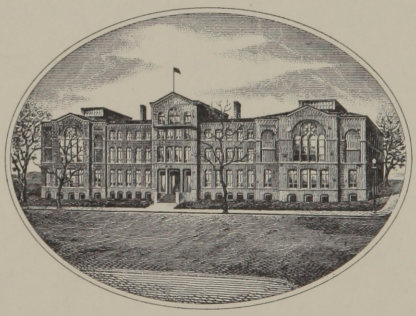
NATIONAL LIBRARY OF MEDICINE

Washington



Founded 1836

U. S. Department of Health, Education, and Welfare

Public Health Service

THE DOCTRINE OF PHLOGISTON *ESTABLISHED*, AND THAT OF THE COMPOSITION OF WATER *REFUTED*.

THE SECOND EDITION WITH ADDITIONS.

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

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

NORTHUMBERLAND:
PRINTED FOR P. BYRNE, No. 72, Chesnut Street, Philadelphia,
BY ANDREW KENNEDY,
FRANKLINS-HEAD, QUEEN STREET.

1803.

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 that 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 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

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

gacity, 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, October 20, 1803.

The PREFACE.

DESIROUS of bringing the important controverſy concerning *phlogiston* to a fair deciſion, 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 the articles being written in direct defence of what I had advanced, and others in reply to objections; and having now, I imagine,

gine, 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 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 appears 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 dowu a falſe light which misleads the mariner, and removing a great obſtacle 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 they are ſtill fewer, 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, are very ample.* And I hope that, conſidering my advanced 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

* 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. Tunostra, 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, his ocia ruris amoeni.
Sed fidi comites absunt, vultusque sereni.
Hoc juvat, hoc cruciat. Nihil illis dulce remotis.

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 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

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 interreſting, eſpecially in the decline of life, and the near proſpect of death, which, if it put a period to our exiſtence, 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 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

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 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 makes men idle, or buſy to no uſeful purpoſe.

Call this, if you pleaſe, the talkativeneſs of 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

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 ſource 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 once more to the ſubject, and I requeſt the favour

favour of an anſwer to my objections. I hope I am not wanting in a proper deference to the opinion of men ſo juſtly eminent as yourſelves and your friends in France, and alſo that of great numbers in England, and wherever chemiſtry is known, who have adopted your hypotheſis. But you will agree with me, that no man ought to ſurrender his own judgment to any mere *authority*, however reſpectable. Otherwiſe, your own ſyſtem would never have been advanced.

As you would not, I am perſuaded, have your reign to reſemble that of *Robespierre*, few as we are who remain diſaffected, we hope you had rather gain us by perſuaſion than ſilence us by power. And tho' we are all apt to flatter ourſelves, we hope we are as willing to be influenced by the former, as we are inflexible to the latter. If you gain as much by your anſwer to me, as you did by that to Mr. Kirwan, your power will be univerſally eſtabliſhed, and there will be no *Vendee* in your dominions.

Differing as we do in this reſpect, we all agree in our wiſhes for the prevalance of *truth*, and alſo of *peace*, which is wanted as much for the intereſts of philoſophy, as thoſe of humanity. And on this account I earneſtly wiſh ſucceſs to the liberty and proſperity of France, which did me the honour to adopt me when I was perſecuted and rejected in my native country. With great ſatisfaction, therefore, I ſubſcribe myſelf

Your fellow-citizen,

JOSEPH PRIESTLEY.

Northumberland in America, June 15th, 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 definitive 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 honourable to him than the diſcovery, or the moſt ſucceſsful defence, of any truth.

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, 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, Oct. 22, 1803.

The PREFACE

TO THE

SECOND EDITION.

THE first edition of this pamphlet was published in the beginning of the year 1800, and it comprized the substance of several proceeding publications on the same subject, after I had heard the observations that had been made on them by the chemists in America and also those in France, the authors of the system that I controvert. I wrote with a view to promote the discussion of a question that is acknowledged to be of very great importance in chemistry, and to which I had not been able to engage much attention; tho' some, and the most material, of my objections had been before the public as soon as it was generally known, and it is now nearly twenty years since this new theory was advanced.

At length, however, I found that I had so far succeeded, that Mr. Cruikshank replied to my pamphlet, tho'

tho' to not more than to an argument from one of my experiments, and that the principle on which he replied is greatly approved by the advocates for the new chemiſtry in France.

To Mr. Cruikſhank's animadverſions (publiſhed in Mr. *Nicholson's Journal* Quarto vol. 5, p. 1) I replied, and on the twenty ſecond of the ſame month, viz. March 1802, in which my anſwer was publiſhed he dates his reply to it, written with an air of the moſt perfect perſuaſion of his ſuperiority in the argument. Immediately on the receipt of this, which (living as I do at a great diſtance and the communication by ſea eſpecially in time of war being ſlow) could not be very early, I wrote my reply, and it was publiſhed by Mr. Nicholſon in his Journal for Feb. 1803.

In this I expreſsed great ſatisfaction in the diſcuſſion being entered upon by a perſon ſo well qualified to do juſtice to his argument as Mr. Cruikſhank is univerſally acknowledged to be, and I invited him to diſcuſs the other articles of the difference between the two ſyſtems, ſeveral of which are of more importance than that which he undertook to canvaſs.

Ready as he was to reply before, I fully expected his anſwer in the month following the publication of mine; but having before me five ſucceeding numbers of the Journal, in none of which is there any notice of my laſt communication by Mr. Cruikſhank, or any other perſon, and there being a demand for a new edition of this pamphlet, I have not thought it neceſſary to wait any longer; and my anſwer to his objections, with ſome little addition, will be found in the fourth ſection. Still,

however, if I hear any thing farther on the ſubject, from Mr. Cruikſhank, or any other chemiſt of note, in England or elſewhere, I ſhall not fail to notice it in ſome way or other; and I hope I am not thought to be ſo much attached to any opinions I have advanced, but that I ſhall frankly acknowledge any miſtake that I may have inadvertently fallen into. Of this, however, our readers will be the proper judges.

I refer the reader to ſeveral judicious and well conducted experiments of Dr. Woodhouſe, Profeſſor of chemiſtry in the univerſity of Philadelphia, in an *Appendix to the American edition of Parkinson's Chemical pocket book*, on the calces of ſeveral of the metals, the reſults of which are manifeſtly inconſiſtent with the principle of one part of the new theory, which the ingenious experimenter conſequently abandons. In my opinion the abandonment of any of its parts muſt lead to that of the whole, all the parts of it having the cloſeſt connection with each other; but the perception, and acknowledgment of this, muſt be the work of time and reflection.

Dr. Woodhouſe clearly proves that the air that is procured from charcoal and water will by no means warrant the concluſion that Mr. Lavoiſier draws from it, and that my inference from it is juſt, p. 209. He ſhews that water is the principal agent in producing part of the inflammable and fixed air that is got from charcoal and the calces of ſeveral of the metals, eſpecially that from finery cinder; that "if the oxygen "in them was the ſole agent, the inflammable air pro- "cured from them in theſe circumſtances could not be

"obtained

"obtained; and that the flowers, or white oxide of zinc, "frequently affording inflammable and no fixed air, "when ſubjected to heat with coal, cannot be account-"ed for by the new doctrine." p. 216.

But if the calces of theſe metals, eſpecially that of iron, be not perfect oxides, the proof of the *decomposition of water* muſt fall to the ground; and there, I am now pretty confident, it lies, and that all efforts to raiſe it will be as ineffectual as thoſe of Mr. Cruikſhank.

My labours of every kind now rapidly drawing to a cloſe, I propoſe ſoon to reprint ſeveral articles of obſervations and experiments that are publiſhed in the *Transactions of the Philosophical Society in Philadelphia*, in *Mr. Nicholson's Journal*, and the *Medical Repository* of New York, and a few other articles not yet publiſhed. Theſe, together with this tract in defence of the doctrine of phlogiſton, will make a ſeventh volume of my philoſophical writings, or a fourth to thoſe that are abridged and methodiſed in three, and they complete all that I have been able to do for the advancement of natural ſcience.

Conſidering the many, and ſometimes long, interruptions of my labours, eſpecially in conſequence of removals, as from Warrington, where I began my experiments in *electricity*, to Leeds, where I began thoſe on the *different kinds of air*; from Leeds to Wiltſhire, when I ſpent the winters in London, and during which time I accompanied the Marquis of Lansdown in a tour through Flanders, Holland, part of Germany, and France; from Wiltſhire to Birmingham, where all my

philoſophical

philoſophical apparatus was deſtroyed; from Birmingham to Hackney, where my ſtay was too ſhort to admit of my doing much buſineſs of any kind; and laſtly from Hackney to this country; and here, ſo often from one houſe to another (the inconvenience and loſs of time attending which only an experimenter can form an idea of) together with many diſadvantages under which I now neceſſarily labour, where almoſt every thing relating to experimental philoſophy and chemiſtry muſt be had from Europe; when, I ſay, theſe circumſtances are conſidered, and that I did not (for want of opportunity) make any original experiments till I was near forty, I hope it will be thought that I have not been deficient in point of *induſtry*, whatever may be thought of the value of my labours.

