Considerations on the doctrine of phlogiston, and the decomposition of water (Volume 2).

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National Library of Medicine (U.S.)

Publication/Creation

Philadelphia : Printed by Thomas Dobson, at the stone-house, no 41, South Second-Street, 1796[-97]

Persistent URL

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OBSERVATIONS

ON THE

DOCTRINE

OF

PHLOGISTON,

AND

The Decompoſition of Water.

Part II.

By JOSEPH PRIESTLEY, *LL. D. F. R. S. &c. &c.*

Sed revocare gradum ——
Hic labor hoc opus eſt. VIRGIL.

PHILADELPHIA;
PRINTED BY THOMAS DOBSON, AT THE STONE-HOUSE,
N° 41, SOUTH SECOND-STREET.

1797.

OBSERVATIONS

ON THE

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DECOMPOSITION OF WATER.

PART II.

THE INTRODUCTION.

I THINK myſelf happy in having already drawn a conſiderable degree of attention to the two oppoſite theories of chemiſtry by my late publication on the ſubject, and I am therefore encouraged to endeavour to keep up this attention a little longer, and, if poſſible, till the queſtion now depending be decided to general ſatisfaction. At preſent I am ſenſible that I ſhall be conſidered as very obſtinate, in not admitting the new theory, when the old one is almoſt univerſally abandoned; though it is not true, that I am the

only perſon who adheres to it. Mr. Kirwan informs me, that Meſſrs. Crell Wertrumb, Gmelin, and Mayer, men of conſiderable reputation in Germany, ſtill maintain the doctrine of phlogiſton. So, I alſo hear, do my friends of the Lunar Society of Birmingham, among whom Mr. Keir has given as much evidence of his judgment in theſe ſubjects as any other perſon whatever. And I ſee by the advertiſements of books, that there is in France itſelf a recent publication againſt the new theory.

As truth can never ſuffer, but muſt always gain, by inveſtigation, I ſhall not offend any rational advocate for the antiphlogiſtic theory, if I endeavour to point out in what reſpects the replies that I have already heard of to my late publication appear to me to be unſatisfactory; and though I have given as much attention to them as I can, they appear to me far from unexceptionable. But my diſtance from the centre of philoſophical information lays me under great diſadvantages in this reſpect, as well as many others.

All

All the anſwers to my book that I have yet heard of are that of Mr. Adet in French, the Monthly and Analytical Reviews of it in England, and that of Dr. Maclean, Profeſſor of Mathematics and Natural Philoſophy in the College of New Jerſey. But as all theſe writers agree, as far as they go, together, I may preſume that other anſwers will go on the ſame general principles; ſo that in replying to them I may be replying to others alſo. I ſhall not, however, think the controverſy cloſed, till I hear from Mr. Berthollet and the other French chemiſts, to whom my Treatiſe was addreſſed.

In matters of much nicety, as the ſubjects of many of my numerous experiments are, I do not always expect to eſcape the charge of inaccuracy, and perhaps of inconſiſtency. Perſons who, from a want of experience, are not ſufficiently aware of the difficulties, will not have the candour that the circumſtances call for. From ſuch I muſt appeal to the judgment of thoſe who have the requiſite experience and qualifications. I will, however,

however, venture to ſay, that no perſon who has made near ſo many experiments as I have, has made ſo few miſtakes. I do not mean with reſpect to *opinions*, but in my reports of *facts*. But after all our care, errors will ſometimes ariſe from a want of attention to ſmall differences of circumſtances; and no perſon can keep his eyes open to every thing that is before him at the ſame time.

SECTION I.

Of the Solution of Iron in the Vitriolic and Marine Acids.

THE moſt ſimple of the experiments that I have propoſed for diſcuſſion, with a view to decide concerning the merits of the two theories in queſtion, is that of the ſolution of iron in the vitriolic and marine acids. Here the queſtion to be ſolved is, from which of the ſubſtances preſent comes the *inflammable air* that is procured in the proceſs.

ceſs. The phlogiſtians ſay it comes from the iron, and the antiphlogiſtians from the water. But to this I have objected that, ſince, according to their own hypotheſis, water conſiſts of about ſix times as much oxygen as it does of hydrogen, there muſt be a large depoſit of oxygen in the veſſel, and that I cannot find it there. That it is not in the acid appears, as the antiphlogiſtians themſelves ſay, by its ſaturating no more alkali after the proceſs than before. They, therefore, ſay, and there is no other alternative, that this addition of oxygen is in the iron.

