THE DOCTRINE OF PHLOGISTON ESTABLISHED,

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AND THAT OF

THE COMPOSITION OF WATER

REFUTED.

BY JOSEPH PRIESTLEY, *L. L. D. F. R. S. &c. &c.*

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

NORTHUMBERLAND:

PRINTED FOR THE *AUTHOR* BY *A. KENNEDY.*

MDCCC.

THE

DEDICATION.

To SAMUEL GALTON, Esq.

DEAR SIR,

PERMIT me to endeavour to perpetuate, as far as I can, the remembrance of your valuable friendship to me (as well as that of Mrs. Galton to my wife) which has continued without interruption from the time that we became acquainted on my ſettlement at Birmingham. The interviews we have had at the *lunar ſociety*, and on other occaſions, I now look back upon with peculiar ſatisfaction, tho' mixed with regret. There is no lunar ſociety to which I can communicate my obſervations, and from which I can receive light in return, in this place.

At my time of life, however, I could not expect to enjoy any ſociety in this world much longer. Others, alſo, of our members muſt now be looking forward, as I do, to a ſtate of greater ſecurity and permanency than the preſent; where no riots will ſeparate us again, and where we ſhall, I doubt not, reſume our pleaſing purſuits, and our ſpeculations concerning the wonderful ſyſtem of which we are a part, and with more advantage and ſatisfaction than ever.*

There only can I have any certain proſpect of meeting with any of you. But the confident expectation that

* Since this was written I have heard that one of the members of this ſociety, viz. Dr. Withering, is dead.

that I have of meeting my philoſophical and chriſtian friends again, is a ſource of conſolation and pleaſing reflection in my preſent ſtate of *exile* from them, that is invaluable.

Tho' compared to this, the moſt important of all ſubjects, I feel but little intereſt in the queſtion which, in this treatiſe, I bring once more before the Public; it is a great ſatisfaction to me that, I have the ſanction of my friends of the lunar ſociety at Birmingham for the doctrine maintained in this treatiſe; and notwithſtanding the great names among the advocates for the new ſyſtem in other countries, as well as in France, there are no where to be found men of more knowledge, ſagacity, and cool obſervation, than in your body. No perſon needs to be aſhamed of being in an error in ſuch company.

Aſſure them all, that I ſhall ever think of them with particular eſteem and affection; and if, contrary to my preſent expectations, there ſhould be an interval of *peace* in this moſt diſaſtrous war, while I am able to bear the voyage, I flatter myſelf with the proſpect of paying a viſit to my friends in England; and then I ſhall certainly take the firſt opportunity of attending one more of your meetings. If providence ſhould order otherwiſe, Adieu till we meet in more favourable circumſtances than we can ever do at Birmingham.

With the greateſt eſteem and affection

I am,

Dear *Sir*,

Yours ſincerely,

J. PRIESTLEY.

Northumberland, Feb. 1, 1800.

THE

PREFACE.

DESIROUS of bringing the important controversy concerning *phlogiston* to a fair decesion, I some years ago, made many experiments with that view, the result of which appeared to myself favourable to the discarded hypothesis. Since my removal to America, where, after a long interruption of my pursuits, I found myself in circumstances tolerably favourable to the resumption of them, one of the first things that I did was to continue the same research, and many of these new experiments being favourable to the old theory, I endeavoured in several publications, especially in the *Medical Repository* printed at New-York, to promote the same discussion; some of of the articles being written in direct defence of what I had advanced, and others in reply to objections; and having now, I imagine, heard all that can be urged in favour of the new theory, from its ablest advocates, both here and in Europe, and thinking it far from being sufficient for its support, I republish in this work all that I think of importance in the former publications, and present it to the public

as

as a *demonſtration of the doctrine of phlogiſton*, and a complete refutation of that of the *compoſition of water*. For, after the beſt attention that I am able to give to the ſubject, ſuch it apears to me.

The refutation of a fallacious hypotheſis, eſpecially one that is ſo fundamental as this, cannot but be of great importance to the future progreſs of ſcience. It is like taking down a falſe light, which miſleads the mariner, and removing a great obſticle in the path of true knowledge. The longer ſuch an hypotheſis has been received, and the more numerous and able are its advocates, the greater ſervice is rendered to ſcience by the refutation. And there is not perhaps any example of a philoſophical hypotheſis, ſince the revival of true ſcience, more generally received, or maintained by perſons of greater eminence, than this of the rejection of *phlogiſton*. In this country I have not heard of a ſingle advocate for phlogiſton. In England they are very few, and none of them have written any thing on the ſubject. In France there are fewer ſtill, and in Germany I hear of no names beſides thoſe of Crell, Weſtrumb, Gmelin, and Mayer. No perſon, however, need to be aſhamed of avowing an opinion which has the ſanction of ſuch names as theſe. But what any of them may have written in defence of phlogiſton is unknown to me; ſo that tho' we are engaged in the ſame cauſe, we are unable to give the leaſt aſſiſtance to each other.

Removed as I now am to ſo great a diſtance from the great theatre of philoſophical purſuit, and out of the way of early intelligence (our communications with Europe being alſo farther interrupted in the preſent unfortunate ſtate of war) I neceſſarily labour under various and great diſadvantages. I am thankful, however, to a kind providence for the *quiet* that I here enjoy in this remote ſituation, and for ſuch means of proſecuting my ſtudies, as, conſidering the ſtate of the

country,

country, are very ample.* And I hope that, confidering my advançed age, I ſhall be thought to have been tolerably aſſiduous in making uſe of them.

But my philoſophical friends muſt excuſe me if, without neglecting natural ſcience, I give a decided preference to theological ſtudies; and if here, as in Europe, I give the greateſt part of my time to them. They are unqueſtionably of unſpeakably more importance to men, as beings deſtined for immortality; and I apply myſelf with ſo great ſatisfaction to the ſtudy of nature, not ſo much on account of the advantage we derive from it at preſent, tho' this is very conſiderable, as from its being a delightful field of ſpeculation barely opening to us here, and to be reſumed with far greater advantage in a future ſtate.

No diſcovery in philoſophy bears any proportion in real value to that of *bringing life and immortality to light*, which is completely effected in the Goſpel, and no where elſe. None of our experiments, or obſervations on the courſe of nature, could have given us the leaſt glimpſe of this. But

* To the account of my reaſons for leaving England I prefixed a motto from Petrarch, whoſe Latin works, and eſpecially his Letters, often amuſe and intereſt me. It was from his addreſs to his patron the Cardinal of Colonna.

C. Quo fugis? Expecta. Liceat condiſcere cauſas
Diſſidii. Tu noſtra, puer, niſi fallor, amabas Paſcua.

P. Parce, Parens, damnare tuum—Tibi lætior annis
Tunc animus fuerat. Nunc intractabilis, aſper.

I may now apply to myſelf what he addreſſed to the Biſhop of Cabaſſole when he was at Vaucluſe, abſent from his native country Italy.

Exul ab Italia, furiis civilibus actus,
Huc ſubii, partimque volens, partimque coactus.
Hic nemus, hic amnes, hic ocia ruris amœni.
Sed fidi comites abſunt, vultuſque ſereni.
Hoc juvat, hoc cruciat. Nihil illis dulce remotis.

But the evidence of this great truth, tho' of the moſt ſatisfactory kind, not being that of *ſenſe*, but requiring *attention* and *reflection*, perſons much engaged in the buſineſs of the world, and even in literary and ſcientifical purſuits, are not always convinced by it. It alſo requires a candid and well diſpoſed mind, and therefore philoſophers (who have their prejudices as well as other men) are not always chriſtians. Among thoſe of this claſs I am, however, happy in being able to rank not a few, who would do honour to any cauſe; and the number of truly philoſophical chriſtians, I am well perſuaded, will in due time increaſe. As Paul ſaid to king Agrippa, who ſaid that he had "almoſt perſuaded him to be a chriſtian," that "he "wiſhed that both he, and all who then heard him, "were both almoſt, and altogether, ſuch as he himſelf "was, except his bonds;" ſo there is no greater happineſs that I can wiſh to all my philoſophical friends, than that, with reſpect to religion, and their future proſpects, they were what I am, without the calumnies, and the ſtill more ſerious injuries, to which I have been expoſed.

Without a view to this future ſituation. all our purſuits appear to me to have little in them that is intereſting, eſpecially in the decline of life, and the near proſpect of death, which, if it put a period to our exiſtance, involves every thing in everlaſting darkneſs, leaving us uncertain whether even the world itſelf, and the whole race of man, as well as all other animals, may not be doomed to deſtruction. How gloomy is this proſpect, and how dead and indifferent does it render a reflecting mind to every great purſuit.

How thankful, then, ought we to be for an aſſurance of an endleſs ſtate of exiſtence, and in circumſtances infinitely more favourable than the preſent. The evidence of this great doctrine (in compariſon with which every other inquiry is as nothing) is ſurely ly worthy of our moſt aſſiduous examination, infinitely more ſo than a title to an eſtate, or a claim to a kingdom in this world, which no wiſe man would be thought juſtifiable in neglecting.

This being perhaps the laſt time that I may have an opportunity of addreſſing myſelf to my philoſophical friends, who, I am concerned to perceive, are generally unbelievers in revelation, I would make it my dying requeſt, proceeding from the moſt ſincere good will to them, to attend to this ſubject, eſpecially to what I took the liberty to urge in the *Preface* to the ſixth volume of my *Obſervations on air*, which was reprinted in the new edition of that work in three volumes, and alſo to my *Letters to the philoſophers and politicians of France on the ſubject of religion*, my *Letters to a philoſophical unbeliever* and my other works in defence of revelation.

Independently of the conſideration of the infinitely ſuperior importance of the ſubject, religion will give a double reliſh to philoſophical purſuits, and will thereby contribute to their ſucceſs. It is only a wretched ſuperſtition, and not religion, that draws men's attention from natural ſcience, or with any other view drives men into retirement, and excludes them from any active and uſeful purſuits. On the contrary, it tends to inſpire men with increaſed activity, and imparts increaſing ſatisfaction and animation in every proper and laudable exertion. Of this I think I may ſay I have exhibited an example in myſelf. My numerous publications will ſhew that from early life I have given the greateſt part of my time to theological ſtudies, and yet few have been more aſſiduous in phyſical inquiries ſince I have had the means of doing it. Do not then ſay that religion neceſſarily makes men idle, or buſy to no uſeful purpoſe.

Call this, if you pleaſe, the talkativeneſs of

age;

age; but believe it to proceed from a zeal in the beſt of cauſes, and ſincere good will to yourſelves. For I find that I have inſenſibly got into a direct addreſs in the form of a *dedication*, rather than that of a *Preface*. With this, however, I conclude. Farewell, and may we meet where our preſent doubts will be removed, and where we ſhall make more rapid advances in knowledge, without that envy and jealouſy, from which philoſophers are no more exempt than other men, and which, tho' it has an excellent effect in making men cautious, and even ardent in their purſuits, from a view to the reputation they hope to acquire by their diſcoveries, too often makes their purſuits the cauſe of more pain than pleaſure to them. Hereafter, we ſhall, I doubt not, be even more actively employed, and more happy in conſequence of it, from better motives.

I ſhall cloſe this Preface with the Letter I addreſſed to the advocates for the new theory in France in the firſt pamphlet I publiſhed in anſwer to them, and alſo a ſecond, which I addreſs to them in the preſent ſtate of the controverſy.

To Meſſrs. Berthollet, De la Place, Monge, Morveau, Fourcroy, and Haſſenfratz, the ſurviving Anſwerers of Mr. Kirwan.

GENTLEMEN,

HAVING drawn up a ſhort defence of the doctrine of *phlogiſton*, I take the liberty of inſcribing it to *you*, as the principal advocates for the Antiphlogiſtic theory. My view in this is to draw your attention

ention once more to the ſubject, and I requeſt the fa-
our of an anſwer to my objections. I hope I am not
anting in a proper deference to the opinion of men ſo
uſtly eminent as yourſelves and your friends in France,
nd alſo that of great numbers in England, and where-
er chemiſtry is known, who have adopted your hypo-
heſis. But you will agree with me, that no man
ught to ſurrender his own judgment to any mere *au-
thority*, however reſpectable. Otherwiſe, your own
yſtem would never have been advanced.

As you would not, I am perſuaded, have your
eign to reſemble that of *Robeſpierre*, few as we are who
emain diſaffected, we hope you had rather gain us by
erſuaſion, than ſilence us by power. And though
e are all apt to flatter ourſelves, we hope we are as
illing to be influenced by the former, as we are inflex-
ble to the latter. If you gain as much by your anſwer
o me, as you did by that to Mr. Kirwan, your pow-
r will be univerſally eſtabliſhed, and there will be no
Vendee in your dominions.

Differing as we do in this reſpect, we all agree in
ur wiſhes for the prevalence of *truth*, and alſo of *peace*,
hich is wanted as much for the intereſts of philoſo-
hy, as thoſe of humanity. And on this account I
arneſtly wiſh ſucceſs to the liberty and proſperity of
rance, which did me the honour to adopt me
hen I was perſecuted and rejected in my native coun-
y. With great ſatisfaction, therefore, I ſubſcribe
myſelf

Your fellow-citizen,

JOSEPH PRIESTLEY.

Northumberland in America, June 15*th*, 1796.

A second Letter to the same

GENTLEMEN,

ABOUT three years ago I took the liberty to request your reconsideration of the doctrine of *phlogiston*, which you had long discarded. A very respectable advocate of your system, Mr. Adet, being then in this country, he replied to my defence of it, and at length I have just received what may be called your dinitive answer, in the *Report of Messrs. Berthollet and Fourcroy* on the merits of our performances, in the 26th volume of the *Annales de Chymie*, in which you consider me as supporting a system *un peu chancelante*. As a friend of the weak, I have, indeed, endeavoured to give it a little assistance; and as there is no giving strength to one of the opposite systems without taking it from the other, I presume that yours is now in the same situation; calling to you for all the support that you can give to it.

On the opening of this controversy I told Mr. Adet that I should have greater pride in acknowledging myself convinced, if I saw reason so to be, than in victory, and should surrender my arms with pleasure. I was sincere in that declaration; and certainly the conquest of a man's prejudices is more honorable to him than the discovery, or the most succesful defence, of any truth. This, however, I must, for the present at least, decline, and leave it to you; contenting myself with the inferior praise of confirming the hypothesis for which I have contended. If, from the politeness habitual to Frenchmen, you should decline this honour,

nour, thinking my claim to it better founded than yours, I may hereafter be induced to receive it; but for the present, yielding to you a palm more glorious than that of any victory, and trusting that your political revolution will be more stable than this chemical one,

I am with the greatest respect,

Gentlemen,

Your fellow-citizen,

J. PRIESTLEY.

Northumberland in America, Feb. 1. 1800.

THE

CONTENTS.

THE

DOCTRINE OF PHLOGISTON

ESTABLISHED.

THE INTRODUCTION.

THERE have been few, if any, revolutions in ſcience ſo great, ſo ſudden, and ſo general, as the prevalence of what is now uſually termed *the new ſyſtem of chemiſtry*, or that of the *Antiphlogiſtians*, over the doctrine of Stahl, which was at one time thought to have been the greateſt diſcovery that had ever been made in the ſcience. I remember hearing Mr. Peter Woulfe, whoſe knowledge of chemiſtry will not be queſtioned, ſay, that there had hardly been any thing that deſerved to be called a *diſcovery* ſubſequent to it. Though there had been ſome who occaſionally expreſſed doubts concerning the exiſtence of ſuch a principle as that of *phlogiſton*, nothing had been advanced that could have laid the foundation *of another ſyſtem* before the labours of Mr. Lavoiſier and his friends, from whom this new ſyſtem is often called that of the *French*.

This ſyſtem had hardly been publiſhed in France,

before

before the principal philoſophers and chemiſts of England, notwithſtanding the rivalſhip which has long ſubſiſted between the two countries, eagerly adopted it. Dr. Black in Edinburgh, and, as far as I hear, all the Scots have declared themſelves converts, and what is more, the ſame has been done by Mr. Kirwan, who wrote a pretty large treatiſe in oppoſition to it. The Engliſh reviewers of books, I perceive, univerſally favour the new doctrine. In America alſo, I hear of nothing elſe. It is taught, I believe, in all the ſchools on this continent, and the old ſyſtem is entirely exploded. And now that Dr. Crawford is dead, I hardly know of any perſon, except my friends of the Lunar ſociety at Birmingham, who adhere to the doctrine of phlogiſton.

