A treatise on air, containing new experiments and thoughts on combustion : being a full investigation of Mr. Lavoisier's system, and proving, by some striking experiments, its erroneous principles : with strictures upon the chemical opinions of some eminent men / by Richard Bewley, M.D.

No. 1

A

TREATISE ON AIR,

CONTAINING

NEW EXPERIMENTS AND THOUGHTS

ON

COMBUSTION;

BEING A FULL INVESTIGATION

OF

MR. LAVOISIER'S SYSTEM;

AND PROVING,

BY SOME STRIKING EXPERIMENTS,

ITS ERRONEOUS PRINCIPLES;

WITH

STRICTURES UPON THE CHEMICAL OPINIONS

OF SOME

EMINENT MEN.

BY RICHARD BEWLEY, M. D.

LONDON:

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Anno 1791.

TO THE

ROYAL SOCIETY.

GENTLEMEN,

I Reſpectfully preſume to dedicate this Treatiſe to you. I would not, in the common language of moſt dedicators, enlarge upon your learning and merit. Theſe, ſince you became a ROYAL SOCIETY, have attracted the notice and admiration of Europe; ſo that, by your ſkilful and uſeful reſearches in the philoſophical world, you have riſen to the firſt rank amongſt the learned ſocieties. Aerial ſtudies have been, for theſe laſt twenty years, the favourite purſuit of learned and ingenious men; and your Society, ardently anxious to carry theſe purſuits to the utmoſt, has been foremoſt in its labours, and moſt ſucceſsful in them.

Theſe conſiderations induce me to preſent you, as the Guardians of ſcience in this happy and learned iſle, with

with a theory, which fully accounts for all the aerial phenomena, by a true and accurate inveſtigation of atmoſpherical air, its formation and purpoſes. And, though it is in oppoſition to the opinions of ſome of your leading and moſt reſpectable members; yet, as juſtice and candour have always been the line of your conduct, I make no doubt, but you will give it an impartial diſcuſſion.

It may be thought, that the moſt regular way would have been, to have ſent the ſheets to the Society, that they might have been publicly read to the members: This could not be conveniently done, as they are too extenſive to have formed a paper for the inſpection of the Royal Society.

Need I ſuggeſt upon the preſent occaſion, the liberal behaviour of the great Bergman to the immortal Scheele, when he preſented to him his theory of heat: For, though it has turned out not to be the true one; yet it merited a fair examination. I am not

ſo

ſo vain as to compare myſelf to that eminent chemiſt; I would only ſuggeſt that, the chemical opinions in my Treatiſe deſerve a patient and candid hearing. The ſyſtem I ſupport, is that of an ingenious chemiſt, whoſe labours and himſelf, have met with uncandid treatment.

The properties and uſes of air, are a ſubject of the utmoſt importance to ſcience; and, as this ſyſtem, reſpecting theſe, carries all the fair marks of truth, I recommend it to your protection, as to its proper Guardians; not doubting, but your conduct towards it will be, conformable to what is liberal and juſt, and that future ages will have occaſion to applaud your behaviour.

GENTLEMEN, *I am,*

With the moſt profound Reſpect,

Your moſt humble Servant,

RICHARD BEWLEY.

LONDON,
February 12th, 1791.

A

TREATISE

ON

AIR.

THAT fire when concentrated and fixed, forms phlogiſton, is an idea that chemiſtry has long taught, it being conſiderably an older doctrine than Sthall's; though they who have adopted the one have likewiſe the other, there being no contradiction in the union.

It has been the opinion of the firſt philoſophers as well as chemiſts. Indeed, upon inveſtigation it appears ſo obvious, that it is almoſt impoſſible to think otherwiſe.

Sir Iſaac Newton (though he ſuppoſed that light and heat were the general particles of matter put in motion) ſaw that light was produced from the integral parts of bodies: He ſays, "Are not groſs bodies and "light convertible into one another? The

" change of groſs bodies into light, and light " into groſs, is very conformable to the courſe " of nature, which ſeems delighted with " tranſmutations."

Dr. Franklin is of the ſame opinion, who ſays, " I have been inclined to think that " the fluid *fire*, as well as the fluid *air*, is at- " tracted by plants in their growth, and be- " comes conſolidated with the other mate- " rials of which they are formed, and makes " a great part of their ſubſtance; that when " they come to be digeſted, and ſuffer in the " veſſels a kind of fermentation, part of the " fire, as well as part of the air, recovers its " fluid *active* ſtate again, and diffuſes itſelf " in the body, digeſting and ſeparating it; " that the fire ſo re-produced by digeſtion " and ſeparation continually leaving the body, " its place is ſupplied by freſh quantities, " ariſing from the continual ſeparation; that " whatever quickens the *motion of the blood* in " an animal, quickens the ſeparation, and re- " produces more of the fire, as exerciſe; that " all the fire emitted by wood and other com- " buſtibles when burning, exiſted in them " before in a ſolid ſtate, being only diſco- " vered when ſeparating; that ſome foſſiles, " as ſulphur, ſea-coal, &c. contain a great " deal of ſolid fire, and that in ſhort, what " eſcapes and is diſſipated in the burning of " bodies, beſides water and earth, is generally " the air and fire that before made parts of " the ſolid."

This

This ſame theory has been regularly taught by all chemiſts; indeed, as Dr. Black obſerves, if we can believe our ſenſes, we muſt ſuppoſe that heat and light proceed from the evolution of phlogiſton.

The late chemiſts upon air, to counterbalance theſe great authorities, have adopted ſome ſingular theories from their late experiments; ſo that, inſtead of following the paths which have been hitherto trodden, they have purſued others in direct oppoſition.

Dr. Crawford's principles of heat have been lately received; theſe ſuppoſe heat and phlogiſton to be two diſtinct bodies which repel each other; the inveſtigation of which we ſhall now enter upon.

Dr. Crawford when he firſt publiſhed, made a very conſiderable difference between the heat that was imparted by pure and impure airs; but upon more accurately attending to the experiments, he found that they were very erroneus: The moiſture in the airs when condenſed, being the great cauſe of producing the heat; and, he likewiſe found, that inſtead of inflammable air giving little or no heat, that it poſſeſſes, agreeable to his experiments, at the rate of 21,4000, while dephlogiſticated air only poſſeſſes 4,7490.

This is ſtriking againſt his firſt principles of caloric and phlogiſton repelling each other; ſo that it appears from his experiments, that the inflammable air, which is principally phlogiſton retains ſo much more heat.

But as we ſhall have to obſerve in other inſtances, a theory once adopted is tenaciouſly purſued by its author to the very laſt extremity. The ſet of experiments he now gives us, after making many fruitleſs attempts, and correcting many errors, we ſhall examine; they are the moſt perfect of his experiments, and were made under the inſpection of Mr. De Luc. It is a moſt ſingular apparatus, to meaſure the heat of ſuch a light body as air; I can compare it to nothing but weighing a feather in a pair of ſcales that would weigh St. Paul's.

EXPERIMENT XI.

" Air in the vault — 60.7

" One pound, three ounces and two drams " of ſpermaceti oil being introduced into each " of the tinned veſſels, the temperature in the " veſſel R, was 59.9; that in L, 60 plus.

" The cylinders, containing atmoſpherical " air, were heated in an adjoining vault by " means of the water bath, and were ſuddenly " immerſed in the oil, upon which the central " heats being accurately marked during the " ſpace of 10 minutes,

Minutes.				
In 2	R was 60.2		L 60.2	
3	60.9		61.	
4	61.5		61.5	plus.
5	62.	minus.	62.1	minus.
6	62.3	plus.	62.4	plus.
7	62.6	minus.	62.7	
8	62.8			

8	62.8	62.9
9	62.9	63.
10	63. plus.	63.1 plus.

" Air in the vault 61.7."

In the firſt two minutes, the veſſel R gained one tenth of a degree more than the veſſel L; in the next minute, the veſſel L gained two tenths of a degree more than the veſſel R; in the next minute, the veſſel R gained two tenths of a degree more than the veſſel L; in the next minute, the veſſel L gained one degree more than the veſſel R. Upon the 9th minute, the veſſel L gave a ſudden ſpring, and gained two tenths of a degree; but, upon the tenth minute, the veſſel R likewiſe exerted itſelf and came up with it. There the race ended; as it was the point the Dr. wiſhed for, he very judiciouſly leaving off, in all his experiments, at the point he wants. The three experiments quoted, continues ten, ſix, and eight minutes, juſt as they anſwer his doctrine; they being ſuch uncertain races, there is no confining them to a certain diſtance. It is truly ridiculous.

EXPERIMENT XII.

" Air in the vault	—	62.6
" Oil in the tinned veſſel R	—	62.1
" In the veſſel L	—	62.2

" Air phlogiſticated by hepar ſulphuris was " introduced into the cylinder R, and at- " moſpherical air into L. The cavities of the " ſtems of the cylinders were accurately cloſed

" by

" by corks in the manner deſcribed above; the " cylinders were then heated as before, and " immerſed in the oil; and the central heats " being obſerved during ſix minutes.

Minute.		
In 1	R was 62.2	L 62.3
2	62.5	62.6 *p.*
3	63.1 *p.*	63.3
4	63.7 *p.*	63.9 *p.*
5	64.2 *p.*	64.3 *p.*
6	64.5	64.6

" Air in the vault at the concluſion of the " experiment 63.3.

" This experiment was repeated with the " following reſult."

We ſhall find this *courſe* a little more equal, though it ought not to have been ſo, as they were different airs employed in the experiments: In the firſt minute, they each gained alike; the ſecond minute, the ſame; in the third minute, the atmoſpherical air imparted a tenth of a degree more; in the fifth minute, it loſt it again; in the ſixth, when the experiment ended, they had both imparted the ſame heat to the oil, which agreeable to the Doctor's theory ought not to have been.

But theſe experiments are inaccurate in every reſpect. In the eleventh experiment, one part of the atmoſpherical air imparted three degrees, and the other two degrees of heat to the oil in two minutes; but in the twelfth experiment, it imparted four degrees of heat to the oil in two minutes, though the experiment

periment was every way the ſame that could affect the thermometer; and ſo through the whole minutes, comparing one experiment with the other, there are the ſame differences in all the experiments. Can any confidence be placed in ſuch experiments, in which there is ſo great irregularity? Firſt one cylinder, and then another, imparting greater heats; this ſhews that ſuch a ſmall degree of heat is not to be depended on.

EXPERIMENT XIII.

" Air in the vault — 63.
" Oil in the veſſel R — 62.3 *m.*
" In L — 62.3 *m.*

" The cylinders, containing phlogiſticated " and atmoſpherical air as before, were heated " and immerſed in the oil, and the central " heats being obſerved during eight minutes,

	R	L
In 1 Minute	62.4	62.4
2	62.7 *p.*	62.7 *p.*
3	63.4	63.5
4	64. *p.*	64.1
5	64.5 *m.*	64.5
6	64.8 *p.*	64.9
7	65.1	65.1 *p.*
8	65.3 *p.*	65.3

" Two meaſures of the phlogiſticated air, " uſed in theſe experiments, having been mix- " ed, previouſly to its introduction into the " cylinder

" cylinder, with one of nitrous air, the mea-
" ſures of the teſt were 2.9. After the experi-
" ments were finiſhed, a part of it was drawn
" from the cylinder, and being again examined
" by nitrous air, the meaſures of the teſt were
" found to be 2.8, very nearly. From which
" it appears, that very little alteration had been
" produced in it by expoſure to heat in thoſe
" experiments."

In the firſt and ſecond minutes, they imparted the ſame heat; in the third, the atmoſpherical air imparted the tenth of a degree more; in the fifth minute, the phlogiſticated air imparted a tenth of a degree more; in the ſixth, the atmoſpherical air imparted a tenth of a degree more; in the ſeventh, the phlogiſticated air imparted a tenth of a degree more; in the eighth minute, they imparted the ſame.

Theſe experiments were the laſt, the others being full of errors, which he acknowledges and corrected, otherwiſe I ſhould have given them all regularly as they occurred. The only obſervation I can make, is, that I am ſurprized that they could be brought as evidence for the hypotheſes, ſuch an apparatus to prove ſo delicate an experiment; the reſults were, as we might expect, very uncertain and irregular.

The Doctor ſeems to be ſenſible of this, for he ſays, " It may perhaps be doubted, whe-
" ther ſuch minute differences may not be
" liable to much uncertainty." We agree with him,

him, that the tenth of a degree is too minute for obſervation; but were it ever ſo plain, it would not in the leaſt avail him, for there is the greateſt irregularity in the whole.

Indeed we ſhould have ſuppoſed, that there would have been ſome difference in pure and impure airs, and likewiſe, that inflammable air would have imparted the greateſt heat, according to its ſpecific gravity; from this cauſe, their having water in their compoſition. Dr. Prieſtley, in forming inflammable air, found that water in a great quantity was imbibed into its compoſition. I have alſo found, that when rarified by heat, it will abſorb moiſture in the act of rarefaction, and give it back again when condenſed: That this is the caſe with atmoſpherical air, has been long ago aſcertained by experiments in the air pump.

Moreover, Mr. Darwen found (ſee Philoſophical Tranſactions, vol. 78.) that air when rarified abſorbs heat, and when condenſed, gives it back again: Therefore, under this review of the circumſtances, we may reaſonably ſuppoſe, that had there been a perfect apparatus for the experiments, ſome more heat would have been imparted by the pure than impure airs; particularly, as moiſture is imparted by their condenſation. This circumſtance will, no doubt, ſtrike Dr. Crawford, and he will ſee, that the great degree of heat produced by his firſt experiments, and by which he was led to adopt wrong concluſions, was heat imparted

by the moiſture to the iron filings. But (not to make uſe of ſuch an awkward apparatus, which may be properly compared to the weighing of St. Paul's and a feather) take dephlogiſticated air in one bladder, and phlogiſticated air in another, their temperature of heat being different; and ſee, if upon their being mixed together, the one imparts ſo much greater heat than the other. This is making the experiment upon the ſame principle, as that of the well known one, of the two fluids, *mercury* and *water* when mixed together; which ſhew ſuch a different capacity for heat, the latter retaining ſo much more heat than the former, though its ſpecific gravity is ſo much leſs: For this ſame experiment gave riſe to this great doctrine of bodies having different capacities for heat.

The explanation of which hath been very happily accompliſhed by Dr. Harrington. He proves, (ſee his thoughts on air) that inflammable air, or phlogiſton, is concentrated fire, and that when bodies are ſaturated with this inflammable air or phlogiſton, they will imbibe a leſs quantity of actual fire, or fire in a free diſengaged ſtate; conſequently, a leſs degree of heat will raiſe them to a certain temperature, than bodies which do not poſſeſs ſo much fixed heat or phlogiſton; therefore, they will impart leſs to colder bodies.

We would by no means contradict Dr. Crawford's ſuppoſition, that the capacities of bodies for actual heat are very different; and alſo,

alſo, that the calces of metals retain a greater quantity of this kind of looſe attracted heat, than the metals themſelves; that the more of concentrated and fixed heat in the ſtate of phlogiſton the calces attract, the greater will be their capacity to repel the more looſe and diſengaged heat. This doctrine appears reaſonable, and is the ſame in other chemical phenomena. Metallic bodies have their full ſaturation of fire; ſo have alſo the neutral ſalts a full ſaturation of acids. But, in one caſe, if you apply higher concentrated fire to the one, you will expel the leſſer concentrated fire in the other. In the ſame manner, if you apply a higher concentrated acid to the neutral ſalt, you will expel the weaker; juſt as the mineral acids expel the vegetable, and ſo on. (See Dr. Harrington, p. 140.)

As to the other arguments brought by Dr. Crawford in defence of his doctrine, they have been long ago produced by Dr. Harrington in favour of his, and are much more agreeable to, and more ſtrongly corroborate his ſimple and elegant theory of animal heat; therefore we ſhall not take notice of them here, but only obſerve in general, that we have been as conciſe as poſſible (yet not to darken, but render the ſubject perſpicuous) throughout the whole of this treatiſe.

Dr. Crawford's apparatus ſeems to have been, in every reſpect, inadequate to the experiments; for it was not impervious to air, ſince the common air of the atmoſphere, he

found after the operation had entered into the cylinders. If ſo, water muſt likewiſe have entered, as the one is more penetrating than the other.

Dr. Black's doctrine of latent heat, was what Dr. Crawford founded his theory upon; but by endeavouring to ſuit it to his theory, he has diſtorted it in the manner we have ſhewn. But this is nothing to what Mr. Lavoiſier has done. Dr. Crawford paid ſome little regard to the principles of it, and to the doctrine of the different capacities of different bodies for heat. But Mr. Lavoiſier has with gigantic ſtrides levelled all before him; all muſt give way to his ſuperior theory. But let us attend to their experiments. In the Philoſophical Tranſactions, vol. 74. p. 348. Mr. Watt ſays, "This "experiment may be made more completely "by means of the excellent apparatus which "Meſſ. Lavoiſier and De la Place have con-"trived for ſimilar purpoſes.

"Until direct experiments are made, we "may conclude, from thoſe which have been "made by the gentlemen juſt named, on the "decompoſitions of air by burning phoſ-"phorus and charcoal, that the heat extricat-"ed during the combuſtion of inflammable "and dephlogiſticated air is much greater "than it appears to be; for they found that "one Paris ounce (= 576 Pariſian grains) "of dephlogiſticated air, when decompoſed "by burning phoſphorus, melted 68,634 "ounces of ice; and as, according to another

"of

" of their experiments, ice, upon being melted,
" abſorbs 135° of heat, by Fahrenheit's ſcale,
" each ounce of air gave out 68,634 × 135°
" = 9265°,590; that is to ſay, a quantity
" of heat which would have heated an ounce
" of water, or any other matter which has the
" ſame capacity for receiving heat as water
" has, from 32° to 9265 10-2: a ſurpriſing
" quantity! (It is to be underſtood, that all
" the latent heats mentioned herein are com-
" pared with the capacity of water). And
" when an ounce of dephlogiſticated air was
" changed into fixed air, by burning char-
" coal, or by the breathing of animals, it melt-
" ed 29,547 ounces of ice; conſequently we
" have 29,547 × 135° = 3988°,845. the
" quantity of heat which an ounce of dephlo-
" giſticated air loſes when it is changed into
" fixed air. By the heat extricated during
" the detonation of one ounce of nitre with
" one ounce of ſulphur, 32 ounces of ice were
" melted; and, by the experiment I have
" mentioned of Dr. Prieſtley's (6), it appears
" that nitre can produce one half of its weight
" of deplogiſticated air."

Here it appears from their own experiments, what an amazing quantity of heat is produced from an ounce of nitre and ſulphur; a quantity ſufficient to melt 32 ounces of ice: Then, agreeable to all their doctrines, this great degree of heat muſt come from the oxygen gas in the nitrous acid. Let us ſee then how much it contains according to theſe great che-

miſts,

miſts, Mr. Lavoiſier, De la Place, and others. They ſay, " Mr. de la Place and I deflagrated " a convenient quantity of nitre and charcoal " in an ice apparatus, and found that twelve " pounds of ice were melted by the deflagra- " tion of one pound of nitre. We ſhall ſee, " in the ſequel, that one pound of nitre is " compoſed, as under, of

Potaſh	7 oz.	6 gros	51.84 grs.	= 4515.84 grs.
Dry acid	8	1	21.16	= 4700.16

The above quantity of dry acid is compoſed of

Oxygen	6 oz.	3 gros	66.34 grs.	= 3738.34 grs.
Azote	1	5	25.82	= 961.82."

But they make no allowance for the water that nitre is ſuppoſed to contain: Here then, oxygen gas that is contained in this ounce of nitre is not half an ounce; being not double the quantity that the phoſphorus conſumed, therefore it ought not to have melted 32 ounces of ice. Beſides, if the experiments had been made with nitre and phoſphorus, the heat would have been conſiderably greater, as phoſphorus is more inflammable than ſulphur; nay, I have found, that if phoſphorus is well mixed with the pureſt nitre, a far leſs quantity of the nitre than one ounce, will do for the combuſtion of one ounce of phoſphorus: Beſides, in forming nitre into dephlogiſticated air, a great part of the nitrous acid goes over entire into the receiver; therefore, it could not all go to the formation of the dephlogiſticated air.

But

But Dr. Harrington hath ſufficiently explained how the dephlogiſticated air in this proceſs is produced.

" It is very extraordinary in myſterious " ſubjects, what imperfect reaſonings will be " diſpenſed with. The water of cryſtalliza- " tion in the nitre, is eſtimated at 34 grains, " 487 grains of air is produced; and yet it is " ſuppoſed that this air is produced by the " decompoſition of the water.

" Let us take a general view of theſe impor- " tant experiments. Dr. Prieſtley got from 2 " ounces, or 960 grains of nitre, 487 grains of " air; the nitre loſt in the experiment, 531 " grains. Mr. Kirwan computes that 960 grs. " of nitre contain about 326 of the nitrous " acid, and 115 of water. Hence there is more " air generated than the weight both of the " acid and water; then we muſt take in the " alkali as making a part of the air, according " to their nice calculations. But when we " conſider that in the diſtillation of nitre, " there is a conſiderable quantity of the ni- " trous acid that goes over entire into the re- " ceiver, more of the alkali muſt in conſe- " quence have gone to the formation of the " air. The loſs in the nitre was 531 grains, " and as none of the alkali is ſuppoſed to go " over entire into the receiver, and as the " nitrous acid does, it muſt therefore have " added more conſiderably to the formation " of the air. In the proceſs, whether it yields " pure or impure air, it matters not, as it is

" allowed

" allowed that the impure air is formed of " the nitrous acid and phlogiſton; and I al- " low that it is the ſame air as dephlogiſticat- " ed air, but only having a weaker concen- " tration of fire."

Dr. Prieſtley has likewiſe clearly proved, that in air being formed from the nitrous acid and mercury, there is a conſiderable loſs of the mercury. He ſays in his preface to vol. III. " Being unwilling, however, to depend wholly " upon my own addreſs in experiments of this " kind, I conſulted Mr. Magellan (who, in- " deed, firſt ſuggeſted to me his ſuſpicion that " the Abbe muſt be miſtaken in the fact) and " he engaged Mr. Wench jun. (whoſe ſkill " and care in chemical proceſſes, no perſon " who is acquainted with him can queſtion) " to make the experiment in the moſt accurate " manner he could deviſe.

" Accordingly he diſſolved an ounce (Apo- " thecaries weight) of the pureſt mercury, in " the pureſt nitrous acid; and both myſelf " and Mr. Magellan were preſent, when he " revivified exactly one half of the red precipi- " tate made from it in a glaſs retort, ſurround- " ed with live coals, in a *reverberatory* furnace; " when the mercury has wholly ſublimed in- " to the neck of the retort, a very ſmall quan- " tity only of a browniſh matter remaining " unſublimed. The whole being carefully " weighed, together with the retort, which " had alſo been weighed before the proceſs, it " appeared there was a loſs of 88 grains, which

" is

" is ſomething more than one third, of the " weight of the quickſilver. We are all ſa- " tisfied that it was not poſſible to make the " experiment with more fairneſs."

' Another comment we muſt make upon ' the experiments of Dr. Prieſtley. Two ounces ' of nitre in one proceſs, yield 787 ounce ' meaſures of impure air, viz. 1.25. The ſame ' body in the ſame proceſs, yields 812 meaſures ' of pure air, viz. 0.95. Now phlogiſticated ' air and dephlogiſticated air, are ſuppoſed to ' be two of the elements or bodies which com- ' poſe the nitrous acid, and here is more of ' theſe airs produced than the weight of the ' nitrous acid. But which is more ſtriking ' than this, the nitrous acid produces in one ' experiment, principally one of its elements; ' and upon repeating the proceſs, another of ' its elements is produced, and to ſuch a con- ' ſiderable quantity, one being above the ' weight of the acid loſt, allowing for what ' comes over entire. Then, agreeable to their ' theory, the alkaline ſalt decompounds the ' nitrous acid in the one proceſs, by attracting ' its pure air, and in the other its impure air; ' can we poſſibly reconcile ſuch contradictions? ' And this impure air being more conſiderable ' than they will allow the acid to contain.

' Whether my theory is juſt, I leave to the ' impartial world; but I muſt aſſert thoſe ' theories to be erroneous. My theory imme- ' diately accounts for theſe phenomena; the ' nitrous acid unites with the alkali and water,

'and with the fire they are all neutralized and 'aerilized. But agreeable to the quantity 'and manner of the fire's being employed in 'this proceſs, the air will have different im-'pregnations.

'The acids generate air as we might have 'ſuppoſed, a priori. The mineral acids do it 'the beſt, as having the ſtrongeſt attraction 'for the earths and alkaline ſalts; therefore 'they are aerialized with the higheſt ſaturation 'of theſe three bodies, viz. acid, fire, water 'and earth, or ſalt. The nitrous acid is the 'beſt of theſe, as having the greateſt attraction '(allowed ſince the firſt days of chemiſtry) for 'phlogiſton or concentrated fire. The metal-'lic earths are better than other earths, for 'the ſame reaſon, having a ſtrong attraction 'for concentrating fire; proved from their 'being capable of being reduced by fire alone, 'and ſtill more particularly, the calx of lead 'and mercury are the beſt, as having the 'greateſt attraction for phlogiſton, being eaſily 'reduced, even with fire alone. But the ve-'getable acid being ſo weak, having ſo much 'leſs attraction for the earths, ſalts, and con-'centrated fire, it therefore makes an imper-'fect or impure air.'

Now let us conſider more particularly the circumſtances, half an ounce of oxygen gas, in being condenſed into the phoſphoric acid, is ſuppoſed (agreeable to their theory) in its condenſation, to produce heat equal to melt 34 ounces of ice: But when a leſs quantity of

of oxygen gas is already condenſed, and hath given out the heat, which it does in its condenſation, that it ſhould produce nearly an equal quantity of heat, ſo as to melt 32 ounces of ice, is an evident abſurdity. Beſides, as we have before obſerved, if this ſame quantity of nitre was applied to phoſphorus, it would have melted more than 34 ounces of ice. Oxygen gas in its aerial ſtate, is not in the leaſt acid, while it is ſuppoſed to poſſeſs its caloric; but it loſes it upon being imbibed into the ſulphur, and phoſphorus forming the vitriolic acid, and phoſphoric acid. Then, when it has entered into the nitrous acid, forming that ſtrong mineral acid, the firſt in point of acidity, at leaſt, next to the vitriolic acid, which they ſuppoſe contains no caloric. Can we ſeriouſly think it takes with it all its caloric? Nay, it muſt poſitively take more with it than the oxygen gas poſſeſſes, as it produces in combuſtion with phlogiſtic bodies, more heat than could be produced from the ſame quantity of oxygen gas, which they ſay it poſſeſſes. But if we are to pholoſophize upon this experiment, agreeable to Dr. Black's and Crawford's theories, the wonder will be ſtill greater; for in the experiment, the ſulphur of burning nitre and ſulphur, is formed into the vitriolic acid, and likewiſe different aerial bodies are formed; ſo much as, had the authors of this extraordinary theory, endeavoured to prove that a degree of heat, equal to the melting of 32 ounces of ice, had diſappeared in-

ſtead of appeared, this experiment would have made them ride triumphant. But it is too abſurd for men who deſervedly rank ſo high in chemiſtry to defend. For heaven's ſake liſten to the voice of reaſon, nothing can be gained by ſuch a controverſy *.

But let us ſee, gentlemen, if combuſtion will aid your theory in other proceſſes. The ſtrong concentrated nitrous acid, being mixed with high phlogiſtic bodies; ſuch as eſſential oils, inflammable air, &c. will produce violent ignition. But in theſe experiments they ſuppoſe

* Which train of reaſoning upon this difficult ſubject is the more logical? There undoubtedly are many caſes in which the temperature is lowered as a body dilates, and where an apparent influx of heat produces no effect but expanſion; and again, where an increaſe of temperature accompanies contraction of bulk. Then what can be more plauſible than the idea conveyed by capacity for heat, and the analogy of water alternately imbibed by a ſpunge, and ſqueezed out of it, by which we gain at leaſt ſome conception of a mechaniſm placed ſo far beyond the ſphere of the ſenſes? It has always been a recommendation of falſe opinions when they were accompanied by ſome analogy eaſily graſped at by the imagination.

I am very doubtful how far the ſpecious theory of our philoſophers will, this time, prove to be the ſyſtem of Nature: does not the very familiar experiment of the deflagration of gunpowder preſent appearances directly contrary to its principles?—not to enumerate the other well-known inſtances of mixture, where much heat is generated, and at the ſame time, an abundant extrication of aeriform matter is obſerved—here we have great expanſion and violent heat. Will it be ſaid, that both the heat, that goes to conſtitute elaſtic fluids, and that becomes ſenſible, flows out from the mixed materials in conſequence of a diminution of their capacity; and was already contained in them? Can this be proved in the caſe of nitrous acid and oil, black wadd and oil, lamp-black and oil, iron filings and ſulphur?———Dr. Beddoe's Chemical Experiments and Opinions, p. 12.

ſuppoſe, that the oxygen gas of the nitrous acid, leaves its nitrous baſe, and unites to the inflammable air, or the eſſential oils. Then, agreeable to Mr. Lavoiſier's theory, the oxygen gas uniting to the *carbone* of the eſſential oils would form fixed air, and this fixed air ſhould be equal in weight to near the whole of the nitrous acid employed. This is agreeable to their theories, but we find no ſuch quantity of fixed air formed; nay, in ſome experiments, not even an atom of it. That acids may be aerialized into different acid airs, either nitrous or fixed, is agreeable to Dr. Harrington's theory, and what we might have ſuppoſed a priori.

Gentlemen, I will not diſpute thoſe extraordinary chemical attractions, but only obſerve, that in all your theories, which are a maſs of ſtrange contradictions and abſurdities, you make the table of chemical attractions run the gauntlet. In this experiment of the nitrous acid and oils, there is a great heat generated, and likewiſe an abundant extrication of aeriform matter. Now, let us ſee if we cannot inveſtigate the cauſe: the oxygenated muriatic acid will equally produce combuſtion in theſe experiments, but the common marine acid will not; the difference in theſe two acids, is neither more nor leſs than this; the one is phlogiſticated, and the other deplogiſticated. Mr. Lavoiſier ſays, That the one poſſeſſes more of the oxygen gas than the other; this is not ſo, for the proceſs by which the

the oxygen marine acid is formed from the common marine acid is, by adding *minium* or *manganeſe* to the latter, which are ſuppoſed to impart their oxygen gas to the acid. Dr. Harrington has given a rational and juſt explanation how the lead acts in dephlogiſticating the marine acid. This effect is produced from the acids and earth, which, when united, have a ſuperior attraction for phlogiſton, than when ſeparated.

'In forming pyrophori, ſo as to retain a 'quantity of fire neceſſary to make them, there 'is required an acid and an earth; for Mr. 'Bewly found, when he took all the acid 'from the earth, that it would not make a 'pyrophorus. One of the moſt ſtriking facts, 'which ſhews how neceſſary it is to have an 'acid and an earth in order to attract fire or 'phlogiſton, is the experiment upon the ni-'trous vapour, which being ſo condenſable, 'as a colder body, is capable of doing it, and 'attracts its fire from it, and condenſes the 'acid; but if it paſs through water it attracts 'a quantity of the water ſufficient to make it 'become a permanent vapour, or the nitrous 'air. Chemiſtry affords us a number of facts. 'Manganeſe or the calces of lead will dephlo-'giſticate the marine acid, ſo as to give it the 'power of diſſolving gold. Both the manga-'neſe and the earth of the lead is ſaturated 'with the acid, and together they have the 'power of attracting the phlogiſton from the 'reſt of the acid. So that when this dephlo-

'giſticated

'gisticated acid is expoſed to gold it will 'diſſolve it from its increaſed appetite for the 'gold's phlogiſton.

'If into a ſolution of iron you put more 'iron it will be diſſolved, and cryſtals will be 'formed. The calx in the ſolution being ſa-'turated with the acid; upon freſh iron being 'added to them, they attract the iron's phlo-'giſton, and cryſtallize. Mr. Kirwan in the 'Philoſ. Tranſ. vol. lxxiii. p. 74. ſays, "This 'increaſed affinity of the calx of iron to 'phlogiſton is not a mere ſuppoſition; for, if 'into a ſolution of iron, ſo far dephlogiſticated 'as to refuſe to cryſtallize, ſome freſh iron be 'put, the impoveriſhed calx will re-attract ſo 'much of the phlogiſton given out during the 'ſolution of the freſh iron, that it will now 'afford cryſtals, as Mr. Monnet has obſerved 'in his excellent Treatiſe on Vitriolization."

'The very ſingular fact is owing to this, 'that copper is capable of decompounding a 'ſolution of iron, and iron of decompounding 'a ſolution of copper. The calces of each, 'when united with the acid, have an increaſed 'appetite for the phlogiſton of each other's 'metal. There are many other facts in che-'miſtry that we may bring to eſtabliſh this 'fact.'

But, according even to Mr. Lavoiſier's theory, the oxygen gas which the calces of the lead can impart to the marine acid, muſt have loſt all its caloric; for in the com-buſtion of metals, a great heat is produced: This

This, he ſays, is from the oxygen gas imbibed in the proceſs: Nevertheleſs, even this oxygen muriatic acid, acts with more violence in combuſtion, than the nitrous acid. Moreover, Dr. Harrington ſays, That the oxygenated muriatic acid is not made from the calx of lead imparting to it its oxygen gas; becauſe, though the calx of lead has had all its oxygen gas, previouſly taken from it by fire, it will nevertheleſs oxygenate the marine acid. But that this acid (which Dr. Prieſtley, with great propriety, calls dephlogiſticated) is formed by being dephlogiſticated by the calx of lead, is, I think clear; for, if phlogiſton, in any form, is added to it, it will become the common marine acid.

But even ſuppoſe we were to allow them theſe ſtrange ſuppoſitions, that combuſtion is ſometimes owing to the oxygen gas in the marine acid, imparted by the calx of lead: The combuſtion is more conſiderable than can be accounted for from ſo ſmall a quantity of oxygen gas as the calx of lead could poſſeſs, or impart to the acid; but with our explanation is perfectly eaſy: For, the dephlogiſticated marine acid attracting phlogiſton ſo ſtrongly, ruſhes to an union with ſuch violence, as to ſet the dormant fire looſe; and ſuch a quantity being ſet at liberty, will ſet fire to the reſt of the compoſition.

But let us attend to Dr. Harrington's explanation of theſe phenomena. The nitrous and dephlogiſticated marine acids, having the greateſt

eſt attraction for phlogiſton, (an old and well aſcertained chemical fact) will, when mixed with high concentrated phlogiſtic bodies, (which phlogiſtic bodies are formed of fixed fire) forcibly and chemically attract this fire; and this fire in theſe phlogiſtic bodies is attracted by the acid of fixed air, or other bodies, which have a leſs attraction for it. Conſequently, ſuch a quantity of it will be ſet looſe as is able to produce ignition. We have, in this eſſay, explained the principles of theſe facts.

That this explanation is juſt, appears very probable from the vitriolic or other acids, which being added to the eſſential oils, great heat will be produced, but not enough to produce ignition, becauſe they poſſeſs not ſo great an attraction for concentrated fire or phlogiſton. Again, if the nitrous and marine acids are phlogiſticated, or not ſo high concentrated, they will produce heat, but not enough to produce ignition.

There is an experiment which very ſtrongly ſhows the principle of combuſtion. If the leaſt moiſture falls upon a body formed of the nitrous acid, the caloric from lime, and the volatile alkali; or even ſome times if it but ſuffers agitation, it will forcibly explode. By moiſture it makes the heat become more actual, (the effect being the ſame as it has upon quick lime) and aided with the power of the nitrous acid, they both penetrate together into

the volatile alkali, and decompound its phlogiſton into actual fire.

But let us attend to combuſtion in other proceſſes. The nitrous vapour, which is obtained by inflammable air, paſſing through nitrous acid, attracts part of the acid and keeps it ſuſpended. Here then is no heat to keep it in an aerial form; for, upon its being again condenſed by water, no heat is received from it: But if theſe two bodies are fired, there will be a great exploſion, with an immenſe quantity of heat. Dr. Crawford is forced to acknowledge, that this nitrous vapour poſſeſſes as much caloric as dephlogiſticated air. And, Dr. Prieſtley, in accounting for this phonomenon, ſuppoſed that this nitrous vapour, from its producing ſuch violent ignition, muſt be dephlogiſticated nitrous air; but, as Dr. Harrington ſays, if this vapour is nicely examined, it will be found to be the true nitrous acid undecompounded *.