But I now aſk, How does this appear? If there be any addition of oxygen in this caſe, it muſt ſhew itſelf either by an addition to the acid, or by its being exhibited in the form of dephlogiſticated air, called by them *oxygenous gas*. The former is not pretended; and ſo far is the latter from being true, that if the precipitate be expoſed to a red heat, it yields much leſs pure air than the ſame quantity of the acid without the iron would have done.

For

For this purpoſe I took as much vitriolic acid as I had found in the experiment recited in Vol. III. p. 197. of my *Obſervations on Air*, (in three vols.) to have yielded 130 ounce meaſures of dephlogiſticated air, of the ſtandard of .15, which is extremely pure, and ſaturated it with iron. But after this it yielded only 52 ounce meaſures of air, of the ſtandard of .55, which is much leſs pure. This ſhews that this precipitate is ſo far from containing more oxygen, that it contains leſs than the acid. It is in reality poſſeſſed of the oppoſite principle, which is agreeable to the phlogiſtic theory. For ſince much more inflammable air is procured from iron by means of ſteam only, than by its ſolution in any acid, more of the principle of which inflammable air conſiſts, viz. phlogiſton, muſt adhere to this calx of iron than to the other.

Dr. Maclean ſays, p. 19, "There is the "moſt ſatisfactory evidence that iron, after "its ſolution in ſulphuric acid is in a ſtate "like that of the black oxyd, or finery cin-"der." But the dephlogiſticated air which

is yielded by this precipitate is all procured before it comes to this form of a calx. After it becomes black, in which ſtate it ought to contain more oxygen in proportion to its bulk than before, it yields no oxygenous gas at all. Alſo, neither in this, nor in any other ſtate, will it oxygenate muriatic acid, which however eaſily diſſolves it. It therefore ſhews no ſign of its containing any oxygen at all. The new theory, however, requires that it be dignified with the appellation of the *black oxyd of iron*. The black oxyd of manganeſe gives more evidence of its right to the name they have given to it.

I have no great objection to admitting that this precipitate from the ſolution of iron in the vitriolic acid, when it is burned black, is the ſame ſubſtance with finery cinder. Both in this form, and in that of a brown powder, this precipitate has ſeveral of the ſame properties with thoſe of finery cinder. They neither of them either gain or loſe any weight by expoſure to the greateſt heat. When heated in atmoſpheric air, they both

diminiſh and, as I uſually ſay, phlogiſticate it, though very ſlowly. They alſo equally imbibe inflammable air when heated in it, but with this difference, that the production of water ſeemed to be greater in the reduction of finery cinder than in that of this precipitate. But the experiment being of no great conſequence, I did not give much attention to this circumſtance.

There is ſomething very extraordinary in the theory of this oxygen attaching itſelf to the iron on its ſolution in an acid. Mr. Adet ſays, p. 60, "Experiments prove that "metals, in order to be combined with an "acid, require to be united with oxygen;" and explaining himſelf farther, he ſays, "In "reality, a metal not combining with acids "but when it is in a ſtate of oxide, and not "paſſing into this ſtate but by its union "with oxygen, muſt neceſſarily abſorb ox-"ygen in order to unite with the acid. But "this oxygen can only be ſupplied by one "of theſe two ſubſtances, the acid itſelf, or "the water which it contains. If the oxygen

"had

" had been given by the acid, it would have " been in part decompoſed, and would in " conſequence have ſaturated leſs alkali. But " ſince it ſaturates the ſame quantity of al- " kali, it has not been decompoſed."