It is no doubt *time*, and of courſe opportunity of examination and diſcuſſion, that gives ſtability to any principles. But this new theory has not only kept its ground, but has been conſtantly and uniformly advancing in reputation, about *fifteen years*, which, as the attention of ſo many perſons, the beſt judges of every thing relating to the ſubject, has been unremittingly given to it, is no inconſiderable period. Every year of the laſt twenty or thirty has been of more importance to ſcience, and eſpecially to chemiſtry, than any ten in the preceding century. So firmly eſtabliſhed has this new theory been conſidered, that a *new nomenclature*, entirely founded upon it, has been invented, and is now almoſt in univerſal uſe; ſo that, whether we adopt the new *ſyſtem* or not, we are under the neceſſity of learning the new *language*, if we would underſtand ſome of the moſt valuable of modern publications.

In this ſtate of things, an advocate for the old ſyſtem has but little proſpect of obtaining a patient hearing. And yet, not having ſeen ſufficient reaſon to change my opinion, and knowing that free diſcuſſion

cuſſion

cuſſion muſt always be favourable to the cauſe of truth, I wiſh to make one appeal more to the philoſophical world on the ſubject. Beſides having many new obſervations to advance, I cannot help thinking that what I have obſerved in ſeveral of my former publications has not been ſufficiently attended to, or well underſtood. I ſhall therefore endeavour to bring into one view what appears to me to be of the greateſt weight, avoiding all extraneous and unimportant matter; and perhaps it may be the means of bringing out ſomething more deciſive in point of *fact*, or of *argument*, than has hitherto appeared.

No perſon acquainted with my philoſophical publications can ſay that I appear to have been particularly attached to any hypotheſis, as I have frequently avowed a change of opinion, and have more than once expreſſed an inclination for the new theory, eſpecially that very important part of it *the decompoſition of water*, for which I was an advocate when I publiſhed the ſixth volume of my *Experiments on Air*, though farther reflection on the ſubject has led me to revert to the creed of the ſchool in which I was educated, if in this reſpect I can be ſaid to have been educated in any ſchool. However, whether this new theory ſhall appear to be well founded or not, the advancing of it will always be conſidered as having been of great importance in chemiſtry, from the attention which it has excited, and the many new experiments which it has occaſioned, owing to the juſt celebrity of its patrons and admirers.

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

judgment of thoſe who have the requiſite experience and qualifications. I will, 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.

That Metals are compound Subſtances, and contain Phlogiſton, proved from the Solution of Iron in the vitriolic and marine Acids, and from ſome other conſiderations.

ACCORDING to the doctrine of phlogiſton, advanced by Becher and Stahl in the beginning of this century, and much ſimplified and improved ſince their time, metals, phoſphorus, ſulphur, and many other ſubſtances which are ſuppoſed to contain it, are compounds, conſiſting of this principle, and another which may be called its *baſe*. Thus each of the metals contains phlogiſton united to a peculiar calx, and ſulphur and phoſphorus conſiſt of the ſame principle and their reſpective acids, or the baſes of them. But according to the antiphlogiſtic theory, all the metals are ſimple ſubſtances, and become calces by imbibing pure

pure air; and sulphur and phosphorus are also simple substances, and become the acids of vitriol and of phosphorus by imbibing the same principle, called by them *oxygen*, or the principle, as it probably is, of universal acidity.

And whenever inflammable air is procured by means of any *metal*, they say that it does not come from the *metal*, but from a decomposition of the *water* that is present, and which they say consists of two elements, viz. *oxygen*, or the base of dephlogisticated air, and *hydrogen*, or the base of inflammable air, in the proportion of 85 parts of the former to 15 of the latter.

1. The most simple of the experiments that I have proposed for discussion, with a view to decide concerning the merits of these two theories, and which I cannot help thinking furnishes an argument no less than *demonstrative* of the fallacy of the antiphlogistic hypothesis, 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. The phlogistians say it comes from the *iron*, and the antiphlogistians from the *water*. But to this I object 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 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

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 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 ſa- " tisfactory evidence that iron, after its ſolution in ſul- " phuric acid is in a ſtate like that of the black oxyd, " or finery cinder." 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, as *minium*, and ſome other ſubſtances which contain dephlogiſticated air, do, 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 *mangancſe* gives more evidence

evidence of its right to the name they have given to it, tho', according to them, it contains much lefs oxygen. It is evident, therefore, that there is no addition of oxygen in this procefs, confequently no decompofition of water in the cafe, and that the inflammable air muft come from the decompofition of the iron.

I have no great objection to admitting that this precipitate from the folution of iron in the vitriolic acid, when it is burned black, is the fame fubftance with finery cinder. But this will appear to be no advantage to the antiphlogeftic theory. Both in this form, and in that of a brown powder, this precipitate has feveral of the fame properties with thofe of finery cinder. They neither of them either gain or lofe any weight by expofure to the greateft heat. When heated in atmofpheric air, they both diminifh, and, as I ufually fay, they phlogifticate it, though very flowly. They alfo equally imbibe inflammable air when heated in it, but with this difference, that the production of water feemed to be greater in the reduction of finery cinder than in that of this precipitate. But the experiment being of no great confequence, I did not give much attention to this circumftance.

There is fomething extraordinary in the manner in which the antiphlogiftians fuppofe that metals become foluble in acids. Mr. Adet fays, p. 60, " Experiments prove that metals, in order to be combin-" ed with an acid, require to be united with oxygen;" and explaining himfelf farther, he fays, " In reality, " a metal not combining with acids but when it is in " a ftate of oxide, and not paffing into this ftate but " by its union with its oxygen, muft neceffarily abforb " oxygen in order to unite with the acid. But " this oxygen can only be fupplied by one of thefe " two fubftances, the acid itfelf, or the water which " it contains. If the oxygen had been given by the " acid, it would have been in part decompofed, and

" would

" would in consequence have saturated less alkali.
" But since it saturates the same quantity of alkali, 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; for if the oxygen of the water be seized by the metal, the hydrogen of the water must escape in the form of 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.

2. Inflammable air is procured when one metal is precipitated from its solution by another in its metallic state. This is a fact that is very easily explained on the supposition that the metal precipitated does not require so much phlogiston as that which is dissolved; but the doctrine of the decomposition of water cannot, as far as I see, account for the fact, at least in an easy and natural way.

When zinc is used to precipitate lead from a solution of sugar of lead, inflammable air is procured; and according to the phlogistic theory it ought to be so; since lead contains much less phlogiston than zinc, so that when the former is revived by means of the latter,

latter, it is able to furniſh more than is requiſite for the purpoſe. But if this inflammable air came from the decompoſition of the water, the oxygen, which muſt be developed at the ſame time, ought to be found either in the water, or in what remains of the zinc. For it will not be pretended to be in the *lead* that is revived, and there are no other ſubſtances preſent.

Iron, I alſo find, will yield more inflammable air by ſolution in acids than zinc; and a ſaturated ſolution of iron in the marine acid yields inflammabl air by the ſolution of zinc.

To the arguments in this ſection to prove that metals are compound ſubſtances, and contain phlogiſton, I ſhall add the following.

My experiments prove to demonſtration that nitrous acid is wholly compoſed of dephlogiſticated and nitrous air; ſince when they form this acid, they unite without any reſiduum, or ſo ſmall as not to enter into any computation. Had there been any phlogiſticated air in either of theſe component parts of the acid, it would have appeared on their uniting, and thereby loſing their aerial ſtate. For as neither of them will unite with it, it muſt then have appeared in its proper form. If, therefore, in any proceſs phlogiſticated air be formed by means of nitrous air, one eſſential ingredient in the conſtitution of that air muſt come from another ſource; and all that can be ſaid is that the nitrous air furniſhed one component part of it.

But phlogiſticated air is produced by heating iron in nitrous air. Something, therefore, muſt come from the iron in order to form it, and conſequently iron cannot be a ſimple ſubſtance; and if iron be a compound, it will not be queſtioned but that other metals muſt, from analogy, be compounds too; and ſince nitrous acid can be formed by means of both in-

C flammable

flammable and phlogisticated air, the same principle, which is denominated *phlogiston*, must enter into them both.

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 taken 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 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 not oxygenate it. Let us, however, hear the account that my opponents give of this circumstance. Mr. Adet says, p. 55. "The nonoxygenation of the muriatic acid by the solution of finery cinder is owing to the latter retaining 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

been able dissolve this substance, this might have been said with some degree of plausibility; but since it does dissolve it compleatly, 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.

Messrs. Berthollet and Fourcroy say that "finery "cinder, like massicot, is unable to dephlogisticate "marine acid, because it contains no more oxygen "than is necessary to its solution; whereas the metals "that have got a greater proportion of oxygen, give "out what they have that is superabundant to a part "of the muriatic acid, which by that means becomes "oxygenated," (*Annales de Chymie*, vol. 26, p. 305) evidently taking it for granted, that finery cinder, like massicot, contains but little oxygen, whereas, if it contain any, it must be much more than any other substance in nature.

Dr. Maclean makes very light of this, as indeed he does of every other difficulty. "It certainly" he says, p. 10, "does not follow that because muriatic "acid can separate a certain quantity of oxygen from "lead, when this is combined with a great quantity of "that substance, that it should likewise separate oxy-"gen from iron, when this is united to a compara-"tively small quantity." But finery cinder, if, as all antiphlogistians say, it owes all its additional weight to pure oxygen, which it gained from the water which it had decomposed, must contain much more of it than lead in any state. 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 antiphlogistians "suppose the addition made to iron to be oxygen, be-

"cause

"cause the compound resembles in every respect, as Dr. "Priestley himself allows, that substance which is formed by burning iron in oxygenous gas, or in atmospheric air. And this they consider as an oxyd, because while it is forming 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 quanty of oxygen may enter the iron along with the water to which it was united; as few substances are 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 circumstance, 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.

That a very small quantity of oxygen is attached to the scales of iron, I have thought probable from a

barely

barely perceivable quantity of fixed air which I have ſometimes found when they are revived in inflammable air. But ſo ſmall a quantity as this makes nothing for the new theory.

That finery cinder does not dephlogiſticate marine acid is, I acknowledge, no abſolute proof that it contains no oxygen; becauſe this effect is not always produced by red precipitate, which is known to contain a great proportion of oxygen, nor by flowers of zinc, or maſſicot, which, I doubt not, contain ſome. On the firſt pouring of marine acid on red precipitate freſh made, I have had an evident ſmell of dephlogiſticated marine acid, but not afterwards. Alſo, the black powder of mercury and lead, which gives pure air by heat, does not dephlogiſticate marine acid, tho' it makes it give an offenſive ſmell. But if it be conſidered how much more oxygen, according to the antiphlogiſtic theory, is contained in finery cinder than in any other ſubſtance, it will appear to amount to little leſs than a demonſtration of its containing none, that it has not this effect. From an ounce of red precipitate, or of minium, about 60 ounce meaſures of dephlogiſticated air may be expelled by heat, which is not more than about a thirtieth part of their weight. But if all the addition gained by iron, when it is converted into finery cinder, be pure oxygen, it amounts, as I have obſerved, to near one third of its weight: which is almoſt ten times more than is contained in either of the other ſubſtances.

Beſides, there is other evidence of all theſe ſubſtances containing oxygen, not only when expoſed to heat, but, with reſpect to the red precipitate, when diſſolved in marine acid; and there is no evidence of any kind that finery cinder contains this principle.

The ſolution of red precipitate, heated with a burning lens in atmoſpherical air, cauſes an addition to its quantity, from the dephlogiſticated air expelled from

from it; whereas, when the ſolution of finery cinder is treated in the ſame manner, the contrary effect is produced. The quantity of air is diminiſhed, and the remainder is leſs pure than before. The ſame was alſo the conſequence of heating the ſolution of iron in the ſame circumſtances, that of finery cinder precipitated by cauſtic volatile alkali, and of iron itſelf treated in the ſame manner.

Since, therefore, finery cinder both in this ſolution and without it has the ſame effect on the atmoſpherical air in which it is heated that iron has, I conclude that they both contain the ſame principle, tho' the finery cinder has much leſs of it than the iron. The ſame is probable from finery cinder being in ſome degree attracted by the magnet. So far, therefore, is finery cinder from containing any oxygen, that it contains ſome of the oppoſite principle.

Another probable evidence of a calx containing oxygen, or dephlogiſticated air, is that when it is revived in inflammable air, fixed air is produced. But this is not the caſe when finery cinder is revived in theſe circumſtances, tho' I purpoſely prepared ſome by melting iron in the open air; in which caſe I had imagined that ſome pure air would be attached to it.*

Since an iron tube is diſſolved by heating mangaueſe in it, I thought it very poſſible that ſome dephlogiſticated air from this ſubſtance might unite with the iron, and therefore that the finery cinder made in this manner might be found to contain ſome. But when I heated ſome iron affected in this manner in inflammable

* In making this finery cinder I obſerved that ſteel gained no ſenſible addition of weight in the proceſs, much leſs than when it is made by means of ſteam in a cloſe veſſel. When it was procured in a glaſs receiver, ſtanding in water, it gained ſome weight; but when it was done over mercury, the addition to its weight was little or nothing.

mable air I did not find any fixed air in the residuum; so that it appeared to have got nothing but water from the manganese, being the same thing with the finery cinder made by means of steam.

Because the calx of mercury derives its additional weight from dephlogisticated air, the antiphlogistians have too hastily concluded that all metallic calces derive their additional weight from the same cause. But this is not by any means a just inference. For the calces of some metals are, in this and other respects, very different from one another, and even the different calces of the same metal.

Finery cinder, for example, is a very different thing from the common *rust of iron*, consisting of different principles. From finery cinder nothing can be got by mere heat, but from the rust of iron a large quantity of fixed air is got in the same process. From 1277 grains of rust I got 45 ounce measures of air, of which only about one thirtieth part was not fixed air.

The addition that is made to iron by rusting in the open air I do not find to be more than 30 or 40 grains to an ounce; whereas the addition to an ounce of iron when it is converted into finery cinder is about 200 grains.

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 hydrogen gas, that "disappears, no oxygen gas is obtained, but a quan-"ty 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

flammable 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 veſſel with the oxygen expelled from the calx, they ought, ſurely, to unite in the ſame proportions, in order 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. It will, however, be ſhewn that it does not always form any union with the inflammable air, but remains mixed with it, ſo as to occaſion dangerous exploſions.

Mr. Lavoiſier and his aſſociates obſerve (*Report* p. 300) that when a calx is revived in inflammable air more water is found in the veſſel than the weight of the inflammable air that diſappears, ſo that it could not have been contained in that air.

In this they only refer to my experiments in general; but as they ſpeak of the water produced as appearing both on the inſide of the veſſel and on the ſurface of the mercury, it can be no other than the experiment of the revival of iron from finery cinder; and the water

ter that is found in this procefs was never fuppofed by me to come from the little that is contained in the inflammable air, but from the much greater quantity contained in the cinder.

Before I conclude this fection concerning finery cinder, I muft take notice of what Dr. Maclean, too confidently advances about it. "The Doctor," he fays, p. 26, "is certainly miftaken in fuppofing that "finery cinder cannot ruft. Mr. Fourcroy fays it "rufts fooner than common iron, and every apothe-"cary knows it does fo. If the ruft of iron be made "red hot in a retort, a quantity of carbonic acid is "difengaged from it, and the iron remains in a ftate of "black oxyd. The ruft, therefore, is a *carbonate of* "*iron*, and muft contain all the principles which "compofe the black oxyd, and therefore can contain "nothing capable of excluding that which would con-"vert it into ruft." But in direct contradiction to what he afferts, I ftill fay that finery cinder is not fubject to ruft. In England no ufe having been made of it before it was attended to by my brother-in-law, Mr. John Wilkinfon, (one of the moft intelligent and fuccefsful of all the iron-mafters in that or any country) but to mend the roads, it has lain in heaps for years, I may even fay ages, without acquiring the leaft tinge of brown. All my fpecimens have ever remained free from ruft, and the phyficians, who are alfo apothecaries, in this place, affure me they never faw or heard of any fuch thing. They get it from the blackfmiths in the form of *fcales of iron*, and the blackfmiths fay the fame. It muft, therefore, as I have obferved, be faturated with fome principle very different from that of the common ruft of iron, and is by no means the fame thing, notwithftanding what Dr. Maclean fays to prove the contrary. If finery cinder be ever converted into ruft, which I have never found to be the cafe, it muft, by fome procefs or other, natural or artificial, have been firſt converted into iron, in which caſe it muſt loſe much of its weight.

SECTION III.

Of inflammable Air from finery Cinder and Charcoal.