Mr. Lavoiſier's hypothéſis ſuppoſes that the acid vapour is decompounded by the inflammable air; that is, the inflammable air attracts the phlogiſticated or nitrous air, and leaves the dephlogiſticated air free to explode with the remainder of the inflammable air. Thoſe great

* It is probable that the vapour of pure nitrous acid, contains as much abſolute heat as atmoſpherical air; for the power of the former, in maintaining flame, is nearly as great as that of the latter. Hence we may account for the ſpontaneous accenſion of a mixture of oil of turpentine and nitrous acid.—Crawford's Experiments and Obſervations on Animal Heat, p. 420.

great theoriſts differ widely upon ſo ſimple an experiment. Inflammable air, by paſſing through the nitrous acid, attracts part of the acid. That this is ſimply the caſe is demonſtrable; for if, (as Dr. Harrington obſerves) previous to the operation of its being burnt, if the inflammable air, along with its acid vapour, is paſſed through water, the acid will be imbibed by the water, and the air will loſe the power of burning; and if the water is examined, it will be found to have imbibed the pure nitrous vapour, or acid. But Dr. Prieſtley, agreeable to his hypotheſis, thinks it is dephlogiſticated nitrous vapour: However, if the acid is examined when it is received, by the water waſhing it from the inflammable air, it will be found to be rather phlogiſticated, having imbibed a little phlogiſton from the inflammable air.

It is ſurpriſing how vague and undetermined all theſe great theoriſts are upon every experiment and phenomenon. They always have different opinions, each being in the dark, and each following their vague conjectures.

It is certain, that not the oxygen gas, but the pure nitrous acid, has to do in this proceſs; for after the combuſtion, the acid is found entire. Likewiſe Dr. Prieſtley found, that if the acid vapour and inflammable air were not fired immediately, but ſtood long enough, ſo as to form an union, they would not burn of themſelves; but upon being ſet on fire with atmoſpherical air, they burnt like

a mixture of nitrous and inflammable airs. See Prieſtley, vol. III. p. 261. However, it ought to have been the reverſe. For while the acid remained entire, it ought not to have burned, but upon its being decompounded, ſo as to form nitrous air, it muſt have impregnated them with its oxygen gas, and ſo have been more adapted to ignition.

But when we examine it, according to Dr. Harrington's theory, we find an eaſy and rational explanation. For, as the acid vapour has a ſtrong attraction for concentrated fire or phlogiſton of the inflammable air, ſo when fire is added to them, the fire and the acid penetrate together into the fixed fire and ſet it free, producing that violent exploſion. But if they act not together, no ignition is produced; becauſe, if the acid is expoſed for one day, or any certain time, ſo as that its acidity may be neutralized with phlogiſton, the conſequence is, it loſes its attraction for phlogiſton or fixed fire. The whole of which Dr. Harrington has ſet in the cleareſt point of view, and proved to a demonſtration.

This will appear in a ſtill more ſtriking light in alkaline air, when formed by heat into inflammable air. No chemiſt, I preſume, can be a ſtranger to the ſtrong attraction which alkalies and acids have to each other; therefore, if in the experiment, the alkaline air is made uſe of, our explanation will appear the more evident. Nature has wiſely ordained that no ignition can be produced but by the

operation

operation of two cauſes at the ſametime. A combuſtible body, though fire is applied to it, will not burn without the agency of another body. This great agent in nature is atmoſpherical air; and in chemical proceſſes, either nitrous acid, or dephlogiſticated marine acid. How they act has been already explained; and this explanation will greatly aſſiſt us in ſhowing how atmoſpherical air acts. Atmoſpherical air, or the pure part of it, is a compound of an acid and phlogiſton: This we ſhall undiſputably prove afterwards, and find that this compoſiton is capable to produce fire. But atmoſpherical air being compoſed of a mild acid, and of a mild not of a high concentration of fixed fire, parts with its own fixed fire, when actual fire is applied to it; however, the heat it produces is not powerful enough to keep the air burning, or decompounding its own fixed fire, unleſs it is expoſed to ſome combuſtible body having a high concentration of fire.

Again, if fire is applied to a combuſtible body, not having the aid of this compound; which is compoſed of an acid and a weak concentration of fire, called atmoſpherical fire, it will not burn; its fixed fire being ſo cloſely and ſtrongly attracted, as renders it impoſſible to be decompounded. This theory is very ſtrikingly confirmed in what chemiſts call dephlogiſticated nitrous air. If this air is formed not of a ſtrong ſaturation of phlogiſton, it will allow a candle to burn in it with a bright and vivid flame; but if it has gotten a higher ſatura-

tion

tion, it will burn with greater vehemence, and the flame will extend over a greater part of the air; that is, the flame will become extended from the candle, and more of the air will be burning at the ſametime: And if ſaturated with a ſtill greater quantity of phlogiſton, it will all take fire together, and burn with an exploſion like to that of inflammable and atmoſpherical air; the agent and the principle being both in the ſame body.

To theſe facts I would have the reader pay a particular attention, ſince by doing ſo, he will be the better able to underſtand the principles of bodies burning. For I pledge myſelf to prove in the cleareſt manner, and I hope to his ſatisfaction, that this dephlogiſticated nitrous air is formed of the nitrous acid and phlogiſton; ſo that, if he is not bit with the tarantula of modern chemiſtry, it is impoſſible he ſhould miſtake it.

Air is a combuſtible body as well as the body which it conſumes. Nature's own air being only a weak ſaturation of fixed fire, forms only a weak combuſtion; therefore, bodies burning in it, burn comparatively very gently, conſuming themſelves in a ſlow and gradual manner. And nature has happily joined its own air with another kind of air, which is not combuſtible; ſo that a ſmall ſurface of the phlogiſtic particles ſhall come in contact with the flame, or burning bodies. But artificial dephlogiſticated air having a higher ſaturation of fixed fire, or phlogiſton, allows

lows a candle to burn with a more extended flame, and with cracklings, or (as Dr. Harrington calls them) partial exploſions. And if a ſtill higher ſaturation, (being before an agent to produce ignition in another body) it will in this caſe become both agent and principle, and decompound itſelf. For Dr. Prieſtley formed this air with ſo high a ſaturation, as to burn and explode like inflammable air *; and, upon a particle of fire being added to it, it immediately conſumed itſelf. For, containing the two principles of combuſtion, viz. the agent, or nitrous acid, and the principle, which is a high ſaturation of phlogiſton, from either eſſential oils, metals, ſpirits of wine, &c. all of which will form it; this effect neceſſarily followed: But had the principle been leſs, highly ſaturated, it would not have conſumed itſelf; becauſe, one of nature's invariable laws is, that the action of two bodies is required to ſupport fire; otherwiſe, were the bowels of the earth ſet on fire, the fire would not ſtop till the whole were conſumed; and the ſame would happen with regard to air.

The aerial forms have been ſo myſterious to chemiſts, that to give a rational and philoſophical account of them, has perplexed them much; yet we can bring ſolid bodies to confirm our theory. For the nitrous acid, or the dephlogiſticated marine acid, when mixed with eſſential

* Viz. dephlogiſticated nitrous inflammable air.—See Prieſtley, vol. IV. p. 455.

eſſential oils, or the volatile alkali, will juſt equally promote ignition. A further corroboration of this theory is, if pure, ſtrong, actual fire, (as in the electrical ſpark or fire) is applied to atmoſpherical air; it will decompound it in a ſlow and gradual manner; only repeated application is required. And, that this electrical fire acts as pure fire, is what we ſhall prove in the ſequel.

We have endeavoured to prove, that atmoſpherical air is formed of an acid, with a weak ſaturation of phlogiſton or fixed fire, that therefore it will not burn or decompound itſelf, without the aid of a body containing a higher ſaturation of fire. This is evident in ſaltpetre, which being an acid united to a weak ſaturation of fixed fire, acts in combuſtion very ſimilar to atmoſpherical air. Therefore, it will not burn of itſelf, unleſs united to a body of a higher ſaturation of fire; and then both together will produce fire with great vehemence. But take the nitrous acid, and unite it to an exactly ſimilar body, only of a higher ſaturation of fire, viz. the volatile alkali, and they will burn of themſelves without the aid of another body. This is juſt the ſame as highly ſaturated dephlogiſticated nitrous air burning without the aid of a third body. Nay, by only aerilizing this nitrous flammans, it will form this identical air, viz. dephlogiſticated nitrous air, though not altogether ſo inflammable, from being not ſo much condenſed.

It

It is ſtrange to think what ſingular theories have been adopted by our modern chemiſts; when this ſame nitrous ammoniac or *nitrum flammans* was aerilized, which is eaſily done, being a volatile body, they would, from its forming the nitrous dephlogiſticated air, have the acid decompounded. The volatile alkali (our fathers in chemiſtry have conſequently miſnamed it) remains fixed, decompounding the acid. The explanation which we have given, being ſimple and plain, cannot poſſibly be miſtaken, as they are both volatilized. For, in the name of wonder and common ſenſe, do they not equally burn in their ſolid ſtate, as in their aerial? Are not our ſenſes convinced that they are united in this ſolid ſtate, without having recourſe to the aid of *the aerial philoſophy?* But according to this theory of theirs, they leap over the moſt obvious facts.

It is natural for the human mind, when travelling in the dark, to fix upon ſomething, no matter how prepoſterouſly abſurd; but when a principle is laid down which fully explains all the phenomena, not to embrace it, is certainly highly wrong.

The dephlogiſticated marine acid air has a conſiderable degree of attraction for concentrated fire; and, when the volatile alkali is added to it, is burns very beautifully, forming (allow the expreſſion) a ſea of fire.—As our doctrine of ignition depends upon fixed fire being ſet looſe, and as we have proved that the nitrous acid will do it of itſelf, when unit-

ed to high phlogiſtic bodies, ſuch as eſſential oils. Nay, even bodies with leſs phlogiſton, will be ſet on fire without heat, when an acid of a ſtronger attraction is employed; viz. the neutral ſalt, formed of the dephlogiſticated marine acid and fixed alkali: It will fulminate with the leaſt friction, as Mr. Sage found. Yet, when the alkali is united to the vitriolic acid, it will not fulminate, even when mixed with combuſtible bodies: But if the nitrous acid is added to the alkali, being an acid of a ſtronger attraction for fire, it will then fulminate, when mixed with combuſtible bodies, as in gunpowder, &c. yet, after the combuſtion of the marine neutral ſalt, if the reſiduum is examined, we will find the marine acid entire, and not decompounded.

A very ſtriking experiment is this; expoſe many different metals to the dephlogiſticated marine acid air; viz. iron, copper, lead, and many others, and ignition will immediately take place, without any heat being added at the time. And let Mr. Lavoiſier and his advocates well examine the calces of the metals, after the combuſtion, and they will find them compoſed of the earths of the metals and the marine acid, and not of the oxygen gas: Becauſe the marine acid air decompounds the phlogiſton, and ſets the fire looſe.

There is another chemical proceſs that can give us great aſſiſtance in this inveſtigation. Lemery found, that if ſulphur, iron and water are made into a paſte, they would take fire in

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ſo ſtriking a manner, as induced him to ſuppoſe that theſe three bodies are the cauſes of the violent concuſſions, which ſometimes happen to our earth. Sulphur is formed of the vitriolic acid and concentrated fire; iron of concentrated fire and earth of iron. Now, there is a ſtrong attraction between the vitriolic acid and the earth of iron; but the concentrated fire hinders their union. Moreover, the water which has likewiſe a ſtrong attraction for the earth of iron, (ſeen in a moſt ſtriking manner in the ſteam of water having a power of itſelf to decompound iron without the aid of the ſulphur) will, along with the ſulphur, decompound the iron. The motion or fermentation produced by theſe new compoſitions and decompoſitions, will ſet the fire looſe, and produce great expanſion and flame. The phlogiſton of the ſulphur and the iron have eſcaped after the combuſtion, and nothing but fire did eſcape: Therefore the phlogiſton muſt have eſcaped as fire; for the reſiduum is the vitriolic acid united to the earth of the iron with the water.

Let us ſee if their theory will give any ſatisfactory ſolution of theſe phenomena. The vitriolic acid is formed and the iron reduced. Air could not do it; for the combuſtion will take place (as Lemery found) where air cannot have acceſs. Then, if we allow them their doctrine, it would (I will take upon me to ſay) be giving countenance to the moſt extravagant

and extraordinary doctrine ever ſeriouſly preſented to the public.

For, according to their theory, water is formed of inflammable and dephlogiſticated airs. Then, (according to Mr. Lavoiſier) upon the ſulphur and iron attracting the dephlogiſticated air of the water, the inflammable air would be free and ſet looſe in the experiment: But were this the caſe, we ſhould not only have found, after the combuſtion, the phlogiſton of the ſulphur and iron, but likewiſe the phlogiſton of the water. But they have all diſappeared; and inſtead of heat being produced, an immenſe degree of cold ſhould take place, the heat going to the expanſion of the inflammable air. If we pay the leaſt regard to Dr. Black's ingenious doctrine of latent heat, we muſt ſee that this is agreeable to it; and it is upon the principle of latent heat that their new doctrines are founded. But even to wave this argument; we find that the water is not decompounded in the proceſs, for it is found entire, the phlogiſton only having *diſappeared*, and the heat *appeared*. Hence we find that, by chemical fermentation (which ſhows itſelf by great agitation or ebulition) heat is ſet free, which brings us near to Sir Iſaac Newton's theory: But, as Dr. Harrington has ſhown, it is only the motion of the particles of this particular body called phlogiſton.

We have tried the truth or falſeneſs of Mr. Lavoiſier's theory by the experiments he has produced to confirm it, and which he thinks

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moſt favourable to his doctrine. But even in theſe experiments, he has taken only one view of them; for, upon an accurate inveſtigation, we ſhall find that even they contradict his theory.

When ſtrong nitrous acid is mixed with eſſential oils, a vivid flame with active combuſtion takes place, and with a comparatively ſmall generation of nitrous and phlogiſticated airs.

The new theoriſts, to explain theſe phenomena, ſay, that the oxygen gas of the acid unites to the *carbone*, or phlogiſton of the eſſential oils: But then, in this caſe, an immenſe quantity of fixed air ought to have been formed; as eſſential oils, when burned with pure air, form fixed air, and that nearly in weight to the pure air. Now, by attending to their own calculations in p. 14, we ſhould have expected to have found a quantity of fixed air, nearly in weight to the quantity of nitrous acid employed in the experiment. However, the fact is this; if the ignition is vivid and ſtrong, not an atom of fixed air is found after the combuſtion.

But, (as Dr. Harrington hath clearly demonſtrated) agreeable to the intenſeneſs of the fire, the air produced will be of different kinds; becauſe theſe acid airs are clearly the different acids aerilized, and their particular kind depends upon the proceſs.

This fact is ſtrongly proved by the experiment of burning nitre and charcoal together, which (as Mr. Cavendiſh ſays) form only phlogiſticated

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ed air. He alſo ſays, (Philoſophical Tranſactions, vol. lxxiv. p. 135.) "Before I enter in-
"to the cauſe of theſe phenomena, it will be
"proper to take notice, that phlogiſticated air
"appears to be nothing elſe than the nitrous
"acid united to phlogiſton; for when nitre is
"deflagrated with charcoal, the acid is almoſt
"entirely converted into this kind of air. As
"far as I can perceive too, at preſent, the air
"into which much the greateſt part of the acid
"is converted, differs in no reſpect from com-
"mon air phlogiſticated."

Now, here is a very ſevere blow given to Mr. Lavoiſier's doctrine; for it is evident that the oxygen gas of the nitrous acid and the charcoal do not produce fixed but phlogiſticated air: Yet this charcoal is his *carbone* and from it his theory of the compoſition of fixed air has originated. But this doctrine ſhall undergo a more particular inveſtigation in the ſequel.

This theory appears ſtill more defective in accounting for the *caloric*, which, in their proceſſes, is produced by burning the nitrous acid with bodies, which, according to Mr. Lavoiſier, contain *carbone*; but phlogiſton, according to others.

The difference of heat produced in combuſtion, when ſuch bodies as charcoal, eſſential oils, &c. are employed; bodies which reduce oxygen gas to fixed air; and phoſphorus, ſulphur, &c. which reduce oxygen gas to acids: I ſay, the difference of heat produced by combuſtion in forming ſtrong acids, is above twice the

the quantity to what it is when the oxygen gas forms fixed air. Therefore, in burning bodies, principally formed of *carbone* with acids; *inſtead of caloric, an equal degree of cold ought to have been generated.* And alſo in the combuſtion of the marine and nitrous acids with eſſential oils, nitre and charcoal; inſtead of heat there ought to have been an immenſe degree of cold produced in aerilizing the fixed air, which theſe proceſſes ſhould generate.

Nature having wiſely ordained, that the atmoſpherical air, a light aerial phlogiſtic body, ſhould be the agent, by which combuſtion is conducted, as being eaſily ſuſceptible of taking fire. But then its fire is ſo light and weak, that it is not capable of conſuming or burning, without the aid of another combuſtible body, which is ſet on fire by the atmoſpherical fire being kindled; and then, the ſtrong combuſtible body, (as a candle for inſtance) is alſo lighted, both fires acting together, ſo as to keep up the combuſtion. But were the fire applied to the candle without the agency of atmoſpherical air, no combuſtion would follow, even ſuppoſe the candle is compoſed of inflammable materials. For, both atmoſpherical air and a combuſtible body are required to produce ignition, and that upon account of the attraction and concentration of fire. There will be no combuſtion, unleſs they both act at the ſame time; or, that intenſe heat neceſſary to ſupport the ignition, or the conſuming of the body, will not be produced.

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The cauſe of which is moſt probably this; the intenſe heat produced acts upon the combuſtible bodies concentrated fire or phlogiſton, ſo as ſuddenly to expand them, and to break their attraction from the bodies with which they were chemically united. For the conſumption of one particle of its fire, is the means of conſuming the neighbouring particles, and ſo on, till the whole body is conſumed, or broken down by the ſeparation of its concentrated fire or phlogiſton; and from the mechanical expanſion, a conſequence produced from great heat; ſo that the whole texture of the body will be broken and reduced to aſhes. This is evident from intenſe heat conſuming or burning bodies without ignition; viz. light combuſtible bodies, as paper, which, being expoſed to a great heat, will be equally conſumed or burnt, as if it had been ignited. But that it is not ignited is evident from the air not being acted upon by the paper: And the ſame phenomenon will take place in foul air, or in *vaccuo*. Hence it is evident that the paper is conſumed by having its phlogiſton or fire ſeparated from it by the heat.

From this cauſe it is, that even attrition can act in the ſame mechanical way, by diſſolving the union of phlogiſton, or ſetting looſe the fire which form it, from its chemical union or attraction; and when it is ſet looſe it becomes heat or fire. The mechanical and chemical operation of bodies is eaſily connected: For inſtance, take two pieces of wood, and, by the

the aid of great mechanical powers, let them be rubbed the one againſt the other; this will produce fire. We ſhall ſuppoſe, that the fire produced by rubbing the two pieces of wood was chemically united in the ſtate of phlogiſton to a degree of power equal to the force of fifty pounds weight. Now, if a degree of power equal to ſixty pounds weight is applied in the form of attrition, it will in conſequence overcome the chemical.

That this really takes place in nature, is clear from the experiments performed by Dr. Darwin; (ſee Philoſophical Tranſactions, vol. lxxviii. p. 43.) He has ſhown by uncontrovertable experiments, that the thermometer falls by the ſudden expanſion of the air, and riſes by its compreſſion. Now, this demonſtrates, that the chemical attraction may be overcome by mechanical force: For when the air is allowed to expand itſelf, it receives and chemically attracts a great quantity of heat; but when compreſſed, that chemical attraction is counteracted, and the heat becomes ſenſible again to the thermometer. According to this general rule, I would thus define chemical attraction; that whenever a body, poſſeſſing a quantity of heat, is expoſed to the influence of another body, of a lower temperature, it will not impart its heat to that body. This therefore implies, that its heat is not looſe or active, but is under the influence of ſome power which retains it; which is real and chemical attraction only. As in all the other powers or

agents in nature conſiſts of different influences; therefore ſome bodies attract their fire with greater force than others. For can chemical philoſophers ſuppoſe, but that any chemical attraction might be overcome by mechanical powers. For example; the particles of acids and alkalies have a ſtrong chemical attraction; but ſuppoſe you could fix a cord to each of theſe particles, would you not be able to ſeparate them? you certainly would. And if two dry bodies, the particles of which act ſtrongly upon each other, are rubbed together, this will be the caſe. Hence, it will be found, by taking a general view of attrition as producing fire, that it acts according to the mechanical force exerted, and to the bodies retaining their concentrated fire. It is more looſely concentrated in wood than in metals: But the texture of wood being ſofter, there will be leſs force; yet, from its phlogiſton or fire being more looſe, it is more eaſily ignited; as is ſeen in its actual ignition by air and fire: Wood burning with more eaſe than metals. The great Sir Iſaac Newton proved that mechanical force can produce fire; but it is only in bodies that poſſeſs phlogiſton. To this I believe there is no exception. However, the phenomenon is ſtrikingly ſeen only in ſuch bodies as will admit of this mechanical force, and which poſſeſs a great quantity of phlogiſton: For example, in iron and flint, bodies which have the moſt phlogiſton.

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This will reconcile the chemical and mechanical idea of heat, which has been the occaſion of ſo much controverſy amongſt philoſophers. For (as Dr. Harrington well obſerves) no bodies, but thoſe which are phlogiſtic will produce fire; phlogiſton being concentrated fire. And he ſays further, (ſee his Thoughts on Air, p. 209.) I agree with Sir Iſaac Newton, that heat conſiſts in motion; but it is only the motion of thoſe peculiar particles of fire or phlogiſton. The motion of other bodies will not produce it.

But when the alkali is united to the dephlogiſticated marine acid, the aid of no other combuſtible bodies, nor even heat, but only a little friction is required; the mechanical action of which forces the two bodies to a cloſer union, and by that means aids their attraction. The acid, though juſt ready to ruſh in upon the fixed fire of the alkali, is not able, becauſe of the weakneſs of its acid, to break the union of the fixed fire; but aided with the friction it accompliſhes it.

Dr. Harrington, whoſe theory in all its different principles has been fully proved, calls alkaline ſalts phlogiſtic bodies; but ſays, that they are compoſed of phlogiſton, with a leſs concentration of fire than oils, bitumens, ſulphur, &c. This is an experiment which fully juſtifies and confirms this opinion, however much it may have been ridiculed. The marine neutral ſalt, from its fulminating power, is an important body in the inveſtigation of

combuſtion. It is a body formed neither of the *carbone*, nor of the inflammable air of Mr. Lavoiſier, nor of the phlogiſton of Dr. Prieſtley and others; and yet it will burn.

And if we attentively examine the reſiduum, we ſhall find that the alkali has diſappeared, and the acid is left with moiſture. What has ſeparated in the fulmination but heat and light? Need I make the application? It would be to affront the good ſenſe and judgment of my chemical readers.

Do we not obſerve their extravagant theories contradicted in the inveſtigation of even their own experimental proofs? But will it not ſurpriſe my reader, when I tell him, that Dr. Harrington publiſhed his complete ſyſtem in the year 1780? That he kept it many years by him, before he had the reſolution to make it public; and, though ſince that time he has been elucidating and explaining it by different publications, yet the only reward he has received has been the greateſt inſults and neglect.

Let Mr. Lavoiſier and Dr. Crawford try, if they can poſſibly reconcile their theory of caloric with the above-mentioned experiment: Upon the acid and the alkali uniting, the fixed air, which the alkali poſſeſſes, attracts the caloric of the acid and alkali, and aſſumes an aerial form. This is their own explanation. That all the heat will be expelled is evident, from the acid uniting with the cauſtic alkali. All the heat then becomes ſenſible: And, agreeable to Dr. Crawford's theory, as the alkali

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appears now a body ſo phlogiſtic, it will expel the heat from the acid, the ſame as the volatile alkali would do: And certainly they muſt acknowledge that to be a phlogiſtic body. Then, as all the heat was expelled from this marine neutral ſalt, from whence came the heat which eſcaped from it, when it fulminated? without doubt from the alkali. The preſent chemiſts muſt be forced to acknowledge the truth, and the examination of the reſiduum is what will effectually do it. The alkali has diſappeared; the marine acid and water are left behind. Can we wiſh for proofs more clear and obvious? Had not the alkali diſappeared; in this caſe the modern chemiſts will, I hope, give me leave to aſſert, that it muſt have united with the marine acid, and ſo have ſtill formed a neutral ſalt in the reſiduum: Their caloric only having diſappeared. I flatter myſelf therefore, that this remnant of old chemiſtry is ſtill left, namely, that acids and alkalies attract each other, ſo as to form neutral ſalts.

It is evident that alkalies are ſalts formed of concentrated or fixed fire, and that in order to ſet that fire looſe, bodies which have the ſtrongeſt attraction for it, viz. the mineral acids, are required. The attraction for fixed fire, when united to the common marine acid, is not ſtrong enough; becauſe, in that ſtate, it is united to ſome fixed fire or phlogiſton. But take that away by the calx of lead, and then, by being robbed as it were of its own phlogiſton,

phlogiſton, its attraction for it becomes ſo ſtrong, that it will, of itſelf, penetrate into fixed fire of the alkali, break its bond of union, and ſet it on fire all at once with a conſiderable exploſion.

The common nitre is ſomething ſimilar to the factitious dephlogiſticated air of chemiſts: Indeed it is the body which forms the largeſt quantity of it. But neither the air nor the nitre will burn of themſelves, without the addition of a higher phlogiſtic body, as oil, coals, &c. the reaſon is, the acid has not ſtrength enough of itſelf. But when the nitrous acid is united to a higher phlogiſtic body, as the phlogiſton of metals, the oil of turpentine, or ſpirits of wine, it will then fulminate, fire being ſet to it, producing Dr. Prieſtley's inflammable nitrous dephlogiſticated air.

The concentration of fire, in atmoſpherical air, is ſomething ſimilar (as Dr. Harrington obſerves) to the concentration of it in alkalies, more than in oils, ſpirits, inflammable air, &c. but not altogether ſo ſtrong as in alkalies *. High phlogiſtic bodies, ſuch as inflammable air, or the volatile alkali, by being firſt expoſed to great heat, and then mixed with the nitrous vapour, will explode. This ſhews that what the acid wanted in ſtrength, is made

* In the metallic ſolutions of metals in acids, when expoſed to a long intenſe heat, metallic earths will form ſaline depoſits. This proves that they become ſaline, from imbibing the fire. See Prieſtley, vol. iv. p. 413, 489. Theſe depoſits, if chemically examined, will be found to have the characteriſtics of ſalts.

made up by the quantity of fixed fire. Many more examples could be brought, but they might be thought unneceſſary.

We could bring ten thouſand examples to prove that the chemical action of bodies can ſet looſe their phlogiſton: The moſt common in nature is by fermentation. But there are others which nature is ſuppoſed to make uſe of, viz. the chemical fermentation of ſulphur, iron and water. Many are of opinion that this fermentation, or chemical action, is often produced in the bowels of the earth, and is the cauſe of volcanos, earthquakes, &c. The violent action of theſe bodies with their phlogiſton, which is evidently turned into actual fire, ſeems adequate to the effect. When lead and mercury are ſhaken together, their chemical attraction, aided by their attrition, decompounds their phlogiſton into heat. In the operation, Dr. Prieſtley found, that he could ſcarcely touch them, the heat was ſo very intenſe.—See Prieſtley.

I need not have given myſelf ſo much trouble to contradict and confute theſe different extraordinary theories. It is, however, ſome ſatisfaction to hope that I have thrown great light upon the principles of combuſtion, founded upon Dr. Harrington's theory. For the overthrow of their other doctrines, I think I have an *experimentum crucis*. Mr. Lavoiſier founds his theory upon the oxygen gas being imbibed into ſulphur and phoſphorus, in combuſtion. But (as our fathers in chemiſtry have judiciouſly

judiciouſly taught us) theſe bodies conſiſt of acids and phlogiſton.

If the marine dephlogiſticated acid is mixed with eſſential oils, or with volatile alkalies, and fired in artificial dephlogiſticated air †. The intenſeneſs of the heat will decompound the dephlogiſticated air, and cauſe it to be principally imbibed by the acids and phlogiſtic bodies, the ſame as in the combuſtion of ſulphur. And, if the reſiduum is examined, the phlogiſton will be found to have diſappeared, forming actual fire, and the acids are left entire. What farther confirms the doctrine of phlogiſton, is this; the marine acid having loſt its dephlogiſticated ſtate, is left in the ſtate of common marine acid, though it had imbibed, in the proceſs of combuſtion, a quantity of oxygen gas.

Chemiſtry has of late ſuffered very great innovations; and, if our modern philoſophical chemiſts go on as they have done, it is not known where they will ſtop. To me it has often been matter of ſurpriſe, that none of the old regular chemiſts have borne their teſtimony againſt ſuch degradations. They all ſeem to be in a lethargic ſtate, as if bitten by the *tarantula*.

One of the firſt principles of chemiſtry is that of chemical attractions; and our anceſtors in

† What I mean by artificial dephlogiſticated air, is that air which is made in the laboratory of chemiſts. For Dr. Harrington ſhows that this air is very different from the empyreal part of atmoſpherical air.

in this ſcience perſuaded, that theſe attractions are one of the moſt ſingular phenomena in nature, have handed down to us a regular table of them. Particular bodies have a very ſtrong attraction for each other, very different from the principles of gravitation. If two bodies, having an attraction for one another, are expoſed to each other's influence in the medium of a fluid, they will coaleſce. Thus, if an acid and alkali are dropt into water, they will form a neutral ſalt: So alſo, whenever bodies are expoſed to each other in a fluid or moiſt form, their attraction will take place. Fluidity ſeems to be neceſſary only to bring their particles into immediate contact.

But how are our preſent chemical attractions conducted by combuſtion? Combuſtion inſtead of allowing bodies to unite, acts as a ſtrong mechanical power in breaking them down. On the contrary, according to their chemical attractions, combuſtion (which is the ſetting looſe an eſſential part of moſt chemical bodies, viz. phlogiſton or concentrated fire, let looſe as actual fire) unites, but does not ſeparate them.

Let us here give an example. I will take a ſalt, as it is a body, (the chemical principles of which we have been fully inſtructed in by our forefathers) and alſo an acid, with which we are well acquainted: Though this knowledge has been much obſcured by ſome late chemiſts. If the volatile alkali is aerilized into an air or vapour, and after that expoſed to the vapour of

the nitrous or marine acid, they will unite, ſo as to form the nitrous or marine ammoniac ſalt; and the heat which kept them in an aerial ſtate becomes ſenſible. But if this alkaline air is expoſed to ſtill greater heat, it will then become inflammable air; and if the ſame nitrous vapour is added to this air, as in the former experiment, and then fired, it will emit an immenſe quantity of heat and light; even fifty times more conſiderable, than came from it in the former experiment; and upon examining the reſiduum, an acid water is found. Here the volatile ſalt has diſappeared, and fire appeared, or was diſcharged in the operation. Indeed, in the former experiment no light comes from it, but only a ſmall quantity of heat. Dr. Prieſtley ſays, vol. iii. p. 415. "Very little heat is produced by the union of acid or alkaline air and water, though, as I have found by experiment, there is *ſome* produced, whereas the decompoſiton of dephlogiſticated and inflammable air never fails to produce a very great degree of heat."

Then thoſe who may be called our fathers in chemiſtry were convinced that the acid and volatile ſalt, by being united by a chemical attraction, would form a neutral ſalt, is now perfectly abſurd; the true attraction does not take place till the combuſtion. And the true union of the alkali and acid is not a neutral ſalt, but water and an acid. Such abſurd doctrines need no comment.

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The very ſame farce has been acted in the experiment of firing the oxygen and inflammable gas. Water and an acid are in the reſiduum. The inflammable air, and alſo the fire, which neutralized the acid of the oxygen gas, have diſappeared, as has been long ago fully proved by Dr. Harrington. The diſappearing of the inflammable and dephlogiſticated air, an acid and water being left in the reſiduum, is (as Drs. Prieſtley and Harrington found) the very ſame reſiduum as that which is left, when the nitrous vapour and inflammable alkaline air are fired. In one caſe our aerial philoſophers cannot deny but that the acid which is left, came from the nitrous vapour and the water; heat and light, from the inflammable alkaline air. Therefore, by the ſame argument, we muſt equally allow, that the acid in the other experiment, came from the dephlogiſticated air, and part of the water from the inflammable air. Nothing can be more plain and obvious. Yet our modern chemiſts will not allow theſe firſt and moſt common principles to be juſt ones; namely, that alkalies attract acids and neutralize them, and that phlogiſton neutralizes acids *.

If the neutral ſalt, which is formed by the marine dephlogiſticated acid, and the fixed

* Dr. Crawford in his explanation of the nitrous acid deflagrating with combuſtible bodies, ſays, that the fire firſt ſet looſe in the firing them, is the cauſe of this. And agreeably to him, this fire runs the gauntlet, of alternately aerilizing the oxygen gas of the acid, and of being conſumed.

alkali are ſet on fire; as neither of theſe bodies are aerilized in the proceſs, ſo there is leſs myſtery in their operation. Before combuſtion they formed a neutral ſalt; but after the combuſtion the alkali has diſappeared. What has become of it? We are altogether at a loſs to anſwer this queſtion according to their theory, unleſs they could introduce the art of *legerdemain* into their experiments of glaſſes, gun-barrels, &c. But here the aid of conjuration is ſtill farther required; for, as one *demon* diſappears, another in the ſhape of an immenſe quantity of light and heat makes his appearance. However, as the tricks of the juggler ceaſe to ſurpriſe us when his art is known, ſo, had we not been ſo eaſy of belief and ſo fond of the marvellous, we would have paid a little more reſpect to what our chemical fathers have taught us; nay, I had almoſt ſaid, what we may be convinced of by our own ſenſes. It is hardly poſſible not to give our aſſent to this truth, namely, that combuſtible bodies, or phlogiſton when fired, are changed into actual heat. By a proper attention to chemical principles, we ſhould have believed, that phlogiſton is concentrated or fixed fire. Yet it often happens, that as new diſcoveries pleaſe, merely becauſe they are new, and every one is anxious to aſcribe all phenomena

ed. To ſuch ridiculous explanations are they obliged to have recourſe to in ſupport of a wrong theory. I think that ſuch powerful phenomena is to be accounted for by ſuch ſlight cauſes.

mena to them, ſo, upon the diſcovery of Dr. Black's elegant doctrine of latent heat, (which becomes latent in fluids, vapour, &c.) our modern chemiſts greedily graſped at it, and forced it head and ſhoulders into their preſent aerial fabric. But Dr. Harrington hath ſatisfactorily ſhown, that the heat in theſe phenomena is chemically united, the ſame as in alkalies and acids.

The idea and proof of heat or fire being chemically attracted by water, juſt as alkalies attract acids, (not a vague idea of its becoming latent) was the diſcovery of Dr. Harrington. And I take this opportunity to declare, that this, with many more of that gentleman's diſcoveries, have been mentioned by his enemies, without ever having glanced at, or even ſuggeſted his name. A behaviour in them ſo uncandid and ungenerous, impels me to make uſe of this pointed language, and to do all in my power to redreſs injured merit. I meant to have drawn a whole length picture of the unkind uſage he has received, and of the unjuſt manner in which his diſcoveries have been treated, and with the plagiariſms of others from him. This however I am prevented from doing, as he informs me that he has been making obſervations for ſometime, which he intends to publiſh. His enemies have acted a very unfair part; every one of them thought they had a right to buffet and pilfer from him at the ſame time. Though I could all along clearly obſerve, that his writings were regularly

larly operating upon the different chemical opinions; and that they are upon the eve of bringing about a general revolution in the chemical ſyſtem. Great is the power of Truth, and ſhe will prevail!

The preſent doctrines adopted by many chemiſts appear, when fairly inveſtigated, to be ſo very abſurd, that it is not poſſible to vindicate them. Heat (as Dr. Harrington obſerves) muſt be matter; unleſs we adopt Plato's opinion, who ſuppoſed it to be ſpirit. Then, (as in their experiments) we have clearly proved, that an immenſe quantity of heat and light comes from the combuſtion of the muriatic neutral ſalt, which could not poſſibly have been lying latent, as they call it, in the neutral ſalts, but muſt have been an integral part of the alkali: Beſides this would be hoſtile to Dr. Black's doctrine, which is the baſis of all the late doctrines of chemiſtry. For, in their experiments, a great generation of air and vapours inſtead of a condenſation takes place; ſo that, as I obſerved before, were the preſent chemical doctrines brought in proof of a generation of cold inſtead of heat and light, they would have had ſome appearance of truth. And, let me aſk this ſerious queſtion: If an amazing quantity of matter comes from a body in the form of heat and light, is it not conſiſtant with reaſon to ſuppoſe, that the light and the heat made a part of the body; and that, upon its being diſcharged, the body would be altered in its chemical principles?

To

To prove theſe principles, we can bring experiments, and ſet them in ſo clear a point of view, that we ſhould think it impoſſible for any to miſtake them. We have ſhown that the diſtinction between the dephlogiſticated marine acid, and the common acid, is owing to the one being phlogiſticated, and the other dephlogiſticated. But to avoid diſputation, let us ſuppoſe the difference to be conformable to Mr. Lavoiſier's doctrine, the one poſſeſſing more of the oxygen gas than the other. And, with reſpect to the formation of alkaline air into inflammable, Dr. Auſtin has written a long paper in the Philoſophical Tranſactions, in which he endeavours to ſhow that it is performed by ſeparating its inflammable from its phlogiſticated air: It being the ſuppoſition of chemiſts, that the alkaline air is formed of theſe two bodies.

