

Some new experiments, with observations upon heat, clearly shewing the erroneous principles of the French theory : also, a letter to Henry Cavendish, containing some pointed animadversions; with strictures upon some late chemical papers in the Philosophical Transactions, and other remarks / by Robert Harrington.

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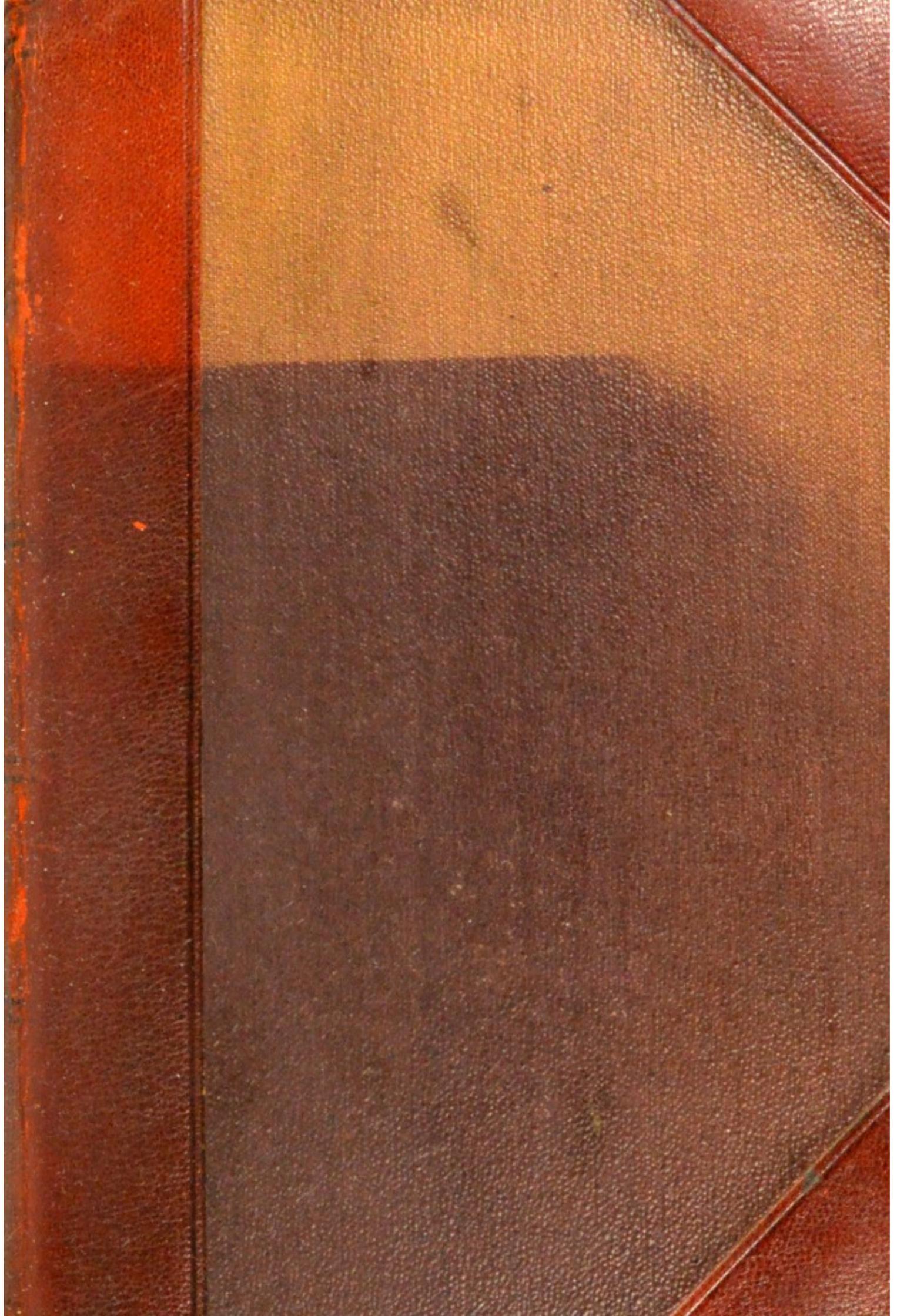
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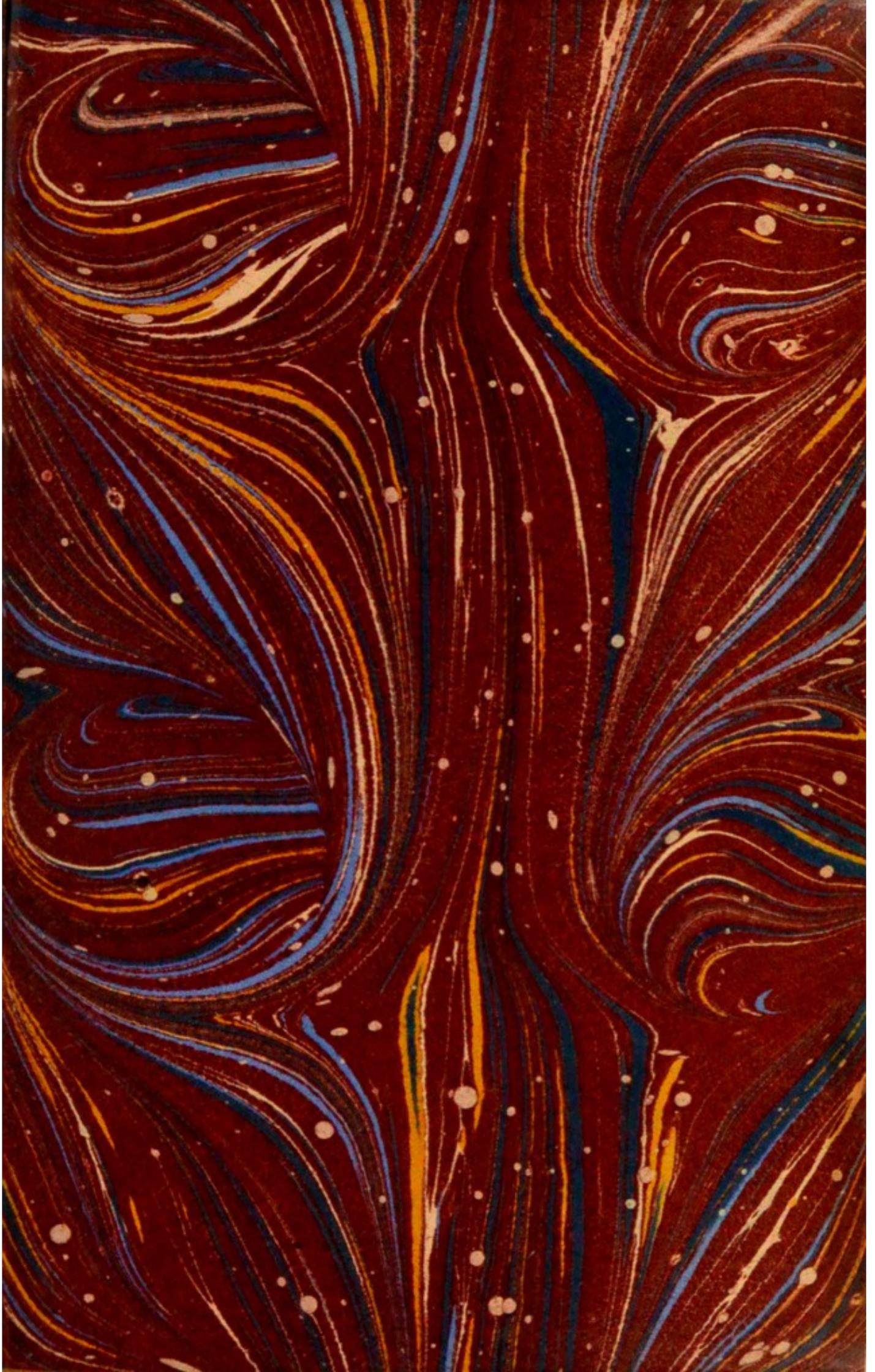