I am truly thankful to the ſovereign diſpoſer of all things, and to thoſe friends of ſcience who have defrayed the expences of my laboratory, that I have been able to do ſo much; and I hope my liberal benefactors will not repent of their generoſity. Indeed, the greater part of them are gone before me, to a ſtate in which I ſhall hope to join them, where I can again expreſs my gratitude, and when we may again unite our reſpective abilities in the ſame pleaſing purſuits; ſeeing more of the wiſdom and goodneſs of the great creator, and having our admiration and devotion excited to a greater height than ever.

The

The CONTENTS.

PAGE

THE INTRODUCTION, 1

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 Consideration.* - - 5

Sect. II. *Of Finery Cinder.* - 12

Sect. III. *Of inflammable Air from Finery Cinder and Charcoal,* - - 21

Sect. IV. *Of Mr. Cruikshanks Hypothesis to account for the Formation of inflammable Air in the Proceſs of heating finery Cinder and Charcoal.* - - - 27

Sect. V. *Of the Calces of Zinc.* - 33

Sect. VI. *Arguments in Favour of the Doctrine of Phlogiſton from ſome Circumſtances in which Sulphur is formed, and nitrous Acid phlogiſticated.* - - - 40

Sect.

PAGE

Sect. VII. *Of the Calces of Mercury.* 48

Sect. VIII. *Experiments on heating Manganese in inflammable Air.* - - 55

Sect IX. *Of the Decomposition of Water* 58

Sect. X. *An Argument against the Decomposition of Water from the different Proportions of the Elements of which it is supposed to consist, according to different Experiments.* 70

Sect. XI. *Of the supposed Decomposition of Water in the Experiments of Van Troostwick and Deiman, in those of Mrs. Fulhame, and various other Processes.* - 74

Sect. XII. *Of the Constitution of Fixed Air* 83

Sect. XIII. *Of Phlogisticated Air.* 91

The Conclusion, - - 102

APPENDIX.

1. *Of Dr. Mitchill's Attempt to reconcile the two Systems.* - - - 105

2. *Of the Discovery of the Production of dephlogisticated Air by the Action of Light on Plants,* - - 107

3. *Of the Discovery of dephlogisticated Air.* 116

4. *Of Mr. Humphry Davy's Essays.* 117

ERRATA.

PAGE 29 l. 9, *after* unite, *add* with the carbone

—— 41 l. 17, *dele* of

—— 102 l. 5 *from the bottom after* procured, *add* by means of water.

—— 108 Note l. 8, *after* upon, *add* them

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. Tho'

 there

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 the principal philoſophers and chemiſts of England, notwithſtanding the rivalſhip that has long ſubſiſted between the two countries, eagerly adopted it. Dr. Black in Edinburg, and, as far as I hear, all the Scots have declared themſelves converts, and what is 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 intirely 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.

tury. So firmly eſtabliſhed has this new theory been conſidered, that a *new momenclature*, 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 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 re-

flection 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 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

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 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

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 eighty five parts of the former, to fifteen 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 phogiſtians 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 dephlogiſtated air, called by them *oxygenous gas*. The former is not pretended, and ſo far

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 vitrolic 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 one hundred and thirty 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 fifty two ounce meaſures of air, of the ſtandard of . 55, which is much leſs pure. This ſhews that this precipitate is ſo far from containing more oxygen, that it contains leſs than the acid. It is in reality poſſeſſed of the oppoſite principle, which is agreeable to the phlogiſtic theory. For ſince much more inflammable air is procured from iron by means of ſteam only, than by its ſolution in any acid, more of the principle of which inflammable air conſiſts, viz. phlogiſton, muſt adhere to this calx of iron than to the other.

Dr. Maclean ſays p. 19, " There is the moſt ſatisfactory evidence that iron, after its ſolution in ſulphuric acid is in a ſtate like that of the black oxyd, or finery cinder." But the dephlogiſticated air that 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 eafily diffolves it. It, therefore, fhews no fign of its containing any oxygen at all. The new theory, however, requires that it be dignified with the appellation of the *black oxyd of iron*. The black oxyd of *manganese* gives more evidence of its right to the name they have given to it, 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 antiphlogiftic 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.

If this black calx that remains after heating this folution of iron in the acid contain all the oxygen both of the water expended, and of the deficiency of

acid

acid in the ſolution, it muſt be doubly oxygenated, containing more oxygen than an equal quantity of common finery cinder; a difficulty of which, I imagine, the advotates for the new theory were not aware.

There is ſomething extraordinary in the manner in which the antiphlogiſtians ſuppoſe that metals become ſoluble in acids. Mr. Adet ſays, p. 60, "Experiments "prove that metals, in order to be combined with an "acid, require to be united with oxygen;" and explaining himſelf farther, he ſays, "In reality, a metal not "combining with acids but when it is in a ſtate of ox-"ide, and not paſſing into this ſtate but by its union "with its oxygen, muſt neceſſarily abſorb oxygen in "order to unite with the acid. But this oxygen can on-"ly be ſupplied by one of theſe two ſubſtances, the "acid, itſelf, or the water which it contains. If the "oxygen had been given by the acid, it would have "been in part decompoſed, and would in conſequence "have ſaturated leſs alkali. But ſince it ſaturates the "ſame quantity of 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 oxigen univerſally, what could prevent the iron from ſaturat-

ing

ing 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 ſo 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, 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 acids yields inflammable 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 can not 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 inflammable and phlogiſticated air, the ſame principle which is denominated *phlogiston*, muſt enter into them both.

SECTION

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

"of the muriatic acid by the ſolution of finery cinder, "is owing to the latter retainiug the oxygen ſo ſtrongly, "as not to be diſengaged by the action of heat, aided "by the attraction of the muriatic acid." To this I anſwer, that if the acid had not been able to diſſolve this ſubſtance, this might have been ſaid with ſome degree of plauſibility; but ſince it does diſſolve it completely, ſo volatile a thing as oxygenous gas, of which it is ſuppoſed to contain ſo large a quantity, and with which this acid has ſo ſtrong an affinity, could hardly eſcape being evolved.

Meſſrs. Berthollet and Fourcroy ſay that "finery "cinder, like maſſicot, is unable to dephlogiſticate ma-"rine 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 oxyge-"nated" (*Annales de Chymie*, Vol. 26, p. 305) evidantly taking it for granted, that fine.y 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 ſub-"ſtance, that it ſhould likewiſe ſeparate oxygen from "iron, when this is united to a comparatively ſmall "quantity." But finery cinder, if, as all antiphlogiſti-

ans

ans 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 converting it into minium, is only about one tenth of itt 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 by oxygen, because the compound reſembles in every reſpect, as 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 oxygen and iron 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 qaantity of oxygen may enter the iron along with the water to which it was united; as

few

few ſubſtances are perfectly ſeparated from each other by any chemical affinity.

When the 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 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

fenſive 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 ſixty 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 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

air in which it is heated that iron has, I conclude that they 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 manganeſe 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 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

* 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.

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 one thouſand two hundred and ſeventy ſeven grains of ruſt, I got forty five 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 thirty or forty grains to an ounce; whereas the addition to an ounce of iron when it is converted into finery cinder is about two hundred 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 mer-
"cury is reduced in hydrogen gas, that diſappears, no
"oxygen gas is obtained, but a quantity of water may
"be collected." Now I am confident that no perſon who had ever ſeen the experiment could have written this. The quantity of water that appears in this caſe is barely perceivable, being no more than ſufficient to conſtitute the baſe of the inflammable air imbibed by the

calx,

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 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 fifteen parts of the former to eighty five 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 that is found in this proceſs was never ſuppoſed 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 ſection concerning finery cinder, I muſt take notice of what Dr. Maclean too confidently advances about it. "The Doctor," he ſays p. 26, "is certainly miſtaken in ſuppoſing that finery cin-
"der cannot ruſt. Mr. Fourcroy ſays it ruſts ſooner
"than common iron, and every apothecary knows it
"does ſo. If the ruſt of iron be made red hot in a re-
"tort, a quantity of carbonic acid is diſengaged from
"it, and the iron remains in a ſtate of black oxyd. The
"ruſt therefore is a *carbonate of iron*, and muſt contain
"all the principles which compoſe the black oxyd, and
"therefore can contain nothing capable of excluding
"that which would convert it into ruſt." But, in direct contradiction to what he aſſerts, I ſtill ſay that finery cinder is not ſubject to ruſt. In England no uſe having been made of it before it was attended to by my brother-in-law Mr. John Wilkinſon (one of the moſt intelligent and ſucceſsful of all the iron-maſters in that or any other country) but to mend the roads, it has lain in heaps for years, I may even ſay ages, without acquiring the leaſt tinge of brown. All my ſpecimens have ever remained free from ruſt, and the phyſicians, who are

alſo

alſo apothecaries in this place, aſſure me they never ſaw or heard of any ſuch thing. They get it from the blackſmiths in the form of *scales of iron*, and the blackſmiths ſay the ſame. It muſt therefore, as I have obſerved, be ſaturated with ſome principle very different from that of the common ruſt of iron, and is by no means the ſame thing, notwithſtanding what Dr. Maclean ſays to prove the contrary. If finery cinder be ever converted into ruſt, which I have never found to be the caſe, it muſt, by ſome proceſs 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 experiments 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 inflammable air, and in great quantity, is procured in this process, the antiphlogistians are under a necessity of finding *water*, by the decomposition of which, and in no other way, they say it is made; and some of them find it in the charcoal, and others in the finery cinder.

