TWO LECTURES

ON

COMBUSTION:

SUPPLEMENTARY TO A

COURSE OF LECTURES

ON CHEMISTRY.

READ AT NASSAU-HALL.

CONTAINING

AN EXAMINATION

OF

DR. PRIESTLEY'S CONSIDERATIONS ON THE DOCTRINE OF PHLOGISTON,

AND

THE DECOMPOSITION OF WATER.

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ADVERTISEMENT.

OWING to other engagements, a part only of the firſt of theſe lectures was read to the ſtudents—They are now printed to ſave the young gentlemen the trouble of tranſcribing them.

J. M.

P. S. *It was not till after they were ſent to the preſs, that I was informed Mr. Adet had publiſhed an anſwer to Dr. Prieſtley's pamphlet.*

LECTURES

ON

COMBUSTION.

GENTLEMEN,

ALTHOUGH the conſequences of the combination of different ſubſtances have been explained in the lectures which I have already had the honour of reading to you; yet, as the appearances attending the expoſure of inflammable bodies and metals to the air at high temperatures, are peculiarly ſtriking, and have occaſioned much diſputation among philoſophers, it will be proper to conſider and compare the different opinions which have been held reſpecting them.

Becher ſuppoſed that inflammable bodies contained a ſubſtance, which he called inflammable earth.

Stahl thought they included a peculiar principle; to this he gave the name of Phlogiſton. He ſuppoſed the phenomena of combuſtion were owing to

its eſcape, and the aſhes or reſidue of a burnt body was matter, by which the phlogiſton had been confined. Thus, he thought, that in the combuſtion of ſulphur, the phlogiſton was merely ſeparated from the ſulphuric acid.

The difference among inflammable bodies, he attributed to the matter containing the phlogiſton; and this opinion he conſidered as confirmed, by the compound of potaſh and ſulphuric acid being readily converted into one of potaſh and ſulphur, by being mixed with powered charcoal, and raiſed to a high temperature: for he ſuppoſed the charcoal communicated to the ſulphuric acid the phlogiſton which it had loſt.

As all metallic bodies became converted into earthy-like ſubſtances when placed in certain circumſtances, and as their original properties were reſtored on their being mixed with inflammable bodies, and raiſed to a high temperature, Stahl conceived that each metal conſiſted of phlogiſton and a peculiar earth or calx.

It is eaſy to perceive that Stahl did not attend to the influence the air of the atmoſphere has on every ordinary proceſs of combuſtion. He was ignorant of the compoſition of the atmoſphere: he did not know that one of its parts combines with every inflammable body and metal which is burnt in it; that the new compound is in many caſes aërial, or gaſeous, and in all exceeds in weight the combuſ-

tible

tible ſubſtance; that this exceſs of weight correſponds exactly to a like loſs ſuſtained by the atmoſphere; that in ſome caſes the whole, and in others part, of the ſubſtance furniſhed by the atmoſphere, may be ſeparated with all its diſtinguiſhing properties; that in every inſtance the whole of it may be detached by the aſſiſtance of other ſubſtances, which have a diſpoſition to combine with it; and that in proportion as the ſeparation takes place, the inflammable body, or metal, is recovered. In fine, it may be perceived, that Stahl was unacquainted with many of the phenomena, and formed a conjecture to account for what he had obſerved.

By the diſcovery of ſeveral of the circumſtances juſt mentioned, Mr. Lavoiſier, a celebrated French chemiſt, was induced to aſcribe the change in the properties of inflammable bodies, and metals, by combuſtion, to their union with the oxygen of the atmoſphere, and the great increaſe of temperature which attends ſome of theſe combinations, to the ſame cauſe that occaſions a like phenomenon, during the combination of other ſubſtances.

Mr. Lavoiſier was ſoon ſupported by ſeveral of his countrymen, who were ſatisfied there was no occaſion for ſuppoſing the exiſtence of ſuch a principle as phlogiſton, to account for the phenomena. Their doctrine has hence been called antiphlogiſtic.

From its having ariſen in conſequence of the diſcovery of ſeveral aerial or gaſeous ſubſtances, it is

alſo

alſo named the Pneumatic Doctrine——It is the ſyſtem in which you have been inſtructed.

At firſt it was ſtrenuouſly oppoſed by many philoſophers, who, although they admitted Stahl's theory was inſufficient, yet thought the exiſtence of phlogiſton was not incompatible with the diſcoveries which had given riſe to the antiphlogiſtic doctrine, and was perhaps even neceſſary for the explanation of ſome, if not of all, of the phenomena.

In conſequence, many attempts were made to reconcile Stahl's notion with theſe diſcoveries; but few phlogiſtians agreed about the nature of phlogiſton. Some of them conſidered it as an immaterial principle; others thought it was light; others hydrogen gas, &c.; nay, it was not uncommon for the ſame perſon to waver between theſe different opinions.

The event of the conteſt has been highly honourable to the French chemiſts. All philoſophers now living, except Doctor Prieſtley, have acceded to their doctrine.

The Doctor has often animadverted on their experiments and concluſions, and being perſuaded, that what he has obſerved has not been properly attended to, or well underſtood, he has lately publiſhed a ſmall pamphlet, entitled, Conſiderations on the Doctrine of Phlogiſton, and the Decompoſition of Water. In it he has collected every thing he conſiders as material, whether as objections to the

pneumatic

pneumatic doctrine, or as arguments in favour of that to which he is attached. An examination of these, while it will unfold to you his modification of Stahl's theory, may serve to put you on your guard against falling into even that temporary delusion, which an erroneous opinion is so apt to produce, when supported by a celebrated name.

The work consists of a dedication, an introduction, and three sections: but as the discussion of the subject is confined to the sections, it will be necessary only to consider these.

In the first, he treats of the constitution of metals; in the second, of the decomposition of water; and in the third, of such objections to the pneumatic doctrine as could not conveniently be introduced into either of the other two.

Although this arrangement is not very well suited to our purpose, yet from an anxiety to avoid misrepresentation, I shall retain it; and for the same reason, instead of giving an abridged view of the Doctor's objections and arguments, I shall read his own words; but as he has not adopted the nomenclature in which you have been instructed, I shall mention the names that correspond with those which he employs.

In the section on the constitution of metals, after giving a very brief account of the principles of Stahl and Lavoisier, he says, "As a proof that metals are simple substances, and that they become

"calces

" calces (oxyds) merely by imbibing air, they al-
" lege the caſe of mercury, which becomes the
" calx called precipitate per ſe (red oxyd of mer-
" cury) by expoſure to the atmoſphere in a certain
" degree of heat, and which becomes running mer-
" cury again by expoſure to a greater degree of
" heat. They therefore think it impoſſible not to
" conclude, that in all other caſes of calcination,
" as well as this, the only difference between the
" calx (oxyd) and the metal, is that the latter has
" parted with the air which it had imbibed."

This is certainly a very inaccurate ſtatement. The antiphlogiſtians conſider metals as ſimple, becauſe they have not diſcovered them to be compound; and they believe the ſubſtances, which the Doctor calls calces, are compounds of metals and one part of the air, which they call oxygen, becauſe they cannot be formed without the preſence of oxygen; they exceed in weight the metal, and this exceſs, correſponds to an equal loſs of oxygen; and the oxygen may be recovered entirely, in the ſtate of gas, from the red oxyd of mercury, by ſimply raiſing its temperature; in part from the red oxyd of lead, and black oxyd of manganeſe by treating them in the ſame way; and may be wholly ſeparated from every metallic oxyd, by mixing them with inflammable ſubſtances, and raiſing the temperature of the mixture.

The formation and decompoſition of the red oxyd of mercury is aſcribed to the oxygen and mercury attracting each other, with different forces, at different temperatures; the partial decompoſition of the oxyds of lead and manganeſe is conſidered as being owing to theſe metals retaining different proportions of oxygen with unequal force; and the effect of the inflammable ſubſtances is attributed to their having a ſtronger diſpoſition, than metals have, to combine with oxygen: All which explanations are the more probable, from their correſponding with the laws of chemical combination, diſcovered by obſerving the action of different bodies on each other.

After giving the forementioned account of the foundation of the antiphlogiſtic opinion, the Doctor attempts to ſhew its inſufficiency. "But this is "the caſe of only this particular calx (oxyd) of "this metal, and there is another calx of the ſame "metal, viz. that which remains after expoſing "turbith mineral to a red heat, which cannot be "completely revived by any degree of heat, but "may be revived in inflammable air (hydrogen "gas), which it imbibes, or when mixed with "charcoal, iron-filings, or other ſubſtances ſup-"poſed to contain phlogiſton. And if this calx of "mercury, or (ſuppoſing it to contain ſome acid "of vitriol,) [ſulphuric acid,] this ſalt neceſſarily "requires ſome addition to conſtitute it a metal,

" all mercury muſt contain the ſame. For though " with the ſame external appearance, the ſame " metal may contain different proportions of any " particular principle, as phlogiſton, they muſt be " denominated different ſubſtances, if ſome ſpeci- " mens contain this element and others be wholly " deſtitute of it. All, therefore, that can be in- " ferred from the experiment with the precipitate " per ſe (red oxyd of mercury) is, that in this par- " ticular caſe, the mercury in becoming that calx " imbibed air, without parting with any, or very " little of its phlogiſton; and if we judge by the " air expelled from the calces of metals and other " circumſtances, there are few, if any of them, but " contain more or leſs of phlogiſton."

From the following paſſages in Fourcroy's Elements of Chemiſtry &c. it appears the reaſoning in the paragraph juſt read is founded on a miſtake. Turbith mineral " when urged with a fire in a re- " tort, at firſt becomes of a deeper colour, and is " afterwards reduced to running mercury, giving " out at the ſame time a conſiderable quantity of " vital air (oxygen gas). Kunckel mentions this " reduction. It ſucceeded with Meſſrs Monnet, " Bucquet, and Lavoiſier, who traced it through " all its circumſtances. I have repeated it ſeveral " times with ſucceſs.

" Perhaps the reaſon why Mr. Baumé did not " obtain running mercury, which has induced him

" to

" to assert that this yellow oxide does not resume " a metallic form unless some combustible substance " be added, was his not having applied to it a suf- " ficient heat."*

These are confirmed by later observations,† and they shew, the mercury in turbith mineral, or any substance into which it may be converted by a red heat, does not require any addition to constitute it a metal.