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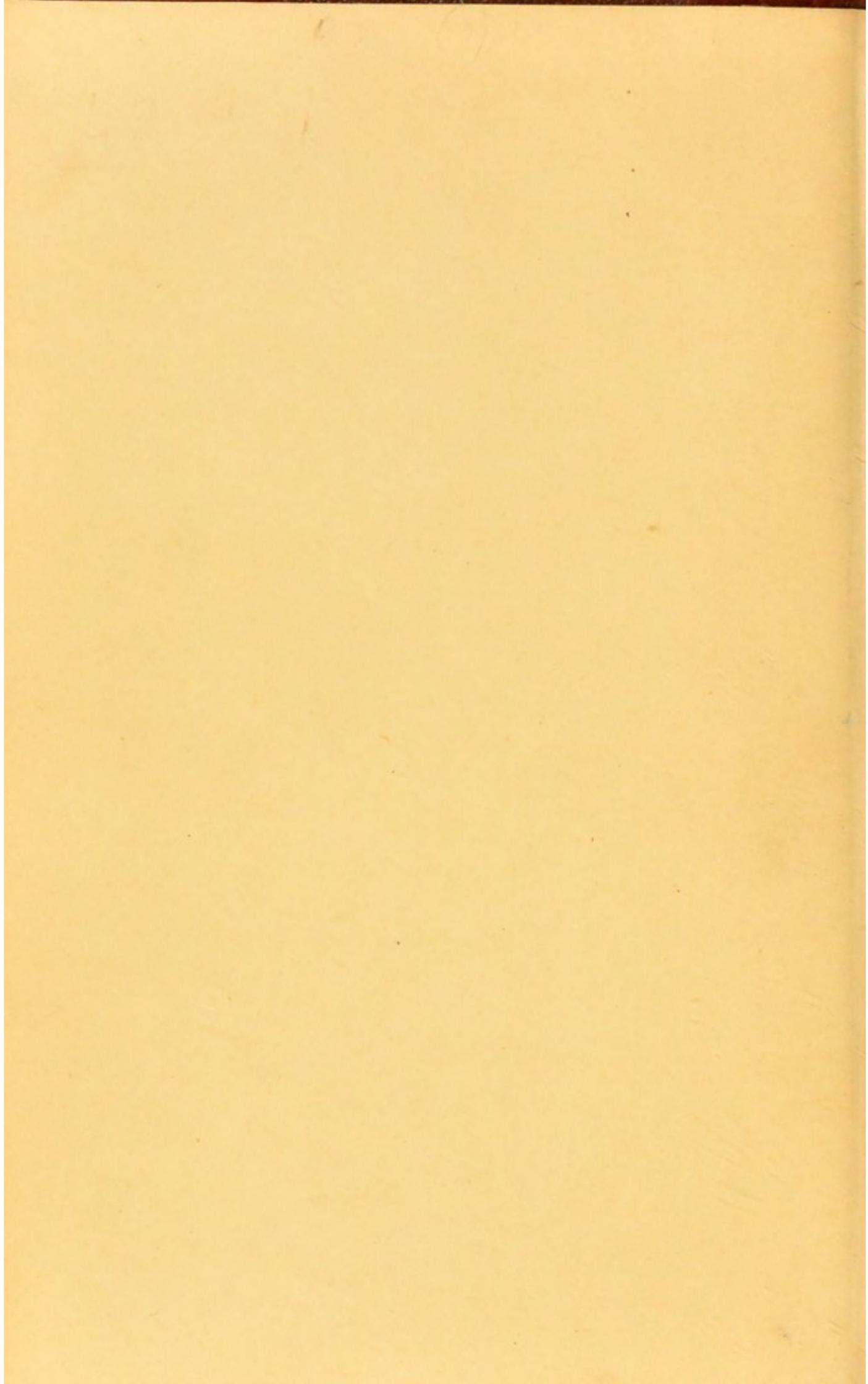
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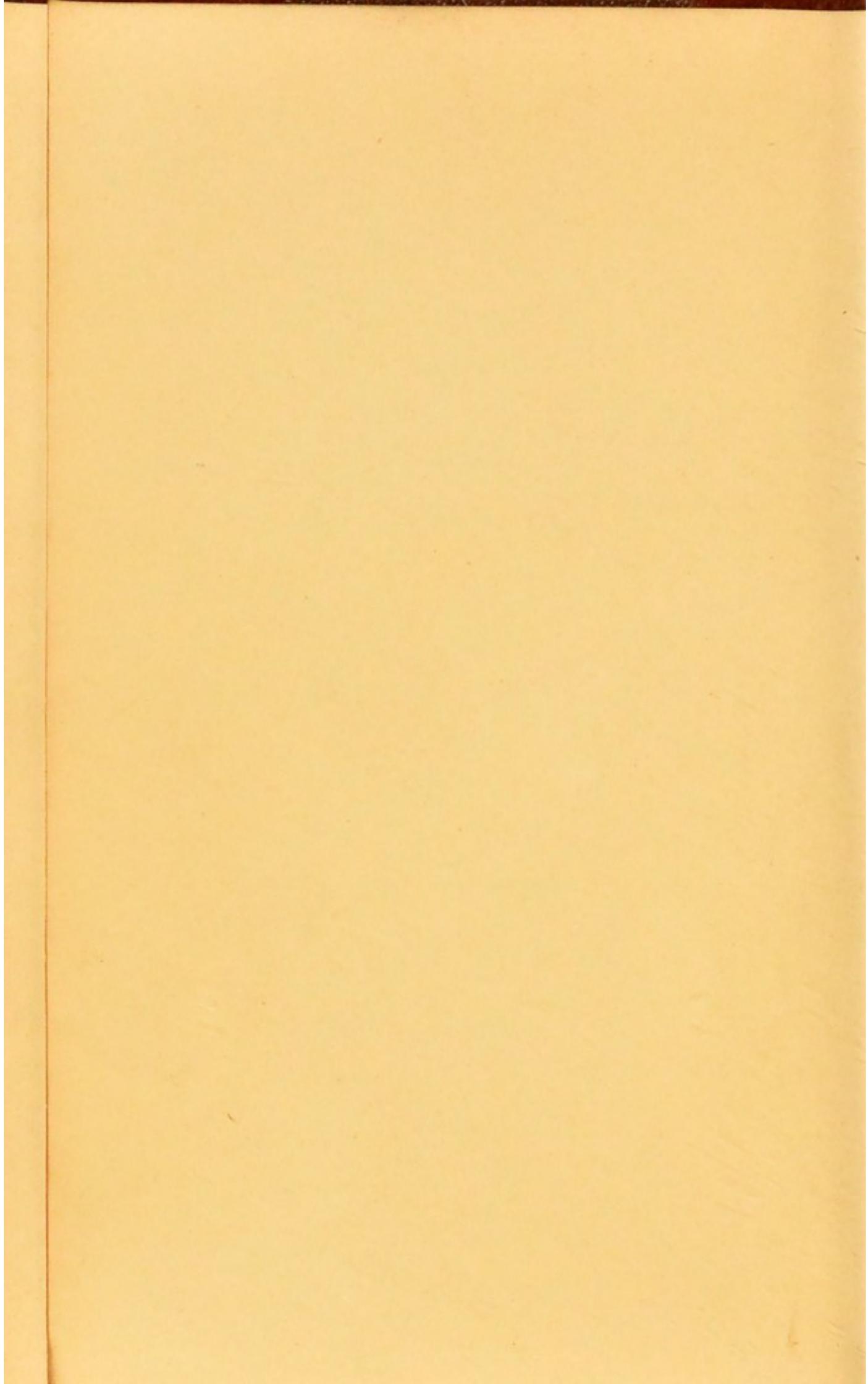