As Dr. Woodhouse repeated this experiment with peculiar exactness, I shall copy his account of it from the *Philosophical Transactions of Philadelphia*, Vol. iv, p. 464. "An ounce of the scales of iron, and the same

"quantity

"quantity of charcoal, were reduced to a very fine "powder, and expoſed ſeparately in covered 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 tirtura-"ted with a red hot peſtle, formed of an iron ramrod, "were poured upon a red hot ſheet of iron, and inſtant-"ly put into a red hot gun barrel, which was fixed in "one of Lewis's black lead furnaces, and which com-"municated with the worm of a refrigeratory, a part "of a hydropneumatic apparatus. Immediately after "luting one end of the gun barrel to the worm, one "hundred and forty two ounce meaſures of inflamma-"ble air came over in torrents, mixed with one tenth "part of carbonic acid gas."

Nothing more could have been done to exclude all water from each of the ſubſtances previous to their mixture; and yet we immediately find the effects of water as much as if water itſelf had been employed inſtead of the finery cinder, which no doubt, contained it. This experiment I ſhould have expected, might have converted the ingenious author of it himſelf. His explanation of it however, is ſo unſatisfactory that I cannot help thinking the conſideration of it will go a great way towards the converſion of others. For he admits that there really is *water*, and in this great quantity, in the finery cinder.

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 princi-

ple 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 charcoal 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 ſuppoſe to be 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

"ſay cannot be ſeparated from it but by forming a new "combination with it." *Annales de Chymie*, Vol. xxvi, 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 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 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 Mr. Cruikshank's Hypothesis to account for the Formation of inflammable Air in the Process of heating finery Cinder and Charcoal.

THE two anſwers to my argument in favour of the doctrine of phlogiſton conſidered in the preceding ſection go upon the proper principle of the new theory, viz. that whatever is *inflammable* in any kind of air muſt come from *water*. But Mr. Cruikſhank, ſeeing, no doubt, the impoſſibility of finding, or decompoſing, water in the caſe, replies upon a principle intirely new; ſince he requires no water at all for the formation of the very great quantity of inflammable air that is produced in this proceſs. He even maintains that this air, tho' it be evidently as inflammable as that from charcoal and water, is an *oxide*, calling it the *gazeous oxide of carbone*, ſuppoſing it to be formed by the union of oxygen from the finery cinder and carbone in the charcoal, but in a very circuitous way. For he ſuppoſes, in the firſt inſtance, that a quantity of fixed air is formed by the oxygen of the finery cinder uniting with carbone in the charcoal, and that this fixed

air

air is decompoſed by one of its conſtituent parts (viz. its oxygen) going back into the finery cinder, and reconverting it into iron; which iron he ſays, by means of heat, decompoſes the fixed air, ſo that part of its oxygen can unite with the carbone in the charcoal. Thus, he ſays, will be formed this *gazeous oxide of carbone* without any water.

This hypotheſis, ſtrange as it appeared to me, and even inconſiſtent with the moſt fundamental principle of the new theory, I found to be quite ſatisfactory to the original authors of this theory in France. One of the moſt eminent of them, in converſation with a friend of mine at Paris, ſaid that "I had kept them for ſome "time in torture by my objections to their ſyſtem, but "that they were intirely relieved by Mr. Cruikſhank." In conſequence of this information, I have given more attention to this hypotheſis than many perſons will probably think to have been neceſſary, and I have replied by ſtating the following objections to it, each of which I cannot help thinking abundantly ſufficient to refute it.

1. Mr. Cruikſhank's hypotheſis requires that, in the proceſs of heating finery cinder and charcoal, the oxygen in the finery cinder ſhould quit that ſubſtance, and unite with carbone in the charcoal, in order to form fixed air. Since, however, this fixed air is to be decompoſed by iron, the oxygen which it has got from the finery cinder muſt be ſeparated from it, and enter into the ſame calx again, for there is nothing elſe to receive it. But while the heat continues the ſame, I deem theſe contrary effects to be impoſſible. If the degree

of

of heat that is applied expel oxygen from the calx, it will certainly prevent its return. Conſequently, if fixed air could be *formed*, it cannot be *decomposed* in theſe circumſtances.

2. I ſee no occaſion that Mr. Cruikſhank had for the formation of this *fixed air*, which, after all, is merely *hypothetical*, as it cannot be exhibited in any part of the proceſs. Why could not the oxygen in the finery cinder unite, in the requiſite proportion, to form the partial oxide required? For the hypotheſis does not admit of a complete oxidation, ſince then it could not take more on being fired with dephlogiſticated air. The finery cinder can only take from the fixed air the very ſame thing that it gave out to form it.

3. If it was poſſible for oxygen to be ſeparated from finery cinder without any thing entering into it (which I think cannot be done) it would not, according to the new theory, form any thing by this union with carbone beſides *fixed air*, this being ſaid to be *carbone diſſolved in oxygen gas*. Of this Mr. Cruikſhank is ſenſible, and therefore he was under the neceſſity of ſuppoſing that, after the oxygen had quitted the finery cinder, it ſhould in part enter into it again. But if this were poſſible, nothing would remain of the fixed air but *carbone*, as before that union. For the oxygen that was left in it could only unite with the ſame proportion of carbone, and this is always a *ſolid ſubſtance*, incapable, without the aid of oxygen gas (or dephlogiſticated air) of aſſuming the form of *air*. Whence, then, comes the *inflammable air* in this proceſs, which ſo nearly reſembles that from char-

coal

coal and water, that they muſt of neceſſity have the ſame origin? And with reſpect to *this*, Mr. Lavoiſier decides that, whatever is *inflammable* in it muſt come from *water*.

4. Admitting all that Mr. Cruikſhank alleges concerning the difference in ſpecific gravity, and other circumſtances, between the air from finery cinder and charcoal, and that from water and charcoal, it is not ſo great as the difference between the latter and the light inflammable air from metals with acids or water. Different as they may be in other reſpects, they are all *inflammable*, having the common property of uniting with oxygen in a certain degree of heat; in conſequence of which they are alike the very reverſe of *oxides*, and muſt be claſſed with *combustible* ſubſtances, equally with ſulphur and phoſphorus.

If this kind of air was a real *oxide*, it would appear to be ſo on the decompoſition of it, when, to make the reſult unexceptionable, the oxygen it contained would either take the form of dephlogiſticated air, or become a component part of ſome other ſubſtance into which oxygen is acknowledged to enter. But this has not been done. When it is decompoſed by being fired together with dephlogiſticated air, the fixed air which is the formed comes, I have no doubt, from the oxygen in the dephlogiſticated air, and the phlogiſton in this ſpecies of inflammable air; the ſame being the reſult, tho' not in the ſame degree, of firing the heavy inflammable air from charcoal and water, from oil &c. &c, into which it is not pretended that any oxygen enters.

It,

It, therefore, appears to me to be an abſolute abandonment of the moſt fundamental principle of the new theory to call the air from finery cinder and charcoal an *oxide*. If ſubſtances be *combustible* in proportion to their affinity to oxygen, and their conſequent readineſs to unite with it, this air, which is inflammable, muſt be of *this* claſs, and therefore the very reverſe of the *oxides*, which are ſaturated with oxygen, and incapable of receiving more.

5. If the oxygen, after quitting the finery cinder, entered into it again, it would make it finery cinder as at the firſt, or at leaſt in ſome degree; whereas the calx is completely revived in this proceſs, the iron ſo revived being as ſoluble in acids as any iron whatever.

6. If the iron ſhould be completely revived by the oxygen wholly leaving it, I maintain that it could not, by any degree of heat, decompoſe fixed air. For my experiment with a *burning lens*, in which it could not be done, is far more unexceptionable than Mr. Cruikſhank's with *bladders and a gun barrel*. His objection to my proceſs has no weight. It was made with only a very few ounce meaſures of the air, over mercury, with a lens ſixteen inches in diameter, and was continued ſeveral hours, generally from ten o'clock to one; ſo that no particle of the air could eſcape being expoſed to a far greater degree of heat than could be communicated through a gun barrel.

His experiment I have frequently made, both in England and here, but I could never be ſatisfied with the reſult. The *scale* which he found upon the iron, I have no doubt, came from *moisture* in the air, or from the

the bladders. Indeed, I cannot think that any perſon, converſant as I have been, with both theſe modes of operating, can heſitate in declaring that the preference muſt be given to mine.

