will, to appearance, be inſtantly converted into this ſtony ſubſtance, and look like a puff of white powder, riſing ſometimes an inch or two up into the air. Alſo the *cryſtallizations*, formed on the ſides of the veſſel, as the water riſes in it, make a very beautiful appearance.

The union of this acid air and water may alſo be exhibited in another manner, which, to ſome perſons, makes a ſtill more ſtriking experiment; viz. by admitting the air, as faſt as it is generated,

to a large body of water reſting on quickſilver, inſtead of introducing the water to the air previouſly formed.

For this purpoſe, I uſually put two or three ounce meaſures of water into a tall cylindrical jar, about an inch in diameter (ſuch as thoſe which I generally uſe as recipients of thoſe kinds of air that muſt be confined by quickſilver) and filling the remainder of the veſſel with quickſilver, I place it inverted in a baſon containing a quantity of the ſame fluid; ſo that the water, immediately riſing to the top, occupies the upper part of the veſſel, while the quickſilver occupies the lower part. I then introduce under it the end of the tube proceeding from the phial, which contains the materials for generating this air. It is, then, very pleaſing to obſerve, that the moment any bubble of air, after paſſing through the quickſilver, reaches the water, it is inſtantly, as it were, converted into a ſtone; but continuing hollow for a ſhort ſpace of time, generally riſes to the top of the water, in the form of a bubble, or a thin white film. If the ſucceſſion of bubbles be rapid, and they riſe freely to the top of the veſſel, through a large body of clear water (which, however, is not always the caſe, as they will ſometimes adhere to the upper ſurface of the quickſilver) I have met with few

perſons

perſons who are ſoon weary of looking at it; and ſome could ſit by it almoſt a whole hour, and be agreeably amuſed all the time.

Every bubble of air, coming into contact with the water on every ſide at once, is like a bladder, being hollow within; but this ſlight cruſt ſoon burſting, the ſides collapſe, and it riſes to the top of the veſſel, in the form of a piece of thin white gauze. The water ſoon penetrating every part of it, the whole maſs of theſe films becomes in a little time like a jelly, which continually thickens by the acceſſion of more films, till at length the whole body of water ſeems to become ſolid; ſo that, being fully ſaturated, eſpecially at the lower part, the air, finding no more moiſture within its reach, will fill all the lower part of the veſſel, expelling the quickſilver, while the water, in the form of a ſtiff jelly, occupies all the upper part of the veſſel.

As, for the purpoſes to be mentioned here-after, I have repeated this experiment a great number of times, I have had an opportunity of obſerving a very great variety in the appearances which it exhibits. One is peculiarly pleaſing, but not very common. A large bubble of air will ſometimes adhere, by its lower part, to the ſurface of the quickſilver; and another bubble, riſing in the ſame place, before the lower part of the former has been cloſed, puſhes out the upper part of it, and advancing

vancing farther into the water, extends the bubble in length: another follows, and does the ſame, till at length a *tube* is formed (the ſides alſo growing thicker continually) extending from the quickſilver, to the top of the water. I have ſeen of them four inches in length; and others being formed cloſe to them, the whole veſſel has been almoſt filled with theſe tubes, adhering to one another, of different lengths, and not much unlike the appearance of the pipes that are placed in the front of an organ.

In leſs than an hour, I have frequently converted two or three ounce meaſures of water into this ſolid maſs. When this is taken out of the veſſel, and preſſed, it will be found to contain a great deal of an acid liquor; the water impregnated with the acid having been intangled in the interſtices of the jelly, out of the reach of the air: and if this liquor be uſed in another proceſs, inſtead of pure water, more of it will ſeem to become ſolid, and the acid liquor will be concentrated every time.

By the repetition of this proceſs, an acid liquor may be procured of a very conſiderable degree of ſtrength. There ſeems, however, to be a limit to its ſtrength; for the acid is exceedingly volatile, as is evident from its extremely pungent ſmell; ſo that I have thought that I gained nothing by repeating the proceſs more than eight or ten times; becauſe it was impoſſible to transfer the water from

one

veſſel to another, but more acid would be loſt evaporation, than would be acquired by another ɔregnation with the acid air.

Theſe appearances I now explain, by ſuppoſing : the acid of the ſpar diſlodged by the vitriolic l, in uniting with the ſpar, is in part volatilized, ıears in the form of air; and that there is alſo ıbined with this air, a portion of the ſolid earthy t of the ſpar, which continues in a ſtate of ſolu- ı, till, coming into contact with the water, the d unites with the acid, and the earth is precipi- :d.

Before I proceed to relate any of the experiments ıch I made with this acid air, I ſhall give a few :ctions and precautions, which may be uſeful to ſons on their firſt entrance upon this courſe.

ı. The tube through which this acid vapour is veyed ſhould not be very narrow, becauſe it is to be furred up, eſpecially when any phial, con- ıing materials for the production of this air, has n uſed ſome time, and with a good deal of heat; ıng, I ſuppoſe, to the hot air retaining in ſolu- ı more of the ſtony matter than it can do when s cold, and therefore depoſiting it as it is con- ·ed through the tube.

2. I began theſe experiments with phials which l ground ſtoppers and tubes, but ſoon found that ras too expenſive a mode of experimenting with

this kind of air; for they were preſently corroded and ſpoiled. Afterwards, therefore, I uſed only common phials, but the thickeſt that I could meet with; and ſtill ſeldom found that they would bear the experiment above an hour. Very frequently, the thickeſt phials that I could get would be worn quite through in a quarter of an hour, when the heat was conſiderable, and the production of the air rapid. This power of diſſolving glaſs is a very remarkable property of this air; but it ſeems to poſſeſs it only when it is hot, at leaſt in any conſiderable degree.

3. When I wiſhed to produce this air pretty faſt, I found it moſt convenient to pound the ſpar, and pour the oil of vitriol upon it, filling one fourth of the phial with the ſpar, and leaving one fourth of it for a ſpace in which the bubbles might expand themſelves, and break, ſo as not to carry any of the liquor into the tube. I now proceed to the particular experiments.

SEC-

SECTION II.

Experiments made with a View to discover the Constitution of Fluor Acid Air.

I Thought it might possibly contribute to decide the question concerning the identity of this acid and the marine, if I put a quantity of the *fluor crust* to marine acid air; thinking that they might form an union, and constitute this fluor acid air. And, indeed, something similar to it was by this means produced; so that another crust was formed upon the admission of water to it; but, in other respects, several circumstances, which I cannot explain, attended the experiments. They were as follows.

To about two ounce measures of marine acid air I put about a quarter of a grain of the fluor crust, and in about three days it had absorbed about half an ounce measure of the air. Water being then admitted to it, left a quarter of an ounce measure of air unabsorbed.

Afterwards I conveyed marine acid air to a pretty large quantity of the fluor crust, confined by quick-

ſilver; and, as the air was imbibed, I continued to throw up more, till, after three or four days, that ſubſtance ſeemed to be fully ſaturated with the air. Then admitting water to it, it was abſorbed exactly like the fluor acid air; but I could not, at that time, very well diſtinguiſh the cruſt on the top of it, on account of the jar being almoſt filled with the cruſt, and part of it floating on the top of the water. About three fourths of this air was abſorbed by the water; but what I thought very remarkable, air kept iſſuing from this fluor cruſt, in large bubbles, till the quantity of air was doubled, and the jar was half filled with it. This air neither affected common air, nor was affected by nitrous air, and it extinguiſhed a candle.

I repeated the experiment, with this only difference, that I admitted water to the air as ſoon as the fluor cruſt ſeemed to be ſaturated; when the experiment being made in a wide jar, the cruſt on the ſurface of the water was as viſible as in the experiment with the fluor acid air itſelf. At this time, however, there was no generation of air from the ſaturated cruſt, as before, but a conſiderable quantity of air, unabſorbed by water, though I took care that the marine acid air was as pure as I could procure it.

Having a quantity of the fluor cruſt ſaturated with marine acid air, I had the curioſity to pour ſome

ſome oil of vitriol upon it, in order to try whether the produce would be pure marine acid air, or a mixture of the two; and the latter ſeemed to be the caſe, though I think the marine acid prevailed in the mixture.

In this proceſs air was produced in great plenty, and the bubbles burſt in the receiver with a white cloud; but when water was admitted to it, it was abſorbed without any cruſt being formed upon its ſurface.

In twenty four hours a piece of ſalt-petre turned yellow in this air, and abſorbed about half an ounce meaſure of it. What remained unabſorbed by water, was exceedingly ſtrong nitrous air, the ſpirit of nitre having been ſet looſe from the ſalt-petre by the marine acid air, and having diſſolved the quickſilver.

A piece of *borax*, in about a fortnight, abſorbed about two ounce meaſures of this air, without leaving any reſiduum not abſorbed by water. The ſurface of the borax was become ſoft; but by waſhing it in water, the ſoft part was eaſily ſeparated from the reſt.

At the beginning of my inveſtigation of this ſubject, I had a ſuſpicion that this new acid air might poſſibly be the *vitriolic acid air*, loaded with the ſparry cruſt; but the following experiments were not favourable to this hypotheſis.

I threw

I threw the focus of a burning lens upon ſome pieces of the ſpar in vitriolic acid air, confined by quickſilver; thinking, that when it was hot, it might diſſolve ſome part of it, and thereby become the ſame thing with the fluor acid air. But though I continued this operation till the ſpar ſmoked, and filled the veſſel with a white fume, there was neither any addition made to the quantity of air, nor any change produced in the quality of it. When water was admitted to it, no cruſt, as I had expected, was formed on the ſurface of it.

In order to try whether the fluor cruſt was the ſame thing with the ſpar, from which it had been produced, I got a quantity of it, and treated it in the ſame manner as I had treated the ſpar, pouring oil of vitriol upon it, and endeavouring to expel air from it. I preſently found, indeed, that it yielded great plenty of air; but not finding it to be the thing I was then in queſt of, viz. an acid air, by means of which a cruſt would be formed on the ſurface of the water admitted to it, I neglected to give ſufficient attention to it.

Water admitted to this acid air, procured from the fluor cruſt by oil of vitriol, abſorbed it all, but without having any cruſt upon its ſurface.

Alkaline air united with the whole of this acid air, forming with it a white ſaline ſubſtance; and part of the inſide of the tube in which the mixture

was

was made, was tinged with a deep yellow, or orange colour, which disappeared after a few hours exposure to the open air. This I have observed to be the case with the vitriolic acid air.

This air did not at all affect *salt-petre* or *borax*.

The acid air expelled from water saturated with the fluor acid, resembles the vitriolic acid air in so many properties, that when I first published my experiments on the subject, I concluded it to be the vitriolic acid air combined with the fluor crust.

To satisfy myself with respect to it, I saturated a quantity of water with the fluor acid air, pressing out the stony matter with which it was filled at each process, and impregnating it over and over again. When it appeared to be sufficiently impregnated for my purpose, I put the liquor into a phial, furnished with a proper tube and recipient, such as is represented Plate II. fig. 8. to receive any of the watery part that might be expelled by heat; and applying the flame of a candle, I presently got from it great plenty of air; which, by every test that I could think of applying, appeared to have the very same properties with the vitriolic acid air.

The air thus expelled from this acid liquor was absorbed by water, without any crust on the surface of it.

When alkaline air was admitted to it, the ſides of the veſſel were tinged with the orange-colour mentioned above, which vaniſhed in about an hour after it had been expoſed to the open air.

This air had no effect upon *ſalt-petre*, a piece of which continued in it about a fortnight; nor yet upon *ſulphur*, *alum*, or *ſal ammoniac*.

Liver of Sulphur abſorbed it, without undergoing any ſenſible change.

This air extinguiſhed a candle, without any particular colour of the flame.

Camphor was diſſolved in this air, exactly as it is in vitriolic acid air.

In theſe properties this acid air will be found, by compariſon, to agree with the vitriolic acid air; as alſo in the two following, which, as far as I know, are peculiar to this ſpecies of air.

Phlogiſton, as I have obſerved, is contained in vitriolic acid air, and in ſuch a manner as to be communicated by it to the common air with which it is mixed, and thereby to phlogiſticate, or injure it. And an equal quantity of this acid air, and common air, having been mixed, and left together twenty-four hours, the common air appeared to be ſo far injured, that two meaſures of it, and one of nitrous air, occupied the ſpace of ſomething more than two meaſures.

The *electric ſpark* has a very remarkable effect upon the vitriolic acid air, or rather upon the glaſs

tube in which the experiment is made, as has been obſerved; for a ſingle exploſion covers all the inſide ſurface with a deep brown or black matter, and the glaſs grows more opake every ſtroke. This very ſingular and ſtriking effect has the electric ſpark taken in the air expelled from this acid liquor.

After I had made this experiment, I had no doubt, but that theſe two kinds of air, viz. the *vitriolic acid* and the *fluor acid*, are, in reality, the ſame. And the difference between them when the latter is diveſted of the ſtony matter which it contains, cannot be great. But ſtill the acid thus far ſeparated from the ſtony matter contains a ſufficient quantity of it; for in making it boil with violence, the inſide of the tube immediately connected with it, was filled with a ſtony matter. It happened twice in the courſe of the above mentioned experiments, that the tube was quite ſtopped up by this means, ſo as to cauſe the exploſion of the phial.

Laſtly, I would obſerve, that the *taſte* of this acid liquor afforded a preſumption, that the acid which enters into it was the vitriolic; for it has exactly the aſtringency of alum. Notwithſtanding all theſe circumſtances, the acid is unqueſtionably of a different nature.

That the fluor contains phlogiſton, is evident, from the attempts that I made to procure dephlogiſticated air from it, by means of ſpirit of nitre; for

the air that I got from it was always phlogiſticated, and ſometimes even nitrous.

At firſt I made this experiment by putting the materials into a phial with a ground-ſtopper and tube, and applying the heat of a candle only. The air I got in this manner neither affected common air, nor was affected by nitrous air. I then put the ſame apparatus into a crucible; and, with a ſtrong ſand-heat, I got from it about two ounce-meaſures of air; in four portions. The firſt of theſe was exactly like the preceding, being phlogiſticated air; the ſecond made lime-water turbid, and a great part of it was readily abſorbed by water: the third and fourth portions were very ſtrong nitrous air.

This experiment was made with the whitiſh part of this ſpar, which therefore probably contains the leaſt phlogiſton. That phlogiſton which contributes to the *colour* of this foſſil, I found, by the following obſervation, to be of a very volatile nature. When the coloured ſpar is diſſolved in oil of vitriol, the fluor cruſt, collected in the water, has the ſame colour; but when it is dried near the fire, the colour vaniſhes, and the whole becomes white: yet this white cruſt, heated again in oil of vitriol, contains, as was obſerved before, ſo much phlogiſton, as to convert oil of vitriol into vitriolic acid air.

The air expelled from this acid liquor did not diſſolve the *fluor cruſt* that was expoſed to it. A quantity

quantity of it remained in this ſituation ſeveral days, without affecting it, or being affected by it. I had imagined that it might have been diſſolved by this air, and have converted it into the fluor acid air.

Oil of turpentine abſorbed about ten times its bulk of this air, and became of an orange colour. After this impregnation it had a pungent acid ſmell, together with its own. I obſerved nothing farther reſpecting it.

Reflecting upon the phoſphoric property of the ſpar, by means of which I had procured this acid air, I thought it was poſſible that its property of enabling oil of vitriol to yield this air, might be common to it with other ſimilar phoſphoric ſubſtances, depending upon that combination of phlogiſton which enables them to imbibe and emit light.

In order to aſcertain this, with reſpect to one other ſubſtance of this kind, I made a quantity of Mr. *Canton's phoſphorus*, and pouring upon it ſome oil of vitriol, I got air that was readily abſorbed by water, and with a cruſt upon its ſurface, exactly like that which is procured from the fluor, only not in ſo great a quantity. The effervescence between this ſubſtance and the oil of vitriol was very great, and alſo the heat occaſioned by it; and the vapour eſcaping into the common air, was white and denſe, much like the vapour of the fluor acid.

I ſhall conclude this ſection with obſerving, that the oil of vitriol in which the fluor is diſſolved,

becomes thick like ice, exactly like the oil of vitriol in which quick-lime has been boiled.