Then, to make experiments with all poſſible candour: Take the common marine acid air, and mix it with alkaline air, and they will form the common ſal ammoniac: Whereas, take the dephlogiſticated marine acid air, and fire it with alkaline air, and they will form, after the combuſtion, a watery marine acid. In the one operation, a great quantity of heat and light came from it, but in the other, comparatively little or no heat, and no light. This being ſo, our modern interpreters muſt ſay, that there was no chemical attraction in the firſt experiment, viz. the union of the common marine acid and alkali; but that it took

place

place only in the ſecond. Moreover, we raſhly venture to correct the theories of our fathers, when we ſay, that alkalies and acids do not form neutral ſalts, but a watery acid. Here however we are ſtill aground; for, in the proceſs which left the reſiduum of an acid and water, there is an immenſe quantity of fire and light to be accounted for. You ſee, gentlemen, into what abſurdities your own doctrines have involved you.

Dr. Auſtin in his laboured and very ſingular paper, built upon theſe hypotheſes, ſuppoſes that when light, inflammable and phlogiſticated air are fired in their compound ſtate, as in alkaline air, they would form fixed air. In this caſe then, conformable to his hypotheſis, alkaline air, and dephlogiſticated marine acid, when fired, ſhould form fixed air; but they do not.

That concentrated fire is phlogiſton, may be proved, both analytically and ſynthetically. Dr. Harrington, in his Thoughts on Air, has proved the ſynthetic part. Of the many arguments he has brought, I would beg leave juſt to mention ſome of them *. As to the analytical,

* Dr. Prieſtley impregnated water with the vitriolic acid vapour, and expoſed it in a glaſs tube to a continued heat in a ſand furnace from the 9th of Sept. to the 20th of Jan. and the reſult was, as he ſays, more curious than he could poſſibly have expected, *a priori*. For the vitriolic acid had actually formed itſelf into ſulphur, and ſome part approaching to it in the form of white cryſtallizations. "The cryſtals, (he ſays) I found were not diſſolved in ſpirit of ſalt, and when

tical, we have, I hope, ſufficiently proved it. Dr. Prieſtley found that heat would make the calces of metals become ſaline bodies; Dr. Harrington found that heat would make alkaline air inflammable; Mr. Scheele found that

when they had been waſhed and dried they had the colour and ſmell of ſulphur; and being laid on a hot iron burned with a blue flame ſo as to leave no doubt of the identity of the ſubſtances." I make no doubt but if the operation had been long enough continued that all the acid would have been converted into theſe cryſtals of ſulphur; for that which was left was particularly pungent, giving indication to have likewiſe received a greater quantity of phlogiſton. I have by a longer proceſs and greater heat entirely neutralized the acid, it being all in a manner changed into cryſtals. Dr. Prieſtley expoſed the vitriolic vapour or air to this heat, and found the ſame cryſtals; and upon opening the tube one third of the air was gone, which no doubt had entered into the cryſtals (forming them with the heat.) But part of the air which remained would not unite with the water; therefore it implied that it had got a ſtronger impregnation of phlogiſton. We cannot entertain the leaſt doubt but that the acid had obtained phlogiſton in theſe experiments, and it is equally clear that there was nothing to give it phlogiſton but the fire. Mr. Scheele, p. 116. ſays, "Do we not obſerve that the vitriolic acid united with a ſmall portion of phlogiſton forms ſpirit of ſulphur, and with ſtill a greater quantity, ſulphur?" P. 115. he likewiſe ſays, "Heat is united with certain ſubſtances, and makes one of their integrant parts." The nitrous and marine acids treated in the ſame manner form a white incruſtation, which I have no doubt ariſes from the acid uniting with the heat, and forming a neutral union. The marine acid is ſuppoſed to corrode the glaſs, which may add to the incruſtation; but the experiment which I think the moſt concluſive is this. I took the phoſphoric acid, and expoſed it for a long time to a ſtrong heat, not by itſelf in the manner Dr. Prieſtley did, but diluted with water, and which will then even bear a red heat, as Mr. Scheele obſerves. After a long expoſure it formed a number of cryſtals, all the

heat would make fixed oils volatile †. And Homberg found that the heat of the ſun would make fixed alkaline ſalts volatile.

Gentlemen, for the ſake of ſcience liſten to to the voice of Truth, for though you have hitherto wielded the chemical ſceptre with undiſturbed ſway, to point out your miſtakes ſhould not offend you, when done with candour, and to ſerve the cauſe of ſcience. By being unwilling that your errors in chemiſtry ſhould be rectified, is to endanger the loſs of that character you merit from your labours.

To

acid being imbibed by them, and which upon examination appeared to be phoſphorus, having all its qualities in ſome degree burning with that bright flame, and leaving an orange coloured ſtain; therefore we can have no doubt of the manner in which the phoſphorus had regained the phlogiſton, which it had before loſt, by conſuming in the open air. The cryſtals were ſo perfectly neutralized that they did not give the leaſt acidity to water ſo as to have any effect in turning the juice of turn-ſole red. Dr. Prieſtley found, that by expoſing common vinegar to a long heat it became black and vapid, loſing its acidity; both of which changes undoubtedly imply its neutralized heat forming it into phlogiſton.

† Subſtances which are united with a redundant heat, as alkalies, quick-lime, and litharge, attract the inflammable which is found in oily mixtures: They diſſolve the fat oils and ſulphur, and yield with it ſoaps. If you pour an acid to a ſaponaceous ſolution, the acid unites with the alkali, and ſets the heat free; and ſince it becomes not ſenſible, it muſt undergo ſome new union; and meeting for that purpoſe with the oil, it unites with it; and the oil acquires by it the peculiar quality of becoming ſoluble in conſiderable quantity in ſpirit of wine, and to form with volatile ſpirits ſal ammoniac prepared with quick-lime a peculiar kind of ſoap: Qualities which the fat oils likewiſe acquire after being diſtilled ſeveral times, when they likewiſe muſt have attracted heat from the fire. See Scheele, p. 174.

To ſay that your aſſent to the Harringtonian doctrine would confirm me the more in it, would be to ſay an untruth; for it would ſhow great ignorance in me, when the two doctrines came to be fairly examined and compared together, to entertain the leaſt doubt or heſitation, which is the true one.

Dr. Harrington ſhows, in the firſt publication of his theory, great penetration and a juſt knowledge of the world; for knowing the high characters he had to contend with, he ſays, "How difficult is it to eſtabliſh truths againſt the prejudices of the day?" But he does not ſtand alone, as he has for his example the immortal Harvey, with reſpect to his diſcovery of the circulation of the blood. Though he gave ſo clear and plain a demonſtration of his doctrine, that even a child's reaſon muſt have aſſented to it; yet his medical brethren cavelled at him with all their ſpleen and vehemence: And when they were forced to allow that he was right, they endeavoured to leſſen his merit, by ſaying he was not the firſt diſcoverer.

But gentlemen, I have another experiment for you, which is, in my opinion, almoſt equal to Harvey's demonſtration of the circulation of the blood. Mr. Kirwan ſays in his Eſſay, p. 31. "If ſulphur be digeſted in oil of turpentine, and then ſlowly diſtilled for 10 or 12 days, it will be converted into vitriolic acid, according to Homberg. Mem. Par. 1703. Here it appears that the ſulphur is firſt dephlogiſticated, and then unites to the fixed air of the oil: It

evidently can receive no pure air from it. It muſt be remembered, that if this experiment be not cautiouſly conducted, it is very dangerous."

Here it appears that two high phlogiſtic bodies, by the action of heat, without either dephlogiſticated air or acids, will have their phlogiſton turned into actual heat. The oils, and likewiſe the phlogiſton of the ſulphur, are conſumed, ſo as to form actual heat, there being a ſlow and gradual combuſtion. But, if the operator is not very cautious, he will be made ſenſible of the combuſtion, by the exploding of the veſſels.

It is ſurpriſing to ſee the moſt clear and obvious facts wreſted by their hypotheſes. Mr. Kirwan ſays it is by the fixed air uniting with the ſulphur, the ſulphur being firſt dephlogiſticated. Now, if we allow that the oil contains fixed air, what was to decompound it, what to dephlogiſticate the ſulphur, and what became of all the phlogiſton? It ſurely muſt have acted the part of an inviſible ſpirit. But by attending accurately to the experiment, you may ſenſibly perceive a ſlow combuſtion, with a ſeparation of heat and light.

I cannot help obſerving, that our preſent chemiſts can, with the greateſt facility, get over the moſt obviouſly plain facts, ſo as to preſs them into the ſervice of their hypotheſes; and certain I am, that when the truth comes to be known, chemiſts will then ſmile at their preſent wrong doctrines, and modes of reaſoning.

Chemiſts

Chemiſts ſeem to poſſeſs a credulity that will receive any thing as true.

But as a farther confirmation of our doctrine, we will give an experiment more conformable to Mr. Lavoiſier's. If dephlogiſticated marine air is mixed with pure or reſpirable air, there will be a decompoſition; and the marine acid, inſtead of now becoming more dephlogiſticated, will become phlogiſticated, or the common marine acid.

The hypotheſes at preſent adopted by many chemiſts, have no regular table of attractions. They ſay, that if in ſome of their proceſſes there is the leaſt water, nitrous phlogiſticated or fixed air, theſe bodies, under an intenſe degree of heat or cold, muſt be decompounded or compounded, and that at their pleaſure; conſequently their experiments are nothing, in point of chemical attraction, but a ſtring of contradictions. Their leading elective attraction, is the oxygen and inflammable gas, or phlogiſton. Theſe two bodies, they ſay, have, in reſpiration, combuſtion, putrifaction, &c. the ſtrongeſt attraction for each other.

All bodies which attract each other, when brought within the ſphere of one another's attraction, unite when in a fluid ſtate. But will not my reader be ſurpriſed when I inform him that the oxygen and inflammable gas, or *carbone*, ſo far from attracting each other, will not unite, when brought into a fluid ſtate: The repeated trials made by chemiſts have not been able to accompliſh it. Nay, the ſtrong mechanical

chanical compreſſion of the two gaſs by heat, and being long mixed together, is ineffectual; and yet they are fluids: Nor will it do, as in metals and oxygen gas, though the phlogiſton and the oxygen fluid are condenſed.

How comes it then that theſe ſhould not unite, ſince chemiſts ſay, that it is by this attraction nature conducts all her operations? Nay, what is ſtill more extraordinary; atmoſpherical air, though compoſed of dephlogiſticated, and another kind of air, which (according to chemiſts) is ſuperſaturated with phlogiſton, will not (as Dr. Harrington obſerves) attract each other, nor unite; and yet they are not only blown from pole to pole, but even generated together in vegetables. Mr. Kirwan endeavours to reconcile this extraordinary abſurdity, (for I can call it nothing elſe) by ſaying that the elementary heat of theſe airs prevents their union. The facts are in direct contradiction to this; for, dephlogiſticated and nitrous air unite, and ſo do fixed and marine acid air unite with alkaline air. And beſides theſe, many other examples might be adduced. He ſays farther, that for the ſame reaſon, fixed air will not unite with lime without the aid of water; and alſo, that concentrated oil of vitriol may be found on water, without uniting with it.

As to the averſeneſs of fixed air to unite with lime, Dr. Harrington hath accounted for it, and brought a ſtrong fact to prove that the fire of the lime is chemically united, and has a ſtronger

a ſtronger attraction for the lime, than the lime has for fixed air; but by the joint action of water, and fixed air on the earth of the lime, the fire's attraction is overcome. And as to the oil of vitriol, there is an oilineſs, or repulſive quality, which checks the union, but that is overcome by the leaſt motion: Therefore neither of theſe examples are in point.

It is ſaid that Dr. Prieſtley made dephlogiſticated and inflammable air unite, by means of a bladder forming fixed air. It is really matter of ſurpriſe and vexation to think, that ſuch experiments ſhould be once named, after what Dr. Harrington has ſaid upon this ſubject: For, in the firſt place, theſe two kinds of air, upon their union, form either water or nitrous acid; and yet, in this caſe, they are ſaid to form fixed air. Again, Dr. Prieſtley himſelf has acknowledged, that the bladder became putrid in the operation: And do not we all know, that animal putrifaction will turn dephlogiſticated into fixed air. The inflammable air (conformable to Dr. Harrington) will aid the putreſcency; and it is a fact not to be diſputed, that bodies become putrid by attracting phlogiſton; and by receiving the phlogiſton of pure air they decompound it, ſo as to form it into fixed air *. Then, gentlemen, the only union between dephlogiſticated and inflammable

* Why ſhould a bladder form this union, if it were not by decompounding the air by putrifaction? For Dr. Prieſtley has endeavoured to unite them by every mechanical influence he

inflammable air, is effected by combuſtion, which indiſputably acts, ſo as to ſet looſe the concentrated or fixed fire of theſe two kinds of air: For, in all caſes of bodies burning, their phlogiſton turns into heat and light: And were there, as chemiſts would make us believe, ſo ſtrong an elective attraction, we would certainly be able to accompliſh it without the aid of burning: But this is poſitively nothing more than ſetting looſe the phlogiſton as actual fire. Of this we have a clear and poſitive proof in the neutral ſalt, which is formed of the dephlogiſticated marine acid and an alkali. Let me aſk chemiſts, are they not chemically united, and is there not a chemical attraction? According to their doctrine, there is not.

Then let us bring in their favourite agent to produce attraction, viz. combuſtion. After combuſtion, the neutral ſalt has diſappeared, and the reſiduum is an acid and water with an aerial expanſion. Our fathers in chemiſtry, how egregiouſly were they miſtaken! They were totally ignorant of chemical attraction. They ſuppoſed that an acid and an alkali would unite, and form a neutral ſalt. They knew nothing of combuſtion, our great chemical agent. They were ſuch *ignoramuſes* as to ſuppoſe that a houſe is built by putting brick and mortar together. Our preſent aerial philoſophers

he could think of; either by force, or *by every ſingular chemical idea*, which is, to admit their union by ſmall quantities at a time; and this he calls their naſcent ſtate. Very extraordinary chemical ideas indeed!

loſophers are much more knowing, they employ their agent, combuſtion, after the houſe is built. Hence, gentlemen, my fears are, that your agent, combuſtion, will become ſo general, as at laſt to blow up your doctrines. It is a dangerous thing to be too familiar with fire; therefore beware of the exploſion.

According to Mr. Lavoiſier's hypotheſis, there is a very ſingular difference in the production of fixed from dephlogiſticated air, and when water is produced from this air: The one being produced from *carbone*, and the other from inflammable air. But Dr. Harrington hath ſatisfactorily proved, that the difference in their production, is owing to the intenſeneſs of the flame. That the nitrous acid and water are produced when it is ſtrong; and when weak, fixed air.

With reſpect to the combuſtion of inflammable air, ſulphur, phoſphorus, &c. Mr. Lavoiſier hath given ſome accurate experiments, which prove the heat to be very conſiderable; but the heat, when fixed air is produced, is conſiderably weakened in theſe combuſtions. The difference is, indeed, very great; but it ſeems evident that that muſt be owing to the intenſeneſs of the combuſtion, and not to the particular ingredients burnt; as the ſame bodies burning produce the different reſiduums. In burning inflammable and oxygen gas, the fire is ſo intenſe, as to burn inſtantaneouſly, going off with a loud exploſion; but when theſe two bodies burn in a

ſlow and gentle manner, they form only fixed air: To prove this, Dr. Prieſtley has given many experiments, and indeed, the proofs from them are very good ones.

But then, that his experiments may correſpond with his very ſingular doctrines, he ſuppoſes that the fixed air is produced from the dephlogiſticated air, by imbibing inflammable air in its naſcent ſtate, as he expreſſes it. To make the experiment, expoſe zinc, iron, red precipitate, turbith mineral, to fire in earthen retorts: And the proceſs is ſimply this; the combuſtion takes place in a gentle manner, as the dephlogiſticated and inflammable airs are generating; for, as the heat produces theſe airs, it will in conſequence ignite them. That it is from this ſimple cauſe, is beyond all doubt; for, if you take care not to raiſe the heat ſo high as to ignite the airs, you will receive them both entire. Dr. Prieſtley was baffled in many of theſe experiments, owing to their firing with an exploſion, after a quantity had been generated: However, he often produced them quite ſeparate; which he could not have done, had there been (as modern chemiſts would have us believe) great attraction between the two airs *. Now, theſe experiments

* Again, I threw the focus of the lens upon red precipitate, in alkaline air, till three meaſures of it were reduced to two. Water was produced in the proceſs, and the air that remained was conſiderably dephlogiſticated; the ſtandard of it, with a mixture of two equal meaſures of nitrous air, being 1.7.

But

ments are in direct oppoſition to Mr. Lavoiſier's doctrine: For here is fixed air generated, when water only ought to have been generated, and there was poſitively no *carbone* ‡.

Nothing can more clearly eſtabliſh our hypotheſis, namely, that the quality of the reſiduum, after burning theſe airs, depends upon the intenſeneſs and degree of the combuſtion;

But in another experiment of this kind, there muſt have been a quantity of inflammable air ſet looſe from the alkaline air, as well as dephlogiſticated air from the red precipitate; becauſe, after the operation had continued ſome time, there was a violent exploſion within the veſſel; which threw it many feet perpendicularly into the open air, as I was holding it in my hands. In this experiment I had been particularly careful to make every thing concerned in it as dry as I poſſibly could, in order to ſatisfy myſelf with reſpect to the production of the *water* which I had found before; and there was time enough before the exploſion to obſerve that water was certainly produced in the proceſs. To appearance the the quantity of air was never much diminiſhed. See Prieſtley, vol. vi. p. 194.

‡ Dr. Prieſtley, in order to prove that phlogiſton and empyreal air, form fixed air, put bodies into a retort, which contained both empyreal and inflammable air, viz. red precipitate and iron, and he got a quantity of fixed air from them; from thence he concluded, that theſe two airs form fixed air. But the explanation of the phenomenon is this: The red precipitate, at firſt, yields its pure air, and the heat that the iron poſſeſſes at the time, with the action of the pure air upon it, ſets it on fire, and there is an actual combuſtion. We need not be ſurpriſed at this, ſince the iron will burn under the ſame circumſtances, out of the retort, viz. being expoſed to pure air and a ſtrong heat. The combuſtion in the retort is done in an imperfect manner; the pure air being generated ſo ſlowly, as not to produce that active ignition, as the fixed air to be decompounded, and be fixed by the calx of the iron. This is the caſe in other imperfect combuſtions,

for if a column of theſe two airs goes off inſtantaneouſly, here all the fire which theſe two airs poſſeſſed, acts at one and the ſame time, and muſt produce a conſiderable quantity of heat. But in thoſe proceſſes, where fixed air is formed, it burns in a gentle and gradual manner. What ſhows this in a moſt ſtriking light is, if the inflammable air from the wood is fired with reſpirable air, it burns more gradually, ſo as to produce fixed air. Yet, if this ſame inflammable air has a greater quantity of fire thrown into it by the electric ſpark, or common fire, it will explode all at once, juſt as the inflammable air from metals does, and produce the nitrous acid.

And

combuſtions. Mr. Kirwan ſays, p. 85. "Mr. Morveau having left a piece of phoſphorus in a large glaſs well ſtopped "for 3 or 4 days, expoſed to a temperature of 70 or 72°, "and afterwards opened it in lime-water, the lime-water "entered and became turbid, and being filtered, left a precipitate which effervefced with the nitrous acid, and conſequently the precipitation did not ariſe from the union of "the lime with the phoſphoric acid. 1 Encyclop. p. 220."

Here, in both theſe caſes, fixed air appears; but when there is a free ignition, the fixed air is decompounded, and enters into the calx in one caſe, and the phoſphoric acid in the other, (the proceſſes are directly the ſame.) But in experiments where the two metals have had a more active proceſs, there has been, I have found, little fixed air generated, and it was found in the calx. And to prove that it is not by the union of theſe two airs (viz. inflammable and empyreal airs) in their nafcent ſtate, that they form fixed air, but from the combuſtion, I took zinc and the red precipitate, and with a gentle fire, ſo as not to produce ignition, I have formed theſe airs quite ſeparate and pure. Or by adding water, either to the red precipitate and iron, or the precipitate and zinc, it will hinder the ignition, and in conſequence, their bodies will produce empyreal and inflammable air.

And now I would ſeriouſly aſk my reader, what is his opinion of that doctrine, which ſuppoſes, that ſuch a body as phlogiſton, can with dephlogiſticated air, form fixed air, which is an acid? I ſay, with deplogiſticated air, which ſhows no ſigns of acidity.

But if he (my reader) is ſo bewildered in the labyrinth of aerial philoſophy, as not to know whether phlogiſton is an acid or an alkali, I would beg leave to inform him, (and if he tries he will find it to be true) that alkalies, by means of more heat, may be formed into inflammable air; that the common alkali will equally explode with the dephlogiſticated marine acid; and that oils, bitumens, &c. neutralize acids. If he wiſhes to have the authority of ſome eminent chemiſts, I will give him one of the firſt rank; Mr. Scheele ſays, It is an indiſputable fact, that phlogiſton neutralizes acids: And be it alſo known, that fixed air with water, expoſed to the rays of the ſun, will attract them, and loſe its acidity, becoming pure air again. Nay, will not his ſurpriſe ſtill increaſe when I inform him, that when this pure air is ſuppoſed to imbibe more alkaline air, the combuſtion being ſtronger, it will turn to the nitrous acid.

Let us conſider theſe wonderful productions. Fixed air with water, by being expoſed to the ſun, becomes pure air; and by attracting a quantity of alkali or phlogiſton, it becomes fixed air; or, as Mr. Bergman calls it, an aerial acid: And, by a greater quantity of the alkali,

it

it becomes the nitrous acid. If the dephlogiſticated nitrous acid is made uſe of in theſe combuſtions, the ſingular changes produced by its being fired in alkaline air, muſt appear ſtill more extraordinary. We ſhall afterwards ſhew, that this doctrine is equally as abſurd when conſidered in another point of view; for we will prove in the moſt evident manner, that this pure air, inſtead of receiving any addition in the combuſtion, really loſes; as it becomes leſs heavy, and more contracted in its bulk.

That the rays of the ſun or heat will phlogiſticate acid vapour, is a truth ſo well known, and ſo properly eſtabliſhed, that I need not enlarge upon it here. Dephlogiſticated air is ſuppoſed to be the acidifying principle, and ſulphur and phoſphorus the ſubſtances which imbibe it. In combuſtion they become ſtrong acids; and they are ſuppoſed to imbibe only as much of dephlogiſticated air, as is equal to their own weight. Metals imbibe in their reduction, a ſmaller quantity of dephlogiſticated air; and therefore, he ſuppoſes from that, that oxyds are not highly aceſent: But there appears to be an evident deficiency in this doctrine; for, when dephlogiſticated air is formed into fixed air, it muſt be all formed of air, or of the acidifying principle; for the fixed air is not equal to the weight of the empyreal air changed. This is the caſe in the combuſtion of all bodies, except charcoal, in the oxygen gas; and in that combuſtion, the fixed air which is formed is heavier than the empyreal air,

air, viz. 1-9 conformable to Mr. Lavoiſier, and 1-5 conformable to Dr. Crawford: But conformable to them all, it is evident, that in the combuſtion of other bodies, the fixed air is lighter than the empyreal air.

This is a direct contradiction; for it is evident here, that it is much more probable to ſuppoſe, that the ſulphur, phoſphorus and carbone, are the acidifying principles, and not the oxygen gas; ſince the more of them is united with the oxygen gas, the more acid is the body; then the acid of fixed air is not to be compared in point of acidity with the vitriolic and phoſphoric acids, and yet it ſhould, conformable to their ſyſtem †. They ſay, That oxygen gas is the principle of acidity; conſequently, the acid of fixed air ſhould be twice as ſtrong as the vitriolic, ſince it contains twice the quantity of oxygen gas. But this is one of their many inconſiſtencies, which they can leap over with the greateſt eaſe. Had Mr. Lavoiſier paid a proper attention to the importance of theſe contradictions to his theory, he muſt have been convinced of its impropriety. In burning the variety of bodies, in nature, both of the animal and vegetable kingdoms, in oxygen gas, it is left in ſuch a ſtate, as to have both its diameter and weight decreaſed: And yet, moſt bodies in nature are ſuppoſed to abound

† And again, if the acidity of bodies depended upon the baſe which imbibed the oxygen gas, and not the oxygen gas itſelf, the oxyds of metals ought to be more acid, than even the acid of ſulphur or phoſphorus.

abound with fixed air; ſo that Mr. Cavendiſh endeavours to prove that the fixed air comes wholly from the body burned, and not from the oxygen gas.

The only body which, when burned, leaves a greater quantity of fixed, than the bulk of the empyreal air, is a factitious chemical body, made by art, called charcoal. And I would inform my reader, that this ſame charcoal, by being heated in a cloſe veſſel, and with the vapour of water, will give out a great quantity of fixed air. Mr. Lavoiſier ſays, p. 86. "When the diſengaged gaſſes are carefully examined, they are found to weigh 113.7 *grs.*; theſe are of two kinds, viz. 144 cubic inches of carbonic acid gas, weighing 100 *grs.* and 380 cubical inches of a very light gas, weighing only 13.7 *grs.* which takes fire when in contact with air, by the approach of a lighted body." Nay, there is no chemiſt but may know that charcoal contains a great quantity of fixed air: However, if his aerial experiments are ſo myſterious, as to have led him and others into errors, we ſhall give other examples ſo obviouſly plain, that it will be impoſſible to miſtake them. The immortal Scheele ſays, p. 182. "The charcoal is moſt proper for the purpoſe, ſince it is a ſulphur compounded of phlogiſton and aerial acid. If coals be ground together with alkali, made cauſtic by quick-lime or fire, and then diſtilled in a glaſs retort in an open fire, a great quantity of inflammable air is thus obtained, containing no aerial acid: If a bladder be

be tied to the mouth, the alkali on the other hand loſes its cauſticity and effervceſces with acids *."

If other phlogiſtic bodies, even thoſe which Mr. Lavoiſier ſays are compoſed of *carbone*, will not, when burned with nitre, leave the alkali in a mild ſtate; then theſe two eſſential points are ſufficiently confirmed. Firſt, That the fixed air reſides in the charcoal before the operation; and ſecondly, That fixed air is not produced in combuſtion, by the oxygen gas of the nitrous acid uniting itſelf to the *carbone* of the combuſtible body: For, if that were the caſe, any combuſtible body, whoſe combuſtion would change pure atmoſpherical air into fixed air, ought, when deflagrated with the nitre, to leave the alkali in a mild ſtate. Therefore, this ſimple experiment alone, viz. the deflagrating nitre with charcoal and other combuſtible bodies, ſhould clearly convince Mr.

* In the deflagration of nitre with charcoal, the coal is totally and rapidly decompounded. Its phlogiſton and mephitic acid are ſuddenly let looſe from their earthy baſis, and from each other. The phlogiſton combines with the nitrous acid, and inſtantly flies off with it in the form of *nitrous*, perhaps mixed with *atmoſpherical air*. Under this modification it had hitherto, particularly in the proceſs for the *clyſſus* of nitre, eſcaped the inquiries of the chemiſts, till you detected it in the form of *air*. While the phlogiſton of the charcoal is thus let looſe, its whole maſs of fixed air, or *mephitic acid*, being likewiſe ſuddenly diſengaged, *but ſtill continuing in its fixed ſtate, or that of a concentrated acid*, ruſhes into the alkali deſerted by the nitrous acid, and occupies its place. With it, it conſtitutes that *ſemi-neutral* ſalt, or that combination of alkali and mephitic acid, denominated Mild Alkali. See Dewly in Prieſtley's Appendix, vol. III. p. 391.

Lavoiſier and others, that the nitrous acid is not decompounded in the combuſtion.

The proceſs is ſimply this; the nitrous acid promotes the combuſtion by aſſiſting the fire to penetrate and ſet looſe the phlogiſton; for, as Mr. Cavendiſh obſerves, it gets united to a part of it, ſo as to form phlogiſticated air. See p. 38 of this Treatiſe. Then, have we not a right to ſuppoſe that part of the fixed air in the combuſtion of the charcoal, comes from the charcoal itſelf, as it evidently poſſeſſes fixed air?

Were we even to allow Mr. Lavoiſier's explanation of the manner in which fixed air is formed, when water is added to charcoal; yet it will not, I ſuppoſe, be contradicted, that in burning bodies in empyreal air, there is a ſmall quantity of water which is depoſited by the oxygen gas in the act of combuſtion. And Dr. Prieſtley has proved by experiments, that he found the attraction of charcoal for water, when heated, to be ſo great, as even to ſteal it from the leathers in his airpump, and with it to produce fixed air. Now, under this review of the ſubject, can the burning of charcoal be brought as a fair example? Nay, even in reſpiration, the fixed air is leſs in weight than the oxygen gas.

I adduce an example, and ſtrange will it be, if it is not admitted as a proper one (though, to inform my reader, as matters are managed by chemiſts, the not admitting it would not ſurpriſe me.) The electric ſpark will change the pure part of atmoſpherical air into fixed air.

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But is charcoal this electric ſpark; or will Mr. Lavoiſier name this as a diſcovery? This ſpark, conformable to his doctrine, muſt be charcoal or *carbone*. According to him, charcoal is the pureſt *carbone*; for he ſays, p. 83. "After the operation is finiſhed, we find nothing but a few atoms of aſhes remaining in the tube EF; the 28 *grs.* of charcoal having entirely diſapapeared."

As the quantity of fixed air is conſiderably leſs, when all bodies are burned in empyreal air, to what it is when charcoal is burned in it; ſo to account for this phenomenon, they ſay there are two kinds of combuſtion, one the *carbone*, and the other inflammable air; which latter combuſtion is ſuppoſed to produce water. The preſent chemical philoſophers are never at a loſs to account for phenomena.

The combuſtion in the burning of inflammable air, ſulphur and phoſphorus, is very intenſe, conſiderably above the heat of burning charcoal, as we have ſhown; and therefore it is with great propriety that Dr. Harrington lays down this general rule, that the air is decompounded in proportion to the intenſeneſs of the fire. If very intenſe, it is decompounded to its original ingredients, fire, an acid and water; if not ſo intenſe, it retains its aerial form in the ſtate of fixed air. This (as he ſhows) is ſeen in a ſtriking light in the burning of phoſphorus; to which, if air is only gently added, the combuſtion will be very imperfectly produced, and the air only changed into fixed air.

Now, as the burning of charcoal is not very intenſe, ſo it produces only fixed air. The burning of wood is conſiderably leſs intenſe than that of charcoal; therefore, we ſhould not expect that there would be, in this combuſtion, any hydrogen gas decompounded. Mr. Lavoiſier found the burning of hydrogen gas to exceed the burning of charcoal, as 52.16280 to 37.52823, in one pound of oxygen gas. Hence, as the burning of wood produces ſo little heat, we cannot ſuppoſe there is any hydrogen gas produced in the proceſs.

But then, much leſs fixed air is found in the burning of wood, than in the burning of charcoal; the oxygen gas being rather decreaſed in weight and meaſure by the proceſs; we have no right therefore to ſuppoſe there is any burning of hydrogen gas in wood! Charcoal itſelf gives out a purer inflammable air, by diſtillation than wood does; yet, they are both the heavy kind of inflammable air, which, when burned, turns the oxygen gas into fixed air. And Dr. Harrington has clearly ſhown, that this inflammable air is chiefly produced from the fire in the act of diſtillation; wood being compoſed of an acid and earth neceſſary to concentrate fixed fire: Moreover, after what Dr. Harrington has ſaid, I ſhould hope, that the abſurd doctrine of oxygen and hydrogen gas, as forming water, is given up.

As chemical bodies, ſuch as charcoal, do in general conſiſt of groſs bodies, the chemical properties of which we are but little acquaint-

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ed with, we ſhall not inſiſt on them. Let us take the pure electric fire, ſo general and abundant in nature, and conſider its properties and operations on air. Electric fire acts upon bodies in the ſame manner that common fire does; it ſets fire to combuſtible bodies, diſſolves iron and calcines it: It increaſes the bulk of alkaline air, and makes it inflammable. All theſe effects are peculiar to fire; but they are much ſtronger than thoſe of any fire which art can produce. Electric fire in paſſing through a bar of iron will melt it, though its paſſage is ſo wonderfully quick, as to be performed in an inſtant of time. Now, we are certain, that its power to melt the iron at once, muſt be from its fire; and no fire that we can produce will have the ſame effect upon the iron, till after a very tedious operation. Iron can likewiſe be calcined by it in the ſame quick and wonderful way.

Let us conſider then, its action upon reſpirable air. Dr. Prieſtley found that electric fire can turn the dephlogiſticated part of atmoſpherical air into fixed air. If then, as is really the caſe, dephlogiſticated air is turned to fixed air, without the agency of the third body, viz. a combuſtible one; this is a direct proof of our theory: The intenſeneſs of the electric fire being ſufficient to ſet looſe the air's fixed fire: And that the air's fixed fire is ſet looſe, is diſcoverable by the ſpark being ſo much more vivid, when taken in atmoſpherical air, to what it is when taken in any other air, or in

vaccuo.

vaccuo. Dr. Harrington hath fully elucidated its action upon air ‡.

Mr. Cavendiſh, in defence of his theory, ſays, that the fixed air produced by the electric ſpark, is from the combuſtion of ſome extraneous body in the operation. But, as a confutation of this vague ſuppoſition, it may be ſufficient to obſerve, that the ſame apparatus will, if

‡ We come next to Mr. Cavendiſh's important experiment in forming the atmoſpherical and dephlogiſticated airs into the nitrous acid; but we ſhall give a different explanation of it, from what Mr. Cavendiſh has done. I have all along ſuppoſed that reſpirable airs are formed of an acid, fire, and water; and that the atmoſpherical air is a very different air from the artificial empyreal air of chemiſts: the latter not at all ſupporting vegetable life, and likewiſe animal life being not able to injure it completely, as animals died in it when it was better by the nitrous teſt, than the pureſt atmoſpherical air.

Hence in Mr. Cavendiſh's experiment, when the atmoſpherical and artificial empyreal airs are mixed, there is a quantity of actual fire thrown into them. We have before proved, that fire will decompound reſpirable air, and according to the intenſity of the fire, the air will be accordingly decompounded. If it is the common combuſtion, ſuch as wood, charcoal, &c. the air will be left in the ſtate of fixed air; but if it is a more intenſe heat, ſuch as ſulphur, phoſphorus, metals, &c. the air will be decompounded into an acid and water.

Now, the electric heat is a moſt intenſe one, it being capable of producing a phenomenon that we cannot imitate by any chemical proceſs. Metals are only melted by heat; the electrical fire is ſo wonderfully intenſe, as to diſſolve iron in one diſcharge of the battery. Dr. Van Marum melting a conſiderable body of iron at one diſcharge; he likewiſe entirely calcined 24 inches of leaden wire, three eights of an inch in diameter, by one exploſion. This ſhows a degree of heat we cannot imitate. Our heat that we can apply, requires a long continued application, but this is inſtantaneous. Therefore,

if ever ſo long continued, equally produce the very ſame effect, and with golden wires to conduct the ſpark. Now, if there were any accidental combuſtible body at the firſt part of the operation, it would in time be conſumed; but the effect equally continues. Moreover, if the ſpark is taken in artificial dephlogiſticated air, its fixed fire being more cloſely united, the ſpark will not be able to decompound

Therefore, when empyreal air is acted upon by this wonderful heat, we ſhould expect it would decompound the air to an acid and water.

This theory is moſt ſtrikingly ſhown, in firing inflammable and empyreal air. If there is a full proportion of inflammable air, there will be ſuch a degree of fire ſet looſe, as to fly off with the acid, as we have before explained; but if there is a leſs proportion of inflammable air, ſo as not to produce ſo great a heat, the acid will be left in the reſiduum. A ſimilar phenomenon will take place in the burning of the reſpirable airs in the electrical fluid. If the electrical fluid is conſiderable, and the empyreal air very rich, part of the acid will be carried off in the ſame manner as when burned in inflammable air. But if the electrical fire is not ſo conſiderable, and the empyreal air not ſo rich, far more of the acid will be left in the reſiduum. This will account for the different reſults between Mr. Cavendiſh's and Dr. Van Marum's experiments; the latter uſing a greater quantity of the electrical fluid, and a richer air than the former.

Mr. Cavendiſh ſuppoſed that the acid came from the phlogiſticated air; this aroſe from their theories, which ſuppoſed that there was no nitrous acid in empyreal air. But he found that he could not obtain any nitrous acid in this proceſs, when he uſed only phlogiſticated air, and he could obtain ſome when he uſed empyreal air; but this he imputed to the impurities of that air; however, there is more of it decompounded, than can be accounted for by that cauſe: But when he mixed the atmoſpherical air with the artificial empyreal air, he then could form them into the nitrous acid with the electrical fluid.

I have

pound it, and conſequently no fixed air is produced; whereas, were it produced by ſome combuſtible body being ignited in the operation, the dephlogiſticateded air, inſtead of reſtraining, would make it burn with more vividneſs: It being a well known fact, that bodies burn more intenſely in this air than in atmoſpherical air.