7. Mr. Cruikſhank ſeems to think that charcoal cannot contain any oxygen; but Mr. Tenant's fine experiment deciſively proves that it does. For where are we to look for the oxygen (which we all acknowledge to be a component part of fixed air) which is ſeparated from the marble, but in the charcoal which is produced? And in that it makes part of a *ſolid ſubſtance*, and does not take the form of *air*.

8. Since oxygen and all combuſtible ſubſtances unite, and explode together in a certain degree of heat, the oxygen that is expelled from the finery cinder, uniting with carbone from the charcoal when *red hot*, muſt enable it to burn, and therefore, in theſe circumſtances, there ought to be an *explosion*, or at leaſt a gradual *combustion*, of them in the courſe of the proceſs, as there is when oxygen is put to the ſame ſubſtance, and heated with it afterwards. Here, however, oxygen meets carbone, a combuſtible ſubſtance, in a red heat, without any exploſion or combuſtion taking place.

Thus do the antiphlogiſtians boaſt of the diſcovery of * a *new oxide*, when they are unable to prove that it contains a particle of oxygen, and when its obvious properties ſhew that it belongs to a claſs of ſubſtances the

* Mr Chenevix, a chemist of the first class in the new school, speaking of Mr. Cruikshank, calls him very justly "an able chemist, to whom "we are indebted for the discovery of the gazeous oxide of carbone." See *Philosophical Transactions* for A. D. 1802, p. 133.

the very reverſe of oxides. If the *discovery*, as it is called, relates to the *substance*, it belongs to me. All that they can pretend to is having given a better account of the *nature* of it, and with what ſucceſs they have done this let the impartial reader judge.

SECTION V.

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 three hundred 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 eighty five parts of oxygen to fifteen of hydrogen (into which it is 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 I also 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

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; 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 oxygen 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 was that by caustic volatile alkali. 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 six and an half to eight 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 means be reduced to a complete calx.* But the imperfect calx to which the metal is then reduced, does not appear to contain any oxygen. When it was heated in atmospherical

* Since this was first printed in the *Medical Repository*, I find that, by long standing, the surface of these filings of zinc is become white, so that they are perfect flowers of zinc.

pherical air the quantity of the air was increaſed, and about one twentieth part of it was fixed air, and the remainder was of the ſtandard of 1. 5. The water in which the filings of zinc had been immerſed, gave out air much worſe than common air, and it was perfectly free from acidity. Iron filings will alſo yield inflammable air in water, and this water alſo 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, becauſe the dephlogiſticated air in the atmoſphere diſappears when they are calcined in it. But there is reaſon to think that the greateſt part of the addition of weight which they by this means acquire is from *water*, while the oxygen attaches itſelf to other ſubſtances in preference to the calx, if they be preſent.

One inſtance of this is that when they are calcined with a burning lens over lime water, the lime is precipitated; whereas if the calx had imbibed all the dephlogiſticated air that diſappeared, the lime water would not have been affected in the proceſs; this precipitation of the lime, coming, no doubt, from fixed air, which I have ſufficiently proved to conſiſt of dephlogiſticated air and phlogiſton, or the baſe of inflammable air. I had this reſult when I calcined iron, copper, zinc, tin, lead, biſmuth, and regulus of antimony in theſe circumſtances. But when the proceſs was made over mercury, I could not always find any 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

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 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 gives 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 VI.

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 in a cloſe veſſel 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 through 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

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 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 of 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 dimunition 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 through 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. 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 through 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

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 VII.

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

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, tho' I did not know it at the time, that it is not completely reduceable by mere heat. But "later observations," Dr Maclean says, p 11, "shews 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. Adet says, p. 43, "that the yellow oxide of mercury 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,

* 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.

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 authoriſed 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 one hundred and twenty one ounce meaſures of the air were reduced to ninety five. 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. Computing from this reſult, it will be found that it contained 7. 22 ounce meaſures of pure air which added to the twenty ſix 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 inflammable air. Neither had it, in this caſe, contributed to the formation of fix-

ed

ed 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 thirty ounce meaſures of inflammable air, till twelve 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 ſixty 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, ſixty ounce meaſures could not be expelled without the abſorption of three hundred and twenty three ounce meaſures of inflammable air; and ſince mercury gains, as Mr. Chaptal ſays, about 8 per cent. in being converted into precipitate, an ounce of mercury muſt contain three hundred

and

and ſixty two ounce meaſures of inflammable air, or rather the phlogiſton that enters into it. An ounce of lead, I have ſhewn, requires one hundred and eight ounce meaſures of inflammable air, an ounce of biſmuth one hundred and eighty five, of tin three hundred and ſeventy ſeven, of copper from verditer, four hundred and three, and of iron eight hundred and ninety.

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 through the hot 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 three hundred and ſixty two 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 through 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? The *electric matter* paſſes readily through glaſs when it is very hot, tho' it will not when it is cold.

Light certainly paſſes through glaſs; and is known to give to ſome ſubſtances colour, ſmell, and taſte, which

which have usually been ascribed to phlogiston. That it does not revive the lead in passing through 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 can. Mr. Scheele supposed that even dephlogisticated air, or the essential element of it, passed through glass.

I have frequently repeated this experiment of the revival of red precipitate in inflammable air, and have never failed to find a great absorption of it, whether there was any fixed air in the remainder or not; and I should have repeated it much oftener, and on a larger scale, in order to ascertain with more exactness the quantity of inflammable air, or of phlogiston, contained in a given quantity of mercury, but that it has frequently happened that the vessels in which I made the experiments were exploded, after a sufficient 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 greatest part of the inflammable air to disappear without any explosion.

The accuracy of this experiment being questioned by Dr. Woodhouse, I repeated it with all the attention I could give to it, and had the following result. I heat-

ed

ed a quantity of red precipitate in twenty eight and an half ounce meaſures of inflammable air, till it was reduced to twenty four and an half, and found that, whereas before the proceſs it was not in the leaſt affected by nitrous air; the ſtandard of it was afterwards 1. 8; ſo that it contained a conſiderable mixture of dephlogiſticated air. I repeated the experiment ſeveral times, and always found pure air mixed with the inflammable, when I had revived any part of the calx.

Continuing one of theſe proceſſes, till, after the diminution, the quantity of air began to increaſe, there was an exploſion; but it only raiſed the receiver in which the air was confined about an inch, and recovering its poſition, it broke the earthen diſh in which it was placed.

After this I made uſe of a tin diſh, and repeating 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, it 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 a alike as poſſible, and
" that I might compare them to the greateſt advantage,
" I made them immediately one after the other, and
" with every circumſtance as nearly as I could the ſame.
" The inflammable air was the ſame in both experi-
" ments, and the ſcales of iron, and the precipitate,
" were made as dry as poſſible. They were heated in
" veſſels of the ſame ſize and form, and equally confin-
" ed by dry mercury. And yet when I heated the
" former, water was formed as copiouſly as I have deſ-
" cribed 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 heat-
" ed the precipitate till eight ounce meaſures of the air
" was abſorbed, and only three fourths of an ounce mea-
" ſure remained, there was hardly any ſenſible quanti-
" ty of water produced, certainly not one tenth of what
" appeared in the experiment with the ſcales of iron.
" There was this difference, however, in the two re-
" ſults. In what remained from the experiment with
" the precipitate I at this time perceived a ſlight ap-
" pearance of fixed air, whereas there was none in
" what remained in the ſcales of iron. The reſiduum
" alſo from the precipitate had in it a ſmall portion of
" dephlogiſticated air. For being mixed with an equal
" meaſure of nitrous air the ſtandard of it was 1. 8. In

" this

"this experiment there can be no doubt but that the "dephlogiſticated air diſlodged from the precipitate "mixed with the inflammable air in the veſſel, and as "no water was produced, they muſt have formed ſome "more ſolid ſubſtance, which in the ſmall quantity I was "obliged to uſe could not be found."

At this time, however, I think it more probable that nothing *ſolid* was produced, but only that the phlogiſton 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 veſſel, and in part united with it and formed fixed air.

In nine ounce meaſures of inflammable air from malleable iron and water I revived part of the precipitate ſent me by Mr. Berthollet, (which I had found to contain no fixed air) till not more than one fourth of the air remained unabſorbed; and examining it, I found one twentieth part of it fixed air. But mixing nitrous air with it, it appeared that the air diſlodged from the precipitate had not wholly united with the inflammable air. For being mixed with an equal quantity of nitrous air it occupied the ſpace of 1. 71. After the proceſs I miſſed eighteen grains of the precipitate. But there are ſeveral cauſes of loſs in this caſe, beſides that from the air expelled from it.

In 5. 5 ounce meaſures of the ſame inflammable air I again revived ſome of the ſame precipitate till it was 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 a 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 from them that no *water* was formed in the process, and this the new theory absolutely requires.

On the whole, I think it can hardly be denied that considering the great quantity of inflammable air that disappears in these experiments, the greatest part of it, at least, must enter into the calx. And since all running mercury must consist of the same elements, the same principle that (with the addition of water) forms inflammable air, and which we call phlogiston, must pass through the red hot glass when the calx of mercury is revived without addition, by means of heat only.