As the fluor contains an acid *ſui generis*, it might have been expected, that it would have been diſlodged from its baſe by ſome other of the acids, as well as the vitriolic; or that, if it was of a ſtronger kind than the nitrous, or marine acids, it might diſlodge them from their baſes: but no experiments whatever ſhow any ſuch thing; nor can the fluor acid be at all produced, except when the ſpar is diſſolved in the vitriolic acid. This has, in ſome meaſure, been obſerved by others. Having carefully repeated the experiments myſelf, I obſerved the following reſults.

Having put a quantity of fluor to ſtrong *nitrous acid*, in a glaſs phial with a ground ſtopper, I received the produce in water, and obſerved that about one ſixth of it was fixed air, precipitating lime in lime-water, and that the remainder was ſtrongly nitrous (a proof that this ſubſtance contains much phlogiſton) but there was no appearance of any thing like a *cruſt* on the ſurface of the water. When the air was received in quickſilver, the ſolution of the metal, and the production of more nitrous air, demonſtrated that the nitrous acid, uncombined with any thing that could alter the property of it, came over.

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SECTION III.

Obſervations on the Freezing of Water, impregnated with Fluor Acid Air, and with Vitriolic Acid Air.

I Had an opportunity, in one froſty winter, of obſerving a pretty remarkable difference between *water* impregnated with vitriolic acid air, and water impregnated with the fluor acid. It is that water impregnated with vitriolic acid air that may be converted into *ice*, whereas water impregnated with fluor acid air will not freeze.

I had obſerved with reſpect to marine acid air, and alkaline air, that they diſſolve ice, and that water impregnated with them is incapable of freezing, at leaſt in ſuch a degree of cold as I had expoſed them to. The ſame, I find, is the caſe with fluor acid air, but it is not ſo at all with vitriolic acid air, which, intirely contrary to my expectation, I find to be altogether different from marine acid air in this reſpect, and to reſemble fixed air. But whereas water impregnated with fixed air diſcharges it when it is converted into *ice*, water impregnated with vitriolic acid air, and then frozen, retains it as ſtrongly as ever.

I expoſed in an open phial a quantity of water fully impregnated with vitriolic acid air, when the thermometer was at 17 degrees, and obſerved that it was preſently frozen quite through, the ſmell of it continuing to be extremely pungent. As it melted, the ice ſunk to the bottom of the liquor, and when it was quite diſſolved, the water appeared to be ſtill very ſtrongly impregnated.

Then, in order to try whether water would freeze in a ſituation in which none of this acid air could poſſibly eſcape from it, I expoſed to the ſame degree of cold a quantity of water ſaturated with this air, remaining upon the quickſilver, in the jar in which the impregnation had been made; but this water alſo was preſently converted into ice, which was opaque, though the bubbles were not diſtinctly perceivable.

I then poured a quantity of *water* upon a quantity of the ice above mentioned, when it froze, without being ſeemingly affected by any effluvium of the impregnated ice. This continued opaque, while that of the water was tranſparent as uſual. Both theſe kinds of ice melted equally, and adhered together.

Ice was not at all diſſolved in vitriolic acid air. Letting it remain in this ſituation all night, the next morning I found all the air abſorbed; but it appeared, by the form of the ice, that it had been melted, and

and frozen again; for it exactly fitted the glass vessel in which it was confined.

Ice was readily dissolved at first in fluor acid air, but afterwards very slowly, as might have been concluded from the manner in which this kind of air is imbibed by water. However, in time, the whole of the air was absorbed by the ice, which was dissolved by it, and continued fluid on the surface of the quicksilver.

In order to make my experiment upon water impregnated with fluor acid air, I saturated a quantity of water with it so fully, that I could with difficulty squeeze any water from a quantity of fluor crust that was deposited in it. This water I exposed to the frost all night, and in the morning found it to be intirely fluid. Presently after this the frost broke, and I had no opportunity of repeating the experiment.

The farther prosecution of this experiment, and a proper attention to it, will probably throw great light on the nature of freezing.

The frost of a subsequent winter confirmed the above mentioned observations, except those which respected water impregnated with fluor acid air, which I before concluded would not freeze.

I now find that it does freeze, though it requires a greater degree of cold than water impregnated with vitriolic

vitriolic acid air. The latter effect I attributed to the presence of some of the fluor crust in the solution, and I think this conjecture is, in some measure, confirmed by the following observations; in which it will be seen, that lime water did not freeze so soon as common water, and that lime water impregnated with vitriolic acid air did not freeze so soon as common water so impregnated.

Jan. 7, 1779. I exposed to the cold all night a phial of pump water, and one of the same water saturated with quick lime. The next morning I found the thermometer at 28, the pump water frozen solid, but the lime water not frozen at all.

Jan. 9. When the thermometer was at least 23 water impregnated with fluor acid air, after being exposed to the cold all night, was imperfectly frozen. At the same time water impregnated with vitriolic acid air was quite solid, and also a quantity of the same in which some chalk had been dissolved. But lime impregnated with vitriolic acid air was quite fluid. Lime water was frozen, and a little of the lime was precipitated.

Jan. 12. When the thermometer was at 20, and had probably been lower in the night, I found the lime water impregnated with vitriolic acid air, and also the water impregnated with fluor acid air, solid

ſolid throughout. The former was quite white, but was tranſparent again when the ice melted. As the ice of the fluor acid melted, it ſwam on the ſurface of the liquid part.

SECTION IV.

Miſcellaneous Experiments on the Properties of Fluor Acid Air.

DIPPING a lighted candle into a veſſel filled with the fluor acid air, it was extinguiſhed without any particular colour of the flame, which is obſervable in the marine acid air.

Nitrous air, mixed with this acid air, had no ſenſible effect upon it. Water abſorbed the acid air, and left the nitrous air poſſeſſed of its peculiar properties.

Having

Having aſcertained the effect of water upon this acid air, I proceeded to try other *fluid ſubſtances.*

Spirit of wine imbibed this air as readily as water, but continued as limpid as ever; and when ſaturated with it, ſeemed to be no leſs inflammable than before.

Oil of turpentine did not imbibe any of this air.

Vitriolic ether imbibed about twenty times its own bulk of it; but was not ſenſibly changed by the impregnation. The caſe was the ſame with *nitrous ether.* But the firſt time that I made the experiment with nitrous ether, I imagine a little water was mixed with it (as much as thoſe ſubſtances are capable of being mixed) for it coagulated as water had done, remaining in the middle of the tube, the acid air being both above and below it. This maſs of coagulated matter, which in colour and conſiſtence reſembled a brown jelly, being taken out of the veſſel, did not take fire at the approach of a candle; but when it had been expoſed to the air about half a minute, it grew hot, threw out a groſs ſmoke, and was preſently all evaporated. Part, however, of the ſame maſs, which had been dipped in water, did not grow hot, or evaporate, in the open air; and when expoſed to the fire, it burned to a white powdery ſubſtance. I imagine this effect to have been owing to a mixture of water; becauſe, with

pure nitrous ether, I could not get another appearance of the kind.

Of *solid substances*, I found that this air had no effect upon *sulphur*, *common salt*, *sal ammoniac*, *iron*, *liver of sulphur*, or *gum-lac*.

Charcoal absorbed the whole of a quantity of this air, and contracted from it a strong pungent smell. The *rust of iron* also absorbed it in like manner.

Alum absorbed this air pretty fast, the surface of it being rendered white and opake. When it was taken out of the air, it looked moist, and was incapable of the operation of roasting, like that which had been exposed to alkaline air. This air having, no doubt, like the other, seized upon the water which enters into the composition of alum.

Quicklime and *chalk*, both absorbed a little of this acid air; but the result was, in no respect, remarkable. The latter had been dissolved by it, and had produced a quantity of fixed air, precipitating lime in lime-water.

In order to judge whether there was any foundation for the opinion of Mr. Boulanger, of this acid being the same with the *marine*, I put to it a piece of salt-petre, which I have observed to be readily dissolved in the marine acid air; and I must own that appearances so much favoured his opinion, that I was at that time very much inclined to adopt it.

When

When the ſalt-petre had been for ſome time ſurrounded with this air, the air began to be diminiſhed, and the inſide of the veſſel was filled with red fumes, which continued about a week, the quick-ſilver riſing all the time, till only one tenth of the air remained, and the inſide of the veſſel was covered with a whitiſh, probably a ſaline, ſubſtance, produced by the ſolution of mercury. After this, the air becoming tranſparent, I examined it, and found it neither to affect common air, nor to be affected by nitrous air, and to extinguiſh a candle. Alſo, about one fourth of it was readily abſorbed by water, and made lime water turbid; ſo that, contrary to my expectation, a great part of the air muſt have been fixed air, and not nitrous. This experiment I did not repeat; but it ſeems to exhibit a fact deſerving of ſome attention.

Fluor acid air, when it is firſt produced, corrodes the glaſs veſſel in which it is generated. But whether it did this of itſelf, merely in conſequence of being heated, or whether the moiſture, or ſomething elſe contained in the oil of vitriol, by means of which it is formed, contributed to this effect, did not certainly appear. When this air is cold, it does not at all affect the glaſs veſſel in which it is confined. In my attempts to confine the different kinds of air in glaſs tubes hermetically ſealed, in

in order to expose them to a continued heat, I observed that it is simply the *heated air* that has this effect. For when I had filled a tube with this kind of air, and was endeavouring to take off different lengths of it with a blow pipe, I found that, when the glass became red hot, it was always so corroded, and dissolved, that it was impossible to close it by sealing.

Having mentioned the effect of heating various substances in different kinds of air by means of a burning lens, that I may not omit to mention any thing that I have observed of the kind, and that I can think any person might wish to be informed of, I shall, in this miscellaneous section, recite the following experiments.

Throwing the focus of the lens upon iron turnings in fluor acid air, a dense white fume presently filled the vessel, and during the process the heat was remarkably great, so that I could not touch the upper part of the glass vessel in which the experiment was made. The quantity of air was considerably diminished, and a quantity remained that was not absorbed by water, but not enough to examine it.

PART

PART IV.

EXPERIMENTS AND OBSERVATIONS RELATING TO ALKALINE AIR.

SECTION I.

The Discovery of Alkaline Air, and of the Impregnation of Water with it.

AFTER I had made the discovery of the *marine acid air*, which the vapour of spirit of salt may properly enough be called, and had made those experiments upon it, of which I have already given an account, it occurred to me, that, by a process similar to that by which this *acid* air is expelled from the spirit of salt, an *alkaline* air might be expelled from substances containing volatile alkali.

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Accordingly I procured ſome volatile ſpirit of ſal ammoniac, and having put it into a thin phial, and heated it with the flame of a candle, I preſently found that a great quantity of vapour was diſcharged from it; and being received in a veſſel of quickſilver, ſtanding in a baſon of quickſilver, it continued in the form of a tranſparent and permanent air, not at all condenſed by cold; ſo that I had the ſame opportunity of making experiments upon it, as I had before on the acid air, being in the ſame favourable circumſtances.

With the ſame eaſe I alſo procured this air from *ſpirit of hartſhorn*, and *ſal volatile* either in a fluid or ſolid form, *i. e.* from thoſe volatile alkaline ſalts which are produced by the diſtillation of ſal ammoniac with fixed alkalis. But in this caſe I ſoon found that the alkaline air I procured was not pure; for the fixed air, which entered into the compoſition of my materials, was expelled along with it. Alſo, uniting again with the alkaline air, in the glaſs tube through which they were conveyed, they ſtopped it up, and were often the means of burſting my veſſels.

While theſe experiments were new to me, I imagined that I was able to procure this air with peculiar advantage and in the greateſt abundance, either from the ſalts in a dry ſtate, when they were juſt covered with water, or in a perfectly fluid

ſtate; for, upon applying a candle to the phials in which they were contained, there was a moſt aſtoniſhing production of air; but having examined it, I found it to be chiefly fixed air, eſpecially after the firſt or ſecond produce from the ſame materials; and removing my apparatus to a trough of water, and uſing the water inſtead of quickſilver, I found that it was not preſently abſorbed by it.

This, however, appears to be an eaſy and elegant method of procuring fixed air, from a ſmall quantity of materials, though there muſt be a mixture of alkaline air along with it; as it is by means of its combination with this principle only, that it is poſſible, that ſo much fixed air ſhould be retained in any liquid. Water, at leaſt, we know, cannot be made to contain much more than its own bulk of fixed air.

After this diſappointment, I confined myſelf to the uſe of that volatile ſpirit of ſal ammoniac which is procured by a diſtillation with ſlaked lime, which contains no fixed air; and which ſeems, in a general ſtate, to contain about as much alkaline air, as an equal quantity of ſpirit of ſalt contains of the marine acid air.

Wanting, however, to procure this air in greater quantities, and this method being rather expenſive, it occurred to me, that alkaline air might, probably,

be procured, with the moſt eaſe and convenience, from the original materials, mixed in the ſame proportions that chemiſts had found by experience to anſwer the beſt for the production of the volatile ſpirit of ſal ammoniac. Accordingly I mixed one fourth of pounded ſal ammoniac, with three fourths of ſlaked lime; and filling a phial with the mixture, I preſently found it completely anſwered my purpoſe. The heat of a candle expelled from this mixture a prodigious quantity of alkaline air; and the ſame materials (as much as filled an ounce phial) would ſerve me a conſiderable time, without changing; eſpecially when, inſtead of a glaſs phial, I made uſe of a ſmall iron tube, which I find much more convenient for the purpoſe.

As water ſoon begins to riſe in this proceſs, it is neceſſary, if the air is intended to be conveyed perfectly *dry* into the veſſel of quickſilver, to have a ſmall veſſel in which this water (which is the common volatile ſpirit of ſal ammoniac) may be received. This ſmall veſſel muſt be interpoſed between the veſſel which contains the materials for the generation of the air, and that in which it is to be received, as *d*, Pl. II. fig. 8.

This *alkaline* air being perfectly analogous to the *acid* air, I was naturally led to inveſtigate the properties of it in the ſame manner, and nearly in the ſame order. From this analogy I concluded,

as I presently found to be the fact, that this alkaline air would be readily imbibed by water, and, by its union with it, would form a volatile spirit of sal ammoniac. And as the water, when admitted to the air in this manner, confined by quicksilver, has an opportunity of fully saturating itself with the alkaline vapour, it is made prodigiously stronger than any volatile spirit of sal ammoniac that I have ever seen; and I believe stronger than it can be made in the common way.

In order to ascertain what addition, with respect to quantity and weight, water would acquire by being saturated with alkaline air, I put one grain and a quarter of rain water into a small glass tube, closed at one end with cement, and open at the other, the column of water measuring seven tenths of an inch; and having introduced it through the quicksilver into a vessel containing alkaline air, observed that it absorbed seven eighths of an ounce measure of the *air*, and had then gained about half a grain in weight, and was increased to eight tenths of an inch and a half in length. I did not make a second experiment of this kind, and therefore will not answer for the exactness of these proportions in future trials. What I did sufficiently answered my purpose, in a general view of the subject.

When I had, at one time, saturated a quantity of distilled water with alkaline air, so that a good deal

deal of the air remained unabſorbed on the ſurface of the water, I obſerved that, as I continued to throw up more air, a conſiderable proportion of it was imbibed, but not the whole; and when I had let the apparatus ſtand a day, much more of the air that lay on the ſurface was imbibed. And after the water would imbibe no more of the *old* air, it imbibed *new*. This ſhews that water requires a conſiderable time to ſaturate itſelf with this kind of air, and that part of it more readily unites with water than the reſt.

The ſame is alſo, probably, the caſe with all the kinds of air with which water can be impregnated. Mr. Cavendiſh made this obſervation with reſpect to fixed air, and I repeated the whole proceſs above-mentioned with marine acid air, and had preciſely the ſame reſult. The alkaline water which I procured in this experiment was, beyond compariſon, ſtronger to the ſmell, than any ſpirit of ſal ammoniac that I had ſeen.

This experiment led me to attempt the making of ſpirit of ſal ammoniac in a larger quantity, by impregnating diſtilled water with this alkaline air. For this purpoſe I filled a piece of a gun barrel with the materials above-mentioned, and luted to the open end of it a ſmall glaſs tube, one end of which was bent, and put within the mouth of a glaſs veſſel, containing a quantity of diſtilled water

upon quickſilver, ſtanding in a baſon of quickſilver. In theſe circumſtances the heat of the fire, applied gradually, expelled the alkaline air, which, paſſing through the tube, and the quickſilver, came at laſt to the water, which, in time, became fully ſaturated with it.