It is true, the presence of hydrogen gas, and other inflammable substances, renders the reduction easier; but this it is contended, only proves, that the substance is decomposed more readily, when there is a body present, which has a disposition to combine with one of its constituent parts, viz. the oxygen, a circumstance analogous to many other chemical decompositions.

The antiphlogistians are induced to believe the hydrogen gas is not imbibed by the mercury, but combines with the oxygen, because, 1*st*. Oxygen gas may be collected, when an oxyd of mercury is reduced by simply raising its temperature: 2*d*. When the reduction is performed in hydrogen gas this disappears, no oxygen gas is obtained, but a quantity of water may be collected: And, 3*d*. From various experiments it seems, that water is a compound of hydrogen and oxygen.

* Fourcroy, Vol. II. p. 318. London Edition, 1790.

† Annales de Chimie, Tome 10me. p. 305.

 That

That a higher temperature is neceſſary for the decompoſition of the ſubſtance from turbith mineral, than for that of the red oxyd of mercury, is not at all ſurpriſing; they are different bodies, and muſt therefore be acted on by other agents, with different forces.

The inference, which the Doctor draws from the experiments with the red oxyd of mercury, and from the gas obtained from the oxyds of other metals, cannot be admitted, until he proves the mercury and gas actually contain phlogiſton.

The Doctor proceeds—" I would obſerve in this " place, that it is aſſerted by ſome very able che- " miſts, that if the precipitate per ſe (red oxyd of " mercury) be made with proper attention, it will " be revived without yielding any air. This is alſo " the caſe with minium (red oxyd of lead) when " freſh made. But this is owing, I doubt not, to " their wanting *water*, which I deem to be eſſential " to the conſtitution of every kind of air (gas); ſo " that they both contain the element of dephlogiſ- " ticated air (oxygen gas), though for want of " water, it is not able to aſſume that form."

I confeſs, I am unacquainted with the experiments, which have led to the aſſertion alluded to in this paragraph.—But, if water be eſſential to the conſtitution of every kind of air, and if mercury and lead are converted into their red oxyds, or calces, by uniting with the element of oxygen gas

or

or dephlogiſticated air, what precaution can prevent the compounds from imbibing the water, if they have a diſpoſition to do ſo? The Doctor believes they have ſuch a diſpoſition; for except to theſe able chemiſts, they have always afforded oxygen gas or dephlogiſticated air, and to do this they muſt, in his opinion, contain water.

The next paragraph is as follows. "That mercury may have the ſame external appearance and "all its eſſential properties, and yet contain different proportions of ſomething that enters into it, "is evident from the phenomena of its ſolution in "the nitrous acid, and the revival of its calx in inflammable air (hydrogen gas). According to the "old theory, there is a loſs of ſome part of its phlogiſton in the ſolution of mercury in the nitrous "acid, ſince nitrous air (gas) is procured in the "proceſs: And though it may be revived from its "precipitates (oxyds) by mere heat, yet if it be "revived in a veſſel of inflammable air (hydrogen "gas), it will imbibe it in great quantities. Mercury revived in theſe circumſtances muſt contain "more phlogiſton than that which is revived from "the ſame calx by mere heat. But though mercury revived by mere heat after a ſolution in "nitrous acid muſt have a deficiency of phlogiſton, "and when it is revived from precipitate per ſe "(red oxyd of mercury) in inflammable air (hydrogen gas) muſt contain a redundancy of the

"ſame

" ſame principle, yet there will hardly be a doubt " but that, in all chemical proceſſes, it would ex- " hibit the ſame phenomena."

If two portions of matter exhibit the ſame phenomena in all chemical operations, they ought moſt certainly to be conſidered as the ſame ſubſtance; and as mercury revived from its oxyds by a mere increaſe of temperature, poſſeſſes all the properties of that revived by the aſſiſtance of hydrogen gas, &c. it cannot be in any reſpect deficient. It is in vain to ſay, that it muſt be ſo, becauſe " according " to the old theory, there is a loſs of ſome part of " its phlogiſton in the ſolution of mercury in the " nitrous acid," and if it be revived in hydrogen gas " it will imbibe it in great quantities;" it is firſt incumbent to prove, that the old theory, as he calls it, is right, and that the gas which diſappears, is actually abſorbed by the mercury: but theſe are juſt the ſubjects of diſpute.

Having ſaid ſo much on mercury he obſerves. " In all other caſes of the calcination (oxydation) " of metals in air, which I have called the phlogiſti- " cation of the air, it is not only evident that they " gain ſomething which adds to their weight, but " that they likewiſe part with ſomething. The " moſt ſimple of theſe proceſſes is the expoſing iron " to the heat of a burning lens in confined air, in " conſequence of which the air is diminiſhed and " the iron becomes a calx (oxyd). But that there

" is

" is ſomething emitted from the iron in this proceſs " is evident from the ſtrong ſmell which ariſes from " it. If the proceſs be continued, inflammable air " [hydrogen gas] will be produced, if there be " any moiſture at hand to form the baſis of it. From " this it is at leaſt probable, that, as the proceſs " went on in an uniform manner, the ſame ſub- " ſtance, viz. the baſis of inflammable air [hydrogen " gas], was continually iſſuing from it; and this is " the ſubſtance, or principle, to which we give the " name of phlogiſton."

" That the effect of this proceſs is not, as the " antiphlogiſtians aſſert, the mere ſeparation of the " dephlogiſticated from the phlogiſticated air [the " oxygen from the azotic gas] in that of the atmoſ- " phere, I have proved in a courſe of experiments, " in which I have ſhewn that a conſiderable part " of the phlogiſticated air [azotic gas] that is found " after this proceſs, is formed in the courſe of it, " by the union of the phlogiſton from the iron " with the dephlogiſticated air [oxygen gas]. And " if the calcination of the iron in this proceſs be " always attended with the loſs of ſome conſti- " tuent part of it, the ſame is, no doubt, the caſe " with all other calcinations of the ſame metal, and " alſo thoſe of all other metals. And further, if " the metals be compound ſubſtances, containing " phlogiſton united to ſome baſe, the ſame is the " caſe with *ſulphur* and *phoſphorus*, becauſe they

" become

" become acids when they are uſed in the ſame " proceſs."

I do not know, that a ſmell ariſes from pure iron when heated in air: but moſt certainly there are other metals, from which no ſmell ariſes, when placed in the ſame circumſtances; and the Doctor has ſaid, if the calcination of iron be attended with the loſs of ſome conſtituent part of it, meaning phlogiſton, the ſame is no doubt the caſe with thoſe of all other metals; and therefore even although iron ſhould afford a ſmell, it is no proof of the eſcape of phlogiſton.

That a quantity of hydrogen gas is obtained when the iron or air is moiſt is moſt certain: but this, it is inſiſted, is owing to the iron uniting with the oxygen of the water, while the hydrogen aſſumes the form of gas or air.

I have not been able to find any courſe of experiments, which *proves*, that azotic gas is formed by expoſing iron in confined air to the rays of the ſun concentrated by a burning lens: but Dr. Prieſtley informs us, that on examining the reſiduum of ſome " pretty pure dephlogiſticated air [oxygen gas]" in which iron had been fired with aſſiſtance of a lens, " it did not appear that any phlogiſticated " air [azotic gas] had been produced in the pro- " ceſs,"* and the ſame thing has been obſerved by

* Experiments and Obſervations on Air, Vol. III. p. 481.

Lavoiſier,* conſequently azotic gas cannot be formed by any thing from the iron uniting with oxygen, and the argument for the exiſtence of phlogiſton drawn from its ſuppoſed formation is invalid.

From the effects of air at high temperatures, the Doctor paſſes to thoſe of acids or metals. " Accor- " ding to the antiphlogiſtic theory, the inflamma- " ble air [hydrogen gas] that is produced in the " ſolution of metals in any acid, comes wholly from " the water combined with it; and not at all from " the metal diſſolved. But the advocates for this " theory do not ſeem to have attended to one ne- " ceſſary conſequence of this ſuppoſition. Accor- " ding to their own principles, water conſiſts of " eighty ſeven parts of oxygen, to only thirteen of " hydrogen in every hundred, which is nearly ſeven " times as much of the former as of the latter. " Conſequently ſince nothing but hydrogen eſcapes " in the proceſs, there muſt remain, from this de- " compoſition of the water, ſeven times as much " oxygen in the ſolution. But both Mr. Lavoiſier " and Mr. de la Place ſay [Examination of Mr. " Kirwan's Treatiſe, p. 197, 198,] what I doubt

* Il eſt de même extrêmement difficile d'obtenir du gaz oxigine parfaitement pur, il contient preſque toujours une petite portion de gaz azote, mais elle ne trouble en rien le réſultat de l'experience, & elle ſe retrouve à la fin en même quantitè qu'au commencement. Annales de Chimie, Tome I. p. 26.

" not is ſtrictly true, that after the proceſs the acid " will ſaturate exactly the ſame quantity (they do " not ſay more) of alkali, that it would have done " before; whereas, with the addition of ſo much " oxygen, it ought to ſaturate conſiderably more. " If the oxygen from the decompoſition of the wa- " ter do not join that in the acid, what becomes " of it?"

The anſwer to this queſtion is very eaſy—Far from ſuppoſing the oxygen from the water joins that in the acid, the antiphlogiſtians believe it unites with the metal, to enable this to combine with the acid. They are of this belief, becauſe the metal is precipitated in a ſtate of oxyd when an alkali is added to the ſolution; while the acid requires the ſame quantity of alkali to ſaturate it, and forms the ſame ſubſtances that it would have done before its action on the metal.

Perhaps it was from being aware of this anſwer that the Doctor has ſaid, in the next paragraph, which concludes the ſection, " If this caſe be ana- " logous to that of the ſuppoſed decompoſition of " water by hot iron, the oxygen ought to be lodg- " ed in the iron, and compoſe *finery cinder* (black " oxyd of iron). But this ſubſtance is not ſoluble " in vitriolic (ſulphuric) acid, if that be employed " in the experiment; and when it is diſſolved in the " marine (muriatic) acid, it does not dephlogiſticate " (oxygenate) it, as minium (red oxyd of lead)

" and

" and other ſubſtances containing oxygen, &c.
" It is evident, therefore, that there is no addition
" of oxygen in this proceſs, conſequently no de-
" compoſition of water in the caſe, and that the in-
" flammable air (hydrogen gas) muſt come from
" the decompoſition of the iron."