Some experiments that I have made on silver, gold, and platina, favour this hypothesis. All these metals yield a considerable quantity of nitrous air, when they are dissolved, the first in nitrous acid, and the last two in aqua regia. And when the solutions were evaporated, and the residuums heated in inflammable air, a great quantity of it disappeared, and the metals were revived. And yet by means of the same acids these dry residuums will yield a great quantity of nitrous air. They must, therefore, have acquired by means of heat only, and this transmitted through a vessel not red hot, the same principle that was communicated to them by imbibing inflammable air.

That nitrous air contains the same principle with inflammable air, or phlogiston, appears from the following

lowing experiment, in which the former was produced 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 twenty one ounce meaſures of inflammable air till the veſſel was filled with red vapour, when it was reduced to ſix 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 twenty grains of iron yielded ſixteen ounce meaſures. When, with other views, I endeavoured to aſcertain the quantity of inflammable air that was yielded by malleable iron, I found that one hundred and twenty grains of it yielded ninety ſix 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 twenty two grains of gold gave eight 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 one hundred and ſeventy ounce meaſures to the ounce. It is, however, more than is contained in lead, but leſs than in biſmuth, and much leſs than in mercury.

That ſomething can paſs through 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

SECTION VIII.

Experiments on heating Manganese in inflammable Air.

SOME experiments that I made on heating manganeſe in inflammable air authorize the ſame general concluſion with thoſe on heating red precipitate in that kind of air, and therefore deſerve to be recited.

Manganeſe and red precipitate of mercury are ſubſtances of a ſimilar nature, as they are both calces of metals, and both contain dephlogiſticated air, and, like all other calces of metals, they imbibe the inflammable air in which they are heated. But whereas the dephlogiſticated air expelled from the precipitate, frequently at leaſt, remains mixed with the inflammable air (which is ſometimes the cauſe of dangerous exploſions) this has never happened to me in conſequence of heating manganeſe in the ſame circumſtances, ſo that it may be operated upon without any apprehenſion.

Tho' in theſe experiments the inflammable air diſappears, and dephlogiſticated air is expelled from both the ſubſtances, no *water* is formed by means of either of them, as the new theory abſolutely requires, and which is pretended to be the caſe when finery cinder is heated in the ſame circumſtances. But ſince the

ſame quantity of inflammated air diſappears in all theſe caſes alike, the ſame quantity of water, if inflamable air be readily a conſtituent part of it, ought to be produced. It is evident, therefore, that the water produced by means of finery cinder was actually contained in that ſubſtance, and only expelled by the heat, to make room for the phlogiſton which then enters into it, by the abſorption of the inflammable air; while the precipitate and manganeſe, containing no water, imbibe the inflammable air without any appearance of water, except the little that is the baſis of the inflammable air, or may be concealed in ſuch powdery ſubſtances as manganeſe and the precipitate are.

The principal difference, however, in the reſult of theſe two ſimilar proceſſes is that in the experiment with the manganeſe a quantity of *phlogiſticated air* appears to be generated, which is not the caſe with the precipitate. The particulars of the experiments were as follows.

Fifteen ounce meaſures of inflammable air were reduced to ſeven by heating in it twenty four grains of manganeſe. In the reſiduum, which was not affected by nitrous air, there was no fixed air; but when a certain portion of it was fired together with a certain quantity of dephlogiſticated air, the dimunition was only to 0. 95; whereas when the ſame quantity of the ſame original inflammable air was exploded in the ſame circumſtances, the dimunition was to 0. 52. In another ſimilar experiment, in which the inflammable air was confined by mercury, and in which ſeven ounce meaſures were reduced to one and a quarter, the dimunition

with

with the reſiduum was to 0. 93, when that with the original inflammable air was to 0. 33. The manganeſe I uſed in theſe experiments yielded dephlogiſticated air without any mixture of fixed air.

I repeated this experiment with manganeſe out of which air had been expelled with a ſtrong heat, and the reſult was the ſame as with the freſh manganeſe, except that much more heat was neceſſary to make it imbibe the air; whereas the manganeſe from which the dephlogiſticated air has been expelled imbibes inflammable air very rapidly, and with a very moderate degree of heat. Twenty grains of it imbibed eight ounce meaſures of inflammable air, and then weighed nineteen grains. In both the kinds of manganeſe the central part on which the focus of the lens fell was of a beautiful green colour, but the next day this had diſappeared, and the whole of the ſubſtance that had been heated was brown.

As no doubt dephlogiſticated air is expelled from the manganeſe in theſe experiments, and neither fixed air nor water is produced, the dephlogiſticated air muſt enter into the fixed air that is found in the reſiduum.

In order to make a more exact compariſon between the experiments in which marganeſe and finery cinder were made to imbibe inflammable air, I repeated them both in ſimilar veſſels, with equal quantities of inflammable air confined by mercury; and I obſerved that, tho' ſome water appeared during the proceſs with the manganeſe, it wholly diſappeared the next morning; whereas the inſide of the veſſel in which the finery cinder had been revived was wholly covered with innumerable drops of water, contiguous to one another.

SECTION

SECTION IX.

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 recompoſition, "of water. And how can we doubt of it, when we ſee "that, in burning together fifteen grains of inflammable "air, and eighty-five of vital air, we obtain 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 philoſophy. We might "even

"even queſtion whether vitriolated tartar be compoſ-
"ed of vitriolic acid and fixed alkali, or ſal ammoniac
"of the marine acid and volatile alkali, &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. Noth-
"ing perhaps more clearly 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 ex-
preſſed 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 an *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 inflam-
mable air.

But in order to prove that this addition of weight to
the iron is really oxygen, they ought to be able to exhi-
bit it in the form of dephlogiſticated air, or of ſome other
ſubſtance into which oxigen 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

appearance from the finery cinder, being *red*, and not *black*; and when treated in similar processes, exhibits very different results. Mr. Fourcroy says, (Ibid p. 251) that this finery cinder is "iron partially oxygenated." But if that were the case, it would go on to attract more oxygen, and in time become a proper rust of iron completely oxygenated; whereas this is so far from being the case, that as I have observed finery cinder never will acquire rust; which shews that the iron in this state is saturated with some very different principle, which even excludes that which would have converted it into rust.

However, neither this, nor any other calx of iron, can be revived unless it be heated in inflammable air, which it eagerly imbibes, or in contact with some other substance which has been supposed to contain phlogiston. The probability therefore is, that the phlogiston 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 process with steam, but by means of some substance which has been supposed to contain phlogiston. Where then, is the certain proof that water is decomposed in this process?

Since, according to the antiphlogistic theory, water itself contains all the elements of both dephlogisticated and inflammable air, and wants only *calorique*, which they can give at pleasure, I see no reason why heat alone, without the aid of any metal, might not convert it into air. When the particles are so far separated as they are in a state of steam, I see no occasion for the superior

perior attraction of any other ſubſtance for either of them. In ſteam each of the elements is already in the form of air, and with its due proportion of calorique, and then why ſhould they not continue in that form, only mixed together, ready for exploſion?

It is ſaid that the oxygen imbibed by this iron, being expelled by heat in contact with inflammable air, unites with that air, and with it conſtitutes the water which is found after the proceſs. But for any thing that appears, this water may be that which the iron had imbibed, and which can only be expelled 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, tho' 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 admit of ſo much accuracy as the concluſion requires; and there is too much of correction, allowance, and computation in deducing the reſult.

A ſo, 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

air, or *azote*, in the dephlogisticated air, the experimenters 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) were 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 acknowledge that, except when the inflammable air was burned *in the slowest manner*, the water they produced had more or less of acidity.

The reason, no doubt, was that, whenever the flame they made use of was too strong, more of the dephlogisticated air in proportion to the inflammable was consumed than when the flame was weak; so that the results of their experiments exactly coincide with those of mine.

Citizens Berthollet and Fourcroy say, with Mr. Adet, that "the small quantity of acid which is commonly "found in this process comes from the *azote*, which is "mixed with the gas." (*Annales de Chymie*, vol. 26, p. 306.) But if this was the case, they could never get water free from acidity, because they can never wholly exclude azote. Besides, how can they think it so easy to procure nitrous acid from azote in this process, when

when Mr. Cavendiſh found it ſo difficult to procure a barely ſenſible quantity by numberleſs electric explo-ſions?

The experiments which I made on the decompoſition of theſe two kinds of air in *close vessels*, appear to me to be much leſs liable to exception, and the concluſion drawn from them is the reverſe of that of the French philoſophers.

When dephlogiſticated and inflammable air, in the proportion of a little more than one meaſure of the former or two of the latter (both ſo pure as to contain no ſenſible quantity of phlogiſticated air) are incloſed in a glaſs or copper veſſel, and decompoſed by taking an electric ſpark it it, a highly phlogiſticated nitrous acid is inſtantly produced; and the purer the airs are, the ſtronger is the acid found to be. If phlogiſticated air be purpoſely introduced into this mixture of dephlogiſticated and inflammable air, it is not affected by the proceſs, though, when there is a conſiderable deficiency of inflammable air, the dephlogiſticated air, for want of it, will unite with the phlogiſticated air, and, as in Mr. Cavendiſh's experiment, form the ſame acid. But ſince both the kinds of air, viz. the inflammable 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.