at that time, must rely upon others, and
it is his duty, according as they find
him; and hence, that those who have
the highest talents of reasoning, will
not be less so, as they are, of
which many will be surprised to hear how
the highest application of common sense
is not sufficient to give them the ab-

A Letter to the Honorable

James Oglethorpe Esq. Secretary of the

Georgia Trustees

in Relation to the

State of Georgia

By James Oglethorpe Esq. Secretary of the

Georgia Trustees

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SOME NEW
Experiments,
WITH
OBSERVATIONS UPON HEAT,
CLEARLY SHEWING
THE
Erroneous Principles
OF THE
FRENCH THEORY.

ALSO,
A Letter to Henry Cavendish, Esq.

CONTAINING SOME
Pointed Animadversions; with Strictures upon some late
CHEMICAL PAPERS
IN THE
PHILOSOPHICAL TRANSACTIONS,
AND
OTHER REMARKS.

BY ROBERT HARRINGTON, M. D.

“ *Magna est veritas et praevalabit.* — CICERO.”

“ ————nothing extenuate

“ Nor set down aught in malice. — SHAKESPEARE.”

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1798.

1800

THE

OBSEVATION UPON HEAT,

AND

OF THE

FRENCH THEORY.

BY

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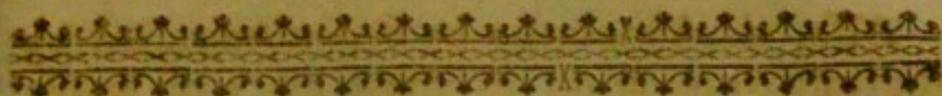
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SOME NEW

EXPERIMENTS,

WITH

OBSERVATIONS UPON HEAT.



THE repulsion of fire is one of the most interesting subjects that chemistry can investigate; and it is a principle which has never been introduced in explaining the phenomena. Fire and matter have a very strong attraction for each other, and this attraction, when it takes place, that is, when fire and matter are united together, can only be decomposed in two ways. First, by other bodies having a greater attraction for either of the compound bodies than they have for each other. As in lime; thus the calcareous earth has a strong attraction for fire, producing lime; but, if an alkali is added to the lime there is a decomposition; the alkali having a stronger attraction for the fire, attracts it from the calcareous earth; and the fixed air of the alkali is attracted by the calcareous earth.

Also, the lime may be decomposed by the joint operation of fixed air and water, but neither can do it separately.

But, Secondly, the most common way by which fire is set loose from its chemical combination, is by combustion; which operates principally by repulsion. Thus, when a body is set on fire, the pure air's fire is set loose, and also the combustible body's fire, which produces so strong a heat, that the fixed fire, which both the air and the burning body possess, is repelled from its chemical combination, so as to become free, or actual fire. A spark being applied to the combustible body, sets free the fire of the air contiguous to it, then both fires act by repulsion, upon the combustible body's fire, and so on; as one part of the air and burning body's fire is set free it acts upon its neighbouring part till the whole body is consumed. Metals attract their fire very strongly, so that they require a very strong application of free fire to be decomposed: gold will not burn; (without the aid of nitre) it comes out of the fire pure. But many of the other metals will burn, iron, for instance; but this decomposition of the metal, or the earth of iron, parting with its fire, is greatly assisted by the earth having a great attraction for the water, and fixed air, which pure air is formed of; so that, in the combustion, they will both enter the earth of iron. This is a general principle

ciple in chemistry, that, as bodies part with one body, they greedily attract others; metals have different degrees of attraction for fire, the noble metals have the strongest.— See Gent. Mag. for 1792, part 2, p 619. §

Therefore,

§ ‘ In addition to the proofs which I have given in my different publications, that the body which Stahl, Scheele, Dr. Priestley, &c. call phlogiston, is fixed fire, and not an element *sui generis*, I shall observe,

‘ That, by exposing iron to the nitrous acid, an active fermentation, and a great generation of nitrous air, is produced, and the iron is reduced to a calx. If iron and water are exposed to atmospheric air, the iron will be reduced to a calx, the pure part of the air will be imbibed by the calx, and an oily scum will swim upon the water. This was the result of Mr. Scheele’s experiment; who says, “pure water only can produce inflammable air from iron; it is a scum which constantly appears on the surface of the water after it had stood over filings for some weeks, and has been somewhat stirred.” If this oil is carefully separated from the water, it will, with the nitrous acid, form nitrous air; or, if applied to the calx of the iron, the iron will be reduced, and, in its reduction will part with the air it had imbibed from the atmosphere, not in the state of pure, but of fixed air. This I have already fully explained in my former publications.— Hence it appears that it is an oily body which forms the nitrous air, and the earth of the metal into its metallic splendour. If heat is applied to this oily body, it will form inflammable air, and, if burned in the state of oil, will turn pure air into fixed air: but (as I have elsewhere fully proved) in the state of inflammable air, it will turn it into an acid and water.