IF inflammable air, or hydrogen, be nothing more than a component part of water, it could never be produced but in circumſtances in which either water itſelf, or ſomething into which water is known to enter, is preſent. But in my expriments on heating finery cinder together with charcoal, inflammable air is produced, though, according to the new theory, no water is concerned. According to this theory, finery cinder, called the *oxide of iron,* conſiſts of nothing beſides iron and oxygen; and the charcoal, made with the greateſt degree of heat that can be applied, is equally free from water; and yet when theſe two ſubſtances are mixed together, and expoſed to heat, they yield inflammable air in the greateſt abundance.

This fact I cannot account for on the principles of the new theory; but nothing is eaſier on thoſe of the old. For the finery cinder containing water, as one of its component parts, gives it out to any ſubſtance from which it can receive phlogiſton in return. The water, therefore, from the finery cinder uniting with the charcoal makes the inflammable air, at the ſame time that part of the phlogiſton from the charcoal contributes to revive the iron. Inflammable air, of the very ſame kind is procured when ſteam is made to paſs over red hot charcoal.

Since

Since inflammable air, and in great quantity, is rocured in this proceſs, the Antiphlogiſtians are un-r a neceſſity of finding *water*, by the decompoſiti-n of which, and in no other way, they ſay it is ade; and ſome of them find it in the charcoal and hers in the finery cinder.

As Dr. Woodhouſe repeated this experiment ith peculiar exactneſs, I ſhall copy his account of it om the *Philoſophical Tranſactions of Philadelphia*, ol. 4, p. 464. " An ounce of the ſcales of iron, and the ſame quantity of charcoal, were reduced to a very fine powder, and expoſed ſeparately in co-vered crucibles in an air furnace well ſupplied with fuel for five hours. They were then taken out of the fire, and mixed while red hot, in a red hot iron mortar, were triturated with a red hot peſtle, formed of an iron ram rod, were poured upon a red hot ſheet of iron, and inſtantly put into a red hot gun barrel, which was fixed in one of Lewis's black lead furnaces, and which communicated with the worm of a refrigeratory, a part of a hydropneumatic ap-paratus. Immediately after luting one end of the gun barrel to the worm, 142 ounce meaſures of in-flammable air came over in torrents, mixed with one tenth part of carbonic acid gas."

Nothing more could have been done to exclude ll water from each of the ſubſtances previous to their ixture; and yet we immediately find the effects of ater, as much as if water itſelf had been employed, ſtead of the finery cinder, which no doubt, contain-l it. This experiment I ſhould have expected ight have converted the ingenious author of it him-lf. His explanation of it, however, is ſo unſatis-ctory, that I cannot help thinking the conſiderati-n of it, will go a great way towards the converſion others. For he admits that there really is *water*, d in this great quantity, in the finery cinder.

But

But if we ſuppoſe finery cinder to contain water, and ſo much of it as is neceſſary to form all the air that is produced in this proceſs, both fixed and inflammable, we muſt, ſurely, abandon the moſt fundamental principle of the new theory, which abſolutely requires water to be decompoſed in paſſing over hot iron, the oxygen alone remaining in the iron, and the hydrogen eſcaping in the form of inflammable air; and it is only by comparing the addition of weight acquired by the iron in this caſe, that the proportion between the oxygen and the hydrogen in the compoſition of water is aſcertained. Beſides, how can it be ſuppoſed that water ſhould both be decompoſed, and not decompoſed, in the ſame circumſtances?

To the experiment with the finery cinder and charcoal Mr. Berthollet objects, *Report.* p. 15, that " I probably got more fixed air than inflammable, " that the inflammable air contains much charcoal " diſſolved in it, and that in many experiments char- " coal appears to retain water very obſtinately."

How obſtinately charcoal retains water is eaſily aſcertained. When water only *adheres* to any ſubſtance without entering into it as a conſtituent part, a degree of heat capable of converting it into ſteam, will always be ſufficient to expell it; and the Antiphlogiſtians have not yet ſaid that water is an eſſential part of this *carbone.* This they make a ſimple ſubſtance, and tho' common charcoal is not pure carbone, they do not pretend to ſay that water can be in it except as an extraneous ſubſtance. Perhaps when they find their theory *un peu chancelante*, they may have recourſe to this ſupport.

Meſſrs. Berthollet and Fourcroy, however, ſay that this inflammable air comes from the decompoſition of the " water contained in the charcoal, and which they " ſay cannot be ſeparated from it but by forming a " new

" new combination with it." *Annales de Chymie*, vol. 26, p. 306.

But as water is no constituent part of charcoal, it certainly may be separated from it by heat, without forming any new combination, or undergoing any decomposition.

If it be the water adhering to the charcoal that is decomposed, and the component parts of this water enter into a new combination with the carbone of it, I ask of what use is the finery cinder in the process, which, however, is essential to the success of it; and why might not the same heat have the same effect in decomposing this water, without the finery cinder, as well as with it?

They do not say they have any occasion for the *oxygen* contained in the finery cinder, which, however, leaves it in this process; since the iron is revived; and how can they account for the separation of this oxygen from the iron without the supposition of something going in to take its place. Heat alone will not effect this. For heat tends to unite, and not to separate them.

In whatever manner this water, adhering to the charcoal, contributes to the formation of inflammable air, Mr. Berthollet himself would say, that when any particular degree of heat would not make charcoal yield any more inflammable air, there was no more water retained in it than the same degree of heat was able, with its assistance, to decompose. But after this, by the assistance of finery cinder, with even a much less degree of heat, it yields inflammable air very copiously, just as if steam had been made to pass over it in that heat; and, judging from evident appearances, there cannot be a doubt but that, with a sufficient quantity of finery cinder to supply it with water, all the phlogiston in the charcoal, exclusive of

that

that which contributed to the revival of the iron, would be converted into inflammable air.

As to the proportion between the fixed and inflammable air procured by this process, it is about the same with that procured from charcoal by means of steam, and will probably vary with the proportion of finery cinder, as that does with more or less water.

That finery cinder contains nothing but water appears not only from its enabling charcoal to give out air exactly as water would do, but from its doing the same with respect to *terra ponderosa aerata*, which also gives out air by means of water, but not without.

I mixed a quantity of this substance, reduced to a powder, with pounded finery cinder, and in a gun barrel, heated red hot, I got from it fixed air as copiously as if steam had passed over it. There was a considerable residuum of inflammable air from the iron.

When I first made this experiment with charcoal and finery cinder, I remember Mr. Watt said, it was one that the Antiphlogistians could never reconcile to their hypothesis; and the more I consider it, and the objections that have been made to it, the more reason I see to be of his opinion.

SECTION

SECTION IV.

Of the Calces of Zinc.

THE only circumstance that gives any plausibility to the opinion of finery cinder being an oxide of iron is the addition that is made to the weight of the iron when it is converted into this calx. But when zinc is treated in the same manner, steam being sent over it in a red heat, tho' inflammable air is procured, the zinc gains no addition of weight; so that in this case there is no pretence whatever for saying that the water is decomposed.

The substance that is produced in these circumstances I have somewhere called *flowers of zinc* because it is a calx of zinc; and at that time I presumed that it must have all the properties of the common flowers of zinc, and contain oxygen. But I have treated this peculiar calx of zinc, made without access of air, in all the methods that I can think of, without being able to find any appearance of oxygen in it, any more than in finery cinder. When I heated it in common air, the air was not increased but diminished, the very same effect that is produced by the finery cinder.

Having put an ounce of zinc into a glazed earthen tube, to which I gave a red heat, I made steam pass over it till I had procured 300 ounce measures of inflammable air, after which I found the greatest part of the zinc reduced to a dark coloured semitransparent glass, adhering pretty closely to the tube. I was able, however, to separate them, and I am confident that the calx did not weigh more than the metal had done; whereas, computing from the proportion of 85 parts of oxygen to 15 of hydrogen, (into which it is

said

ſaid that water is reſolvable) it ought to have gained about a hundred grains. Since, then, this great proportion of oxygen is not found either in the calx, or in the water (for this alſo I examined) where will the Antiphlogiſtians ſay that we are to look for it? For ſince the water, they ſay, is decompoſed, in order to furniſh the inflammable air, it ought to be found ſomewhere.

Another experiment that I made with zinc proves, that when inflammable air is procured by means of it, it muſt come from the metal, and not from any water.

On throwing the focus of a burning lens on a quantity of zinc in common air, confined by water, in a glaſs veſſel, the firſt effect is the production of *flowers of zinc*, which make a beautiful appearance, by their diſperſion within the veſſel; and during this part of the proceſs the air is diminiſhed, the pure part of it, no doubt entering the calx, while the phlogiſticated part remains unaffected. After this, the application of the heat being continued, there is an increaſe of the quantity of air by the production of inflammable air; and inſtead of flowers of zinc, a *black powder* ariſes, and adheres to the inſide of the veſſel, and with care may be collected.

Now, ſince inflammable air is produced, the antiphlogiſtians muſt ſay, that part of the water over which the experiment was made, was decompoſed. But then I aſk, where is the oxygen which, according to them, conſtitutes the far greater part of the water? I cannot find it any where. The *water* is entirely free from acidity, and the air expelled from it afterwards is even leſs pure than that which it yields before the proceſs. And if I examine the *black powder*, (which muſt be the metal ſublimed) by heating it in confined common air, it becomes a whitiſh ſubſtance, the air is diminiſhed, and rendered in a conſiderable degree impure;

impure; whereas, if it had contained any oxygen, the quantity would have been increaſed, and it would have been purer than common air; as when *red precipitate*, or *minium*, is treated in the ſame manner. It is evident, therefore, that it contained no oxygen, but a quantity of phlogiſton, on the expulſion of which, and the imbibing of pure air, it became flowers of zinc.

This experiment is rather more deciſive than the ſimilar one with iron; becauſe the black powder to which zinc is reduced can be affected by heat in common air, which finery cinder cannot.

It will hardly be pretended that the oxgen ariſing from the decompoſition of the water is lodged in the flowers of zinc; ſince they were completely formed before any inflammable air was procured. Beſides, it will appear that little or no oxygen can be found in flowers of zinc produced in any proceſs.

As I could not find any oxygen in the precipitates of iron diſſolved in acids, I have not been able to find any in thoſe of zinc. The moſt unexceptionable that I could think of is that by cauſtic volatile alkaki. This ſubſtance I heated in atmoſpherical air, both moiſt and dry (leſt expoſure to the atmoſphere ſhould have made ſome difference in it) but it was with the ſame reſult. The air in which it was heated was made more impure than it was before, tho' in one caſe the quantity was increaſed from $6\frac{1}{2}$ to 8 ounce meaſures. Of this half an ounce meaſure was fixed air, and the remainder of the ſtandard 1. 8. extinguiſhing a candle, ſo that it was almoſt wholly phlogiſticated. It ſeemed, therefore, to have imbibed part of the pure air, and to have given out phlogiſticated air.

Filings of zinc yield much inflammable air in pure water, tho' I do not find that they can by this

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means be reduced to a complete calx.* But the im-perfect calx to which the metal is then reduced, does not appear to contain any oxygen. When it was heated in atmofpherical air, the quantity of the air was increafed, about one twentieth part of it was fixed air, and the remainder was of the ftandard of 1. 5. The water in which the filings of zinc had been im-merfed, gave out air much worfe than common air, and it was perfectly free from acidity. Iron filings will alfo yield inflammable air in water, and this wa-ter alfo gives out air that is more impure than common air, as does the water over which tin and other metals are calcined.

That the calces of metals do in general contain oxygen I have no doubt, becaufe the dephlogifticated air in the atmofphere difappears when they are calci-ned in it. But there is reafon to think that the greateft part of the addition of weight which they by this means acquire is from *water*, while the oxygen attaches itfelf to other fubftances in preference to the calx, if they be prefent.

One inftance of this is that when they are calcined with a burning lens over lime water, the lime is pre-cipitated; whereas if the calx had imbibed all the de-phlogifticated air that difappeared, the lime water would not have been affected in the procefs; this pre-cipitation of the lime, coming, no doubt, from fixed air, which I have fufficiently proved to confift of de-phlogifticated air and phlogifton, or the bafe of in-flammable air. I had this refult when I calcined iron, copper, zinc, tin, lead, bifmuth, and regulus of an-timony in thefe circumftances. But when the procefs was made over mercury, I could not always find any fixed

* Since this was firft printed in the *Medical Repofitory*, I find that, by long ftanding, the furface of thefe filings of zinc is become white, fo that they are perfect flowers of zinc.

fixed air; and therefore I presume that all the oxygen was imbibed by the calx, tho' it may be impossible in many cases to extract it again in that form. For when the quantity is small, it may be so united to the phlogiston remaining in the calx, as to form the basis of phlogisticated air, which I have proved to consist of dephlogisticated and inflammable air.

Lead furnishes an example of this. No oxygen I believe can by any means be got from *massicot*, tho' it has imbibed some. But when this calx is supersaturated with it, and is become *minium*, it will yield the purest dephlogisticated air by heat only, and will likewise dephlogisticate marine acid. And since flowers of zinc will not dephlogisticate marine acid, I presume that this calx also is nearly in the same state with massicot in this respect; and that in any state it contains but little oxygen, or so united to phlogiston, as not to be extracted either in the form of acid, or of dephlogisticated air.

Tho' the flowers of zinc may contain some oxygen, I have not been able to discover any in them by any process that I have made use of for the purpose. As this substance is formed in a considerable degree of heat, I was not surprized to find that heat would not expel any thing from it; but I thought that when it was mixed with iron filings it might, with them, yield some fixed air, as red precipitate does. But I did not find this to be the case. I got nothing in this process besides inflammable air. Also, when mixed with perfect charcoal, such as gives no air with heat, a great quantity of both fixed and inflammable air is produced; which shews that, like this substance, flowers of zinc contain little or nothing besides water, which will have just the same effect.

To make this experiment with fairness, the iron filings must be heated till they give no air. They must then be well washed, till the water put on them

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be quite clear, and be again found to give no fixed air with heat. For foreign ſubſtances are very apt to be mixed with iron filings, and this proceſs will ſeparate them. With iron filings thus prepared red precipitate gave fixed air, but flowers of zinc none.

There is a grey calx of zinc, ſimilar to that of lead, which Mr. Chaptal calls a *perfect oxyd*. This I find to be only zinc partially calcined. For on heating it in atmoſpherical air it became white, the air was diminiſhed, was without fixed air, and conſiderably phlogiſticated. The perfect flowers of zinc treated in the ſame manner made no ſenſible change in the quantity of the air; but, as in the former caſe, there was no fixed air in it, and it was conſiderably phlogiſticated.

The melting of maſſicot in theſe circumſtances made no change of any kind in the air, which ſhews that it contains no more phlogiſton than flowers of zinc.

Oxygen in a calx is perhaps moſt eaſily detected by its forming fixed air when it is heated in inflammable air; but I did not find this to be the reſult of an attempt to revive flowers of zinc in thoſe circumſtances. Owing to the whiteneſs of this ſubſtance, which diſpoſes it to reflect, and not to abſorb, the light that is thrown upon it, I could not revive any part of this calx completely. A black ſpot only was made on a part of it, and about an ounce meaſure of inflammable air was imbibed; but I found no fixed air in the remainder, any more than I did when I revived finery cinder in the ſame proceſs.

SECTION

SECTION V.

Arguments in Favour of the Doctrine of Phlogiſton from ſome Circumſtances in which Sulphur is formed, and nitrous Acid phlogiſticated.

1. AN argument may, I think, be drawn in favour of the doctrine of phlogiſton from my experiment of the formation of ſulphur, from the acid of vitriol heated in inflammable air, and alſo from water impregnated with vitriolic acid air, expoſed to a continued heat.

Sulphur, the Antiphlogiſtians ſay, is a ſimple ſubſtance, and that the vitriolic acid is that ſubſtance with the addition of oxygen, or dephlogiſticated air. Why, then, I aſk, is not ſulphur produced when dephlogiſticated air is expelled from it by heat, rather than in the proceſs with water impregnated with vitriolic acid air? For when this air is procured by making the acid paſs thro' a red hot earthen tube, no ſulphur is found. But when it is heated to dryneſs in inflammable air, which can ſupply it with phlogiſton, ſulphur is formed.

The production of *phoſphorus* from the phoſphoric acid heated in inflammable air furniſhes the ſame proof of this ſubſtance alſo being a compound, and that phlogiſton enters into the compoſition of it, as well as into ſulphur.

According to the phlogiſtic theory, the formation of ſulphur from water impregnated with vitriolic acid air is very eaſy; both the ingredients of which it is compoſed being preſent, viz. its baſis, vitriolic acid, and

and phlogiston. They are only made to form a different mode of combination by the heat in a tube hermetically sealed. For the vitriolic acid air is produced by heating in vitriolic acid most of the metals, or any other substance, solid or liquid, that is said to contain phlogiston.