By this means I got a very ſtrong alkaline liquor, from which I could again expel the alkaline air which I had put into it, whenever it happened to be more convenient to me to get it in that manner. This proceſs may eaſily be performed in a ſtill larger way; and by this means a liquor of the ſame nature with the volatile ſpirit of ſal ammoniac, might be made much ſtronger, and much cheaper, than it is now made.

I had ſome expectation that alkaline air might be expelled from cauſtic *fixed alkali*, eſpecially as it is known that the fixed and volatile alkalis differ only in their combinations; but I was diſappointed in my expectations. Having procured a quantity of cauſtic alkali from Mr. Lane, who is known to prepare it with particular accuracy, I treated it in the ſame manner as I had done the ſpirit of ſalt, and found that the vapour expelled from it conſiſted of nothing but water, being immediately condenſed when it came to the cold quickſilver.

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SECTION II.

The Mixture of Alkaline Air with various Substances, and miscellaneous Properties of it.

HAVING satisfied myself with respect to the relation that alkaline air bears to water, I was impatient to find what would be the consequence of mixing this new air with the other kinds with which I was acquainted before, and especially with marine *acid* air; having a notion that these two airs, being of opposite natures, might compose a *neutral air*, and perhaps the very same thing with common air. But the moment that these two kinds of air came into contact, a beautiful white cloud was formed, and presently filled the whole vessel in which they were contained. At the same time the quantity of air began to diminish, and, at length, when the cloud was subsided, there appeared to be formed a solid *white salt*, which was found to be the common *sal ammoniac*, or the marine acid united to the volatile alkali.

The first quantity that I produced immediately deliquesced, upon being exposed to the common

air; but if it was expoſed in a very dry and warm place, it almoſt all evaporated, in a white cloud. I have, however, ſince, from the ſame materials, produced the ſalt above-mentioned in a ſtate not ſubject to deliqueſce, or evaporate. This difference, I find, is owing to the proportion of the two kinds of air in the compound. It is only volatile when there is more than a due proportion of either of the conſtituent parts. In theſe caſes the ſmell of the ſalts is extremely pungent, but very different from one another; being manifeſtly acid, or alkaline, according to the prevalence of each of theſe airs reſpectively.

Nitrous air admitted to alkaline air likewiſe occaſioned a whitiſh cloud, and part of the air was abſorbed; but it preſently grew clear again; leaving only a little dimneſs on the ſides of the veſſel. This, however, might be a kind of ſalt, formed by the union of the two kinds of air. There was no other ſalt formed that I could perceive. Water being admitted to this mixture of nitrous and alkaline air preſently abſorbed the latter, and left the former poſſeſſed of its peculiar properties.

Fixed air admitted to alkaline air formed oblong and ſlender cryſtals, which croſſed one another, and covered the ſides of the veſſel in the form of net-work. Theſe cryſtals muſt be the ſame thing with the volatile alkalis which chemiſts get in a

ſolid

ſolid form, by the diſtillation of ſal ammoniac with fixed alkaline ſalts.

Inflammable air admitted to alkaline air exhibited no particular appearance. Water, as in the former experiment, abſorbed the alkaline air, and left the inflammable air as it was before. It was remarkable, however, that the water which was admitted to them became whitiſh, and that this white cloud ſettled, in the form of a white powder, to the bottom of the veſſel.

Alkaline air mixed with *common air*, and ſtanding together ſeveral days, firſt in quickſilver, and then in water (which abſorbed the alkaline air) it did not appear that there was any change produced in the common air; at leaſt it was as much diminiſhed by nitrous air as before. The ſame was the caſe with a mixture of marine acid air and common air.

Having mixed air that had been diminiſhed by the fermentation of a mixture of iron filings and ſulphur with alkaline air, the water abſorbed the latter, but left the former, with reſpect to the teſt of nitrous air (and therefore, as I conclude, with reſpect to all its properties) the ſame that it was before.

Spirit of wine imbibes alkaline air as readily as water, and ſeems to be as inflammable afterwards as before.

Alkaline

Alkaline air contracts no union with *olive oil.* They were in contact almost two days, without any diminution of the air. Oil of turpentine, and essential oil of mint, absorbed a very small quantity of alkaline air, but were not sensibly changed by it.

Ether, however, imbibed alkaline air pretty freely; but it was afterwards as inflammable as before, and the colour was not changed. It also evaporated as before, but I did not attend to this last circumstance very accurately.

Sulphur, *nitre*, *common salt*, and *flints*, were put to alkaline air without imbibing any part of it; but *charcoal*, *sponge*, bits of *linen cloth*, and other substances of that nature, seemed to condense this air upon their surfaces; for it began to diminish immediately upon their being put to it; and when they were taken out the alkaline smell they had contracted was so pungent as to be almost intolerable, especially that of the sponge. Perhaps it might be of use to recover persons from swooning. A bit of sponge, about as big as a hazel nut, presently imbibed an ounce measure of alkaline air.

A piece of the inspissated juice of *turnsole* was made very dry and warm, and yet it imbibed a great quantity of the air; by which it contracted a most pungent smell, but the colour of it was not changed.

Alum

Alum undergoes a very remarkable change by the action of alkaline air. The outward ſhape and ſize remain the ſame, but the internal ſtructure is quite changed, becoming opaque, beautifully white, and, to appearance, in all reſpects like alum which had been roaſted; and ſo as not to be at all affected by a degree of heat that would have reduced it to that ſtate by roaſting. This effect is produced ſlowly; and if a piece of alum be taken out of alkaline air before the operation is over, the inſide will be tranſparent, and the outſide, to an equal thickneſs, will be a white cruſt.

I imagine that the alkaline vapour ſeizes upon the water that enters into the conſtitution of crude alum, and which would have been expelled by heat. Roaſted alum alſo imbibes alkaline air, and, like the raw alum that has been expoſed to it, acquires a taſte that is peculiarly diſagreeable.

Phoſphorus gave no light in alkaline air, and made no laſting change in its dimenſions. It varied, indeed, a little, being ſometimes increaſed and ſometimes diminiſhed, but after a day and a night, it was in the ſame ſtate as at the firſt. Water abſorbed this air juſt as if nothing had been put to it.

In ſome chemical proceſſes, volatile alkali diſſolves copper. This I alſo have obſerved in my account

account of the experiments in which I put ſome pieces of volatile alkaline ſalt to a quantity of common air, at the time that I introduced nitrous air to it. For, if the alkaline ſalt be ſupported by copper wire, it preſently becomes blue, and is ſoon corroded. I therefore thought that pieces of copper, expoſed to pure *alkaline air*, would have been affected in the ſame manner; but I did not find this to be the caſe. A number of pieces of copper wire remained a whole night in alkaline air without ſenſibly affecting it, or being affected by it. That the alkaline air was pure, appeared by its being wholly abſorbed by water afterwards.

Having put ſome *ſpirit of ſalt* to alkaline air, the air was preſently abſorbed, and a little of the white ſalt above-mentioned was formed. A little remained unabſorbed, and tranſparent, but upon the admiſſion of common air to it, it inſtantly became white.

Oil of vitriol, alſo formed a white ſalt with alkaline air, and this did not riſe in white fumes.

Marine acid air, as I have obſerved, extinguiſhes a candle. Alkaline air, on the contrary, I was ſurprized to find, is ſlightly inflammable; which, however, ſeems to confirm the opinion of chemiſts, that the volatile alkali contains phlogiſton.

I dipped

I dipped a lighted candle into a tall cylindrical veſſel, filled with alkaline air, when it went out three or four times ſucceſſively; but at each time the flame was conſiderably enlarged, by the addition of another flame, of a pale yellow colour; and at the laſt time this light flame deſcended from the top of the veſſel to the bottom. At another time, upon preſenting a lighted candle to the mouth of the ſame veſſel, filled with the ſame kind of air, the yellowiſh flame aſcended two inches higher than the flame of the candle. The electric ſpark taken in alkaline air is red, as it is in common inflammable air.

Though alkaline air be inflammable, it appeared, by the following experiment, to be heavier than the common inflammable air, as well as to contract no union with it. Into a veſſel containing a quantity of inflammable air, I put half as much alkaline air, and then about the ſame quantity of marine acid air. Theſe immediately formed a white cloud, but it did not riſe within the ſpace that was occupied by the inflammable air; ſo that this latter had kept its place above the alkaline air, and had not mixed with it.

That alkaline air is lighter than acid air is evident from the appearances that attend the mixture, which are indeed very beautiful. When acid air

is introduced into a veſſel containing alkaline air, the white cloud which they form appears at the bottom only, and aſcends gradually. But when the alkaline air is put to the acid, the whole becomes immediately cloudy, quite to the top of the veſſel.

In the laſt place, I ſhall obſerve that alkaline air, as well as acid, diſſolves *ice* as faſt as a hot fire can do it. This was tried when both the kinds of air, and every inſtrument made uſe of in the experiment, had been expoſed to a pretty intenſe froſt ſeveral hours. In both caſes, alſo, the water into which the ice was melted diſſolved more ice, to a conſiderable quantity.

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SECTION III.

Of the Mixture of all the Kinds of Acid Air with Alkaline Air.

THAT the fluor acid air ſaturates the ſame quantity of *alkaline air* with vitriolic acid air, appeared in a courſe of experiments not made with this particular view, but deſigned to aſcertain the quantity of acid in all the kinds of acid air. I firſt thought of attempting this by finding what quantity of water, tinged blue with the juice of turnſole, an equal quantity of each of the kinds of air would turn red. But it could not have been determined with ſo much accuracy in this way, at leaſt not ſo *viſibly*, and demonſtrably, as by the ſaturation of them with alkaline air; *diminution of bulk* being a thing eaſily meaſured. The reſult of theſe experiments is, in ſeveral reſpects, rather extraordinary; being ſuch as, I imagine, no perſon would have conjectured *a priori*. At leaſt it was contrary to my expectations. As I made theſe experiments in two different ways, and the reſults, though agreeing in general,

general, differ a little, it may be proper to recite the particulars of both.

In order to determine what quantity of acid and alkaline air would ſaturate one another, I firſt filled a glaſs jar with quickſilver, and put into one meaſure (which was about an ounce meaſure and a half) of alkaline air, and then the ſame quantity (being from the very ſame phial) of *marine acid air*; when the whole of both very nearly diſappeared, a quantity of ſal ammoniac being formed. I then threw into the ſame jar another meaſure of marine acid air, and obſerved that it occupied about one ſixth leſs ſpace than the original meaſure of alkaline air; ſo that a little more of the acid than of the alkaline air had diſappeared. I then threw in the ſame meaſure of each of the kinds of air alternately, till I had, in all, five meaſures of each; after which hardly any air remained, except what was not abſorbed by water, which was about a fourth part of the original meaſure, and was very little different from common air; two meaſures of it and one of nitrous air occupying the ſpace of one meaſure and a quarter. Upon the whole, therefore, it may ſafely be concluded from this experiment, that equal quantities of theſe two kinds of air ſaturate one another.

When

When I put a meafure of *vitriolic acid air* to the alkaline air, I found that a given quantity of the latter was faturated, as nearly as I could judge, by half as much of the former. For when they were mixed in that proportion, the whole quantity of both kinds of air entirely difappeared. It was in making this experiment, always throwing in two meafures of alkaline air for one of the vitriolic, that I got the thick *yellow coating* of the infide of the jar mentioned above, and which became white by being expofed to the open air.

Making the experiment in the fame manner, I alfo found that a meafure of alkaline air was completely faturated by half a meafure of *fluor acid air*, fo that both intirely difappeared. In this remarkable circumftance the fluor acid air and vitroilic acid air perfectly agree; which appeared to me to afford a very ftrong argument of their ultimate identity.

Having put one meafure of *fixed air* to one of alkaline air, the quickfilver rofe to half a meafure; and when I introduced another meafure of fixed air, no more was abforbed; and water being admitted to it, imbibed almoft all the remainder; fo that half a meafure of fixed air had faturated a whole meafure of alkaline air. But the union of thefe two kinds of air is not effected fo quickly as in the other cafes; otherwife the whole of the alkaline air muft have difappeared, when half the quantity of fixed

air had been admitted to it. Afterwards, making this mixture very gradually, one third of the quantity of fixed air ſaturated almoſt the whole of the alkaline air.

Laſtly, I filled the veſſel in which I had meaſured the preceding kinds of air, with *red vapour of ſpirit of nitre*, and put to it an equal quantity, and one fourth more, of alkaline air; when it immediately became very turbid and white, and the quickſilver roſe within the veſſel. Then filling the ſame phial again with the red vapour, as nearly as I could in the ſame manner, and admitting water to it, one fourth of the contents of the phial was abſorbed. This, therefore, muſt have been the ſpace that would have been occupied by the pure nitrous vapour, if there had been no common air mixed with it. Conſequently one meaſure of nitrous vapour, in fact, ſaturated about five meaſures of alkaline air. But there is a conſiderable degree of uncertainty with reſpect to the real quantity of nitrous vapour in a phial that is ſeemingly filled with it.

In the next ſet of experiments, without taking equal meaſures both of alkaline and of the ſeveral kinds of acid air, I took equal meaſures of the acid airs only; and having a large jar of alkaline air, I endeavoured to find how much of it equal quantities of the ſeveral acid airs would ſaturate. The veſſel, which in all theſe caſes I filled with acid air, contained

contained an ounce meaſure and one ſixth, and the diminution of bulk in the jar of alkaline air (which was carefully filled a new in all the trials) by the ſeveral kinds of acid air was, as nearly as poſſible, in the following proportion:

A meaſure of fluor acid air, abſorbed of the alkaline air	$1\frac{19}{26}$
————vitriolic	2
————marine	$1\frac{1}{6}$
————fixed air	$1\frac{6}{7}$

Had I mixed the fixed air very ſlowly, as in one of the caſes of the preceding ſet of experiments, more of the alkaline air would probably have been ſaturated with it.

I would obſerve alſo, that, in one reſpect, this latter method of making the experiments, is leſs accurate than the other; as I did not make allowance for the different degrees of dilatation of the air above the quickſilver by the weight of the quickſilver in the jar above the level of the quickſilver without the jar. But to have avoided this inaccuracy, I muſt have had a deeper veſſel, and more quickſilver than I happened to have at hand. But the reſults of the experiments, in both the methods of making them, agree ſo very nearly, that I have

little doubt but that the conclusions may be sufficiently depended upon.

That vitriolic acid air should saturate more alkaline air than marine acid air can do, is agreeable enough to the observation of the quantities of *water* they respectively saturate; the difference between them, though much greater in this case, being *in the same way* as in the other. For an equal quantity of water will imbibe ten times as much marine acid air as of vitriolic. Water will also take little more than its bulk of fixed air; so that a given quantity of alkaline air ought, for the same reason, to take less of this, than of any other kind of acid air, which agrees with the experiment. And yet, considering the apparent *strength* of the acids, one would imagine that the vitriolic, which, in this form, is weaker than the marine, should saturate less of the alkaline air; and again that the fixed air, being the weakest of all the acids, should saturate the least quantity of alkaline air. But the former view, in which the experiments do not appear at all extraordinary, is, in fact, more agreeable to the analogy of the saturation of water with these acids; and the difference in the proportion absorbed of each of them, corresponds to the difference of the specific gravities of water and air. But I did not see the subject in this light *a priori*.

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SECTION IV.

Of the electric Spark in Alkaline Air, and its Conversion into inflammable Air.

I Took the electric explosion in a small quantity of *alkaline air*, and observed, that every stroke added considerably to the quantity of air; and when water was admitted to it, just so much remained unabsorbed as had been added by the explosions. I then took about an hundred explosions of the same jar, in a larger quantity of alkaline air; after which, so much of it remained unabsorbed by water, that I could examine it with the greatest certainty. It neither affected common air, nor was affected by nitrous air, and was as strongly inflammable as any air that I had ever procured.

In the next place, I ascertained the *quantity* of inflammable air that may be produced from any given quantity of alkaline air. And this production having its limits, certainly shews that the alkaline air supplies some essential part in the constitution of it.

