XV. *Experiments and Obſervations relating to Air and Water. By the Rev.* Joſeph Prieſtley, *LL.D. F.R.S.*

Read February 24, 1785.

EVER ſince the diſcovery of the diminution of reſpirable air in thoſe proceſſes which are generally called *phlogiſtic*, it has been a great object with philoſophers to find what becomes of the air which diſappears in them. Among others, I have made and publiſhed a variety of experiments with that view; but though by this means ſome farther progreſs was made in the philoſophy of air, and conſequently our knowledge of the principles, or conſtituent parts, of natural ſubſtances was extended, I did not by any means ſucceed to my ſatisfaction with reſpect to the immediate object of my reſearches. Others, however, were more ſucceſsful, and their ſucceſs has at length enabled me to reſume my experiments with more advantage; by which means I have been led both to confirm their concluſions, and, by diverſifying the experiments, to throw conſiderable light upon various other chemical proceſſes. The reſult of theſe obſervations I ſhall lay before the Society, with as much brevity and diſtinctneſs as I can.

In the experiments of which I ſhall now give an account, I was principally guided by a view to the opinions which have lately been advanced by Mr. CAVENDISH, Mr. WATT, and M. LAVOISIER. Mr. CAVENDISH was of opinion, that when *air* is decompoſed, *water* only is produced; and Mr. WATT concluded

concluded from ſome experiments, of which I gave an account to the Society, and alſo from ſome obſervations of his own, that water conſiſts of dephlogiſticated and inflammable air, in which Mr. CAVENDISH and M. LAVOISIER concur with him; but Mr. LAVOISIER is well known to maintain, that there is no ſuch thing as what has been called *phlogiſton*, affirming inflammable air to be nothing elſe but one of the elements or conſtituent parts of water. In the following experiments I alſo had a particular view to a concluſion which I had drawn from thoſe experiments, of which an account is given in my laſt communications to the Royal Society; *viz.* that inflammable air is pure phlogiſton in the form of air, at leaſt with the element of *heat*; and that fixed air conſiſts of dephlogiſticated and inflammable air; both which doctrines had been firſt advanced by Mr. KIRWAN, before I had made the experiments which I then thought clearly proved them.

Such were the hypotheſes to which I had a view when I began the following courſe of experiments, which I hope will be an admonition to myſelf, as well as to others, to adhere as rigorouſly as poſſible to *actual obſervations*, and to be extremely careful not to overlook any circumſtance that may poſſibly contribute to any particular reſult. I ſhall have occaſion to notice my own miſtakes with reſpect to *concluſions*, though all the *facts* were ſtrictly as I have repreſented them. But whilſt philoſophers are faithful narrators of what they obſerve, no perſon can juſtly complain of being miſled by them; for to *reaſon* from the facts with which they are ſupplied is no more the province of the perſon who diſcovers them, than of him to whom they are diſcovered.

One of the moſt ſimple of all phlogiſtic proceſſes is that in which metals are ignited in dephlogiſticated air. I therefore began with this, with a view to aſcertain whether any *water* is produced when the air is made to diſappear in it. Accordingly, into a glaſs veſſel containing 7 ounce meaſures of pretty pure dephlogiſticated air, I introduced a quantity of iron turnings (which is iron in ſmall thin pieces, exceedingly convenient for theſe and many other experiments) having previouſly made them, together with the veſſel, the air, and the mercury by which it was confined, as dry as I poſſibly could. Alſo, to prevent the air from imbibing any moiſture, I received it immediately in the veſſel in which the experiment was made, from the proceſs of procuring it from red precipitate; ſo that it had never been in contact with any water.

I then fired the iron, by means of a burning lens, and preſently reduced the 7 ounce meaſures of air to .65; but I found no more water after this proceſs than I imagined it had not been poſſible for me to exclude, as it bore no proportion to the air which had diſappeared. Examining the reſiduum of the air, I found one-fifth of it to be fixed air, and when I tried the purity of that which remained by the teſt of nitrous air, it did not appear that any phlogiſticated air had been produced in the proceſs: for though it was more impure than I ſuppoſe the air with which I began the experiment muſt have been, it was not more ſo than the phlogiſticated air of the 7 ounce meaſures, which had not been affected by the proceſs, and which muſt have been contained in the reſiduum, would neceſſarily make it. In this caſe one meaſure of this reſiduum and two of nitrous air occupied the ſpace of .32.

In another experiment of this kind, ten ounce meaſures of dephlogiſticated air were reduced to .8, and by waſhing in lime water to .38. In another experiment, in which $7\frac{1}{2}$ ounce meaſures of dephlogiſticated air were reduced to half an ounce

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meaſure, of which one-fifth was fixed air, the reſiduum was quite as pure as the air with which I began the experiment, the teſt with nitrous air, in the proportions above-mentioned, giving .4 in both caſes. To what circumſtance the difference might be owing I cannot tell.

In theſe experiments the fixed air muſt, I preſume, have been formed by the union of the phlogiſton from the iron and the dephlogiſticated air in which it was ignited; but the quantity of it was very ſmall in proportion to the air which had diſappeared, and at that time I had no ſuſpicion that the iron, which had been melted, and gathered into round balls, could have imbibed it; a melting heat having been ſufficient, as I had imagined, to expel every thing that was capable of aſſuming the form of air from any ſubſtance whatever. I was therefore intirely at a loſs about what muſt have become of the air.

Senſible, however, that ſuch a quantity of air muſt have been imbibed by *ſomething* to which it muſt have given a very perceivable addition of weight, and ſeeing nothing elſe that could have imbibed it, it occurred to me to weigh the calx into which the iron had been reduced; and I preſently found, that the dephlogiſticated air had actually been imbibed by the melted iron, in the ſame manner as inflammable air, in my former experiments, had been imbibed by the melted calces of metals, however impoſſible ſuch an abſorption might have appeared to me *a priori*. In the firſt inſtance, about twelve ounce meaſures of dephlogiſticated air had diſappeared, and the iron had gained ſix grains in weight. Repeating the experiment very frequently, I always found, that other quantities of iron, treated in the ſame manner, gained ſimilar additions of weight, which was always very nearly that of the air which had diſappeared.