If it be said that the sulphur may be formed in this experiment by the heat separating the acid from its base; I answer that then the remaining water should be more acid than before; whereas I find it to be less so. This diminution of acidity I account for from the extreme volatility of this phlogisticated acid. But had the acid been that of vitriol unphlogisticated, it would have been obstinately retained by the water. Besides, it would, surely, be more easy to expel all acid from a liquor passing thro' a red hot open tube, than from a liquor confined in a glass tube hermetically sealed, so that it cannot possibly escape; and when it is exposed to no more than a moderate degree of heat. For had it approached to a red heat, the tube would have burst.

But the formation of sulphur and phosphorus, by heating the vitriolic and phosphoric acids, so as to evaporate them to dryness, in inflammable air, which then disappears, and this effect not being produced without it, or some other substance containing phlogiston, is, I think, decisive in favour of their receiving an addition of something from the inflammable air, or phlogiston, when they are converted into sulphur and phosphorus; and therefore that these substances are the compounds, and the acids the more simple substances of the two.

2. It is said by the Antiphlogistians that the nitrous acid never becomes coloured by imbibing any thing, but always in consequence of giving out oxygen. I think, however, that the contrary is proved by its absorbing nitrous air, which it does with great rapidity.

rapidity. But the ſame effect is produced, tho' not in ſo remarkable a manner, by means of inflammable air.

I put a quantity of dephlogiſticated nitrous acid into a phial with a ground glaſs ſtopper, with inflammable air on its ſurface; and in another ſimilar phial atmoſpherical air was confined with it. Both theſe phials I covered with water in inverted glaſs jars, to prevent their having any communication with the atmoſphere. After long expoſure in theſe circumſtances, that which had the common air on its ſurface never acquired any colour, or only a very little, from the effect of *light* tranſmitted thro' two glaſſes with water between them; but that on the ſurface of which inflammable air was incumbent acquired colour very ſoon. I alſo found, on repeating the experiment, that a part of the inflammable air had been imbibed by the acid. In order to make this experiment, a phial filled with the acid muſt be introduced into a jar of inflammable air; and, part of it being poured out, the ſtopper muſt be put into it in that ſituation. Other precautions muſt be uſed which a little experience will teach.

SECTION

SECTION VI.

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.

As there are calces of mercury which certainly imbibe inflammable air, this ſubſtance, or the baſe of it, phlogiſton, muſt be concluded to exiſt in that metal as an element. This is true both with reſpect to red precipitate, and turbith mineral.

As to the calx of mercury from the acid of vitriol, Mr. Beaume *, 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 "obſervations," Dr. Maclean ſays, p. 11, "ſhew that "the turbith mineral, or any other ſubſtance into "which it may be converted by a red heat, does not "require any addition to conſtitute it a metal," And Mr.

* With Mr. Beaume 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.

Mr. Adet says, p. 43, "that the yellow oxide of mer-"cury has been revived without addition by Messrs. "Monnet, Bouquet, Lavoisier, 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 result of my experiments on this precipitate before I left England. 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.

A decisive evidence, as it appears to me, that mercury contains phlogiston is the absorption of a great proportion of inflammable air in the revival of red precipitate in it.

By means of a burning lens I heated a quantity of red precipitate in inflammable air, in a glass vessel confined by water, till 121 ounce measures of the air were reduced to 95. Then, examining the residuum, I found that one measure of it mixed with an equal quantity of nitrous air occupied the space of 1.77

 measures.

measures. Computing from this result, it will be found that it contained 7. 22 ounce measures of pure air which added to the 26 which had disappeared make 33. 22 ounce measures of inflammable air which had been absorbed by the calx in its revival. For that the air expelled from the calx had not contributed to the formation of *water*, was evident from its being found mixed with the remainder of the inflammable air. Neither had it, in this case, contributed to the formation of fixed air. For there was no sensible quantity of this air found in it, tho' I have sometimes found a little of it in this process. Nor can this difference in the result be thought extraordinary, when it is considered that fixed air certainly consists of pure air and inflammable air, and that it is found in other processes similar to this.

In another experiment of this kind I revived a quantity of the precipitate in 30 ounce measures of inflammable air, till 12 ounce measures disappeared, and the standard of the remainder, examined as in the preceding case, was 1. 75. From this it appeared that 1. 495 ounce measures of air had been expelled from the calx, and that 13. 495 ounce measures of inflammable air had been imbibed by it.

Since much of the calx was sublimed in the process, the best method of ascertaining how much inflammable air is imbibed in the revival of a given quantity of mercury, is to compare the quantity of pure air that is yielded by a given quantity of the calx with the quantity of inflammable air that corresponds to it in these experiments. Now an ounce of the precipitate yields about 60 ounce measures of pure air; and since in these experiments 46. 71 ounce measures of inflammable air were absorbed when 8. 71 ounce measures of pure air were emitted, 60 ounce measures could not be expelled without the absorption of 323 ounce measures of inflammable air; and since mercury gains, as Mr.

Mr. Chaptal ſays, about 8 per cent. in being converted into precipitate, an ounce of mercury muſt contain 362 ounce meaſures of inflammable air, or rather the phlogiſton that enters into it. An ounce of lead, I have ſhewn, requires 108 ounce meaſures of inflammable air, an ounce of biſmuth 185, of tin 377, of copper from verditer 403, and of iron 890.

That mercury revived either by inflammable air or in cloſe veſſels has the ſame properties will not be denied; and if ſo, it muſt conſiſt of the ſame principles, and in the ſame proportions, or nearly ſo. I am therefore inclined to think, improbable as it may appear, that the ſame principle which is eſſential to the conſtitution of inflammable air, that is phlogiſton, paſſes from the fewel thro' the glaſs when the calx is revived by heat in a glaſs veſſel.

There is, however, only the choice of this difficulty, and of that of an ounce of mercury containing either 362 ounce meaſures of inflammable air (that is the phlogiſton in it) or none at all. It is not denied that *light* and *heat*, both of which are allowed to be *ſubſtances*, tho' the weight of them cannot be aſcertained, paſs thro' glaſs. They both have certain properties, and are transferable from one ſubſtance to another, according to their known affinities. And why may not this be the caſe with *phlogiſton* alſo. Light certainly paſſes thro' glaſs; and is known to give to ſome ſubſtances colour, ſmell, and taſte, which have uſually been aſcribed to phlogiſton. That it does not revive the lead in paſſing thro' the hot flint glaſs is no ſufficient objection. For the ſame ſubſtances in different combinations, and in different ſtates, have different properties. The doctrine of chemical affinities has yet many difficulties attending it, and it requires the niceſt diſcrimination of circumſtances to make conſiſtent tables of them. However, I can only propoſe *facts*, let others account for them in the beſt manner that they

they can Mr. Scheele fuppofed that even dephlogifticated air, or the effential element of it, paffed thro' glafs.

I have frequently repeated this experiment of the revival of precipitate in inflammable air, and have never failed to find a great abforption of it, whether there was any fixed air in the remainder or not; and I fhould have repeated it much oftener, and on a larger fcale, in order to afcertain with more exactnefs the quantity of inflammable air, or of phlogifton, contained in a given quantity of mercury, but that it has frequently happened that the veffels in which I made the experiments were exploded, after a fufficient quantity of pure air was expelled from the calx. This accident, however, is a proof that the air expelled from the precipitate had not formed either water or fixed air. Sometimes, however, I have made the greateft part of the inflammable air to difappear without any explofion.

The accuracy of this experiment being queftioned by Dr. Woodhoufe, I repeated it with all the attention I could give to it, and had the following refult. I heated a quantity of red precipitate in $28\frac{1}{2}$ ounce meafures of inflammable air till it was reduced to $24\frac{1}{2}$ and found that, whereas before the procefs it was not the leaft affected by nitrous air, the ftandard of it afterwards was 1. 8; fo that it contained a confiderable mixture of dephlogifticated air. I repeated the experiment feveral times, and always found pure air mixed with the inflammable, when I had revived any part of the calx.

Continuing one of thefe proceffes till, after the dimunition, the quantity of air began to increafe, there was an explofion; but it only raifed the receiver in which the air was confined about an inch, and recovering its pofition, it broke the earthen difh in which it was placed.

After this, I made ufe of a tin difh, and repeating the

the experiment, there was an exploſion ſo loud, that a perſon at a conſiderable diſtance was alarmed, and came running to ſee what had happened. The receiver, which was a very heavy one, was blown much higher than my head; but falling on the graſs was not broken. After this, I thought it unneceſſary to make any more experiments of the kind.

Having formerly made many experiments on the revival of red precipitate in inflammable air, when I was a convert to the doctrine of the compoſition of water, I ſhall ſubjoin what I then obſerved with reſpect to the ſubject from the 6th volume of my *Obſervations on air*, p. 128.

" The greateſt difficulty that occurred with reſpect " to the theory of the conſtitution of water, aroſe from " my never having been able to procure any *water* when " I revived mercury from red precipitate in inflamma- " ble air, or at leaſt more than may be ſuppoſed to have " been contained in the inflammable air. In order " to make the experiment with the *ſcales of iron*, and " that with the *precipitate*, as much alike as poſſible, " and that I might compare them to the greateſt ad- " vantage, I made them immediately one after the o- " ther, and with every circumſtance as nearly as I could " the ſame. The inflammable air was the ſame in both " the experiments, and the ſcales of iron, and the pre- " cipitate, were made as dry as poſſible. They were " heated in veſſels of the ſame ſize and form, and e- " qually confined by dry mercury. And yet when I " heated the former, water was formed as copiouſly as " I have deſcribed it before, viz. actually running " down the inſide of the veſſel in drops, tho' only four " ounce meaſures of inflammable air were abſorbed. " But tho' I heated the precipitate till eight ounce mea- " ſures of the air was abſorbed, and only three fourths " of an ounce meaſure remained, there was hardly a- " ny ſenſible quantity of water produced, certainly

" not

" not one tenth of what appeared in the experiment " with the fcales of iron. There was this difference, " however, in the two refults. In what remained from " the experiment with the precipitate I at this time " perceived a flight appearance of fixed air whereas " there was none in what remained from the fcales of " iron. The refiduum alfo from the precipitate had " in it a fmall portion of dephlogifticated air. For " being mixed with an equal meafure of nitrous air the " ftandard of it was 1. 8. In this experiment there " can be no doubt but that the dephlogifticated air " diflodged from the precipitate mixed with the inflam- " mable air in the veffel, and as no water was produ- " ced, they muft have formed fome more folid fub- " ftance, which in the fmall quantity I was obliged to " ufe could not be found."

At this time, however, I think it more probable that nothing *folid* was produced, but only that the phlogifton of the inflammable air was imbibed by the calx, while the pure air emitted from it was in part found mixed with the inflammable air in the veffel, and in part united with it and formed fixed air.

In nine ounce meafures of inflammable air from malleable iron and water I revived part of the precipitate fent me by Mr. Berthollet, (which I had found to contain no fixed air,) till not more than one fourth of the air remained unabforbed; and examining it, I found about one twentieth part of it fixed air. But mixing nitrous air with it, it appeared that the air diflodged from the precipitate had not wholly united with the inflammable air. For being mixed with an equal quantity of nitrous air it occupied the fpace of 1. 71. After the procefs I miffed 18 grains of the precipitate. But there are feveral caufes of lofs in this cafe, befides that from the air expelled from it.

In 5. 5 ounce meafures of the fame inflammable air I again revived fome of the fame precipitate till it

was

vas reduced to 0. 77 of an ounce meaſure. Of this one ſixth part was fixed air, and the reſt of the ſtandard of 1. 6. It exploded at once when the flame of a candle was preſented to it.

In making theſe experiments over mercury we neceſſarily uſe but ſmall quantities of air, and therefore the reſults may not in ſome reſpects, be ſo much depended upon. But I think it ſufficiently appears rom them that no *water* was formed in the proceſs, nd this the new theory abſolutely requires.

On the whole, I think it can hardly be denied hat conſidering the great quantity of inflammable air that diſappears in theſe experiments, the greateſt art of it, at leaſt, muſt enter into the calx. And nce all running mercury muſt conſiſt of the ſame ements, the ſame principle that (with the addition f water) forms inflammable air, and which we call hlogiſton, muſt paſs thro' the red hot glaſs when he calx of mercury is revived without addition, by eans of heat only.

Some experiments that I have made on ſilver, old, and platina, favour this hypotheſis. All theſe etals yield a conſiderable quantity of nitrous air, hen they are diſſolved, the firſt in nitrous acid, and e two laſt in aqua regia. And when the ſolutions ere evaporated, and the reſiduums heated in inflamable air, a great quantity of it diſappeared, and the etals were revived. And yet by means of the ſame ids theſe dry reſiduums will yield a great quantity of trous air. They muſt, therefore, have acquired, by eans of heat only, and this tranſmitted thro' a veſſel t red hot, the ſame principle that was communicated them by imbibing inflammable air.

That nitrous air contains the ſame principle with lammable air, or phlogiſton, appears from the folving experiment, in which the former was produced by by means of the latter, if the nitrated calx of any metal be heated in it.

If copper be diſſolved in nitrous acid, and the water be expelled to a certain point, there remains a *green ſubſtance*, which is not at all deliqueſcent; but when expoſed to heat gives out a red vapour. Some of this ſubſtance I heated in 21 ounce meaſures of inflammable air till the veſſel was filled with red vapour, when it was reduced to 6 ounce meaſures, and I found that when it was mixed with common air the ſtandard was 1. 35; ſo that it was almoſt wholly nitrous air. There was in it a ſmall quantity of fixed air, but there was nothing inflammable in it. It extinguiſhed a candle.

I formerly endeavoured to aſcertain the proportion of phlogiſton in nitrous and inflammable air, and found it to be nearly the ſame in both. That this is not far from the truth may, I think, appear from comparing the reſult of two of my former experiments, which I never before thought of doing with this view. When I firſt diſcovered nitrous air, I endeavoured to find what quantity of it would be yielded by the different metals, and found that 20 grains of iron yielded 16 ounce meaſures. When, with other views, I endeavoured to aſcertain the quantity of inflammable air that was yielded by malleable iron, I found that 120 grains of it yielded 96 ounce meaſures; and this is exactly the quantity of nitrous air that the ſame weight of iron would give. For 120 is to 96 as 20 is to 16.

Twenty grains of platina gave nine ounce meaſures of pure nitrous air, and 22 grains of gold gave 8 ounce meaſures. They therefore contain nearly the ſame proportion of phlogiſton (for 20 is to 9 as 22 is to 9. 9.) and little more than half as much as iron. For it will be in the proportion of 170 ounce meaſures to the ounce. It is, however, more than is contained in lead, but leſs than biſmuth, and much leſs than in mercury.

That

That something can pass thro' glass is evident from many observations respecting both light and heat, one of the most remarkable of which is perhaps that of minium, or red precipitate (which when cold are of the colour of arterial blood) heated in a glass tube, acquiring the dark colour of venous blood, tho' they lose it again when they become cold. What to infer from this curious fact I do not distinctly see.

SECTION VII.

Of the Decomposition of Water.

THE antiphlogistic theory has received its greatest support from the supposed discovery that water is resolvable into two principles, one that of *oxygen*, the base of dephlogisticated air, and the other, because it has no other origin than water, *hydrogen*, or that which, with the addition of *calorique*, or the element of *heat*, constitutes inflammable air. " One of the parts of the " modern doctrine the most solidly established, say " Mr. Berthollet, and the other authors of the *Report* " on this subject (*Examination of Kirwan*, p. 17) is " the formation, the decomposition, and recompositi- " on, of water. And how can we doubt of it, when " we see that, in burning together fifteen grains of in- " flammable air, and eighty-five of vital air, we ob- " tain exactly an hundred grains of water, in which, " by decomposition, we find again the same principles, " and in the same proportions. If we doubt of a truth " established by experiments so simple, and palpable, " there would be nothing certain in natural philoso- " phy. We might even question whether vitriolated

" tartar be composed of vitriolic acid and fixed alkali, " or sal ammoniac of the marine acid and volatile al- " kali, &c. &c. For the proofs that we have of the " composition of these salts are of the same kind, and " not more rigorous, than those which establish the " composition of water. Nothing perhaps more clear- " ly proves the weakness of the old theory, than the " forced explanations that have been attempted to be " given of these experiments."

Notwithstanding the confidence thus strongly expressed by these able and experienced chemists, I must take the liberty to say, that the experiments to which they allude appear to me to be very liable to exception, and that the doctrine of phlogiston easily accounts for all that they observed.