On this I would obſerve, that if the ſeparation of the oxygen from the water, in order to its attaching itſelf to the iron, take place prior to its ſolution in the acid, that ſolution is not neceſſary to its producing inflammable air; and this effect would in all caſes be produced by ſome affinity between the iron and the oxygen in the water only. If the affinity be between the iron and the oxygen univerſally, what could prevent the iron from ſaturating itſelf in the firſt inſtance with that which belongs to the acid, as well as with that which was a conſtituent part of the water, in which it is at leaſt much leſs evident. I would alſo aſk, if an acid will not diſſolve iron till it be oxydated, but will do when it is, why will not the acid of vitriol diſſolve the black oxyd of iron, or finery cinder, more readily than it

does iron; since in this substance it finds the iron already abundantly oxydated; and yet the reverse of this is the case.

SECTION II.

Of Finery Cinder.

THE great question between the advocates for phlogiston and their opponents is, whether the substance that has usually been called *finery cinder*, which is formed by the contact of steam with iron when it is red hot, be a proper *oxide of iron*, that is, whether it contain any principle which can be exhibited either in the form of an acid, or of dephlogisticated air; and yet this, which is the only proper evidence in the case, has not been given. To say that it forms *water* when heated in inflammable air, and that water cannot be formed without oxygen, is taking for granted the very thing to be proved; since the water so procured, I say, is that which was imbibed by the iron, and

is

is now expelled on the introduction of the phlogiſton with which it had parted.

One of my arguments to prove that finery cinder contains no oxygen is, that when it is diſſolved in marine acid, it does oxygenate it. Let us, however, hear the account that my opponents give of this circumſtance. Mr. Adet ſays, p. 55. "The nonoxygena-" tion of the muriatic acid by the ſolution " of finery cinder is owing to the latter re-" taining the oxygen ſo ſtrongly, as not to " be diſengaged by the action of heat, aided " by the attraction of the muriatic acid." To this I anſwer, that if the acid had not been able to diſſolve this ſubſtance, this might have been ſaid with ſome degree of plauſibility; but ſince it does diſſolve it completely, ſo volatile a thing as oxygenous gas, of which it is ſuppoſed to contain ſo large a quantity, and with which this acid has ſo ſtrong an affinity, could hardly eſcape being evolved.

Dr. Maclean makes very light of this, as indeed he does of every other difficulty. " It

" It certainly" he ſays, p. 10, " does not " follow that becauſe muriatic acid can ſe- " parate a certain quantity of oxygen from " lead, when this is combined with a great " quantity of that ſubſtance, that it ſhould " likewiſe ſeparate oxygen from iron, when " this is united to a comparatively ſmall " quantity." But finery cinder, if, as all antiphlogiſtians ſay, it owes all its additional weight to the pure oxygen, which it gained from the water which it had decompoſed, muſt contain much more of it than lead in any ſtate, or indeed than any known ſubſtance in nature. For the addition to its weight is nearly one third; whereas the addition to the weight of lead by making it into minium, is only about one tenth of its weight. Can this be all pure oxygen, that the iron acquires, and yet not oxygenate muriatic acid?

He farther ſays, p. 24. " The antiphlo- " giſtians ſuppoſe the addition made to " iron to be oxygen, becauſe the compound " reſembles in every reſpect, as Dr. Prieſtley " himſelf

" himſelf allows, that ſubſtance which is " formed by burning iron in oxygenous gas, " or in atmoſpheric air. And this they con- " ſider as an oxyd, becauſe while it is form- " ing the oxygenous gas diſappears, and its " weight is exactly equal to that of the iron " and oxygen conſumed."

But it is evident to me, that though the pure air, or oxygen, diſappears in this proceſs, it is not imbibed by the *iron*, but only the *water* which was its baſe, and which formed at leaſt the principal part of its weight; the pure air, or oxygen, ſerving to form the *fixed air* which is always found in this proceſs, and which cannot have any other origin. Conſequently, the calx of iron ſo formed when heated in inflammable air gives out nothing but water. The quantity of fixed air produced in this proceſs appears to me to be quite ſufficient to take all the pure air that diſappears in it. It is poſſible, however, that a ſmall quantity of oxygen may enter the iron along with the water to which it was united; as few ſubſtances are perfectly

perfectly ſeparated from each other by any chemical affinity.