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This calx of iron, I then concluded, was by no means what I had before taken it to be, *viz.* a *pure calx* or *ſlag*, but either the calx, or the iron itſelf, ſaturated with pure air. This calciform ſubſtance I found, by various experiments, to be the ſame thing with the *ſcales* that fly from iron when it is made red-hot, or the ſubſtance into which it runs in a very intenſe heat, in an open fire.

Concluding from the preceding experiment, that iron, ſufficiently heated, was capable of ſaturating itſelf with pure air, extracted from the maſs of the atmoſphere, I then proceeded to melt it with the heat of a burning lens in the open air; and I preſently found, that perfect iron was eaſily fuſed in this way, and continued in this fuſion a certain time, exhibiting the appearance of *boiling* or *throwing out* air, whereas it was on the contrary *imbibing* air; and when it was ſaturated the fuſion ceaſed, and the heat of my lens could not make any farther impreſſion upon it. When this was the caſe, I always found that it had gained weight in the proportion of 7½ to 24, which is very nearly *one-third* of its original weight. The ſame was the effect when I melted *ſteel* in the ſame circumſtances, and alſo every kind of iron on which the experiment could be tried. But I have ſome reaſon to think, that with a greater degree of heat than I could apply, the iron might have been kept in a ſtate of fuſion ſomewhat longer, and by that means have imbibed more air, even more than one-third of its original weight.

There was a peculiar circumſtance attending the melting of *caſt iron* with a burning lens, which made it impoſſible to aſcertain the addition that was made to its weight, and at the ſame time afforded an amazing ſpectacle; for the moment that any quantity of it was melted, and gathered into a round ball, it began to diſperſe in a thouſand directions, exhibiting the appearance of a moſt beautiful fire-work, ſome of the particles flying to the diſtance of half a yard from the place of fuſion; and the whole was attended with a conſiderable hiſſing noiſe. Some of the largeſt pieces which had been diſperſed in this manner I was able to collect, and having ſubjected them to the heat of the lens, they exhibited the ſame appearance as the larger maſs from which they had been ſcattered.

When I melted this caſt iron in the bottom of a deep glaſs receiver, in order to collect all the particles that were diſperſed, they firmly adhered to the glaſs, melting it ſuperficially, though without making it crack, ſo that it was ſtill impoſſible to collect and weigh the particles. However, I generally found that, notwithſtanding the copious diſperſion, what remained after the experiment rather exceeded than fell ſhort of the original weight of the iron. Sometimes a piece of common iron, and eſpecially ſteel, would make a little hiſſing in the fuſion, and a particle or two would fly off; but this was never conſiderable *.

Having now procured what I thought to be a new calx of iron, or a calx ſaturated with pure air, I endeavoured to revive it by making it imbibe inflammable air, in the ſame manner that I had before made iron, and various other metals, by melting them in a veſſel containing inflammable air. In this I ſucceeded; but in the courſe of the experiment a new and very unexpected appearance occurred. I took a piece of iron which I had ſaturated with pure air, and putting it into a glaſs veſſel

* On being informed of the above-mentioned phænomena, Mr. WATT concluded, that the baſis of the dephlogiſticated air united to the phlogiſton of the iron, and formed *water*, which was attracted by, and remained ſo firmly united to the calx of iron, as to reſiſt the effects of heat to ſeparate them.

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containing inflammable air, confined by water, threw upon it the focus of the lens, and presently perceived the inflammable air to disappear, and without thinking of any thing escaping from the calx of iron (which had been subjected to a greater heat before) I imagined that I should have found the addition of the weight of air in the iron, and the result might be an iron different from the common sort. But I found, to my surprise, that the iron, which had exhibited no new appearance in this mode of treatment, had lost weight, instead of gaining any. The piece of iron on which I made this first experiment weighed 11½ grains, and 7½ ounce measures of inflammable air had disappeared while the iron had lost 2½ grains.

Considering the quantity of inflammable air that had disappeared, *viz.* 7½ ounce measures, and the dephlogisticated air which had been expelled from the iron, *viz.* 2½ grains, which is equal to about 4.1 ounce measures, I found that they were very nearly in the proper proportion to saturate each other, when decomposed by the electrical spark, *viz.* two measures of inflammable air to one of dephlogisticated air. I therefore had now no doubt but that the two kinds of air had united, and had formed either *fixed air* or *water*; but which it was I could not tell, having had water on the receiver in which the experiment was made, and having neglected to examine the state of the air that remained, except in a general way, by which I found, that it was still, to appearance, as inflammable as ever.

With a view to determine whether *fixed air*, or *water*, would be the produce of this mode of combining inflammable and dephlogisticated air, I repeated the experiment in a vessel in which the inflammable air was confined by mercury, and both the vessel and the mercury had been previously made as dry as possible. I had no sooner begun to heat the iron, or rather *slag*,

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in theſe circumſtances, than I perceived the air to diminiſh, and at the ſame time the inſide of the veſſel to grow very cloudy, with particles of dew, that covered almoſt the whole of it. Theſe particles by degrees gathered into drops, and ran down the ſides of the veſſel in all places, except where it was heated by the ſun-beams; ſo that it then appeared to me very evident, that *water*, with or without fixed air, was the produce of the inflammable air, and the pure air let looſe from the iron in this mode of operation; though afterwards I was taught by Mr. WATT to correct this hypotheſis, and to account for this reſult in a different manner. When I had examined the remaining air, it was as inflammable as ever, without containing any mixture of fixed air at all.