To take my meaſures with more accuracy, I, at this time, confined the alkaline air in a glaſs tube, of the ſame dimenſions throughout; and having it confined, as it neceſſarily muſt be, by quickſilver, I carefully marked the ſpace which it occupied in the tube. I then took the electric ſpark, or exploſion, which ever of them happened to be the moſt convenient, till I perceived that no more addition was made to the quantity of air; and then meaſuring the ſpace which it occupied, I found that the whole was, as nearly as poſſible, three times as much as that which the alkaline air alone had occupied. When I examined this air, I found it to have an inflammability of the ſtrongeſt kind, firing with exploſions, and in no reſpect to be diſtinguiſhed from that which is extracted from metals by acids. Alſo the electric ſpark taken in this air was always red, though, as is alſo the caſe with other inflammable air, it was white in the centre of any conſiderable exploſion taken in it.

After this experiment it ſtill remained a doubt whether, when the proceſs was completed, there did not remain, at leaſt, ſome portion of alkaline air not affected by it, and capable of being abſorbed by water afterwards. To determine this, and likewiſe to repeat ſo important an experiment upon a larger ſcale, I began with one third of an ounce meaſure of alkaline air, and I took the electric ſpark

in

in it till I had got a complete ounce measure of air. Then admitting a little water to it, I observed with the greatest attention, but could not perceive that any part of the air was absorbed by it. However, when I had made this air explode, by means of the flame of a candle, and immediately after applied my nostrils to the mouth of the vessel in which it had been contained, I perceived a very evident alkaline smell; so that the whole of the volatile alkali had not been completely incorporated with this air, though it was so much so, as not to be seized by the *water*. And to give it a fairer trial, this water had been confined along with the air, upon the quicksilver, and had been even frequently agitated with it, during two whole days; and though it was but a very small quantity, it had no perceivable alkaline smell afterwards.

When I reflected upon this change of the alkaline air by the electric explosion, I considered that it might be produced either by *electricity as such*, or by the *light*, or the *heat*, which accompany the experiment. In order to try the effect of *light*, I exposed a small quantity of alkaline air to a strong light made by a burning lens, for several hours. But this made no sensible change in the quantity of it. I therefore concluded that the proper agent in this process was not light.

I did not immediately proceed to try the effect of heat, but obſerved it by accident ſome time afterwards, in the following manner. At the time that I was reviving metals in inflammable and alkaline air, I put a quantity of ochre into the latter, expecting to revive it in the form of iron; and in this I ſeemed to ſucceed, as the ochre became black in the courſe of the proceſs. But what ſtruck me the moſt was, that inſtead of perceiving the quantity of air to decreaſe, as I had expected, there was very ſoon a viſible increaſe of it. Examining the air, I found it to contain no fixed air, and to be all ſtrongly inflammable.

Still attending to other circumſtances beſides the change of the alkaline air, I proceeded to heat in it ſcales of iron, and iron itſelf, of different kinds, and ſtill found that the air was increaſed in quantity, and changed in its quality. But in this way I could never increaſe the bulk of the air more than twice, and ſeldom quite ſo much. Alſo, after the proceſs, when I could make no farther change in the air, I ſtill perceived a very ſtrong ſmell of volatile alkali. The iron had undergone no change that I could diſcover, ſo that I concluded that the air had been affected by means of *heat* only.

To aſcertain this, I then proceeded to heat in the alkaline air bits of dry crucibles, or of earthen retorts, which had been juſt before expoſed to very great

great heats, ſo that they could not be ſuppoſed to give out any air in this proceſs, and therefore could only ſerve to produce a ſtrong heat in the alkaline air. The reſult ſeemed to confirm my ſuppoſition. For in theſe experiments with earthy ſubſtances the effect was always the ſame as when I had made uſe of ochre or iron. The air was increaſed in bulk, but the quantity not quite doubled, and it was become inflammable.

The bits of *white* earthen ware having always become *black* in this proceſs, I ſuſpected that this kind of ſubſtance might have affected the air in ſome other way than by merely communicating heat to it. To try this, I uſed the ſame ſmall bit of crucible again and again, and finding the ſame effect from heating it at the laſt, as at the firſt, I could not help concluding that the change produced *in the air had been effected by means of heat* only.

In all theſe experiments, however, there was a ſtrong *light* as well as an intenſe heat concerned. In order to aſcertain, therefore, whether the light, at leaſt in conjunction with heat, might not have had ſome effect in this change of the air, I heated a quantity of it in a coated green glaſs retort. The mouth of the retort was plunged in a baſon of mercury, and I received in a glaſs tube filled with

with water all the air that was driven out of the retort by heat. At firſt it was all abſorbed by the water; being merely alkaline air expelled by rarefaction. But when I concluded that the bulb of the retort was become red hot, the bubbles that were driven out were not wholly abſorbed by water, and by degrees no part of them was abſorbed. This air I examined, and found it to be inflammable. The retort was melted in the proceſs, ſo that I could not examine the air that remained in it; but my purpoſe was ſufficiently anſwered, as I ſaw no room to doubt, but that the change of the alkaline into inflammable air was produced by *mere heat*, without the aid of light, if a red heat can be ſo termed. How the heat operates in this caſe I cannot tell.

It was obſervable, that whenever I changed alkaline air into inflammable, by heating in it bits of crucibles or retorts, into the compoſition of which *clay* enters, they always became black. There was room, therefore, to ſuppoſe that it was not by means of *mere heat* that this change was made, but that ſomething might be depoſited from the air, which might attach itſelf to the clay. Indeed, if this was not the caſe, I do not ſee why the clay ſhould become black; though, perhaps, part of the ſame phlogiſton which forms the inflammable air

air may be attracted by the red hot clay, without there being any proper decomposition of the air.

That this is the case, seems probable from an experiment in which I used white *porcelain*, instead of bits of earthen ware, into the composition of which clay enters. For this substance did not become black in the process, and yet inflammable air was produced. But I must observe, that a considerable *time* was requisite to produce this effect in these circumstances, and that it was not much inflammable air that I was able to procure in this way. It was, however, in winter, and the heat that I could apply was not great.

Alkaline air is converted into inflammable air by making it pass through a red hot earthen tube, as well as by the electric spark, but by no means, I think, in so great a degree. I put two ounces and ten pennyweights of water, pretty strongly impregnated with alkaline air, into the retort, and heating it, sent the vapour through the hot tube; when I collected two ounces three pennyweights of liquor, which had a disagreeable empyreumatic smell, as well as that of volatile alkali; and it was quite opake with a black matter which subsided to the bottom of the vessel. Also the tube through which the air and vapour had been conveyed was left quite black,

One of the junctures in the apparatus not having been air tight, I did not collect all the air; but it came only at the beginning of the process, and before the tube became black, or any liquor was distilled; and it was all strongly inflammable.

SECTION V.

The Analysis of Alkaline Air.

THAT alkaline air contains phlogiston appears both by the calces of metals being revived in it, and by its being converted into a species of inflammable air, by the electric explosion, and other processes. By the following experiments it seems to contain less phlogiston than an equal bulk of inflammable air from iron, and about one fourth of its bulk of phlogisticated air. But it is not possible to ascertain this with perfect exactness, from the nature of the processes. I shall therefore recite the experiments in the order in which they were made, some of them being more, and others less accurate.

 I first

I first observed that some *massicot*, which I used in some of these experiments, contained a portion of fixed air, which could be expelled from it by heat; but when I melted it in alkaline air, by which means the lead was always revived, I found no fixed air in the residuum, but only pure phlogisticated air. This, however, is easily accounted for, as the alkaline air would immediately unite with any fixed air that should be set loose in the process.

With respect to the quantity of lead revived in alkaline air, it is difficult to be ascertained, on two accounts. In the first place, some of the calx is blackened, and imperfectly revived; and again, if particular care be not taken, the lead that is revived will be dissolved in the mercury, by which the air must necessarily be confined. To prevent this last inconvenience, I put the powdered massicot into small earthen cups, which I contrived to place with their mouths upwards, so that when the lead was revived, by means of a burning lens, it would remain in the cup, and not mix with the mercury by which it was supported. On these accounts, however, the largest quantity of lead that was revived must always be considered as nearest the truth.

In three ounce measures of alkaline air, I first revived six grains of lead, then in three ounces and a half

a half ſixteen grains, in two ounces and a half thirteen grains, and in three ounces three fourths twelve grains. In five ounce meaſures I revived ten grains, beſides blackening a quantity of it, and there remained one ounce meaſure of phlogiſticated air. But the experiment which I made with the moſt care, and on which I place the greateſt dependence, was the following.

In ſeven ounce meaſures and a half of alkaline air, I revived twenty ſix grains and a half of lead, and there remained two ounce meaſures and a half of phlogiſticated air. In this proportion 100 ounce meaſures of alkaline air would revive 352 grains of lead; whereas 100 ounce meaſures of inflammable air from iron would revive 480 grains of lead. But alkaline air is reſolvable into conſiderably more than twice its bulk of inflammable air, and therefore it might have been expected that it would have revived much more lead. Would it not, therefore follow from this, that phlogiſticated air contains a very great quantity of phlogiſton, many times more than an equal bulk of pure inflammable air. Suppoſing alkaline air to be reſolvable into no more than twice its bulk of inflammable air, a hundred ounce meaſures of it ought to revive 960 grains of lead. But in the proportion mentioned above, it would revive no more than 608 grains. As much phlogiſton, therefore,

fore, as would revive 342 grains of lead (which is the difference between these two numbers) should be contained in about twenty five ounce measures of phlogisticated air. Consequently a hundred ounce measures of phlogisticated air would revive 1368 grains of lead, which is more than twice as much as an equal quantity of pure inflammable air would revive. It must, however, be considered, that the inflammable air, into which alkaline air is resolvable, is not the same with that which is procured from iron, and may contain much less phlogiston.

To these experiments, on the revival of lead in alkaline air, I shall subjoin an account of some that were made with other calces, heating them all, when confined by this air, by means of a burning lens.

Glass of antimony absorbed a part of the alkaline air, and there was the appearance of metal in several small globules, whereas I could get no such appearance from the same substance in inflammable air.

By heating scales of iron in alkaline air, it was much increased in quantity, and then I found it to contain no fixed air, but the whole was strongly inflammable. No water was produced in this process. The iron was revived, being perfectly soluble in diluted oil of vitriol; and from being twenty four

four grains had loſt one grain in weight. It is evident from this experiment, that there is water in alkaline air.

As alkaline air evidently contains phlogiſton, and as ſuch was capable of reviving lead, and no doubt the other metals, I had the curioſity to make uſe of it in other circumſtances in which I had before uſed inflammable air; and among other things I tried the effect of heating red precipitate in it. The conſequence was, that the mercury was revived, and at the ſame time a conſiderable quantity of water was produced. It has even run down in drops in the inſide of a veſſel which contained about five ounce meaſures of the air. The air which remained after this proceſs was about half as much as the original quantity of alkaline air, and it had in it a quantity of undecompoſed dephlogiſticated air. For with two equal meaſures of nitrous air, the ſtandard of it was 1.4, and there was nothing inflammable in it.

Again, I threw the focus of the lens upon red precipitate, in alkaline air, till three meaſures of it were reduced to two. Water was produced in the proceſs, and the air that remained was conſiderably dephlogiſticated; the ſtandard of it, with a mixture of two equal meaſures of nitrous air, being 1.7.

But

But in another experiment of this kind, there muſt have been a quantity of inflammable air ſet looſe from the alkaline air, as well as dephlogiſticated air from the red precipitate; becauſe, after the operation had continued ſome time, there was a violent exploſion within the veſſel; which threw it many feet perpendicularly into the open air, as I was holding it in my hands. In this experiment I had been particularly careful to make every thing concerned in it as dry as I poſſibly could, in order to ſatisfy myſelf with reſpect to the production of the *water* which I had found before; and there was time enough before the exploſion to obſerve that water was certainly produced in the proceſs. To appearance the quantity of air was never much diminiſhed.

That water ſhould be produced when the ſcales of iron are heated in alkaline air, which I found to be the caſe, is not extraordinary, as the ſame is found on heating this ſubſtance in inflammable air; and it appears to contain within itſelf the water that is expelled from it.

In this caſe it muſt have come, in part from the alkaline air, which I have obſerved, certainly contains it, and in part from the precipitate; and ſince it is capable of yielding dephlogiſticated air by heat, it muſt contain water, which enters in a great proportion into this kind of air.

BOOK VII.

MISCELLANEOUS EXPERIMENTS AND OBSERVATIONS RELATING TO AIR.

SECTION I.

Of the Aerial Form of Subſtances.

THOUGH, ſince the accidental diſcovery of the *marine-acid air*, I have purpoſely ſought for others, and not without ſucceſs, having found ſeveral other kinds of acid air, beſides the *alkaline principle*, which I have alſo been able to exhibit in the ſame form, free from any combination with water, I by no means conclude that I have diſcovered *all* the kinds of air that may exiſt in nature; or, in other words, all the kinds of ſubſtances, ſimple, or compound, that are capable of being reduced

duced to a dry and permanently elastic vapour. For I believe there is no substance in nature but what is capable of assuming that form, in a *certain degree of heat.* And though my experience in these matters might enable me to judge pretty well *a priori* what substances are likely to yield air, and of what kind it will be, I have frequently been exceedingly disappointed in my expectations in this respect; and I have reserved for this place the mention of several substances, and combinations of substances, which, in general, contrary to my expectations, though not always so, yield no air that I can discover; and I shall be glad to be informed by any person *why* they do not.

1. Having expelled a permanently elastic fluid, to which I give the name of *air*, from spirit of salt, volatile spirit of sal ammoniac, and other fluids, I took it for granted, that from *radical vinegar*, which is the pure *vegetable* acid, in its state of greatest concentration, I should have got a true vegetable acid air; but it certainly yields no such thing by the application of heat only. Though radical vinegar yields air by the dissolution of some metallic substances, I could not make it give any air by the help of any other substances containing phlogiston, which is the case with oil of vitriol; and I gave it, for this purpose, a boiling heat, with all the substances following, viz. *copper*, *quicksilver*, *bis-*

muth, charcoal, oil of turpentine, ſpirit of wine, and *liver of ſulphur,* which laſt, however, yielded a little fixed air; but this muſt have been contained in it before.

2. I was hardly ever more ſurprized than when I found that I could get little or no air from the *ſmoking liquor of Labavius,* though the fume iſſuing from it is ſo exceedingly denſe. I heated a conſiderable quantity of this liquor prepared by Mr. Woulfe, and which he obligingly brought me for the ſake of the experiment, which he himſelf ſuggeſted, as not likely to fail. This I did in a glaſs phial with a ground ſtopper, intending to receive the produce in quickſilver. But little or nothing came over, beſides the common air which had lodged in the top of the phial, and which appeared not to have been in the leaſt injured after the experiment. A very ſmall part, however, of what came over, ſeemed to be abſorbed by water; and alſo when, in the preſence of Mr. Woulfe, I endeavoured to convey the air that might be expelled from this liquor into a quantity of alkaline air, a ſlight degree of *whiteneſs* was produced. But both theſe effects might have been occaſioned by a little redundant ſpirit of ſalt contained in this liquor.

3. I have not yet been able to combine either the fixed or the volatile *alkali,* with any ſubſtance whatever,

whatever, ſo as to form a permanent air, though the alkaline principle itſelf may be exhibited in that form. Some facts had been mentioned to me by Mr. Woulfe, which led us both to conclude, that air might be procured from it by means of a ſolution of *iron*. But when, in conſequence of this hint, I endeavoured to procure air from iron by means of the alkalis, the experiments which I made for this purpoſe did not ſucceed. I boiled a quantity of very fine iron wire in cauſtic fixed alkali, after finding that nothing could be produced from it without heat; and I put another quantity of the ſame wire into a ſtrong volatile ſpirit of ſal ammoniac, and alſo let ſome of it remain about a month in alkaline air; but in all theſe caſes, without any viſible effect. At the end of the laſt-mentioned proceſs, water being admitted to the air, abſorbed the whole of it inſtantly, which ſhewed that it was perfectly pure, and had got nothing from the iron. Alſo, no air was produced from liver of ſulphur, dry or moiſt, in alkaline air; and they continued together almoſt two days.

4. I expected to have expelled ſome kind of air (and at that time thought it not impoſſible but it might be even phlogiſton itſelf in the form of air) from *ſpirit of wine*. But, like water, it was only converted by heat into *vapour*, which was con-

denſed again in the cool recipient. The addition of *camphor* to it produced no other effect.

5. I was not without hopes, at one time, that the matter of the *odoriferous particles*, which affect the ſenſe of ſmell, might have been exhibited in the form of permanent air, as well as the exhalations of acid or alkaline ſubſtances; but though I made the experiment with ſeveral odoriferous ſubſtances, ſome grateful, and others offenſive to the noſtrils, it has hitherto been without any effect.