When ſpirit of ſalt is diſtilled over a quantity of ſcales of iron, which, being made in the open air, are moſt likely to have ſome of this principle attached to them, it has ſomething of that faint ſmell which a very ſmall quantity of dephlogiſticated air will give it. But it is the more evident from this, that if this ſpecies of finery cinder had contained any conſiderable quantity of oxygen, it would have been extricated in this proceſs. That a little, and not more, appeared, I conſider as a proof that it contained no more; whereas, according to the new theory, it muſt contain more than any other ſubſtance.

A compariſon of the effects of the application of ſpirit of ſalt to finery cinder, and to red precipitate, is much in favour of the former containing no ſenſible quantity of oxygen. This acid preſently deprives the precipitate of its colour; during which a

great degree of heat is produced, and the ſmell of the dephlogiſticated acid is pretty pungent, though it ſoon becomes faint. When, after this, it is expoſed to the heat of a burning lens in confined air, the veſſel is filled with denſe white fumes; but when the ſubſtance becomes dry, it recovers its red colour, and the air is increaſed. But when the acid is applied to finery cinder, there is no heat, and little or no ſmell; and when it is heated in confined air, the air is diminiſhed. Can both theſe ſubſtances, which when treated in the ſame manner exhibit ſuch different phenomena, be equally oxyds?

That a very ſmall quantity of oxygen is attached to the ſcales of iron, I have thought probable from a barely perceivable quantity of fixed air which I have found when they are revived in inflammable air. But ſo ſmall a quantity as this makes nothing for the new theory.

Dr. Maclean farther ſays, p. 28, "The "quantity of carbonic acid formed by the "combuſtion of iron in oxygenous gas is

" very trifling, and this is owing partly to " the gas containing ſome before the opera- " tion, and partly to the plumbago contain- " ed in the iron." Now this, I will venture to ſay, cannot poſſibly be the ſource of the fixed air which appears in this proceſs. If the air before the proceſs contained any ſenſible quantity of fixed air, it could not fail to appear on its tranſmiſſion through lime-water. I appeal to the experience of any unbiaſſed experimenter in this caſe againſt the declaration of Mr. Berthollet, or any of the defenders of the antiphlogiſtic ſyſtem whatever; and Dr. Maclean, I preſume, only writes after them; for he never once refers to any experiments of his own.

The quantity of *plumbago* in the iron that is uſed in this experiment, and which this proceſs could not diſengage from it, could not, if it was wholly fixed air, yield a hundredth part of that which is produced. There is nothing whatever, concerning which, I am, from much experience, better ſatisfied than I am of the truth of theſe obſervations.

What

What makes it almoſt a certainty that the water which is found on the revival of finery cinder in inflammable air has not the ſource that the antiphlogiſtians ſuppoſe, is the great difference in the quantity which is found in this caſe, and that of the revival of other calces in it. Dr. Maclean ſays, p. 11. 'When oxyd of mercury is reduced in hy-" drogen gas, that diſappears, no oxygen gas " is obtained, but a quantity of water may " be collected." Now I am confident that no perſon who had ever ſeen the experiment could have written this. The quantity of water that appears in this caſe is barely perceivable, being no more than ſufficient to conſtitute the baſe of the inflammable air imbibed by the calx, or that might have been concealed in the ſubſtance operated upon; whereas when finery cinder is revived in the ſame circumſtances, the water forms itſelf into hundreds of ſmall drops, which unite, and run down the inſide of the veſſel in all directions.

Now if this water was really formed by the union of the inflammable air in the veſ-

ſel with the oxygen expelled from the calx, they ought ſurely to unite in the ſame proportions, to form the ſame thing. The antiphlogiſtians themſelves always ſay, that the proportion of hydrogen and oxygen in water is univerſally 15 parts of the former to 85 of the latter. Here, therefore, is much more water produced than their principles can account for. The ſame quantity of inflammable air diſappears, but the ſame quantity of water is by no means formed. The obvious concluſion therefore is, that in the caſe of the calx of iron, the great quantity of water produced was ſimply expelled from the calx when the inflammable air was imbibed; whereas the calx of mercury contains little or no water to be expelled, and only unites with the phlogiſton in the inflammable air that diſappears.