But

I have all along (in my publications) ſhown the atmoſpherical air to be a very different air from the artificial reſpirable air made by chemiſts. The former is made by nature's own delicate hand; her ingredients being water, fixed air, and the ſun: the latter being made in an elaboratory, with mineral acids, water, and earth, or ſalts, forced into an aerial form by an intenſe fire. The one will ſupport both animal and vegetable life, but the other will ſupport neither of them; vegetables dying immediately in it, and animals long before they have much injured it, even when it is in a far purer ſtate than the beſt atmoſpherical air.

Hence in this experiment of Mr. Cavendiſh, the electrical fluid is not able to burn ſo conſiderably in empyreal airs, ſo as to reduce them, except the atmoſpherical air makes a part of them. By the atmoſpherical air, the electrical fire is ſo ignited, as then to operate forcibly upon the artificial empyreal air, and to produce ſo ſtrong a degree of heat, as to decompound the airs.

This artificial empyreal air, we have likewiſe all along ſhown to be of a ſtrong combuſtible quality, burning with partial exploſions (according to Dr. Prieſtley, as if it was full of ſome combuſtible matter). But by adding a greater quantity of phlogiſton or concentrated fire to it, I have made it ſo inflammable, as to explode all at once.

Theſe two airs then, viz. nature's atmoſpherical air, and the high concentrated artificial air of chemiſts, being mixed together, and expoſed to the influence of the electrical fire, will be decompounded, producing the nitrous acid; but the acid is not produced from the decompoſition of the phlogiſticated air. For take only the empyreal part of the atmoſpherical

But all the phenomena produced by the electric ſpark, when taken in atmoſpherical and artificial dephlogiſticated airs, are to be accounted for only by Dr. Harrington's theory. When theſe produce the nitrous acid, it is conformable to our theory, which alſo fully accounts for the phenomena being from the artificial dephlogiſticated air acting as the combuſtible body, to atmoſpherical air as the

rical air (which is eaſily done, by expelling the air from water; and then expoſing the water to the atmoſphere, and then expelling the air again:) After that, mix it with the pureſt artificial empyreal air, and there will be the ſame phenomena.

Or take the phlogiſticated part of the atmoſphere only, and mix it with the pureſt empyreal air, and there will not be theſe phenomena. No doubt ſome acid will appear, but it will be no more than what would have been, if the phlogiſticated part of the atmoſphere had not been added to the artificial empyreal air; for the higher the concentration of fire in aerial bodies, the greater difficulty there is in decompounding them. Thus in animal reſpiration, I can decompound the whole of the empyreal part of the atmoſpherical air, and only about one third of the artificial empyreal air, and that with difficulty; ſo that inſtead of its being a more luxuriant air for animal life, it is the reverſe; this is diſcovered by your breathing in it; and very ſenſibly in the reſpiration of mice, that delicate animal is ſeen to have the greateſt difficulty of breathing in it from the firſt, and dies before it has half injured it.

But even inflammable air may be breathed and decompounded; the immortal Scheele reducing a great quantity of it to foul air, by breathing it alone. Therefore if the artificial empyreal air is expoſed to the electrical fluid itſelf, it will in ſome meaſure be acted upon as we might expect. But Dr. Prieſtley has ſhown us long ago, that the empyreal part of the atmoſphere may be all decompounded, by the electric ſpark.

agent; indeed they elucidate one another. The electric ſpark is capable of being ignited only in atmoſpherical air; and by the great heat which comes from it, a conſiderable quantity of the fixed fire of the artificial dephlogiſticated air is ſet looſe, by which the fire becomes ſo intenſe as to decompound the airs; and by taking them from their aerial forms, to produce the nitrous acid. That phlogiſticated air has

This then is the true explanation of the experiment; and the acid which is dopoſited in the experiment, comes from the empyreal air.

In making the red precipitate, we may be certain in what manner empyreal air is formed. The red precipitate, and the mercurius calcinatus, both yield empyreal air, though they are formed by very different proceſſes. We have before ſuppoſed, that mercurius calcinatus received its empyreal air from the atmoſphere; but we can by no means ſuppoſe the red precipitate to do the ſame.

I took a quantity of the calx of mercury, formed by the nitrous acid, and after drying it, I examined it, and found it to be the calx of mercury and the nitrous acid. I then expoſed it to a gradual degree of heat in a retort, and collected whatever was diſtilled from it; the heat was added very gradually to it, and I obſerved it regularly changed its colour to a yellow, and after that, to a bright red, called the red precipitate. After having formed the red precipitate, I examined if any thing had been ſeparated in the courſe of this proceſs from the ſalt, but I found nothing. In this experiment which I made at different times, I found if I puſhed the heat ſtrongly at firſt, I would get a little nitrous acid from it. But if the ſalt is well dried before the experiment, and the heat gently and gradually applied to it, nothing will be ſeparated.

The bodies then which compoſe the red precipitate, are the calx of mercury, the nitrous acid, and fire. That fire ſhould have this power in uniting with it, and changing it to red, is what we might have expected; as Dr. Prieſtley has long

has nothing to do in the proceſs, is clear from this, that the electric ſpark, without atmoſpherical air, is not able to produce the nitrous acid from artificial dephlogiſticated and phlogiſticated airs. Moreover, as Dr. Harrington obſerves, if you take the pure part of atmoſpherical air, excluding the impure or phlogiſticated part, it will equally, with artificial dephlogiſticated air, produce the nitrous acid.

Would but our aerial chemiſts attend to reaſon, every doubt about the truth of this doctrine might be removed by the following fact. The electric ſpark will produce fixed air, when taken in atmoſpherical air. Now, need I inform chemiſts, that in moſt combuſtions, dephlogiſticated air is turned to fixed air; that when the combuſtion is more intenſe, it is

 turned

long taught that the nitrous acid becomes red, and phlogiſticated by fire or heat; and that the mercurial calx has ſo great an attraction for fire or heat, that it will be even reduced by it alone: Nay, the phoſphoric and the vitriolic acids will form phoſphorus and ſulphur with it (ſee Prieſtley). But I have unqueſtionably proved that an earth and an acid have, when united, the ſtrongeſt attraction for heat; therefore the mercurial ſalt in this experiment, will certainly ſaturate itſelf with heat, producing the red precipitate.

Here then is empyreal air formed in the mercurial calx, it being juſt the ſame calx as that which imbibes the atmoſpherical air, viz. the *mercurius calcinatus per ſe*. They will both, if expoſed to great heat, diſcharge their empyreal air and be reduced, or not, according as the fire is puſhed. But, as we have ſhown all along, that the atmoſpherical and artificial empyreal airs are very different airs, ſo they will make deffe-rent calces; it is well known to chemiſts and phyſicians, that theſe two calces, viz. the red precipitate, and the mercurius calcinatus, are different. See Harrington's Letter, p. 126.

turned to the nitrous acid, as in the combuſtion of dephlogiſticated and inflammable airs: Nay, Mr. Cavendiſh ſays he actually turned atmoſpherical air into the nitrous acid, and not fixed air, in this ſame experiment. Dr. Prieſtley, I think, need not to be told this, ſince he has followed Dr. Harrington in proving it; though from an illiberal policy, he has omitted to mention that gentleman's name: But time which exhauſts all things, truth excepted, ſtrengthens thoſe doctrines which are founded upon juſt principles. However, the Dr. has this ſalvo; viz. he muſt differ from Dr. Harrington in ſuppoſing (conformable to the general hypotheſis of chemiſts) that in the proceſs, the nitrous acid muſt be compounded of inflammable and dephlogiſticated airs, as water is ſuppoſed to be produced by theſe airs; but ſuch a ſuppoſition is from not reflecting that inflammable air is principally fixed fire, which eſcapes in the combuſtion, together with the fire that neutralized the acid in dephlogiſticated air. Dr. Harrington hath, in the moſt public manner, called upon Mr. Cavendiſh, either to acknowledge the truth of his theory, or to defend his own: And certainly it becomes that gentlemen to do it publicly.

Dr. Beddoes ſays, p. 5. "Nitrous acid has been procured by making vital air from manganeſe paſs along with atmoſpherical air thro' an heated tube, though the experiment has not yet been ſufficiently varied, nor repeated, to be publiſhed at large, and with names." To

ſtrengthen

ſtrengthen therefore our arguments concerning heat if neceſſary, we may obſerve that heat is able to accompliſh what electric heat can do; for it appears that a heated tube will equally operate on theſe two airs, ſo as to produce the nitrous acid.

Thus we have the ſtrongeſt proofs in favour of our theory of combuſtion; viz. that the combuſtion of bodies is always in proportion to the degree of intenſeneſs of the fire: For, when the intenſeneſs is conſiderable, as in the electric ſpark, it will even decompound or ſet free the fire of atmoſpherical air. So alſo, even common fire in a heated gun barrel can decompound fixed fire, when the concentrated artificial dephlogiſticated air is added to the air of the atmoſphere: But when a combuſtible body is added to atmoſpherical air, it will eaſily ſet them on fire, or decompound their heat; becauſe atmoſpherical air, when acted upon by the intenſeneſs of heat or fire, which is produced from the ignition of a combuſtible body, is ſoon ignited: Both acting together, viz. the heat produced from the ignition of atmoſpherical air, and alſo that from a combuſtible body.

Combuſtion may be thus defined: The fixed fire of bodies can be ſet looſe or made actual fire, only by being acted upon by a very high degree of actual fire; and then only upon a combuſtible body, and its agent atmoſpherical air. The agent ſeems to act by having its acid and fire more looſely attracted, and therefore the

the eaſieſt ſet free; which then acts upon the combuſtible body. This is evident from the fire of the electric ſpark being able to ſet looſe the fixed fire of atmoſpherical air: But it is not even able to ſet looſe the fire of artificial dephlogiſticated air, when it is more fixed or concentrated. Moreover, as to the fixed fire in iron; though iron greatly attracts the electric ſpark, yet unleſs it acts at the ſame time upon atmoſpherical air, nature's great agent, it will not conſume or let looſe the iron's phlogiſton, but only melt it: But if they both act together, the iron will be calcined, and the air decompounded, becauſe the fire of both becomes actual.

This theory of ours accounts in the moſt beautiful manner for the fire produced by attrition: For, as we have already obſerved, the fixed fire of bodies is chemically attracted; and we have likewiſe ſhown from Dr. Darwin's experiments upon the expanſion and attraction of atmoſpherical air, that mechanical force can reſiſt the chemical attraction of heat. It diſcharges heat by compreſſion, and attracts it by expanſion. Here then it evidently appears, that mechanical preſſure overcomes chemical attraction: However, I do not mean to prove by theſe experiments, that it is the actual attrition which ſeparates the heat; but only, that air attracts a certain quantity of fire conformable to its expanſion; and by altering the expanſion, you alter its quantity of fire: *i. e.* Mechanical compreſſion overcomes chemical attraction.

attraction. But what affords a moſt ample explanation and illuſtration of our theory is, the ſparks produced by the mechanical attrition of flint and ſteel, when ſtruck, the one againſt the other. Theſe two bodies contain a great quantity of fixed fire; and being of a hard ſubſtance produce much friction in coaction; but if they are ſtruck together in *vacuo*, no fire or ſpark will be produced: And upon the ſame principle, and without the aid of the ſame agent, atmoſpherical air, the electric fire will not produce the ignition of iron.

Mr. Cavendiſh and Dr. Prieſtley ſeem to have had different reſults, from the paſſing of the electric ſpark through common air. The former thinks he decompounded the air into the nitrous acid, and the latter is of opinion, that the common air was changed into fixed air. Theſe are the two firſt experimenters, and therefore deſerve the greateſt credit. I can eaſily ſuppoſe, that the atmoſpherical air would be acted upon according to the intenſeneſs of the ſpark; if then a ſmall ſpark, it might produce only that degree of heat or combuſtion, capable to generate fixed air; but if more intenſe, the nitrous acid would be the reſult: And the obſervations I have made upon the experiments favour this concluſion, which is perfectly conformable to our theory of combuſtion *. The only obſervation that can be made

* The foregoing experiments ſhow, that the chief cauſe of the diminution which common air, or a mixture of common and

made from this experiment of Mr. Cavendiſh, juſt named in the note, is, that the electric ſpark will even decompound a part of artificial dephlogiſticated air; for, had Mr. Cavendiſh operated upon a purer kind of artificial dephlogiſticated air, he would have decompounded a part of it. The decompoſition depending upon the concentration, or fixed power of the fire

and dephlogiſticated air, ſuffers by the electrical ſpark, is the converſion of the air into nitrous acid; but yet it ſeemed not unlikely, that when any liquor, containing inflammable matter, was in contact with the air in the tube, ſome of this matter might be burnt by the ſpark, and thereby diminiſh the air, as I ſuppoſed in the above-mentioned paper to be the caſe. The beſt way which occurred to me of diſcovering whether this happened or not, was to paſs the ſpark through dephlogiſticated air, included between different liquors: For then, if the diminution proceeded ſolely from the converſion of air into nitrous acid, it is plain that, when the dephlogiſticated air was perfectly pure, no diminution would take place; but when it contained any phlogiſticated air, all this phlogiſticated air, joined to as much of the phlogiſticated air as muſt unite to it in order to reduce into acid, that is, two or three times its bulk, would diſappear, and no more; ſo that the whole diminution could not exceed three or four times the bulk of the phlogiſticated air: Whereas, if the diminution proceeded from the burning of the inflammable matter, the purer the dephlogiſticated air was, the greater and quicker would be the diminution.

The reſult of the experiments was, that when dephlogiſticated air, containing only 1-20 of its bulk of phlogiſticated air (that being the pureſt air I then had), was confined between ſhort columns of ſoap-lees, and the ſpark paſſed through it till no further diminution could be preceived, the air loſt 43-200 of its bulk; which is not a greater diminution than might very likely proceed from the firſt-mentioned cauſe; as the dephlogiſticated air might eaſily be mixed with a little common air while introducing into the tube. See Cavendiſh in the Philoſophical Tranſactions, vol. lxxv. p. 382.

fire of the dephlogiſticated air, and not at all upon the phlogiſticated air employed.

He ſays, he even decompounded phlogiſticated air; but I much doubt the capacity of this air for decompoſition. He admitted liver of ſulphur to it, which, when under a great fermentation in abſorbing dephlogiſticated artificial air, I have known it abſorb phlogiſticated air, the ſame as charcoal. But perfectly aſcertained of this fact, we may conclude, that if the electrical ſpark produces fixed air, it muſt be by decompounding the dephlogiſticated air: And, a proper attention to Dr. Harrington's experiments and arguments, and to Dr. Prieſtley's, who has followed him, muſt convince us that dephlogiſticated air, when decompounded by an intenſe combuſtion, (viz. in the firing of inflammable and dephlogiſticated airs) produces the nitrous acid, without the aid of phlogiſticated air.

And, if the experiment is repeated with atmoſpheric air only, the electric ſpark will decompound it, ſometimes into fixed air, and ſometimes into the nitrous acid. This reconciles the different reſults of Mr. Cavendiſh and Dr. Prieſtley, and can no otherwiſe be accounted for but by our theory.

I have, I hope, ſufficiently proved that Mr. Lavoiſier has entirely deſerted Dr. Black's doctrine; and yet, in the beginning of his nomenclature, he opens his introduction with ſuppoſing that all vapours are the ſame as airs, and that they contain their heat in the ſame

manner; he explains his hypotheſis by that evaporable fluid ether. Ether, he ſays, when the weight of the atmoſphere is taken from it, will become vapour at the heat of 32°. Then, agreeable to this hypotheſis, the caloric, which the ether poſſeſſes, is what goes to aerilize or evaporize it. When condenſed in water, it ſhows very little heat; but when fired, an immenſe quantity of heat and light comes from it: And this, he ſays, is principally owing to the heat which evaporized it. But, that the heat which evaporized, has little to do in the proceſs, is proved from the well known experiment of a few drops of ether being ſuddenly dropt into atmoſpherical or dephlogiſticated air; for this is no ſooner done, than (if they are ſet on fire) there will be an immediate and vehement exploſion. If the ether receives any heat, it muſt be from the air; becauſe the operation being ſuddenly performed, it has no time to take any heat; or at leaſt, what it does take muſt be from the atmoſpherical air.

Mr. Lavoiſier would lay down this general hypotheſis, and make it paſs for true doctrine; that caloric repels the particles of bodies, like water expanding a ſpunge; and that the heat which this water attracts, upon the common principle of vapour, is the cauſe of our atmoſphere: Nay, he ſuppoſes, conformable to his hypotheſis, that the atmoſphere is compoſed of a heterogeneous mixture of all bodies, even of metallic;—ſuch an imagination has more of the *air of romance* in it, than of the principles of

of true philoſophy. The caloric of ether then is from the heat imbibed in the evaporation; and the caloric which comes from it, when burned, is not owing, in the leaſt, to the ſpirits of wine of which it is formed. But Mr. Lavoiſier, with all his narrowneſs of caloric to ether, acts an oppoſite part to the nitrous and dephlogiſticated marine acids: For, unmindful of this ſmooth theory of his, he forgets his ſtate of aerial vapour, and will have all the caloric that is produced from the burning of nitre and ſulphur, to come from the condenſed gas in the acid. Gentlemen, Does ſuch a hypotheſis require a refutation? A little ſerious reflection may convince you that it refutes itſelf.

Mr. Lavoiſier begins his chapter upon the analyſis of atmoſpherical air, with ſaying, "From what has been premiſed, it follows, "that our atmoſphere is compoſed of a mix-"ture of every ſubſtance capable of retaining "the gaſſeous or aeriform ſtate in the common "temperature, and under the uſual preſſure "which it experiences. Theſe fluids conſtitute "a maſs, in ſome meaſure homogeneous, ex-"tending from the ſurface of the earth to the "greateſt height hitherto attained, of which "the denſity continually decreaſes in the in-"verſe ratio of the ſuperincumbent weight. "But, as I have before obſerved, it is poſſible "that this firſt ſtratum is ſurmounted by ſeve-"ral others conſiſting of very different fluids."

The only comment I would make upon this paragraph is, that the word *heterogeneous* ſhould be uſed inſtead of *homogeneous*.

The experiment which firſt ſuggeſted, and is the foundation of Mr. Lavoiſier's doctrine, appears, upon the firſt glance, to be very much in favour of it; and, as he ſays, to prove it both analytically and ſynthetically. If mercury is calcined in atmoſpherical air, the air will loſe its oxygen gas, being imbibed by the mercury; and, upon reducing the mercury, a quantity of oxygen gas will come from it; the mercury will be reduced, and the air become reſpirable again. Now let us inveſtigate theſe phenomena, and ſee whether Mr. Lavoiſier's theory, or that of Dr. Harrington, will beſt account for them.

Mr. Lavoiſier ſays that the oxygen gas is a pure element. Dr. Harrington ſays atmoſpherical air is formed of fire, fixed air, (or, as Mr. Bergman with greater propriety calls it, the aerial acid) and water; but the factitious oxygen gas of an acid, water and earth. Now Mr. Lavoiſier allows that the reducing of metals is an act of combuſtion, which is ſtrikingly ſeen in the reducing of iron by burning. And Mr. Lavoiſier muſt alſo allow that, when moſt bodies are burned in atmoſpherical air, the oxygen gas turns to the aerial acid or fixed air; and that, into whatever this oxygen gas is turned in the burning of mercury, it is imbibed by the calx. I ſhould expect that Mr. Lavoiſier will allow me to ſuppoſe, that the combuſtion

combuſtion of one body is much the ſame as the combuſtion of another, (juſt as the reſpiration of one animal is like to that of another) that is, acts the ſame upon oxygen gas. Then I will ſuppoſe that the burning of the mercury, and moſt of the bodies upon this earth, is the ſame, and that they all turn oxygen gas to an acid air. In one caſe only, the oxygen gas is not imbibed by the burning body, but in the other it is. Then let us ſuppoſe that this fixed air, or aerial acid, is imbibed by the calx of mercury. That the calx of mercury has a great attraction for air, is a fact which has been long known in chemiſtry. I will give Mr. Lavoiſier an example. The corroſive ſublimate of mercury, if united to an alkaline ſalt, the *ſal abſinthii* for inſtance; the marine acid will leave the calx of mercury, and unite itſelf to the alkali; and the fixed air of the alkali will unite itſelf to the calx of mercury. This experiment is well known to chemiſts: And it is equally well known, that if this calx of mercury is expoſed to heat, it will form oxygen gas, and the mercury will be reduced. Dr. Harrington ſays in his Letter, p. 88. "But even fixed air may be formed into "pure dephlogiſticated air, if united with the "calx of mercury (this calx we have all along "proved to have the greateſt attraction for "concentrated fire) viz. in the experiment of "the corroſive ſublimate being decompounded "by an alkali; if expoſed to fire, it will yield "empyreal air; but it cannot bear the expla-

"nation

" nation that Mr. Kirwan gives of it, viz. the " fixed air being decompounded; for if the fire " is not puſhed, you will get the dephlogiſti- " cated air from it, and the calx will not be " reduced: Nay, to ſhew that the calx is not " phlogiſticated after this proceſs, agreeable " to Dr. Prieſtley's own teſt, it will form with " the nitrous acid, pure dephlogiſticated air " again; or if united with the marine acid, " and then precipitated again with the alkali, " it will form again pure dephlogiſticated air. " Our theory here appears ſo rational, that " when this dephlogiſticated air is burned, it " will be decompounded; that is, its fire will " be ſeparated from the fixed air, both being " produced in the proceſs of burning."

Now, it is evident that combuſtion, or the ſetting looſe a quantity of actual fire, has the power of turning the oxygen gas to an acid; and we muſt ſuppoſe that the aerial acid, when condenſed in the mercury, muſt become concentrated, forming an acid of much greater acidity. That fire will turn the pure part of atmoſpherical air into fixed air, has been fully ſhown in this letter; viz. the electrical ſpark taken in atmoſpherical air; and it will alſo turn oxygen gas into the nitrous acid. It has likewiſe been proved, that when nature's oxygen gas of the atmoſpherical air, and the factitious oxygen gas of chemiſts, are operated upon by pure fire, (ſee p. 84. of this Treatiſe) that they will be turned to the nitrous acid.

Then

Then we are clearly brought to this concluſion, that acids and the calx of mercury produce oxygen gas; and what directly confirms this concluſion is, that if an acid, either of nitrous, vitriolic, or fixed air, is added to the calx of mercury, they produce oxygen gas.

Chemiſts ſay, that this production of oxygen gas, is from the oxygen gas, which they ſuppoſe the acids and the calx poſſeſs. But with reſpect to thoſe other bodies which are ſaid to poſſeſs oxygen gas, try if they will produce oxygen gas with the calx of mercury; and ſteam, as they ſay, is capable of decompoſition; then water (for inſtance) ſeems to be the moſt proper body, as containing the greateſt quantity of oxygen gas: And likewiſe the calx of mercury has a ſtrong attraction for its inflammable air. Water then, conformable to them, ſhould be the moſt proper for the calces to get oxygen gas from; and therefore the propereſt body to unite to the calx of mercury: But it will not anſwer to unite the calx with this nor any other body, but only with acids. Moreover, what ſtrikes directly againſt their hypotheſis is; the factitious oxygen gas is fully proved, by Dr. Harrington, to be a different kind of gas, from the pure part of atmoſpherical air.

Then, in this caſe, we muſt be allowed to ſay, that the bodies, neceſſary to produce oxygen gas, are an acid, the calx of mercury and water. It is well known that acids and the earth of metals have a ſtrong attraction for each other; and theſe two bodies have a ſtrong attraction

traction for fire or phlogiſton. The nitrous acid, by being expoſed to a great heat, becomes red and phlogiſticated; and the calx of mercury has ſo ſtrong an attraction for fire, that heat alone will reduce it. And, as to phlogiſton, the nitrous acid has ſo very ſtrong an attraction for it, that chemiſts have placed theſe two at the head of the table of attractions; and the calx of mercury greatly attracts heat and becomes reduced. It is no leſs true that heat will aerilize both the nitrous acid and the mercury.

Now, under a review of all theſe circumſtances, are we not authorized to ſuppoſe that, when theſe bodies, the acid, the calx and water, are for a long time expoſed to a great heat, that they will fix a quantity of this heat or fire, and be aerilized with it, forming that neutral phlogiſticated body, called oxygen gas. The action of the fire implies this; for when they become red, they give out only empyreal air: And it is very well known that fire or phlogiſton reddens the nitrous acid.

That the acid gets neutralized with the fire and water, appears from Mr. Cavendiſh's experiments *. This proceſs is ſtill more evident

* A ſtrong confirmation of this is, that red precipitate, which is one of the ſubſtances yielding dephlogiſticated air in the greateſt quantity, and which is prepared by means of the nitrous acid, contains in reality no acid. This I found by grinding 400 grains of it with ſpirits of ſal ammoniac, and keeping them together for ſome days in a bottle, taking care to ſhake them frequently. The red colour of precipitate was rendered pale, but not entirely deſtroyed; being then waſhed with

dent in lead. If lead is burned in a quick way in atmoſpherical air, it will form only the grey calx of lead, as it imbibes the acid of the air in its reduction: And if the calx is expoſed to a great heat, it will give back again only that acid. But if this lead is calcined in a gentle way, by being expoſed to the reverberation of the heat, it becomes gradually red, till at laſt the red is highly florid, and of the ſame colour as the calx of mercury.

That the calces become red from the fixed fire in them, is ſtrongly confirmed by this, that their colour is ſimilar to that which the blood receives in the lungs; this has been fully proved by Dr. Harrington, to proceed from no other cauſe, but receiving fire or phlogiſton: And what farther corroborates this opinion is, that alkaline ſalts, which I hope, I may now be allowed to call fixed fire, will produce the very ſame effect upon the blood, forming it

with water and filtered, the clear liquor yielded on evaporation not the leaſt ammoniacal ſalt.

· It is natural to think, that if any nitrous acid had been contained in the red precipitate, it would have united to the volatile alkali and have formed ammoniacal nitre, and would have been perceived on evaporation; but in order to determine more certainly whether this would be the caſe, I dried ſome of the ſame ſolution of quickſilver from which the red precipitate was prepared with a leſs heat, ſo that it acquired only an orange colour, and treated the ſame quantity of it with volatile alkali in the ſame manner as before. It immediately cauſed an effervescence, changed the colour to grey, and yielded 52 grains of ammoniacal nitre. See Cavendiſh in the Philoſophical Tranſactions, vol. lxxiv. p. 142.

This phenomenon is owing to the fire neutralizing the acid.

into that high florid red colour, and oils the ſame.

That part of the mercury goes along with the acid, in forming the oxygen gas, hath been fully proved by Dr. Prieſtley: This is ſtill farther confirmed by nitre. Nitre is that body beſt adapted to form oxygen gas, an ounce producing half an ounce of oxygen gas. Now, alkaline ſalts are found to be already fixed and concentrated by nature; therefore, part of the proceſs is already executed, the fire being already fixed: Conſequently, the acid and the alkali only want fire enough to aerilize them.

What then muſt we think of that hypotheſis, which ſuppoſes that a ſtrong acid and an alkali can be ſeparated after they are united, and they ſay, that by a ſuppoſed ſeparation, and a ſuppoſed attraction, the acid is decompounded of its oxygen gas and phlogiſticated air. But where is their proofs that alkalies have any attraction for phlogiſticated air? They have none. Nay, the moſt whimſical (or ſhall I call it abſurd) part of all is, (as Dr. Harrington has ſhown, ſee p. 17. of this Treatiſe) that Dr. Prieſtley got ſuch a quantity of airs from nitre; that in one proceſs the alkali muſt have attracted the acid's dephlogiſticated air, and its phlogiſticated air in the other proceſs; and in both caſes, both airs being nearly above the weight of the acid: But enough of ſuch abſurdities.

Dr. Harrington ſays in his Letter, p. 87, "But the moſt ſtriking, is the acid called fixed

"air.

" air. Calcareous earths are well known to " chemiſts to conſiſt of pure fixed air, and the " pure calcareous earth; if expoſed to fire, they " will produce different airs, viz. fixed air, " phlogiſticated air, common air, dephlogiſti- " cated, and inflammable air: Juſt agreeable " to this regular gradation, proving that the " fire combines with the acid and earth, firſt " expelling the fixed air that is looſely combin- " ed with the earth; then the acid that has " got a ſmall ſaturation of fire, and as the fire " increaſes, forming a regular gradation, ſo as " at laſt to form inflammable air: But, agree- " able to their explanation, the fixed air muſt be " formed of all theſe airs. That the calcareous " earth is not impregnated with phlogiſton, is " proved from its forming pure dephlogiſti- " cated air with the nitrous acid, and from its " being all capable of being formed into phlo- " giſticated air with the vegetable acid; here, " agreeable to the new aerial proofs, it cannot " certainly be ſuppoſed to be all pure phlogiſ- " ton. Therefore it ſurely cannot admit of " any other explanation, than the one which " our theory give of it." The fixed air juſt runs the ſame gauntlet with the electric ſpark, producing the ſame airs. Dr. Prieſtley likewiſe ſays, vol. III. p. 89, " When I had recourſe to " my tall glaſs-veſſels, I uſed an ounce of ſalt- " petre pounded; and filling the veſſel up to " the mouth with pounded flint, I took the " produce of air at nine times, each about " three quarters of an ounce-meaſure. The

" firſt produce was not quite ſo good as com-
" mon air, the ſecond was of the ſame degree
" of purity with common air, the third rather
" worſe; but the fourth was ſo far dephlogiſ-
" cated, that one meaſure of it, and two of ni-
" trous air, occupied the ſpace of one-fifth leſs
" than one meaſure. The fifth produce was
" ſtill better; for one meaſure of it, and two
" of nitrous air, occupied the ſpace of half a
" meaſure. The ninth was about the ſame
" degree of purity; and the reſt, I preſume,
" were not much different."

One of the experiments which induced chemiſts to adopt their preſent hypotheſis, was, that upon the nitrous air being added to pure air, they would produce a fermentation, and decompound each other. Now, to take the oxygen gas, which is formed from nitre, and mix it with nitrous air, which is proved to be an acid air, as the acid predominates in it: For, if it is neutralized as in nitrous dephlogiſticated air, ſo as not to affect the vegetable juices, it will not decompound pure air. That nitrous air is compounded of the nitrous acid, appears clear from the electric ſpark decompounding 3-4 of it into that acid. Upon its being mixed with the oxygen gas from nitre, the acid will chemically attract the fixed fire or alkaline ſalt of the oxygen gas, as being a ſtronger acid than its own; which is aerilized and weakened: In conſequence of this, they will produce an effervеſcence or fermentation, and ſo much fire will be ſet looſe from both airs, as

ſhall

ſhall cauſe them to be decompounded, forming a phlogiſticated nitrous acid. That this is really the caſe, is evident form one of Dr. Prieſtley's experiments. He ſays the nitrous vapour will equally decompound pure air. He alſo ſays, vol. III. p. 193, "Laſtly, I found, in the "courſe of theſe experiments that the power of "this red vapour to phlogiſticate common air "was much greater, and acted much quicker, "than I had imagined when I made the firſt ob-"ſervation of the kind. For after the former "obſervation, I filled another phial with the "red vapour, and immediately afterwards "opened it under water; when the water, "ruſhing in, filled about half of it, and the "remaining air was found completely phlogiſ-"ticated, not being in the leaſt affected by ni-"trous air." The vapour here named is the pure condenſable nitrous vapour.

This is Mr. Lavoiſier's grand argument; for ſuppoſing, that the oxygen gas, with a great part of its caloric, is condenſed into the nitrous acid; or when the nitrous and oxygen gas decompound each other, that they will produce little heat comparatively. But the cauſe is this, in their condenſation, the greateſt part of their fixed fire or phlogiſton is not ſet looſe or free, but is condenſed with the airs.

For, according to our theory, we have ſhown that pure air muſt neceſſarily poſſeſs both fixed fire and an acid, in order to ſupport combuſtion. The ſame argument will take place, when the factitious dephlogiſticated air, from the

calx

calx of metals and the nitrous acid, are decompounded by the nitrous air: For the nitrous acid of the nitrous air attracts the fixed fire, which is neutralized or concentrated with the acid, water, and a ſmall part of the earth of the calx, which form oxygen gas; as having a ſtronger attraction for the fire, than the airs acid have. Dephlogiſticated nitrous air, which is a compound of the nitrous acid and phlogiſton, more amply confirms this doctrine; for the nitrous air will not decompound it; its acid being equally as ſtrong, being the ſame acid as that which the nitrous air poſſeſſes; *i. e.* being weakened by no operation, but what the nitrous acid itſelf has undergone *.

Now, I hope, we have clearly ſhewn by a regular chain of reaſoning, and convincing arguments, how, and of what, dephlogiſticated air is formed. We ſhall next examine Mr. Lavoiſier's explanation of theſe phenomena. Agreeable to his theory, all calces of metals ſhould poſſeſs dephlogiſticated air; and ſhould, upon their reduction, give out their oxygen gas. However, we find no calces do it, but the *mercurius calcinatus per ſe* and *minium*. Dr. Harrington ſays, that their giving out dephlogiſticated air, may be owing to their being formed into calces by ſo mild a proceſs; and therefore, they imbibe their oxygen gas entire, without decompounding it: For, as the calcination is effected by the heat expelling their phlogiſton

* Dr. Harrington has fully proved, that fire is attracted and fixed by acids and earths. See his Thoughts on Air.

phlogiſton entire, and imbibing the oxygen gas; ſo, no other calces, but theſe two, will give out oxygen gas, becauſe their calcinations being ſtronger, require actual combuſtion.

But here I muſt beg leave to differ from Dr. Harrington, and ſuppoſe, that actual combuſtion takes place in all the calcinations: For, Mr. Lavoiſier found, that in the calcination of mercury, he could not get it to imbibe all the oxygen gas of atmoſpherical air: The calcination ſtopped before the air was quite injured: The caſe is juſt the ſame in every combuſtion. A candle, for inſtance, will not burn in atmoſpherical air, till it has totally conſumed the air, leaving it in the ſame ſtate as the calx of mercury. And Dr. Harrington, I dare ſay, will agree with me, that combuſtion muſt turn oxygan gas to an acid; *i. e.* take from it its fixed fire, which neutralized the acid: Beſides, the time requiſite for both theſe calces to be expoſed to a ſtrong heat, implies, that they are both receiving or concentrating heat at the ſame time; otherwiſe they will not give out dephlogiſticated air.

Mr. Lavoiſier himſelf found, that moſt calces can be reduced by a ſtrong mirror, without addition; and Dr. Prieſtley found, that in their reduction, they do not give out oxygen gas, but only an acid air. Nay, ſome retain their acid ſo ſtrong, that the fire will not expel it, though, nevertheleſs, they will be reduced. And (as Dr. Harrington very properly obſerves) did their reduction depend upon their being

being ſeparated from their fixed air, or oxygen gas, they ſhould be reduced, as they parted with them. But you may get all their airs from them, and they will ſtill continue calces. I took an ounce of minium, and reduced it with inflammable air; and I accurately noted the quantity it imbibed from its reduction. I then took from the ſame quantity of minium all its oxygen gas, it producing a conſiderable quantity; I then reduced this minium in the ſame inflammable air, and found, that it took an equal quantity of the air for its reduction. Now, Mr. Lavoiſier, I ſhould certainly think, muſt allow this experiment to be deciſive againſt his theory.

Dr. Harrington hath clearly proved the following facts; namely, that the calx of iron, made by ſteam, is formed by the calx imbibing the pure water; and, that the calx of iron, formed by burning it with the oxygen gas, is from its imbibing the acid contained in the gas: But nothing but pure water is found in the calx made by ſteam. I have likewiſe found, that the calx of mercury made in diſtilled water, by ſtrong agitation, (a mill, for inſtance, one of Dr. Prieſtley's experiments) will give out no oxygen gas, but pure water only, at its reduction; and yet Dr. Prieſtley has proved it to be a calx, viz. the *precipitate per ſe*. See Prieſtley, vol. iv. And I have found, that, at its reduction, it imbibes inflammable air, and is a calx ſimilar to that made from iron by water.

We

We have proved that metals may be calcined without oxygen gas; and had Mr. Lavoiſier only looked into the different recent chemical experiments, he muſt have ſeen his hypotheſis flatly contradicted. I have found, that if air is admitted in a ſlow and gradual manner to phoſphorus, the phoſphorus being ſlightly moiſtened, ſo as to diminiſh the intenſeneſs of its combuſtion, the deflagration will be ſo gentle, as not to imbibe the pure air, but only turn it into fixed air: Yet nevertheleſs, the phoſphorus is turned into the phoſphoric acid. Now, both theſe facts are in direct oppoſition to their theories. For, the pure air ought not to have been changed into fixed air, and the phoſphorus ſhould not, without imbibing the pure air, have formed the phoſphoric acid. In a word, conformable to Mr. Lavoiſier's hypotheſis, there ought to have been no combuſtion or heat, unleſs the air was imbibed into the body of the phoſphorus; for the phoſphorus poſſeſſes no carbone *. But what Dr. Harrington ſays in his Letter, is ſtill more oppoſite to, and more ſtrongly proves the falſeneſs of Mr. Lavoiſier's theory §.

* Theſe obſervations and experiments certainly require an anſwer, and yet the chemical philoſophers, to whom they were addreſſed, have not yet deigned to give one: Do they not think them worthy of an anſwer? Or, (which is very probably the reaſon of their ſilence) Do they deem them unanſwerable?