Their proof that water is decomposed, and resolved into two kinds of air, is that when steam is made to pass over red-hot iron inflammable air is produced, and the iron acquires an addition of weight, becoming what is called *finery cinder*, but what they call *oxide of iron ;* supposing that there is lodged in it the oxygen which was one of the constituent parts of the water expended in the process, while the other part, or the hydrogen, with the addition of heat, assumed the form of inflammable air.

But in order to prove that this addition of weight to the iron is really oxygen, they ought to be able to exhibit it in the form of dephlogisticated air, or of some other substance into which oxygen is allowed to enter, and this they have not done. Iron that has really imbibed air, or the common *rust of iron*, has a very different appearance from this finery cinder, being *red*, and not *black ;* and when treated in similar processes, exhibits very different results. Mr. Fourcroy says, (Ib. p. 251.) that this finery cinder is " iron partial- " ly oxygenated." But if that were the case, it would go on to attract more oxygen, and in time become a

proper

proper ruft of iron, completely oxygenated. But this is fo far from being the cafe, that, as I have obferved, finery cinder never will acquire ruft; which fhews that the iron in this ftate is faturated with fome very different principle, which even excludes that which would have converted it into ruft.

However, neither this, nor any other calx of iron, can be revived unlefs it be heated in inflammable air, which it eagerly imbibes, or in contact with fome other fubftance which has been fuppofed to contain phlogifton. The probability therefore is, that the phlogifton then enters this calx of iron, replacing that which had been expelled to form the inflammable air. Nor can any inflammable air be procured in this procefs with fteam, but by means of fome fubftance which has been fuppofed to contain phlogifton Where then, is the certain proof that water is decompofed in this procefs?

Since, according to the antiphlogiftie theory, water itfelf contains all the elements of both dephlogifticated and inflammable air, and wants only *calorique*, which they can give at pleafure, I fee no reafon why heat alone, without the aid of any metal, might not convert it into air. When the particles were fo far feparated as they are in a ftate of fteam, I fee no occafion for the fuperior attraction of any other fubftance for either of them. In fteam each of the elements is already in the form of air, and with its due proportion of calorique, and then why fhould they not continue in that form, only mixed together, ready for explofion?

It is faid that the oxygen imbibed by this iron, being expelled by heat in contact with inflammable air, unites with that air, and with it conftitutes the water which is found after the procefs. But for any thing that appears, this water may be that which the iron had imbibed, and which can only be expell-

ed

ed from it by the entrance of that phlogiston which it had lost. Besides, it has been shewn that the water produced in this manner is much more than in the required proportion of the inflammable air that disappears.

Another pretended proof that water is composed of dephlogisticated and inflammable air, is that when the latter is burned slowly in the former, they both disappear, and a quantity of water is produced, equal to their weight. I do not, however, find that it was in more than a single experiment that the water so produced is said to have been entirely free from acidity, though this experiment was on a large scale, not less than twelve ounces of water being procured. But the apparatus employed does not appear to me to admit of so much accuracy as the conclusion requires; and there is too much of correction, allowance, and computation in deducing the result.

Also, it is, after all, acknowledged that, after decomposing this quantity of the two kinds of air, and making all the allowance they could for phlogisticated air, or *azote*, in the dephlogisticated air, they found fifty-one cubic inches of this kind of air more than they could well account for. This quantity, therefore, and perhaps something more (since the operators were interested to make it as small as possible) must have been formed in the process. And when this kind of air, as well as inflammable, is decomposed together with dephlogisticated air, nitrous acid is produced. The probability therefore is, that the acidifying principle, or the oxygen, in the dephlogisticated air which they decomposed, was contained in that phlogisticated air, and that, had the process been conducted in any other manner, it would have assumed the form of nitrous acid. They akcnowledge that, except when the inflammable air was burned *in the slowest manner*, the water they produced had more or less of acidity.

The

The reafon, no doubt, was that, whenever the flame they made ufe of was too ftrong, more of the dephlogifticated air in proportion to the inflammable was confumed, than when the flame was weak; fo that the refults of their experiments exactly coincide with thofe of mine.

Citizens Berthollet and Fourcroy fay with Mr. Adet that "the fmall quantity of acid which is commonly found in this procefs comes from the *azote*, "which is mixed with the gas." (*Annales de Chymie*, vol. 26, p. 306.) But if this was the cafe, they could never get water free from acid, becaufe they can never wholly exclude azote. Befides, how can they think it fo eafy to procure nitrous acid from azote in this procefs, when Mr. Cavendifh found it fo difficult to procure a barely fenfible quantity by numberlefs electric explofions?

The experiments which I made on the decompofition of thefe two kinds of air in *clofe veffels*, appear to me to be much lefs liable to exception, and the conclufion drawn from them is the reverfe of that of the French philofophers.

When dephlogifticated and inflammable air, in the proportion of a little more than one meafure of the former to two of the latter (both fo pure as to contain no fenfible quantity of phlogifticated air) are inclofed in a glafs or copper veffel, and decompofed by taking an electric fpark in it, a highly phlogifticated nitrous acid is inftantly produced; and the purer the airs are, the ftronger is the acid found to be. If phlogifticated air be purpofely introduced into this mixture of dephlogifticated and inflammable air, it is not affected by the procefs, though, when there is a confiderable deficiency of inflammable air, the dephlogifticated air, for want of it, will unite with the phlogifticated air, and, as in Mr. Cavendifh's experiment, form the fame acid. But fince both the kinds of air, viz. the inflammable

mable and the phlogiſticated, contribute to form the ſame acid, they muſt contain the ſame principle, viz. phlogiſton.

If there be a redundancy of inflammable air in this proceſs, no acid will be produced, as in the great experiment of the French chemiſts, but in the place of it there will be a quantity of phlogiſticated air together with water.

Meſſrs. Berthollet and Fourcroy ſay, with Mr. Adet, that the water procured in this manner cannot be held in ſolution in the gaſſes, but muſt neceſſarily be a new production (*Annales de Chymie*, vol. 26, p. 306.) But I do not ſay that this water was held in ſolution in the gaſſes, but was a conſtituent part of them; and for any thing that is certainly known is all that can be aſcertained by *weight*. I wiſh, however, to have more repetitions of this experiment, in order to aſcertain this curious circumſtance. I was never able to get the whole weight of the airs in water. In my experiments, when no acid is produced a conſiderable quantity of phlogiſticated air is always formed.

When the decompoſition of phlogiſticated and inflammable air is made in a glaſs veſſel, a peculiar *denſe vapour* is formed, which the eye can eaſily diſtinguiſh not to be mere vapour of *water*, and if the juice of turnſole be put into the veſſel, it immediately becomes of a deep red, which ſhews that it was an acid vapour.

Since the acid that I procured in this proceſs was in conſiderable quantity, and no phlogiſticated air was preſent (for in the laſt of the experiments I did not even make uſe of an air-pump, but firſt filled the veſſel with water, and then diſplaced it by the mixture of the airs) I do not ſee how it is poſſible to account for the formation of this acid but from the union of the two kinds of air; and it can hardly be ſuppoſed that, in the very ſame proceſs, the decompoſition of the

ſame

ame substances should compose others so very different from each other as *water* and *spirit of nitre.* I think I have sufficiently accounted for the result of the experiments made by the French chemists on the common hypothesis, which supposes inflammable air to contain phlogiston; but I do not yet see how it is possible for them to explain mine on theirs, according to which there is no such principle in nature. Upon the whole, it does not appear to me that the evidence either for the composition, or the decomposition, of water is at all satisfactory; and certainly the arguments in support of an hypothesis so extraordinary, and so novel, ought to be of the most conclusive kind.

Dr. Maclean boasts greatly 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 extensive scale, great care was taken to ascertain the degree of purity of the gasses before combustion, and the apparatus was so constructed, that the results could be determined with the greatest nicety. The Doctor's on the contrary, were made with very trifling quantities of materials, their purity was not tried, and their weight not accurately determined."

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 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

rity of the gaſſes, whereas with reſpect to mine, he ſays, the purity was not tried. Now that of mine was not only tried, and 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 probably not in the dephlogiſticated, but in the nitrous air, which 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 pro-" duced 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 anti-phlogiſtian would care to drink. Will Dr. Maclean ſay that my proceſs is leſs accurate than that of the French, becauſe it can be finiſhed in leſs than five minutes, and theirs requires the aſſiduous attendance of ſome day?

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 procure water as free from acidity 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 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; which may be as ſatisfactorily aſcertained by drops, as by the largeſt quantities; and till the French chemifts can make their experiments in a manner lefs operofe and expenfive, requiring fewer precautions, and lefs of computation, I fhall continue to think my refults more to be depended upon than theirs.

Mr. Berthollet objects to my experiment, that the weight of the liquor which I produced from the decompofition of dephlogifticated and inflammable air was never equal to the weight of the air. But this I account for by the efcape of the highly phlogifticated acid.

He alfo fays that I took no account of the refiduum of the air in the veffel in which I made the explofions. But I did not overlook this circumftance; fince I meafured the capacity of the veffel by the quantity of air that actually difappeared, by having been completely decompofed in the procefs; fo that there was no occafion whatever to take an account of the air that was not affected by it.

I fhall conclude this fection with obferving that in order to complete their proof of the decompofition of water, the antiphlogiftians fhould produce fome fubftance which, by uniting with hydrogen in water, fhould let go the oxygen, in the form of dephlogifticated air, or of fome acid; and furely fome fuch fubftance might be found, if their theory be true. The component parts of water are not fo intimately united, but that they may be feparated by means of affinities. Otherwife the hydrogen could not have been detached from it.

SECTION

SECTION VIII.

n Argument against the Dceomposition of Water from the different Proportions of the Elements of which it is supposed to consist, according to different Experiments.

ACCORDING to the new theory, water consists of two principles, oxygen and hydrogen; and ey are separated by iron, or charcoal, in a red heat, niting with one of them, and suffering the other to cape; and therefore if in any case a quantity of wa- r be wholly expended in forming air, and only one the kinds be found, it will be evident that this wa- r does not consist of two elements. Now according one of my experiments water would appear to consist only one of the kinds of air, and according to ano- er of the other.

I have shewn that by a slow supply of water in nding steam over red hot charcoal, the whole of e produce is inflammable air, without any mix- re of fixed air, or the production of any thing, ae- l, fluid, or solid, into which oxygen can be sup- sed to enter. From this experiment, therefore, nducted in this manner, it might be concluded that ter consists of hydrogen only, without any oxygen.

This observation of mine is confirmed by Mr. att, whose accuracy no person will call in question. e says in his *Description of a pneumatical apparatus*, njoined to *Dr. Beddoes's Considerations on the medicinal of factitious air*, p. 84 "It has been observed by Dr. Priestley, and confirmed by my experience, that when much water passes in the form of steam, there

"is

" is much fixed air formed; but little or none when " the water is admitted ſo ſparingly that no ſteam reach- " es the refrigeratory.

I once thought with the Antiphlogiſtians that fix- ed air was held in ſolution in this heavy inflammable air, becauſe when it is fired together with dephlogiſ- ticated air much fixed air is produced; but I am now ſatisfied that all the oxygen in that fixed air comes from the dephlogiſticated air with which it is fired. That this *muſt* be the caſe in ſome experiments is evi- dent, becauſe the fixed air ſo procured greatly exceeds the weight of all the inflammable air employed; and therefore could not poſſibly have been contained in it.

The reaſon why more fixed air is produced when the ſupply of water is copious is, I imagine, becauſe more water is neceſſary to the conſtitution of fixed air than to that of inflammable air.

According to this experiment with charcoal, water may be ſhewn to conſiſt of hydrogen only; but ac- cording to my experiment with *terra ponderoſa aera- ta*, it may be proved to conſiſt of oxygen only. For when ſteam is ſent over this ſubſtance in a red heat no- thing but the pureſt fixed air is produced; and yet the whole of any quantity of water may be expended in that production. As water is not ſaid to contain any *carbone*, this muſt be ſupplied by the *terra ponderoſa*, and all the oxygen by the water. For according to the theory fixed air conſiſts of 28 parts of carbone, and 72 of oxygen.

Theſe experiments favour my hypotheſis that wa- ter is the baſis of all kinds of air, and therefore that without it no kind of air can be produced. In ſome caſes, as in that of the light inflammable air, it may be all that can be aſcertained by *weight*.

To my experiments with the terra ponderoſa, which in my opinion demonſtrably proves that water is a conſtituent part of fixed air, and therefore proba- bly

bly of other kinds of air alſo, Mr. Berthollet objects (*Report*, p. 82) that I did not examine the loſs of weight in this ſubſtance. But after the proceſs it adhered ſo cloſely to the earthen tube in which the experiment was made, that the loſs of weight could not be aſcertained with accuracy. This, however, was not at all neceſſary. I found very exactly how much fixed air a given quantity of this ſubſtance would yield by means of water, which appeared to be the ſame that it yielded by ſolution in marine acid, and that it yielded no air at all by mere heat without water. It was quite ſufficient, therefore, to find how much water was expended in procuring any quantity of fixed air from this ſubſtance. And as there was no other ſource of loſs of water beſides the fixed air, it could not but be concluded that it entered into its compoſition, as a neceſſary part of it, and in the proportion which I aſcertained.

The truly ingenious and equally candid Mr. Rupp hints that the water might be imbibed by the terra ponderoſa; but I ſee no reaſon to think that it did. It is not at all probable that there is any affinity between this ſubſtance and water; and if water exiſt in it as an extraneous ſubſtance, the heat that I applied would have expelled it.

Mr. Rupp produces ſeveral experiments, made ſeemingly with great accuracy, to prove that fixed air contains no water. But experiments which require the ſolution of ſubſtances in acids, and evaporation, together with the computation of the proportion of earth, acid and water contained in ſalts, are much more complex than mine; and therefore, will not, I think, authorize ſo poſitive a concluſion. I have not repeated his experiments, and leave others more expert than I am in ſuch proceſſes to judge between us.

SECTION

SECTION IX.

Of the ſuppoſed Decompoſition of Water in the Experiments of Van Trooſtwick and Deiman, in theſe of Mrs. Fulhame, and various other Proceſſes.

IT is alleged in favour of the decompoſition of water, that both dephlogiſticated and inflammable air have been procured by taking electric exploſions in water. Experiments with this reſult were made by Meſſrs. Van Trooſtwick and Deiman, and have been repeated with the greateſt attention by Dr. Pearſon. See the *Philoſophical Tranſactions* for 1797, p. 142.

The accuracy of theſe experiments I am by no means diſpoſed to queſtion. Both dephlogiſticated and inflammable air were, no doubt, produced, tho' with infinite labour, by this means; and I conſider the experiment as exceedingly curious and important in ſeveral reſpects. But it is a very complex one. Several agents are concerned, and what, and how much, to aſcribe to each of them it is not eaſy to ſay. I have not yet found any termination to the production of air from water only, and the laſt product, which is equable, is wholly phlogiſticated air, of the nature of which we know but little. Some of my experiments ſeem to prove that it is compoſed of dephlogiſticated and inflammable air; and *light*, which is peculiarly intenſe in the electric ſpark, is in my experiments on plants, and probably in other proceſſes, a neceſſary agent in the production of dephlogiſticated air, when there is water for its baſis. And the metals that are employed, viz. gold and platina, may contribute to this ſlow production of inflammable air. But the accenſion

cenſion of theſe airs being ſometimes ſpontaneous, without the electric ſpark being taken in them, ſhews that part at leaſt of the air produced is *phoſphoric;* and it is well known that the electric ſpark is always accompanied with the ſmell of phoſphorus.

I hope theſe experiments will be repeated with a ſtill greater variety of circumſtances, tho' I do not ſee how they can be made ſo that water only ſhall be employed, except perhaps in a glaſs ſyphon, ſo that the electric ſpark ſhall be made to paſs from the water in one of the legs to the water in the other, and to this there are many objections.

To theſe obſervations I ſhall ſubjoin what I obſerved with reſpect to this experiment when I made a new arrangement of my *Obſervations on air* in three volumes, vol. 3. p. 543.

" It muſt be acknowledged that ſubſtances poſ-
" ſeſſed of very different properties may be compoſed
" of the ſame elements, in different proportions,
" and different modes of combination. It cannot,
" therefore, be ſaid to be abſolutely impoſſible but that
" water may be compoſed of dephlogiſticated and in-
" flammable air, or of any other elements. But then
" the ſuppoſition ſhould not be admitted without
" proof; and if a former theory will ſufficiently ac-
" count for all the *facts*, there is no occaſion to have
" recourſe to a new one, attended with no peculiar ad-
" vantage."