When I collected the water which was produced in this experiment by means of a piece of filtering paper, carefully introduced to abſorb it, I found it to be, as nearly as poſſible, of the ſame weight with that which had been loſt by the iron: and alſo, in every experiment of this kind, in which I attended to this circumſtance, I found that the quantity of inflammable air which had diſappeared was about double to that of the dephlogiſticated air ſet looſe from the iron, ſuppoſing that weight to have been reduced into air. Thus at one time I made a piece of this ſlag imbibe 5½ ounce meaſures of inflammable air, while it loſt as much as the weight of about 3 ounce meaſures of dephlogiſticated air, and the water collected weighed 2 grains. Another time a piece of ſlag loſt 1.5 grains, and the water produced was 1.7 grains; but perfect accuracy is not to be expected. I ſhall only mention one more experiment of this kind, in which 6½ ounce meaſures of inflammable air were reduced to .92 ounce meaſure, and the iron had loſt 2 grains, equal in weight to 3.3 ounce meaſures of dephlogiſticated air.

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In all the above-mentioned experiments, the inflammable air was that which is produced by the ſolution of iron in acids.

As before I had finiſhed this courſe of experiments I had ſatisfied myſelf that inflammable air always contains a portion of water, and alſo, that when it has been ſome time confined by water, it imbibes more, ſo as to be increaſed in its ſpecific gravity by that means, I repeated the experiment with inflammable air which had not been confined by water, but which was received in a veſſel of dry mercury from the veſſel in which it was generated; but I preſently perceived that water was produced in this caſe alſo, and to appearance as copiouſly as in the former experiment. Indeed, the quantity of water produced, which ſo greatly exceeded the weight of all the inflammable air, is ſufficient to prove that it muſt have had ſome other ſource than any conſtituent part of that air, or the whole of it, together with the water contained in it, without taking into conſideration the correſponding loſs of weight in the iron.

I muſt here obſerve, that the iron ſlag which I had treated in this manner, and which had thereby loſt the weight which it had acquired by melting in dephlogiſticated air, became *perfect iron* as at firſt, and was then capable of being melted by the burning lens again; ſo that the ſame piece of iron would ſerve for theſe experiments as long as the operator ſhould chuſe. It was evident, therefore, that if the iron had loſt its phlogiſton in the preceding fuſion, it had acquired it again from the inflammable air which it had abſorbed; and I do not ſee how the experiment can be accounted for in any other way, which neceſſarily implies the reality of phlogiſton as a conſtituent principle in bodies. This, at leaſt, is the moſt natural way of accounting for the appearances.

Having had this ſucceſs with the calx, or ſcales of *iron*, I tried the calx of *copper*, or thoſe ſcales which fly from it when it is made red-hot; and I found water produced in the inflammable air in the ſame manner as when I uſed the ſcales of iron in the ſame circumſtances. I alſo had the ſame reſult when I revived *precipitate per ſe* in inflammable air; but having at that time a very weak winter's ſun, I could not make the experiment with ſo much advantage as I could have wiſhed.

Iron, I found, acquired this additional weight by melting in an earthen retort, as well as in the open air by the ſun-beams, if it were poſſible for it to attract air, or whatever elſe it is that is the immediate cauſe of its additional weight. Three ounces of common iron filings, expoſed to a ſtrong heat in an earthen retort, gained 11 dwts, or 264 grains, and yet was very far from having been completely fuſed. Having a glaſs tube communicating with the retort, in order to collect any air that the iron filings might give out, I found that when they were very hot, the water aſcended within the tube; which ſhews that the iron was then in a ſtate of abſorbing, and not of giving out any air.

Seeing ſo much water produced in theſe experiments with inflammable air, I was particularly led to reflect on the relation which they bore to each other, and eſpecially to Mr. CAVENDISH's ideas on the ſubject. He had told me that, notwithſtanding the experiments of which I had given an account to the Royal Society, and from which I had concluded that inflammable air was pure phlogiſton, he was perſuaded that *water* was eſſential to the production of it, and even entered into it as a conſtituent principle. At that time I did not perceive the force of the arguments which he ſtated to me, eſpecially as, in the experiments with charcoal, I totally diſperſed any quantity

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of it with a burning lens *in vacuo*, and thereby filled my receiver with nothing but inflammable air. I had no ſuſpicion that the wet leather on which my receiver ſtood could have any influence in the caſe, while the piece of charcoal was ſubject to the intenſe heat of the lens, and placed ſeveral inches above the leather. I had alſo procured inflammable air from charcoal in a glazed earthen retort two whole days ſucceſſively, in which it had given inflammable air without intermiſſion. Alſo iron filings in a gun-barrel, and a gun-barrel itſelf, had always given inflammable air whenever I tried the experiment.

Theſe circumſtances, however, deceived me, and perhaps would have deceived any other perſon; for I did not know, and could not have believed, the powerful attraction that *charcoal*, or *iron*, appear to have for *water* when they are intenſely hot. They will find, and attract it, in the midſt of the hotteſt fire, and through any pores that may be left open in a retort; and iron filings are ſeldom ſo dry as not to have moiſture enough adhering to them, capable of enabling them to give a conſiderable quantity of inflammable air. But my attention being now fully awake to the ſubject, I preſently found that the circumſtances above mentioned had actually miſled me; I mean with reſpect to the *concluſion* which I drew from the experiments, and not with reſpect to the experiments themſelves, every one of which, I doubt not, will be found to anſwer, whenever they are tried by perſons of ſufficient ſkill and properly attentive to all the circumſtances.

Being thus appriſed of the influence of unperceived moiſture in the production of inflammable air, and willing to aſcertain it to my perfect ſatisfaction, I began with filling a gun-barrel with iron filings in their common ſtate, without taking any particular precaution to dry them, and I found that they gave air as they had been used to do, and continued to do so many hours; I even got ten ounce measures of inflammable air from two ounces of iron filings in a coated glass retort. At length, however, the production of inflammable air from the gun-barrel ceased; but on putting water into it, the air was produced again, and a few repetitions of the experiment fully satisfied me that I had been too precipitate in concluding that inflammable air is pure phlogiston.

I then repeated the experiment with the charcoal, making the receiver, the stand on which I placed the charcoal, and the charcoal itself, as dry and as hot as possible, and using cement instead of a wet leather to exclude the air. In these circumstances I was not able, with the advantage of a good sun, and an excellent burning lens, to decompose quite so much as two grains of the piece of charcoal, which gave me ten ounce measures of inflammable air; and this, I imagine, was effected by means of so much moisture as was deposited from the air in its state of rarefaction, and before it could be drawn from the receiver. To the production of this kind of inflammable air I was therefore now convinced, that water is as necessary as to that from iron.