§ But let us try the phoſphoric acid with the calces of metals; by being mixed with minium, Dr. Prieſtley found, inſtead of its giving empyreal air, it in reality formed inflammable

In reading the works of the immortal Scheele, who was well grounded in the doctrines of the old chemical ſchool, and perfectly acquanted with its principles; we find a clear proof, that different chemical bodies, by being expoſed to heat, become phlogiſticated. But he erred by embracing that theory which maintains, that fire is compounded of phlogiſton

mable air. He ſays, vol. IV. p. 136, "In order to try whether this acid had any of the properties of the nitrous acid, I mixed it with ſome minium out of which all the air had been expelled by heat. This ſubſtance, in this ſtate, I had found, when mixed with nitrous acid, yields dephlogiſticated air, but no air at all with the vitriolic or the marine acid. The phoſphoric acid mixed with this minium with little or no ſenſible heat, but the mixture expoſed to the flame of a candle, yielded air very plentifully, and it was very turbid. I received it in lime water, but it did not precipitate the lime, except in the ſmalleſt degree. The air I got in this method was not affected by nitrous air, nor did it affect common air, but was ſtrongly inflammable, burning with a bright white flame; and the ſmell of the air was the ſame with that of the ſtrong ſmell of phoſphorus. The yellow minium became of a darkiſh grey colour, or nearly black by this proceſs."

This muſt appear inexplicable according to the modern doctrines, for it is well aſcertained by experiment, that the phoſphoric acid is phoſphorus robbed of its inflammable air; for, by uniting it with inflammable air, phoſphorus is again formed. But Mr. Lavoiſier ſays, that the phoſphoric acid is phoſphorus and empyreal air; then, if that was the caſe, from whence comes the inflammable air when the acid is diſtilled with the calx of lead? The calx of lead, agreeable to the modern theories, is likewiſe formed of empyreal air and lead; if the proceſs is puſhed with a ſtrong fire, the calx is formed into lead; here there is not only the empyreal air of the phoſphorus, but likewiſe that of the calx loſt. And likewiſe here is a great deal of phlogiſton generated; ſo much phlogiſton or inflammable air (without taking to account the

phlogiſton

ton and pure air; and conſequently he ſuppoſed, that fire is decompounded in paſſing thro' bodies, its phlogiſton being attracted by the body, and its pure air ſet looſe. However, the fact, that bodies become phlogiſticated, is proved by him in its fulleſt extent.

He alſo very clearly proves, that the calx of lead and manganeſe, have the power of dephlogiſticating

phlogiſton of the metal) as to form the ſame quantity of the phoſphoric acid and minium which was uſed in the proceſs, into phoſphorus and lead.

This is an experiment which of itſelf overturns all their theories. According to Mr. Lavoiſier's theory, here was nothing to form the inflammable air produced in the proceſs, and the calx when reduced, would part with its empyreal air. Then what comes of it, and what comes of that which the phoſphoric acid is ſuppoſed to be formed of, according to Mr. Lavoiſier? The phoſphoric acid ſhould have been decompounded, forming phoſphorus, and its empyreal air ſhould have been formed in the proceſs, inſtead of inflammable air. Mr. Kirwan ſays the phenomena from the diſtillation of the calx of lead and acids, are from the fixed air being decompounded; which decompoſition forms empyreal air, and reduces the calx: But here there is no empyreal air formed, but inflammable air, and yet the calx is reduced.

Without running into wild theories, there is not a more clear chemical fact, than that the fire is neutralized and concentrated in this proceſs, forming with the phoſphoric acid, the inflammable air, which burns like phoſphorus (proved by reducing it) and with the metal forming phlogiſton. That this is clear, is proved, becauſe the phoſphoric acid, when expoſed to a ſtrong permanent heat, will form phoſphorus; and likewiſe the calx of lead with a ſtrong fire will be reduced. But it is abſurd to ſay, that in one caſe the phoſphoric acid muſt part with its ſuppoſed empyreal air before it can form phoſphorus, and likewiſe the calx its empyreal air before it can be reduced. For in this proceſs (viz. with the phoſphoric acid and minium) there is no empyreal air generated.

giſticating the mineral acids. Were Mr. Lavoiſier to take an accurate and candid review of his experiments, he would ſee the propriety of giving up his opinion, that the dephlogiſtication is produced, from imbibing the oxygen gas. For, it is impoſſible for him to produce this effect, by expoſing the mineral acids to pure air; and yet they ſhould do it, ſince their attraction

Inſtead of diſputing plain unanſwerable facts, we ſhould have ſuppoſed it a priori; for heat or fire muſt be matter, and that this matter, like other bodies, is capable of uniting with other kinds of matter. There is no way of getting over this, without ſuppoſing, as Plato did, that it was a ſpirit. One of the greateſt philoſophers, viz. Dr. Franklin, whoſe judgment and penetration in viewing into nature's operations, are the ſtrongeſt, formed the opinion of fire uniting with different bodies. He judged from his general obſervations; but here we have clear and obvious chemical experiments, which will bear no other explanation. It is an opinion that chemiſts have always have entertained.

The calces of metals, they ſay, are formed by the metals imbibing the oxogynous principle, which is, according to Mr. Lavoiſier, empyreal air, and according to Mr. Kirwan, fixed air. Though metals are calcined by air, acids, and water, nay ſulphur; yet it is all, they ſay, through their attracting fixed or empyreal air. But to allow them the ſtrange ſuppoſition, that air, acids, and water, contain either empyreal or fixed air they themſelves teach that ſulphur contains neither of theſe airs. The immortal Scheele found that, in diſtilling the calces of metals with ſulphur, the calx attracted part of the ſulphur's phlogiſton, becoming phlogiſticated, and that the ſulphur was changed in conſequence, into a volatile ſpirit of ſulphur; but that will be ſaid is owing to the ſulphur's attracting the oxogynous principle from the calx. Even the calx of iron, formed by the ſteam of water, will produce theſe phenomena. I took the different calces of lead, minium, litharge, &c. and after diſcharging all the air from them, (this ſuppoſed oxogynous principle) yet the ſame volatile ſpirit was formed, and the calx became phlogiſticated.

attraction for it is ſaid to be ſo great, as to rob the calx of lead of pure air, whoſe attraction for it is ſaid to be ſo great. But let Mr. Lavoiſier take the mineral acids in their moſt dephlogiſticated ſtate, and paſs pure, or their artificial dephlogiſticated air through them, which may eaſily be done in the veſſel for ſaturating water with fixed air, and he will find the air injured, and the acids phlogiſticated: But it ſhould have been the reverſe.

My readers, I hope, can entertain little doubt, from what has been ſaid, that alkaline ſalts, oils, and other phlogiſtic bodies, when decompounded, give out an amazing quantity of fire; which clearly ſhews them to be formed of fixed fire. This being ſo, we find that acids are the bodies which nature employs to concentrate it, juſt as the vegetable acid or fixed air, water and earths, are employed in oils, bitumens, wood, &c. and the mineral acids in ſulphur, nitre, &c.: Therefore, when theſe acids are united with earths or water, which (as Dr. Harrington obſerves) are the great bodies to concentrate fixed fire, we ſhould not be ſupriſed, if fire could be artificially concentrated; particularly by aerilizing theſe bodies.

phlogiſticated. But to make the experiment beyond all cavil, I diſtilled ſulphur and iron with a ſtrong fire; now they all allow, that neither of theſe bodies contain the leaſt of the oxogynous principle. At the firſt, as Mr. Scheele obſerves, the ſulphur's expels the iron's phlogiſton as inflammable air; but as the fire becomes ſtronger, the calx of the iron attracts part of the ſulphur's phlogiſton, which becomes the volatile ſpirit of ſulphur, and the iron becomes phlogiſticated. See Harrington's Letter, p. 103.

dies. Dr. Black has fully ſhewn, that a great quantity of fire is neceſſary to aerilize even vapours; but we may be certainly aſſured, that a greater quantity of heat is required for the formation of permanent airs or vapours; or to form a fire air, compounded of an acid, fixed fire, and water: But in forming the ſalt air, if I may be allowed the term for oxygen gas, a little earth may be neceſſary.

Many reaſons may be alledged to ſhew that it is very probable that they form a body ſome thing ſimilar to nitre. Nitre forms the largeſt quantity of oxygen gas; and, like oxygen gas, it makes combuſtible bodies burn in the ſame manner. This is ſtrikingly proved in the burning of nitre and charcoal, or of any other combuſtible body. They burn equally, as if they were burning in oxygen gas. Moreover, it is evident that alkali has much to do in the proceſs; for, if the acid is united to the charcoal without the alkali, they will not burn, and the alkali is conſumed in the proceſs: Therefore this is a concluſive proof that charcoal, &c. will not burn, but with an acid and an alkali, or fixed fire. And as oxygen gas equally promotes the combuſtion, we may ſuppoſe that it is formed of ſimilar bodies as nitre, viz. an acid, an alkali, and water: This Dr. Harrington has long ago obſerved to be the compoſition of oxygen gas.

And if, (as Dr. Prieſtley and others have found) in the ſolutions of metallic earths and acids, by being expoſed to a ſtrong heat, long continued,

continued, the earths would be depoſited with ſaline qualities and properties; then, when theſe bodies were aerilized, they would ſhew more of theſe qualities, from the greater quantity of fixed fire neceſſary to aerilize them. This is ſtrikingly ſeen in the ruſt of iron, which is formed of the acid of the atmoſphere. It will, by being forced in a ſtill, take the form of vapour, and receive ſo much fixed fire as to form the volatile alkali. The ruſt of iron, ſays Mr. Keir, by being diſtilled, forms the volatile alkali. This explains the reaſon why many chemiſts have found the volatile alkali in their experiments, when they leaſt expected it, without having recourſe to thoſe wonderful tranſmutations they ſpeak of. For, as Scheele and other chemiſts found that oils, ſalts, and other bodies become volatile by receiving a larger quantity of fire; ſo alkaline ſalts are the ſame, upon the ſame principle: For as ſalts are formed from fixed fire; fixed by acid, earths, and water; ſo a larger quantity of fire thrown in will make them become volatile.

The acetite of potaſh, as Mr. Lavoiſier calls it, is a neutral ſalt formed of the acetous acid and the vegetable alkali. Now, it is well known, that this ſalt (as Mr. Lavoiſier obſerves, page 270.) will give out ammoniac in diſtillation; and by ammoniac he means the volatile alkali. Then, can there be a ſtronger proof of the truth of our hypotheſis, that an acid, when united to other bodies, as

ſalts,

ſalts, earths, &c. concentrates a great quantity of fire, ſo as to change the fixed to the volatile alkali, as in this proceſs of the acetite of potaſh? And it alſo clearly aſcertains this fact; that by an addition of fixed fire, the fixed alkali will become volatile. And it proves, which is a fact of ſtill greater conſequence, that when an acid and an alkali are expoſed to heat, they will concentrate a very great quantity of fire: Therefore the nitrous acid, which is an acid of a ſtronger attraction for the alkali, and for fixed fire, will, (as we have ſhewn) in the diſtillation of nitre, attract and concentrate a quantity of fire, and form oxygen gas. All theſe are fair and clear deductions from unqueſtionable facts.

With this review we may be able to account for all the phenomena of the late numerous experiments made by chemiſts for theſe twenty years; and I aver, that there is not one of theſe phenomena, but may receive an eaſy and rational explanation from this hypotheſis. When oxygen gas is formed from acids and earths, we have ſuppoſed that the acid, with fixed fire, and a little of the earth, are aerilized into the neutral aerial ſalt or nitre, called oxygen gas. That there is a ſmall quantity of earth, as much as is neceſſary to combine the fixed fire, the acid and water together, is very probable: For (as Mr. Beaume obſerves) it has all along been ſuppoſed, by old chemiſts, to be a kind of baſis to ſalts; and theſe bodies, I ſhould think, form both oils and ſalts. That all bodies,

bodies, which contain a great quantity of fixed fire, are formed of acids, water, and earths, is what Dr. Harrington has always taught, nay I will venture to ſay, proved. But he proves that the pure air of the atmoſphere is not formed of earth or the ſtrong mineral acids, but of water and a weak acid, called the aerial acid or fixed air; and therefore an air very different from the factitious oxygen gas; But that the gas may contain a little earth, when made from metals, is very probable. Dr. Harrington ſays in his Letter, p. 135, "As "our theory ſuppoſes that there is a ſmall "quantity of earth in the artificial empyreal "air, to aſcertain this, I examined very accu- "rately the reſiduum, after decompounding "the empyreal air by the electric fluid; and I "always found a depoſition of an earthy ſedi- "ment. That the quickſilver could not pro- "duce it, I aſcertained, by putting in a column "of diſtilled water between it and the air; "ſuch a quantity as would dilute the acid, "ſo as that it could not in the leaſt act upon "the mercury. Beſides, if the proceſs is con- "ducted over ſoap-lees or lime water, by a "chemical examination, you will equally find "that there has been an earthy depoſition "from the air's being decompounded." And Dr. Prieſtley has brought a number of experi- ments to prove, that nitrous air contains a lit- tle earth, and how difficult it is to detect it. Beſides, as Dr. Harrington rationally ſuppoſes, water is earth, with a great quantity of latent

heat. Ice, by having a greater quantity of heat thrown into it, becomes water; ſo earth, by having a greater quantity of heat thrown into it, may become water. Therefore, he farther ſuppoſes, that by the earth's imbibing a quantity of fixed fire, ſo as to be aerilized, as in oxygen gas, it then has the properties of water.

Dr. Harrington has given an indiſputable fact to prove that fixed air, water, and the fire of the ſun, produce pure air; and I wonder how chemiſts can overlook ſuch obvious experiments. He ſays in his Letter, p. 116, "But after analyzing the air, we will likewiſe "prove the ſynthetic part. I took the fixed air "I got from the magneſia, and expoſing it to "heat and moiſture, formed it into pure air "again. See my Thoughts on Air, p. 307." "But we have another experiment in making "this artificial air, which will ſhew us the pro-"ceſs in a more clear way, ſo that we cannot "poſitively miſtake it; which is, making it "from the fixed air and the pureſt water. In "impregnating water with fixed air, and ex-"poſing it to the ſun, the fixed air will be "formed into empyreal air. Agreeable to our "theory, the ſun, fixed air, and water, ſatu-"rate themſelves into this compound empy-"real air. But agreeable to their theory, the "fixed air is a compound, and muſt, in this "proceſs, have been decompounded; that is, "each ounce of fixed air muſt have been de-"compounded into three ounces of inflam-"mable air, and one of dephlogiſticated air.

"As

" As this fixed air then, is compounded of ſo
" much phlogiſton, we muſt certainly obſerve
" it upon the decompoſition. I took a gallon
" of the pureſt diſtilled water, impregnated
" with fixed air, and expoſed it to the ſun. I
" took another gallon of the ſame water, and ex-
" poſed it equally to the ſun. Upon examining
" them after expoſure, the water impregnated
" with fixed air, yielded me a large quantity of
" empyreal air; but the pure water none.
" Then I endeavoured to find the quantity of
" inflammable air that muſt have been ſet
" looſe from the fixed air, but there was not a
" particle to be found in it: Nor was it depo-
" ſited as phlogiſton; for if we are to retain
" one old chemical fact, phlogiſton and water
" repel each other; therefore, we ſhould have
" found it ſwimmiug upon the water's ſurface:
" But in ſhort, the moſt ſceptic ingenuity could
" not find the leaſt of it any way. Upon ex-
" amining the other gallon of water, there was
" a greater appearance of phlogiſton in it.
" But undoubtedly it is only to be accounted
" for in this way, the ſun's rays united with
" the fixed air and water, and formed this em-
" pyreal air; for in the other water, as it had
" no fixed air to ſaturate the rays, they united
" themſelves in ſome meaſure to the water,
" and very ſlightly phlogiſticated it; and like-
" wiſe a thermometer that was in the impreg-
" nated water, ſhewed a little leſs heat than
" the other water, owing no doubt to the ſun's
" rays being abſorbed; alſo, there was a little

" more loſs of the impregnated water than the " other, owing to its being abſorbed into the " compound empyreal air. That it does poſſeſs " water, as does likewiſe inflammable air, ap- " pears from this, that when they are fired to- " gether they turn to water."

When water robs iron of its phlogiſton, it forms an oily ſcum upon it, which is very conſiderable; therefore, in this experiment of the fixed air, being formed into dephlogiſticated air, there ought to be the ſame ſcum.

Now, can Mr. Lavoiſier, upon the formation of pure air, from fixed air, find the carbone, which ought to have been depoſited in the water, being ſet free from its combination with fixed air? Nay, will it not give our reader a laughable ſurpriſe, when I tell him, that Mr. Lavoiſier ſeriouſly propoſes a manufactory to obtain charcoal by the decompoſition of fixed air. See his Elements, page 230. But I will hint to him a better manufactory, and one more conformable to his hypotheſis. He ſays, that water conſiſts of hydrogen and oxygen gaſſes; and, that theſe gaſſes, with the addition of *carbone* or charcoal, form alkohol or ſpirits. Now, as the river *Seine* produces plenty of water, and as charcoal is a cheap commodity, the tranſmutation of water into ſpirits would be a manufactory that would turn to good account. This would lower the price of French brandy in Old England; or, as ſeveral of our Engliſh chemiſts are no leſs induſtrious and ingenious than Mr. Lavoiſier, the Thames might

might be turned into good Britiſh ſpirits; which would render that article ſtill cheaper: But alas! this, I am afraid, will ſtill be one of the chemical diſſiderata: And, as this kind of chemiſtry will not effect ſo much good; an alarm may be ſpread on the other hand. For, according to them, water is formed of inflammable and oxygen gaſſes, two bodies the moſt combuſtible in nature. If therefore they ſhould be able to ſet the Thames on fire, London would be in danger of being reduced to aſhes.

The very ſingular hypotheſis of water being formed of inflammable and dephlogiſticated air, is ſtrenuouſly ſupported by modern chemiſts; as, upon this, their new chemiſtry depends. Mr. Lavoiſier endeavours to ſupport this hypotheſis; for he ſays, that by the decompoſition of water, by ſteam paſſing through charcoal, the water is decompounded, the hydrogen gas being ſeparated from the oxygen gas: But, were his hypotheſis true, this inflammable air or hydrogen gas, inſtead of changing oxygen gas into fixed air, ſhould turn it into water, which it does not. Nothing more, I think, needs to be ſaid, ſince Dr. Harrington has fully explained, how the inflammable air is formed in this proceſs; which is a direct confirmation of his doctrine, that fixed fire is phlogiſton *.

Flame

* The theory is ſimply this. Vegetable bodies are principally formed of the vegetable acid, with water and an earthy baſis; which is nature's own compound to attract the fire

Flame is very delicate. It will be extinguiſhed in air that will ſtill ſupport animal and vegetable life. We have, I hope, ſatisfactorily proved, that the life of flame exiſts by ſetting looſe fixed fire; and alſo, that both an acid and fire are requiſite to its ſupport. *Therefore it is a natural concluſion, that the atmoſphere, unleſs nature had formed it of both an acid and fixed fire, would not have been proper for the ſupport of flame.* And what more fully confirms this is, that the rich factitious dephlogiſticated airs, (dephlogiſticated nitrous air for inſtance) which poſſeſs a ſtrong acid, will ſupport flame with the greateſt avidity, owing to the

fire of the ſun concentrating it. When it is expoſed to the fire, the fire is concentrated or attracted into the compound, forming charcoal; and when the heat is puſhed further, there is ſuch a high concentration, as to form inflammable air; but, if an over proportion of water enters into the compound, a conſiderable leſs quantity of fire enters, and they form fixed air.

This is by no means ſingular; all compounds, agreeable to the greater or leſs proportion of the different ingredients, form different bodies, ſeen clearly in the acid airs. Thus, I have proved, that phlogiſton and the nitrous acid form nitrous air; but by adding a greater proportion of phlogiſton to the air already generated, or before its generation, it will produce either inflammable or empyreal nitrous air. The ſame happens with regard to the vitriolic acid air; a greater proportion of phlogiſton, forms it into the hepatic air. In the marine acid air, it is the ſame; by expoſing it to iron, it will reduce the iron into a calx, ſeizing upon its phlogiſton, and forming inflammable air; which is juſt the ſame proceſs as the nitrous acid air, in forming inflammable or empyreal nitrous air with iron. But, from the ſingular theories of airs, they will have the latter to be a different proceſs. See Harrington's Letter, p. 26.

the ſtrong nitrous acid in it; but it will not do for the ſupport of either animal or vegetable life. As a farther confirmation of this, the fixed alkali and ſulphur will not enflame together; nor will phlogiſton, or the ſpirits of wine, ſupport flame without the aid of an acid, dephlogiſticated nitrous air, for inſtance.

Dephlogiſticated air is ſuppoſed to be an element: But, can chemiſts really ſuppoſe, that the pure part of atmoſpherical air, and the factitious dephlogiſticated airs, particularly the dephlogiſticated nitrous air, are the ſame; which, conformable to their hypotheſis, they ought to be? Nay, the laſt ſupports flame with the greateſt avidity, but is as noxious to animal and vegetable life, as the moſt mephitic air we know of. Its effect in depriving an animal of life is the very ſame, as that of the moſt noxious phlogiſticated air. Examine them chemically, and there will be found the ſame differences in all the oxygen gaſſes; by weight the ſame; in ſhort, the ſame differences will appear in every compariſon.

We have already obſerved, and it is conformable to our hypotheſis, that the atmoſpherical air is a body ſimilar to nitre, which is formed of an acid, and an alkali, (which is fixed fire and water.) Atmoſpherical air, or the pure part of it, is formed of an acid, fixed fire, and water: This ſimilarity is ſtrikingly ſeen in combuſtion. Combuſtible bodies will not burn without the aid of atmoſpherical air, or nitre; and the nitre and the air are left, after

after the combuſtion, in the ſame ſtate, both having loſt the fixed fire that neutralized them. The alkali has diſappeared, and alſo the alkaline body which neutralized the air's acid; it being left in the ſtate of fixed air, or an acid. And, that this body which aërilized both the nitre's acid, and the air's acid, is neceſſary to combuſtion, is a certain truth; for, if you attempt to burn combuſtible bodies in theſe acids, they will not burn. And, as we before obſerved, this nitre is the body which will produce the moſt of factitious dephlogiſticated air. Dr. Prieſtley formed above half of the nitre into dephlogiſticated air; and by adding more nitrous acid, you may ſtill go on forming more air. That the alkali diſappears, making a part of the air, is certain, from the quantity formed: That it cannot be formed, either from the acid or the water, we have fully proved, ſee page 15. And Mr. Watt, from Dr. Prieſtley's experiments, examined the acid, and found ſuch a quantity of it had come over entire, that he ſays, it was the greateſt part of the acid employed in the proceſs.

My reader is, I hope, ſatisfied by what has been ſaid, that fixed fire forms phlogiſton, alkaline ſalts, &c. That thoſe bodies beſt adapted to neutralize fixed fire, are acids and water, or earths; and, that atmoſpherical air is formed of a mild acid, a mild concentration of fire, and water. But, as we can make a factitious gas that will perform the offices of

air

air, we ſhall endeavour to inveſtigate it: For, according to our theory, the air, neceſſary to combuſtion, muſt be formed of an acid, and fixed fire or phlogiſton. The acid is neceſſary in combuſtion to aſſiſt the fixed fire in being let looſe, or breaking from its bond of union, into a free or actual ſtate.

We ſhall take the nitrous acid, which, from its attraction for fire, ſeems to be one of the beſt acids, and unite it with concentrated fire or phlogiſton.

By expoſing the nitrous acid to the eſſential oils, ſpirits of wine, the phlogiſton of metals, we may form an air much better adapted to the purpoſes of combuſtion, than atmoſpherical air; becauſe it is formed of a ſtronger acid, and of a richer or ſtronger phlogiſton, or concentrated fire. Chemiſts call this air, *dephlogiſticated* nitrous air; an error which proceeds from their having adopted very abſurd chemical theories concerning air: But that it is a phlogiſticated air, formed of the bodies we have named, I ſhall, (agreeable to my former promiſe) prove in the moſt ſatisfactory manner: And, I will venture to ſay, that thoſe who diſallow it, from the power of prejudice, deſerve not the name of chemiſts *.

It appears to be juſt, even at the firſt glance, of what has already been ſaid: For, will not

* But in the name of wonder! is not the nitrous ether formed of the nitrous acid and phlogiſton? And is it not made in the ſame manner and of the ſame bodies as the nitrous *dephlogiſticated air*?

the nitrous acid, and phlogiſtic bodies, eſſential oils, &c. burn in their condenſed ſtate? And, will our modern aerial chemiſts deny this to be a phlogiſtic compoſition? Experiments have always been the great and ſure path to walk in, when we inveſtigate the phenomena of nature; but experiments without much circumſpection and knowledge, are apt to lead philoſophers into error: This we ſhall prove in this treatiſe, in the moſt ſtriking manner, and ſhew, how our modern theoriſts have been led into ſuch errors by their experiments.

Eſſential oils and the volatile alkali are the moſt volatile bodies in nature. Theſe, when united with the nitrous acid, the attraction of which for the oils is ſo ſtrong and rapid, produce, in their union, very great heat. Now, that theſe bodies, when united, ſhould not attract each other, nor ſhould be aerilized together; but that the volatile oils and the volatile alkali ſhould ſo far counteract their volatility, as to become fixed, when expoſed to the ſtrongeſt fire; ſhould ſo decompound the acid, as to leave no attraction for it in its compound ſtate; nay, to decompound it into three diſtinct bodies; viz. dephlogiſticated air, nitrous air, and phlogiſticated air: I ſay, all this is ſuch an abſurdity, as not to be reconciled to common ſenſe. But, as one error imperceptibly leads to another, ſo is one abſurdity the cauſe of another; mortification is the conſequence, which might have been prevented,

had

had chemical philoſophers but paid a proper attention to Dr. Harrington's theory: And certainly, not to have attended to what he has ſaid, is highly culpable: I ſhall give it in a long quotation.

There is no end of their errors; for, is it not evident, that if an alkaline air is fired in this nitrous dephlogiſticated air, an acid and water will be in the reſiduum? the phlogiſton of the air and the alkali have diſappeared, going off as heat or fire in the combuſtion. This, our aerial philoſophers deny. The alkali muſt have entered the dephlogiſticated air, and the latent heat only muſt have been ſet looſe. But, wonderful! this deplogiſticated nitrous air, ſhews no teſt of acidity, no not even to the vegetable juices. The nitrous air, which ſhews a ſtrong acidity by the vegetable juices, by adding phlogiſtic bodies to it, to neutralize it, will form dephlogiſticated nitrous air; and if alkaline air is fired in this wonderful dephlogiſticated nitrous air, it will become the nitrous acid; which, they ſay, is from having attracted this alkaline air: Such is their hypotheſis. This is neither a lame, nor falſe repreſentation; for, (as Doctors Harrington and Prieſtley found) if this alkali is fired in their common dephlogiſticated air from nitre, they will produce the nitrous acid. It may then be aſked, what is chemiſtry indebted to our modern aerial teachers? Alkalies do not neutralize acids, but form bodies into the ſtrong mineral acids; bodies, which ſhew no ſigns of

acidity; they having been acids, but having had their acidity neutralized with phlogiſton: Nay, they blunder from one abſurdity to another. Dr. Prieſtley ſometimes calls phlogiſton the principle of alkalinity, and ſometimes he makes it the principle of acidity. In the Phil. Tranſ. vol. lxxviii. p. 156, he ſays, 'As there is ſomething in dephlogiſticated air that ſeems to be the principle of *univerſal acidity*, ſo I am ſtill inclined to think, as I obſerved in my laſt Volume of Experiments, that phlogiſton is the princple of *alkalinity*, if ſuch a term may be uſed; eſpecially as alkaline air may be converted into inflammable air.' In vol. VI. p. 99, where ſpeaking of vapour operating upon charcoal, and producing fixed air, he ſuppoſes, that inflammable air and water produce fixed air.

Let us ſee how our hypotheſis agrees with this phenomenon. But it is ſo obvious, that I ſhould apologize to my reader for explaining it. The dephlogiſticated nitrous air, when richly made, having fire applied to it, penetrates its own phlogiſton, which kindles ſuch a quantity of fire, as thereby to ſet looſe all the fixed fire which formed theſe bodies, alkaline air, &c. leaving the acid and water in the reſiduum, with a little aeriform matter, formed of the acid, and ſome phlogiſton not totally ſet looſe.

But I ſhall give a long quotation from Dr. Harrington's Thoughts on Air, publiſhed in 1785. And, I think, my reader will be much ſurpriſed, that his principles, and the arguments which ſupport them, have not been more

more attended to;—a plain proof that the true *ſolid principles* of chemiſtry have been kicked out of doors, to make room for the *aerial flights* of modern chemiſts. He ſays, p. 56, 'Dr. 'Prieſtley has made this kind of air by ſe-'veral proceſſes; adding a larger quantity of 'phlogiſton to nitrous air. It ſeems remarkable, 'that Dr. Prieſtley ſhould ſuppoſe it was ni-'trous air dephlogiſticated. Vol. II. p. 121. 'The Doctor ſays,' 'At Mr. Lavoiſier's I ſaw, 'with great aſtoniſhment, the rapid production 'of, I believe, near two gallons of air from a 'mixture of ſpirit of nitre and ſpirit of wine 'heated with a pan of charcoal; and when 'that ingenious philoſopher drew this air out 'of the receiver with a pump, and applied the 'flame of a candle to the orifice of the tube 'through which it was conveyed into the open 'air, it burned with a blue flame; and work-'ing the pump pretty vigorouſly, he made the 'ſtreams of the blue flame extend to a conſi-'derable diſtance.'

'He was ſtruck with the experiment, and 'repeated it when he came into England, uſing 'one-third of the ſpirit of nitre to two of the 'ſpirit of wine; but this experiment does not 'ſeem to have been ſo remarkable. His air 'not being ſo inflammable, nor burning with 'that enlarged flame *.

'Dr.

* The Doctor ſays, vol. II. p. 122. "My firſt idea was, that this air was the ſame thing with the phlogiſticated nitrous air, which I had procured by expoſing pieces of iron or liver

'Dr. Prieſtley calls theſe airs dephlogiſticated, from their reſembling dephlogiſticated reſpirable air, as they admit a candle to burn with that extended flame, and in a briſk vigorous manner.

'It appears to me very ſingular that this vapour ſhould be dephlogiſticated; but let us examine it. The Doctor uſed two-thirds of ſpirit of wine, and one of the ſpirit of nitre, and formed them into vapour by the flame of a candle. But this heat was, by no means, equal to Mr. Lavoiſier's; his heat being from hot charcoal. This vapour, though not ſo inflammable as Mr. Lavoiſier's, conſiſted of theſe two bodies, two parts of alcohol and one of ſpirit of nitre. The inflammability of alcohol I need not mention, being well known to chemiſts; as to the ſpirit of nitre, the Doctor thinks, it contains ſo large a proportion of phlogiſton, as to be of itſelf capable of phlogiſticating common air. Theſe two bodies likewiſe receive a large quantity of heat from the hot charcoal in the courſe of the proceſs, as we have ſhewn that bodies require ſo much to form them into vapour; how then can we call this vapour dephlogiſticated? It is by the ſame proportion of ingredients, and by much the ſame proceſs, that chemiſts make nitrous æther. The vitriolic æther, 'which

liver of ſulphur to nitrous air, the phlogiſton of the ſpirit of wine being, as I ſuppoſed, diſengaged in this proceſs, and becoming incorporated with the nitrous acid, in the ſame manner as the phlogiſton that is diſengaged from the two other ſubſtances."

'which is only the vitriolic acid uſed inſtead 'of the nitrous, is ſo inflammable, that if two 'drops of it be put into two quarts of common 'air, and ſet fire to, it will make the whole ex- 'plode with a loud report, phlogiſticating, as 'Dr. Prieſtley calls it, the remaining air.

'In vol. II. p. 130. The Doctor formed a 'vapour of the ſpirit of nitre and oil of tur- 'pentine, and in order to judge, if an acid pre- 'vailed in this air, (which he calls dephlogiſ- 'ticated) he mixed it with alkaline air; they 'inſtantly formed a white cloud, making the 'nitrous ammoniac as he ſuppoſed; only half 'of the vapour united with the alkaline air, 'the other half, upon applying a candle, made 'a conſiderable exploſion. This experiment 'ſcarce requires a comment. That part of the 'acid, which was not ſaturated with the oil of 'turpentine, formed an union with the alkali, 'and then the other ſtrongly ſaturated with 'the oil of turpentine, being left more at li- 'berty, exploded *. But the Doctor ſays,' 'there was not quite ſo great an exploſion as 'I have obſerved to have been made by a quan- 'tity of nitrous air, of what he now ſuppoſes 'dephlogiſticated.' 'Vol. I. p. 217. The Doc-

'tor

* Vol. II. p. 125. he ſays, "Conſidering this flame with attention, I thought it very much reſembled that which is produced by a mixture of about one-third inflammable air, and two-thirds nitrous air; and concluded, that it was probably compoſed of them both; the nitrous acid forming nitrous air, by ſeizing upon the phlogiſton of ſpirit of wine; and there being a redundancy of inflammable matter, ſufficient to render the air partially inflammable."

'tor at this time formed an hypotheſis of the 'vapours being phlogiſticated, though in his 'latter publication he has adopted the idea of 'its being a dephlogiſticated vapour ‡.

'If we ſhould mix the two ingredients in a 'veſſel, the ſpirit of wine, or the oil of turpen-'tine, and the ſpirit of nitre, would chemiſts 'call it a dephlogiſticated mixture? certainly 'not; then certainly, with far leſs propriety it 'may be called a dephlogiſtic vapour, after it 'has imbibed ſo much of the fire or heat from 'the charcoal or candle, as to make it a per-'manent vapour. In arguing in this manner, 'we can appeal more immediately to our ſenſes, 'and determine with a greater degree of cer-'tainty and ſatisfaction †.

'The three metallic bodies which the hon. 'Mr. Cavendiſh found to produce inflammable 'air with the vitriolic and marine acids, viz. 'iron, zinc, and tin, with the nitrous acid, pro-'duce

‡ Now, agreeable to the Doctor's preſent hypotheſis, the alkaline air muſt have united with the oil of turpentine, and formed the nitrous ammoniac; and the nitrous acid muſt have been left at liberty to have admitted a candle to burn in it as dephlogiſticated air. When I repeated the proceſs I found, that if I made uſe of ſtrong oil of turpentine, and mixed the air with the alkaline, it would explode; but when I uſed weak oil of turpentine, it only burnt like dephlogiſti-cated air, or what I call empyreal air.

† What an idea! that the pure nitrous vapour ſhould be ſuppoſed to be ſo highly phlogiſticated, which they ſuppoſe, from its injuring atmoſpheric air; while the ſame acid, form-ed into vapour with the addition of twice the quantity of ſpirit of wine, and this to be called a pure dephlogiſticated vapour, is certainly wonderful.

'duce this peculiar kind of air. Sometimes it 'will explode, ſometimes burn directly like 'empyreal air, admitting a candle to burn in 'it with great vehemence, and a bright ex- 'tended lively flame, with a crackling noiſe, and 'ſometimes with only a bright natural flame; 'all theſe varieties Dr. Prieſtley found in it *.

S 'He

* Here is a queſtion that particularly forces itſelf upon our judgment; how comes it, if it is a dephlogiſticated nitrous air or vapour, that the ſame bodies, which yield with the moſt acids, viz. the vitriolic, phoſphoric, marine, and vegetable acids, inflammable air, ſhould, with the nitrous, yield a dephlogiſticated nitrous air or vapour? When the other metals which produce ſtrong nitrous air, (which Dr. Prieſtley ſuppoſes to poſſeſs ſo much phlogiſton, equal to inflammable air) with the above acids, vitriolic, &c. will produce no inflammable air. And thoſe metals which produce this dephlogiſticated nitrous vapour, as the Doctor calls it, will even produce inflammable air, by themſelves, expoſed to heat. Certainly there is an obvious deficiency in this theory; for this fact alone implies an error in it. Nay, the inflammable air that they yield with the acids, is more than the dephlogiſticated vapour with the nitrous acid.

But to be more particular, I took two ounces of zinc, and expoſed it to the vitriolic acid, and collected all the air that I could get from it with the aſſiſtance of heat, which was ſtrong inflammable air. I expoſed other two ounces of it to the nitrous acid under the ſame circumſtances, and collected the air I got from it, which was this peculiar nitrous air. I argued, *(a priori)* that if the zinc that was expoſed to the vitriolic acid, which undoubtedly had given out its phlogiſton, would be highly dephlogiſticated; then that which had given out the dephlogiſticated nitrous air would, in courſe, (if the Doctor's theory be true) be highly phlogiſticated. Upon putting the reſiduum of each of theſe experiments into a retort, after mixing them with the nitrous acid, I got purer air, that is, more free of phlogiſton, from that which had yielded the dephlogiſticated nitrous vapour, than from that which had yielded the inflammable air. And this is a teſt agreeable to the Doctor's own experiments and reaſoning, which is incontrovertable.