" alſo, that phlogiſton is an element in the com-
" poſition of water is, as I have more than once ob-
" ſerved, not improbable; ſince water conducts elec-
" tricity like metals and charcoal, into which the ſame
" principle enters; and becauſe, when freſh diſtilled,
" it attracts dephlogiſticated air from the atmoſphere,
" which is the property of other ſubſtances containing
" phlogiſton. By this means water may, in fact, con-
" tain both the principles of which, according to the

new

" new theory, it wholly consists, and in what propor-
" tion it contains them we cannot tell. For tho' heat
" may expel them in part, in the form of air, the force
" of this action may be limited, so that water boiled
" ever so long may retain much air, which only such
" a degree of heat as is communicated by electricity
" can discover. But this proves nothing against the
" doctrine of phlogiston; since it only proves that this
" principle is contained in water, more or less inti-
" mately combined, as well as in many other sub-
" stances."

Dr. G. Fordyce found, by an experiment which has the appearance of great accuracy, (See *Philosophical Transactions* for 1792, p. 374) that the addition of weight to zinc, when it is converted into a calx, comes from the *water*. But he advances nothing to prove that the water was decomposed in the process; and water is all that I can find in flowers of zinc.

It is pretended that water is decomposed by the growth of plants acted upon by light. But if this was the case, why will not a plant continue to grow in the same water till the whole of it be decomposed? Whereas I always found that only a certain quantity of dephlogisticated air could by this means be procured in the same water, and very little in proportion to its bulk. After this the production of air ceased, and the plant died.

To me it appeared, that the food of the plant was the phlogiston contained in the water. For when by the growth of the plant the air contained in the water was perfectly pure, the process always ceased. But the same plant, removed to water that contained impure air, would grow again, and give pure air as before. See my *Experiments* on air, Vol. 5, p. 25.

Also, having put various vegetable and animal substances into water, which by putrefying became offensive in the dark (yielding inflammable air mixed

with

with fixed air) when the veſſel containing them was placed in the light, and green vegetable matter was ſuffered to grow in it, the pureſt dephlogiſticated air was produced; the phlogiſton, as I obſerved, p. 42, which, in other circumſtances, would have been converted into inflammable air, now going to the nouriſhment of this plant, and by the influence of light yielding ſuch pure air. On this ſubject I then made the following remarks, p. 62.

" It is impoſſible not to obſerve from theſe experiments the admirable proviſion there is in nature to prevent, or to leſſen, the fatal effects of putrefaction, eſpecially in countries where the rays of the ſun are moſt direct, and the heat the moſt intenſe. For whereas animal and vegetable ſubſtances, by ſimply putrefying, would neceſſarily taint great maſſes of air, and render it wholly unfit for reſpiration, the ſame ſubſtances putrefying in water, ſupply a moſt abundant pabulum for this wonderful vegetable ſubſtance, the ſeeds of which appear to be in all places, diſperſed inviſibly thro' the atmoſphere, and capable, at all ſeaſons of the year, of taking root, and immediately propagating themſelves to the greateſt extent. By this means, inſtead of the air being corrupted, a vaſt addition of the pureſt air is continually thrown into it."

" By this means alſo ſtagnated waters are rendered much leſs offenſive and unwholeſome than they would otherwiſe be. That froth which we ſee on the ſurface of ſuch waters, and which is apt to create diſguſt, generally conſiſts of the pureſt dephlogiſticated air, ſupplied by aquatic plants, which always grow in the greateſt abundance, and flouriſh moſt, in water that abounds with putrid matter. When the ſun ſhines theſe plants may alſo be ſeen to emit great quantities of pure air."

" Even where animal and vegetable ſubſtances pu-

" trefy in air, as they have ſome moiſture in them, va-
" rious other plants, in the form of *mold*, &c. find a
" proper nutriment in them ; and by converting a con-
" ſiderable part of the phlogiſtic effluvium into their
" own nouriſhment, arreſt it in its progreſs to corrupt
" the ſurrounding atmoſphere. So wonderfully is e-
" very part of the ſyſtem of nature formed, that good
" never fails to ariſe out of all the evils, to which, in
" conſequence of general laws, moſt beneficial to the
" whole, it isneceſſarily ſubject. It is hardly poſſible for
" a perſon of a ſpeculative turn not to perceive, and
" admire, this moſtwonderful and excellent proviſion."

2. Since charcoal is reſolvable by means of water into fixed and inflammable air, and fixed air conſiſts of dephlogiſticated air and phlogiſton, theſe principles have been united in the ingenious experiments of Mr. Tenant, diverſified by Dr. Pearſon, ſo as to form charcoal. It was accompliſhed by heating ſubſtances containing fixed air, as marble, &c. together with phoſphorus, which contains phlogiſton. This experiment has been alleged in favour of the decompoſition of water ; but I only ſee in it the compoſition of a ſubſtance from the elements of which it was known to conſiſt.

3. The production of inflammable air from liver of ſulphur with water, Mr. Gingembre ſays, ariſes from a decompoſition of this water ; becauſe without the water no inflammable air is procured. But water, I find, is neceſſary to the conſtitution of all kinds of air, and of inflammable air moſt evidently.

4. Mrs. Fulhame imagines that ſhe has proved the decompoſition of water from a number of exceedingly curious experiments on the revival of metals by means of inflammable air, phoſphorus, ſulphur, charcoal, and various other ſubſtances of a ſimilar nature, becauſe the effect is never produced without the preſence of moiſture. Her experiments are ſuch as I

ſhould

should not have expected a *priori*; and when she was so obliging as to shew me the result of some of them in London, I was greatly struck with them; but I do not think that they prove the decomposition of water.

She does not pretend to exhibit separately either of the parts of which the water is said to be composed; since she does not produce either inflammable or dephlogisticated air from this water; and she supposes the very same quantity of water to be recomposed that is decomposed in the process. Nor does she pretend to be able to revive any metal without some substance into which phlogiston has always been supposed to enter.

All, therefore, that can be fairly inferred from her ingenious experiments is, that water assists the separation of oxygen from the calces of metals, and the entrance of phlogiston into them; which it may do without any decomposition. Alkohol, she observes, will not answer the same purpose. But to this it is sufficient to say that alkohol is not water, and therefore has not the same properties. The presence of water is necessary to the rusting of iron, and also to the acquisition of fixed air by lime; but the manner in which it contributes to these and other processes has not yet been ascertained.

Had she made her experiments with inflammable air in close vessels, she would, I doubt not, have found a diminution of the quantity of it, which could not be accounted for but on the supposition of its having entered into the calx, and thereby contributed to the revival of the metals.

5. She says, p. 163, that "the formation of nitrous "acid in Mr. Cavendish's noted experiment, cannot "be explained on any other principle than the decom-"position of water." But Mr. Cavendish himself draws no such inference from it, All that I see in it is that phlogisticated air contains the same principle with

inflammable

inflammable air, viz. phlogiston; and therefore that when they are decomposed together with dephlogisticated air, they form the same thing, viz. nitrous acid. The *water* that is produced, and which adheres to the acid, I suppose to be that which is essential to the constitution of all kinds of air, and to be the greatest part of their weight.

6. Rain, she supposes, p. 167 to be water formed at the time from its proper elements in the upper regions of the atmosphere. From the respiration of fishes, and from every case of combustion, she draws the same conclusion. But in every case she says that whenever one quantity of water is decomposed, another equal quantity is composed in the same process; so that as she says, p. 180, "equal quantities are form-"ed and rise regenerated like the phenix from her ashes." But this appears to me to be as fanciful, and fabulous, as the story of the phenix itself.

SECTION X.

Of the Constitution of Fixed Air.

IF I have proved that inflammable air comes from the *metals*, and not from the *water* in which the solution of them is made, and that water has not been decomposed, so that it connot be proved to consist of two kinds of air, I have done all that is necessary to establish the doctrine of *phlogiston*. There are, however, two other assumptions in the new theory which I think have by no means been proved, viz. that fixed air consists of *carbone* dissolved in dephlogisticated air, and that phlogisticated air, called *azote*, is a simple substance,

substance, and no compound. Neither of these suppositions appear to me to have been proved, and I think there is much positive evidence against them.

Though the new theory discards phlogiston, and in this respect is more simple than the old, it admits another new principle, to which its advocates give the name of *carbone*, which they define to be the same thing with charcoal, free from earth, salts, and all other extraneous substances; and whereas we say that fixed air consists of inflammable air and dephlogisticated air, or oxygen, they say that it consists of this carbone dissolved in dephlogisticated air. See *Examination of Mr. Kirwan*, p. 79. Mr. Lavoisier says, ib. p. 63, that "wherever fixed air has been obtained, "there is charcoal." They therefore call it the *carbonic acid.*

But in many of my experiments large quantities of fixed air have been procured where neither charcoal, nor any thing containing charcoal, was concerned, or none in quantity sufficient to account for it.

There is no metal that I have ever heated with a burning lens over lime water in atmospherical air without producing a thick scum on its surface, which was, no doubt, *lime*, formed by the quicklime in the water and the dephlogisticated air contained in the portion of atmospherical air in which the process was made. For this purpose I have tried not only iron and zinc which are said to contain plumbago (a kind of carbone from which some fixed air may be expelled) and also lead, tin, bismuth, copper, &c. as observed before, but even gold, silver, and platina, and it will hardly be pretended that all these metals contain carbone.

From a quantity of calx of lead, part grey and part yellow, in a glass tube, I got its bulk of almost pure fixed air, and the residuum extinguished a candle. Where could be the carbone in this case?

Fixed

Fixed air is always produced when iron is melted, and thereby converted into finery cinder, in atmospherical or dephlogisticated air, and also when some kinds of inflammable and dephlogisticated air are fired together. But Mr. Berthollet, Mr. Adet, and all my opponents, say that this fixed air comes from the plumbago contained in the iron, and that when it is found after the union of inflammable and dephlogisticated air, in an explosion of them, it was from plumbago contained in the inflammable air. But besides that there is no evidence of inflammable air containing any plumbago (since when iron is dissolved in any acid the plumbago is left behind) the fixed air contained in this substance, of which the antiphlogistians make so much use, is very inconsiderable; the air into which it may be resolved being chiefly inflammable.

From 6 dwts. of the purest plumbago, procured from an iron furnace, in the form of a shining black powder, I expelled, in a glazed earthen tube, 40 ounce measures of air, one twelfth part of which was fixed air, and the rest inflammable, burning with a blue flame. Then, sending steam thro' the tube, I got 240 ounce measures more, the whole of which was inflammable air, of the purest kind, exactly resembling that from iron by the vitriolic acid. The plumbago was converted into one mass, resembling a hard cinder, and weighed $2\frac{1}{2}$ dwts.

Another experiment on plumbago I shall just mention in this place. Melting one dwt. of it with a burning lens in the open air, it threw off sparks, like cast iron treated in the same manner, but not quite so much; after which it was reduced to a slag like finery cinder, weighing four grains less than it had done. I repeated the experiment with the same result.

If plumbago be held in solution in inflammable air, not only must both the kinds of air contained in it, viz. fixed and inflammable, but the *slag* too, which re-

mains

mains after all air is expelled from it. But after the exploſion which it is ſaid diſcovers the fixed air that was contained in it, there is no apparent addition made to the inflammable air, nor the leaſt appearance of the *ſlag*. It is evident, therefore, that no ſuch ſubſtance was contained in the inflammable air from any kind of iron, and leaſt of all from malleable iron.

If the inflammable air had held in ſolution not the plumbago itſelf, but only the carbone that was in it, the reſiduum could not be plumbago; ſince it would want the carbone; and the inflammable air holding the carbone in ſolution would be of the heavy, and not of the lighter and purer, kind.

Fixed air is alſo produced when minium, and ſeveral other ſubſtances that contain dephlogiſticated air, are heated in inflammable air. This produce I had when I uſed ſome precipitate per ſe with which Mr. Berthollet ſupplied me. On being informed of this, he ſaid that he found afterwards, that the precipitate he had ſent me contained fixed air; and yet he allowed that when the air expelled from it by heat was received in lime water, it did not *immediately* make it turbid, which it is well known that a hundredth part of the fixed air that I procured by means of it would have done inſtantly. The turbulency that came on afterwards muſt, therefore, have had ſome other cauſe, probably ſome acid of vitriol in the water in which he made the experiment, and which gradually inſinuating itſelf into the lime water in his tube, would form *ſelenite*; a thing that has frequently occurred in the courſe of my own experiments, and which for ſome time puzzled me not a little.

It is generally thought that the fixed air contained in fallen lime has been attracted from the atmoſphere, in which it is ſaid to float in a looſe uncombined ſtate. But from no other experiment that I am acquainted with can it be proved that any fixed air

neceſſarily

neceſſarily exiſts in the atmoſphere, and lime or lime water, will become ſaturated with it in all ſituations. I am therefore inclined to think that this fixed air is compoſed of phlogiſton imparted to the lime from the fire to which it had been expoſed, and the dephlogiſticated air in the atmoſphere; and I have always found that a portion of atmoſpherical air expoſed, ſome time to lime, or to lime water, is ſenſibly leſs pure than before; ſome part of the dephlogiſticated air of which it is compoſed having been taken from it by the lime, and it is never found again except as a component part of the fixed air, which is afterwards expelled from it. The reſult of the experiment was the ſame, whether the lime was confined by water or by mercury.

The fixed air which is expelled from the yellow calx of lead which has been ſome time expoſed to the atmoſphere has, I doubt not, the ſame ſource. For when it is heated preſently after it is made, little or no air can be expelled from it, as it may ſome time afterwards. And I find that this ſubſtance alſo expoſed to a portion of atmoſpherical air makes it leſs pure than it was before, juſt as in the caſe of quick lime.

As pyrophorus imbibes pure air when it is expoſed to atmoſpherical air, leaving nothing but phlogiſticated air, in which it reſembles a mixture of iron filings and ſulphur, which alſo makes a pyrophorus, the fixed air expelled from it afterwards muſt have been formed by the union of the dephlogiſticated air imbibed by it and the phlogiſton contained in itſelf.

From a quantity of old and ſpoiled pyrophorus I got 180 ounce meaſures of air, of the firſt part of which one half was fixed air, and the reſt phlogiſticated. At the laſt, tho' one half was fixed air, the reſt was inflammable. In another experiment of this kind I found ſeven tenths of the air fixed and the reſt inflammable.

From

From 15 dwts. of fallen lime I got 45 ounce measures of fixed air, and 25 of inflammable, from the gunbarrel in which the experiment was made. Whether quick-lime has been exposed to the atmosphere, so as to become what is called *fallen lime*, or has been saturated with water, they come in time to be of the same weight, and to have the same properties; the former continually gaining weight, and the latter losing it. From 15 dwts. of lime saturated with water and then exposed to the atmosphere, I got 55 ounce measures of fixed air.

I had a result similar to this when I exposed some old *plaister of Paris* to heat in earthen an retort. In these circumstances $3\frac{1}{4}$ ounces yielded 200 ounce measures of air, of which the first part contained about one twentieth of its bulk of fixed air, and the remainder was considerably phlogisticated, viz. of the standard of 1. 5, tho' at the last of 1. 35.

It may be said that pyrophorus attracts *water* from the atmosphere, and that the water is decomposed by exposure to heat. But in no other case is water so attracted decomposed by mere exposure to heat. Water is attracted by lime, by vitriolic acid, and various other substances; but heat will always expel it again, and it may be collected in the form of water, without any decomposition. There is, therefore, every reason to conclude that it is the same with water attracted by pyrophorus.

It is, indeed, an obvious objection to the antiphlogistic theory, that it supposes water to be decomposed in such different circumstances. What can be more so than when it is applied in the form of steam to iron red hot, also when it is quite cold, and merely present in the same vessel in which the iron, also cold, is dissolved by an acid, and by the action of light on growing vegetables, &c. &c. But if inflammable air be procured, the theory absolutely requires that water be decompofed, and no difference of circumftances is fo much as attended to.

To thefe experiments relating to fixed air I fhall add one that I made on the heating of the diamond in atmofpherical air, much of the produce being fixed air.

That the diamond is a combuftible fubftance has been long known, but not having heard of its being burned in atmofpherical, or any kind of confined air, I long wifhed to do it; and being fupplied by a friend with two fmall ones, I treated them in this manner, and found that near 90 parts in 100 of the air in which they were burned were completely phlogifticated, and the quantity not being fenfibly changed, the remainder was fixed air; which is an effect fimilar to that of heating charcoal of copper in air. The diamonds being very fmall, and the quantity of air in which they were burned being very fmall in proportion, I will not vouch for much exactnefs in the refult, tho' the experiment was carefully made over mercury. Whenever I get larger diamonds, I will endeavour to make the experiment in a more fatisfactory manner. Both the diamonds weighed only three tenths of a grain, and they loft in the procefs fifteen hundredths of a grain. The air in which they were burned was three fourths of an ounce meafure.