It was in this state of my experiments that I received an authentic account of those of M. LAVOISIER, on transmitting water through an hot iron tube and also through a hot copper tube containing charcoal, and thereby procuring large quantities of inflammable air, M. LAVOISIER himself having been so obliging as to send me a copy of his Memoir on that subject. I had heard an account of the experiments some months before; but it was so imperfect a one, that I own I paid little attention to them. At this time, however, I was prepared to be sufficiently sensible of their value.

In my laſt communications to the Royal Society, it will be ſeen that I had tranſmitted the vapour of ſeveral fluid ſubſtances through red-hot *earthen tubes*, and thereby procured different kinds of air. M. LAVOISIER adopted the ſame proceſs, but uſed an *iron tube*; and by means of that circumſtance made a very valuable diſcovery which had eſcaped me. I had indeed on one occaſion made uſe of an iron tube, and tranſmitted ſteam through it; but not having at that time any view to the production of *air*, I did not collect it at all, contenting myſelf with obſerving that *water*, after being made red-hot, was ſtill water, there being no change in its ſenſible properties. Being now farther inſtructed by the experiment of M. LAVOISIER, I was determined to repeat the proceſs with all the attention I could give to it; but I ſhould not have done this with ſo much advantage, if I had not had the aſſiſtance of Mr. WATT, who always thought that M. LAVOISIER's experiments by no means favoured the concluſion that he drew from them. As to myſelf, I was a long time of opinion that his concluſion was juſt, and that the inflammable air was really furniſhed by the water being decompoſed in the proceſs. But though I continued to be of this opinion for ſome time, the frequent repetition of the experiments, with the light which Mr. WATT's obſervations threw upon them, ſatisfied me at length that the inflammable air came principally from the charcoal, or the iron.

I ſhall firſt relate the reſult of the experiment that was made with *charcoal*, and then thoſe with iron and other ſubſtances, in contact with which (when they were in a ſtate of fuſion, or at leaſt red-hot) I made ſteam, or the vapour of other liquid ſubſtances, to paſs. I ſhall only obſerve that, previous to this, I began to make the experiments with coated glaſs tubes, which

I found to anſwer very well during the proceſs, though they never failed to break in cooling. At length I procured a tube of *copper*, on which, as M. LAVOISIER diſcovered, ſteam had no effect; and at laſt I made uſe of earthen tubes, with which Mr. WEDGEWOOD, that moſt generous promoter of ſcience, liberally ſupplied me for the purpoſe; and theſe glazed on the outſide only I find far preferable to copper. They are, indeed, every thing that I could wiſh for in experiments of this kind; the reaſon of which will appear in my account of another courſe of experiments, which I hope to lay before the Society in due time.

The diſpoſition of the apparatus, with which theſe experiments were made, was as follows. The water was made to boil in a glaſs retort, which communicated with the copper or earthen tube which contained the charcoal or iron, &c. and which, being placed in an horizontal poſition, was ſurrounded with hot coals. The end of this tube oppoſite to the retort communicated with the pipe of a common *worm tub*, ſuch as is generally uſed in diſtillations, by means of which all the ſuperfluous ſteam was condenſed, and collected in a proper receptacle, while the air which had been produced, and had come along with it through the worm tub, was tranſmitted into a trough of water, where proper veſſels were placed to receive it, and aſcertain the quantity of it; after which I could examine the quality of it at leiſure.

In the experiment with *charcoal*, I found unexpected difficulties, and conſiderable variations in the reſult; the proportion between the *charcoal* and *water* expended, and alſo between each ofthem and the *air* produced, not being ſo ne arly the ſame as I imagined they would have been. Alſo the quantity of fixed air that was mixed with the inflammable air varied very much.

much. This laſt circumſtance, however, ſome of my experiments may ſerve to explain. Whenever I had no more water than was ſufficient for the production of the air, there was never any ſenſible quantity of uncombined fixed air mixed with the inflammable air from charcoal. This was particularly the caſe when I produced the air by means of a burning lens in an exhauſted receiver, and alſo in an earthen retort with the application of an intenſe heat. I therefore preſume, that when the ſteam tranſmitted through the hot tube containing the charcoal was very copious, the fixed air in the produce was greater than it would otherwiſe have been. The extremes that I have obſerved in the proportion of the fixed to the inflammable air have been from one-twelfth to one-fifth of the whole. As I generally produced this air, the latter was the uſual proportion; and this was excluſive of the fixed air that was intimately combined with the inflammable air, and which could not be ſeparated from it except by decompoſition with dephlogiſticated air; and this combined fixed air I ſometimes found to be one-third of the whole maſs, though at other times not quite ſo much.

To aſcertain this, I mixed one meaſure of this inflammable air from charcoal (after the uncombined fixed air had been ſeparated from it by lime-water) with one meaſure of dephlogiſticated air, and then fired them by the electric ſpark. After this I always found that the air which remained made lime-water very turbid, and the proportion in which it was now diminiſhed, by waſhing in lime-water, ſhewed the quantity of fixed air that had been combined with the inflammable. That the fixed air is not *generated* in this proceſs, is evident from there being no fixed air found after the exploſion of dephlogiſticated air and inflammable air from iron.

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Notwithſtanding the above-mentioned variations, the loſs of weight in the charcoal was always much exceeded by the weight of the water expended, which was generally more than double of the charcoal; and this water was intimately combined with the air; for when I received a portion of it in mercury, no water was ever depoſited from it.

The experiment which, upon the whole, gave me the moſt ſatisfaction, and the particulars of which I ſhall therefore recite, was the following. Expending 94 grains of perfect charcoal (by which I mean charcoal made with a very ſtrong heat, ſo as to expel all fixed air from it) and 240 grains of water, I procured 840 ounce meaſures of air, one-fifth of which was fixed air, and of the inflammable part nearly one-third more appeared to be fixed air by decompoſition.