Before I conclude this ſection concerning finery cinder, I muſt take notice of what Dr. Maclean too confidently advances about it. "The Doctor," he ſays, p. 26, "is "certainly miſtaken in ſuppoſing that finery "cinder cannot ruſt. Mr. Fourcroy ſays it

"ruſts

"ruſts ſooner than common iron, and every "apothecary knows it does ſo. If the ruſt "of iron be made red hot in a retort, a "quantity of carbonic acid is diſengaged "from it, and the iron remains in a ſtate "of black oxyd. The ruſt, therefore, is a "*carbonate of iron*, and muſt contain all the "principles which compoſe the black oxyd, "and therefore can contain nothing capable "of excluding that which would convert it "into ruſt." This very confident aſſertion would aſtoniſh me if it were not too much of a piece with the reſt of the Doctor's performance. In direct contradiction to what he aſſerts, I ſtill ſay that finery cinder is not ſubject to ruſt. In England no uſe having been made of it before it was attended to by my brother-in-law, Mr. John Wilkinſon, (one of the moſt intelligent and ſucceſsful of all the iron-maſters in that or any country), but to mend the roads, it has lain in heaps for years, I may even ſay ages, without acquiring the leaſt tinge of brown. All my ſpecimens have ever remained free from ruſt, and the phyſicians, who are alſo apotheca-

ries,

ries, in this place, aſſure me they never ſaw or heard of any ſuch thing. They get it from the blackſmiths in the form of *ſcales of iron*, and the blackſmiths ſay the ſame. It muſt, therefore, as I have obſerved, be ſaturated with ſome principle very different from that of the common ruſt of iron, and is by no means the ſame thing, notwithſtanding what Dr. Maclean ſays to prove the contrary.

He alſo conſiders the ruſt of iron as containing more oxygen than finery cinder. But, though I do not know exactly what addition of weight iron acquires by being converted into ruſt, it cannot, I am confident, be near ſo much as it acquires by paſſing into the ſtate of finery cinder. If, therefore, as the antiphlogiſtians aſſert, all the additional weight be oxygen, finery cinder muſt contain more of it than the ruſt. But neither of theſe ſubſtances, whether they contain more or leſs of oxygen, will oxygenate muriatic acid. Nor what I think of no leſs conſequence, will finery cinder (which,

if

if it contain any oxygen, contains the moſt of it) when revived in inflammable air, produce any *fixed air*, as the revival of minium, which contains much leſs oxygen, in the ſame circumſtances does.

SECTION III.

Of the Calces of Mercury.

THE phlogiſtic theory, I readily acknowledge, is moſt preſſed by the phenomena of the calces of mercury. But in forming any general theory we muſt content ourſelves with the feweſt difficulties. It will hardly be pretended by the greateſt admirers of the antiphlogiſtic theory, that it is attended with none. Thoſe which attend the phlogiſtic with reſpect to theſe calces I do not think to be inſuperable, and farther experiments may throw more light upon them.

It is always aſſerted by the antiphlogiſtians that the calces of mercury are revived not only

only without addition, but without loſs. This, however, I have never found to be the caſe, and after many trials, often aſſiſted by other perſons, I have concluded that, after the ſolution of mercury in the nitrous acid, there is a loſs of one twentieth of the whole. And I muſt ſtill ſay that there are calces of mercury which certainly imbibe inflammable air, and therefore that this ſubſtance, or the baſe of it, phlogiſton, exiſts in that metal as an element. This is true both with reſpect to red precipitate, and turbith mineral.

In reviving red precipitate in inflammable air, I find no ſenſible quantity of water, of which there appears abundance during the revival of finery cinder in the ſame circumſtances, but I ſometimes get fixed air. Mr. Adet ſays, p. 64, "The fixed air which is "generally obtained by the revival of red "precipitate in inflammable air, comes from "the carbone held in ſolution in that air." But it cannot be proved that this kind of air ever holds any carbone, or any element of fixed air, in ſolution. That which ſome-

times appears on the decompoſition of it, when it is fired with dephlogiſticated air, is in ſome caſes certainly, and therefore in all the others probably, formed by their union in the exploſion. For in ſome caſes, I have ſhewn, that the quantity produced is ſo great, as to exceed the weight of all the inflammable air employed; ſo that its being ſuppoſed to conſiſt wholly of fixed air will not ſolve the difficulty.