I moſt ſincerely wiſh that the *reaſon* of theſe phenomena may be fully inveſtigated, as ſuch an inveſtigation, if ſucceſsful, would probably carry us a good way into the knowledge of the ultimate conſtitution of natural bodies, and help us to explain ſome of the moſt fundamental laws reſpecting them. Hitherto I do not find that this ſubject has even been thought of by philoſophers.

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SECTION II.

Experiments relating to the ſeeming Converſion of Water into Air.*

SINCE many perſons have expreſſed a wiſh to be acquainted with the experiments I have lately made, which at firſt ſeemed to favour the idea of a *converſion of water into air*, but which terminated in the diſcovery of a fact, in my opinion, ſtill more extraordinary, I ſhall ſubmit to the Royal Society the reſult of the obſervations I have already made; though, as yet, I have by no means been able to ſatisfy myſelf ſo fully as I could wiſh with reſpect to ſome particulars connected with the ſubject. All the *facts* which I ſhall ſtate may be depended upon; but it is probable, that different perſons may draw different *concluſions* from them; and to mere *opinions*, I have never ſhewed myſelf much attached.

* This ſection is taken from the Philoſophical Tranſactions, vol. lxxiii. p. 414.

Having formerly obſerved ſeveral remarkable changes in fluid ſubſtances, in conſequence of long expoſure to heat in glaſs veſſels hermetically ſealed, I then formed a deſign of expoſing all kinds of ſolid ſubſtances to great heats, in a ſimilar ſtate of confinement; and for that purpoſe provided myſelf with a caſt iron veſſel, which I could cloſe at one end, like a digeſter, and of ſuch a length, that one of the ends might be red hot, while the other was ſufficiently cool to be handled. To this end there was a cock connected to a tube, by means of which I could let off ſteam, or air, in any period of the proceſs.

I imagined, that when ſubſtances, conſiſting of parts ſo volatile as to fly off before they had attained any conſiderable degree of heat, in the uſual preſſure of the atmoſphere, were compelled to bear great heats under a greater preſſure, they might aſſume new forms, and undergo remarkable changes, ſimilar to what we may ſuppoſe to be the caſe within the bowels of the earth, where, by means of ſubterraneous fires, various ſubſtances bear great heats under very great preſſures.

I have had this inſtrument ſome years; but it was ſo ill conſtructed, that I could not make the uſe of it that I had originally intended. I therefore lately fitted up ſome gun barrels in the ſame manner, and made my firſt experiment with lime ſtone;

ſtone; expecting that when the fixed air, and other volatile matters, that might be contained in it, ſhould be compelled to bear a red heat, without a poſſibility of making their eſcape, the ſubſtance itſelf might undergo ſome change; but I had no particular expectation concerning the nature of that change. I had, however, been ſo often favoured with valuable reſults, from merely putting things into new ſituations, that I was encouraged to make the experiment; but I found an unexpected difficulty in getting a cock that would be air-tight, and ſteam-tight, under ſo great a preſſure as I wiſhed to apply.

I was mentioning theſe ideas to Mr. Watt, in whoſe neighbourhood I have the happineſs to be ſituated, when he mentioned a ſimilar idea of his, *viz.* that of the poſſibility of the converſion of water, or ſteam, into permanent air; ſaying, that ſome appearances in the working of his fire engine had led him to expect this. He thought that if ſteam could be made red hot, ſo that all its latent heat ſhould be converted into ſenſible heat, either this or ſome other change would probably take place in its conſtitution. The idea was new to me, and led me to attend more particularly to my former projects of a ſimilar nature, and I began with lime ſtone, wiſhing to try the effect of giving a red heat to lime, in which water only ſhould be previouſly combined, thinking

thinking it might poſſibly have the ſame effect with making the water itſelf red hot.

Accordingly, I took a quantity of well calcined lime, and mixing with it a little water, out of which all air had been carefully boiled, I expoſed it gradually to a ſtrong heat in an earthen retort, ſuch as I had been uſually ſupplied with by Mr. Wedgwood (who is as much diſtinguiſhed by his love, and generous encouragement, of ſcience, as he is by his improvements in his own curious art) not imagining that it could make any difference whether the lime, ſo prepared, ſhould receive its heat in an earthen retort, or in a veſſel of iron or glaſs. Proceeding, however, in this manner, I found that nothing came over in the form of *ſteam*, but that there was a great quantity of *air*, ſeveral hundred times more than the bulk of the water, and at that time there was in it a conſiderable proportion of fixed air, which I imagined might either be that which had not been ſufficiently expelled before, or might be compoſed of ſome phlogiſtic matter contained in the lime, and the purer air that was yielded by the water. For I own I then concluded, that the air which I got (and which, when the fixed air was extracted from it, was ſuch as a candle would juſt burn in) came from the water, eſpecially as in ſome of the proceſſes, the weight of the air I caught was very nearly, if not quite equal to that of the water,

water, and interpoſing a large glaſs balloon between the retort and the recipient for the air, I obſerved that it remained perfectly cool and dry during the whole proceſs; and ſeveral hours afterwards there was not the leaſt moiſture condenſed in it. I alſo received a quantity of another produce of air, made in this manner in mercury, and having viewed it with the greateſt attention, obſerved that, after ſeveral days, it never depoſited the leaſt moiſture.

I then calcined a quantity of natural lime ſtone, with this glaſs balloon interpoſed in the ſame manner, and found no water, but only air to come from it, though the ſtone is generally ſuppoſed to contain water. But when I uſed much more than half an ounce of water to the quantity of whiting or lime above mentioned, I always had ſome water come over, though very little in proportion to the quantity made uſe of.

I did not fail to examine whether there had been any loſs in the weight of the lime, or whiting, in order to determine whether any part of theſe ſolid ſubſtances had entered into the compoſition of the air; but I found much difficulty in weighing them with exactneſs, after ſhaking them out of an earthen retort, into which I could not ſee, and to which part of theſe earthy matters often adhered, ſo that I could not obtain much ſatisfaction even when I broke

broke the retort. Beſides, there was always ſome loſs of the earth in the cloudineſs of the air, whenever the production of it was rapid. In a future proceſs I had abundant proof that the air did not come from any earthy matter with which the water had been combined.

Hitherto I had no idea but that all that was neceſſary to the converſion, as I concluded it to be, of water into air, was to give it a red heat, without which it would not quit the calcareous earth; and I imagined that by this means the matter, or principle, of *heat* was ſo intimately combined with it, as not to be ſeparated from it by cooling, as in the caſe of ſteam. But I, as well as all my friends, was a long time utterly diſconcerted upon finding that when I put the whiting and water into a coated glaſs retort, the water came over in the form of ſteam, and little or no air was produced. The reſult was alſo the ſame when I made the proceſs in a gun barrel, in a porcelain retort, or even in an earthen retort glazed in the inſide.

That the earth had not loſt its property of doing its part in the buſineſs, I found by putting more water to the ſame whiting which had failed in the glaſs retort, and which had been uſed no leſs than four times before, and then heating it in an earthen retort; when again it gave air only, and no water,

the ſame as before. And at this time I obſerved, that part of the air was hardly to be diſtinguiſhed from that of the atmoſphere.

I cannot expreſs my ſurpriſe at my unexpected failure with the glaſs retort; and my ſpeculations on the ſubject were various, but at that time altogether ineffectual. Among other things it occurred to me that, poſſibly, ſome phlogiſton, either contained in the earthen retort, or coming through it (though I could not tell *how*, or on what principle) from the fire, might be neceſſary to water, and all other ſubſtances aſſuming the form of air. But when, with this idea, I put ſpirit of wine, oil, or iron filings to the lime, I got nothing from theſe mixtures in glaſs retorts beſides ſteam and inflammable air, from the decompoſition of theſe ſubſtances containing phlogiſton.

That there was nothing in the materials of which the earthen retort was made, that neceſſarily produced the air, was evident from my not ſucceeding when I pounded a broken retort, and heated it, mixed with water, in one of glaſs.

Being ſatisfied that the production of air depended very much upon the retort itſelf, I thought of uſing the retort only with water, but without any lime, or earthy ſubſtance; and I found it ſucceed far beyond my expectation. For when I put a ſmall quantity of water into one of theſe retorts, and endeavoured

deavoured to diſtil it gently, I never failed to procure about an hundred ounce meaſures of air; and this I could do as often as I pleaſed, with the ſame retort, and without its loſing any weight; and the air produced in this manner never had any portion of fixed air in it, and was always but very little inferior to that of the atmoſphere.

In all theſe proceſſes I obſerved, that very little of this air was procured till all the water that could be poured out of the retort was evaporated, for the difference in the produce was very little, whether I expoſed the retort to the fire quite full of water, or with only about an ounce meaſure of water in it, or even after letting it remain full for a ſhort time, and then pouring out all that I could from it; ſo that it was only that water which was entangled, as it were, in the pores of the retort, and which had been in ſome meaſure united to the ſubſtance of it, that had contributed to this production of air.

Theſe retorts (which Mr. Wedgwood informs me are made of a mixture of freſh and of burnt Devonſhire pipe clay) are pervious to water, though not to air; ſo that while the air is produced from that water which has entered the pores, the reſt is ſometimes viſibly making its eſcape in the form of a copious ſmoke on the outſide. It was evidently impoſſible, however, as I then thought, and contrary to all the laws of hydroſtatics, that air ſhould enter by

the ſame pores by which the water or ſteam was eſcaping, and at the ſame time that its endeavour to force its way out of the retort was ſuch that it overcame a conſiderable reſiſtance from the column of water, at the mouth of my recipients. Air might have *eſcaped* through any unobſerved pores in the retort, but none could have *entered* that way; and if there was the leaſt ſenſible crack in any part of the retort, I was never able to collect any air at all*.

But the following experiments ſhewed, as I thought, that it is ſufficient for the production of air that ſteam come into contact with clay ſufficiently heated. Between a copper ſtill and the glaſs tube communicating with my recipient for air, I introduced the ſtem of a tobacco pipe; and by means of a ſmall furnace, I kept about three inches of the middle part of it moderately red hot. In this ſtate, making the water boil, I uniformly received air, though mixed with ſteam, at the rate of five ounce meaſures in twelve minutes for more than an hour; but when I let the pipe cool, nothing but ſteam was delivered by it without any air at all. There was no fixed air in this produce, and it was all ſuch as a candle would hardly have burned in it. It might, I thought, have been better, and alſo more in quan-

* It will appear, however, that the air muſt have entered by the pores, but by means of a power very different from that of *preſſure*, and able to counteract it.

tity,

tity, if I had not uſed the ſtem of a foul pipe. But when I uſed a clean pipe in the ſame manner, I did not find the air much, if at all, improved. Suſpecting this to ariſe from the near contact of the fuel, I incloſed the tobacco pipe in an earthen tube, and then I had air as good as I had generally got in the earthen retort, and not much worſe than that of the atmoſphere.

Another circumſtance I obſerved was, that if the outſide of the veſſel which contained the water or ſteam, through which it paſſed, when the requiſite heat was applied to it, was not dry, or perhaps ſurrounded with good air (for in thoſe circumſtances the following experiment differs from the preceding ones) the experiment did not ſucceed.

When I put the ball of an earthen retort, filled with moiſt clay, into an iron digeſter, and applied heat to it, I got only a very little fixed air (which was probably compoſed of a ſmall quantity of air beginning to be produced from the materials) and inflammable air from the veſſel. All that came over beſides was ſteam, and at laſt inflammable air from the veſſel itſelf.

Being now able to procure air by means of water, in this moſt ſimple method, *viz.* by water only in the earthen retort, I had an opportunity of aſcertaining, with great eaſe and exactneſs, ſeveral circumſtances relating to the proceſs, and of obviating,

as

as I thought, ſome objections to the concluſion that I had drawn from it. Among other things, I fully ſatisfied myſelf that the *earth of the retort* contributed nothing at all to this production of air, but the *water only:* for having uſed the ſame retort till I had got from it nearly an ounce weight of air, or 800 ounce meaſures, I found that it had not loſt ſo much as a ſingle grain in weight. After the firſt proceſs it weighed juſt three grains more than it did at firſt, and it continued to weigh the ſame till after the laſt proceſs. This ſmall addition of weight might eaſily have come from a little of the water having been imbibed by the neck of the retort, where the heat of the fire could not reach it. When all the proceſſes were over, I kept the whole retort in a red heat for ſeveral hours, and then found that, beſides loſing thoſe three grains, it weighed eight grains leſs than it did at firſt.

Before this I had found, that the calcined whiting, which I had uſed in the firſt experiment, could not, as ſome had ſuppoſed, attract from the atmoſphere, any conſiderable part of the air which I got from it, after combining water with it: for two ounces of the whiting (which was the quantity that I generally made uſe of) did not attract more than eight grains of any thing when it was expoſed a whole day in an open diſh, though it had loſt more than half its weight in calcination.

It had been imagined by ſome, that the air which I got in theſe earthen retorts was that which had been attracted from the atmoſphere by the inſide ſurface of them. But, beſides that no air could ever be produced without water, to obviate this objection more particularly, when one of theſe retorts was giving its laſt portion of air, I immerſed the mouth of it in a baſon of water; and letting it cool in that ſituation, filled it again without admitting any acceſs of air to the inſide; and yet, on repeating the proceſs with it, the air was produced juſt as freely as before. This operation I repeated ſeveral times. If it be ſaid, that the outſide of the retort attracted the air, ſtill the inſide, being compoſed of the ſame materials, muſt have attracted air alſo; and it would have appeared by the aſcent of the water from the baſon, the retort being ſufficiently impervious to air.

By ſome it was imagined, that either the air itſelf that I procured, or at leaſt the power of the retort to contribute to the production of it, was owing to ſomething that was tranſmitted from the burning coals, but which could not paſs through glaſs or metals. To determine this, I took an earthen tube, of the ſame compoſition with the retort, and putting a little water in it, placed it, ſurrounded with ſand, in a glaſs veſſel, and this again, ſurrounded alſo with ſand, in an iron one; and yet the heat tranſmitted

through all these substances, enabled the earthen tube to give air, in the same proportion, and of the same quality, as it would have done if it had been exposed to the naked fire.

Having now procured air, by means of water, in a very simple, and as I thought, an unexceptionable manner, I wished to make it in greater quantities, in proportion to the water employed; and for this purpose I first thought of increasing the size, or the thickness of the porous retorts; but I thought it might answer as well if I put into the retort, in powder, the materials of which they were made, or other substances of the same kind.

Accordingly, by mixing ground flint and clay in various proportions, I presently increased the quantity of air much beyond my expectation. In the first trials, in which I had much flint and a little clay, I never failed to get 200 ounce measures of air from one of water. Then, using more clay and less flint, I had still more air; and at last, leaving out the flint altogether, and using clay only, I never failed to get much more than 400, and generally between 500 and 600 ounce measures of air from one of water, which was about three fourths of the weight of the water; and in one particular process I procured very little less than nine tenths of the weight of the water in air, and this air was never much less pure than that of the atmosphere. Some-

times it could not be diſtinguiſhed from it at all by the teſt of nitrous air; and once or twice I thought it even purer than that of the atmoſphere.

I muſt here obſerve, that I found it not convenient to put ſo much water to any quantity of clay as would make it cohere in one maſs, but only ſo much as that it ſhould remain in the form of powder. By this means it might eaſily be poured out of the retort when the experiment was over.

The weight of the water expended in this production of air I aſcertained, in the moſt unexceptionable manner, by weighing the retort, with all its contents, before and after the proceſs. I ſhall explain this by the reſult of two of the proceſſes. In one of them, the retort and moiſtened clay together, loſt in weight 588 grains, after yielding 741 ounce meaſures of air, which (in the proportion of ſix grains to one ounce meaſure) would have weighed 444 grains, and conſequently, about three fourths of the weight of the water.

In the other proceſs, the loſs of weight was 358 grains, after yielding 556 ounce meaſures of air, which would have weighed 333 grains. The proportion, therefore, between the weight of the air and that of the water was 111 to 116, or nearly nine to ten.