Receiving this kind of air in a variety of experiments, but not in the preceding ones in particular (for then I could not have aſcertained the quantity of it) conſiſting of fixed and inflammable air together, I found ſome variations in its ſpecific gravity, owing, I imagine, to the different proportions of fixed air contained in it; but upon the whole, I think, that the proportion of 14 grains to 40 ounce meaſures is pretty near the truth, when the proportion of fixed air is about one-fifth of the whole. With reſpect to the weight of the inflammable air after the fixed air was ſeparated from it, I found no great difference, and think it may be eſtimated at 8 grains to 30 ounce meaſures.

Upon theſe principles, the whole weight of the 840 ounce meaſures of air will be - 294 grains

that of the charcoal will be 94
that of the water - 240

334 which, conſidering the na-

ture of the experiment, will perhaps be thought to be tolerably near that of the air.

If the air be analyzed, the 840 ounce meaſures will be found to contain - 168 of uncombined fixed air = 151 grains.
and 672 impure imflammable = 179

ſo that the whole 840 will weigh - - 330

Laſtly, if the 672 ounce meaſures of impure inflammable air be decompoſed, it will be found to contain
164 ounce meaſures of fixed air = 147.6 grs.
and 508 inflammable - - = 30.7

ſo that the whole 672 will weigh - - 178.3

which is very near to 179, the weight of the whole together.

It may, however, be ſafely concluded from this experiment, and indeed from every other that I made with charcoal, that there was no more pure inflammable air produced than the charcoal itſelf may be very well ſuppoſed to have ſupplied.

There is, therefore, no reaſon for deſerting the old eſtabliſhed hypotheſis of *phlogiſton* on account of theſe experiments, ſince the fact is by no means inconſiſtent with it. The pure inflammable air with the water neceſſarily contained in it would weigh no more than about 30 grains, while the loſs of weight in the charcoal was 94 grains. But to this muſt be added the phlogiſton contained in 392 ounce meaſures of fixed air, which, according to Mr. KIRWAN's proportion, will be nearly 65 grains, and this and the 30 grains will be 95 grains.

The baſis to this fixed air, as well as to the inflammable, muſt have been furniſhed by the *water*; and from this it may be concluded, that the water muſt have been ſo far altered as to be changed into fixed air, which will be thought not to be any great paradox, if it be conſidered that, according to the

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lateſt diſcoveries, fixed air and water appear to conſiſt of the ſame ingredients, namely dephlogiſticated and inflammable air. However, in this change of the water we cannot be abſolutely ſure that the ſame proportion of the ingredients is contained, and therefore it cannot be abſolutely determined whether the inflammable air which it contains enters wholly into the fixed air, or not. Farther experiments, or a careful compariſon of theſe experiments with thoſe made by Mr. KIRWAN and others, may perhaps throw ſome light upon this ſubject. Whether the combined fixed air comes wholly from the charcoal, or whether the charcoal only ſupplies the phlogiſton, and the water its baſis, that is, the dephlogiſticated air, deſerves to be inveſtigated.

Before I conclude my account of the experiments with charcoal, I would obſerve, that there is another in which I place ſome dependance, in which, with the loſs of 178 grains of charcoal, and 528 grains of water, I procured 1410 ounce meaſures of air, of which the laſt portion (for I did not examine the reſt) contained one-ſixth part of uncombined fixed air. This was made in an earthen tube glazed on the outſide.

The experiments with *iron* were more ſatisfactory than thoſe with charcoal, being ſubject to leſs variation; and it is ſtill more evident from them, that the inflammable air does not come from the *water*, but only from the *iron*, as the quantity of water expended, added to the weight of the air produced, was as nearly as could be expected in experiments of this kind, found in the addition of weight gained by the iron. And though the inflammable air procured in this proceſs is between one-third and one-half more than can be procured from iron by a ſolution in acids, the reaſon may be, that much phlogiſton is retained in the ſolutions, and therefore much more may be expelled from iron, when pure water, without any acid, takes the place of it. I would further obſerve, that the produce of

air,

air, and alſo the addition of weight gained by the iron, are much more eaſily aſcertained in theſe experiments than the quantity of water expended in them, on account of the great length of the veſſels uſed in the proceſs, and the different quantities that may perhaps be retained in the worm of the tub, though I did not fail to uſe all the precautions that I could think of to guard againſt any variation on theſe accounts.

Of the many experiments which I made with *iron*, I ſhall content myſelf with reciting the following reſults. With the addition of 267 grains to a quantity of iron, and the loſs of 336 grains of water, I procured 840 ounce meaſures of inflammable air; and with the addition of 140 grains to another quantity of iron, and the conſumption of 253 grains of water, I got 420 ounce meaſures of air *.

The inflammable air produced in this manner is of the lighteſt kind, and free from that very *offenſive ſmell* which is generally occaſioned by the rapid ſolution of metals in oil of vitriol, and it is extricated in as little time in this way as it is poſſible to do it by any mode of ſolution. On this account it occurred to me, that it muſt be by much the cheapeſt method that has yet been uſed of filling *balloons* with the lighteſt inflammable air. For this purpoſe it will be proper to make uſe of caſt-iron cylinders of a conſiderable length, and about three or four inches, or perhaps more, in diameter. Though the iron tube itſelf will contribute to the production of air, and therefore may become unfit for the purpoſe in time; yet, for any

* If the perfect accuracy of the former of theſe experiments may be depended on (and it may always be preſumed, that thoſe in which *little water* is expended are preferable to thoſe in which *more* is conſumed) the *water* that neceſſarily enters into this kind of inflammable air is about equal in weight to the *phlogiſton* that is in it. But I propoſe to give more particular attention to this ſubject.

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thing that I know to the contrary, the ſame tube may ſerve for a very great number of proceſſes, and perhaps the change made in the inſide ſurface may protect it from any farther action of the water, if the tube be of ſufficient thickneſs; but this can only be determined by experiment.