SECTION XI

Of Phlogiſticated Air.

THE ſubject of phlogiſticated air is attended with conſiderable difficulty. I am able, however, to oduce ſufficient evidence that it is not, as the anti-ilogiſtians ſay, an elementary ſubſtance, but that it de-es its origin from phlogiſton ; ſince it may be gene-ed from inflammable air, ſometimes alone, but in ier caſes by means of dephlogiſticated air, whether y portion of this kind of air be united with it or not. this my readers will judge from the experiments it I ſhall lay before them.

1. One deciſive proof that phlogiſticated air may formed, and ſeemingly by the union of dephlogiſ-ated air and phlogiſton, is the quantity of phlogiſ-ated air that remains after any exploſion of dephlo-icated or common air with inflammable air, more n what remains after the mixture of nitrous air with ier of them.

Having procured a quantity of dephlogiſticated ſo pure that one meaſure of it mixed with two aſures of nitrous air was reduced to 0. 04, I could by any mixture of the pureſt inflammable air fi-along with it reduce it to leſs than 0. 25.

2. The very different proportions in which atmoſ-rical air is diminiſhed in different proceſſes is a of that in ſome of them there muſt be a generation ohlogiſticated air. When air is diminiſhed by iron igs and ſulphur moiſtened with water, the propor-i of phlogiſticated air is that which Mr. Lavoiſier es, viz. 73 parts in 100. But when I made the

mixture

mixture without any water, I found that 100 meafures were in fix days reduced only to 90 completely phlogifticated, which is in the proportion of 81. 8, in 100. Again 140 ounce meafures were by the fame dry mixture reduced to 113, which is in the proportion of 80. 6 in 100. But fome water getting to the mixture the third time that it was ufed, 155 ounce meafures of air were reduced by it to 116; which is in the proportion of 74 to 100.

By the flow burning of phofphorus 60 ounce meafures of atmofpherical air were reduced to 48, at another time to 48. 5; and 50 ounce meafures were reduced to 40, which is in the proportion of 80 parts of phlogifticated air in 100. But by repeatedly firing the phofphorus with a burning lens, 100 ounce meafures were reduced only to 89 completely phlogifticateded.

Meffrs. Berthollet and Fourcroy, however, fay (*Annales de Chymie,* vol. 26, p. 308) " We muft abandon " the teft of the purity of air by means of nitrous air; " and fubftitute that by phofphorus, by means of which " we get uniform refults. They are different with ni- " trous air, on account of the different proportions in " which this air combines with dephlogifticated air to " form nitrous acid."

But how can thefe proportions vary when the circumftances in which they are mixed are exactly the fame? The nitrous air admitted in the fame manner to any kind of air, containing in it a portion of dephlogifticated air, muft unite with this, and this only; leaving the other, with which it cannot combine, as it was; and it requires no particular degree of heat to do this. The refult is, therefore, the fame in all temperatures. On this account it is the moft convenient, and perhaps as accurate teft as we can apply. It is only neceffary that there be a fufficient quantity of nitrous air to faturate all the dephlogifticated air that

it

it can meet with, and that the ſame time intervene between the mixture and the meaſuring of the dimunition occaſioned by it. The dimunition of atmoſpherical air by means of phoſphorus is both a tedious, and even a leſs certain, proceſs, as well as attended with expence; and I find that the uſe of inflammable air inſtead of nitrous air, which ſome perſons recommend, is liable to much greater objections, the reſult of the firing of them by the electric ſpark being, exceedingly various in circumſtances as nearly as we judge the very ſame.*

3. Since pure nitrous air wholly vaniſhes when it unites with pure dephlogiſticated air, the phlogiſticated air that is found after heating iron in it cannot be a ſimple element, but muſt have been formed from ſomething in the nitrous air and phlogiſton from the iron. Heating malleable iron in 60 ounce meaſures of nitrous air, it was reduced to 24, all phlogiſticated. When I continued this proceſs beyond the point of greateſt dimunition, the air produced was inflammable.

4. Since water contains but a ſmall quantity of air

* Mr. Rupp alſo objects to the uſe of nitrous air as a teſt of the purity of atmoſpherical air, and quotes a former experiment of mine, in which it appeared that by only pouring a mixture of nitrous and atmoſpherical air from one veſſel to another, and alſo by letting the mixture ſtand ſome days without any agitation, the degrees of diminution were very various; and he ſays that therefore from the ſtill greater diminution of this mixture which I have ſince obſerved, it cannot be concluded that atmoſpherical air contains more deplogiſticated air than has hitherto been ſuppoſed. I acknowledge that my concluſion from that obſervation was not juſt, but for a reaſon that I was not at that time acquainted with. For I have ſince found that not only does that mixture of air continue to diminiſh ſtill farther by being longer confined by water, but that a quantity of any kind of air will in time be

air in proportion to its bulk, and generally confiderably purer than that of the atmofphere, the phlogifticated air that is produced by heating fteam in a copper veffel muft have been formed from phlogifton in the copper and the air contained in the water; and whenever I have heated water in this manner (the upper part of a clofed copper tube being kept in a red heat, while the lower and open part was immerfed in water) I have found a confiderable quantity of air completely phlogifticated, and the longer I kept it in this ftate the more of this air I found. I had fimilar refults when I ufed a filver tube.

That this phlogifticated air is not that which had paffed from the centre of the fire thro' the metal tubes (tho' fome of my late experiments prove that fome metals are permeable to air in thefe circumftances) appears from the refults of my experiments with glazed earthen tubes in the fame circumftances. For the air that gets into the infide of thefe is often little worfe than atmofpherical air.

5. It is well known that hot charcoal imbibes any kind of air; and I have obferved that when it is afterwards put into water it gives this air out again. But if the air be that of the atmofphere, it takes the dephlogifticated

wholly abforbed in the fame circumftances, and that fome time before they difappear they all become phlogifticated air, inflammable air as well as the reft.

This is a fact of which I am not able to give any rational account, any more than of feveral others that have fallen under my obfervation. I have given a detailed account of the facts in an article I lately fent to the philofophical fociety at Philadelphia, together with another, on any two kinds of air, feparated by an earthen veffel, or a bladder, changing places, which I had obferved before with refpect to fteam and air. This is a fact of great importance in the fyftem of nature, efpecially with refpect to refpiration; but of the caufe of it I have not even a conjecture worth propofing.

gisticated part in preference to the other, leaving the remainder phlogisticated; and the air that it gives out after this in water is chiefly phlogisticated. What, then, becomes of the dephlogisticated air that has disappeared? Will it be said that it remains in the charcoal, which had imbibed it. Whence then came the phlogisticated air which it gave out, when, according to the new theory, charcoal does not contain any such principle? It is not found in the water into which it is put; for this gives out air less pure than it did before the process.

6. A solution of copper in volatile alkali gave phlogisticated air with marine acid, and it will not be easy to say where this azote existed before the process.

7. Most of the substances which have been used to phlogisticate air gain an addition to their weight in the process, in consequence of which it has been taken for granted by the antiphlogistians, that nothing is emitted from them, and that they only imbibe the dephlogisticated air, which is one constituent part of the atmosphere, leaving the other part, which they call *azote*, unaltered. It was, therefore, desirable to find some substance which would not gain any weight in the process, and yet have the same effect in phlogisticating the air. For the dephlogisticated air not uniting with the substance exposed to it must necessarily form some other combination.

This end was in some measure answered by *steel*, which, according to the common hypothesis, containing more phlogiston than iron, would, I thought, part with more on the application of heat, and receive less addition; and this I found to be the case. But it was more completely answered by *black bones*, which without gaining any thing by the application of heat in any circumstances, became white in the process.

If this be done in common air, as the bones do not imbibe the dephlogisticated air that disappears, this air

air is difpofed of in two different ways. For one part of it contributes to form fixed air, and another part may form a different union with fomething emitted from the bones, and make an addition to the phlogifticated air. Accordingly, there is more of it found after the procefs with the black bones than with iron, and many other fubftances which receive an addition of weight in the procefs.

Whence, then, I afk, can come this addition of phlogifticated air, but from an union of phlogifton emitted from the bones, and the dephlogifticated air in the atmofpherical air to which they are expofed? Confequently, phlogifticated air, or azote, is not a fimple fubftance, as the antiphlogiftians maintain, but a compound. Alfo whence can come the fixed air that is procured in the fame procefs, but from a different combination of the fame elements, and not, as they fay from *carbone*, which is a fubftance of vegetable origin, and has no place here. Mr. Rupp is of opinion that the fixed air is formed from the *carbone* in the bone, and the dephlogifticated air that difappears. But when the heat is applied with care, there is no lofs of weight in the bone; fo that nothing is driven from it befides the *phlogifton*, which appears to have no weight at all, or none that we can afcertain.

That the thing which conftitues the blacknefs of the bones is the fame with that which has always been called phlogifton, is evident from its forming inflammable air if there be water to fupply it with a bafis. For I find that if they be heated in phlogifticated air, which cannot by parting with any thing contribute to this whitenefs, they neverthelefs become white; the air in which they are heated is increafed in quantity, and this increafe is inflammable air.

For thefe experiments I find *ivory black*, which is the coal of ivory ufed by painters, more convenient than common bones. To prepare this fubftance for

the

the experiments, I fill an earthen tube with it, and closing it with clay, expose it for a considerable time, at least a quarter or an hour, to the greatest heat of a smith's fire, which will expel from it every thing that is volatile; so that no heat to which I can expose it afterwards will affect it, except by means of some other substance with which that which constitutes its blackness has an affinity, and with which it can combine.

Heating a quantity of ivory black prepared in this manner in $6\frac{1}{2}$ ounce measures of atmospherical air, there was no sensible change in the quantity; but, on examining it, I found in it an ounce measure of fixed air, and the remainder was completely phlogisticated, which is in the proportion of 84 parts in 100; whereas the antiphlogistians say that any portion of atmospherical air contains only 73 parts in 100 of phlogisticated air. It is evident, also, that both these substances consist of the same elements, viz. dephlogisticated air and phlogiston.

A writer in the *Medical and Physical journal*, p. 30, finds no production of phlogisticated air, but only of fixed air, by heating a black bone in dephlogisticated air, and therefore he concludes that my experiment with atmospherical air was inaccurate. But he should have used the same kind of air that I did. What I have observed is that sometimes fixed, and sometimes phlogisticated air is produced from the same elements, tho' I have not been able to discriminate all the circumstances in which one or the other is the result of their combination.

7. Having made much use of a mixture of *iron filings and sulphur* for the purpose of phlogisticating air, I have always had a large quantity which had been long exposed to the atmosphere, from which it is allowed that it attracts nothing besides dephlogisticated air. Of this mixture, become quite dry and brown,

 $3\frac{1}{2}$ ounces

$3\frac{1}{2}$ ounces expofed to heat in an earthen tube gave out 120 ounce meafures of air, of which about one tenth was fixed air, and the reft almoft wholly phlogifticated. Both thefe kinds of air, therefore, muft confift of dephlogifticated air from the atmofphere, and fomething contained in the iron, or the fulphur, both of which are maintained to be fimple fubftances. There remained a black powder, ftrongly attracted by the magnet.

8. In general iron filings and fulphur immerfed in mercury, or water, or placed in a vacuum, yield inflammable air; but in fome cafes (tho' I do not know the reafon of the difference) this mixture has yielded phlogifticated air.

Having placed a pot containing fome of this mixture in a vacuum, I found, after fome days, that it had yielded $2\frac{1}{2}$ ounce meafures of air; and examining it, I found it to be completely phlogifticated. I then put the fame mixture under water, and placing it near the fire, it gave an ounce meafure more, all phlogifticated.

At another time two ounce meafures of air were yielded by a mixture of this kind; and being examined, tho' not till long after it was formed, it was found to be wholly phlogifticated. It might have been originally inflammable air, and afterwads have changed to phlogifticated.

9. Of the change of inflammable air into phlogifticated air, feveral inftances may be feen in the account of fome of my early experiments; but I am not yet able to fay on what this change depends. Suppofing that it required the union of a portion of dephlogifticated air, I expofed to it pieces of iron, which being covered with ruft, had attracted and contained that air; but the refults were not uniform. I fhall, therefore, content myfelf with relating what I obferved, wifhing that other perfons may diverfify the

circumftances,

circumſtances, and endeavour to aſcertain the cauſe of the different reſults.

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 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 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?

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.

On the 15th of Auguſt 1799, I examined a quantity of inflammable air which had been confined by mercury with dry iron ruſted in nitrous acid, from the 18th of March 1798, and found nothing inflammable in it, tho' there was no apparent change in the

colour

colour of the iron. This was also the case with another quantity of the same kind of air which had been confined in the same manner from the 14th of July, However, another quantity of inflammable air that had been confined the same time, and in the same manner, with iron rusted in vitriolic acid, was not much changed, tho' the iron was become black.

The CONCLUSION.

BEFORE the new theory of chemistry can be unexceptionably established the following things must be done.

1. Whenever inflammable air, or *hydrogen*, is procured, evidence must be given of the production of a due proportion of *oxygen*, that is of 85 parts of this to 15 of the other; and this evidence must be something more than an addition of *weight*. It must be either actual *acidity*, or *dephlogisticated air*. Otherwise there is no proof of the inflammable air havidg come from the decomposition of the water. This, however, has not been done with respect to iron, or any other substance by means of which inflammable air is procured.

2. When water is procured by the burning of inflammable air in dephlogisticated air, not only must the water be free from acidity, but there must have been no production of phlogisticated air in the process. For by the decomposition of this air the nitrous acid may be procured.

On the whole, I cannot help saying, that it appears to me not a little extraordinary, that a theory so new, and of such importance, overturning every thing that

that was thought to be the best established in chemistry, should rest on so very narrow and precarious a foundation; the experiments adduced in support of it being not only ambiguous, or explicable on either hypothesis, but exceedingly few. I think I have recited them all, and that on which the greatest stress is laid, viz. that of the formation of water from the decomposition of the two kinds of air, has not been sufficiently repeated. Indeed, it requires so difficult and expensive an apparatus, and so many precautions in the use of it, that the frequent repetition of the experiment cannot be expected; and in these circumstances the practiced experimenter cannot help suspecting the accuracy of the result, and consequently the certainty of the conclusion.

But I check myself. It does not become one of a minority, and especially of so small a minority, to speak or write with confidence; and tho' I have endeavoured to keep my eyes open, and to be as attentive as I could to every thing that has been done in this business, I may have overlooked some circumstances which have impressed the minds of others, and their sagacity is at least equal to mine.

Tho' the title of this work expresses perfect confidence in the principles for which I contend, I shall still be ready publicly to adopt those of my opponents, if it appear to me that they are able to support them. Nay, the more satisfied I am at present with the doctrine of phlogiston, the more honourable shall I think it to give it up upon conviction of its fallacy; following the noble example of Mr. Kirwan, who has acquired more honour by this conduct than he could have done by the most brilliant discoveries that he could have made.

The phlogistic theory is not without its difficulties. The chief of them is that we are not able to ascertain the *weight* of phlogiston, or indeed that of the oxygeneous nous principle. But neither do any of us pretend to have weighed *light*, or the element of *heat*, though few perfons doubt but that they are properly *fubftances*,* capable, by their addition, or abftraction, of making great changes in the properties of bodies, and of being tranfmitted from one fubftance to another.

As to the *new nomenclature*, adapted to the new theory, no objection would be made to it, if it were formed, as is pretended, upon a knowledge of the real conftitution of natural fubftances; but we cannot adopt one, the principles of which we conceive not to be fufficiently afcertained. For other objections to this nomenclature, I refer to the Preface of *Mr. Keir's* excellent *Dictionary of Chemiftry*. However, whether we approve of this new language or not, it is now fo generally adopted, that we are under a neceffity of learning, though not of ufing it.

* Since this was written it has, I think, been proved by Count Rumford, and Mr. Davy, that *heat* is not produced by any proper *fubftance*, fuch as is now called *calorique*, and which is fo effential to the new theory.

APPENDIX.

APPENDIX.

1. *Of Dr. Mitchill's Attempt to reconcile the two Systems.*

DR. MITCHILL, professor of chemistry in the college of New-York, to whom, as an impartial and excellent judge of the subject, I addressed my Letters in defence of the doctrine of phlogiston in the *Medical Repository*, has employed much ingenuity in an Essay in that work (vol. 1, p. 514) on a scheme calculated to reconcile the phlogistians and antiphlogistians. But his proposal will hardly be admitted by either of the contending parties.