* I found, after many trials, that when an hundred ounce meaſures o

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 inflammadle 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 ſubſtances ſhould

inflammable air were fired together with fifty one measures of dephlogisticated air *acid* was formed, but that only *water* was produced when the same quantity of inflammable air was fired with forty seven measures of the same dephlogisticated air.

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 (meaning mine) 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 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, tho' if the air be purpoſely leſs pure, I never fail to find that impurity, viz. the phlogiſticated air, unaffected by the proceſs; ſo that it could not poſſibly have contributed to the production of the acid.

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

decompoſed, it would not have amounted to the hundreth part of the weight of the acid 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. Tho' I have not gallons of this liquor, I have ſome ounces, which no antiphlogiſtian would care to drink. Will Dr. Maclean ſay that my proceſs is leſs accurate than that of the French, becauſe it can be finiſhed in leſs than five minutes, and theirs requires the aſſiduous attendance of ſome days?

Uſing the ſame moſt ſimple apparatus, I can, by only varying the proportions of the two kinds of air, produce the reſult which the French chemiſts ſo much boaſt of. For I can 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 acknow-

ledge 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 chemiſts can make their experiments in a manner leſs operoſe and expenſive, requiring fewer precautions, and leſs of computation, I ſhall continue to think my reſults more to be depended upon than theirs.

Mr. Berthollet objects to my experiment, that the weight of the liquor which I produced from the decompoſition of the dephlogiſticated and inflammable air was never equal to the weight of the air. But this I account for by the eſcape of the highly phlogiſticated acid.

He alſo ſays that I took no account of the reſiduum of the air in the veſſel in which I made the exploſions. But I did not overlook this circumſtance; ſince I meaſured the capacity of the veſſel by the quantity of air that actually diſappeared, by having been completely decom-

decompoſed in the proceſs ; ſo that there was no occaſion whatever to take an account of the air that was not affected by it.

I ſhall conclude this ſection with obſerving that in order to complete their proof of the decompoſition of water, the antiphlogiſtians ſhould produce ſome ſubſtance which by uniting with hydrogen in water, ſhould let go the oxygen, in the form of dephlogiſticated air, or ſome acid ; and ſurely ſome ſuch ſubſtance may be found, if their theory be true. The component parts of water are not ſo intimately united, but that they may be ſeparated by means of affinities. Otherwiſe the hydrogen could not have been detached from it.

SECTION X.

An Argument against the Decomposition 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 they are separated by iron, or charcoal, in a red heat, uniting with one of them, and suffering the other to escape; and therefore if, in any case, a quantity of water be wholly expended in forming air, and only one of the kinds be found, it will be evident that this water does not consist of two elements. Now according to one of my experiments water would appear to consist of only one of the kinds of air, and according to another of the other.

I have shewn that by a slow supply of water in sending steam over red hot charcoal, the whole of the produce is inflammable air, without any mixture of fixed air, or the production of any thing, aerial, fluid, or solid, into which oxygen can be supposed to enter. From this experiment, therefore, conducted in this manner, it

it might be concluded that water conſiſts of hydrogen only, without any oxygen.

This obſervation of mine is confirmed by Mr. Watt, whoſe accuracy no perſon will call in queſtion. He ſays in his *Description of a pneumatical apparatus* ſubjoined to *Dr. Beddoes's Considerations on the medicinal use 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 much fixed air formed; but little or none when "the water is admitted ſo ſparingly that no ſteam "reaches the refrigeratory."

I once thought with the Antiphlogiſtians that fixed air was held in ſolution in this heavy inflammable air, becauſe when it is fired together with dephlogiſticated 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 evident, 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 according to my experiments with *terra ponderosa aerata*, it may be proved to conſiſt of oxygen only. For when ſteam is ent over this ſubſtance in a red heat nothing

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 twenty eight parts of carbone, and seventy two of oxygen.

These experiments favour my hypothesis that water 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 probably 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

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 XI.

Of the supposed Decomposition of Water in the Experiments of Van Troostwick and Deiman, in those 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 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.

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 of theſe airs being ſometimes ſpontaneous, without the electric ſpark being taken in them, ſhews that part at leaſt of the air produced is *phosphonic*; 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 poſſeſſed "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 com-"poſed of dephlogiſticated and inflammable air, or of

"any

"any other elements. But then the supposition should "not be admitted without proof; and if a former the- "ory will sufficiently account for all the *facts*, there is "no occasion to have recourse to a new one, attended "with no peculiar advantage."

"Also that phlogiston is an element in the composi- "tion of water is, as I have more than once observed, "not at all improbable, since water conducts electricity "like metals and charcoal, into which the same princi- "ple enters; and because, when fresh distilled, it at- "tracts dephlogisticated air from the atmosphere, which "is the property of other substances containing phlo- "giston. By this means water may, in fact, contain "both the principles of which, according to the new "theory, it wholly consists, and in what proportion it "contains them we cannot tell. For tho' heat may ex- "pel 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 disco- "ver. But this proves nothing against the doctrine of "phlogiston; since it only proves that this principle is "contained in water, more or less intimately combined, "as well as in many other substances."

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 fixed air) and 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 the 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 experi-
"ments the admirable proviſion there is in nature to
"prevent, or to leſſen, the fatal effects of putrefaction,
"eſpeci-

" especially in countries where the rays of the sun are
" most direct, and the heat the most intense. For where-
" as animal and vegetable substances, by simply putrefy-
" ing would necessarily taint great masses of air, and
" render it wholly unfit for respiration, the same sub-
" stances 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 sea-
" sons 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 dephlogisticat-
" ed 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 pro-
" per nutriment in them; and by converting a consider-
" able part of the phlogistic effluvium into their own
" nourishment, arrest it in its progress to corrupt the sur-
" rounding atmosphere. So wonderfully is every part
" of

" 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 is " neceſſarily ſubject. It is hardly poſſible for a perſon " of a ſpeculative turn not to perceive, and admire, this " moſt wonderful 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 compoſiti on 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 not have expected a *priori*; and when ſhe was ſo obliging as to

ſhew

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 metals 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

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 decompoſi-"tion 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 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 formed and riſe re-"generated like the phenix from her aſhes." But this appears to me to be as fanciful, and fabulous, as the ſtory of the phenix itſelf.

7. From the wonderful experiments that are claſſed under the head of *Galvanism*, it was at firſt univerſally inferred that water was decompoſed in ſome of the proceſſes, becauſe both dephlogiſticated and inflammable air were produced in the water that was employed. But it was ſoon obſerved that the dephlogiſticated air was no other than that which was drawn from the incumbent atmoſphere; for if the communication with it

was cut off, the procefs ftopped. Nor were the two kinds of air produced in the proportions that the new theory required in the compofition of it; the dephlogifticated air being generally too little for the purpofe.

From my own experiments on the fubject, publifhed in *Mr. Nicholson's Journal*, it is evident that the inflammable air muft come from the decompofition of the zinc, which is calcined in the procefs. For when the whole pile was covered with a receiver ftanding in water, about one fourth of the inclofed air difappeared, and the remainder was wholly phlogifticated, exactly as it would have been if a pafte made of iron filings and fulphur had been put in the place of the pile. Had any dephlogifticated air been produced, this could not have been the refult, for that would have kept the inclofed air in the fame ftate with that of the atmofphere, and the quantity would have been increafed, and not diminifhed. The dephlogifticated air that difappeared muft have gone into calx of the zinc; but certainly there had been no decompofition of the water.

Tho' the doctrine of the decompofition of water on the principle of the new theory is fallacious, my experiments make it exceedingly probable that it is refolvable into phlogifticated air, which I fuppofe to confift of the union of the two principles of phlogifton and dephlogifticated air. For whenever water is converted into vapour, either by means of heat, or in the vacuities of ice by freezing, a portion of this becomes permanent air, and this is always phlogifticated. For the proof of this I muft here content myfelf with referring

referring to the account of my experiments relating to the *conversion of water into air*, begun in England, and thoſe on *the freezing of water* made in this country.

SECTION XII.

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 cannot 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, and no compound. Neither of theſe ſuppoſitions appear to me to have been proved, and I think there is much poſitive evidence againſt them.

Tho' 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 char-"coal." 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 fix-

ed

ed air, and the reſiduum extinguiſhed a candle. Where could be the carbone in this caſe ?

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 or 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, forty ounce meaſures of air, one twelfth part of which was fixed air, and the reſt inflammable, burning with a blue flame. Then, ſending ſteam through the tube, I got two hundred and forty 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 two dwts and an half.