Some eſtimate of what may be expected from this method of procuring inflammable air may be formed from the following obſervations. About twelve inches in length of a copper tube, three-fourths of an inch in diameter, filled with *iron turnings* (which are more convenient for this purpoſe than *iron filings*, as they do not lie ſo cloſe, but admit the ſteam to paſs through their interſtices) when it was heated, and a ſufficient quantity of ſteam paſſed through it, yielded thirty ounce meaſures of air in fifty ſeconds; and eighteen inches of another copper tube, an inch and a quarter in diameter, filled and treated in the ſame manner, gave two hundred ounce meaſures in one minute and twenty-five ſeconds; ſo that this larger tube gave air in proportion to its ſolid contents compared with the ſmaller; but to what extent this might be depended upon I cannot tell. However, as the heat penetrates ſo readily to ſome diſtance, the rate of giving air will always be in a greater proportion than that of the ſimple diameter of the tube.

The following experiment was made with a view to aſcertain the quantity of inflammable air that may be procured in this way from any given quantity of iron. Two ounces of iron, or 960 grains, when diſſolved in acids, will yield about 800 ounce meaſures of air; but treated in this manner it yielded 1054 ounce meaſures, and then the iron had gained 329 grains in weight, which is little ſhort of one-third of the weight of the iron.

Conſidering

Confidering how little this inflammable air weighs, *viz.* the whole 1054 ounce meafures not more than 63 grains, and the difficulty of afcertaining the lofs of water to fo fmall a quantity as this, it is not poffible to determine, from a procefs of this kind, how much water enters into the compofition of the inflammable air of metals. It would be more eafy to determine this circumftance with refpect to the inflammable air of charcoal, efpecially by means of the experiment made with a burning lens *in vacuo.* In this method two grains of charcoal gave at a medium thirteen ounce meafures of inflammable air, which, in the proportion of 30 ounce meafures to 8 grains, will weigh 3.3 grains; fo that water in the compofition of this kind of inflammable air is in the proportion of 1.3 to 2, though there will be fome difficulty with refpect to the fixed air intimately combined with this kind of inflammable air.

Since iron gains the fame addition of weight by melting in *dephlogifticated air*, and alfo by the addition of *water* when red-hot, and becomes, as I have already obferved, in all refpects the fame fubftance, it is evident, that this *air* or *water*, as exifting in the iron, is the very fame thing; and this can hardly be explained but upon the fuppofition that water confifts of two kinds of air; *viz.* inflammable and dephlogifticated. I fhall endeavour to explain thefe proceffes in the following manner.

When iron is melted in dephlogifticated air, we may fuppofe that, though part of its phlogifton efcapes, to enter into the compofition of the fmall quantity of fixed air which is then procured, yet enough remains to form *water* with the addition of dephlogifticated air which it has imbibed, fo that this *calx* of iron confifts of the intimate union of the pure *earth of iron* and of *water*; and therefore when the fame calx, thus faturated

rated with water, is expoſed to heat in inflammable air, this air enters into it, deſtroys the attraction between the water and the earth, and revives the iron, while the water is expelled in its proper form.

Conſequently, in the proceſs with *ſteam*, nothing is neceſſary to be ſuppoſed but the entrance of the water, and the expulſion of the phlogiſton belonging to the iron, no more phlogiſton remaining in it than what the water brought along with it, and which is retained as a conſtituent part of the water, or of the new compound.

Having procured water from the ſcales of iron (which I muſt again obſerve is, in all reſpects, the ſame ſubſtance with iron melted in dephlogiſticated air, or ſaturated with ſteam by means of heat) and having thereby converted it into perfect iron again, I did not entertain a doubt but that I ſhould be able to produce the ſame effect by heating it with charcoal in a retort; and I had likewiſe no doubt but I ſhould be able to extract the additional weight which the iron had gained (*viz.* one-third of the whole) in *water*. In the former of theſe conjectures I was right; but with reſpect to the latter, I was totally miſtaken.

Having made the ſcales of iron, and alſo the powder of charcoal very hot, previous to the experiment, ſo that I was ſatisfied that no air could be extracted from either of them ſeparately by any degree of heat, and having mixed them together while they were hot, I put them into an earthen retort, glazed within and without, which was quite impervious to air. This I placed in a furnace, in which I could give it a very ſtrong heat; and connected with it proper veſſels to condenſe and collect the water which I expected to receive in the courſe of the proceſs. But, to my great ſurpriſe, not one particle

of

of *moisture* came over, but a prodigious quantity of *air*, and the rapidity of its production astonished me; so that I had no doubt but that the weight of the air would have been equal to the loss of weight both in the scales and in the charcoal; and when I examined the air, which I repeatedly did, I found it to contain one-tenth of fixed air, and the inflammable air, which remained when the fixed air was separated from it, was of a very remarkable kind, being quite as heavy as common air. The reason of this was sufficiently apparent when it was decomposed by means of dephlogisticated air; for the greatest part of it was fixed air.

The theory of this process I imagine to be, that the phlogiston from the charcoal reviving the iron, the water with which it had been saturated, being now set loose, affected the hot charcoal as it would have done if it had been applied to it in the form of *steam* as in the preceding experiments; and therefore the air produced in these two different modes have a near resemblance to each other, each containing fixed air, both combined and uncombined, though in different proportions; and in both the cases I found these proportions subject to variations. In one process with charcoal and scales of iron, the first produce contained one-fifth of uncombined fixed air, the middle part one-tenth, and the last none at all. But in all these cases the proportion of combined fixed air varied very little.

Why *air* and not *water* should be produced in this case, as well as in the preceding, when the iron is equally revived in both, I do not pretend perfectly to understand. There is, indeed, an obvious difference in the circumstances of the two experiments; as in that with charcoal the phlogiston is found in a combined state; whereas in that of inflammable air, it is

loose,

loofe, or only united to water; and perhaps future experiments may difcover the operation of this circumftance.