It is rather ſurpriſing the Doctor ſhould aſſert, that finery cinder (black oxyd of iron) is not ſoluble in ſulphuric acid, when, in page 505 of the third volume of his own experiments and obſervations, it is ſaid, that of ſixty grains of finery cinder put into vitriolic (ſulphuric) acid,* fifteen grains remained undiſſolved. Beſides, there is the moſt ſatisfactory evidence that iron, after its ſolution in the ſulphuric acid, is in a ſtate like that of the black oxyd or finery cinder. This ſubſtance diſſolves in the acid without effervéſcence, † and if the precipitate which the ſolution of iron in the ſulphuric acid affords, on the addition of an alkali, be collected as ſoon as it falls, and dried in cloſe veſſels, it will be the black oxyd. ‡ There is no effervéſcence during the ſolution, becauſe there is no hydrogen gas evolved; and this, the antiphlogiſtians believe, is owing to the iron being previouſly combined with enough of oxygen to ſuperſede the neceſſity of de-

* The quantity of acid is not ſpecified.

† Lavoiſier's Elements of Chemiſtry, p. 88. Edinb. Edition, 1790.

‡ Fourcroy, Vol. II. p. 425.

 compoſing

compoſing the water. The precipitate muſt be dried immediately, and in cloſe veſſels, becauſe, from its minute diviſion, it is very liable to be oxydated to a greater degree.

The other cirrumſtance which he has adduced as a proof, that the black oxyd does not contain oxygen, is not more convincing. It certainly does not follow, becauſe muriatic acid can ſeparate a certain portion of oxygen from lead, when this is combined with a great quantity of that ſubſtance, that it ſhould likewiſe ſeparate oxygen from iron, when this is united to a comparatively ſmall quantity.

From this firſt ſection of the Doctor's work it ſeems, his objections to the opinion, that metals are ſimple, and become converted into earthy-like bodies by an union with oxygen, are, 1*ſt*. The ſubſtance which remains after expoſing turbith mineral to a red heat, cannot be made to yield its mercury, unleſs it be mixed with bodies ſuppoſed to contain phlogiſton. 2*d*. When iron is fired in air it emits a ſmell, and if there be moiſture at hand, inflammable air (hydrogen gas) is produced. 3*d*. Phlogiſticated air (azotic gas) is formed during the combuſtion of iron. 4*th*. If oxygen was ſeparated from hydrogen during the ſolution of iron in acids, theſe ſhould acquire a proportional addition of ſtrength. And, 5*th*. Iron diſſolved in the ſulphuric acid is not in the ſtate of the black oxyd.

It alſo appears that the Doctor is of opinion,

1. That

1. That metals contain phlogiſton.

2. That, in the formation of precipitate per ſe, (red oxyd of mercury) air is abſorbed, and little or no phlogiſton emitted.

3. That mercury revived from its calces (oxyds) by mere heat contains leſs phlogiſton than when revived with the aſſiſtance of inflammable air (hydrogen gas.)

4. That the ſmell which ariſes from heated iron, is owing to the phlogiſton which is eſcaping, and that the inflammable air (hydrogen gas) which is produced when moiſture is at hand, is formed by the union of the phlogiſton with the water.

5. That phlogiſticated air (azotic gas) is formed by the union of phlogiſton with dephlogiſticated air (oxygen gas.)

6. That the inflammable air (hydrogen gas) emitted during the ſolution of iron in acids, is owing to the decompoſition of the iron. And,

7. That *finery cinder* (black oxyd of iron) does not contain oxygen.

But from the facts mentioned in the review of the ſection it follows, 1. The ſubſtance into which turbith mineral may be converted by a red heat, affords its mercury by a ſimple increaſe of temperature.

2. Mercury, revived from its oxyds by a ſimple increaſe of temperature, does not differ from that revived in hydrogen gas.

3. If

3. If a ſmell ariſes from heated iron, it is no proof of the emiſſion of phlogiſton.

4. Azotic gas cannot be formed by the union of oxygen with any thing emitted from heated iron.

And, 5. Iron, when diſſolved in ſulphuric acid, is reduced to a ſtate like that of the black oxyd.

And further, if it can be proved that water is a compound of hydrogen and oxygen, and ſuſceptible of decompoſition, as is inſiſted by the antiphlogiſtians, it muſt alſo follow, that hydrogen gas, and other inflammable ſubſtances, aſſiſt the reduction of metallic oxyds, by combining with their oxygen. And that the hydrogen gas obtained by expoſing heated iron to moiſture, or by diſſolving iron in diluted ſulphuric acid, proceeds from the decompoſition of the water, and not from that of the iron. —But the proofs of theſe will be exhibited in the examination of the ſecond ſection.

The ſecond ſection of Dr. Prieſtley's work is the moſt important. He begins it with obſerving, "The antiphlogiſtic theory has received its greateſt "ſupport from the ſuppoſed diſcovery, that water "is reſolvable into two principles; one that of oxy-"gen, 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 in-"flammable air."

The

The term hydrogen is derived from ὑδωρ, aqua, and γεινομαι, gignor, and was designed to express the principle engendering water. It has been criticised by some, who maintain it signifies, engendered by water, and this is the sense in which Dr. Priestley seems inclined to understand it. But, as has been well observed by Mr. Lavoisier, it may be used in either of these acceptations; for where water is decomposed hydrogen is produced, and when hydrogen is combined with oxygen water is produced.

After giving an extract from a joint work by several of the antiphlogistians, in which they declare their firm belief in the formation, the decomposition, and recomposition of water, the Doctor says,

" Notwithstanding the confidence thus strongly " expressed by these able and experienced chemists, " I must take the liberty to say, that the experi- " ments 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 ob- " served.

" Their proof that water is decomposed, and re- " solved into two kinds of air, is, that when steam " is made to pass over red-hot iron inflammable air " is produced, and the iron acquires an addition of " weight, becoming what is called finery cinder; " but what they call oxide of iron, supposing that " there is lodged in it the oxygen which was one " of the constituent parts of the water expended in " the

" the proceſs, while the other part, or the hydro-
" gen, with the addition of heat, aſſumed the form
" of inflammable air."

It ought to have been ſtated, that in this experiment there is a loſs of water exactly equal to the joint weight of the addition made to the iron, and of the hydrogen gas obtained.

The antiphlogiſtians ſuppoſe the addition made to the iron to be oxygen, becauſe the compound reſembles, in every reſpect, as the Doctor himſelf allows, that ſubſtance which is formed by burning iron in oxygen gas, or in atmoſpherical air; and this they conſider as an oxyd, becauſe, *1ſt.* While it is forming the oxygen gas diſappears, and its weight is exactly equal to that of the iron and the oxygen conſumed: *——And, *2d.* When iron-filings are mixed with red oxyd of mercury, and the mixture made nearly red-hot, the iron is converted into the ſame black ſubſtance as in the laſt experiment, while the oxyd of mercury is reduced, and the weight acquired by the iron correſponds to the exceſs of that of the red oxyd above the mercury. †

* Quand on a donné à cette expérience toute l'attention qu'elle mérite, l'air ſe trouve diminué d'une quantité en poids exactement égale à celle dont le fer eſt augmenté. Annales de Chimie, Tome I. p. 24.

† J'ai mêlé enſemble (ſays Mr. Lavoiſier), 450 grains d'oxide rouge de mercure, par le feu, bien pur, & 100 grains de limaille d'un fer tres doux, et qui n'etoit nullement atta-

The hydrogen gas is ſuppoſed to come from the water, and not from the iron, becauſe it is not obtained when the black oxyd is formed, without the aſſiſtance of water; and, as will be ſhewn, water may be formed by uniting oxygen with hydrogen.

The Doctor obſerves, on the experiment with iron and water, "But in order to prove that this "addition of weight to the iron is really oxygen, "they ought to be able to exhibit it in the form of "dephlogiſticated air (oxygen gas) or ſome other "ſubſtance into which oxygen is allowed to enter, "and this they have not done. Iron that has really "imbibed air, or the common *ruſt of iron*, has a "very different appearance from this finery cinder "(black oxyd of iron) being red, and not black; "and when treated in ſimilar proceſſes, exhibits "very different reſults. Mr. Fourcroy ſays (Exa- "mination of Kirwan, p. 251.) that this finery cin- "der is 'iron partially oxygenated.' But if that

qué de rouille. J'ai introduit ce mêlange dans une petite cornue, & J'ai fait chauffer juſqu'au moment ſeulment où les vaiſſeaux ont commencé obſcurément á rougir. Il ne s'eſt dégagé aucun gaz pendant cette operation, ſi ce n'eſt une tres-médiocre quantité d'air fixe ou acide carbonique aériforme; elle n'excedoit pas deux ou trois pouces cubiques. Il a paſſé dans la diſtillation 415 grains de mercure coulant; ayant enſuite caſsè la cornue, J'ai trouvé la limaille de fer dans l'état d'un fer brûlé, elle etoit friable, elle ſe reduiſoit aiſément en poudre, elle etoit dans l'etat d'un veritable *ethiops* (black oxyd) & peſoit 132 grains. An. de Chimie, Tome I. p. 28.

" were the caſe, it would go on to attract more ox-
" ygen, and in time become a proper ruſt of iron,
" completely oxygenated. But this is ſo far from
" being the caſe, that finery cinder never will ac-
" quire ruſt ; which ſhews, that the iron in this ſtate
" is ſaturated with ſome very different principle,
" which even excludes that which would have con-
" verted it into ruſt."

The evidence which has been given, ſeems to me to be ſufficient to prove the addition made to the iron muſt be oxygen ; but more will be given in the courſe of theſe lectures.

Without doubt common ruſt of iron is very different from the black oxyd : but the Doctor is certainly miſtaken in ſuppoſing this cannot acquire ruſt ; Mr. Fourcroy ſays, it ruſts ſooner than common iron, and every apothecary knows it does ſo. Beſides, we learn from the experiments of Meſſrs. Joſſe and Fourcroy, that if ruſt be made red hot in a retort, a quantity of carbonic acid is diſengaged from it, and the iron remains in the ſtate of black oxyd. The ruſt therefore is a carbonate of iron, and muſt contain all the principles which compoſe the black oxyd ; and this can contain none capable of excluding that which would convert it into ruſt.

The Doctor then remarks, " However, neither
" this, nor any other calx of iron, can be revived,
" unleſs it be heated in inflammable air (hydrogen
" gas), which it eagerly imbibes, or in contact with
" ſome

" 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 cer- " tain proof that water is decompoſed in this pro- " ceſs ?"

The ſuppoſition that a body contains phlogiſton is no proof that it does ſo.