I alſo found now, that ſo much heat as I had hitherto applied was neither neceſſary nor uſeful.

ful. In the laſt mentioned proceſs the retort was conſtantly ſuſpended about ſix inches above a moderate charcoal fire; at another time more than twelve or fifteen inches above it, where a Fahrenheit's thermometer did not ſhew more than 210°. With this moderate heat I got 465 ounce meaſures in the courſe of about twelve hours. When the retort was ſuſpended within ſix inches of the fire, the air was generally produced at the rate of thirty ounce meaſures in five minutes. But a thermometer, the bulb of which was immerſed in the clay, was ſtill only at the heat of boiling water.

In all theſe proceſſes, however, there was evidently ſome loſs of water; for, excepting the firſt experiment with the lime, I never got the whole weight of the water in air; and it might be ſaid that I only expelled the air before contained in the water, though from theſe experiments it appeared to contain much more air than it had been thought capable of containing. To obviate this objection, I contrived to catch all the water that eſcaped through the pores of the retort in the following manner.

Having put the moiſtened clay into an earthen tube, to which I had fitted a cock and a long glaſs tube (by means of which I could collect all the air that came from it) I put this within an iron tube, which was cloſed at the end next the fire, but open at the other end, and ſo long that I could eaſily

keep this open, and quite cool, while the other was in the fire; consequently, whatever water escaped through the pores of the earthen tube, it would be condensed in the cool part of the iron one. This water I carefully collected, and always found that the weight of it, together with that of the air produced in the experiment, was nearly that of the original weight of the water, estimated by the loss of weight in the earthen tube and its contents. I also found, that the water so collected served for the production of more air, just as well as any other water whatever, so that there had been no decomposition of the water in the case.

In the last process that I went through of this kind, the loss of weight in the earthen tube, or rather of the water contained in it, was 392 grains; the air collected was 173 ounce measures, which would have weighed eighty nine grains, and the water which escaped through the pores of the earthen tube, and which I collected, was nearly 195 grains; so that the air and this water together weighed 302 grains, or ten grains more than the original water. But as I estimated the weight of the water only by the space which it occupied in a cylindrical glass tube, divided according to ounces and parts of ounces of water, it was not easy to avoid an error of a few grains. At other times there was an error of a small magnitude on the other side. But it will appear

appear hereafter, that more ſteam muſt have eſcaped inviſibly at the open mouth of the iron tube than I was aware of.

That nothing could enter by the pores of the retort at the ſame time that the water was making its eſcape out of them, I thought I aſcertained pretty ſatisfactorily by immerſing the bulb of it in mercury, contained in an iron veſſel. In theſe circumſtances I obtained air as uſual, only the produce was not ſo rapid. In this way, however, I procured above an hundred ounce meaſures of air from moiſtened clay; and I diſcontinued the proceſs without perceiving any termination of it. But the moment the retort was raiſed out of the mercury, it gave air three times as faſt as it had done before. The quality of the air was the ſame in both caſes, *viz.* a little worſe than that of the atmoſphere.

I even collected thirty ounce meaſures of air when the bulb of the ſame retort was immerſed in hot linſeed oil; but the production of air gradually ceaſed, and the next day I found the retort almoſt full of the oil, which had ſoaked through it. Diſtilling this oil, I got 300 ounce meaſures of air wholly inflammable, except a very few ounce meaſures at the laſt, which were only phlogiſticated.

Another preſumption in favour of the generation of our atmoſphere from water was, that the purity of

the air that I produced from it is ſo very nearly the ſame with that of the atmoſphere. And the degree of heat requiſite to produce it is no greater than may be given by the rays of the ſun in certain circumſtances. Subterraneous fires, however, would be abundantly ſufficient for the purpoſe, as it appeared to be ſufficient for the converſion of water into reſpirable air, that it come into contact with clay, and perhaps many other earthy ſubſtances in the form of vapour. I muſt, however, obſerve, that when I threw the focus of a burning lens upon a quantity of moiſt clay, either *in vacuo*, or in common air, I got no air from it.

I made this experiment both with the clay expoſed in an open diſh, and alſo confined in a ſhort earthen tube. Had I then proceeded to repeat this laſt proceſs with a communication between the inſide of the earthen tube and the external air (as I then propoſed to do, but was prevented) I ſhould much ſooner have diſcovered what I did afterwards, *viz.* that there was no real converſion of water into air in this proceſs.

The difficulty that ſtrikes many perſons moſt forcibly, is the want of analogy between the converſion of water into air with any other known facts in philoſophy, or in nature. But admitting that this converſion is effected by the intimate union of what is called the *principle of heat* with the water, it appears

to

to me to be ſufficiently analogous to other changes, or rather combinations, of ſubſtances. Is not the acid of nitre, and alſo that of vitriol, a thing as unlike to air as water is, their properties being as remarkably different? And yet it is demonſtrable, that the acid of nitre is convertible into the pureſt reſpirable air, and probably by the union of the ſame principle of heat.

By the ſame proceſs by which reſpirable air ſeems to be made from water, inflammable air may certainly be made from liquid ſubſtances containing phlogiſton. Making ſpirit of wine to boil in a glaſs retort, I made the vapour paſs through the ſtem of a hot tobacco pipe, and found that it was all converted into inflammable air, and it was of that kind which burns with a lambent white flame. But when I let the pipe cool, no air was produced, but only vapour, which was inſtantly condenſed in the water.

Being now maſter of a new and eaſy proceſs, I was willing to extend it to other liquid ſubſtances; and I preſently found, as I then imagined, that, by this means, I could give a permanent aërial form to any liquid ſubſtance that had been previouſly thrown into the form of vapour.

When I made the vapour of ſpirit of nitre, heated in a glaſs retort, paſs through the ſtem of the hot tobacco pipe, I got as pure dephlogiſticated air

as

as ever I have procured from nitre; though the cork, by which the retort was connected with the pipe, was diſſolved, and muſt have contributed to contaminate it, and give it a ſlight mixture of fixed air.

With oil of vitriol I got air conſiderably phlogiſticated, ſo that a candle would not have burned in it; but this I alſo attribute to the cork, which was diſſolved in the proceſs. The reſult was nearly the ſame when I uſed water impregnated with vitriolic acid air, though the cork was not diſſolved. But this acid is known to contain much phlogiſton.

When I uſed water impregnated with fixed air, this air was expelled by the heat, and came over without any change that I could perceive, except that the reſiduum was larger, from the water that came along with it. The air I got afterwards was only that from the water, and of the ſame quality as if it had not been impregnated with fixed air.

Water impregnated with alkaline air gave neither fixed nor inflammable air, which I had rather expected, but only air conſiderably phlogiſticated; though ſome of it was ſo pure that a candle would have burned in it.

N. B. In all theſe experiments with the tobacco pipe, all the air was remarkably turbid, like milk, and

and even the common air in the retort before the proceſs properly began.

In this ſtate of the experiments I think I may venture to ſay, that no perſon could have ſeen them without concluding that there was a real converſion of water into air, there being no known principle, or fact, in philoſophy, that could have led any perſon to ſuſpect a fallacy in the caſe. In this, therefore, I muſt have acquieſced, as indeed did all my acquaintance, even thoſe who had been the moſt incredulous on the ſubject, after they had themſelves ſeen the experiments. But I was led to the farther proſecution of this buſineſs, in conſequence of having obſerved that the *purity* of the air which I procured depended upon the ſtate of that which was immediately contiguous to the earthen retort, or tube, in which I ſuppoſed the converſion to have been made; and that ſome communication with the atmoſphere was neceſſary to the production of any air, as in the experiment with the digeſter, and thoſe with the clay and the burning lens. And ſince pure external air was neceſſary in order to procure good air, it was concluded by ſeveral of my friends, and eſpecially Mr. Watt, that the operation of the earthen retort was, to tranſmit phlogiſton from the water contained in the clay to the external air; and that the water, thus dephlogiſticated, was capable of being converted

verted into respirable air by the intimate union of the principle of heat.

In order to ascertain what the influence of the external air in this case really was, I inclosed an earthen retort filled with moistened clay, in a large glass receiver, open at both ends, through the upper orifice of which (being narrow) I thrust the neck of the retort, luting it so as to be perfectly air tight; and placing the receiver in a bason of water, by which the air within was cut off from all communication with the external air, I fitted to the mouth of the retort a glass tube, through which I could receive whatever was produced in the process*. In this situation I heated the retort, by means of Mr. Parker's excellent burning lens, when air was received through the tube communicating with the inside of the retort as usual; but at the same time the water rose within the receiver. This effect might be owing to a phlogistication of the air within the receiver; but it was soon diminished far beyond the utmost limit of that process, so that very little of it remained; and examining this air, I found it to be nearly the same with that of the atmosphere, as also was that which came from the retort.

* Pl. VII. fig. 1. exhibits a view of the disposition of the apparatus in this experiment, and in many others of the same nature.

This experiment made it probable, that the air on the outside of the receiver had actually passed through it; and yet it was contrary to all the known principles of hydrostatics, and even any thing hitherto known in chemistry, that air should be transmitted through a vessel of this kind, and in a direction contrary to that in which it would have been forced by the pressure of the atmosphere; while the water, with which the clay was moistened, went the other way. For had the retort been pervious to air, as the inside had a free communication with the atmosphere, the water could not have risen within the receiver. This, however, appeared to be the case, by the following decisive experiments.

Having filled the earthen retort with the moistened clay as before, I made the inside of the receiver perfectly dry, and placed it in a bason of mercury; when, upon heating the retort as before, the receiver was all covered with dew, which collecting into drops trickled down the inside of the receiver, and remained upon the mercury, which rose within the receiver, while air was received from the retort as usual. I had no doubt, therefore, but that all the water within the retort would have got through into the receiver. Spirit of wine, or something that had the smell of it, was transmitted

mitted from the clay through the retort in the ſame manner.

I then filled the receiver with inflammable air, and upon heating the retort, it was all drawn through it, and delivered as ſtrongly inflammable as ever by the tube communicating with the inſide of it; while the water roſe within the receiver, and even covered the retort, which was fixed at the very top of it, ſo that hardly any of the inflammable air remained within it. In like manner nitrous air paſſed through the retort unchanged.

From theſe experiments it ſeemed impoſſible not to infer, that the clay of the earthen retort, being thus heated, deſtroys for a time the aërial form of whatever air is expoſed to the outſide of it; which aërial form it recovers after it has been tranſmitted in combination from one part of the clay to another, till it has reached the inſide of the retort, while the water is drawn through it in a contrary direction*.

Had this hypotheſis been propoſed *a priori*, it would, I doubt not, have been thought more extraordinary than the converſion of water into air.

* Notwithſtanding this, reaſons will preſently be given, which ſhew that this is no juſt concluſion, and that the proceſs muſt have been carried on by means of *pores*.

The

The great difficulty with respect to the experiment with the lens is, that the water should pass through the retort one way, and the air the other, and yet that the air should not be able to pass without the water. It is also not a little extraordinary, that the weight of the air and that of the water should be so nearly equal.

I cannot conclude this account without acknowledging my obligation to Mr. Parker for the use of his incomparable lens, and his obligingly assisting me in the management of it. Indeed, without this very instrument, or one of greater power than my own, I do not know that the last-mentioned experiments could have been made at all; certainly not to so much satisfaction*.

When I wrote the preceding part of this section, it was my opinion, that the air which was transmitted through the earthen retort, had lost for a moment its *form of air*, and was properly incorporated with the *earth*, as fixed air is in lime stone, and that it had resumed its aërial form in the inside of the retort; because these vessels had been air tight in all my other experiments. But though these earthen retorts be sufficiently air tight for many purposes, and we cannot blow through them

* By means of small earthen vessels, of an inch diameter in the bulb, such as is represented in Pl. VII. fig. 1. the experiment is made with sufficient advantage, with a lens of twelve inches in diameter.

when

when they are plunged in water, yet unleſs they be *glazed*, they are not ſo air tight, but that, with an air pump, air may be drawn through them. Conſequently, any power acting with the force of an air pump (which is only equal to the preſſure of the atmoſphere) may overcome the difficulty of air paſſing through the ſmall pores of ſuch retorts; and there are many powers in chemiſtry much greater than this.

At preſent it is my opinion, that the agent in this caſe is that principle which we call *attraction of coheſion*, or that power by which water is raiſed in capillary tubes. But in what manner it acts in this caſe, I am far from being able to explain. Much leſs can I imagine how *air* ſhould paſs one way, and *vapour* the other, in the ſame pores, and how the tranſmiſſion of the one ſhould be neceſſary to the tranſmiſſion of the other. I am ſatisfied, however, that it is by means ſuch pores as air may be forced through, that this curious proceſs is performed, becauſe the experiment never ſucceeds but in ſuch veſſels as, by the air pump at leaſt, appear to be *porous*, though in all ſuch. But the ſmallneſs of the pores ſeems to be rather an advantage in this experiment; and the ſmalleſt capillary tubes alſo are known to act with the greateſt force.

A veſſel ſcooped out of *chalk* anſwered as well as an earthen retort; ſo alſo did one of *free-ſtone*, firſt

firſt about a quarter of an inch in thickneſs, and then very near an inch, which I got made for the purpoſe; and ſo alſo did a veſſel of *white marble* of the cloſeſt texture, though it is ſufficiently air tight for moſt purpoſes, and nothing but the air pump could have convinced me that it had any pores.

In order to meaſure the force of this attraction, I placed the glaſs veſſel through which I tranſmitted the rays of the ſun (and which contained the air that was to be drawn through the retort) in a baſon of mercury, and found that while the vapour of water was tranſmitted from moiſtened clay in the inſide of the retort, into the inſide of the glaſs veſſel (and conſequently the air paſſed the contrary way, ſo as to enable the atmoſphere to preſs the mercury into the inſide of the glaſs veſſel) it roſe three inches and a half above the level on the outſide of the glaſs. It might probably have riſen ſomething higher, if all the water in the clay had not then been expended. This was a veſſel made on purpoſe, of pipe clay only, and very compact. When I uſed more porous earthen veſſels, I was never able to raiſe the mercury ſo high. For when the pores are large, the mechanical action occaſioned by the weight of ſuch a column of mercury counteracts the chemical one, and prevents the tranſmiſſion of the air or ſteam.

Having ſufficiently aſcertained what kind of *veſſels* would anſwer for this experiment, I then tried what kind of *fluid ſubſtances* would, by their tranſmiſſion in one direction, promote the paſſing of the air in the other; and I found that *oils*, as well as *ſpirit of wine*, anſwered this purpoſe perfectly well; and ſo alſo did *mercury*, but not any other metal made fluid by heat.

The mercury paſſed through the retort in the form of a ſubtle *black powder*; and on this occaſion I obſerved that the veſſel of pipe clay with which the experiment was made, was ſo far air tight, that when I took it to the air pump, nearly full of mercury, quite hot from the experiment in the ſun, and placed it upon the plate of the air pump, I was not able to force any mercury through it. Probably this black powder was much more attenuated than mercury in its natural fluid ſtate can be. There is alſo a degree of repulſion between white mercury and all glaſs or earthen veſſels. This repulſion mechanical preſſure might not be able to overcome, though this chemical action could do it. It remains, however, to be conſidered, how the mercury in this proceſs could be changed from its natural fluid ſtate into this black powder.

While I was making theſe experiments with mercury, I took the opportunity of trying one of

the kinds of air that cannot be confined by water; and having filled the glaſs veſſel with *alkaline* air, and having mercury in the veſſel of clay, I ſoon found, by the ſmell, that the alkaline air was paſſing one way, while the mercury went the other, juſt as readily, I doubt not, as inflammable or nitrous air had gone one way, while water had gone the other.

Of the *metals* in a ſtate of fluidity, I tried *lead*, *tin*, *zinc*, and *biſmuth*, melting them in porous earthen retorts; but neither did any metal paſs one way, or air the other.

I made ſome attempts to perform this experiment with veſſels which had *viſible pores*, eſpecially with fine capillary tubes of glaſs; having the ſtoppers of the glaſs tubes made with fine perforations of this kind, but hitherto without ſucceſs; and yet I do not abſolutely deſpair of ſucceeding in an experiment of this kind, and thereby ſeeing a little farther into this curious proceſs.

SECTION III.

Experiments on Ether.

IT is ſomething extraordinary that, though ether, as I found, cannot be made to aſſume the form of air (the vapour ariſing from it by heat, being ſoon condenſed by cold, even in quickſilver) yet that a very ſmall quantity of ether put to any kind of air, except the acid, and alkaline, which it imbibes, almoſt inſtantly doubles the apparent quantity of it; but upon paſſing this air through water, it is preſently reduced to its original quantity again, with little or no change of quality.