As to the calx of mercury from the acid of vitriol, Mr. Beaumé *, I find, agrees with me in the obſervation, though I did not know it at the time, that it is not completely reducible by mere heat. But "later obſer-"vations," Dr. Maclean ſays, p. 11, "ſhew "that the turbith mineral, or any other "ſubſtance into which it may be converted

* With Mr. Beaumé I was a little acquainted. Mr. Macquer introduced me to him in his laboratory in Paris, and though he was an avowed opponent of the whole of the pneumatic chemiſtry, he was a good operator in the old way, and his fires, I am perſuaded, were as hot as any raiſed by the perſons mentioned by Mr. Adet, or thoſe by Dr. Hope.

" by a red heat, does not require any addi-
" tion to conſtitute it a metal." And Mr. Adet ſays, p. 43, " that the yellow oxide
" of mercury has been revived without ad-
" dition by Meſſrs. Monnet, Bouquet, La-
" voiſier, and Fourcroy."

To this I can only ſay, that I have never been able to reduce the whole of this calx by any heat that I could apply, not even that of a burning lens of ſixteen inches diameter; and this, I am confident, is a greater heat than can be raiſed by any furnace whatever. From being a red friable ſubſtance, this heat converts it into a yellowiſh glaſs, with the loſs of about three-tenths of its weight; but after this, no continuance of the ſame heat makes any farther change in it. Yet after this, when it is heated in inflammable air, the air is imbibed, and it is covered with a black powder, evidently *ethiops mineral*, into which mercury, with all its component parts, whatever they be, is known to enter. This ſubſtance alſo, and not directly running mercury, was frequently

the

the reſult of my experiments on this precipitate before I left England.

I wiſh that Dr. Maclean would repeat this experiment himſelf, as well as others which are differently related by myſelf and my opponents. Whatever is aſſerted by any antiphlogiſtian he never heſitates to admit; but he makes no difficulty of diſregarding any thing that I aſſert to the contrary. This is certainly an experiment of conſiderable conſequence. For if it be true that inflammable air be really imbibed by any calx of mercury, that it is revived by it, and cannot be revived without it, we are authorized to ſay univerſally, that ſome element of which it conſiſts, and no doubt phlogiſton, is a neceſſary component part of that metal, and therefore of all the other metals alſo.

In contradiction to what I and Dr. Withering have ſaid of mere heat not being able to ſeparate fixed air from the aerated barytes, Dr. Maclean ſays, p. 50, "Dr. "Hope has diſcovered that it can be done

" by ſuch a temperature as can be raiſed in " a ſmith's forge." This, however, I will venture to ſay could not be done in Birmingham, where the forges and furnaces are as good as thoſe of Edinburgh.

In reply to what I have obſerved of water being eſſential to this kind of air, becauſe readily procured with it, and not at all without it; he ſays, p. 50, " He has entirely " overlooked the property which carbonic " acid gas has of diſſolving water. Every " chemiſt knows it has this property, and " in a greater degree at a high than at a low " temperature. But water is not neceſſary " to the conſtitution of this gas, becauſe it " exiſts before the ſolution of the water, " and may be deprived of water by the ſul- " phuric acid, or any deliqueſcent ſubſtance, " and ſtill remain carbonic acid gas."

Whether Dr. Maclean will allow me to know what every chemiſt knows, or not, I was not ignorant of, nor did I overlook, the property of fixed air, or of any kind of air, diſſolving

diſſolving water. But that vitriolic acid, or any other ſubſtance, will deprive that, or any kind of air, of *all* the water which it only holds in ſolution, is more than any chemiſt can pretend to know. But this is nothing to the purpoſe. I find no air at all, nothing in the form of air, without the application of water, a great quantity of which diſappears in the proceſs, and can only remain in the air. I therefore conclude that water is eſſential to this kind of air. I ſpeak from my own obſervations, and I only wiſh that Dr. Maclean would ſpeak from his. If he have no aerated barytes, I will ſupply him with ſome for the experiment.