" It may be ſaid, that the oxygen imbibed by " this iron, being expelled by heat in contact with " inflammable air (hydrogen gas), unites with that " air, and with it conſtitutes the water found af- " ter the proceſs. But for any thing that appears, " this water may be that which the iron had im- " bibed, and which can only be expelled from it " by the entrance of that phlogiſton which it had " loſt."

Doubtleſs you will be ſurpriſed to hear, that water is the ſubſtance which the Doctor ſuppoſes is capable, when combined with iron, of excluding that which would convert it into ruſt ; and you will recollect that black oxyd of iron can be formed without the aſſiſtance of water.

It is true, the Doctor obſerves on the formation of the black oxyd in oxygen gas and atmoſpherical

air, that " by far the greateſt part of the weight " of dephlogiſticated air (oxygen gas) is water, and " the air being decompoſed in the proceſs, the wa- " ter is imbibed by the iron, and the acidifying " principle (oxygen) contributes to form," according to the publication now under review, phlogiſticated air (azotic gas, *) but by his experiments and obſervations, " fixed air (carbonic acid gas), with the " phlogiſton which is at the ſame time expelled from " the iron." †

It has been already ſhewn, that no azotic gas is formed by the combuſtion of iron in oxygen gas; and the quantity of carbonic acid which has been found in the remainder of oxygen gas, in which iron has been burned, is very trifling, and is owing partly to the gas containing ſome before the operation, and partly to plumbago contained in the iron. ‡

But moreover, if the Doctor's explanation of the formation of the carbonic acid gas be accurately examined, it will be found inconſiſtent with many of his own principles.

He believes water is a conſtituent part of oxygen gas, becauſe, from certain experiments, he has inferred it conſtitutes one half of carbonic acid gas,

* See P. 15.

† Experiments and Obſervations, Vol. III. p. 551.

‡ Annales de Chimie, Tome III. p. 91—97.

and

and enters into the compoſition of all aerial fluids: and from the quantity of water obtained on burning hydrogen in oxygen gas, he ſuppoſes it conſtitutes nine parts in ten of oxygen gas. *

It will be ſhewn in the next lecture, that his opinion reſpecting the compoſition of gaſes is not well founded; but for the ſake of argument, let it for the preſent be granted, that oxygen gas is compounded as he ſuppoſes, and that carbonic acid gas conſiſts "of about one half water, and the other phlogiſ-"ton and dephlogiſticated air † (oxygen gas) in the "proportion of one fourth of the former, to three "fourths of the latter." ‡

In page 159 of the firſt volume of his Experiments and Obſervations, he ſays, "In ſix ounce meaſures "and a half of dephlogiſticated air (oxygen gas), "I melted turnings of malleable iron till there re-"mained only an ounce meaſure and one third; "and of this twenty-ſeven thirtieths of an ounce "meaſure was fixed air (carbonic acid gas)." Conſequently the oxygen gas concerned in the formation of the carbonic acid gas, muſt have occupied the ſpace of 6,06 ounce meaſures. §

* Experiments and Obſervations, Vol. III. p. 535.

† He means what he calls the acidifying principle or oxygen.

‡ Experiments and Obſervations, Vol. III. p. 536.

§ The volume of gas that diſappeared was 5,16 ounce meaſures, and that of the carbonic acid was ,9 of an ounce meaſure.

An

An ounce meaſure is equal to 1,8980 cubic inch, and therefore 6,06 ounce meaſures are equal to 11,501 cubic inches.

A cubic inch of oxygen gas weighs ,34211 of a grain; * and 11,501 cubic inches weigh 3,9346 grains.

The weight of a cubic inch of carbonic acid gas is ,44108 of a grain; † and that of ,9 of an ounce meaſure, or 1,7082 cubic inch is ,75345 of a grain.

The 3,9346 grains of oxygen gas, conſumed in the experiment, conſiſted, according to the Doctor's eſtimation, of 3,54114 grains of water, and ,39346 of a grain of oxygen: and the ,75345 of a grain of carbonic acid gas conſiſted of ,37672 of a grain of water, ,09418 of a grain of phlogiſton, and ,28254 of a grain of oxygen. Therefore the oxygen admitted into the carbonic acid gas was leſs than that contained in the oxygen gas. What became of the reſt? ‡

But the inſufficiency of the Doctor's account of the formation of the carbonic acid gas will more clearly appear, on comparing the combuſtion of iron with that of charcoal.

* Lavoiſier's Elements, Edinb. edit. Appendix, p. 490.

† Ibid.

‡ The above calculation is made on the ſuppoſition that all the carbonic acid was formed during the proceſs, although it is probable ſome of it was contained in the oxygen gas before the combuſtion of the iron.

" I heated

" I heated (ſays he *) eight grains and a quarter " of perfect charcoal, in 70 ounce meaſures of de- " phlogiſticated air (oxygen gas) of the ſtandard of " 0,46, when it ſtill continued 70 ounce meaſures ; " but after waſhing in water, it was reduced to 40 " ounce meaſures of the ſtandard of 0,6, and the " charcoal then weighed a grain and a quarter."

Suppoſing this experiment to have been accurate, which is not eaſy to do, the quantity of oxygen gas conſumed was 19,47974 grains, and that of the carbonic acid obtained was 25,11509 grains. According to the Doctor, the firſt conſiſted of 1,947974 grains of oxygen, and 17,531766 grains of water ; and the ſecond of 9,418159 grains of oxygen, 3,139386 grains of phlogiſton, and 12,557545 grains of water.

By this ſtatement, the carbonic acid gas did not contain ſo much water as the oxygen gas ; yet there is none of that fluid depoſited when perfect charcoal is burnt in dry oxygen gas : the phlogiſton in the carbonic acid was not half the weight of the conſumed charcoal ; although the Doctor ſays, page 166, Vol. I. of his Experiments and Obſervations, that charcoal is very nearly pure phlogiſton ; the oxygen in the carbonic acid gas was almoſt five times as much as that in the oxygen gas ; and from the experiment with iron, the oxygen in the oxygen

* Experiments and Obſervations, Vol. III. p. 377.

gas

gas ought to have formed 3,73023 grains of carbonic acid gas, whereas the quantity said to have been obtained was 25,11509 grains.

It is plain, therefore, the formation of the carbonic acid gas cannot be accounted for, by supposing the two gases to be compounded in the manner alleged by the Doctor, and charcoal to be pure phlogiston.

But he has said in page 547 of the third Volume "that charcoal contains all the element of fixed air "[carbonic acid gas], the acidifying principle as well "as phlogiston."——However, even this supposition will not be sufficient to remove the difficulty.

It has been already mentioned, that of the 25,11509 grains of carbonic acid gas, 3,73023 grains should on the Doctor's principles be formed by the assistance of the oxygen gas: the phlogiston supposed by him to be necessary for this quantity is equal to ,46627 of a grain, and ought to have been furnished by the charcoal. The remainder of the seven grains of charcoal should have consisted of phlogiston and oxygen, in a proportion fit for making, on the addition of water, the carbonic acid gas. But the remainder is 6,53373 grains; and as water is supposed to constitute one half of the carbonic acid gas, it ought to have formed 13,06746 grains, and these with the 3,73023 grains are less than the quantity said to have been formed by 8,31740 grains.

Now, as the formation of the carbonic acid gas by the combuſtion of charcoal cannot be reconciled to the ſuppoſed compoſition of the two gaſes, and as the explanation which the Doctor has given, of the formation of the ſame gas by the combuſtion of iron is founded on the ſame principles, it cannot poſſibly be juſt. And further, as his opinion of the compoſition of finery cinder reſts on the propriety of the explanation which he has given of the formation of the azotic gas, or of that of the carbonic acid gas, and as both have been ſhewn to be wrong, it alſo muſt be groundleſs.

In confirmation of his opinion the Doctor proceeds, " This is the more probable, ſince when " any other ſubſtance, which is certainly known to " contain oxygen, is heated in the ſame circum- " ſtances, fixed air [carbonic acid] (which is al- " lowed to contain oxygen) is found, and this is " not the caſe with this calx of iron. If for ex- " ample precipitate per ſe, or minium [the red " oxyds of mercury and lead] be heated in inflam- " mable air, the mercury and the lead will be re- " vived, and a conſiderable quantity of fixed air [car- " bonic acid gas] will be produced at the ſame time. " But if the air be previouſly expelled from the " minium which converts it into a yellow ſubſtance " called maſſicot [yellow oxyd of lead] though the " lead will be revived, no fixed air [carbonic acid " gas] will be generated. Since therefore, the re-

" ſult of treating finery cinder [black oxyd of iron]
" and maſſicot is preciſely the ſame, in the ſame
" circumſtances, we are fully authoriſed to conclude
" that the ſubſtances themſelves are ſimilar, and
" conſequently that the finery cinder contains no
" more oxygen than maſſicot."

Together with ſome water, a ſmall but not a conſiderable quantity of carbonic acid gas is commonly obtained, when the red oxyds of mercury and lead are revived in hydrogen gas. In one experiment made with red oxyd of mercury, ſent to the Doctor by Mr. Berthollet, he obtained 0,04 of an ounce meaſure of carbonic acid gas; and in another, made with red oxyd of lead, he procured 0,028 of an ounce meaſure.*

Metallic oxyds are much diſpoſed to unite with carbonic acid, and therefore the ſmall quantity obtained in the above inſtances, might with ſafety be attributed, to their having attracted it from the atmoſphere and parted with it during the reduction.

But Mr. Berthollet informs us,† the oxyd which he gave to the Doctor actually did contain carbonic acid: On diſtilling 50 grains of it and receiving the gas over lime water, although this was not at firſt made turbid, after about a quarter of an hour it depoſited a conſiderable precipitate. The precipitate he aſcribed to carbonic acid uniting with the

* Experiments and Obſervations, Vol. I. p. 168.

† Annales de Chimie, Tome III. p. 91.

lime,

lime; and its ſlow formation to the acid being held in ſolution by a great quantity of oxygen gas, and to the portions of the carbonate of lime which were firſt formed being diſſolved by the unchanged lime water.