I put about the quantity of half a nut ſhell full of ether, incloſed in a glaſs tube, through a body of quickſilver, into an ounce meaſure of common air, confined by quickſilver; upon which it preſently began to expand, till it occupied the ſpace of two ounce meaſures. It then gradually contracted about one ſixth of an ounce meaſure. Putting more ether to it, it again expanded to two ounce meaſures; but no more addition of ether would make it expand any farther. Withdrawing the quickſilver, and admitting water to this air, without

without any agitation, it began to be abſorbed; but only about half an ounce meaſure had diſappeared after it had ſtood an hour in the water. But by once paſſing it through water the air was reduced to its original dimenſions. Being tried by a mixture of nitrous air, it appeared not to be ſo good as freſh air, though the injury it had received was not conſiderable.

All the phenomena of dilatation and contraction were nearly the ſame, when, inſtead of common air, I uſed nitrous air, fixed air, inflammable air, or any ſpecies of phlogiſticated common air. The quantity of each of theſe kinds of air was nearly doubled while they were kept in quickſilver, but fixed air was not ſo much increaſed as the reſt, and phlogiſticated air leſs. But after paſſing through the water, they appeared not to have been ſenſibly changed by the proceſs.

Theſe experiments were made with vitriolic ether. Having, ſince, procured a quantity of *nitrous ether*, made by Mr. Godfrey, I had the curioſity to try whether this would produce the ſame effect; but I found that it increaſed common air only about one ſixth of its bulk. After this mixture had continued two days and a night, water abſorbed the ether, and left the common air exactly, or very nearly, the ſame as before, judging by the teſt of nitrous air.

SECTION IV.

Of the Difference of the Quantity of Air by the rapid Production of it.

I Have more than once found, that when I expelled air from ſeveral ſubſtances by heat, the produce was different in quantity, according as the heat was *ſlowly* or *ſuddenly applied*; though, at the laſt, it was always made the ſame, and was continued as long as any air could be produced. The firſt obſervations of the kind were thoſe in which *inflammable air* was produced from vegetable and animal ſubſtances. I afterwards made another experiment of the kind in the production of *dephlogiſticated air* from red lead and ſpirit of nitre; uſing every precaution that I could think of to prevent my impoſing upon myſelf, with reſpect to a circumſtance which, I own, appears rather extraordinary. I imagine, however, that when the heat is applied ſlowly, ſome parts of the ſubſtances, that are leſs diſpoſed to volatilize, become, by degrees, *accuſtomed*, as it were, to bear a force, which, if it had been applied ſuddenly, would have inevitably broke

broke their cohesion with the rest; which is in some measure similar to what is observed with respect to the *load stone*, which will sustain more weight when it is applied gradually, than it can be made to do when it is applied all at once.

From equal quantities of the same red lead, without any mixture of spirit of nitre, and using the same gun barrel, I got, by means of heat suddenly applied, more air than I got by heat applied slowly in proportion of *ten* to *six*. The proportion of fixed air was the same in both the cases, and the remainder equally dephlogisticated.

Moistening a quantity of red lead very well with strong spirit of nitre, and putting equal quantities of it at different times into the same gun barrel, in the first process I applied the heat pretty suddenly, and in the second very slowly; and in the first I got three times as much air as in the other. The air was also equally pure in both these cases, and contained equal proportions of fixed air; but towards the end of the slower process the air became nitrous.

From an ounce of red lead, by a sudden and brisk heat I got above two quarts of air, a great part of which was fixed air, and the rest was about twice as good as common air; and immediately after, putting the very same quantity of the same parcel of red lead into the same gun barrel, by heat

very *ſlowly applied*, but urged vehemently at laſt, I got no more than two ounce meaſures of air, a great part of which was fixed air, and the reſt not ſo good as common air.

From an ounce and a half of whiting, in a gun barrel, I got, with a ſudden and great heat, 240 ounce meaſures of air, of which ſome portions contained nine tenths of fixed air, and the reſiduum burned with a lambent blue flame. Repeating the ſame proceſs very ſlowly, I got no more than 150 ounce meaſures of air from the ſame quantity of whiting. And again repeating the proceſs with a greater and more ſudden heat than at firſt, I got, from the ſame quantity of materials, 270 ounce meaſures of air.

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SECTION V.

Experiments on the Mixture of different Kinds of Air, that have no mutual Action.

CONSIDERING the very different ſpecific gravities, and other remarkably different properties of different kinds of air, it might naturally enough be taken for granted, that thoſe which differ very much in *ſpecific gravity* at leaſt, would ſeparate from each other after they were mixed, the heavier occupying the lower place, and the lighter the upper; and that, by this means, the heavier kind of air might be made to expel the lighter, if there ſhould be an opportunity for its eſcape from the upper part of the veſſel. As different kinds of air will often be unavoidably mixed in a variety of experiments, I thought it a matter of ſome conſequence to aſcertain preciſely how the fact was in this reſpect; that I might not, upon any occaſion, deceive myſelf, by imagining that I had one kind of air only, when, in reality, I might have a much greater proportion of ſome other kind than I ſuſpected.

The

The result of my trials has been this general conclusion; that when two kinds of air have been mixed, it is not possible to separate them again, by any method of *decanting*, or pouring them off, though the greatest possible care be taken in doing it. They may not properly *incorporate*, so as to form a *third species of air*, possessed of new properties; but they will remain equally diffused through the mass of each other; and whether it be the upper or the lower part of the air that is taken out of the vessel, without disturbing the rest, it will contain an equal mixture of them both.

After some experiments with common phials, but which I rejected, from the consideration that the agitation of the air within the phial, by the letting in of the water, would necessarily mix the two kinds of air contained in it, though they had been ever so carefully separated, I avoided that inconvenience, by making the experiments in the following more accurate manner, applying it to all the kinds of air that will bear to be confined by water.

Into a cylindrical vessel, which I could open at both ends, having ground stoppers fitted to them, I put equal quantities of *fixed* and *common air*; and after suffering them to continue a whole day, the vessel remaining in a perpendicular position, in which they had opportunity, and time enough, to have separated from each other, I very carefully let

let out the contents of the upper part of the veſſel only, the water entering ſlowly at the lower opening; and put the remainder, which had occupied the lower part of the veſſel, into a ſeparate phial; then examining them one after the other, I found no ſenſible difference between them, equal proportions of both being abſorbed by water. If there was any difference, it did not, however, exceed the trifling one in the next experiment, made with an eight ounce phial, in the bottom of which I had made a hole, by which the water might enter as the air was let out at the mouth of it.

This phial I filled with equal quantities of fixed and common air; and after a conſiderable time, I let off the upper part, while the water entered below; and obſerved that, of the four meaſures which had occupied the upper part of the phial, two and an half remained unabſorbed by water, and of the lower part, two and a quarter; ſo that there was a little more fixed air in the lower than in the upper part of the veſſel.

Having mixed equal quantities of *inflammable* and *nitrous air* in the phial which had a perforation at the bottom, and letting it out in five different parts, I obſerved that they all burned with a lambent flame, without any ſenſible difference between them.

I tried

I tried in the ſame manner a mixture of *nitrous* and *common air*, and after letting them continue together all night, found that the lower part of this mixture diminiſhed common air a little more than the upper part; but the difference was very ſmall.

I put together equal quantities of *nitrous* and *fixed air*, in the perforated phial; and decanting it at ſeveral times, with the ſame care as before, and mixing the ſeveral parts with common air, could obſerve no ſenſible difference in the diminution produced by any of them.

Fixed air is equally diffuſed through the whole maſs of any quantity of air from putrefying ſubſtances with which it is mixed; for dividing the mixture into two equal parts, they were reduced in the ſame proportion by paſſing through water. This is alſo the caſe with inflammable air, and air in which ſulphur has burned.

At the ſame time that I was trying the purity of 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; 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 propor-

tion, by paſſing through the water, ſo that the whole maſs had been of an uniform compoſition.

Having filled the ſtrong glaſs veſſel, in which I uſually fire them by the electric ſpark, with the uſual mixture of one third dephlogiſticated and two thirds inflammable air, I let it remain in a perpendicular poſition all night, in which there was certainly time enough for them to have ſeparated from each other, if their very different ſpecific gravities would have diſpoſed them to do ſo. But though the two wires between which the exploſion is made were at the very top of the veſſel, which in the caſe of a ſeparation would have been the region of pure inflammable air (which cannot take fire of itſelf) yet when the ſpark paſſed between them, the two kinds of air were exploded as completely as they could have been immediately after they had been mixed.

It is alſo obſervable, that the phlogiſticated and dephlogiſticated air which compoſe the atmoſphere, are of very different natures, though without any known principle of attraction between them, and alſo of different ſpecific gravities; and yet they are never ſeparated but by the chemical attraction of ſubſtances, which unite with the one, and leave the other, as in all thoſe proceſſes which I have termed *phlogiſtic*.

The

The ſame is the caſe with common air and alkaline, or any of the acid airs. For though all theſe kinds of air differ in ſpecific gravity from common air, yet if they be mixed with common air, and water be admitted to them, the quantity will decreaſe more or leſs ſlowly in proportion to the quantity of common air in the mixture. Whereas, if the alkaline or acid airs had been heavier than the common air (as the latter, at leaſt, manifeſtly are) and did not mix with it, the water would abſorb them as readily as it does when the jar contains no other kind of air; as, on the other hand, if the common air had been the heavier, it would have protected them from the acceſs of the water, which would not, in this caſe, be able to come at the acid or alkaline air, and therefore could not abſorb any part of the quantity. I have noted, however, one exception to this rule reſpecting alkaline and inflammable air, which did not ſeem to mix together.

I have ſince made a mixture of vitriolic acid air and fluor acid air, and find that they continue intermixed throughout. I mixed equal quantities of them in a jar of quickſilver, and obſerved, that when water was admitted to the whole maſs, the cruſt was formed equably from the bottom to the top of the veſſel.

Notwithſtanding

Notwithſtanding theſe experiments, I do not ſay but that if two kinds of air, of very different ſpecific gravities, were put into the ſame veſſel, with very great care, without the leaſt agitation that might mix or blend them together, they might continue ſeparate, as with the ſame care *wine* and *water* may be made to do; but that, when once they have been mixed, they will continue to be ſo, like wine and water, after having been ſhaken together.

From the experiments above recited, it will follow, that when fixed air, or air of any other kind, is conveyed into a veſſel containing common air, a quantity equal to the contents of the whole veſſel muſt be thrown in, before half of the common air be expelled; becauſe half of the air that is introduced will always be expelled along with it; that after another equal quantity, ſtill one fourth of the common air will remain; and after another, one eighth, &c.

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SECTION VI.

Of the Expanſion of different Kinds of Air by Heat.

FROM a very coarſe experiment, which I made very early, I concluded that fixed and common air were expanded alike with the ſame degree of heat. But by the method which I then made uſe of, it was not poſſible to diſcover ſmall differences in this reſpect. I have ſince attempted it in another and more accurate manner, and have applied it to *all the kinds of air* with which I am acquainted; and though I do not think that the reſults can be intirely depended on, on account of the difficulty that I found in making the experiments, and my not having time to repeat them ſo often as I could have wiſhed, it may not be amiſs to give an account of what I *did* obſerve, though it ſhould only be a means of putting others upon doing the ſame thing in ſome better manner, and with more attention.

I took a phial, containing thirteen ounce meaſures of water, and filled it ſucceſſively with all the different kinds of air, having been previouſly made very

very dry, and filled with quickſilver. Then holding the mouth of it under the quickſilver, I inſerted into it a perforated cork, to which was annexed a long glaſs tube, as is repreſented, Pl.V. fig. 5, leaving a little quickſilver in the neck of the phial, that the expanſion of the air might drive it along the tube. This phial, ſo prepared, I placed in a ſmall wooden box, and carrying it into a warm room, carefully marked the place at which the quickſilver ſtood after it had become perfectly ſtationary in that degree of heat. Then carrying it into a colder room, and letting it remain there a ſufficient time, I marked the place in which it ſtood in that degree of heat, having a Fahrenheit's thermometer accompanying it in each ſituation. I then carried it back into the warm room, and obſerved whether the quickſilver roſe to the ſame place as before; and after uſing other little precautions, to prevent any miſtake, I found the following intervals in the place of the quickſilver in the different kinds of air, the ſame phial and the ſame glaſs tube being uſed in all the caſes. The quantity anſwering to ten degrees was made by computation, to ſhew the proportional expanſion at one eaſy view.

	The diſtance between		inches	10°. being
In Common air	34° and	48° was	1.85	1.32
—Inflammable	34	48	2.87	2.05
—Nitrous	33	50	3.44	2.02
—Fixed	32	50	3.97	2.20
—Marine acid	32	50	2.40	1.33
—Dephlogiſticated	32	51	4.2	2.21
—Phlogiſticated	32	51	3.15	1.65
—Vitriolic acid	32	48	3.70	2.37
—Fluor acid	46	56	2.83	2.83
—Alkaline	32	50	8.56	4.75

The expanſion of the *alkaline* air in this experiment will appear extraordinary; but I muſt obſerve that there was a little *moiſture* in the phial, which had been expelled from the materials together with air; and to this water, emitting more air by heat, I am inclined to aſcribe the much greater dilatation of this than of the other kinds of air; and at that time I could not conveniently repeat the experiment, not having any better materials at hand.

That *inflammable*, *nitrous*, and *phlogiſticated* air, containing more phlogiſton than common air, ſhould expand more with equal degrees of heat, is agreeable to analogy, the ſame thing being obſerved in

in fluids: but how it ſhould happen that *dephlogiſticated* air, which contains leſs phlogiſton, ſhould expand more than common air, I do not ſee, and therefore I wiſh that the experiments might be repeated in ſome better manner.

SECTION VII.

Of the ſpecific Gravity of different Kinds of Air.

IN order to aſcertain the ſpecific gravity of dephlogiſticated, and other kinds of air, I took a glaſs tube about nine inches long, and faſtening it to the neck of a bladder, which, with ſuch a degree of diſtenſion as I could give it, in the manner in which the experiment was made, contained fifty five ounce meaſures, or one pennyweight nine grains of common air. The tube was ſo faſtened, that I could take it out at pleaſure; and having the bladder thus prepared, I carefully compreſſed it, then filling it in part with that kind of air which I was

about to weigh, I compreſſed it again, and then filled it intirely; ſo that I was pretty confident that the air within the bladder contained very little common, or any other kind of air. In this manner I proceeded to weigh *dephlogiſticated air*, and at the ſame time *nitrous air*, and *air phlogiſticated with iron filings and ſulphur*, which I take for granted is the ſame thing with air phlogiſticated by any other proceſs.

The following ſhort table exhibits the reſult of all theſe experiments at one view.

The bladder, filled with	dwts.	gr.
phlogiſticated air, weighed - - - -	7	15
——————— nitrous air - -	7	16
——————— common air - -	7	17
——————— dephlogiſticated air -	7	19

This reſult agrees ſufficiently well with my former obſervations, though they were not made with ſo much accuracy, viz. that both nitrous air, and air diminiſhed by phlogiſtic proceſſes, are rather lighter than common air; and it is conſonant to this, that, in the preſent experiment, dephlogiſticated air appears to be a little heavier than common air.

In this experiment, the dephlogisticated air was so pure, that one measure of it, and two of nitrous air, occupied the space of four fifths of a measure. Had the air been more pure, it would, no doubt, have been specifically heavier still.

SECTION VIII.

Of Sound in different Kinds of Air.

ALMOST all the experiments that have hitherto been made relating to *sound*, have been made in common air, of which it is known to be a vibration, though it is likewise known to be capable of being transmitted by other substances. There could be little doubt, however, of the possibility of sound *originating* in any other kind of air, as well as of being *transmitted* by them; but the trial had not been actually made, and I had an easy opportunity of making it.

Besides, the experiments promised to ascertain whether the *intensity* of sound was affected by any

other property of the air in which it was made than the mere *denſity* of it. For the different kinds of air in which I was able to make the ſame ſound, beſides differing in ſpecific gravity, have likewiſe other remarkable chemical differences, the influence of which with reſpect to ſound would, at the ſame time, be ſubmitted to examination.