SECTION IV.

Of the Compoſition and Decompoſition of Water.

I WISH I could ſay that I have met with any thing in Dr. Maclean's Obſervations on my Experiments relating to the Compoſition and Decompoſition of Water, beſides general exclamations, ſome falſe aſſertions, and much boaſting of the ſuperior accuracy of the French chemiſts. "In what reſpects," ſays he, p. 45, "his experiments were leſs liable "to exception than thoſe of the French "chemiſts, is what I do not comprehend. "Theirs were performed on a very exten- "ſive ſcale, great care was taken to aſcertain "the degree of purity of the gaſſes before "combuſtion, and the apparatus was ſo con- "ſtructed, that the reſults could be determi- "ned with the greateſt nicety. The Doc- "tor's, on the contrary, were made with "very trifling quantities of materials, their "purity was not tried, and their weight not "accurately determined."

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Let us now conſider what theſe high ſounding words amount to. Experiments made with a great quantity of materials are not, always on that account, the moſt accurate, eſpecially where, as in this caſe, the thing to be determined is ſimply the *quality* of the reſult. When I can produce but a few drops of a ſtrong *acid*, and as often as I pleaſe, from the very ſame materials from which I am told that I ought to get only *pure water*, what is it to me whether they produce gallons?

Great care, he ſays, was taken to aſcertain the purity of the gaſſes, wherein with reſpect to me, he ſays, the purity was not tried. Now that of mine was not only tried, with as great accuracy as they could try theirs, but the dephlogiſticated air that I uſed was purer than any that I believe they ever pretended to have made. For with two equal meaſures of nitrous air, the reſiduum was only four hundredth parts of a meaſure, and this ſlight impurity was certainly not in the dephlogiſticated, but in the nitrous air, which

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is very apt to vary in its quality, and very difficult to obtain pure. And yet with this very pure dephlogisticated air, and a proportion, exactly defined, of the purest possible inflammable air, I got drops of a stronger acid than can be procured by means of air less pure. To this impurity, viz, a mixture of phlogisticated air, the antiphlogistians always ascribe the production of the acid, though if the air be purposely less pure, I never fail to find that impurity, viz. the phlogisticated air, unaffected by the process; so that it could not possibly have contributed to the production of the acid.

With the greatest confidence, however, Dr. Maclean says, p. 53, "the dense acid "vapour that I produced by the explosion "of the two kinds of air was occasioned by "the azote contained in the oxygenous gas "that I employed." He might as well have said it was occasioned by that which I did *not* employ. If ten times the quantity of azote in the air I used had been wholly decomposed, it would not have amounted to

the hundredth part of the weight of the acid that I procured.

Their apparatus, he ſays, was ſo conſtructed, that the reſult could be determined with the greateſt nicety. On the contrary, it was extremely complex, as a view of their plates will ſhew, and mine was perfectly ſimple, ſo that nothing can be imagined to be leſs liable to be a ſource of error. How, indeed, was this poſſible? I uſe only one large veſſel, of glaſs, or copper. I put into it at once a certain proportion of the two kinds of air, the purity of which, when it is neceſſary, I can aſcertain as well as other perſons. From the ſimplicity of the apparatus no other ſubſtance can poſſibly mix with them, and I then explode the whole at once by an electric ſpark. After this I preſently find the reſult by examining the liquor that is drained from the veſſel. Though I have not gallons of this liquor, I have ſome ounces, which no antiphlogiſtian would care to drink. Will Dr. Maclean ſay that my proceſs is leſs accurate than that of the French,

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becauſe it can be finiſhed in leſs than five minutes, and theirs requires the aſſiduous attendance of ſome days.