Dr. Prieſtley obſerves on this experiment of Mr. Berthollet, "The precipitate per ſe with which "Mr. Berthollet furniſhed me, he ſays, contained "a conſiderable quantity of fixed air; and yet he "allows that when admitted to lime water, it did "not immediately make it turbid, which it is well "known a tenth part of the fixed air which I pro- "cured would have made it inſtantly and complete- "ly white. The turbulency that came on after- "wards muſt therefore have had ſome other cauſe, "probably ſome acid of vitriol [ſulphuric acid] in "the water of the trough in which the experiment "was made, and which gradually inſinuating itſelf "into the lime water in his tube, would make *a* "*ſelenite* [ſulphate of lime], a thing that has fre- "quently occurred in the courſe of my own experi- "ments, and which for ſome time puzzled me not a "little."*

But although the carbonic acid gas which he obtained would make a certain quantity of lime water inſtantly turbid, it is not well known that it would do ſo when combined with ſeven or eight cubical

* Experiments and Obſervations, Vol. III. p. 559.

 inches

inches of oxygen gas. Mr. Berthollet does not ſay, that the water in the trough of his pneumato-chemical apparatus was different from that in the veſſel in which he received the oxygen gas; and I cannot believe a man of his accuracy would think of filling the trough, for ſuch an experiment, with any other than lime water.

The yellow oxyd of lead may be formed by making the red oxyd red hot, or by expoſing lead to the ſame temperature and in contact with air. It affords no carbonic acid gas when it is reduced in hydrogen gas, not from its being deſtitute of oxygen, but from its containing no carbonic acid. The temperature to which it has been previouſly ſubmitted is unfavourable to its union with carbonic acid; but if when cooled, it be expoſed for a ſhort time to the air, it will imbibe that ſubſtance and yield it when reduced in hydrogen gas.

It will be readily allowed by the antiphlogiſtians, that if yellow oxyd of lead does not contain oxygen, the black oxyd of iron does not do ſo either: but the yellow oxyd cannot be made without the preſence of oxygen gas or atmoſpherical air; it is heavier than the lead in its compoſition; and its not affording carbonic acid is no proof that it does not contain oxygen.

The Doctor obſerves further in ſupport of his opinion, "In another important reſpect finery "cinder [black oxyd of iron] and maſſicot are ſimi-

"lar.

" lar. They are both ſoluble in marine [muriatic]
" acid without dephlogiſticating [oxygenating] it,
" which minium [red oxyd of lead] inſtantly does.
" And yet Mr. Berthollet ſays, (Annales de Chi-
" mie, Vol. III. p. 96,) that ' the heat by which
" minium becomes maſſicot cannot change its na-
" ture.' What is the evidence of a change in
" the nature of any thing, but a change of its pro-
" perties? On the whole, therefore, the proba-
" bility is, that when iron is converted into finery
" cinder, it loſes its phlogiſton, and imbibes only
" water; and that when it is reconverted into
" iron, it parts with the water, and recovers its
" phlogiſton. N. B. The experiment with the maſ-
" ſicot muſt be tried preſently after it is made, ſince
" it will very ſoon imbibe air from the atmo-
" ſphere."

The red contains more oxygen than the yellow oxyd of lead. Its parting with ſome oxygen to the muriatic acid is only a proof of what has been already noticed, that the lead retains different quantities of oxygen with unequal force.

The quotation from Mr. Berthollet is not exact. It is his intention to expreſs, that the carbonic acid and azote, with which red oxyd of lead is commonly contaminated, may be ſeparated by a high temperature, and yet the lead remain in the ſtate of an oxyd.

The

The reaſon why the experiment with maſſicot muſt be tried preſently after it is made has been already aſſigned.

Now, conſidering the manner in which the finery cinder and maſſicot are formed, and that the different circumſtances which have been alleged as proofs of their not containing oxygen are incompetent, the probability is, that they do not conſiſt of water, and iron, and lead, deprived of phlogiſton.

The Doctor then ſays, " In this place I would " obſerve that, if it be admitted that there is a prin- " ciple in inflammable air [hydrogen gas], which, " being imbibed by the calx of a metal, converts it " into a metallic ſubſtance, it will follow that the " ſame principle is contained in charcoal, and other " combuſtible ſubſtances; becauſe they will all pro- " duce the ſame effect, and therefore that the prin- " ciple of inflammability, or phlogiſton, is the ſame " in them all."

This will readily be admitted, but by the ſame mode of reaſoning it muſt follow, that if a ſubſtance cauſes the reduction of a metallic oxyd without communicating any thing to it, there can be no occaſion for the addition of any principle, and conſequently no ſuch principle as phlogiſton exiſting.

From the decompoſition, the Doctor paſſes to the recompoſition of water: but the conſideration of his obſervations on this ſubject muſt be deferred to the next lecture.

LECTURE

LECTURE II.

DR. PRIESTLEY is as much diſſatisfied with the proofs of the recompoſition, as with thoſe of the decompoſition of water. "Another pre-" tended proof [ſays he] that water is compoſed " of dephlogiſticated and inflammable air [oxygen " and hydrogen gaſes], 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 water " ſo produced is ſaid to have been entirely free " from acidity, though this experiment was on a " large ſcale, not leſs than twelve ounces of water " being procured. But the apparatus employed " does not appear to me to admit of ſo much ac-" curacy as the concluſion requires; and there is " too much of correction, allowance, and computa-" tion, in deducing the reſult. Alſo it is, after " all, acknowledged that, after decompoſing this " quantity of the two kinds of air, and making all " the allowance they could for the phlogiſticated " air, or azote, in the dephlogiſticated air, they " found fifty one cubic inches of this kind of air

" more

" more than they could well account for. This " quantity therefore, and perhaps ſomething more " (ſince the operators were intereſted to make it as " ſmall as poſſible) muſt have been formed in the " proceſs. And when this kind of air, as well as " inflammable [hydrogen gas] is decompoſed to- " gether with dephlogiſticated air [oxygen gas], " nitrous acid is produced. The probability there- " fore is, that the acidifying principle, or oxygen, " in the dephlogiſticated air which they decom- " poſed, was contained in that phlogiſticated air " [azotic gas], and that, had the proceſs been con- " ducted in any other manner, it would have aſſum- " ed the form of nitrous acid. They acknowledge " that, except when the inflammable air [hydrogen " gas] was burned in the ſloweſt manner, the " water they produced had more or leſs of acidity."

The Doctor, at one time, believed that water was compoſed of oxygen and hydrogen. But as, on repeating the experiment of burning hydrogen in oxygen gas, he could not collect as much water as was equal in weight to the gaſes conſumed; and as that, which he did obtain was mixed with nitrous acid, he was induced to change his opinion, and to ſuppoſe the water was not generated, but depoſited by the gaſes during the combuſtion, and that the body formed was the nitrous acid.*

* Experiments and Obſervations, Vol. III. p. 43, et ſeq.

The antiphlogiſtians alleged, when the experiment was performed on a large ſcale, the deficiency of water was very trifling, and never more than might with propriety be attributed to the unavoidable loſs to which ſuch experiments were liable; and they ſuppoſed the nitrous acid, found in the water, proceeded from ſome azotic gas having been contained in the oxygen gas employed, and this the more eſpecially, as no way had been diſcovered for procuring oxygen gas perfectly free from azotic gas.

The experiment alluded to by the Doctor, in the paragraph laſt read, juſtifies the reaſoning of the antiphlogiſtians. An account of it is to be found in the eighth and ninth volumes of the Annales de Chimie.

The union of the two ſubſtances was effected by filling a balloon with oxygen gas, adding to it hydrogen gas in a ſmall ſtream, and ſetting them on fire by paſſing the electric ſpark through them. To the balloon were connected reſervoirs, called gazometers, containing the two gaſes. By certain contrivances, theſe were made to ſupply the balloon with freſh portions of the gaſes, as faſt as the combination took place.

The experiment laſted 185 hours, and at the end of it there was collected of water 12 ounces 4 gros and 45 grains French weight, and there remained in the balloon a quantity of gas.

By comparing the volume of the two gaſes before the combuſtion, with that of the gas remain-

ing in the balloon, and making every neceſſary correction for the difference of temperature and preſſure, it appeared, that 12 ounces 4 gros and 49,2270 grains of the gaſes had been conſumed: the difference between this weight and that of the water is a mere trifle.

The water was perfectly pure: yet this was not owing to the oxygen gas being free from admixture; for by a preliminary experiment it was diſcovered, that 100 cubic inches of it contained three of azotic gas; and 467 French cubic inches of this ſubſtance were found in the balloon, at the end of the experiment.

The cauſe of the purity of the water was diſcovered by Mr. Seguin to be the ſlowneſs with which the combuſtion was conducted: for he has aſcertained, that, with materials of the ſame kind, the nitrous acid may be formed merely by carrying on the combuſtion quickly, and by that means raiſing the temperature to the point at which azotic and oxygen gaſes act on each other.

This obſervation of Mr. Seguin has been confirmed by Meſſrs. Pelletier and Jacquin,* and alſo by Mr. Van Marum;† ſo that pure water has been obtained in more than one experiment.

* Annales de Chimie, Tome X. p. 140.

† Ibid. Tome XII. p. 139. Mr. Van-Marum's words are, "Dans une de mes expériences la combuſtion du gaz hydro-"gène était très-lente, en employant trois heures et demie pour "la conſumption de mille pouces cubiques du gaz hydrogène,

&

But Dr. Prieſtley ſuppoſes the oxygen and phlogiſton formed azotic gas in Mr. Seguin's experiment. Certain it is, there were 51,7440 French cubic inches of azotic gas found in the balloon at the end of the experiment, above what had been found in the oxygen gas before the combuſtion.

This quantity was ſuppoſed by Mr. Seguin to have been owing to atmoſpherical air from which, the gazometers could not be completely emptied, before they were filled with the other gaſes: but whether this be the true reaſon or not, for the appearance of theſe 51,7440 cubic inches, they could not poſſibly have been formed in the manner ſuppoſed by Dr. Prieſtley.

The oxygen gas conſumed weighed 6209,869 grains and ought according to the Doctor to have contained 620,9869 grains of oxygen or the acidifying principle; but 51,7440 French cubic inches of azotic gas weigh only 22,9971 French grains. Nay the whole quantity of azotic gas, found in the balloon, was equal only to 207,55348 French grains; and yet, beſides oxygen, it ought to have contained phlogiſton and water.

" & l'eau produite par cette expérience n'avoit abſolument " point d'acide. Une autre fois la viteſſe avec laquelle l'air " entroit dans la ballon étoit à peu près d'un tiers plus grande, " & alors l'eau produite contenoit de l'acide foiblement ſen- " ſible."

In addition to these striking proofs of the inconsistency of his principles, it may be remarked, the phlogiston of the inflammable air [hydrogen gas] ought to have weighed more than the whole quantity of azotic gas.