There is fome analogy between the experiment of the calx of iron imbibing inflammable air, and the iron itfelf imbibing dephlogifticated air. In the former cafe *water* is produced, and in the latter *fixed air*. However, this cafe of iron imbibing dephlogifticated air more nearly refembles the cafe of the *blood in the lungs* imbibing the fame kind of air, and in both the cafes as dephlogifticated air is imbibed, fixed air is formed. This, therefore, feems to be a confirmation of the conclufion which I drew from my former experiments on blood, *viz.* that it parts with phlogifton in refpiration. Only I would now add, that at the fame time that it parts with phlogifton it takes in dephlogifticated air, which makes the cafe perfectly fimilar to that of the experiment with *iron*, which likewife parts with phlogifton to form fixed air, at the fame time that it imbibes dephlogifticated air in contact with which it is fufed.

I propofe to referve for a future communication the continuation of thefe experiments, containing an account of the application of the fame procefs to other fubftances; but it may not be amifs juft to mention a few of the *general refults*, and thofe which have the neareft connexion with the experiments recited above.

After having tranfmitted fteam in contact with *charcoal* and *iron* in a copper tube, I propofed to do the fame with other fubftances containing phlogifton, and I began with *bones*, which were burnt black, and had been fubjected to an intenfe heat, covered with fand, in an earthen retort. From three ounces of bone thus prepared, and treated as I had done the charcoal, I got 840 ounce meafures of air, with the lofs of 288 grains of water. The bones were by this means made perfectly white,

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and had loſt 110 grains of their weight. As the air ceaſed to come a conſiderable time before all the water had been tranſmitted through the tube containing them, I concluded that the air was formed from the phlogiſton contained in the bones, and ſo much water as was neceſſary to give it the form of air.

This air differs conſiderably from any other kind of inflammable air, being in ſeveral reſpects a medium between that from charcoal and that from iron. It contains about one-fourth of its bulk of uncombined fixed air, but not quite one-tenth intimately combined with the remainder. The water that came over was blue, and pretty ſtrongly alkaline, which muſt have been occaſioned by the volatile alkali not having been intirely expelled from the bones in the former proceſs, and its having in part diſſolved the copper of the tube in which the experiment was made.

I ſubjected to the ſame proceſs a variety of ſubſtances that are ſaid not to contain phlogiſton, but I was never able to procure inflammable air by means of them; which ſtrengthens the hypotheſis of the principal element in the conſtitution of this air having been derived from the ſubſtance ſuppoſed to contain phlogiſton, and therefore that phlogiſton is a real ſubſtance, capable of aſſuming the form of air by means of water and heat.

The experiments above-mentioned relating to iron were made with that kind which is *malleable*; but I had the ſame reſult when I made uſe of ſmall nails of *caſt iron*, except that theſe were firmly faſtened together after the experiment, the ſurfaces of them being cryſtallized, and the cryſtals mixing with each other, ſo that it was with great difficulty that they could be got out of the tube after the experiment, and in general the ſolid parts of the nails were broken before they were ſeparated

fro:

from each other. Indeed the pieces of malleable iron adhered together after the experiment, but by no means fo firmly.

Caft iron annealed (by being kept red-hot in charcoal) is remarkably different from the caft iron which has not undergone that operation, efpecially in its being, to an extraordinary degree, more foluble in acids. With the turnings of annealed caft iron I made the following experiment. From 960 grains of this iron, and with the lofs of 480 grains of water, I got 870 ounce meafures of inflammable air, and tranfmitting fteam through them a fecond time, I got 150 ounce meafures more. The iron had then gained 246 grains in weight, and the pieces adhered firmly together; but being thin they were eafily broken and got out of the tube, whereas it had required a long time, and a fharp fteel inftrument, to clear the tube of the caft-iron nails.

Having got water from the fcales of iron and of copper faturated with dephlogifticated air, by heating them in inflammable air, it occurred to me to make the fame experiment with *precipitate per fe*, and I found, that the moment that the focus of the lens fell upon this fubftance the mercury began to revive, the inflammable air rapidly difappeared, and *water* was formed on the fides of the veffel in which the experiment was made. For want of a better fun, I could not afcertain every circumftance relating to this procefs; but what I did feemed to afford a fufficient proof that mercury contains phlogifton, and that it is not revived by the mere expulfion of dephlogifticated air, as M. LAVOISIER fuppofes; efpecially as *no fixed air* was found in what remained of the inflammable air. In one of thefe experiments 4.5 ounce meafures of inflammable air had difappeared, and 1.6 ounce meafure remained; and this appeared to contain fome dephlogifticated air mixed with the inflammable.

Willing to try the effect of heating iron, and other substances, in all the different kinds of air, without any particular expectation, I found that iron melted more readily in *vitriolic acid* air than in dephlogisticated air, the air was diminished as rapidly, and the inside of the vessel was covered with a *black sooty matter*, which, when exposed to heat, readily sublimed in the form of a white vapour, and left the glass quite clean. The iron, after the experiment, was quite brittle, and must, I presume, be the same thing with iron that is *sulphurated*; but I did not particularly examine it. Of seven ounce measures of vitriolic acid air, in one of these experiments, not more than three-tenths of an ounce measure remained; of this two-thirds was fixed air, and the residuum of this was inflammable. I had put three of such residuums together, in order to make the experiment with the greater certainty.

Having transmitted *steam*, or the vapour of water, through a copper tube, I was willing to try the effects of *spirit of wine* through the same tube when red-hot, having before procured inflammable air by sending the same vapour through a red-hot tobacco-pipe. In this case, the vapour of the spirit of wine had no sooner entered the hot copper tube, than I was perfectly astonished at the rapid production of air. It resembled the blowing of a pair of bellows. But I had not used four ounces of the spirit of wine before I very unexpectedly found, that the tube was perforated in several places; and presently afterwards it was so far destroyed, that in attempting to remove it from the fire it actually fell in pieces. The inside was full of a black sooty matter resembling lamp-black.

Upon this I had recourse to *earthen tubes*, and found, that by melting copper and other metals in them, and transmitting the

 vapour

vapour of fpirit of wine in contact with them, different fubftances were formed according to the metals employed. The new fubftances hereby formed may be faid to be the feveral metals fuper-faturated with phlogifton, and may perhaps not be improperly called the *charcoal of metals*.