The phlogistians will not admit that water contains an inflammable principle merely because the blast of an eolipyle will promote the burning of fuel; since whenever this is the case, a current of *air* always accompanies the current of *steam*; and if this be prevented, the steam extinguishes the fire as effectually as cold water, or phlogisticated air.

On the other hand the antiphlogistians will not acknowledge that even common sulphur, phosphorus, iron, or zinc, contain any hydrogen, which Dr. Mitchill makes synonymous to phlogiston. And the phlogistians maintain that if any of these substances contain phlogiston, they all must, and every metal without exception, as gold, and others which he thinks contain none; because the calces, or bases, of them all become these substances in consequence of imbibing inflammable air; and because either this air, or nitrous air (which contains the same principle) is evolved

evolved whenever they are diſſolved in acids. In ſhort, the metals, as well as ſulphur and phoſphorus, are either neceſſarily ſimple ſubſtances, or neceſſarily and univerſally compounds; and water is either reſolvable into two kinds of air, or it is not; and upon the deciſion of theſe queſtions the whole controverſy hinges.

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2. *Of the Diſcovery of the Production of dephlogiſticated Air by the Action of Light on Plants.*

IN this work I call the diſcovery of the emiſſion of dephlogiſticated air by the action of light upon plants *mine*, whereas writers in general give it to *Dr. Ingenhouſz*. That juſtice may be done to us both, I ſhall copy a *Note* introduced into my *Experiments on the generation of air from water*, which was printed in the year 1793, and alſo a letter of his to me which I have ſince found among my papers (which were partly deſtroyed, and partly diſperſed, in the riots of Birmingham) from which it will appear that we do not differ in opinion with reſpect to any *facts*, but only with reſpect to the degree of merit that belongs to each; and of this others will be more impartial judges than either of us.

" As ſome perſons imagine there is an interference " in Dr. Ingenhouſz's claims to diſcovery and mine, " I ſhall ſubjoin an extract of a letter I wrote to him " ſix years ago, when a young phyſician on his travels " deſired of me letters of introduction to my philoſo- " phical friends on the continent, ſince it will tend to " ſet the matter in a proper light, and ſhew that there " is no ground of interference between us at all."

Dear

Birmingham, Nov. 21, 1787.

DEAR SIR,

"I THANK you for the French edition " of your *Experiments on Vegetables*, which I received " ſome time ago; but I am ſorry to ſee in the Preface " ſomething that looks as if you, or your friends, " thought I wiſhed to detract from your merit, which " is very far from my diſpoſition."

"I do not, indeed, diſtinctly ſee what ground " there is for any interference between us. That plants " reſtore vitiated air I diſcovered in a very early peri- " od. I afterwards found that the air in which they " were confined was ſometimes even better than atmoſ- " pherical air, and that the *green matter* (which I " at firſt, and ſeveral of my friends always, thought " to be a vegetable) produced pure air by means of " light; and immediately after the publication of this " fact, and before I ſaw your book, I found that other " whole plants did the ſame." *

"All the time that I was employed in making theſe " experiments I wrote to my friends about them, par- " ticularly to Mr. Magellan, and deſired him to com-

M " municate

* Dr. Ingenhouſz, however, ſays in his *Eſſay on the food of plants*, printed in the *Additional Appendix to the propoſed General report from the board of Agriculture*, p. 14, that I "ab- " ſolutely deny all emiſſion of air from the ſurface of plants " as well as from the ſkin of animals." The latter I certainly do deny; and I think I have given ſufficient evidence of the truth of my opinion. As to *plants*, I obſerved that not only the leaves, but the green ſtalks, gave dephlogiſticated air when the ſun ſhone upon them. That they give *any air* in the dark, when they are in a healthy ſtate, tho' maintained by Dr. Ingenhouſz, I never found. According to him, the injury they do to the atmoſphere in the night tends to counteract the ſervice they render to it in the day.

" municate my obſervations to you, as well as to others ; " but I believe you did not hear of them, ſo that what " you did with *leaves* was altogether independent of " what I was doing with *whole plants ;* but the ſame " ſummer, and the ſame ſun, operated for us both, " and you certainly publiſhed before me."

" This appears to me to be the true ſtate of the " caſe ; and ſurely it leaves no room for the ſuſpicion of " any thing unfair, or unfriendly. But whatever your " friends may ſay, I have no thought of troubling the " Public with any vindication. I value you, and " your friendſhip, too much to wiſh to have any alterca- " tion on the ſubject. Indeed, there is nothing to con- " tend about. If on any future occaſion you will do me " the juſtice to give this ſtatement of the matter, I ſhall " be happy. If not, I ſhall not complain—I ſhall " always be happy to hear from you, and am

Dear Sir,

Yours ſincerely,

J. PRIESTLEY."

" This I think will ſhew that I was not very anx- " ious about the merit of this diſcovery. The origi- " nal obſervation that plants reſtore vitiated air, and " the ſubſequent one, that *green vegetable matter* yields " dephlogiſticated air by means of light, were both " accidental ; and ſurely there was no great ſagacity re- " quired to try the effect of *certain plants,* when I " had aſcertained the fact with reſpect to one of a *doubt-* " *ful* nature. That *leaves* had this power, I own I had

" had no ſuſpicion. But the merit of all philoſophi-
" cal diſcoveries is, in my opinion, greatly overrated."

The following is the letter I received from him ſome time after.

London, July 7, 1790.

DEAR SIR,

" RECEIVE my hearty thanks for the " valuable preſent of your philoſophical work in three " volumes,"

" As I do not know whether I underſtood rightly " the ſeventh ſection of the third volume, I was at the " firſt reading of it ſomewhat puzzled, and thought " that the contents of it might induce the reader to be- " lieve that *you*, and not *I*, were the firſt who pub- " liſhed that it *is the light of the ſun that is the cauſe why* " *real plants correct bad air, and yield vital air*, tho' all " thoſe of our common friends whom I conſulted on " this head are of opinion that ſuch an aſſertion is by " no means intended by you. If you have really pub- " liſhed this doctrine before me, I owe you the juſtice " to acknowledge it publicly in the firſt volume of my " books that will be publiſhed, or reprinted; and I " will chearfully retract, by acknowledging that in " reading your philoſophical works I have thro' inad- " vertence, and not deſign, overlooked this doctrine; " and I will very readily quote the volume of your " works, and the page in which you will inform me " this doctrine is clearly and explicitly to be found. " But if this doctrine was firſt publiſhed by me, as I " have till now been perſuaded is the caſe, I will leave " things as they are. The confidence I have in your " liberal manner of thinking makes me hope that you

" will

"will favour me ſoon with an anſwer, which will be
"very greatfully received by

Your obliged

humble ſervant,

J. INGENHOUSZ."

The copy of my anſwer was deſtroyed in the riots; but after ſtating to him the above-mentioned circumſtances (which, if he had attended to in the preceding letter, would have ſaved him the trouble of writing this) and the degree of merit to which I thought him entitled in the buſineſs, I concluded with ſaying that, for his ſake, I wiſhed it had been greater than it was.

It is obvious to remark that if the merit of this diſcovery, be it more or leſs, depend, as he here acknowledges, on the ſubſtance which yields dephlogiſticated or vital air by means of light being a *real plant*, all philoſophers except Dr. Ingenhouſz himſelf, will, I doubt not, give it to me; becauſe the *green matter* which I had found to give this air by means of light, is acknowledged to be a vegetable.

Dr. Ingenhouſz, however, maintains a very ſingular opinion on the ſubject. For he ſays, p. 90, that "the water itſelf, or ſome ſubſtance in the water, "is, I think, changed into this vegetation." This is nothing leſs than reverting to the long exploded doctrine of *equivocal generation*. For if this vegetable could have its origin from mere water, or any thing in the water that was not a vegetable, or the ſeed of one, an oak might ariſe from water, or ſomething in water, that was not an acorn.

"This

" This real tranſmutation," he, however, ſays, ' tho' wonderful in the eye of a philoſopher, is no ' more extraordinary than the change of graſs and ' other vegetables into fat in the body of a gramini- ' vorous animal, and the production of oil from the ' watery juice of an olive" Other philoſophers will, I doubt not, ſee theſe caſes to be very differnt; and why Dr. Ingenhouſz alone ſhould heſitate to call this *green vegetable matter* (as he himſelf terms it) to be a *proper plant*, with the power of propagating itſelf, and of producing dephlogiſticated air like other plants, cannot, I think, be accounted for, but from his perſuaſion, that if it be a real plant, ariſing from a ſeed, like other plants, there would remain little that he could claim in the merit of this diſcovery."*

When I had made, and publiſhed, this diſcovery, which was before he had made any experiments on the ſubject, and only heſitated to call it a vegetable becauſe, when I examined it with a microſcope with that view, I could not diſcover the form of one, could I forbear examining the firſt opportunity whether other *certain plants* would do the ſame; and would not a generous friend have forborne to anticipate me in a diſcovery, which if not already made, I was known to be in purſuit of, and could not poſſibly miſs. Confiding in this, tho' I completed the diſcovery the next ſummer, at the ſame time that Dr. Ingenhouſz made his experiments, I did not make the haſte that he did in publiſhing it; but, contenting myſelf with announcing it to my friends, deferred the publication till I had materials for another volume. While I, according

* In his *Eſſay on the food of plants*, he repreſents it as my opinion that this *green matter* is a ſubſtance *ſui generis*, and conſequently not a proper plant, tho' I uſed that language only till I had ſatisfied myſelf that it was one, and afterwards never intimated a doubt on the ſubject.

according to my known cuſtom, was writing to acquaint my friends with what I had done, Mr. Magellon, I well remember, ſaid that Dr. Ingenhouſz kept himſelf very cloſe; and no body knew what he was about till his book was printed.

If however the *making a thing known* by ſpeaking or writing, without printing, be *publiſhing*, my publication of plants in general yielding dephlogiſticated air by means of light, as well as this property of the green vegetable matter in particular, was prior to that of Dr. Ingenhouſz. It is well known to all my friends and acquaintance that I never deferred this method of publiſhing my diſcoveries a ſingle day, if I had an opportunity of communicating them to others by ſpeaking or writing about them. Beſides that this was always my natural inclination and habit, I have more than once obſerved, that the ſpeedy communication of diſcoveries is of great importance to the advancement of ſcience.

The ſeveral ſteps in this inveſtigation were as follows. In 1772, I found that the growth of plants reſtored air vitiated by animal reſpiration. For this diſcovery chiefly I received the gold medal of the Royal Society; and Sir John Pringle, in his ſpeech on the occaſion, enlarged on my idea of one part of the creation being the means of repairing the injury done to the atmoſphere by the other. In 1778, being at Limington, on the ſea ſhore, I found the air in the bladders of the ſea-weed to be much purer than that of the atmoſphere. In the ſame ſummer I found the air in which ſome plants had grown much purer than the external air, an effect which could not be aſcribed to any thing but the production of dephlogiſticated air. And it was at the cloſe of the ſame year that, obſerving bubbles of air emitted by the green matter with which the inſide of ſome of my phials was covered, I examined it, and found it to be highly

dephlogiſticated

lephlogiſticated. Excluding the *light*, the production ɔf air always ceaſed, tho' in the ſame degree of *heat*; o that the effect was owing to light only.

Being in London the winter following, I ſhewed his experiment to all my friends, and among the reſt o Dr. Ingenhouſz, who was particularly ſtruck with t. The queſtion among us then was what this *green natter* could be; and it being generally thought to ɔe a *vegetable*, I determined to try the effect of known plants as ſoon as I ſhould return to the country. Accordingly I did ſo with the firſt ſun ſhine that I had, and completed the diſcovery. But in the mean ime Dr. Ingenhouſz anticipated me by his publication, which I think I ſhould not have done with reſpect to him, if I had found him in the ſame train of nveſtigation in which he found me.

Such are the *facts*. Let others judge of them as hey ſee reaſon. The aſcertaining any perſon's right o the diſcoveries he makes is of no farther uſe than as a motive to others; ſhewing them that they will not oſe the ſhare of praiſe to which their ſagacity or induſtry ſhall fairly entitle them. As it is now more han twenty years ſince the diſcovery was made, in all which time Dr. Ingenhouſz has enjoyed the merit of t, I cannot be ſaid to have ſhewn much anxiety about it.

Dr. Ingenhouſz in his *Eſſay on the food of plants*, ɔ. 2, ſpeaks of my "known eagerneſs for fame," and alſo that of Mr. Scheele. It has not, however, been very conſpicuous in this buſineſs, and if it be a fault, I think Dr. Ingenhouſz himſelf will be thought to be as chargeable with it as either of us.

3. *Of*

3. *Of the Discovery of dephlogisticated Air.*

NOW that I am on the subject of the *right to discoveries*, I will, as the Spaniards say, leave no ink of this kind in my inkhorn; hoping it will be the last time that I shall have any occasion to trouble the public about it.

Mr. Lavoisier says, (*Elements of Chemistry* English translation, p. 36) " this species of air" (meaning dephlogisticated) " was discovered almost at the same time by Mr. Priestley, Mr. Scheele, and myself." The case was this. Having made the discovery some time before I was in Paris in 1774, I mentioned it at the table of Mr. Lavoisier, when most of the philosophical people in the city were present; saying that it was a kind of air in which a candle burned much better than in common air, but I had not then given it any name. At this all the company, and Mr. and Madame Lavoisier as much as any, expressed great surprise. I told them that I had gotten it from *precipitate per se*, and also form *red lead*. Speaking French very imperfectly, and being little acquained with the terms of chemistry, I said *plomb rouge*, which was not understood till Mr. Macquer said I must mean *minium*. Mr. Scheele's discovery was certainly independent of mine, tho' I believe not made quite so early.

4. *Of*

4. *Of Mr. Humphry Davy's Essays.*

WHEN some progress was made in printing this work, I met with *Dr. Beddoes's Contributions to physical and medical knowledge*, and in it Mr. H. *Davy's Essays*, which have impressed me with a high opinion of his philosophical acumen. His ideas were to me new, and very striking, but they are of too great consequence to be decided upon hastily. I wish so seemingly accurate an experimenter would repeat, with the attention that he would give to all the circumstances, the French experiment of the composition of water. I cannot help suspecting that his account of it would not be so very decisively in favour of their conclusion as theirs.

Mr. Davy takes it for granted that water is decomposed by the growth of plants, and thinks this to be proved by finding dephlogisticated air produced in this manner in water out of which air had been expelled by boiling, or by the air pump. But he was not aware that water even recently boiled, and examined while warm, contains nearly as much air as it did before boiling, and by no means so pure; so that it can probably supply more nourishment to a plant than water which had not been boiled. Air expelled from water by the air pump, or even the Torricellian vacuum, which does it more effectually, is soon replaced by exposure to the atmosphere.

What I complain of in Mr. Davy, and many others, is the too hasty introduction of *new terms* in chemistry. They perplex those who are most conversant with the subject, and are a great impediment to beginners. For the old language will never be wholly ly obſolete while old books are read. If the new theory ſhould not ſtand its ground, many terms in the new nomenclature muſt fall with it. And Mr. Davy's *nitrogen*, I ſuſpect, will be no longer lived than the French *hydrogen*.

I have myſelf been exceedingly cautious in introducing new terms, and have never done it but when there was an abſolute neceſſity to give a name to a ſubſtance that had no name before. Air rendered unfit for reſpiration, or combuſtion, having no name appropriated to it, I, having frequent occaſion to mention it, called it *phlogiſticated* air, becauſe atmoſpherical air I found was reduced to that ſtate by ſubſtances containing phlogiſton, if there be ſuch a thing as phlogiſton (which was then univerſally taken for granted) and by no other means. Having afterwards diſcovered another kind of air, the properties of which were the reverſe of thoſe of phlogiſticated air, I called it *dephlogiſticated* air. I alſo introduced the terms *nitrous* air, *dephlogiſticated nitrous* air, *Marine acid* air, *vitriolic acid* air, *fluor acid* air, *alkaline* air, and *ſulfurated inflammable* air, denominating them from their ſuppoſed conſtituent parts, or their known properties, and I generally conſulted ſome philoſophical friend before I fixed upon any of them.

I ſaw no occaſion for the term *gas*, becauſe I found the term *air* uſed generically already; the term *fixed air* and *inflammable air*, as well as *atmoſpherical air* being in uſe before. If we want an adjective, the *aerial form* of a ſubſtance will do as well as the *gazeous form*; and Mr. Davy, who introduces the term *gazity* to expreſs the abſtract idea in the ſubſtantive form, might have made as good a word of a ſimilar ſignification from *air*.