In page 290 of Vol. I. of his Experiments and Observations, there is a calculation on the supposition that phlogiston composes one half of hydrogen gas: And in page 535 of Vol. III. he says, "Water seems to constitute about nine parts in ten of "dephlogisticated air (oxygen gas), but there seems "to be a much less proportion of it in inflammable "air (hydrogen gas.)"

The hydrogen gas expended in Mr. Seguin's experiment amounted to 1039,358 grains; and if the phlogiston be estimated at only one fifth of that weight, it will be 207,8716 grains, which is more than the weight of the whole azotic gas.

Since, therefore, the azotic gas could not have been formed by the oxygen and hydrogen, and since no other product was obtained than water, and the weight of this corresponded to that of the two gases consumed, it may with safety be inferred that they formed water by their combination.

But, continues the Doctor, "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

" drawn from them is the reverſe of that of the " French philoſophers."

In what reſpect his experiments were leſs liable to exception than thoſe of the French chemiſts, is what I cannot comprehend. Theirs were performed on a very extenſive ſcale; great care was taken to aſcertain the degree of purity of the gaſes 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 was not accurately determined.

In one experiment, he employed ſuch a quantity of the gaſes, as, he ſays, ought to have afforded a grain of water; but he collected only a quarter of a grain: in another, he ought to have got two grains, whereas he obtained only a grain and an half. And theſe are the experiments which he oppoſes to thoſe of the French chemiſts, and from which he concludes, the water is not equal to the weight of the gaſes conſumed! *

Satisfied, however, of the ſuperiority of his experiments, the Doctor proceeds to give their reſults.

" When dephlogiſticated and inflammable air " (oxygen and hydrogen gaſes), in the proportion " of a little more than one meaſure of the former

* Experiments and Obſervations, Vol. III. p. 45.

" to

" to two of the latter, both ſo pure as to contain no " ſenſible quantity of phlogiſticated air (azotic gas), " are incloſed in a glaſs or copper veſſel, and de- " compoſed by taking an electric ſpark in it, a " highly phlogiſticated nitrous acid is inſtantly pro- " duced; and the purer the airs are, the ſtronger " is the acid found to be. If phlogiſticated air (azo- " tic gas) be purpoſely introduced into this mixture " of dephlogiſticated and inflammable air (oxygen " and hydrogen gaſes), it is not affected by the " proceſs; though when there is a conſiderable de- " deficiency of inflammable air (hydrogen gas), 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 (hydrogen and azotic) 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 phlo- " giſticated air (azotic gas). A conſiderable quan- " tity of water is always produced in theſe decom- " poſitions of air. But this circumſtance only proves " that the greateſt part of the weight of all kinds " of air is water. I have, in my experiments on " terra ponderoſa aerata (carbonate of barytes) de-

" monſtrated

" monſtrated that water conſtitutes about half the " weight of fixed air (carbonic acid gas.)"

It has been already ſhewn, that hydrogen gas can neither form azotic gas nor nitrous acid; but it may be worth while to point out the reaſons for theſe reſults.

In the detail which is given of theſe experiments in the third volume of his Experiments and Obſervations, there is no notice of any preliminary attempts to aſcertain the degree of purity of the gaſes; but it is there ſaid, the oxygen gas uſed in the two trifling experiments formerly mentioned was got from the oxyd of manganeſe; and that employed in other experiments, during which the exploſions were performed in a copper veſſel, was ſometimes got from the ſame ſource, and at others from the red oxyd of mercury by the nitrous acid, and from the red oxyd of lead. Now it is well known, that all theſe ſubſtances in general contain azote.—The firſt does ſo, in ſo remarkable a degree, that the firſt portions of gas which it yields on being heated are frequently pure azotic gas. The ſecond, being made with the nitrous acid, alſo contains ſome of it. And the third attracts it from the atmoſphere, as the Doctor himſelf has diſcovered.

Mr. Cavendiſh aſcertained by his experiments, that, if there be leſs hydrogen uſed than is neceſſary for the ſaturation of the oxygen, a quantity of nitrous acid is formed; and that, if azotic gas be added

ded to the mixture, cæteris paribus, the quantity of acid is always increaſed; but, that if there be a ſuperabundance of hydrogen, no acid is produced.

Hence the reaſon why the azotic gas was not affected in Dr. Prieſtley's experiment, ſeems to have been, he uſed as much hydrogen as, with the azote contained in the oxygen gas and not attended to, was ſufficient for the whole oxygen.

It has been already made evident, that water cannot enter into the compoſition of oxygen and hydrogen gaſes in the proportion alleged by the Doctor; and by a little attention to the experiments with the carbonate of barytes it will clearly appear, there is not the ſmalleſt foundation for the opinion that water is neceſſary for the conſtitution of carbonic acid gas.

The native carbonate of barytes was ſaid, by Dr. Withering and others, not to yield its carbonic acid at any temperature to which it could be expoſed: but Dr. Prieſtley found, that when the vapour of water was ſent over it when red hot in an earthen tube, it afforded carbonic acid gas with the greateſt rapidity, and in an equal quantity as when it was diſſolved in the muriatic acid; while at the ſame time ſome of the water diſappeared. He hence concluded, that water muſt be a conſtituent part of carbonic acid gas.

"Attending," ſays he, "to the water expended "in the proceſs, I found that I procured 330 ounce

" meaſures of fixed air (carbonic acid gas) with the " loſs of 160 grains of water. According to this, as " the air weighed 294 grains, the water in the " fixed air muſt have been 80 parts of 147 of the " whole.

" In another experiment, having previouſly found " that three ounces of the terra ponderoſa (carbo- " nate of barytes) yielded about 250 ounce meaſures " of fixed air (carbonic acid gas), I attended only " to the loſs of water in procuring it, and I found " it to be one fifth of an ounce in two ſucceſſive " trials. The quantity of fixed air (carbonic acid gas) " would weigh 225 grains, and the water expend- " ed about 100 grains, ſo that in this experiment " alſo the fixed air (carbonic acid gas) muſt have " contained about one half of its weight of wa- ter." *

This calculation, however, cannot be depended upon; for the loſs which the carbonate of barytes ſuſtained was not examined, and the carbonic acid gas muſt have diſſolved a quantity of water which it would depoſite on returning to the temperature of the atmoſphere.

To theſe objections, which were firſt made by Mr. Berthollet, the Doctor has returned, " I found very " exactly how much fixed air [carbonic acid gas] a " given quantity of this ſubſtance [carbonate of

* Experiments and Obſervations, Vol. I. p. 131.

" barytes] would yield by means of water, which " appeared to be the very ſame that it yielded by " ſolution in ſpirit of ſalt (muriatic acid), and that " it yielded no air at all by mere heat without wa- " ter. It was quite ſufficient therefore to find how " much water was expended in procuring any quan- " tity of fixed air (carbonic acid gas) from this ſub- " ſtance. And as there was no other ſource of loſs " of water beſides the fixed air (carbonic acid gas), " it could not but be concluded, that it entered into " its compoſition as a neceſſary part of it, and in " the proportion which I aſcertained." *

In this anſwer, he has entirely overlooked the property which carbonic acid gas has of diſſolving water. Every chemiſt knows it has that property, and in a greater degree at a high than at a low temperature. But the water is not neceſſary to the conſtitution of the gas, becauſe it exiſts before the ſolution of the water; and it may be deprived of the water, by ſulphuric acid or any deliqueſcent ſubſtance, and ſtill remain carbonic acid gas.

Beſides, Dr. Hope, now Profeſſor of Chemiſtry in the Univerſity of Edinburgh, has diſcovered that the carbonic acid can be ſeparated from the barytes, by expoſing the compound to ſuch a temperature as can be raiſed in a ſmith's forge. †

* Experiments and Obſervations, Vol. III. p. 557.

† Edinburgh Philoſophical Tranſactions.

To

To be ſure, the diſengagement of the carbonic acid takes place at a lower temperature when water is uſed; but this is only a proof that the ſeparation is promoted by the tendency which carbonic acid gas has to combine with water.

Hence then the celebrated experiments with the terra ponderoſa aerata, or carbonate of barytes, afford no ſupport to the Doctor's principles.

"The reaſon, no doubt," ſays the Doctor, in a note at the end of the pamphlet, "why, in the experiments of the French chemiſts, the water they produced was not without acidity, whenever the flame they made uſe of was too ſtrong, was that, in that caſe, more of the dephlogiſticated air [oxygen gas] in proportion to the inflammable [hydrogen gas] was conſumed, than when the flame was weak; ſo that the reſults of their experiments exactly coincide with thoſe of mine."

When his experiments are accurately examined, they are found to confirm thoſe of the French chemiſts; but the reaſoning in the note which has been read cannot be admitted.

The appearance of flame attends the combination of an inflammable ſubſtance and oxygen, when both are in the ſtate of gas:—It is owing to their union taking place at many points at the ſame time; but as the union depends as much on the one as on the other, a proportional quantity of each muſt be as neceſſary to exhibit a weak as a ſtrong flame.

The Doctor further relates of his experiments, "When the decompoſition of dephlogiſticated and "inflammable air [oxygen and hydrogen gaſes] is "made in a glaſs veſſel, a peculiarly 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 be-"comes 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 [azotic gas] was preſent, (for in the laſt of "my 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 forma-"tion 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 compoſe others ſo very "different from each other as *water* and ſpirit of "nitre [nitrous acid]. I think I have ſufficiently "accounted for the reſult of the experiments made "by the French chemiſts on the common hypothe-"ſis, which ſuppoſes inflammable air to contain "phlogiſton; but I do not 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 evi-"dence, either for the compoſition or the decompo-

"ſition

" ſition of water, is at all ſatisfactory; and cer-
" tainly the arguments in ſupport of an hypotheſis
" ſo extraordinary, and ſo novel, ought to be of the
" moſt concluſive kind."

Having in ſome of his experiments emptied the veſſel in which the exploſions were made of common air, by means of an air pump, the Doctor ſuppoſed it might be objected, that he could not entirely exhauſt the veſſel; and it is on that account he has mentioned, that in the laſt of his experiments he did not uſe an air pump: but the azote which occaſioned the production of the denſe acid vapour was contained in the oxygen gas which he employed.