Another experiment on plumbago I ſhall juſt mention in this place. Melting one dwt. of it with a burn-

ing 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 he kinds of air contained in it, viz. fixed and inflammable, but the *slag* too, which remains 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 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 fix-

ed

ed 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 exiſts in the atmoſpheric, 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 prefently after it is made, little or no air can be expelled from it, as it may fome time afterwards. And I find that this fubftance alfo expofed to a portion of atmofpherical air makes it lefs pure than it was before, juft as in the cafe of quick lime.

As pyrophorus imbibes pure air when it is expofed to atmofpherical air, leaving nothing but phlogifticated air (in which it refembles a mixture of iron filings and fulphur, which alfo makes a pyrophorus) the fixed air expelled from it afterwards muft have been formed by the union of the dephlogifticated air imbibed by it, and the phlogifton contained in itfelf.

From a quantity of old and fpoiled pyrophorus I got one hundred and eighty ounce meafures of air, of the firft part of which one half was fixed air, and the reft phlogifticated. At the laft, tho' one half was fixed air, the reft was inflammable. In another experiment of this kind I found feven tenths of the air fixed, and the reft inflammable.

From 15 dwts. of fallen lime I got forty five ounce meafures of fixed air, and twenty five of inflammable from the gun barrel in which the experiment was made: Whether quicklime has been expofed to the atmofphere fo as to become what is called *fallen lime*, or has been faturated with water, they come in time to be of the fame weight, and to have the fame properties; the former continually gaining weight, and the latter lofing it. From 15 dwts of lime faturated with water and then expofed to the atmofphere, I got fifty five ounce meafures of fixed air.

I had

I had a reſult ſimilar to this when I expoſed ſome old *plaister of Paris* to heat in an earthen retort. In theſe circumſtances 3. 25 ounces yielded two hundred 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 decompoſed, and no difference of circumſtances is ſo much as attended to.

To theſe experiments relating to fixed air I ſhall add one that I made on the heating of the diamond in atmoſpherical air, much of the produce being fixed air.

That

That the diamond is a combuſtible ſubſtance has been long known, but not having heard of its being burned in atmoſpherical, or any kind of confined air, I long wiſhed to do it; and being ſupplied by a friend with two ſmall ones, I treated them in this manner, and found that near ninety parts in one hundred of the air in which they were burned was completely phlogiſticated, and the quantity not being ſenſibly changed, the remainder was fixed air; which is an effect ſimilar to that of heating charcoal of copper in air. The diamonds being very ſmall, and the quantity of air in which they were burned being very ſmall in proportion, I will not vouch for much exactneſs in the reſult, tho' the experiment was carefully made over mercury. Both the diamonds weighed only three tenths of a grain, and they loſt in the proceſs fifteen hundredths of a grain. The air in which they were burned was three fourths of an ounce meaſure.

With diamonds furniſhed me afterwards by Mr. Jeffries, a jeweller in London, I made the following experiments.

In 4. 5 ounce meaſures of dephlogiſticated air (of the ſtandard of 0. 6, with two equal quantities of nitrous air) I heated a diamond that weighed one ſeventh of a grain till it intirely vaniſhed, after which the air was reduced by means of lime water to an ounce meaſure and an half of the ſtandard of 1. 92.

The weight of the fixed air produced greatly exceeding that of the diamond, it is evident that the ſubſtance of it muſt have united with the dephlogiſticated air

air to form the fixed air. As the weight of this air was nearly three grains, and that of the diamond about a fourth part of it, the proportion of dephlogiſticated air in fixed air muſt be the ſame that I found it to be in my former experiments.

According to this experiment, the *diamond* and *charcoal of copper*, tho' ſo different in appearance (the one ſolid and beautifully tranſparent, and the other a black and friable ſubſtance) are both as nearly as poſſible pure phlogiſton. For tho' ſomething, no doubt, remains after their diſperſion, it is too ſmall to be diſcovered.

SECTION XIII.

Of Phlogisticated Air.

THE ſubject of phlogiſticated air is attended with conſiderable difficulty. I am able, however, to produce ſufficient evidence that it is not, as the antiphlogiſtians ſay, an elementary ſubſtance, but that it derives its origin from phlogiſton; ſince it may be generated

generated from inflammable air, ſometimes alone,* but in other caſes by means of dephlogiſticated air, whether any portion of this kind of air be united with it or not. Of this my readers will judge from the experiments that I ſhall lay before them.

1. One deciſive proof that phlogiſticated air may be formed, and ſeemingly by the union of dephlogiſticated air and phlogiſton, is the quantity of phlogiſticated air that remains after any exploſion of dephlogiſticated or common air with inflammable air, more than what remains after the mixture of nitrous air with either of them.

Having procured a quantity of dephlogiſticated air ſo pure that one meaſure of it mixed with two meaſures of nitrous air was reduced to 0. 04, I could not by any mixture of the pureſt inflammable air fired along with it reduce it to leſs than 0. 25.

2. The very different proportions in which atmoſpherical air is diminiſhed in different proceſſes, is a proof that in ſome of them there muſt be a generation of phlogiſticated air. When air is diminiſhed by iron filings and ſulphur moiſtened with water, the proportion of phlogiſticated air is that which Mr. Lavoiſier ſtates, viz. 73 parts in 100. But when I made the mixture without any water, I found that one hundred meaſures were in ſix days reduced only to ninety completely phlogiſticated, which is in the proportion of 81. 8, in 100. Again one hundred and forty ounce meaſures were

* I am now pretty well satisfied that when inflammable air becomes phlogisticated air, it gets dephlogisticated air through the water by which it is confined.

were by the ſame dry mixture reduced to one hundred and thirteen, which is in the proportion of 80. 6, in 100. But ſome water getting to the mixture the third time that it was uſed, one hundred and fifty five ounce meaſures of air were reduced by it to one hundred and ſixteen; which is in the proportion of 74 to 100.

By the ſlow burning of phoſphorus ſixty ounce meaſures of atmoſpherical air was reduced to forty eight; at another time to 48. 5; and fifty ounce meaſures were reduced to forty, which is in the proportion of 80 parts of phlogiſticated air in 100. But by repeatedly firing the phoſphorus with a burning lens, one hundred ounce meaſures were reduced to eighty nine completely phlogiſticated.

Meſſrs. Berthollet and Fourcroy however, ſay (*Annales de Chymie*, Vol. 26, p. 308) "We muſt abandon "the teſt of the purity of air by means of nitrous air; "and ſubſtitute that of phoſphorus, by means of which "we get uniform reſults. They are different with ni-"trous air, on account of the different proportions in "which this air combines with dephlogiſticated air to "form nitrous acid."

But how can theſe proportions vary when the circumſtances in which they are mixed are exactly the ſame? The nitrous air admitted in the ſame manner to any kind of air, containing in it a portion of dephlogiſticated air, muſt 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 reſult is, therefore, the ſame in all temperatures. On this account it is the moſt convenient, and perhaps

as

as accurate teſt as we can apply. It is only neceſſary that there be a ſufficient quantity of nitrous air to ſaturate all the dephlogiſticated air that 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 can judge the very ſame.*

8 Since

* 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 dimunition were very various; and he says that therefore from the still greater dimunition of this mixture which I have since observed, it cannot be concluded that atmospherical air contains more dephlogisticated 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 wholly absorbed in the same circumstances, and that some time before they disappear they all become phlogisticated air, inflammable air as well as the rest.

This is a fact of which I am not able to give any rational account, any more than of several others that have fallen under my observation. I have given a detailed account of the facts in an article I lately sent to the philosophical society at Philadelphia, together with another, on any two kinds of air, separated by an earthen vessel, or a bladder, changing

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 ſixty ounce meaſures of nitrous air, it was reduced to twenty four, all phogiſticated, 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 in proportion to its bulk, and generally conſiderably purer than that of the atmoſphere, the phlogiſticated air that is produced by heating ſteam in a copper veſſel muſt have been formed from phlogiſton in the copper and the air contained in the wate ; and whenever I have heated water in this manner (the upper part of a cloſed copper tube being kept in a red heat, while the lower and open part was immerſed in water) I have found a conſiderable quantity of air completely phlogiſticated, and the longer I kept it in this ſtate the more of this air I found. I had ſimilar reſults when I uſed a ſilver tube.

That this phlogiſticated air is not that which had paſſed from the centre of the fire thro' the metal tubes (tho' ſome of my late experiments prove that ſome metals

places, which I had observed before with respect to steam and air. This is a fact of great importance in the system of nature, especially with respect to respiration ; but of the cause of it I have not even a conjecture worth proposing. Both the above mentioned articles are published together with others, in the fifth volume of the Transactions.

tals are permeable to air in these circumstances) appears from the results of my experiments with glazed earthen tubes in the same circumstances. For the air that gets into the inside of these is often little worse than atmospherical air.

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

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

Whence, then, I aſk, can come this addition of phlogiſticated air, but from an union of phlogiſton emitted from the bones, with the dephlogiſticated air in the atmoſpherical air to which they are expoſed? Conſequently phlogiſticated air, or azote, is not a ſimple ſubſtance, as the antiphlogiſtians maintain, but a compound. Alſo whence can come the fixed air that is procured in the ſame proceſs, but from a different com-

bination of the ſame elements, and not as they ſay from *carbone*, which is a ſubſtance 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 dephlogiſtieated air that diſappears. But when the heat is applied with care, there is no loſs of weight in the bone; ſo that nothing is driven from it beſides the *phlogiston*, which appears to have no weight at all, or none that we can aſcertain.