Being provided with a piece of clock work, in which was a bell, and a hammer to ſtrike upon it (which I could cover with a receiver, and which, when it was properly covered up, I could ſet in motion by the preſſure of a braſs rod, going through a collar of leathers) I placed it on ſome ſoft paper on a transfer. Then taking a receiver, the top of which was cloſed with a plate of braſs, through which the braſs rod and collar of leathers was inſerted, I placed the whole on the plate of an air pump, and exhauſted the receiver of all the air that it contained. Then removing this exhauſted receiver, containing the piece of clock work, I filled it with ſome of thoſe kinds of air that are capable of being confined by water, by means of a bent glaſs tube inſerted into a piece of braſs, which I could ſcrew into the bottom of the transfer, ſo as to introduce the bended tube, through the water of my trough, into a jar containing the air on which I wiſhed to make the experiment.

When

When this was done, I removed the glaſs tube, and had the receiver filled with that ſpecies of air in which I wiſhed to produce the ſound, and the apparatus for making the ſound within it. Then by forcing down the braſs rod through the collar of leathers, I made the hammer ſtrike the bell, which it would do more than a dozen times after each preſſure. And the inſtrument was contrived to do the ſame thing many times ſucceſſively, after being once wound up.

Every thing being thus prepared, I had nothing to do, after filling the ſame receiver with each of the kinds of air in its turn, but receding from the apparatus, while an aſſiſtant produced the ſound, to obſerve at what diſtance I could diſtinctly hear it. The reſult of all my obſervations, as far as I could judge, was that the intenſity of ſound depends ſolely upon the *denſity* of the air in which it is made, and not at all upon any chemical principle in its conſtitution.

In inflammable air the ſound of the bell was hardly to be diſtinguiſhed from the ſame in a pretty good vacuum; and this air is ten times rarer than common air.

In fixed air the ſound was much louder than in common air, ſo as to be heard about half as far

again; and this air is in about the ſame proportion denſer than common air.

In dephlogiſticated air the ſound was alſo ſenſibly louder than in common air, and as I thought rather more than in the proportion of its ſuperior denſity; but of this I cannot pretend to be quite ſure.

In all theſe experiments the common ſtandard was the ſound of the ſame bell in the ſame receiver, every other circumſtance alſo being the ſame; the air only been changed, by removing the receiver from the transfer, and blowing through it, &c.

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SECTION IX.

Of the Power of the different Kinds of Air to conduct Heat.

ONE of the firſt things that I propoſed to try, with reſpect to the different kinds of air, was to obſerve their *power of conducting heat*, and I had a contrivance for that purpoſe when I was at Leeds. The thing, however, appearing to me to be of leſs conſequence than other things that I had in view, I deferred it ſome time, till, in conſequence of the doctrine of heat becoming, by means of Dr. Crawford's book, the ſubject of general converſation, I determined to execute what I had ſo long projected.

For this purpoſe I prepared the veſſel repreſented Pl. VI. fig. 2, and deſcribed in the *Introduction*. The thermometer was a very ſenſible one, and the ſcale large, ſo that I could mark upon it twenty diviſions, each larger than half an inch between the mean temperature of the atmoſphere, and a heat much below that of boiling water. After ſeveral trials I at length adjuſted it in ſuch a manner, that, having

having filled the veſſel with any kind of air, I could plunge it to a certain depth, firſt in *hot*, and then in *cold* water, ſo that the mercury would riſe to the diviſion 20, and fall to that of 6 or 7 in a reaſonable time. I had a clock that beat ſeconds cloſe by me, and was ſo ſituated, that I could not well make a miſtake of more than two ſeconds, in noting the time when the mercury came to any particular diviſion. The precautions I uſed to plunge the veſſel the *ſame depth* in the water, in all the experiments, and to exclude all other differences, except what might be occaſioned by the different kinds of air, it would be tedious to recount; and no perſon converſant in experiments, and who is diſpoſed to repeat them, will need to be ſo minutely inſtructed.

I will juſt obſerve, however, that in the veſſel I uſed for *hot* water, I always made it boil, and it was ſo full, that the plunging of my air veſſel into it made it run over; and the veſſel for cold water was always freſh brought from the ſame pump. The mouth of the air veſſel was in a cup of mercury, always filled to the ſame height; and by this means I could try in the ſame manner even thoſe kinds of air, that could not be confined by water.

The beſt account that I could give of the reſult of theſe experiments would be to exhibit them in the

the form of *tables*, of the time at which the mercury reached all the degrees of the ſcale, both in aſcending and deſcending; which tables I have drawn up. But this I ſhall defer till I have an opportunity of repeating all the obſervations. At preſent I would only obſerve, that all the differences were not ſo ſtriking as I expected to have found them, but that *inflammable air* conducted heat much better than any other kind of air, the mercury aſcending the ſame ſpace in about half the time that it took up in common air. *Fixed air*, and all the kinds of *acid air*, conducted heat conſiderably worſe than common air. *Alkaline air* conducted heat rather better than the acid airs, and dephlogiſticated air a little worſe than common air, but ſo little, that I would not anſwer for the ſame reſult in repeating the experiment.

N. B. In the courſe of theſe experiments, I could not avoid obſerving ſo great an expanſion of *alkaline* air by heat, that I conclude the obſervation, p. 450, may be perfectly accurate, though the extraordinary nature of it made me entertain the doubt I have there expreſſed concerning it.

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SECTION X.

Of the refractive Power of different Kinds of Air.

CONSIDERING the very different properties of the different kinds of air with which I have been conversant, it was impossible not to think of the probability of their having different *refractive powers*, and of some method of ascertaining this circumstance. But I am sorry to inform my readers, that my experiments have been without any success.

For this purpose, I procured a prism, consisting of three plates of glass, fastened together by cement, the cavity being large enough to contain about a quarter of a pint. This prism I fixed upon a stand, at the distance of ten feet from a window, in which I had a small apparatus, contrived to throw a beam of the sun's rays into the room. This beam was received by a board, furnished with a piece of brass work, containing several small holes, through any of which I could transmit a beam of light upon the prism, which was placed, in a vertical position, close behind it; and the wall on which the

the image of the ſun was received was twenty feet from the priſm.

With this apparatus, which I thought promiſing enough, I proceeded to try the refractive powers of nitrous and inflammable air; but I could perceive no difference in the place of the image, whether the beam of light was tranſmitted through the priſm, carefully filled with either of theſe kinds of air, or not through it; allowance being made for a ſmall degree of refraction occaſioned by a want of perfect parallelifm in the plates of the priſm. The reſult was the very ſame, whether it contained common air, or either of the two kinds above-mentioned.

Having had ſo little ſucceſs with theſe two very different kinds of air, I thought it would be in vain to try any of the other kinds; and therefore, for the preſent, have deſiſted from my purſuit; but I am not without a deſign to reſume it with a different kind of apparatus, if I be ſo happy as to ſucceed in the conſtruction of it.

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SECTION XI.

Of Air in the Bladders of Fishes.

I Made a beginning of a course of experiments, which, I think, may be pursued to considerable advantage, on the state of the air which is contained in the *bladders of fishes*. It is commonly supposed, that these bladders are of no other use to the fishes than to assist them in rising or sinking in the water: but I have some doubt about this hypothesis; at least they may have some other use. Some fishes, I believe, are not furnished with these bladders. When they are taken out of the fish, the air cannot be got from them by pressure, but I was always obliged to burst or cut them; and yet that the air does change in these bladders, is, I think, pretty evident, from my having found it in different states.

The first time that it occurred to me to examine the air contained in these bladders, I found it, in a great number of them, to be perfectly noxious, not being at all affected by nitrous air. This was on the 31st of May, 1774. But at another time, viz. the

the 30th of March following, I found air that I had preſſed from the bladders of the ſame kind of fiſhes, viz. roaches, not to be quite noxious, being affected by nitrous air, though not to a great degree. I have not purſued theſe experiments any farther; but I ſhould think that it might not be difficult, by diverſifying them properly to make ſome diſcoveries concerning the animal œconomy of fiſhes, and the uſe of air to them.

SECTION XII.

Of Changes produced in various Kinds of Air by Expoſure to Urine.

THAT phlogiſton exhaling from vegetable or animal ſubſtances ſhould ſenſibly affect common air, or dephlogiſticated air, which contain little or no phlogiſton, and have a ſtrong affinity with it, is far from being extraordinary; but that the ſame ſubſtances which phlogiſticate common air, or dephlogiſticated air, ſhould likewiſe affect nitrous air, or inflammable air, which already contain

tain phlogiſton (and, as it ſhould ſeem, to a complete ſaturation) is a fact that I cannot well explain. This, however, I have obſerved to be the caſe with liver of ſulphur, iron filings, and ſulphur, and various other ſubſtances on nitrous air.

Inflammable air I have not obſerved to have any impreſſion made upon it by theſe ſubſtances, any more than by the electric ſpark, at leaſt in the temperature of the atmoſphere; though, in conſequence of ſimple confinement by water, it has at length, in ſeveral inſtances, loſt its inflammability, and, like nitrous air in the preceding circumſtances, has become mere phlogiſticated air. I have ſome ſuſpicion, however, that inflammable air may be decompoſed by all the ſame ſubſtances that decompoſe nitrous air, if more *heat*, or more *time*, be given to the proceſs; and perhaps what I conſidered as *pure water* might, in time at leaſt, have got an impregnation of ſomething that might affect the inflammable air, in thoſe caſes in which I found it reduced to the ſtate of phlogiſticated air. Theſe ſuſpicions I have been led to in conſequence of obſerving that *urine* had this effect, both on nitrous and inflammable air; an obſervation which I made accidentally, in the courſe of expoſing a great variety of ſubſtances to the ſun during the ſummer of 1779.

Among

Among other things, I had filled a glaſs tube, about half an inch in diameter, and three feet and a half long, with urine, and had placed it inverted in a baſon of the ſame. In this ſituation it was kept ſeveral months, when it yielded at firſt a ſmall quantity of air, all of which was afterwards abſorbed. After which I perceived cryſtals to be formed in ſeveral places of the inſide of the tube, and the urine, from being of a pretty high colour, became very pale.

Seeing no farther change in the urine, and having obſerved its power of emitting and abſorbing air, I expoſed to its influence all the kinds of air that could be confined by it, in ſeparate ſix ounce phials, of which the air of each kind occupied about one fourth, the remainder of the phial containing this old pale urine; and the phials were inverted in baſons of the ſame, and, as it evaporated, were ſupplied from time to time with more urine.

Things were diſpoſed in this manner on the 27th of July, and I obſerved that there was no immediate change, either in the inflammable or the phlogiſticated air; but the ſurface of the urine in contact with the common air, dephlogiſticated air, and nitrous air, from a pale ſtraw colour, preſently became of a deep brown, and eſpecially next the dephlogiſticated air. The next morning the colour of the urine in contact with the dephlogiſti-

cated air was almoſt black, and extended through the whole phial. But in the phial in which the common air was confined, the brown colour extended only a little way within the body of the urine. Under the nitrous air the urine was pretty uniformly brown, but not ſo much ſo as under the dephlogiſticated air. A little both of the dephlogiſticated and nitrous air was abſorbed, and nearly an equal quantity of each.

The dephlogiſticated air having diminiſhed very faſt, and becoming thoroughly phlogiſticated, I introduced more of the ſame air into the phial; and I let all the phials ſtand in this ſituation till the 22d of July following, when I was obliged to put an end to the proceſs, and I then noted the following appearances. The common air was diminiſhed about one fourth, and was thoroughly phlogiſticated; the urine being of an orange colour, but not very deep. The dephlogiſticated air, having been renewed, was not completely phlogiſticated, but was nearly ſo. The nitrous air was diminiſhed one half, had changed the urine black, extinguiſhed a candle, and did not affect common air at all, ſo that it was mere phlogiſticated air. But what is remarkable, this phlogiſticated air was in a much greater proportion than is generally procured from nitrous air. This effect I aſcribe to the *length of time* that the proceſs took up, and there are other

other remarkable facts in confirmation of this opinion.

The inflammable air was diminiſhed to about one eighth of its bulk, and was ſtill ſlightly inflammable. With more time I doubt not this air would have loſt all its inflammability. The urine in this phial was of a very pale colour.

The phlogiſticated air alone remained quite unaffected during the whole proceſs, and the urine was of the ſame colour with that under the common air, viz. a light orange; but this change probably came from that part of the urine, which had been expoſed to the common air in the cup, and had gradually extended itſelf to the urine in the phial.

If the diminution of all theſe kinds of air was owing to phlogiſton, it may be inferred that this principle in the phlogiſticated air has a firmer union with its baſe than it has in nitrous or inflammable air, being leſs capable of either receiving more, or of parting with what it has got.

But perhaps the moſt puzzling circumſtance in this proceſs is, that the diminution of both the dephlogiſticated and nitrous air ſhould be accompanied with the ſame change of colour in the urine expoſed to them. A ſimilar change of colour in a ſolution of copperas, I thought was owing to the phlogiſton depoſited from the decompoſed nitrous

air. But if this was the cauſe of the ſimilar change of colour in this caſe, how came the ſame change to take place in conſequence of the diminution of dephlogiſticated air, this diminution being, no doubt, owing to its receiving phlogiſton from the urine? It can hardly be that the ſame change ſhould take place in the colour of the urine, whether it contains more or leſs phlogiſton than it naturally has. Perhaps it may be ſomething common to the conſtitution both of nitrous and dephlogiſticated air, and not phlogiſton, that, when they are decompoſed, is precipitated, and produces this change of colour in the urine.

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SECTION XIII.

Of Air in the Calces of Metals.

THAT the calces of metals contain air, of ſome kind or other, and that this air contributes to the additional weight of the calces, above that of the metals from which they are made, had been obſerved by Dr. Hales; and Mr. Hartley had informed me, that when red lead is boiled in linſeed oil, there is a prodigious diſcharge of air before they incorporate. I had likewiſe found, that no weight is either gained or loſt by the heating of tin in a cloſe glaſs veſſel; but I purpoſely deferred making any more experiments on the ſubject, till we ſhould have ſome weather in which I could make uſe of a large burning lens, which I had provided for that and other purpoſes; but, in the mean time, I was led to the diſcovery in a different manner.

Having, by ſome of my early experiments, been led to conſider the electric matter as phlogiſton, or ſomething containing phlogiſton, I was endeavouring to revive the calx of lead with it; when I was ſurpriſed to perceive a conſiderable generation of

air. It occurred to me, that poſſibly this effect might ariſe from the *heat* communicated to the red lead by the electric ſparks, and therefore I immediately filled a ſmall phial with the red lead, and heating it with a candle, I preſently expelled from it a quantity of air about four or five times the bulk of the lead, the air being received in a veſſel of quickſilver. How much more air it would have yielded, I did not try.

Along with the air, a ſmall quantity of *water* was likewiſe thrown out; and it immediately occurred to me, that this water and air together muſt certainly be the cauſe of the addition of weight in the calx. It ſtill remained to examine what kind of air this was; but admitting water to it, I found that it was imbibed by it, exactly like *fixed air*, which I therefore immediately concluded it muſt be.

After this, I found that Mr. Lavoiſier had more completely diſcovered the ſame thing.

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SECTION XIV.

Of Air ſuppoſed to be contained in Mercury.

WHEN quickſilver is boiled in a glaſs tube, a conſiderable quantity of air is ſeemingly diſcharged from it, and it has been imagined by ſome that this air had been contained not only in the ſpace between the quickſilver and the glaſs, but within the pores of the quickſilver itſelf. If this had been the caſe, it muſt have been the property of quickſilver, as of other fluids, to *imbibe air*, and therefore probably to produce a change of quality in the air which it had imbibed, or in that which it left unimbibed. In order to aſcertain this, I boiled as much quickſilver as one of the largeſt of my phials with ground ſtoppers and tubes would contain, taking care to exclude all the air that I could get from it by the common methods before this operation; and having got a ſmall quantity, I found it to be common air only, applying to it the teſt of nitrous air. It was probable, therefore, that this air had not been expelled from the body of the quickſilver itſelf, but only from between the quickſilver and the glaſs.

After

After this, in order to try whether quickſilver, deprived of all air, had the power of imbibing air, I boiled a pretty large quantity of quickſilver, for near half an hour, and while it was very warm, I put it into a jar, and inverting it in a baſon of quickſilver, left a quantity of common air on the ſurface of it. This air was contracted with cold, but was not ſenſibly affected in any other manner. For after two days, when I perceived that the diminution had proceeded no farther, I examined it, and found it to be diminiſhed by nitrous air as much as any common air whatever.

END OF THE SECOND VOLUME.