The objections contained in this ſection to the concluſions drawn from the experiments which the antiphlogiſtians conſider as proofs of the decompoſition and recompoſition of water, are,

1. Finery cinder [black oxyd of iron] does not contain oxygen.

2. The weight of the water collected after burning inflammable and dephlogiſticated airs [hydrogen and oxygen gaſes] is not equal to that of the airs conſumed. And,

3. Either the water ſo obtained is mixed with nitrous acid, or a quantity of phlogiſticated air [azotic gas] is formed.

The Doctor is of opinion,

1. That finery cinder [black oxyd of iron] conſiſts of water and iron depriyed of phlogiſton.

2. That

2. That when a metallic calx containing oxygen is reduced in inflammable air [hydrogen gas], fixed air [carbonic acid gas] is formed by the union of the oxygen with the phlogiſton and water of the inflammable air. And,

3. That during the combuſtion of inflammable in dephlogiſticated air [hydrogen in oxygen gas], the phlogiſton and oxygen form, according to circumſtances, nitrous acid or phlogiſticated air [azotic gas]; and the water obtained by the proceſs is not generated, but from being a conſtituent part of the two airs is depoſited on their union.

But from what has been ſtated in the review of this ſection it appears,

1. The ſame ſubſtance is formed by expoſing iron to the ſteam of water, by burning iron in oxygen gas, and by heating iron filings mixed with red oxyd of mercury.

2. Hydrogen gas is obtained when the iron is changed by being expoſed to the vapour of water; but there is none afforded when the change is effected by either of the other proceſſes.

3. The carbonic acid, which has been found after the reduction of certain metallic oxyds in hydrogen gas, was previouſly contained in theſe oxyds.

4. The water which may be collected when hydrogen is burned in oxygen gas is exactly equal in weight to the two ſubſtances which diſappear.

4. The

4. The azotic gas which has been found in the reſiduum was contained in the gaſes before the combuſtion.

6. The nitrous acid, formed when the combuſtion was rapid, was owing to the union of azote with oxygen.

7. The experiments with terra ponderoſa do not ſhew that water is neceſſary to the conſtitution of the gaſes. And conſequently from theſe it follows,

That when water is brought in contact with red hot iron it is reſolved into two ſubſtances, one of which combines with the iron, while the other aſſumes the form of gas—and, That water may be reproduced by reuniting theſe ſubſtances.

The third ſection begins as follows:

" Having conſidered the evidence that has been " alleged in ſupport of the antiphlogiſtic theory, " and found it to be inſufficient, I ſhall in this ſec- " tion mention a few objections that may be made " to it from other conſiderations.

" 1. 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 wa- " ter is known to enter, is preſent. But in my ex- " periments on heating finery cinder [black oxyd " of iron] with charcoal, inflammable air is pro- " duced, though, according to the new theory, no

" water

" water is concerned. According to this theory, " finery cinder, called the oxyd of iron, conſiſts of " nothing beſides iron and oxygen; and the char- " coal, 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 princi- " ples of the new theory; but nothing is eaſier on " thoſe of the old. For the finery cinder [the black " oxyd] 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 [black oxyd of " iron] uniting with the charcoal makes the inflam- " mable air [hydrogen gas], at the ſame time that " part of the phlogiſton from the charcoal contri- " butes to revive the iron. Inflammable air [hydro- " gen gas] of the very ſame kind is procured when " ſteam is made to paſs over red-hot charcoal."

Although hydrogen be a conſtituent part of water it enters into the compoſition of many other bodies, and therefore the preſence of water is not neceſſary to account for its production.

The particulars of the experiment are related in page 279 of the firſt volume of the Doctor's Experiments and Obſervations. " Having" ſays he, " made the ſcales of iron [black oxyd of iron], and

" alſo the powder of charcoal very hot, previous to " the experiment, ſo that I was ſatisfied that no air " could be extracted from either of them ſeparately " by any degree of heat; and having mixed them " together while they were very hot, I put them " into an earthen retort, glazed within and with- " out, which was quite impervious to air. This I " placed in a furnace, in which I could give it a " very ſtrong heat; and connected it with proper " veſſels to condenſe and collect the water which I " expected to receive in the courſe of the proceſs. " But to my great ſurpriſe, not one particle of " moiſture came over, but a prodigious quantity of " air, and the rapidity of its production aſtoniſhed " me; ſo that *I had no doubt* but that the weight " of the air would have been equal to the loſs of " weight both in the ſcales [the black oxyd] and " the charcoal; and when I examined the air which " I repeatedly did, I found it to contain one tenth " of fixed air [carbonic acid gas]; and the inflam- " mable air which remained when the fixed air was " ſeparated from it, was of a very remarkable kind, " being quite as heavy as common air. The rea- " ſon of this was ſufficiently apparent when it was " decompoſed by means of dephlogiſticated air, " [oxygen gas] for the greateſt part of it was fixed " air."

The Doctor now thinks this laſt mentioned fixed air was not a conſtituent part of the inflammable

air, but formed by the union of its phlogiſton with the dephlogiſticated air.

The reducing of wood to charcoal conſiſts in ſeparating the more volatile from the leſs volatile parts. This is done very imperfectly in common charcoal. The hydrogen eſpecially is retained with ſo great force that the coal muſt be expoſed to *an intenſe and long continued heat*.

It is not mentioned in the detail of the experiment, that the charcoal was previouſly expoſed to the greateſt degree of heat that could be applied, it is merely ſaid it was made very hot, and that was very far from being ſufficient.

Unglazed earthen veſſels abſorb moiſture from the atmoſphere very greedily, and it is ſcarcely poſſible to glaze accurately the inſide of an earthen retort; at all events it is quite impoſſible, without breaking the retort, to know whether it has been perfectly done or not.

The charcoal, the iron ſcales (black oxyd), and the retort ſhould all have been expoſed ſeparately to an intenſe and long continued heat, immediately before being uſed; the weight of the charcoal and iron before the experiment ſhould have been compared with their weight after it; and the weight of the gaſes obtained inſtead of being gueſſed at, ſhould have been accurately determined and compared with the loſs ſuſtained by the mixture. The experiment is, in its preſent ſtate, of no value.

Mr.

Mr. Berthollet firſt objected to the experiment that hydrogen was with great difficulty ſeparated from carbone, and the Doctor in reply to him has ſaid in page 556 Vol. III. of his Experiments and Obſervations, " How obſtinately charcoal retains " water, is eaſily aſcertained. For Mr. Berthollet " himſelf would ſay, that when any particular de- " gree 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 by " the aſſiſtance of finery cinder (black oxyd of iron) " 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 can be " no 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, will " be converted into inflammable air."

From this anſwer, the Doctor ſeems to have miſunderſtood Mr. Berthollet. He has not ſaid that charcoal retains *water* obſtinately, his words are " il paroît, par un grand nombre d'expériences que " le charbon retient fortement de *l'hydrogène;* auſſi " avons nous diſtingué le principe charboneux au " carbone du charbon ordinaire."

The results of the Doctor's experiment may be accounted for in this manner. The carbonic acid gas was formed by the union of the oxygen in the oxyd of iron with the carbone in the charcoal; and the heavy inflammable gas proceeded from the solution of some carbone in hydrogen gas furnished either by the charcoal or moisture contained in the retort.

It can be no objection to this explanation, that the charcoal afforded an inflammable gas at a lower temperature when mixed with oxyd of iron than when used by itself. If the proportion of hydrogen be very small in comparison with that of the carbone, the compound is solid at even a very high temperature; but when the proportion of hydrogen is greater, it is easily made gaseous. In the foregoing experiment the proportion of carbone was diminished by the union of part of it with the oxygen of the oxyd of iron.

At all events the explanation offered by the Doctor cannot be a just one. It has been already shewn the supposition respecting the composition of oxygen gas is unfounded, and that, even after admitting that supposition, finery cinder cannot consist of water and iron deprived of phlogiston.

2. "Though the new theory, says the Doctor, "discards phlogiston, and in this respect is more "simple than the old, it admits another new prin- "ciple, to which its advocates give the name of "*Carbone*, which they define to be the same thing "with

" 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 dephlogiſ- " ticated air, *ſee Examination of Mr. Kirwan*, p. 79. " Mr. Lavoiſier ſays, Ibid. p. 63, that ' wherever " fixed air has been obtained, there is charcoal.' " They therefore call it the carbonic acid.

" But in many of my experiments large quantities " of fixed air have been procured where neither " charcoal, nor any thing containing charcoal, was " concerned, or none in quantity ſufficient to ac- " count for it. When the pureſt malleable iron is " heated in dephlogiſticated air [oxygen gas] or in " vitriolic acid air [ſulphureous acid gas], a con- " ſiderable quantity of fixed air is formed. It is " ſaid that *plumbago* is contained in iron. But it " is not found in malleable iron, and leaſt of all in " the *air* that is expelled from it. Fixed air is " alſo produced by reviving minium [red oxyd of " lead] in inflammable air [hydrogen gas], and if " charcoal of copper be heated in dephlogiſticated " air, a quantity of fixed air equal to nine tenths of " the dephlogiſticated air will be formed. More " than thirty ounce meaſures of the pureſt fixed " air were by this means procured from ſix grains " of this charcoal, which is made by the union of " ſpirit of wine and this metal.

" Laſtly,

" Laſtly, fixed air is procured in great abun-
" dance in animal reſpiration. It is true that fixed
" air is procured by expoſing lime water to atmo-
" ſpherical air, but it is never procured by this
" means in air confined in any veſſel. There muſt,
" for this purpoſe, be an open communication with
" the atmoſphere. But fixed air will be procured
" in great abundance by breathing air contained in
" the ſmalleſt receiver, and eſpecially if the air be
" dephlogiſticated. It muſt therefore be formed
" by phlogiſton, or ſomething emitted from the
" lungs, uniting with the dephlogiſticated air which
" it meets there. It may be ſaid that ſince we
" feed in a great meaſure upon vegetables (and
" even animal food is originally formed from them)
" and this principle of *carbone* is found in all vege-
" tables, this may be the ſubſtance that is exhaled
" from the lungs. But ſince in this proceſs, it
" forms the ſame ſubſtance that inflammable air
" from iron does with dephlogiſticated air, or oxy-
" gen, it muſt be the ſame thing with it; and then
" this *carbone* will only be another name for phlo-
" giſton."

The objection, that carbone is a hypothetical being, was formerly made by Mr. Keir, and anſwered by Mr. Berthollet, " If there was no method,
" ſays he, of procuring diſtilled water, and that
" in the explanation of phenomena which are ow-
" ing to that fluid, it was conſidered independently
" of

" of the ſmall quantity of ſalts which it holds in " ſolution, would Mr. Keir look upon water as an " hypothetical being of which no idea could be " formed? Charcoal which has been well urged " by the fire contains ſometimes leſs than an hun- " dredth part of foreign matter which has no in- " fluence on its combinations; ſometimes it contains " much more: abſtraction is made of that part " foreign to its properties, and to avoid circumlo- " cution, the name of carbone is given to the char- " coal conſidered in a ſtate of purity."*

Notwithſtanding what the Doctor has aſſerted, it is ſcarcely poſſible to obtain iron free from plumbago; and this, from the quantity of carbone which it contains, can, with a due proportion of oxygen, make nearly four times its weight of carbonic acid.