That the thing which conſtitutes the blackneſs of the bones is the ſame with that which has always been called phlogiſton, is evident from its forming inflammable air if there be water to ſupply it with a baſis. For I find that if they be heated in phlogiſticated air, which cannot by parting with any thing contribute to this whiteneſs, they nevertheleſs become white; the air in which they are heated is increaſed in quantity, and this increaſe is inflammable air.

For theſe experiments I find *ivory black*, which is the coal of ivory uſed by painters, more convenient than common bones. To prepare this ſubſtance for the experiment, I fill an earthen tube with it, and cloſing it with clay, expoſe it for a conſiderable time, at leaſt a quarter of 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. 5 ounce meaſures of atmoſpherical air, there

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 proportion 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 air, 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, three and an half ounces expoſed to heat in an earthen tube gave out one hundred and twenty ounce meaſures of air, of which about one tenth was fixed air, and the reſt almoſt wholly phlogiſticated. Both theſe kinds of air, therefore, muſt conſiſt of dephlogiſticated air from

the atmoſphere, and ſomething contained in the iron, or the ſulphur, both of which are maintained to be ſimple ſubſtances. There remained a black powder, ſtrongly attracted by the magnet.

8. In general iron filings and ſulphur immerſed in mercury or water, or placed in a vacuum, yield inflammable air; but in ſome caſes (tho' I do not know the reaſon of the difference) this mixture has yielded phlogiſticated air.

Having placed a pot containing ſome of this mixture in a vacuum, I found after ſome days that it had yielded two and an half ounce meaſures of air; and examining it, I found it to be completely phlogiſticated. I then put the ſame mixture under water, and placing it near the fire, it gave an ounce meaſure more, all phlogiſticated.

At another time two ounce meaſures 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 phlogiſticated. It might have been originally inflammable air, and afterwards have changed to phlogiſticated.

9. Of the change of inflammable air into phlogiſticated air, ſeveral inſtances may be ſeen in the account of ſome of my early experiments; but I am not yet able to ſay on what this change depends. Suppoſing that it required the union of a portion of dephlogiſticated air, I expoſed to it pieces of iron, which being covered with ruſt, had attracted and contained that air; but the reſults were not uniform. I ſhall, therefore, content myſelf with relating what I obſerved, wiſhing that o-

the

ther perſons may diverſify the 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 antighlogiſtians 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 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 having 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 the 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 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 practiſed 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 endeavour-

ed 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 oxygenous principle. But neither do any of us pretend to have weighed *light*, or the element of *heat*, tho' few perſons doubt but that they are properly *ſubſtances*, capable by their addition, or abſtraction, of making great changes in the properties of bodies, and of being tranſmitted from one ſubſtance 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 conſtitution of natural ſubſtances; but we cannot adopt one, the principles of which we conceive not to be ſufficiently aſcertained. For other objections to this nomenclature, I refer to the preface of *Mr. Keir's* excel-

lent

lent *Dictionary of Chemistry.* However, whether we approve of this new language or not, it is now ſo generally adopted, that we are under a neceſſity of learning, tho' not of uſing, it.

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

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 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.

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 philoſophi-"cal 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

* 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 "absolutely deny all emission of air "from the surface of plants as well as from the skins 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. That they give *any air* in the dark, when they are in a healthy state, tho' maintaiued by Dr. Ingenhousz, I never fouud. 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.

"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- "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 altercation "on the ſubject. Indeed, there is nothing to contend "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 original
"obſervation that plants reſtore vitiated air, and the
"ſubſequent one, that *green vegetable matter* yields de-
"phlogiſticated air by means of light, were both acci-
"dental; 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
"no ſuſpicion. But the merit of all philoſophical
"diſcoveries is, in my opinion, greatly overated."

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

London, July 7, 1790.

DEAR SIR,

"RECEIVE my hearty thanks for the va-
"luable preſent of your philoſophical work in three vo-
"lumes."

"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 publiſhed
"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 publiſhed
"this

" this doctrine before me, I owe you the juſtice to ac-
" knowledge it publicly in the firſt volume of my books
" that will be publiſhed, or reprinted ; and I will cheer-
" fully retract, by acknowledging that in reading your
" philoſophical works I have through inadvertence, 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 perſua-
" ded 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 favour me ſoon with
" an anſwer, which will be very grateſully 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

him entitled in the bufinefs, I concluded with faying, that for his fake I wifhed it had been greater than it was.

It is obvious to remark that if the merit of this difcovery, be it more or lefs, depend, as he here acknowledges, on the fubftance which yields dephlogifticated or vital air by means of light being a *real plant*, all philofophers except Dr. Ingenhoufz himfelf, will, I doubt not, give it to me; becaufe the *green matter* which I had found to give this air by means of light, is acknowledged to be a vegetable.

Dr. Ingenhoufz, however, maintains a very fingular opinion on the fubject. For he fays p. 90, that "the "water itfelf, or fome fubftance in the water, is, I think, "changed into this vegitation." This is nothing lefs than reverting to the long exploded doctrine of *equivocal generation.* For if this vegetable could have its origen from mere water, or any thing in the water that was not a vegetable, or the feed of one, an oak might arife from water, or fomething in water that was not an acorn.

"This real transmutation," he however fays, "tho' "wonderful in the eye of a philofopher, is no more ex-"traordinary than the change of grafs and other vege-"tables into fat in the body of a graminivorous animal, "and the production of oil from the watery juice of an "olive." Other philofophers will, I doubt not, fee thefe cafes to be very different; and why Dr. Ingenhoufz alone fhould hefitate to call this *green vegetable matter* (as he himfelf terms it) to be a *proper plant*, with the power of propagating itfelf, and of producing dephlogif-

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 o this diſcovery.*

When I had made, and publiſhed, this diſcovery, which was before he had made any experiment 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. When I, according to my known cuſtom, was writing to acquaint my friends with what I had done, Mr. Magellan, 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

* 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

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

Being

Being in London the winter following, I ſhewed this experiment to all my friends, and among the reſt to Dr. Ingenhouſz, who was particularly ſtruck with it. The queſtion amongſt us then was what this *green matter* could be; and it being generally thought to be 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 time 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 inveſtigation in which he found me.

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

Dr. Ingenhouſz in his *Essay on the food of plants*, p. 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 from *red lead*. Speaking French very imperfectly, and being little acquainted 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

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 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* be-

ing

ing 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*.

BY THE AUTHOR OF THIS TRACT.

1. THE Hiſtory and preſent ſtate of *Electricity*, with original Experiments, illuſtrated with Copper Plates, 5th Edition, corrected, 1l. is. in boards.

2. A Familiar *Introduction* to the *Study of Electricity*, 5th Edition, 8vo. 2s. 6d. ſewed.

3. The Hiſtory and preſent State of Diſcoveries relating to *Vision*, *Light*, and *Colours*, 2 vols. 4to, illuſtrated with a great number of Copper Plates, 1l. 11s. 6d. in bds. 1l. 18s. bd.

4. *Experiments* and *Observations* on different Kinds of Air, and other Branches of *Natural Philosophy* connected with the Subject, 3 vols. in boards, being the former 6 vols. abridged and methodiſed, with many Additions, 1l. 1s.

5. *Experiments* relating to the Decompoſition of Dephlogiſticated and Inflammable Air; and on the Generation of Air from Water, 1s.

6 Heads

6. Heads of a Course of Lectures on Experimental Philosophy, including Chemistry, 3s. 6d. in boards.

7. A Familiar Introduction to the Theory and Practice of *Perspective*, with Copper Plates, second Edition, 5s. boards, 6s. bound.

8. A *Chart* of *Biography*, with a Book containing an Explanation of it, and a Catalogue of all the names inserted in it, 6th Edition, very much improved, 10s. 6d.

9. A New *Chart* of *History*, containing a View of the principal Revolutions of Empire that have taken place in the World; with a Book describing it, containing an Epitome of universal History, 4th Edition, 10s. 6d.

Published in America.

10. A Comparison of the Institutions of Moses with those of the Hindoos and other Antient nations; with Remarks on Mr. Dupuis's Origin of all Religions, the Laws and Institutions of Moses methodised, and an Address to the Jews on the present state of the World and the Prophecies relating to it.

11. A general History of the Christian Church from the Fall of the Western empire to the present time, 4 vols. 8vo. dedicated to Thomas Jefferson President of the United States.

12. Socrates and Jesus Compared, 50 Cents.

13. A Defence of the same in answer to Dr. Linn, 36 Cents.

14. Letters to an Antipœdobaptist, 36 Cents.

15. Two volumes of Discourses relating to the Evidences of Christianity, in Addition to the one published in England. Each 1 dollar and 50 Cents.

Med. Hist.
WZ
270
P949d
1803
c.1