'He procured this inflammable nitrous air 'by expoſing nitrous air to iron filings and 'brimſtone, in a degree of heat that would 'make the mixture give out its inflammable 'air; and which the nitrous air imbibed. 'But if the mixture is not in that ſtate of diſ- 'charging its phlogiſton, it will decompound 'the nitrous air in the ſame manner as re- 'ſpirable air.

'I have myſelf repeated theſe experiments, 'and am perfectly clear, that if it is new made, 'and put in a cool place, it will decompound 'the nitrous air; but if it be put into a warm 'ſituation, and has been made ſome time, it 'will produce this inflammable nitrous air. 'In the firſt proceſs it has juſt the ſame opera- 'tion upon the nitrous air as the nitrous acid 'has in imbibing its phlogiſton.

'Another proceſs in which the Doctor got 'this air was, by expoſing nitrous air to iron; 'which is performed in this way. The acid 'predominates in the common nitrous air, 'which is diſcovered by its turning the juice 'of turnſole red, and upon admitting the leaſt 'common air to it, giving that rough aſtrin- 'gent acidity to water, which acid, when the 'nitrous air is applied to iron, corrodes it, 'forms into a calx, and is ſaturated with the 'phlogiſton *.

'I once

* Nothing can be more obvious than this experiment. The iron is formed into a calx by the acid, and ſaturates itſelf with its phlogiſton; for how other can we account for it?

'I once had an opportunity of ſeeing a pro-
'ceſs of this kind go on very diſtinctly. After
'making ſome nitrous air, I filled up the phial
'with water, which had been weakly acidu-
'lated with the nitrous acid. The next morn-
'ing, throwing out the water, I added to it
'ſome ſpirit of nitre; and after the phial was
'full of nitrous air, I obſerved an evident ap-
'pearance of more air being generated, and it
'nevertheleſs did not increaſe the bulk of that
'already generated. It muſt have been the
'phlogiſton of the metal diſcharging itſelf, and
'which would be abſorbed by the ſuperabun-
'dant acid of the nitrous air. Dr. Prieſtley
'has given a ſimilar experiment, and with the
'direct ſame reſult in every circumſtance.

'The proceſs by which the Doctor procured
'the greateſt quantity of it, was, by putting
'iron to a ſolution of copper in the nitrous
'acid, he expected to have procured nitrous
'air, but he procured in great abundance this
'kind of air; which bears an eaſy explanation.
'In diſſolving iron in the nitrous acid, the
'Doctor procured firſt the nitrous air; but
'when no more would come, on applying a
'candle, he procured this air.

'I obſerved before, that the vitriolic acid,
'which is not ſo volatile with iron, produces
'inflammable air; therefore this part of the

 'nitrous

it? Agreeable to Dr. Prieſtley's theory of the air's being de-phlogiſticated, what has become of the phlogiſton of the iron upon its being reduced to a calx, and the phlogiſton of the nitrous air upon its being dephlogiſticated? nothing can be more obvious.

'nitrous acid, which was not evaporized ſo 'ſoon, but was attracted by the phlogiſton, 'and there fully ſaturated itſelf with it in a 'ſlow and gradual manner, not having a rapid 'generation of heat to diſcharge it immedi-'ately. Dr. Prieſtley obſerved he got more 'nitrous air by a quick efferveſcence than a 'ſlow one; a quick efferveſcence generates a 'ſtronger heat, and therefore volatilizes more 'of the acid. Now, we ſhall not find this to be 'the caſe here. The acid being attracted by 'the iron, but being already united to the 'copper, leaves it very ſlowly, and being a 'weak ſolution with a large quantity of wa-'ter in it; ſo that as the heat is generated it 'will help to depreſs it; the acid, therefore, 'will act ſlowly and gradually upon the iron, 'and by that means it will not form air till it 'is highly ſaturated with phlogiſton; it being, 'as Dr. Prieſtley obſerves, produced in a gra-'dual ſlow manner *.

'After what has been ſaid, it may appear a 'little ſurpriſing, that the learned Doctor 'ſhould adopt the hypotheſis of its being a 'dephlogiſticated vapour. But its great re-'ſemblance to empyreal reſpirable air might 'incline him to embrace that opinion; and al-'ſo, becauſe the flame of a candle was ſupport-'ed in the ſame bright manner as empyreal air 'does. To have adopted the contrary opinion 'would

* As iron contains more phlogiſton than copper; therefore it is the ſurplus, which remains after reducing the copper, which forms this air; otherwiſe what comes of it?

'would have ſhaken the great hypotheſis, 'which, not only Dr. Prieſtley, but moſt philo-'ſophers, have eſpouſed. There is no doubt 'but animal, vegetable, and the life of flame, 'are ſupported by the very ſame principles in 'reſpirable air; therefore, to give up the life of 'flame, would be to give up the whole. Let 'us, however, more maturely conſider the 'foundation of this doctrine.

'Dr. Prieſtley, agreeable to his hypotheſis, 'ſuppoſed that this particular nitrous air poſ-'ſeſſed a nitrous vapour, which attracted the 'phlogiſton, which he imagined was produced 'by the flame, and by that means ſupported it. 'But, upon examination, it appears juſt the re-'verſe; for we have proved this nitrous va-'pour in the ſame degree that it is neutralized 'ſupports the flame with more vigour; and 'as it will be found in the Doctor's experi-'ments, which he himſelf acknowledges, (ſee 'vol. III. p. 143.) that when it is in its higheſt 'and moſt perfect ſtate, it then ſhews not the 'leaſt ſign of an acid vapour, neither in its 'burning nor by the niceſt teſt that chemiſtry 'is maſter of, by the juice of turnſole *. And 'in the experiment mentioned p. 58. when the 'alkaline air neutralized the nitrous vapour, ſo 'as to form the nitrous ammoniac, that it then 'became ſo inflammable as to explode with a 'great

* And this teſt, the Doctor himſelf and all chemiſts allow, is the niceſt and moſt certain; for by it, the impregnations of fixed air in water we can always detect, and certainly it is an acid conſiderably weaker than the nitrous.

'great report. And certainly as the ſtrongeſt 'attraction exiſts between alkalies and acids, 'if dephlogiſticated nitrous air was formed of 'a nitrous acid vapour, that alkaline air would 'unite with it.

'Dr. Prieſtley, in his ſupplemental obſerva- 'tions, mentions a fact in ſupport of his hypo- 'theſis. I take this opportunity of remarking, 'that as my motive is to inveſtigate truth, I 'would not be thought ſo diſingenuous as to 'ſuppreſs any experiment that may ſeem to 'militate againſt my own opinion. No doubt 'it would have given me a degree of confi- 'dence to have coincided with ſo ingenious a 'philoſopher; but as truth ought to be the 'object of every one's enquiry, and as the 'Doctor's liberality of ſentiment is ſo well 'known, I ſhall make no farther apology for 'the preſent inveſtigation.

'The fact I allude to is, that nitrous air ex- 'poſed to the ſcales of iron, which Dr. Prieſt- 'ley ſuppoſes a partial calx, produces this air; 'but if it is not wholly ſo, if there is ſtill a part 'of it metal, it will be ſufficient to give it phlo- 'giſton; and why does not the perfect calx it- 'ſelf form this air? And how comes the iron 'to be reduced to a calx by forming this air? 'And beſides, if the Doctor had expoſed theſe 'ſcales of iron to diluted oil of vitriol, he would 'have obtained inflammable air.

'The Doctor ſays, vol. V. p. 344. in the 'ſummary view of his principal facts,' 'It is 'procured from iron filings and brimſtone, by

'nitrous

'nitrous air, before it becomes phlogiſticated 'air.' 'This obſervation of the Doctor's ſhews 'that, when the air is perfectly phlogiſticated, 'its acid being neutralized, then it cannot pro- 'duce this air, having no acid to unite with 'the inflammable air which eſcapes from the 'mixture of iron and brimſtone. This mix- 'ture newly made, as I obſerved before, will 'equally decompound nitrous and atmoſphe- 'rical air; but if it is in a ſtate of giving out 'its inflammable air, it will make both theſe 'kinds of air inflammable. (See Prieſtley.)

'Dr. Prieſtley found that this air would 'ſometimes injure common air, and ſometimes 'not; he could not ſay poſitively whether it 'would or not. But his experiments evince 'this, that when it burns with a blue nitrous 'flame, then it would affect common air; but 'when it did not, it would not have the leaſt 'action upon it *. We ſhall find in the courſe 'of this treatiſe, that it is the acid that injures 'reſpirable air; therefore, whenever it is fully 'neutralized with phlogiſton, it in courſe has 'no effect.

'In demonſtration of this, I took a quantity 'of this air in which the acid predominated, 'and expoſed it to common air, and it injured 'it. There was a decompoſition. Then I took 'ſome of the ſame air from the ſame proceſs, 'and expoſed it to iron filings and brimſtone 'in

* Its burning with this blue nitrous flame is a direct indication of the nitrous acid; for by, adding the nitrous vapour to any inflammable air it will make it burn in this manner.

'in a heat that would diſcharge their inflam-
'mable air; and after a little expoſure I found
'the air had loſt the quality of injuring com-
'mon air. After this impregnation, I took
'part of it, and mixed it with ſome of the ni-
'trous vapour made from the pure nitrous
'acid, and it regained its power of injuring
'reſpirable air. I have done this alternately
'many times together; for when nitrous air
'has loſt its power of turning the juice of turn-
'ſole red, it will have loſt its power of injur-
'ing common air.

'But before the iron filings and brimſtone
'give out their inflammable air, if they have
'ſo far abſorbed the nitrous air as to have im-
'bibed all its acid, leaving a reſiduum, which
'will neither ſhew the principles of this nitrous
'air nor common nitrous air; (the properties
'of which we ſhall elſewhere explain) then
'if they give out their inflammable phlogiſtic
'matter, there is no acid to unite with it;
'therefore the nitrous air will not form this
'ſpecies of air. But Dr. Prieſtley has given
'us a very accurate deſcription of the forma-
'tion of this air, by which we ſhall be able to
'judge of its analyſis. Expoſing the nitrous
'air to a mixture of iron filings and brimſtone
'over ſome pure water, he found that the acid
'was ſeized on in this proceſs, and not the
'phlogiſton of the nitrous air that was expoſed
'to this mixture, in making this particular ſpe-
'cies of air in which a candle will burn. But
'we ſhall give the Doctor's own words, vol. III.
'page

'page 142 *.' 'I ſhall now proceed to note 'other phenomena attending this proceſs, 'which the philoſophical reader will eaſily per-'ceive, may be of great uſe in the analyſis of 'this ſpecies of air, as well as help to explain 'the proceſs itſelf.' '(The proceſs which the Doctor means, is expoſing of the nitrous air to a mixture of iron filings and brimſtone, and the air becoming what he calls dephlogiſticated.)' 'In order to determine whether the *acid*, or *phlogiſton*, of the nitrous air had been ſeized upon in this proceſs, I made it over a quantity of very pure water; thinking that, if it ſhould acquire any acidity, it would ſhew that the phlogiſton only had been ſeized; but that if it ſhould not have become acid, it would appear that the decompoſition had been effected by the acid having been ſeized, and the phlogiſton left: And the reſult ſeems to determine this queſtion in favour of the latter ſuppoſitiou. For, when the proceſs was over, I examined the water with the greateſt attention, and found not the leaſt appearance of acidity in it. It did not even turn the juice of turnſole red; and I do not know a more accurate teſt of ſmall degrees of acidity than this. On the other hand, in the decompoſition of nitrous air by nitrous acid, the water

* With Dr. Prieſtley's explanation of this proceſs, that the acid is ſeized on by the mixture, and the phlogiſton left; how then can he poſſibly call it dephlogiſticated air, when his opinion is, that even nitrous air poſſeſſes ſo large a quantity of phlogiſton? certainly then it muſt poſſeſs more in proportion, when it has loſt its acid, and retains its phlogiſton.

'over which the diminution was made, be-
'comes much more acid than can be account-
'ed for by the evaporation of the acid intro-
'duced into it *.'

'This perfectly agrees with our hypotheſis
'that the phlogiſton emitted from this mixture
'and from iron, &c. ſeizes upon the acid of the
'nitrous air, neutralizing it ſo, that it could
'not acidulate the water, and making this pecu-
'liar nitrous air, in which a candle burns ſome-
'candle

* But we ſhall relate another experiment of the Doctor's, which equally proves the ſame. He ſays, vol. III. p. 146, "That *liver of ſulphur* alſo decompoſes nitrous air, by ſeizing upon its acid, ſeems to be proved by the following experiment. I put ſome pieces of liver of ſulphur to two quantities of nitrous air confined by quickſilver, and I obſerved that, in about ten hours, between one third and one half of the air in each of them was abſorbed. The next day I admitted water to one of them, but no part of the air was abſorbed and after it had paſſed ſeveral times through water, a candle burned in it with an enlarged flame, crackling very much. Alſo a candle burned in the very ſame manner in the air contained in the other veſſel, which I had not made to paſs through water. In this experiment I took particular notice that there was no appearance of the mercury having been corroded, or having been in any other reſpect viſibly affected during the proceſs; as it would have been if every thing acid in the nitrous air had not been united to the liver of ſulphur. Perhaps the reaſon why the air in this experiment bore the paſſage through water without loſing its property of admitting a candle to burn in it was, that, by ſtanding a long time out of water, the conſtituent parts of it had acquired a firmer union, ſo as not to be eaſily ſo decompoſed by the acceſs of water aftewards."

"A more deciſive proof of this decompoſition being effected by the *acid* of the nitrous air being ſeized upon, is, that the water over which the decompoſition is made, does not acquire the leaſt acidity, not even diſcoverable by the juice of turnſole. In this caſe, alſo, there was an *earthy precipitate*, exactly as in the proceſs with the iron filings and brimſtone."

'times with a natural flame; ſometimes with 'an enlarged flame; and ſometimes with a 'vehement and crackling noiſe like dephlogiſ-'ticated reſpirable air, as if it contained inflam-'mable matter; and ſometimes perfectly in-'flammable, ſo as to explode like genuine in-'flammable air. If the acid was abſorbed be-'fore the mixture emitted its phlogiſton, then its 'phlogiſton would form, with the reſiduum of 'the nitrous air, the common inflammable air.

'That this peculiar nitrous air contains 'more phlogiſton than the common nitrous 'air, is proved from a determined quantity of 'it phlogiſticating a certain quantity of the 'nitrous acid, more than an equal proportion 'of the nitrous air would have phlogiſtcated it.

'The Doctor in his fourth volume, p. 455, 'mentions a ſtriking fact of the nitrous air 'turning inflammable, which is a follows:'

'I have mentioned a caſe, vol. I. p. 217, in 'which nitrous air, after having been expoſed to 'iron, became, not only partially inflammable, 'admitting a candle to burn in it with an en-'larged flame, but was even fired with an ex-'ploſion, like inflammable air from metals by 'oil of vitriol. I have ſince met with a more 'remarkable fact of the kind.

'At the latter end of September 1778, I had 'put a pot of iron filings and brimſtone into 'a jar of nitrous air, which, in the courſe of 'ſeveral days, was diminiſhed by it in the 'uſual proportion. From that time till the 'beginng of December it had continued with-

'out any change that I had perceived; but 'about that time, imagining it was increaſed 'in bulk, I took exact notice of the dimen-'ſions of it, and preſently found that the quan-'tity was certainly increaſing. Upon the 'whole, I concluded that it had increaſed 'about one ſixth of its bulk, from the ſtate of 'its greateſt diminution. On the 11th of De-'cember I examined it, and found it to be 'proper inflammble air, being fired with many 'exploſions when tried in the uſual manner, 'but they were not ſo vigorous as thoſe with 'freſh made inflammable air from iron and 'oil of vitriol.

'After this, on the 12th of December, I put 'a pot of iron filings and brimſtone to another 'quantity of nitrous air, and on the 4th of 'February following it had increaſed in bulk 'about one third, and then burned with ex-'ploſions like the former.'

'The Doctor's opinion of theſe experiments, 'as we have obſerved, was, at firſt as every one 'muſt think, that this nitrous air was phlogiſ-'ticated. He ſays, vol. III. p. 24.' 'If that 'which conſtituted the enlarged flame (ſpeak-'ing of this air) be phlogiſton contained in the 'air itſelf, (and indeed it can hardly admit of 'any other ſuppoſition) it muſt, &c.' 'In 'other parts of his works he directly gives the 'ſame opinion. But what biaſſed him to the 'contrary opinion was, its perfect relemblance 'to what he calls dephlogiſtic reſpirable air; 'therefore the theory that he had formed con-

'cerning

'cerning this air, made him adopt this ſingular 'opinion in oppoſition to theſe plain, obvious, 'and unanſwerable facts. For this dephlogiſti-'cated nitrous air, can be formed into inflam-'mable air, as he himſelf found. He ſays, vol. 'I. p. 57.' 'By a very eaſy proceſs I can al-'ways make inflammable air from the nitrous 'acid, viz. by putting iron or liver of ſulphur 'into nitrous air.' 'In moſt caſes the nitrous 'air in theſe proceſſes only changes to that 'ſtate in which a candle burns with a bright 'crackling flame; but if longer continued it 'will ſometimes become inflammable.

'As this is an indiſputable fact; then if we 'ſuppoſe all kinds of air which will make a 'candle burn are dephlogiſticated, we muſt 'be under the neceſſity likewiſe of ſuppoſing, 'inflammable air to be a dephlogiſticated air, as 'they are poſitively one and the ſame air, only 'the one being a richer air of the kind from a 'longer continuance of the proceſs.

'It will appear ſingular, that this obvious 'fact of this nitrous air being phlogiſticated, 'ſhould have been diſputed, and likewiſe that 'I ſhould have taken ſo much pains to prove 'what is ſo clear. But it is to decide this 'important queſtion, how theſe airs ſupport 'flame? For the eſtabliſhed opinion that air 'ſupports flame from imbibing phlogiſton, 'made Dr. Prieſtley ſuppoſe that this air was 'dephlogiſticated. But we ſhall in the courſe 'of this work ſhew, that that opinion is equally 'as ill founded.

'Dr.

'Dr. Prieſtley being ſo impreſt with the opi-
'nion of this air being dephlogiſticated, thinks,
'agreeable to his general theory, that its being
'capable of ſupporting flame with ſo much
'vigour is owing to a nitrous vapour, which
'muſt have come from the acid being dephlo-
'giſticated. But to wave the convincing ar-
'guments, which we have juſt given to ſhew
'the contrary, let us ſee how far this idea may
'be ſupported.

'If the nitrous air is dephlogiſticated, ſhould
'we not have expected to have found it acidu-
'lated? But wonderful! that it ſhould ſhew
'an acidity before, but now it is not to be de-
'tected by the niceſt teſt. The Doctor ſays,
'vol. III. p. 143.' 'The inveſtigation of the
'nature of this vapour, (which he ſuppoſes
'unites with the phlogiſton, and from that ad-
'mits a candle to burn in it) which though
'derived from the nitrous, is not of itſelf acid.'
'If there is a fact in chemiſtry, this is one,
'that phlogiſton neutralizes acids; therefore,
'if I take phlogiſton, either from the nitrous
'acid in its condenſed ſtate, or in its ſtate of
'vapour, it will make its acidity ſtronger. To
'ſuppoſe we ſhould find the acid neutralized
'after taking phlogiſton from it, is a ſingular
'opinion indeed; but (in our theory) to ſup-
'poſe that phlogiſton can perfectly neutralize
'an acid vapour, which has received a great
'quantity of phlogiſton before, is agreeable to
'the firſt rudiments of chemiſtry, which have
'never been queſtioned.

'Theſe

'Theſe arguments will hold equally in all 'points, with the empyreal air made from the 'nitrous acid and earth. It likewiſe ſhews no 'indication of acidity, either to the delicate 'teſt of the vegetable juices, or the alkaline air 'uniting with it. And juſt from the very 'ſame opinion, and the very ſelf ſame argu-'ments, they ſuppoſe it the nitrous acid de-'phlogiſticated, and formed into an air. But 'we ſhall find, that in catching *ſuppoſed novel* '*facts*, they have loſt ſight of the firſt rudi-'ments of chemiſtry? for we ſhall, I hope, 'make it clearly appear that that *ſuppoſed fact* 'of the reſpirable air's being phlogiſticated 'when injured, is equally as fallacious as 'their other opinion, in ſuppoſing this ni-'trous air dephlogiſticated. It certainly muſt 'appear a very ſingular hypotheſis, when it 'makes them ſuppoſe that the pure nitrous va-'pour, formed from the dephlogiſticated ni-'trous acid, ſhould be ſo highly phlogiſticated, 'injuring the common air, (Dr. Prieſtley ſhews 'that it injures it as much as nitrous air *) and 'that a vapour formed of one part of nitrous 'acid,

* The Doctor, vol. III. p. 193, ſays, "Laſtly, I found, in the courſe of theſe experiments, that the power of this red vapour, to phlogiſticate common air, was much greater, and acted much quicker, than I had imagined when I made the firſt obſervation of the kind. For, after the former obſervations, I filled another phial with the red vapour, and immediately afterwards opened it under water; when the water, ruſhing in, filled about half of it, and the remaining air was found completely phlogiſticated, not being in the leaſt affected by nitrous air." The vapour, here named, is the pure condenſable nitrous vapour.

'acid, and two parts of oil of turpentine, or 'ſpirit of wine, would be perfectly dephlogiſ-'ticated like empyreal air; nay, that it ſhould 'become inflammable and explode, and yet be 'dephlogiſticated. Is it poſſible to reconcile 'ſuch contradictions? I ſhould think, the 'theory that leads them to theſe opinions, muſt 'certainly be erroneous.

'I ſhall conclude this chapter with the fol-'lowing ſtriking facts and obſervations:

'We cannot help ſtill remarking the very 'ſingular deductions and calculations which 'are made, and may be made, from this theory 'of reſpirable air, when injured air is phlo-'giſticated. Dr. Prieſtley ſuppoſes that two 'parts of ſpirit of wine, and one of the nitrous 'acid, formed into an air, is a dephlogiſticated 'air. Now, two drops of æther will make a 'quart of common air explode, and the reſi-'duum will be injured *. When, agreeable 'to Dr. Prieſtley, this quart of injured air muſt 'have received phlogiſton in quantity equal to 'what a pint of inflammable air contains †; 'and all this muſt have come from the two 'drops

* Dr. Ingenhouſz ſays, "I have, perhaps, fulfilled theſe conditions as near as poſſible; for all the inflammable air neceſſary for a piſtol, ſuch as Mr. Volta contrived, is contained in the ſpace of one ſingle drop of a liquid. So that a pint bottle may contain as much inflammable air, exiſting as it were, in a concentrated ſtate, (meaning æther) as is required to fire an air piſtol many thouſand times."

† Dr. Prieſtley ſays, that inflammable air and nitrous air contained equal quantities of phlogiſton, from their injuring atmoſpherical air equally alike, which is in the proportion of one meaſure of theſe kinds of air, to two of atmoſpherical air.

'drops of æther. But yet, which is more extraordinary than all this, the nitrous æther and vapour, (formed of the very ſame materials and proportions, only the nitrous acid inſtead of the vitriolic ‡) they ſay is dephlogiſticated, nay ſo highly ſo as to make the moſt perfect dephlogiſticated mixture, equal to what they call dephlogiſticated air. If the æther is mixed with empyreal air, theſe calculations will appear more extraordinary.

'If I fire gunpowder in fixed air, it will explode; if I fire it in atmoſpherical air, the fire and exploſion will be ſtronger. If in the nitrous phlogiſticated, or the common empyreal air, it will ſtill explode more vigorouſly, and a far greater quantity of fire and light will come from it. Now, is it not rational to ſuppoſe that all its fire came from it when fired in the fixed air, and the extraordinary fire and light that came from it when fired in the phlogiſticated nitrous and empyreal air came from theſe airs; their fire and light being decompounded. That it is ſo, we are certain from both phlogiſticated nitrous and

‡ Dr. Ingenhouſz ſays, Prieſtley, vol. IV. p. 478. "As I make no doubt but this air (meaning the inflammable air made by æther being dropt into common air) is the ſame that might be extracted from oil of vitriol and ſpirit of wine by heat, I will give you the following account of the ſpecific gravity of different inflammable airs compared with common air, with which account I was favoured by Mr. Enee: A veſſel containing common air to the weight of 138 grains; will contain of inflammable air extracted from iron, 25 grains, of air extracted from marſhes, 92 grains; and of that extracted *from oil of vitriol and ſpirit of wine*, 150 *grains*."

'the empyreal air before not ſhewing the leaſt 'ſign of acidity, either with the juice of turn-'ſole or alkaline air, will now after the ex-'ploſion neutralize the latter, and turn the for-'mer red; therefore their acids, which were 'undoubtedly neutralized by the light and fire 'after it's diſcharge, will be free and predomi-'nant. The phlogiſton which undoubtedly 'neutralized the nitrous acid, will have been 'diſcharged as fire and light, and therefore 'the exploſion was ſo much louder.

'Dr. Prieſtley's two experiments in p. 68, 'and 69, likewiſe ſeem to ſhew that the ſuper-'fluous acid of the nitrous air is attracted by 'the liver of ſulphur, &c. and the phlogiſton 'is left, and for that reaſon the candle will 'burn in it, and will ſometimes explode.'

The arguments concerning the nature of hepatic air, when mixed with nitrous air, cannot, I think, be anſwered. Mr. Kirwan has given an excellent paper upon hepatic air, in which he clearly ſhews that it is ſulphur in an aerial ſtate; and that if nitrous and hepatic airs are mixed together, a conſiderable decompoſition will take place. See Philoſ. Tranſ. vol. lxxvi. p. 134. Let us accurately attend to this experiment. The nitrous air is ſuppoſed to be formed of phlogiſticated air, and of a little dephlogiſticated air, or the oxygen principle. Now, theſe two airs receive a great decompoſition in the proceſs; a quantity of ſulphur being depoſited, and the reſiduum, admits a candle to burn in it with a very

vivid

vivid flame. The ſulphur, conformable to the doctrines of modern chemical philoſophers, muſt have attracted the phlogiſticated air of the nitrous air, leaving the oxygen principle: But ſulphur has no attraction for phlogiſticated air; for, in burning ſulphur in atmoſpherical air, the dephlogiſticated, and not the phlogiſticated air is attracted: The ſulphur and dephlogiſticated air being ſuppoſed to form the vitriolic acid.

To ſuppoſe, that nitrous air conſiſts of dephlogiſticated and phlogiſticated airs, muſt appear very extraordinary, when we reflect, that theſe two airs are ſo abundantly generated in nature, by vegetables and animals, and by art; ſo that there is ſcarce a proceſs which produces the one, but alſo produces the other: viz. the air from nitre, &c. That nitrous air is not formed of theſe two airs, is clear, from the experiment of paſſing this air through hot earthen tubes. The nitrous air in this proceſs will be all turned to phlogiſticated air.

Every old chemiſt, who has the leaſt knowledge of the affinities of bodies, muſt be ſtruck with ſurpriſe to ſuppoſe, that ſuch highly phlogiſtic bodies as ſulphur, the cauſtic alkali aerilized, and nitrous air, ſhould form ſo large a reſiduum of dephlogiſticated air. Mr. Kirwan's experiments convince him, that it is the acid which produces the precipitation of the hepatic air, and therefore, he neutralizes it with alkaline air. But Mr. Kirwan will find another fact, which is, that the more the

alkaline air is employed in this proceſs, the more eaſily will the air admit a candle to burn in it with greater vividneſs: But it ſhould have been the reverſe. In a word, the ideas, which moſt of our modern chemiſts entertain of phlogiſtication and acidifying airs, are the very reverſe to what they have ſuppoſed; becauſe, in direct oppoſition to this old chemical fact, that alkalies and phlogiſton neutralize acids. And this other important fact may be added; namely, that if this dephlogiſticated nitrous air is imbibed by the pure nitrous acid, it will highly phlogiſticate the acid; whereas it ſhould have dephlogiſticated it.

I ſhould think enough has been ſaid to convince every chemiſt unprejudiced by the theories of modern chemiſtry; though alas! they appear not diſpoſed to ſee the truth.

Having formed nitrous air from the acid and vegetable oils, I added a quantity of alkaline air to it. Two motives induced me to do this. To neutralize and decompound that part of the acid, which was but looſely attracted; and likewiſe, to neutralize that part of the acid, which would ſtill adhere to the nitrous air. And I proved, by experiments, that part of the alkaline air was precipitated as nitrous ammoniac, and likewiſe, that part ſtill adhered to the nitrous air; and, upon examining the air, I found it was what is called the dephlogiſticated air, admitting a candle to burn in it. But, need chemiſts be told, that it is nitrous air more highly phlogiſticated? Nay, in this

this ſame proceſs, it is ſometimes ſo highly phlogiſticated, as to explode like the pure inflammable air; burning with ſuch vivid flaſhings, (as Dr. Harrington juſtly obſerves) as to reſemble partial exploſions. I have found in the nitrous air from vegetable oils, a quantity of fixed air; and this fixed air, upon alkaline air being added to it, is not precipitated, but united to the alkaline air: And they likewiſe ſeem to add to the quantity of the air, which allows a candle naturally to burn in it.

After having received ſo much phlogiſton or alkaline air, as is ſufficient to deprive it of the power it had when nitrous air, of turning litmus red, it then becomes oxygen gas; and this oxygen gas is ſuppoſed to reſide in the acid, ſo as to produce its acidity; for as it loſes that, it becomes more like nitrous air, or the acid leſs conſpicuous; but it ought to have been the reverſe; and it is evident that, when it has this quality of admitting a candle to burn in it, it becomes leſs acid; the acid being more ſaturated or neutralized with phlogiſton, or alkali, which is the ſame as phlogiſton; but only (as Dr. Harrington has proved) leſs concentrated.

Can any one poſſibly be miſtaken of this nitrous dephlogiſticated air, even from Mr. Kirwan's hiſtory of it? Indeed, after reading Dr. Harrington's account, it was impoſſible for Mr. Kirwan, or any other chemiſt, who was in the leaſt acquainted with chemical principles, to ſuppoſe it was dephlogiſticated: But

that

that diſpoſition, which has been the ruling mark of our *aerial philoſophers*, is, to make it a point not to name Dr. Harrington. What does Mr. Kirwan do? he does not make or call this air dephlogiſticated, but calls it deacidified. We have got a number of new terms into chemiſtry from their extraordinary ideas of it. But I ſuppoſe he means by this, the air is more neutralized; could he not have ſaid, agreeable to Dr. Harrington, more phlogiſticated. But even to take his own term, deacidified; what bodies were there to deacidify it but the ſulphur and alkaline air? And as, according to their hypotheſis, air that will admit of the life of combuſtion, (call it dephlogiſticated, deacidified, or what they will) it is, they ſay, the aceſcent principle or principles of acidity. Then, muſt not it appear to form a chaſm in reaſoning, to ſuppoſe that an air which has got its acid taken from it, ſhould, from that cauſe, become the aceſcent principle? But ſuch are their abſurdities.

But to proceed with the arguments. Nitrous air expoſed to phoſphoric hepatic air, and treated in the ſame manner as the common hepatic air, will likewiſe become the dephlogiſticated or deacidified air of modern chemiſts. But then, if this phoſphoric hepatic air is burned with atmoſpherical air, which it will do without the aid of any other combuſtible matter; the atmoſpherical air will be left in the very ſame ſituation, as if any other combuſtible body had been burned in it; or,

if

if the phlogiſton of phoſphorus or alkali had been burned in it. Thoſe who oppoſe this true and not obſcure doctrine, cannot require more convincing proofs; but hitherto they have ſhut their eyes: This is an important fact. There is an air, called deacidified or dephlogiſticated, (for modern theoriſts have given it both theſe names) that acts upon atmoſpherical air, in the ſame manner as any combuſtible body. Now, this is a good proof in favour of Dr. Harrington's theory, that atmoſpherical air and combuſtible bodies, are both phlogiſtic bodies, ſince the phoſphoric hepatic air acts in one proceſs, in the ſame manner as dephlogiſticated air, in admitting, the ſame as atmoſpherical air, a candle to burn in it: And in the other proceſs, it acts upon atmoſpherical air, juſt as a candle would do by burning in it, and making it foul air, and that to a conſiderable degree; a ſmall quantity of phoſphoric air, acting upon a large quantity of atmoſpherical air *.

One of the moſt ancient, and beſt eſtabliſhed facts in chemiſtry is, that acids and alkalies attract each other. Our modern chemiſts have done all they could to deſtroy this fact; nay, by introducing new principles, they have hewn it down, with all the reſt, as a cumberer of the ground.

* The phoſphoric and dephlogiſticated airs acting, agreeable to our hypotheſis, in one caſe as the agent, in another as the principle; therefore, certainly we muſt ſuppoſe, that there is not that difference in the compoſition of the agent and principle.

ground. Modern experiments and theories ſay, that alkalies do not attract acids. But heat, which is their great agent in finding out the affinities and compoſitions of bodies, they ſay, proves it: For, in expoſing nitre to heat, the alkali attracts the phlogiſticated air, and the oxygen principle is aerilized. But this is more particularly ſingular in the nitrous ammoniac, as the volatile alkali is ſo volatile. Yet, they ſay, by expoſing the nitrous ammoniac to heat, the decompoſition of the acid takes place, its phlogiſticated part being attracted by the volatile alkali; and this not only occaſions the loſs of the alkali's attraction for acids, but even the loſs of its volatility. But to advert to experiments, which, in the opinion of aerial chemiſts, can explain every thing.

Upon expoſing the nitrous ammoniac to heat, a very great abundance of this dephlogiſticated nitrous air is generated. But if chemiſts would wiſh only to ſee truth, let them take this ſame air produced from the nitrous ammoniac, and expoſe it to the dephlogiſticated nitrous acid, and then they will know, whether their theory, or that of Dr. Harrington, is juſt. If the air is dephlogiſticated, upon the acid's imbibing it, the acid will then become more acid or dephlogiſticated; but if Dr. Harrington's theory is juſt, it will become the ſame, as if ſo much of the volatile alkali, and a little nitrous acid, (which he ſays the air contains) were added to it. Upon expoſing them together, the acid imbibes the air, ſo as to produce

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the very ſame phenomenon, as if I had added a quantity of volatile alkali to it. Or, if this ſame dephlogiſticated air is expoſed to the calx of iron, the calx will attract it; and upon examining the calx, it will be found to have imbibed the pure nitrous ammoniac. In burning the calx, it will injure or phlogiſticate, as they call it, atmoſpherical air, juſt the ſame as the nitrous ammoniac.

Can aerial chemiſts, after this review paſs by Dr. Harrington's theory, as not deſerving notice? If they do, it is evident, they are not willing (however much convinced in their own minds) to acknowledge to the world, that they have been miſtaken. But chemical philoſophers who will not attend to truth, when it is told them, do not deſerve the name.

This ſingular theory of theirs deſtroys every old chemical fact. The vegetable juices turn red by being expoſed to an acid. This is a teſt by which to know, whether or not, nitrous air is more acidified, when it is dephlogiſticated. Litmus, by being expoſed to nitrous air, turns red; but if this nitrous air is formed into dephlogiſticated air, it will not turn the litmus red, but rather green. Can this extraordinary doctrine be ſupported by any fact? For, when this dephlogiſticated air, as they call it, is expoſed to the ſtrong concentrated nitrous acid, the ſtronger acid will attract the alkali from the weaker; becauſe the acid, by being phlogiſticated, loſes it's ſtrength. This

is the caſe, when the nitrous acid is phlogiſticated in a condenſed ſtate; for, by adding a ſtronger, or dephlogiſticated nitrous acid to a body which has attracted it, it will expel it.

Every chemical proceſs is a proof to confirm this truth, that fire is chemically united to the different acids; particularly when acids are mixed with earths or alkaline ſalts. When the vitriolic acid, metallic, or other earths, are expoſed to a ſtrong heat, they become aerilized into a high phlogiſtic air, improperly called dephlogiſticated: But will alſo yield under the ſame proceſs, phlogiſticated and fixed air. The vitriolic acid, conformable to the preſent theories, is only a compound of dephlogiſticated air and ſulphur, and therefore ſhould yield neither fixed nor phlogiſticated air; and ſurely, they will not allow the earth of alum to be a compound of either of theſe airs. The nitrous acid will alſo, with the different earths, yield theſe airs; and the marine acid is ſo ſtrongly attracted by theſe earths, that no heat can comparatively volatilize it. A ſtrong heat is required to volatilze the phoſphoric acid; and therefore, the air which it produces, will be inflammable air. See Dr. Prieſtley. With the calx of lead and this acid, he produced nothing but inflammable air, or air of a higher impregnation: But obtained it in great abundance.

The phoſphoric acid, conformable to Mr. Lavoiſier, conſiſts of 69 parts of dephlogiſticated air, and only of 31 of its peculiar baſis;

and,

and, conformable to Mr. Kirwan, of 69 of fixed air, and of 31 of its baſis. Now, if this acid is diſtilled with the calx of lead, minium for inſtance, inflammable air only is produced; all the dephlogiſticated air, which the phoſphoric acid and the minium are ſuppoſed to poſſeſs, has diſappeared. What is become of it? It cannot be in the lead; for, if the fire is puſhed, the lead is reduced. Is not this hoſtile to their hypotheſis, and friendly to that of Dr. Harrington? That gentleman has called upon them to explain theſe phenonomena; but they have not, nor can they explain them.