The carbonic acid gas procured by the revival of the red oxyd of lead has been already accounted for.

Charcoal of copper, as Dr. Prieſtley calls it, is made by paſſing the vapour of alcohol or of oil of turpentine through a red hot copper tube: a great quantity of hydrogen gas is evolved and a black ſubſtance collects in the tube. Of 446 grains of this black ſubſtance obtained in one experiment, 28 grains were copper; and of 508 got by another, 19 grains were copper: the remainder when burned afforded carbonic acid gas.

* Annales de Chimie, Tome X. p. 145.

Theſe

Theſe experiments prove that alcohol or ſpirit of wine and oil of turpentine contain hydrogen and carbone; and that copper can ſeparate the carbone from the hydrogen. Charcoal of copper, therefore, is not a compound of ſpirit of wine and copper, but of carbone and that metal.

That carbonic acid is formed during reſpiration is moſt certain, and that it is ſo by the addition of ſomething to the oxygen contained in the atmoſphere is equally certain; but the Doctor has forgot when he ſays, "it forms the ſame ſubſtance which "inflammaable air from iron does with dephlogiſ-"ticated air, or oxygen."

In page 285 of the firſt volume of his Experiments and Obſervations, when ſpeaking of the carbonic acid gas obtained by burning the inflammable gas which is procured by paſſing the vapour of water over red hot charcoal, he ſays in the text. "That "the fixed air [carbonic acid gas] is not generated "in this proceſs, is evident *from there being no fixed "air found after the exploſion of dephlogiſticated air "[oxygen gas] and inflammable air from iron.*" And in a note at the bottom of the page he obſerves, "When I wrote this paper, I imagined that "the fixed air, which was found on the decompoſi-"tion of this inflammable air with dephlogiſticated "air, had been contained in the inflammable air. "But it will appear, that it muſt have been formed "by the union of phlogiſton [or inflammable air]

" and dephlogiſticated air, made by the exploſion ;
" though it is remarkable *that no fixed air is formed*
" *when the inflammable air from iron is uſed.*"

Beſides in p. 562. Vol. III. he ſays of inflammable air from iron, " that it may not only be waſhed " in lime water, but even be wholly decompoſed by " being fired together with dephlogiſticated air, " *without diſcovering any fixed air at all.*"

Therefore the identity of carbone and the ſuppoſed phlogiſton has not been eſtabliſhed.

The third objection is a repetition of what he has ſaid before. " 3d. The antiphlogiſtians always ſup- " poſe azote, or phlogiſticated air, to be a ſimple ſub- " ſtance, though I think abundant evidence has been " given (and more will be found in my laſt memoir, " printed in the Tranſactions of the Philoſophical " Society at Philadelphia), that it is compoſed of " phlogiſton and dephlogiſticated air."

The abundant evidence which has been given amounts to, an aſſertion that he has ſhewn in a ſeries of experiments that azotic gas is formed during the oxydation of iron ; and the circumſtance of 51,744 cubic inches of azotic gas, having been found in the reſiduum of the great experiment made by Mr. Seguin and others, above what had been diſcovered in the oxygen gas before the combuſtion.

It is ſcarcely neceſſary to remind you, the firſt is contradicted by his own experiments and thoſe of Mr. Lavoiſier ; and that the laſt, cannot be account-

ed for on his principles, even after granting a number of unfounded suppositions.

It is the Doctor's object, in the memoir which is to be published in the fourth volume of the Transactions of the Philosophical Society of Philadelphia, to prove, that there is a greater quantity of oxygen in the atmosphere than is supposed by the antiphlogistians, and was formerly believed by the Doctor himself; and that some of the azotic gas found after the combustion of certain substances in atmospherical air, is formed by the union of their phlogiston with the oxygen of the atmosphere. But as the Doctor has not favoured us with a detail of his experiments, and as they bear the most striking marks of not having been performed with accuracy, I will not take up your time with a review of them.

The Doctor then remarks,

" 4. 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 know- " ledge of the real constitution of natural sub- " stances; but we cannot adopt one, the principles " of which we conceive not to be sufficiently ascer- " tained. For other objections to this nomencla- " ture, I refer to the Preface to Mr. Keir's excel- " lent Dictionary of Chemistry. However, whe- " ther we approve of this new language or not, it " is now so generally adopted, that we are under " the necessity of learning, though not of using it."

Although the new nomenclature is not ſtrictly methodical, and its terms are rather uncouth and harſh, yet as, in as far as the ſtate of our knowledge enables us to judge, it in general expreſſes either the properties or compoſition of bodies, I moſt heartily recommend it.

The Doctor ſums up, " On the whole, I cannot " help ſaying, that it appears to me not a little ex- " traordinary, 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. In- " deed, it requires ſo difficult and expenſive an ap- " paratus, and ſo many precautions in the uſe of it, " that the frequent repetition of the experiment can- " not be expected; and in theſe circumſtances the " practiſed experimenter cannot help ſuſpecting the " accuracy of the reſult, and conſequently the cer- " tainty of the concluſion.

" But I check myſelf. It does not become one " of a minority, and eſpecially of ſo ſmall a mino- " rity, to ſpeak or write with confidence; and

" though I have endeavoured to keep my eyes open, " and to be as attentive as I could to every thing " that has been done in this buſineſs, I may have " overlooked ſome circumſtances which have im- " preſſed the minds of others, and their ſagacity is " at leaſt equal to mine.

" The phlogiſtic theory is not without its difficul- " ties. 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 ele- " ment of *heat*, though we do not doubt but that " they are properly *ſubſtances*, capable, by their ad- " dition, or abſtraction, of making great changes in " the properties of bodies, and of being tranſmitted " from one ſubſtance to another."

The experiments adduced in ſupport of the anti-phlogiſtic doctrine are neither ambiguous, nor explicable on either *hypotheſis*, nor few.—Thoſe of the French chemiſts were performed with the greateſt care and nicety, and a few ſuch are of more conſequence than thouſands made without a due regard to accuracy and preciſion; and if I miſtake not it has been ſhewn, that they cannot be explained on the Doctor's principles, and that his numerous experiments confirm theirs.

The experiment of the formation of water has been frequently repeated. It has been performed on a large ſcale by Mr. Monge, by Meſſrs. Lavoiſier

and

and Meuſnier, by Meſſrs. Fourcroy, Vauquelin, and Seguin, by Meſſrs Pelletier and Jacquin, and by Mr. Van Marum.

The Doctor deſerves credit for his candour; but I confeſs I am rather ſurpriſed, that after having calculated the weight of phlogiſton and oxygen, and ſupported a theory on the ſuppoſition of the calculations being right, he ſhould ſtate, as the chief difficulty attending that theory, the impoſſibility of aſcertaining the weight of theſe ſuppoſed ſubſtances.

However, if theſe ſubſtances or principles did actually exiſt, one or both of them would have weight; for charcoal, whether it be "very nearly pure phlogiſton," or contains "the acidifying principle as well as phlogiſton," can be weighed.

It has been ſhewn, that the formation of carbonic acid gas by the burning of charcoal, cannot be explained on the Doctor's principles, if both the phlogiſton and the oxygen have weight; and it will be found equally inexplicable if one of them be deſtitute of that property.

Let it firſt be ſuppoſed the phlogiſton has no weight; and let it be again granted, that his experiment on the combuſtion of charcoal in oxygen gas was accurate. The oxygen in the conſumed charcoal could weigh no more than ſeven grains; and theſe, added to the tenth part of the weight of oxygen gas, would make 8,947974 grains. One half of the weight of carbonic acid gas is ſuppoſed to be water, and conſequently if the phlogiſton has no weight,

weight, the other half ought to be owing to the oxygen. But there could not be more of this oxygen than 8,947974 grains, and theſe, with an equal quantity of water, ought to have formed only 17,895948 grains of carbonic acid gas, while the quantity ſaid to have been obtained was 25,1150 grains.

On the other hand, if the oxygen be ſuppoſed to be deſtitute of weight, and phlogiſton to be heavy, as the phlogiſton could not exceed ſeven grains, the quantity of carbonic acid gas ſhould have been fourteen grains.

Beſides, the water in the oxygen gas ought, in either caſe, to have exceeded that ſuppoſed to be neceſſary for the conſtitution of the carbonic acid gas. What became of this exceſs? Why did it not combine with the one grain and quarter of unconſumed charcoal?

Although it is more than probable that light, and the cauſe which excites in us the ſenſation of heat or caloric, are bodies; yet their exiſtence as ſuch does not make a neceſſary part of the antiphlogiſtic doctrine.

As the different parts of this ſection have no immediate connection, it is unneceſſary to make any recapitulation.

The following note is ſubjoined to the laſt ſection: "N. B. For anſwers to the objections of "Mr. Lavoiſier and Mr. Berthollet to ſome expe- "riments of mine relating to this ſubject, I refer

"to

" to the laſt edition of my Obſervations on Air, " Vol. III. p. 554."

Such of theſe anſwers as were applicable to the objections, which have been laid before you, have been already conſidered.

I have now, Gentlemen, finiſhed the reading and examination of Dr. Prieſtley's pamphlet. Perhaps, from having been ſo particular, I have almoſt exhauſted your patience; but I truſt you will excuſe me, as the fate of ſeveral important branches of chemical ſcience is involved in that of this ſubject.

From the view which has been given of the different explanations of the phenomena of combuſtion it appears, that Becher's is incomplete; Stahl's, though ingenious, is defective; the antiphlogiſtic is ſimple, conſiſtent, and ſufficient; while Dr. Prieſtley's, reſembling Stahl's but in name, is complicated, contradictory, and inadequate. You doubtleſs therefore will be inclined to prefer the antiphlogiſtic doctrine: Indeed you may adopt it with ſafety; for from being a plain relation of facts, it is founded on no ideal principle, on no creature of the imagination; it is propt by no vague ſuppoſition, by no random conjecture; it is dependent upon nothing whoſe exiſtence cannot actually be demonſtrated; whoſe properties cannot be ſubmitted to the moſt rigorous examination; and whoſe quantity cannot be determined by the teſts of weight and meaſure.

THE END.