Uſing the ſame moſt ſimple apparatus, I can, by only varying the proportions of the two kinds of air, produce the reſult which the French chemiſts ſo much boaſt of. For I can produce water as free from acid as theirs, and with much greater certainty, as I have no attention to give to a flame, leſt it ſhould at any time burn too fiercely. But in this caſe I always produce a quantity of *phlogiſticated air*, in which they acknowledge that the principle of acidity reſides. They alſo do not deny that they had a ſurplus of the ſame kind of air; and as to the quantity of it, I cannot help ſuppoſing that, intereſted as they were to make it as little as poſſible, being men, and of courſe liable to the biaſſes of other men, they may have repreſented it, by the allowances they made in their computation, ſomething leſs than it really was. All the inſide of my large veſſel being, of courſe, wet with the liquor produced

duced by the explosion, I could not pretend to *weigh* that which was drained from it with much accuracy. But then very little depended upon the *quantity*, compared to the consideration of the *quality* of the liquor; and this may be as clearly ascertained by drops, as by the largest quantities; and till the French chemists can make their experiments in a manner less operose and expensive, requiring fewer precautions, and less of computation, I shall continue to think my results more to be depended upon than theirs.

That phlogisticated air *can* be produced from the same materials from which I get nitrous acid, viz. dephlogisticated and inflammable air, I have given various and sufficient proof. Dr. Maclean, however, says of them, and of other of my experiments, p. 66, "As the Doctor has not favoured us with a "detail of his experiments, and as they bear "the most striking marks of not having been "performed with accuracy, I will not take "up your time" (speaking to his pupils) "with a review of them."

Though an account of the experiments to which he here refers was not inſerted in the pamphlet on phlogiſton, it was printed for the Tranſactions of the Philoſophical Society at Philadelphia, which I expected would have been publiſhed long ago. It is evident, however, that Dr. Maclean had ſeen a copy of thoſe articles. How elſe could he ſay that they bear ſuch evident marks of not having been performed with accuracy? He ought certainly to have ſhewn *how* they could have been made with more accuracy, with reſpect to the proper object of them, and I requeſt that he will do it.

Notwithſtanding this authoritative condemnation of thoſe experiments, on which, however, till I hear ſome good reaſon to the contrary, I ſhall continue to lay ſome ſtreſs, I ſhall here give an account of another experiment, though I do not pretend to ſay that it is more accurate than the reſt. Having made a number of pieces of iron ruſty by dipping them in marine acid, I put them into a glaſs veſſel, which I then filled up with mercury, and I diſplaced this mercury by

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inflammable air. After waiting about eight months, I examined the air, and found it to be very ſlightly inflammable, the far greater part of it being evidently phlogiſticated air. The iron, from being red, which all antiphlogiſtians will ſay was owing to its containing oxygen, was become black, being covered with a kind of ſoot, which was eaſily wiped off, ſtaining the fingers and paper. Under this coating the iron was of its uſual colour.

Whence, now, came this phlogiſticated air, if not from the union of dephlogiſticated and inflammable air? I have pretty clear proof of the ſame elements forming in other circumſtances fixed air, eſpecially the production of a great quantity of this kind of air from heating a mixture of iron filings and red precipitate; ſo that, in contradiction to the maxim of Mr. Lavoiſier, this *carbonic acid*, as it is called, is formed without *carbone*. This remarkable fact I am told is diſputed by the antiphlogiſtians, but I have lately repeated the experiment with the ſame reſult as before.

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This experiment is very little liable to the objection of the Monthly Reviewer, p. 371, as the pieces of iron had not been expoſed to the atmoſphere any great length of time, and I am confident that by no proceſs whatever could any phlogiſticated air have been extracted from them.

If the above-mentioned black ſubſtance with which the pieces of iron were coated be *plumbago* (and of this little doubt can be entertained) it will appear to be a calx of iron ſuperſaturated with phlogiſton, and that the whole of the iron might have been converted into it, but that plumbago cannot be contained in iron, ſo as to yield, on its ſolution in an acid, the phlogiſticated air of which my opponents have endeavoured to avail themſelves.

As to the experiments recited in my third ſection, I ſhall not enlarge upon them at this time, but leave my readers to compare them with the remarks that have been, or may be made upon them, and judge for themſelves.

THE END.