The vegetable acid, which is ſo much weaker than the preceding ones, forms with earths, only phlogiſticated air, or air with a leſs impregnation of fire. The aerial acid united to earths, as in lime-ſtone, magneſia, &c. forms, by means of a ſtrong heat, different kinds of air; viz. fixed air, phlogiſticated air, dephlogiſticated air, and inflammable airs. But according to their hypotheſis, it ſhould form only dephlogiſticated air, phlogiſton or carbone. The electric ſpark taken in fixed air, will produce all theſe different airs, the ſame as lime-ſtone or magneſia. In all theſe productions, the airs generated, ſhew very forcibly, that this is owing to the concentration of fire. For the airs are produced in that regular order, as is conformable to the quantity of fire they poſſeſs. As a proper example of this, let us take the air from lime-ſtone. Firſt, fixed air is

expelled without any impregnation; then phlogiſticated air, with ſufficient to neutralize the acid; then dephlogiſticated air, with a ſtill higher impregnation; and at laſt inflammable air, with the higheſt concentration. But fixed air, when united to the calx of mercury (which, conformable to Dr. Harrington's hypotheſis, is more eaſily aerilized, and therefore more adapted with acids, to form air with a higher concentration of fire) will form only dephlogiſticated air.

There is a proceſs, by which fixed air may be all formed into a phlogiſticated air: This is done by impregnating water with it, and then expelling it again by heat. There will always be a reſiduum of this phlogiſticated; and by repeating the proceſs upon the ſame air, you at length bring it all to this kind of phlogiſticated air. As there is nothing but water and heat uſed in the proceſs, we cannot poſſibly miſtake it. It could have received only theſe two bodies: Moreover, the quantity exactly correſponds with receiving only a ſmall addition, conformable to the alteration which theſe two bodies would make upon it. Dr. Prieſtley likewiſe, by expoſing it to phlogiſtic proceſſes, turned it to this air.

But, a very deciſive experiment, an experiment impoſſible to be miſtaken, is, the paſſing fixed air through heated manganeſe. By repeatedly paſſing, it becomes at laſt dephlogiſticated air. That phlogiſticated air is a medium, between fixed and dephlogiſticated air,

is

is thus proved: It firſt becomes phlogiſticated, before it forms what they call dephlogiſticated air, receiving each time in paſſing more concentrated fire, till it at laſt becomes highly dephlogiſticated: This we have elſewhere more fully explained.

Now, I aſk thoſe who have the leaſt knowledge of chemiſtry, Can they poſſibly miſtake facts ſo obviouſly plain? It is impoſſible, if we retain true notions of what is juſt.

If I add oils, volatile alkali aerilized, or phlogiſton in any other form, to the mineral acids, the nitrous, vitriolic, and marine airs will be formed: But theſe airs, ſay they, are the acids, having part of their oxygenous principle taken from them. But, as I before obſerved, if theſe airs are abſorbed by the ſame acids, from which they were generated, they will clearly appear to be the phlogiſticated acids. But, as phlogiſton is, with many chemiſts, a vague term, and not well underſtood by them; if I add the aerilized volatile alkali, it being highly phlogiſtic, to the mineral acids, they will produce the ſame phenomena; namely, nitrous air, and phlogiſticated acids.

Then, in this caſe, are we to loſe ſight of the firſt rudiments of chemiſtry, and of one of its chief principles; that acids and alkalies have a chemical attraction for, and neutralize each other? Were our chemical anceſtors miſtaken? Does the volatile alkali not unite to the acid, but decompound it, taking from it's oxygenous principle, which, with the alkali, forms fixed

fixed air, according to Mr. Kirwan? Nay, moreover, our anceſtors in chemiſtry, according to them, were ſtill farther miſtaken; for they ſuppoſed that the ſtronger acids would expel the weaker: But here the fixed air is retained by the compound, together with the nitrous acid, ſo that the nitrous, volatile, marine airs are expelled; the fixed air remaining ſnug with the mineral acids. But a ſtill greater difficulty remains: For, if the volatile oils, or alkali, by being united to dephlogiſticated air, do form different bodies with it, agreeable as the alkali is made inflammable; ſo it produces, when burned with the dephlogiſticated air in one ſtate, fixed air; but if in the ſtate of inflammable air, (which it will form, either by heat or the electric ſpark) the nitrous acid will be formed. Here then, (as Dr. Harrington very properly obſerves) the nitrous acid, by loſing its phlogiſticated air, and receiving the volatile alkali in its ſtead, is again formed. Can we reconcile ſuch abſurdities?

But ſome ſay, that the nitrous acid produced in this proceſs, is from the union of the dephlogiſticated and phlogiſticated airs. However, Dr. Prieſtley has ſhewn in all his chemical writings, that dephlogiſticated airs may be turned into phlogiſticated airs, by what he calls the phlogiſticating proceſs. See Prieſtley.

Then, exactly conformable to theſe experiments, and to their hypotheſis, dephlogiſticated air and phlogiſton form the nitrous acid. To ſhew this in a conſpicuous light, I take

twenty

twenty meaſures of dephlogiſticated air; half of this I phlogiſticate by phlogiſtic proceſſes, and then unite it to the other half of the dephlogiſticated air; and after that, by taking the electric ſpark in them, I muſt form them into the nitrous acid. Or, to reverſe the experiment, take twenty meaſures of phlogiſticated air, and form the half of it into dephlogiſticated air, by expoſing it in water to the rays of the ſun; then add the remaining half of the phlogiſticated air to it, and we ſhall be able, with the electric proceſs, to form them into the nitrous acid. Hence, we are under the neceſſity of giving our aſſent to this truth, which Dr. Harrington has always taught, viz. that both phlogiſticated and dephlogiſticated air contain an acid. It muſt therefore appear very extraordinary to ſuppoſe, that dephlogiſticated nitrous air, is not the nitrous acid aerilized with phlogiſton: For, take this air, and carry it through the ſelf ſame proceſſes above mentioned, and the proof will be found to be equally ſtrong and fully ſhews itſelf to be formed of the nitrous acid.

Mr. Kirwan will ſoon be convinced of the juſtneſs of theſe arguments; for he himſelf expreſsly ſays, that the nitrous dephlogiſticated air is *not dephlogiſticated*, but deacidified. After what Dr. Harrington has ſaid, he muſt be convinced of the error of calling it dephlogiſticated.

Mr. Lavoiſier's opinion, that inflammable and dephlogiſticated airs form water, muſt, when candidly conſidered, be thought very extraor-

extraordinary. He excludes phlogiſton, and invents another imaginary principle in it's ſtead, calling it *carbone;* and ſuppoſes that this *carbone*, with dephlogiſticated air, form fixed air: Therefore, this *carbone* muſt be very general, as moſt proceſſes in nature form dephlogicated air into fixed air: The burning of almoſt all combuſtible bodies; the putrefaction and reſpiration of all bodies; and likewiſe vegetation. So then, no body upon this globe can undergo any change, without imparting this ſuppoſed coal to the air. The preſent received chemical doctrines have given riſe to, and ſanctioned ſuch extravagant opinions. When a ſcience is in it's infancy, it is ſurpriſing to think what vague and ill-founded opinions men form of that ſcience! And, as a proper attention to the hiſtory of any diſcovery in ſcience, evinces the truth of this obſervation, ſo it is particularly true of the preſent doctrines reſpecting air. This is an enlightened age. The prejudices and imperfections of dark periods are now removed. Every perſon of learning and ſcience profeſſes, that all his reſearches are to find out truth, and to receive it by whomſoever offered. How unpardonable then muſt it be in ſuch perſons, not to have received the truth, when it has been ſo clearly pointed out to them.

Chemiſts ſay, that by burning bodies in dephlogiſticated air, the air is turned to water, to phlogiſticated and fixed airs. In the reſpiration of animals, in the putrefaction and combuſtion

combuſtion of bodies, the air is neceſſary; becauſe, it either imparts, or receives ſomething from the bodies acted upon by theſe proceſſes. But the preſent chemiſts, wandering from one error to another, have ſet aſide this ſimple doctrine, and have given us a very ſingular one; viz. that the air in ſome proceſſes of combuſtion, receives a body, which they call inflammable air; and in others, a body called *carbone*. In this manner, to be ſure, they make theſe bodies the two great elements in nature. But we find the air in ſome proceſſes of combuſtion, reſpiration, and putrefaction, turned to what they call phlogiſticated air. In Mr. Lavoiſier's ſyſtem, there is no theory to account for this fact: However, the ſupporters of phlogiſton ſay, all theſe changes of the air in theſe different proceſſes are made, by the air's receiving one and the ſame body, called phlogiſton; or, which I ſhould rather call the phlogiſtic ſyſtem: Therefore, conformable to the phlogiſtians, dephlogiſticated air and phlogiſton do, by their chemical union, form water, the nitrous acid, fixed and phlogiſticated airs. Can we, as chemiſts, poſſibly reconcile ſuch a paradox to truth? A paradox, which ſuppoſes that water, the nitrous acid, fixed and phlogiſticated airs, are one and the ſame body, formed of the ſame ingredients: Such, however, are their opinions.

Upon a full view of the preſent chemical theories, we are forcibly ſtruck with the extraordinary doctrines contained in them: Doc-

trines, to which chemiſts could certainly never have been reconciled, had they not wandered from error to error. Were their principles to be unfolded all at once to a pupil of the old ſchool, he would think them the moſt extravagant that could be adopted. But moſt of the great names, which rank ſo high in the preſent chemical world, belong to men, who were not regularly bred in the old ſchool; for if they had, ſome of its fundamental and moſt evident principles, would not have been ſet aſide for aerial flights.

There are a great variety, or different kinds of inflammable air. This air is procured from metals, charcoal, and ſpirits of wine; eſſential oils, and olive oil, by their vapour paſſing through earthen tubes. All theſe ſeem to be high phlogiſtic bodies; as they will reduce the calces of metals, form the nitrous acid into nitrous air; and likewiſe inflame with dephlogiſticated air. Theſe inflammable airs ſeem to be one and the ſame, as having the ſame general characteriſtics. There are many others, though formed by different proceſſes; viz. inflammable alkaline air, and the marine acid air, formed into inflammable air: See Prieſtley. But theſe likewiſe have all the characteriſtics of inflammable air.

Theſe different inflammable airs, leave very different reſiduums, when burnt in dephlogiſticated airs: And therefore ſome modern chemiſts, according to the theory received by them, will have theſe to be different bodies; the

the one kind formed of inflammable air, and the other having no relation to it, but formed of an imaginary element, called coal or *carbone*. On the other hand, ſeveral modern chemiſts believe there is ſuch a body as phlogiſton, and that of this body all inflammable airs are principally formed.

But to adopt their principles, or to ſuppoſe that the nitrous acid, water, fixed and phlogiſticated airs, are all the ſame body, would be to adopt greater ſingularities, and to encounter greater and more numerous difficulties, than we could be taxed with, or expoſed to, by adhering to the doctrines of the old ſchool. Let any chemiſt try to reconcile ſuch contradictions as we muſt aſſent to, by maintaining that theſe different airs are all formed of the ſame body. I have very clearly proved by a variety of experiments, and likewiſe from their reducing metals, that the heavy inflammable airs, by having a greater quantity of fire thrown into them, will become conſiderably lighter, and act upon dephlogiſticated air, the ſame as the light inflammable air upon metals. This is done by paſſing the electric ſpark through them; or by expoſing them to intenſe heat; or by putting red hot bodies into them: Or, thoſe inflammable airs which are produced from vapour, by paſſing the vapour through a larger ſurface of earthen tubes. By treating the air produced from the ſame body in theſe different ways, when fired in dephlogiſticated air, I have found that they

would form different reſiduums, viz. the nitrous acid, water, fixed and phlogiſticated airs.

But not to follow the practice of others, by dwelling upon the hiſtory of dry experiments, I will give you the reſult of my own: And here let me obviate an objection that may be made. It perhaps may be objected, that my knowledge of experiments is not ſo complete, as to take them upon my own opinion. I anſwer, Let the experiments be repeated again and again, and I make no doubt, but that the reſults will be found to be ſuch as I have ſtated them. Chemiſts have paid too much attention to experiments, whilſt they have neglected the knowledge proper to interpret them; and the conſequence of this has been, that thoſe who rank high in the chemical world, have adopted the moſt abſurd hypotheſes. I do not mean by this obſervation, to detract from the merit of many great men, whoſe chemical experiments do them immortal honour; no, I only want them to liſten to the voice of reaſon, of truth, and of juſtice: For, if they ſhew no more candour than they have hitherto done, their accounts with ſcience will be, I am afraid, more upon the debtor than the creditor ſide. Dr. Harrington has been treated in a very ungenteel (I had almoſt ſaid cruel) manner. Thoſe who differ from him in ſentiment have, inſtead of a candid examination of his experiments of inveſtigation, done all they could to damp and reſtrain them.

Inflammable

Inflammable airs burn in dephlogiſticated airs with an intenſeneſs proportioned to their purity, or to the quantity of their concentrated fire; and they will accordingly decompound dephlogiſticated air. If they poſſeſs a great quantity of fire, as the inflammable air from metals, they will entirely decompound it, and form the nitrous acid and water; but if they do not poſſeſs ſo much fire, part of the dephlogiſticated air will be left in the ſtate of phlogiſticated and fixed airs. I have found in ſome experiments a great quantity of fixed air formed; but by throwing into thoſe airs, which are formed by paſſing through hot earthen tubes, a greater quantity of fire, either by heat, or the electric ſpark, and by enlarging the ſurface of the tube through which the vapour had to paſs; all the inflammable airs, by having more concentrated heat thrown into them, will act accordingly upon the dephlogiſticated air. And alſo, by a proper adjuſtment of the proportion of the inflammable airs to the dephlogiſticated airs, I could accurately aſcertain a *priori*, what would be the reſiduum; and clearly deduce this concluſion, that inflammable airs are concentrated fire; and that they (conformable to Dr. Harrington's hypotheſis) will act upon the dephlogiſticated airs, in proportion to the quantity of fire ſet looſe. In the pure inflammable airs from metals, the fire is ſeparated all at once, ſo as to produce an exploſion; but in the heavier airs they burn more gradually: However, if a greater quantity

tity of fire is thrown into them, they will likewiſe explode.

The volatile alkali burnt with dephlogiſticated air, forms principally fixed air. From Mr. Milner's experiment of paſſing the volatile alkali through a red hot barrel, in which was manganeſe, it is evident, that as the heat expelled the dephlogiſticated air, the alkali was ſet on fire by it; and being conſumed in that hot tube under ſo great and intenſe a degree of heat, the dephlogiſticated air will be decompounded into the nitrous acid and water, which, acting upon the iron barrel, form nitrous air. Heat, by expanding the heavy airs and making them lighter, acts here as one might expect. There is no occaſion to have recourſe to Dr. Auſtin's laboured explanation of heat ſeparating the two airs, in which he ſuppoſes the heavy inflammable airs to be compoſed of inflammable and phlogiſticated airs.

That metals contain phlogiſton, is proved from this very ſimple experiment. By expoſing iron to water and dephlogiſticated air, the iron will be turned into a calx, and a phlogiſtic oily body will ſwim upon the top of the water. As our modern chemical theoriſts are always ready to adjuſt every phenomenon to their theory, how abſurd ſoever it may be; let us ſee if this experiment is favourable to them. I grant that this oil is inflammable air condenſed; but, conformable to their theory, they muſt alſo allow, that the water was decompounded in this proceſs; the oxygen principle entering into

into the calx, and the phlogiſton or inflammable air being ſeparated. But here we are aground; for the dephlogiſticated air which certainly forms the calx, is imbibed by the calx, and ſo diſappears. For, if there is no dephlogiſticated air in the proceſs, the iron will not be formed into a calx. This ſame oily phlogiſtic body will reduce the iron again; or, if expoſed to a great heat, will form inflammable air. And more particularly, if this calx ſhould be expoſed to the pariſian minor, it would be reduced without addition, and would undergo the ſame proceſs again with dephlogiſticated air and water.

That it is the dephlogiſticated air that forms the iron into a calx, is probable from all the doctrines of chemiſtry. It cannot be ſuppoſed it is from the water being decompounded, as the dephlogiſticated air was free and diſengaged, while the water's ſuppoſed dephlogiſticated air was united to inflammable air: But if even they ſhould contend for it; the dephlogiſticated air of the atmoſphere ſhould have united with the inflammable air of the water, and formed with it water again.

Mr. Kirwan, in endeavouring to reconcile the phlogiſtic theory with Mr. Lavoiſier's, upon the formation of acids, has fallen into great errors. He ſuppoſes that inflammable and dephlogiſticated airs form water; that metals are formed of phlogiſton and the earth of the calx; and that the calx is formed of fixed air and the earth of metals. Now, this doctrine,

doctrine, though Mr. Kirwan is not aware of it, directly contradicts Lavoiſier's. The former ſuppoſes that fixed air is formed of the ſame kind of inflammable and dephlogiſticated airs, which form water: For the inflammable air obtained from metals, and which will reduce them again, when fired with dephlogiſticated air, will form water. And moreover, when metals are reduced without addition, their being decompounded, is, he ſays, from the fixed air contained in them; the dephlogiſticated air being ſet looſe from it. But can Mr. Kirwan poſſibly, as a chemiſt, reconcile the abſurd idea, that fixed air and water are formed of the identical ſelfſame bodies, and by the identical ſelfſame proceſs, viz. combuſtion: But this I do poſitively declare, that our aerial chemiſts can reconcile any thing, if ever ſo prepoſterous. They are ſo much attached to their experiments in gun barrels, retorts, &c. imagining ſome charm in them, (which are to diſcover every thing) that I can compare their credulity as being equal to that which appeared in the affair of the bottle conjurer in the Hay-Market. Dr. Harrington ſays in his Letter, p. 4. "Dr. Prieſtley, in the great variety of his experiments, proves, that in ſome caſes the burning of theſe two airs produces water, and ſometimes fixed air.

"Then let us juſt compare fixed air with water. Fixed air, when condenſed, neutralizes alkalies and calcareous earths; will any chemiſt ſuppoſe water to do the ſame? When fixed

fixed air condenſed in the vegetable alkali, is juſt upon the ſame footing, in reſpect to ſolidity and being aerilized, as the water is in the ſame vegetable alkali. And if our fathers in chemiſtry ſhould be told, that the water and the fixed air in the ſalt were both the ſame chemical bodies, compoſed of condenſed dephlogiſticated, and inflammable airs; would they believe it?"

I ſhall now take a view of thoſe chemical writings, with which Dr. Prieſtley has favoured the world ſince the publication of Dr. Harrington's Letter: But the reader will allow me to make a previous obſervation; which is, that Dr. Prieſtley has been very careful, not to mention that gentleman as a fellow-labourer. What reaſon ſhall we aſſign for his ſilence? The queſtion, I think, may be very eaſily anſwered. There is an oppoſition of hypotheſes; and, if Dr. Harrington's is the true one, Dr. Prieſtley's muſt of conſequence be falſe: However, not to mention the chemical doctrines of his antagoniſt, is, in my opinion, very wrong: Fair diſcuſſion is the beſt way to know who has truth on his ſide; let then the two hypotheſes be candidly canvaſſed by thoſe of an impartial public, who are able to judge. Will it be ſaid in extenuation, that Dr. Harrington's theory deſerves no anſwer? Were any man, who in the leaſt pretends to the name of a chemiſt, to make ſuch an aſſertion, I ſhould not ſcruple confidently to aſſert, that he knows nothing of chemiſtry.

It is very poſſible, after the uſage Dr. Harrington has met with, that my labours may receive the ſame treatment. However that may be, I publicly call upon modern chemiſts, (ſome of whom deſerve the higheſt merit, and their works will be eſteemed as long as true ſcience laſts) not to ſhrink from the preſent inveſtigation, but come boldly to it. If they do not, their labours, inſtead of promoting ſcience, will rather retard it. For, when a perſon, preſuming upon the reputation he has obtained in the world, endeavours by an uncandid behaviour to draw the curtain over truth; this conduct will in the end counterbalance all his labours. I throw down the gauntlet, as Dr. Harrington has done; and, if none takes it up, it muſt be for fear of being foiled; or to drop the metaphor, I openly avow my chemical principles, and challenge a fair diſcuſſion of them. But ſhould Dr. Harrington's theory, and theſe my well meant attempts in favour of it, continue ſtill to be paſſed by with a contemptuous ſilence; and ſhould time, which does juſtice to philoſophers and their principles, ſhew our's to be right; in this caſe, to avoid diſcuſſion, which leads to truth, is worthy of blame, and poſterity will, in this inſtance, undoubtedly condemn their conduct. I might mark ſuch behaviour with its proper ſtigma: This, however, I will not do, but leave the reader to make his own reflections.

This age is with great propriety called enlightened: It is the age of ſcience, and the many

ny diſcoveries made in it, have been happily applied to the purpoſes of human life. Nay more, it is the age of truth, and philoſophers, both natural and moral, profeſs to have only truth in view, in all their inveſtigations. Hence, to me it is matter of ſurpriſe, that no old chemiſt has before me, paid a proper attention to Dr. Harrington's principles! For, if a theory which accounts for, and proves by ſolid reaſoning, all the chemical phenomena, in oppoſition to principles contrary to nature and reaſon, and which account for none of the phenomena, ſhould be attended to and adopted, that gentleman's is the one: And if any chemiſt is not diſpoſed to adopt it, let him at leaſt treat it with the candour it deſerves. Dr. Prieſtley is anxious to know what he breathes, before he ceaſes to breathe. When this is told him, he will not, I hope, think it below him to accept of information.

Dr. Harrington clearly proved as long ago as the year 1780, that an acid and water are neutralized with fire, and aerilized into atmoſpheric air. That in reſpiration, this fixed fire is attracted by the blood from the acid and water; that the acid is left in the ſtate of fixed air, and a great quantity of the water is condenſed in the proceſs. That in putrefaction, the air undergoes a ſimilar decompoſition, and the fixed fire is attracted by the putrid body, ſo as to become putrid or alkaleſcent. That in combuſtion, the fixed fire (as we have proved) is ſet looſe. Theſe diſcoveries were only a

prelude to thoſe of the firſt principles of animal and vegetable life, the phenomenon of animal heat, with other ſecondary phenomena. He publiſhed in 1785 a full hiſtory of the different airs, clearly ſhewing the formation of each. That the air is again renewed after being injured, he proves from it's levity; owing to which, it is taken up into the higher regions of the atmoſphere, where the fixed air and water are again ſaturated with fixed fire, by which, becoming more ſpecifically heavy, it deſcends again. That phlogiſton is fixed fire chemically attracted; and that it is capable of being ſet looſe again, by various proceſſes in nature *.

Now, I ſcruple not to declare, that all the principal and leading diſcoveries reſpecting atmoſpherical

* I would beg leave to mention, that Mr. De Luc, after Dr. Harrington, ſuppoſes, that the air is renewed and purified again in the clouds; and he endeavours to account for it upon the ridiculous hypotheſis of water being formed of inflammable and dephlogiſticated airs, which is decompounded in the clouds. But even this ſuppoſition was, after Dr. Harrington had proved by experiments, and publiſhed to the world, that the clouds are nature's laboratory, as he ſignificantly expreſſes it. Of this diſcovery Mr. De Luc takes no notice, but mentions it as his own opinion. And though Dr. Harrington, as far back as 1780, clearly proved, that fire is capable of being chemically attracted; yet Mr. De Luc, long after this, ſpeaks of it as his own obſervation, and ſo has the merit of both diſcoveries. Such is the uſage that gentleman has received from thoſe, who, very probably, were indebted to him, for diſcoveries they call their own. Moreover, Mr. De Luc follows Dr. Harrington cloſely in the arguments, to prove the homogeneouſneſs of the atmoſpheric air, though, conformable to the general policy of his conduct, he never once mentions his name.

moſpherical air, it's formation and compoſition, with the manner in which it ſupports animal and vegetable life, and the life of combuſtion; I ſay, that all theſe are fully ſhewn by Dr. Harrington. The only thing that appeared to me rather obſcure was, the life of combuſtion; but this, I hope I have, conformable to Dr. Harrington's principles, fully demonſtrated in this Treatiſe. I ſhould be ſorry to endeavour, like ſome chemiſts, to take away any part of his merit. However, I venture to predict, that the time is faſt advancing, when every thing relating to this chemiſtry, will be properly underſtood, and ſettled upon a right foundation. Dr. Harrington has in his Letter (printed in 1788) very clearly and fully detected the errors of his opponents, concluding it with this pointed language, but as yet he has received no anſwer: "I hope I have made it appear in this Letter, and in the reſt of my writings, that the preſent hypotheſes of philoſophers account for none of the phenomena we have conſidered; and that mine give an explanation of them all, both conſiſtent and ſatisfactory.

"Therefore, gentlemen, I thus publicly call upon you, either to vindicate your opinions or renounce them; ſcience and the public claim it of you."

There is an experiment mentioned by Dr. Higgins, which he ſays is in favour of Mr. Lavoiſier's ſyſtem; as indeed we muſt agree with him that it is directly contradictory to the

the ſyſtem of phlogiſton, with the belief that dephlogiſticated air is, in reality, dephlogiſticated. But if Dr. Higgins and others will pleaſe to ſee the truth; this dephlogiſticated nitrous air is formed of the nitrous acid and the phlogiſton of the copper. It will be a ſtrong proof in favour of the doctrine of phlogiſton. The Doctor ſays, p. 550. "If tin be introduced into a neutral ſolution of the tin in the nitrous acid, it is calcined, a calx is thrown down, and the dephlogiſticated or imperfect nitrous air is produced. Dephlogiſticated nitrous air, according to the phlogiſtians, contains no phlogiſton; then I aſk, what becomes of the phlogiſton of the newly calcined metal? If tin contained phlogiſton, either inflammable air or nitrous air would be produced, or a portion of the diſſolved tin would be precipitated in its metallic ſtate; neither of which will take place, if the experiment be well conducted. Hence I ſhould ſuppoſe, that metals do not precipitate each other in their metallic ſtate, in conſequence of a double affinity proceeding from the matter of light inflammable air, (or phlogiſton) and likewiſe that metals part with no ſuch thing during their calcination in acids."

Dr. Prieſtley, after Dr. Harrington's publications, gave different papers to the Royal Society, in which he endeavours to prove that water is not formed of inflammable and dephlogiſticated airs. His arguments and experiments are the very ſame as thoſe of that ingenious

genious chemiſt; therefore, we ſhall ſay nothing more to refute ſo abſurd an hypotheſis, which ſuppoſes theſe airs form the compoſition of water: Only he has in theſe papers blended an abſurd hypotheſis, and much falſe reaſoning, with the truth. That learned chemiſt has in theſe papers given, with his luxuriant pen, a multiplicity of experiments and reaſons: And, as an experimenter, he has certainly great merit; but as a chemical reaſoner, I can by no means pay him that compliment. In reviving the red precipitate in inflammable air, he found a ſmall quantity of fixed air left in the reſiduum; and from thence he, conformable to his ſingular hypotheſis, ſuppoſes that inflammable and dephlogiſticated airs, in their naſcent ſtate, form fixed air. But the fixed air produced was not equal to what ſhould have been produced from the quantity of the two airs employed, it being ten times leſs. But this is ſuch a trifle with modern chemiſts, as not to give the leaſt ſhock to their hypotheſes. Had Dr. Prieſtley but attended to what Dr. Harrington ſays, he would have found the reaſon why fixed air was formed. But to have done ſo, would have led him to renounce his former opinions. For the reaſon why fixed air is formed in the proceſs, ſee p. 75.

March 26th, 1789, Dr. Prieſtley preſented another paper to the Royal Society, in which he attemps to prove that acids contain dephlogiſticated air, as one of their conſtituent parts. He has followed Dr. Harrington in contradicting

ting the opinion of the ſuppoſed formation of water. But in this paper he ſticks to an opinion (equally abſurd) that the acids are formed of airs. To have given up this would have confirmed Dr. Harrington's theory; he therefore exerts all his powers of rhetoric in it's favour. We ſhall now inveſtigate what he advances.

Though Dr. Prieſtley proves in all his former chemical writings, that heat phlogiſticates the nitrous acid, and ſays, vol. V. in a ſummary view of his principal facts, "Spirit of nitre may be procured almoſt colourleſs by a careful diſtillation in the common way, iv. 453. From being of a deep orange, it becomes green by long keeping, 453; afterwards of a deep blue, 454. But if it be expoſed to the open air, it becomes coloured again, ib.

Heat deepens the colour of this acid, iii. 249. It's colour is univerſally owing either to phlogiſton or heat, iv. 2." Yet, as theſe facts are hoſtile to his preſent chemical opinions, and as he ſees the danger they are in from them; he endeavours to account for the acids being formed of airs, in a moſt *extraordinary manner indeed*. The reader will, I hope, indulge this obſervation, namely, that our preſent chemiſts by being bred up in the aerial ſchool, are ready to receive any hypotheſis, however extraordinary it may be *. He talks of heat, as it were the

* In my former experiments, vol. IV. p. 2. I found that the colourleſs acid became ſmoaking, or orange-coloured, and

the phlogiſton previouſly contained in the acid. Chemiſtry knows no ſuch ideas as thoſe of evolving, without ſome chemical attraction or influence. But he afterwards found it to be light only that produced this effect; and again he found it to be heat: So fluctuating are his opinions. He gives in vol. V. ſome

and emitted orange-coloured vapours, on being expoſed to heat in long glaſs tubes, hermetically ſealed; and I then concluded, that this effect was produced by the action of *heat*, evolving, as it were, the phlogiſton previouſly contained in the acid. Afterwards, having found that it was not *heat*, but *light* only, that was capable of giving colour to ſpirit of nitre, contained in phials with ground ſtoppers, in the courſe of ſeveral days; and that in this caſe the effect was produced by the action of *light* upon the *vapour*, which gradually imparted its colour to the liquor on which it was incumbent. (ſee vol. V. p. 342.) I was led to ſuſpect, that as the glaſs tubes, in which I had formerly expoſed this acid to the action of heat, were only held near to a fire, in the day-light, or candle-light, it might have been this *light*, which, in theſe circumſtances, had, at leaſt in part contributed to produce the effect.

In order to aſcertain whether the light had had any influence in this caſe, I now put the colourleſs ſpirit of nitre into long glaſs tubes, like thoſe which I had uſed before, and alſo ſealed them hermetically, as I had done the others; but, inſtead of expoſing them to heat in the open air, from which light could not be excluded, I now ſhut them up in gun barrels, cloſed with metal ſcrews, ſo that it was impoſſible for any particle of light to have acceſs to them; and I then placed one end of the barrels ſo near to a fire as was ſufficient to make the liquor contained in the tube to boil, which I could eaſily diſtinguiſh by the ſound which it yielded. The conſequence was, that in a ſhort time the acid became as highly coloured as ever it had been when expoſed to heat without the gun barrel. It was evident, therefore, that it had been mere *heat*, and not *light*, which had been the means of giving this colour to the acid, and which has been uſually termed *phlogiſticating* it. See Prieſtley, Phil. Tranſ. vol. lxxix. p. 139.

ſtriking facts which ſhew, that the heat acts principally upon the vapour of the nitrous acid, phlogiſticating it. But in this paper to the Royal Society, he ſuppoſes that the heat acts upon the nitrous acid, expelling the vapour, as being dephlogiſticated.

Mr. Kirwan attempts to refute that excellent chemiſt, the immortal Scheele, who endeavoured to prove that the fire of the ſun phlogiſticates the nitrous vapour. Mr. Scheele ſays in his Experiments, p. 80. "*d.* I poured a ſmall quantity of the pureſt fuming ſpirit of nitre (No. 25) into a white phial made of cryſtal glaſs, provided with a glaſs ſtopper, and expoſed it to the light of the ſun. Three hours after I found the phial filled with red vapours; the ſame happens when the phial is ſet on a German earthen-ware ſtove; but it requires four weeks before the red colour becomes diſcernable." Dr. Harrington ſays in his Thoughts on Air, p. 30. "Mr. Kirwan, in oppoſition to Mr. Scheele's experiments of the nitrous acid being phlogiſticated from expoſure to the ſun, ſays in his notes upon Mr. Scheele's book, p. 230." "By pure nitrous acid, the author means the dephlogiſticated colourleſs nitrous acid. I have expoſed this in a phial half full, and cloſed with a glaſs ſtopper, to the ſolar light, and in a quarter of an hour found it phlogiſticated; but when the phial was quite full, this did not happen, though I expoſed it two hours to the ſame light. It appeared to me in the firſt caſe, that the vapour only

only was acted upon, being changed from white to red; this vapour was abſorbed by the liquor which then became greeniſh. In this caſe, the vapour may have taken phlogiſton from the air, whoſe capacity for containing fire was increaſed; this I am the more inclined to ſuſpect, becauſe the more of the bottle was left empty, the deeper green the liquor appeared to me to aſſume."

"But unfortunately for this theory; inſtead of the air being better, (which it muſt have been agreeable to this opinion, and likewiſe the general one of injured air being phlogiſticated) upon examination it has become highly noxious."

Dr. Prieſtley ſays, Philoſ. Tranſ. vol. lxxix. p. 441. "My friend Mr. Kirwan, however, having always ſuſpected, that the *air* was a principal agent in the buſineſs, I at this time gave particular attention to this circumſtance; ſuppoſing that, if any part of the common air had been imbibed, it muſt have been the *phlogiſticated*, and that it was the phlogiſton from this kind of air which had phlogiſticated the acid. The real reſult, however, was not ſo much in favour of this ſuppoſition as I had expected; for the principal effect of the proceſs was the emiſſion of dephlogiſticated air, ſo that the acid ſeems to become what we call phlogiſticated, by parting with this ingredient in its compoſition."

The reader muſt excuſe my not following Dr. Prieſtley through all his calculations in this

paper, as I do not think that his hypotheſis, and mode of reaſoning upon it, deſerve ſuch attention. He now leaps to the oppoſite ground, and ſuppoſes that the vapour emitted from the heated acids, is dephlogiſticated air; and he endeavours to prove it by calculations, which are equally as erroneous, being upon as wrong a *data* as his hypotheſis. In this caſe there is an exact conformity between them.

But the Doctor, who is indefatigable, (this is one of his chief talents as an author) produces another paper to the Royal Society, in favour of his ſingular hypotheſis. And here I would obſerve, that the Doctor muſt exert himſelf in behalf of his hypotheſis, as the opinion, that the acids are formed of different airs, is principally founded upon the doctrine of different airs entering into the compoſition of water: Therefore, as he himſelf has, after Dr. Harrington, endeavoured to deſtroy that hypotheſis, the other muſt ſtand upon unſafe ground. But what is it Dr. Prieſtley cannot do? All muſt bow to his ſuperior talents: Therefore, confiding in this ſuperiority, he ſends the Royal Society another paper, which we ſhall examine. He begins, p. 289. "In my late experiments on the *phlogiſtication of ſpirit of nitre by heat* it appeared, that when pure air was expelled from what is called dephlogiſticated ſpirit of nitre, the remainder was left phlogiſticated. This I find abundantly confirmed by repeating the experiments in a different manner, and on a larger ſcale; and I have applied the

the ſame proceſs to other acids and liquors of a different kind. From theſe it will appear, that oil of vitriol and ſpirit of nitre, in their moſt dephlogiſticated ſtate, conſiſt of a proper ſaturation of the acids with phlogiſton, ſo that what we have called the *phlogiſtication* of them, ought rather to have been called their *ſuper-phlogiſtication*.

"I began with treating a quantity of oil of vitriol as I had done the ſpirit of nitre, viz. expoſing it to heat in a glaſs tube, hermetically ſealed, and nearly exhauſted; and the reſult was ſimilar to that of the experiment with the nitrous acid, with reſpect to the expulſion of air from it, though the phlogiſtication not appearing by any change of *colour*, I did not in this method aſcertain that circumſtance. The particulars were as follow:

"After the acid had been made to boil ſome time, a denſe white vapour appeared in quick motion at a diſtance above the acid, and tho', on withdrawing the fire, that vapour diſappeared, it inſtantly re-appeared on renewing the heat. When the tube was cool, I opened it under water, and a quantity of air ruſhed out, though the acid had been made to boil violently while it was cloſing, ſo that there could not have been much air in the tube. This air, which muſt therefore have been generated in the tube, was a little worſe than common air, being of the ſtandard of 1.12 when the latter was 1.04. I repeated the experiment

periment ſeveral times, and always with the ſame reſult.

" That this air ſhould be worſe than common air, I cannot well explain. But in my former experiments it appeared that vitriolic acid air injures common air; and that in proportion as pure air is expelled from this acid, and the remainder becomes phlogiſticated, or charged with vitriolic acid air, clearly appeared in the following experiment.

" Making a quantity of oil of vitriol boil in a glaſs retort, and making the vapour paſs through a red-hot earthen tube, glazed inſide and out, and filled with pieces of broken tubes, I collected the liquor that diſtilled over, and found it to be the ſame thing with water impregnated with vitriolic acid air. The ſmell of it was exceedingly pungent, and it was evident, that more of this air had eſcaped than could be retained by that quantity of water. The oil of vitriol uſed in this proceſs was 1 oz. 9 dw. 18 gr. and the liquor collected was 6 dw. 12 gr. When I collected the air that was produced in this manner, which I did not do at this time, it appeared to be very pure, about the ſtandard of 0.3 with two equal meaſures of nitrous air.