That this appellation is not very improper, may appear from thefe fubftances yielding inflammable air very copioufly when they are made red-hot, and the fteam of *water* is tranfmitted in contact with them, juft as when the charcoal of wood is treated in the fame manner. The detail of thefe experiments I referve for another communication, as alfo thofe of the converfion of *fpirit of wine*, *æther*, and *oil*, into different kinds of inflammable air, by tranfmitting them, in vapour, through hot earthen tubes. In the mean time, I fhall think myfelf happy if the communication of the preceding experiments fhall give any fatisfaction to the Members of the Society.

P O S T S C R I P T.

BEFORE I clofe this paper, I wifh to make a few *general inferences* from the principal of the experiments above-mentioned, efpecially relating to the proportional quantity of phlogifton contained in *iron* and *water*.

When any quantity of iron is melted in dephlogifticated air, it imbibes the greateft part of it, and gains an addition of weight very nearly equal to that of the air imbibed. Thus the abforption of twelve ounce meafures of dephlogifticated air gave an addition of ſix grains to the piece of iron which had been melted in it. But there was always a quantity of *fixed air* produced in this proceſs; and on the ſuppoſition that this air conſiſts of the union of dephlogiſticated and inflammable air, it proves that the dephlogiſticated air which enters the iron expels more phlogiſton than is neceſſary to conſtitute an equal weight of water, ſo that *water* does not contain ſo much phlogiſton as *iron*; but the difference is not very conſiderable.

Admitting Mr. KIRWAN's concluſion, *viz.* that 100 cubic inches of fixed air contain 8,357 grains of phlogiſton, the .13 ounce meaſure of fixed air, which (in an experiment recited in theſe papers) was found in the reſiduum of ſeven ounce meaſures of dephlogiſticated air abſorbed by iron, would not have contained more than .01 grain of phlogiſton, or about .16 ounce meaſure of inflammable air. Then, as the abſorption of 12 ounce meaſures of dephlogiſticated air occaſioned an addition of 6 grains to the weight of the iron which had abſorbed it, the abſorption of ſeven ounce meaſures muſt have occaſioned the addition of 3.5 grains to the iron which had imbibed it. But the ſame addition of weight to iron given by *ſteam* (which carries its own inflammable air along with it) would have expelled near 12 ounce meaſures of inflammable air: conſequently, about ten ounce meaſures of inflammable air (or the phlogiſton requiſite to form it) muſt, in the former experiment, have been retained in the iron, in order to compoſe the *water* which was now made by the union of the dephlogiſticated air imbibed by the iron and the phlogiſton contained in it: and therefore the proportion between the quantity of phlogiſton in *iron* to that which is contained in an equal weight of water, may be about 12 to 10, or more accurately to 10.4.

Had no fixed air at all been found in the reſiduum abovementioned, it might have been concluded, that water had contained the very ſame proportion of phlogiſton with iron. Since when iron that has been ſaturated with dephlogiſticated air is heated in inflammable air (in which proceſs an equal weight of water is produced, and the loſs of weight in the iron is equal to that of ſuch a quantity of dephlogiſticated air as would have been one-half of the bulk of the inflammable air which diſappears in that proceſs) it might have been concluded, that one-fifth of any quantity in water had been inflammable air.

For, neglecting the difference between the weight of dephlogiſticated and common air, which is not conſiderable, and eſtimating the latter $\frac{1}{800}$th part of water, and inflammable air at one-tenth of the weight of common air, an ounce meaſure of dephlogiſticated air will weigh .6 grain, and two ounce meaſures of inflammable air will weigh .12 grain, which numbers are to each other as 5 to 1 *.

Though, in conſequence of the ſmall quantity of fixed air which is found in the proceſs of melting iron in dephlogiſticated air, this concluſion is not accurate, it is pretty nearly ſo; and it is remarkable that, upon this ſuppoſition, about as much inflammable air is expelled from iron when water is com-

* It appears from the proſecution of theſe experiments, that the water which is found on heating the ſcales of iron in inflammable air, is not formed by the dephlogiſticated air expelled from them uniting with the inflammable air in the veſſel, but was the water previouſly contained in the ſcales, which is made to quit its place by the introduction of the phlogiſton from the inflammable air; yet that water carries out with it not much leſs phlogiſton than was taken in by the iron, and a little more muſt be allowed for that water which was neceſſary to make inflammable air, and which could not enter the iron when it was revived; ſo that, on the whole, the phlogiſton in the water that is found after the proceſs muſt be very nearly the ſame quantity that is imbibed by the iron, and the water is nearly the ſame that would have been produced, on the ſuppoſition of its being made from dephlogiſticated air expelled from the ſcales uniting with the inflammabel air in the veſſel.

bined with it, as the water itſelf brings along with it, as an eſſential ingredient in its compoſition. For in one experiment 296 grains added to the weight of a quantity of iron by ſteam, made it to yield about 1000 ounce meaſures of inflammable air. This would weigh 60 grains, and one-fifth of the 296 grains of water will be 59.2 grains. Again, 267 grains added to iron by ſteam made it to yield 840 ounce meaſures of inflammable air, which would weigh 50.4 grains, and one-fifth of the 267 would be 53.4 grains.

When the experiments on the melting of iron in dephlogiſticated air ſhall be repeated on a larger ſcale, which it will not be difficult to do by the help of a larger burning lens than I am at preſent poſſeſſed of, it will be eaſy to reduce theſe calculations to a greater certainty. All that I can do at preſent is to approximate to ſuch general concluſions as I have mentioned; but they are of ſo much conſequence in philoſophy, that it will certainly be well worth while to aſcertain them with as much accuracy as poſſible. Nice calculations would be ill beſtowed on the imperfect *data* which I am as yet able to furniſh. Attention muſt alſo be given to the quantity of water contained in inflammable air from iron; which not being yet aſcertained is not conſidered in theſe inferences. I wiſh only to hint in this Poſtſcript, that ſome important concluſions ſeems to be nearly within our reach.

END OF PART I. OF VOL. LXXV.