" At another time, expending 1 oz. 11 dw. 18 gr. of oil of vitriol, of the ſpecific gravity of 1856 (that of water being 1000), I collected 19 dw. 6 gr. of the volatile acid, of the ſpecific gravity of 1340, and 130 oz. meaſures of dephlogiſticated

phlogiſticated air of the pureſt kind, viz. of the ſtandard of 0.15.

"It is eaſy in this manner to collect a great quantity of dephlogiſticated air; but the principal objection to the proceſs is, that after uſing a few times, the earthen tubes become tender, and too eaſily break, eſpecially in heating or cooling. It is alſo difficult to lute the retort containing the acid and the earthen tube. The air produced in this manner is filled with the denſeſt white cloud imaginable.

"Going through the ſame proceſs with ſpirit of nitre, the reſult was in all reſpects ſimilar, but much more ſtriking, the production of both dephlogiſticated air and phlogiſticated acid vapour being prodigiouſly quicker, and more abundant. Expanding 5 oz. 8 dw. 6 gr. of ſpirit of nitre, I collected 600 oz. meaſures of very pure dephlogiſticated air, being of the ſtandard of 0.2. I alſo collected 1 oz. 7 dw. 14 gr. of greeniſh acid of nitre, which emitted copious red fumes. All the apparatus beyond the hot tube was filled with the denſeſt red vapour, and the water of the trough in which the air was received, was ſo much impregnated with it, that the ſmell was very ſtrong; and it ſpontaneouſly yielded nitrous air ſeveral days, juſt as water does when impregnated with nitrous vapour. Perceiving the emiſſion of air from the water, after it had ſtood ſome time, I filled a jar containing 30 oz. meaſures with it, and without any heat it yielded 2 oz. meaſures of the ſtrongeſt nitrous air."

Does

Does this require a comment? My reader, if he remembers what I have already ſaid, may anticipate me in my explanation. The vitriolic acid becomes phlogiſticated, yet at the ſame time diſcharged an air worſe than common air, which, ſay they, contains three parts of phlogiſticated, to one of dephlogiſticated air. A moſt ridiculous explanation: For, if vitriolic acid air injures common air, why ſhould it injure it's own dephlogiſticated air it had juſt parted with? Beſides, if the vitriolic acid injured the air, the acid would become dephlogiſticated; but, agreeable to Dr. Prieſtley, it becomes highly phlogiſticated in the proceſs; and more particularly the nitrous acid, which has conſiderably a greater power of injuring common air, gives out, in this ſame proceſs, pure air which is not injured; though it is likewiſe in contact with the nitrous acid, and its vapour is the ſame as in the proceſs of the vitriolic acid.

I need, I think, ſcarcely mention, that the ſtrong acids ſaturate themſelves with a great quantity of fire, and become phlogiſticated; that the nitrous acid becomes ſo highly phlogiſticated, as to emit a rich vapour or phlogiſticated air, (erroneouſly ſo called, but I muſt give them their names) being the acid vapour highly ſaturated with phlogiſton. That this air is produced from the acids by heat, Mr. Scheele found in the diſtillation of the nitrous acid. And I will inform Dr. Prieſtley of ſomething, which very probably he does not know.

know. If, upon expoſing theſe acids to great heat, he chuſes to examine this vapour or air in the different ſtages, he will find it at firſt, what he calls phlogiſticated: But, by ſtill going on with the proceſs, it will get ſo great a ſaturation of phlogiſton, as to form his dephlogiſticated air. As to the acids paſſing through hot tubes, and forming air of different ſaturations of phlogiſton or fixed fire, which is his other experiment; it is the very ſame as that of the aerial acid or fixed air. This air, by having the electric fire paſs through it, will form different ſaturations; viz. phlogiſticated, dephlogiſticated, and inflammable airs: Or heat, acting upon it when united to calcareous earths, will do the ſame. If this explanation is not deemed ſufficient, we can make it ſtill more evident. For, by adding ſpirit of wine or the eſſential oils to the nitrous acid, we can make it form all theſe different ſtates of phlogiſton; viz. dephlogiſticated airs and the phlogiſticated acid vapour. But had Dr. Prieſtley condeſcended to attend to what Dr. Harrington ſays, he could not have been at a loſs to account for theſe phenomena. Dr. Harrington ſays in his Thoughts on Air, p. 309. "As we have all along proved the identity of the electric matter with phlogiſton, it being allowed by all chemiſts, ſo we ſhall ſhew its effects upon different bodies. Mr. Henry in his preface (p. 14.) to the tranſlation of Mr. Lavoiſier's eſſays, ſays," "Dr. Prieſtley having mentioned his having formed ſulphur by the union of in-

flammable air with vitriolic acid, as a proof of the identity of that air with phlogiſton; he adds, that he had alſo proved, more unexceptionably than before, that the electric matter contains phlogiſton, by making it to paſs through the air, confined by the acids, in a ſyphon." 'When,' ſays he, 'I uſe the dephlogiſticated marine acid, the air is diminiſhed by the proceſs, and dephlogiſticated. If I uſe the *phoſphoric acid* or the *phlogiſticated alkali*, the air is firſt diminiſhed, and then increaſed by an addition of inflammable air. If I uſe VITRIOLIC ACID *or the* NITROUS ACID, *there is a production of* DEPHLOGISTICATED AIR, *faſter than the electricity can injure it.*'

"In the firſt inſtance the marine acid being dephlogiſticated, will naturally attract phlogiſton, therefore the air is decompounded.

"In the ſecond inſtance with the phoſphoric acid, or the phlogiſticated alkali, the electric matter paſſing through them, there is a generation of inflammable air. We have ſhewn that the phoſphoric acid has ſo great attraction for fire or phlogiſton, that when it is expoſed to a great heat, phoſphorus will be produced; that, when united with the calx of lead, they will produce inflammable air. And the connection between the alkalies and inflammable air is well known. The electrical matter will turn alkaline air inflammable.

"The third inſtance of the vitriolic acid and nitrous acid, generating dephlogiſticated air with it, may be eaſily accounted for. I ſuppoſe

poſe it will not be diſputed that the electrical matter reduces the calces of metals by giving them phlogiſton; that it forms alkaline bodies into inflammable air by giving them phlogiſton; and likewiſe the phoſphoric acid into inflammable air from this cauſe.

"Theſe facts being granted, can it be ſuppoſed that it can form the vitriolic and nitrous acids into air by dephlogiſticating them? One of the acids to be formed by it into inflammable air, and the other into dephlogiſticated air, are ſuch paradoxes as cannot be admitted. No, they have all received phlogiſton, only the phoſphoric acid has received a higher impregnation. This is demonſtrable; for if this air that is formed from the nitrous and vitriolic acids in this proceſs be expoſed to the nitrous acid, the air will be decompounded, and the acid ſtrongly phlogiſticated; nay, by a peculiar nitrous and vitriolic acid, I have, by continuing the proceſs for a long time with the electrical matter, formed them into an inflammable air. Theſe are ſuch obvious facts, as muſt force conviction. Chemiſts have attempted to account for theſe phenomena in a very vague manner indeed. Mr. Henry ſays, p. 15." "Now, from whence can this dephlogiſticated air proceed, but from a decompoſition of the acids by the phlogiſton of the electric matter, in which the pure air is ſeparated, while the phlogiſton, combining with the remaining part or baſis of this acid, forms a ſulphur?"

"To ſuppoſe that theſe acids, by receiving phlogiſton, ſhould produce dephlogiſticated air, is a moſt ſingular ſuppoſition indeed. If we add phlogiſton either to the nitrous or vitriolic acids, we produce nitrous and vitriolic airs. But is it poſſible that phlogiſton being added to one acid could make it part with dephlogiſticated air, and cauſe the other to part with inflammable air? Then, agreeable to this theory, the nitrous and vitriolic acids are formed of empyreal air, and the phoſphoric acid of inflammable air. But in burning ſulphur, pyrites, &c. Mr. Lavoiſier found a quantity of empyreal air abſorbed, and they turned to the vitriolic acid. Therefore, agreeable to this theory, if you burn phoſphorus there is an equal quantity of empyreal air abſorbed, (which he acknowledges himſelf;) but then the phoſphoric acid with this electrical matter ſhould yield empyreal air, as well as the other acids, if this be true. However, it yields an air, which they ſay, is the oppoſite to it, viz. inflammable air.

"There is juſt the ſame objection to their theory of the acids forming airs with earths. Mr. Lavoiſier ſuppoſes it is by the empyreal air being ſet looſe. Then, how comes ſome acids, viz. the phoſphoric, the vegetable, the aerial acid, with theſe bodies to form inflammable and phlogiſticated air; and the other, viz. the nitrous and the vitriolic with them to form empyreal air. Beſides, in introducing this theory, we deſtroy Stahl's great doctrine of phlogiſton.

phlogiſton. But in ſhort, chemiſts are really in a labyrinth with their doctrine of air, ſo that every new experiment gives a different hypotheſis, and all the old rudiments are ſet at nought."

But what appears moſt extraordinary is, that Dr. Prieſtley, after he had been labouring to prove, that inflammable and dephlogiſticated airs form water and the nitrous acid, ſhould, with the ſame opinions, and hypotheſis, endeavour to prove, that the nitrous acid is formed of phlogiſticated, nitrous, and dephlogiſticated airs *.

In this mode of reaſoning he proceeds upon no confirmed *data*, but only adopts the fanciful conjectures of the moment; and ſuch are the theories of our modern chemiſts. Dr. Prieſtley has made experiment upon experiment, which compoſe many large volumes; and his opinions, taken from thoſe experiments, have been continually

* As nitrous acid is reproduced when dephlogiſticated air is admitted to nitrous air; or, according to this theory, to dephlogiſticated nitrous vapour and phlogiſton, do not theſe principles, together with *water*, which is requiſite, at leaſt, to the forming of an union between them, make nitrous acid. If ſo, this acid muſt conſiſt of dephlogiſticated air, dephlogiſticated nitrous vapour, phlogiſton, and water. In the ſolution of metals in this acid, then, the firſt and laſt mentioned of theſe four elements, together with part of the third, muſt unite with the calx of the metal, while the ſecond, joined with the remainder of the third (or which is the ſame thing with phlogiſton from the metal) conſtitutes nitrous air. I do not need to add that *latent heat* ſeems to be a neceſſary ingredient in every kind of air. See Prieſtley's Experiments, vol. VI. page 411.

continually changing: Still in the dark, he could ſee no object diſtinctly. This induced him to follow an imagination unimpreſſed with the firſt rudiments of chemiſtry, and his great name has given authority to his conjectures.

It ſurpriſes me not a little to ſee Dr. Prieſtley change his ground, with reſpect to the proceſs of reſpiration: This was the proceſs upon which Dr. Harrington firſt attacked him. That chemiſt would have it to be a dephlogiſticating proceſs to the air; whereas Dr. Prieſtley maintained it to be a phlogiſticated one. But the Doctor, who muſt be allowed to poſſeſs great penetration, finding his own ground untenable, ſhifts, like a wary general, to that of Dr. Goodwyn; yet would not, upon any conſideration, ſuggeſt any thing concerning Dr. Harrington's hypotheſis. However, I am afraid, that when this ground, I mean Dr. Goodwyn's hypotheſis, comes to be fairly examined, it will be found equally untenable. I can ſcarcely think, that ſo penetrating and knowing a man as Dr. Prieſtley, can be ſerious in the choice of his preſent ground. Probably he has choſen it, through political policy, of obſcuring the ſubject. Being ſo accuſtomed to conquer, he cannot ſupport, even the idea of a defeat.

The Doctor begins with his experiments and arguments upon the burning of charcoal in atmoſpherical air, from which he eſtimates the quantity of dephlogiſticated air turned into fixed air: But I ſhall enlarge no farther upon this

wrong

wrong *data*. My reader may ſee, page 72, that part of the fixed air comes from the charcoal.

The Doctor's next *data* is taken from his own reſpiration. He breathes a certain quantity of air; and by examining the air after reſpiration, he finds that both the dephlogiſticated air, and great part of the phlogiſticated air have diſappeared. Hence he concludes, that a great part of the dephlogiſticated air is received entire into the blood, only a ſmall part of it being turned into fixed air. It is matter of ſurpriſe, that Dr. Prieſtley, after being ſo long converſant with aerial experiments, ſhould bring theſe experiments to prove ſo ſingular an hypotheſis.

In the firſt place, the Doctor is for making the life of animals act differently upon reſpirable air, to what the life of combuſtion does, and likewiſe putrefaction. But there is no fact in chemiſtry better eſtabliſhed than this, that the air is acted upon by theſe proceſſes in the ſame manner. Again, which is ſtill more ſurpriſing! The Doctor makes phlogiſticated air neceſſary to reſpiration; as a conſiderable greater quantity of it, than of dephlogiſticated air, diſappears in reſpiration. Here Dr. Blagden's explanation will no ways aſſiſt him to get out of the dilemma. The Doctor ſays, Philoſophical Tranſactions, vol. lxxx. p. 110. "But, at the obliging ſuggeſtion of Dr. Blagden, I now think it more probable, that the deficiency of phlogiſticated air was owing to the greater

greater proportion of it in the lungs *after* the proceſs than *before*." But why is only phlogiſticated air left in the lungs? Why not the fixed air, into which the dephlogiſticated air muſt have been turned? Nay, why not dephlogiſticated air? Indeed, that the lungs have not power to turn all the factitious dephlogiſticated air into fixed air, has been fully ſhewn by Dr. Harrington, Mr. Scheele, and even by Dr. Prieſtley himſelf *.

The Doctor, who is confined in his chemical opinions, to his experiments in gun barrels, &c. never takes a more extenſive field. But if he will conſider, that his theory ſuppoſes, that dephlogiſticated air is the acidifying principle; and if ſuch a quantity of dephlogiſticated air is received every moment into the blood, what muſt a ſheep's blood be, which is formed from the watery aceſcent vegetables, and with all this aceſcent air. What ſupplies its fat that it poſſeſſes in ſuch great abundance? That

* In the preceding experiments, and ſeveral others which I made about the ſame time, I found that mice would not live in dephlogiſticated air till they had completely phlogiſticated it, though they lived longer in it than, in proportion to its purity, with reſpect to common air; and for this I cannot aſſign any ſufficient reaſon. I had once imagined that this was owing to my being obliged to make the mice paſs through a quantity of water, by which the air was confined; but I put a mouſe through the ſame water into a quantity of common air, and it lived in it till it was thoroughly phlogiſticated. This may deſerve a farther inveſtigation. I ſhould have put other mice into what remainder of the dephlogiſticated air. See Prieſtley's Experiments, vol. V. p. 163.

That the air forms the red globules of the blood, is an undoubted fact: And likewiſe, that they are a high phlogiſtic body, is equally as well aſcertained by the great Gaubius, and every other eminent phyſiologiſt. But to ſatisfy the Doctor by chemical experiments: If you take phlogiſton in any form, either as an alkaline ſalt, oil, &c. and add them to the venous blood, they will immediately change it to the colour and qualities of arterial blood; the ſame as it receives when it comes in contact with the air. But if you apply an aceſcent body to the arterial blood, it immediately turns it to the colour and quality of the venous.

The Doctor talks of the dephlogiſticated air carrying the phlogiſticated air along with it into the blood, as if there was ſome ſtrong attraction between them. But, in combuſtion, when the air is imbibed by ſulphur and phoſphorus, they are not imbibed together.

However, according to theſe experiments of Dr. Prieſtley, phlogiſticated air is more neceſſary to reſpiration than dephlogiſticated air, as more of the one than of the other diſappears. But let the Doctor only try the experiment upon animals, which have *no hypotheſis to eſtabliſh*, and he will find very different reſults: We ſhall quote ſome of his own experiments. He ſays, vol. V. p. 161. "But to make the experiment in the moſt unexceptionable manner that I could contrive, I, in the next place, got two mice, of nearly equal ſize, and put them into exactly equal quantities, viz. about five ounce

meaſures, of the ſame dephlogiſticated air (the meaſure of its purity, with two equal quantities of nitrous air, being 0.24) in nearly equal and ſimilar glaſs jars, one ſtanding in lime-water, and the other in common water. Both the mice continued in this ſituation ſomething more than two hours and an half, after which the air which had been confined by lime-water appeared to be reduced in the proportion of 9 to 5 1-4 the meaſures of the teſt being 0.96; and the air which had not been confined by lime-water was diminiſhed in the proportion of 9 to 6 3-4, the meaſures of the teſt being 0.98. Both the mice, though kept pretty warm, laboured alike with a difficulty of reſpiration, ſome time before I put an end to the experiment. In the courſe of it I agitated the lime-water a little now and then, in order to make it abſorb the fixed air the better, by admitting freſh lime-water to the air that had been reſpired."

Mr. Scheele, by the reſpiration of bees, turned a large quantity of dephlogiſticated air entirely into fixed air. See likewiſe Dr. Crawford's experiments upon the reſpiration of a pig: But neither the mice, the bees, nor the pig, had any hypotheſis to eſtabliſh; nor were their lungs ſo capacious as to ſecrete all theſe airs. You may, by laborious breathing, act upon almoſt any air. Mr. Scheele, whoſe knowledge of experiments was deficient to none, ſays, p. 160. "I filled a bladder with air produced from iron filings, and acid of vitriol, (No. 30, letter *c*.) and inhaled it in the manner

manner deſcribed before (No. 48.) I could inhale it no more than twenty times; and having again recovered in ſome meaſure, I again expelled the air from the lungs as much as poſſible, and then drew in again this inflammable air. After ten inhalations I found myſelf obliged to leave off, and found that it was no more inflammable, nor would it unite with lime-water; in a word, it was foul air."

The Doctor may ſoon know by an eaſy experiment, if there is any analogy between the life of animals and the life of flame, the latter turning dephlogiſticated air into fixed air; and alſo, if he will reflect, that fixed air is fifty times more eaſily imbibed by water or moiſture, than dephlogiſticated air. I ſay, the experiment is eaſily made: Agitate fixed and dephlogiſticated airs and water together, and ſee which the water more readily imbibes: That there is water and agitation in the act of reſpiration, particularly the latter, is what every one muſt be ſenſible of. The Doctor, I am ſure, would breathe hard while attempting to eſtabliſh his hypotheſis.

I will now give you Dr. Harrington's explanation of this proceſs, made public by him in 1780. His proofs are clear, and ſuited to the underſtanding of every chemiſt, that is not bewildered and infatuated with the new chemical doctrines, and the experiments of glaſſes, gun barrels, &c. He proves that atmoſpherical air, or at leaſt the richeſt part of it, is formed of fixed air and water highly ſaturated

and neutralized with fire, or with the rays of the ſun. That upon this air's being expoſed to the blood; the blood, as having a greater attraction for the air's fixed fire, attracts it, and leaves the fixed air and the water in the expired air. He alſo proves that as there is a great depoſition of water upon the lungs, ſo it is abſorbed into the blood: And that the water carries a part of this fixed air along with it into the blood, which ſeems probable; becauſe the water has fifty times a ſtronger affinity for fixed air, than it has for dephlogiſticated air. Likewiſe, that the high factitious dephlogiſticated air, is not ſo eaſily turned to fixed air. This is evident in the reſpiration of the mice juſt now quoted; therefore the mice could not live ſo well in it, as in atmoſpherical air, till they had injured it. If Dr. Prieſtley and other chemiſts will not bewilder themſelves with freſh experiments, but only attended to Dr. Harrington's explanation of thoſe already made, they would ſoon come to the true knowledge of what they are in queſt of. Nay, they will attain to this, if Dr. Prieſtley and others but act up to what he himſelf ſays in the preface to his laſt vol. "Let us not, however, contend about *merit*, but let us all be intent on forwarding the *common enterprize*, and equally enjoy any progreſs we may make towards ſucceeding in it; and above all, let us acknowledge the guidance of that Great Being, *who has put a ſpirit in man, and whoſe inſpiration giveth him underſtanding.*"

Dr.

Dr. Harrington intended to have given a full hiſtory of animal and vegetable life, with all the phenomena of the animal heat, muſcular motion, glandular ſecretions, &c. The explanation how the air acts upon the blood, being only a preface to that work. But the treatment he has received, has checked his reſearches. His labours (to uſe his own words) inſtead of having met with that encouragement, which might incite him to promote the advantage of ſcience, have been treated with neglect and inſolence. Owing to this it is, that latterly his time has been employed, not in the *inveſtigation of nature's truths;* but in the *correction of modern chemical errors.* I ring the alarm-bell to *ſcience* and to *truth.*

The Doctor is now obliged to adopt the opinion that dephlogiſticated air contains a great quantity of water. But had he attended to Dr. Harrington's experiments, he would have ſeen, that animal moiſture is principally received from the air, upon its decompoſition by the lungs; and likewiſe, that the moiſture of expired air comes from the air, and not from the lungs. Dr. Harrington has ſhewn, in his Thoughts on Air, how this water of compoſition, as he calls it, is united to the acid and the fixed fire.

Dr. Harrington's ſyſtem of airs did not originate from the experiments of gun barrels, &c. but from an accurate obſervation of nature. Take an egg, and examine its fluids; they are mild and bland, conſiſting of a watery *mucus;*

but

but expoſe it to the air, and it will become highly putrid: Its fluids are changed from a pure ſtate to one highly alkaleſcent and noxious, emitting a moſt nauſeous ſtench. I need not enlarge upon this topic, as every one muſt be acquainted with the phlogiſtic alkaleſcent ſtate of its putrid fluids. Then, how muſt the egg have received all this alkaleſcency? From the air, no doubt, as there was no other body that acted upon it. The air, according to their opinion, is highly phlogiſticated; then the wonder ſtill increaſes, as we have not only the phlogiſton of the putrid egg, but alſo the phlogiſton which the air has received, to account for. According to Dr. Prieſtley's explanation, one egg will phlogiſticate two thouſand gallons of air; therefore, agreeable to his experiments, this egg muſt have given to the air, one thouſand gallons of inflammable air; or, according to Lavoiſier, ſome ounces of charcoal; or, to others, an immenſe quantity of phlogiſton.

My reader muſt excuſe me, if I cannot bring myſelf to believe, that this fine, mild, bland lymph could poſſeſs ſo much phlogiſton, as not only to turn it into a ſtate ſo highly putrid and offenſive, but even to phlogiſticate ſuch a quantity of pure air. And I beg leave to diſſent from the opinion of thoſe chemiſts who believe it; their reaſons and chemiſtry being ſo very different from mine. To convince their judgment that they are wrong, is perhaps not in my power; yet I hope, I ſhall be able to convince their ſtomachs. Let thoſe who hold the opinion

opinion I am combating, firſt ſwallow a ſound egg, and then a putrid one, and I am ſure their ſtomachs will be convinced, and of courſe their heads; the ſympathy and relation between theſe two parts of the body, being ſo near and intimate: After this doſe they would, I think, be of my opinion. Mr. Lavoiſier is ſo much ſtruck with the ſtate of putreſcency, that he wonders that chemiſts have not been more attentive to it: Some chemiſts have not paſſed it by; for, if he will pleaſe to attend to what Dr. Harrington ſays in his publication in 1780, he will there ſee the proceſs of putrefaction fully demonſtrated and proved. In the putrefaction of vinegar, the acid is turned alkaleſcent. See Dr. Harrington.

It will, no doubt, be expected that I ſhould take notice of the Haerlem experiment of paſſing the electric ſpark through water. "They employed a tube hermetically ſealed at one end, and the other opening into a reſervoir of diſtilled water. At the ſealed end a golden wire was inſerted ſo as to project an inch and half into the tube. At the diſtance of five inches and one-eighth was another wire, which was carried through the open extremity: The firſt was connected to the prime conductor of a very powerful electrical machine; the other to the outer ſurface of a Leyden vial, the bottom of which communicated with the prime conductor, and which had a ſquare foot of coating. As thoſe wires, therefore, formed, by means of the water, the electrical circle, the ſpark

was

was paſſed through the fluid; and ſoon after ſome very powerful ſhocks had been given, bubbles of air appeared in the water and gradually collected into large maſſes. When the column was ſo great as to extend to the end of the ſuperior wire, the whole inflamed, and a very ſmall reſiduum was left." Now the phenomenon is nothing elſe but this. The electric fire unites itſelf to a part of the water, forming an electrical vapour or cloud, the ſame as the phenomenon we ſee in the clouds. When the vapour is ſo conſiderable as to extend to the point of the wire, and come within the influence of the electrical fluid, it ſtrikes the vapour or cloud, ſo as to break it with an electrical exploſion; juſt as when a cloud is ſtruck with the electrical fire, it will burſt and explode.

Dr. Harrington has fully ſhewn how theſe phenomena take place in the exploſion of inflammable and dephlogiſticated airs. In the exploſion, the fixed fire of theſe airs is ſet looſe; the acid of the dephlogiſticated air, and the water of both airs being in the reſiduum. If there is a great proportion of inflammable air, it (as he moſt ſatisfactorily ſhews) flies off with the acid, producing the exploſion; but if in leſs proportion, it only decompounds the dephlogiſticated airs into an acid and water. But for a more full explanation of all theſe phenomena, the reader may conſult his Letter to Dr. Prieſtley and others, in which he will

ſee

ſee theſe experiments diſcuſſed in a very clear and extenſive manner.

The Royal Society have paid great attention to every experiment upon air: And indeed it is, and has been for ſome time paſt, a favourite ſubject of inveſtigation all over Europe. I have given a regular detail of the different papers upon that ſubject, preſented to the Society, ſince Dr. Harrington's Letter was publiſhed. Mr. Milner's paper comes now under our obſervation. He thought he detected the volatile alkali, by paſſing dephlogiſticated nitrous air through a gun barrel. There is nothing wonderful in this: For, the effect of paſſing the nitrous acid and phlogiſton through a heated gun barrel would be this; the acid then, having as it were an opportunity of ſaturating itſelf with more phlogiſton from the iron, would be ſo concentrated as to give the alkaline ſmell. But this ſtrongly confirms our hypotheſis; viz. that alkalies are a concentration of fire; and that the volatile alkali is a high concentration, ſimilar to inflammable air. Nay, it proves itſelf; as the volatile alkali may be formed into inflammable air; for Mr. Milner's dephlogiſticated nitrous air, which produced the alkali, is formed from the inflammable air of the metal.

But, as our modern chemiſts are apt to wonder at any experiment they think new, ſo Mr. Milner calls the production of the nitrous air, by paſſing the volatile alkali through manganeſe, in a heated gun barrel, a wonderful tranſ-

mutation. He thought that the nitrous air was produced by the dephlogiſticated air of the manganeſe and the phlogiſticated air, which according to him, the volatile alkali was ſuppoſed to poſſeſs. However, the experiment is nothing more than this. The gun barrel heated by the charcoal, expels the dephlogiſticated air of the manganeſe; and, upon the volatile alkali coming in contact with it in the red hot tube, ſets them on fire; and the dephlogiſticated air is, by the intenſeneſs of the fire, decompounded into the nitrous acid. This will be the effect, if they are fired together out of the gun barrel; which is ſo evident, that none can poſſibly miſtake it. The acid then, (as Mr. Milner judiciouſly obſerves) acts upon the iron, and forms nitrous air.

Now, as this experiment can admit of no other explanation than the one we have given it, let me aſk Mr. Milner a few queſtions. Will not manganeſe, when expoſed to the heat that he placed in it, yield dephlogiſticated air? Will not this dephlogiſticated air and the volatile alkali, when formed into a vapour, burn together? Will not the combuſtion, under that intenſe degree of heat from the gun barrel, produce nitrous acid? The anſwers to theſe queſtions, which may be eaſily given, are a ſufficient explanation of the phenomena. Mr. Milner's explanation is a very curious one, founded upon the preſent theories of the day. For, if it were from the union of the dephlogiſticated air, and the phlogiſticated air of the volatile

volatile alkali, ſuppoſing it contained this air; and ſuppoſe theſe two airs would produce the nitrous acid, yet it would not have the effect he aſcribes to it. To make the experiment according to his hypotheſis: Let him paſs the phlogiſticated air, and not the volatile alkali, through the manganeſe, and ſee if they will produce the ſame phenomena. In this caſe, the phlogiſticated air, being free and diſengaged, ſhould act more ſtrongly upon the dephlogiſticated air of the manganeſe: But that the volatile alkali does not poſſeſs any phlogiſticated air, being only one uniform fluid, ſeems true, from the following obſervations: Upon its being burned, a part may be left not ſo highly ſaturated with phlogiſton; juſt as in metallic earths, there is generally part left, which is not capable of being reduced: Even the inflammable air, from metals, has a ſmall quantity of ſimilar dregs. A thouſand examples might be brought to prove, that this is the caſe with all bodies in nature. The atmoſpheric air is a perfect homogeneous fluid, the ſame as milk: One part of it only, when ſeparated, conſiſts of a richer fluid, ſimilar to cream; but while undecompounded, it is, like milk, one homogeneous fluid.

I cannot help ſmiling at the preſent mode of experiments, and the conſtruction of thoſe experiments, that are at preſent adopted. Mr. Milner, in paſſing the vapour of the nitrous acid through a heated gun barrel, filled with iron filings, ſays, "Firſt, nitrous air is formed,

then dephlogiſticated nitrous air; and laſtly, phlogiſticated air." Theſe changes to take place upon the vapour, while it is paſſing through the hot barrel, is a moſt ſudden tranſmutation indeed; but ſuch are their theories, and ſuch their reaſonings.

I read in the analytical review (juſt before the laſt ſheet was printed) an extract from Mr. Weſtrumb's experiments *. They very forcibly corroborate

* Mr. Weſtrumb obſerved, that on plunging a ſpoon filled with cinnabar into dephlogiſticated marine acid air, a copious white vapour aroſe. He afterwards introduced a ſtick of fir well covered with powdered cinnabar into the ſame kind of air, when a ſimilar vapour aroſe, the extremity of the ſtick was covered *to a coal*, and on performing the experiment in the dark, *flame* was frequently viſible. Thirty or forty inches of the air, taken at the latter part of the proceſs in which it is made, was confined over water, and brought to a temperature of 60° or 70°. Opening it ſuddenly, and throwing into it thirty or forty grains of cinnabar, *a briſk inflammation aroſe*, accompanied with a ſuffocating vapour; and a portion of marine ſalt of quickſilver was found in the veſſel, without the leaſt *veſtige of ſulphur*. Thirty grains of ſulphur being treated in the ſame manner, it was partly decompoſed, but without any appearance of inflammation or vapour. Camphor likewiſe did not inflame; but a portion of it aſſumed an oily form. Oil of cloves was heated without undergoing any change. Oil of turpentine grew warm, and was converted into reſin. Spirit of wine grew hot, ſmoked, but did not inflame, and acquired an agreeable ſmell, yet was not changed into æther. Magneſia combined with the gas without any heat ariſing. Aerated volatile akali formed ſal ammoniac, with heat, and ſome vapours. In all theſe experiments there was an abſorption of gas. Golden ſulphur of antimony was converted into white vapours, without inflammation, and produced butter of antimony: If introduced on the point of a ſtick of fir, the extremity of the ſtick was converted into coal. Kermes mineral inflamed with a clear riddiſh

corroborate my principles of combuſtion, and that phlogiſton is concentrated fire: For, tho' Mr. Weſtrumb made them with a different view, and to eſtabliſh opinions in oppoſition to ours; yet, if my reader will carefully run his eye over that extract, he cannot but conſtrue the experiments conformable to our hypotheſis, that phlogiſton is concentrated fire. Had I ſeen them ſooner, I might have interwoven them into my arguments.

I beg

diſh *white light, accompanied with ſparks*, and butter of antimony was produced. Antimony produced a beautiful clear, *white light, with ſparks*; the bottom of the glaſs became red-hot and the reſiduum was butter of antimony. Regulus of antimony gave the ſame reſults. Regulus of arſenic inflamed with a beautiful green and blue flame, and butter of arſenic was produced. Regulus of biſmuth gave a clear, bright, blueiſh flame; and the reſiduum was marine ſalt of biſmuth. Regulus of nickel burnt with a yellowiſh white flame, with ſparks, and left marine ſalt of nickel. Regulus of cobalt gave a blueiſh white light, and produced a marine ſalt of cobalt, capable of making ſympathetic ink. Regulus of zinc burnt with a beautiful white flame, gave fewer ſparks than biſmuth, and produced marine ſalt of zinc. Tin filings burnt with a weak blueiſh flame. For this experiment, and the two following, there ſhould be no water in the veſſel, and the gas ſhould be of a deep yellow. Lead filings burnt with a clear, white, and ſparkling flame. Copper filings gave a red flame. Iron filings burnt at the bottom of the veſſel with a red light. In all theſe experiments marine ſalts of the different metals were produced. Forty grains of iron filings in eighty inches of gas, a ſmall quantity of water being in the bottom of the veſſel, burnt in the ſame manner, without detonating, or diſengaging inflammable air: Yet inflammable air ought to have been produced, if it ariſe from the decompoſition of water, and the baſe of vital air really conſtitute the difference betwixt common and dephlogiſticated marine acid. Quickſilver did not enflame, but loſt its fluidity, and was partly decompoſed. In all the experiments

mentioned

I beg leave to ſuggeſt one hint, I hope my treatment will not be the ſame as Dr. Harrington's; for, ſimilar opinions to his have been given, without ever hinting by whom they were firſt given; nay, even in ſome caſes delivered by others as their own.

I flatter myſelf that I have extended Dr. Harrington's theory of the aerial ſyſtem, therefore I ſhall keep watch very tenaciouſly. But his theory muſt ſtand firm, and no depredations upon it can eſſentially injure it; there being no doubt but time will do it juſtice: But it is very hard to have that time ſo long procraſtinated. He, from great labour and aſſiduity, and by ſtrict and accurate obſervations and experiments in the different philoſophical

mentioned ſince that with the Kermes mineral a large quantity of vapours of marine acid with a metallic taſte, and diſagreeable ſmell, approaching that of burnt horn aroſe. Aerated volatile alkali being firſt thrown into this gas, then an equal quantity of cauſtic volatile alkali, and afterwards a ſmall portion of regulus of antimony, a briſk detonation enſues. Cauſtic volatile alkali produces heat, and white vapours. Two drams of cauſtic volatile alkali thrown at once into thirty or forty inches of gas, produce a red flame, reſembling the aurora borealis: Sometimes a noiſe is heard, which might be miſtaken for detonations, but it is only occaſioned by the water reduced into vapour, for the heat is great: The produce is ſal ammoniac. One part of the charcoal, and two of regulus of antimony, inflame like antimony. Thirty grains of mineral coal inflame in fourſcore inches of this gas at a temperature of 90°. Many other ſubſtances are ſuſceptible of inflammation in this gas.

From theſe experiments, Mr. Weſtrumb draws many concluſions in favour of phlogiſton, and ſhows, that all the phenomena are not explicable on the antiphlogiſtic ſyſtem. See the Analytical Review for December, 1790, p. 471.

phical departments, formed his preſent hypotheſis, which has ſtood firm ſince he firſt gave it to the public: And though it did not come out with the pompous apparatus of modern chemiſts; yet, while their opinions have been as changeable as the wind, (for, upon the whole, there never was ſuch a heterogeneous mixture of opinions; the only juſt ones being thoſe which are the ſame as his) his ſtood firm as a rock; time, inſtead of impairing, has given them more ſtrength.

He firſt ſhewed, by the moſt convincing experiments and arguments, that water makes an eſſential conſtituent part of atmoſpherical air; yet the idea was ſcouted. But now one of the firſt leading aerial chemiſts gives it likewiſe as his opinion, but without ever adverting to Dr. Harrington: Yet his preſent hypotheſes of airs, which ſeem to change as often as the day, I will declare, with confidence, are a maſs of ſtrange abſurdities.

It may be conſtrued, that I have treated the opinions of ſome eminent men rather too cavalierly. The anſwer I make to that is, the ſevere treatment which Dr. Harrington's opinions have met with. I think no one who views his ſyſtem, as being the true one, can look cooly upon the uſage he has received. Not the leaſt attention paid to his labours; but, in ſome inſtances, directly ſimilar opinions given by others without once ſuggeſting his name: But, I ſhould hope it has been from inadvertencies; and that the ſame conduct will

take

take place no more. With ſuch a perſuaſion I ſhall now reſt; and to confirm that, they muſt either acquieſce with Dr. Harrington's ſyſtem; or, at leaſt, give it a fair and open diſcuſſion: But whatever way they do, to do it publicly.

Before I bid adieu at this time to the public, I would aſk a favour, (though in propriety of language it is no more than juſtice) that every friend of ſcience would ſo far intereſt himſelf in the behalf of injured merit, as to allow a candid inveſtigation of the principles laid down in this Treatiſe. The characteriſtic of this free, learned, and generous nation is, to do juſtice to men and things, and to make every literary character, however much exalted, amenable to their tribunal.

This TREATISE *will be publiſhed on March the 7th*, 1791. *My Reader will, no doubt, anticipate my reaſon for naming it here.*