‘ And, to corroborate this doctrine, if I take common oil, it will produce the same phenomena, i. e. it will reduce the calx, and with the nitrous acid will generate nitrous air; or, if I burn it with pure air, it will turn into
fixed

Therefore, when fire alone is applied to the decomposition of combustible bodies; the most intense degree of it is required, and the most intense degree of fire that the art of man can apply, is the electrical fire. See Mr. Cuthbertson's experiments.

“ Mr.

fixed air; or, if I form it into an air by heat, as Dr. Priestley has done, it will explode with pure air, leaving an acid and water in the residuum. Atmospheric air, when ignited, loses the fire which neutralized its fixed air and water, which are consequently precipitated. This is strikingly seen in passing the electric spark through it; it is then formed into fixed air and water. And this is the case when iron is burned, and it is absorbed by the iron.

‘ If filings of iron are dropped into water impregnated with fixed air, the water and fixed air will attract the iron, the acid air and part of the water will be imbibed by the calx, and the phlogiston of the metal will be separated. But, agreeably to the opinion of some chemists, the fixed air should not be imbibed altogether, but only be decomposed from the pure air, of which they suppose it to be formed, and should enter into the calx, and the carbone be left in the water; which is not the case. And I am aware that many will say, the iron is calcined by the decomposition of the water, and not of the fixed air, in this process. But as inflammable air is generated here the same as when vitriolic acid and water are used, we are right in supposing it to be from the same cause.— And not a doubt can remain but that it is from the fixed air entering the calx, as it disappears in the process. Then certainly it is the vitriolic acid that enters the calx in the vitriolic solution. And, that not a doubt may remain but that the operation in both cases is the same, let water be impregnated with fixed air, then acidulate an equal quantity of water to the same proportion of acid with vitriolic acid, the smallest quantity of which is sufficient; then
let

“ Mr. Cuthbertson has since found that, with a machine with two plates of eighteen inches diameter, he could charge a single jar of 160 inches of coated glass so highly as to make an inch of the smallest iron wire vanish into smoke, and three inches exhibit the phenomena of filaments floating in the smoke into which the wire was converted.”

And

let iron be added to each of these portions of water, thus acidulated, and in both the acid will disappear, entering the iron, forming it into a calx, and what has been called the phlogiston of the iron will be separated. And to adduce a still more unexceptionable proof; if the solution of iron, which is formed from the fixed air, be precipitated by the caustic alkali, or by other means, and an acid be added to the precipitate, fixed air will be expelled from it, pure and unchanged.

But, as Stahl and the philosophers after him, seeing phlogiston pass from one body to another, and having no idea of the chemical attraction of fire, supposed it to be a body different from fire. I shall shew, in opposition to that idea, that *actual* fire, or perhaps, more properly speaking, fire in its free disengaged state, when sensible to our feelings, and to the expansion of mercury in the thermometer, is equally under the influence of chemical attraction, passing from one body to another; and that, during its transitions, being under the influence of chemical attraction, it does not diffuse itself, but passes into the attracting body the same as an alkali into an acid.

‘ I would desire the reader to pay particular attention to what may be brought in proof of this, because it has never been properly understood or attended to.

‘ Lime and caustic alkaline salts have lost their fixed air, or aerial acid and water, and in their stead have united to fire. If lime, for instance, is mixed with the fixed
alkaline

And this electrical fire, from its intensity, can decompose pure air, which is nature's great agent for combustion; for, Dr. Priestley, Mr. Bergman, &c. burnt pure air into fixed air, by the electrical fire.

Also,

alkaline salt, the alkali having a stronger attraction for the fire of the lime than for its own fixed air, will attract the fire of the lime, which has a less attraction for it than the alkali has; consequently, the fire will leave the lime, and be attracted by the alkali.* This cannot be from the lime having a stronger attraction for the fixed air, or aerial acid, than the alkali, as alkalies attract acids more strongly than calcareous earths do.

In consequence of this mistake, Dr. Black is wrong and contradictory in his table of attractions. He says,

Acids.	Fixed air.
Fixed alkali,	Calcareous earth,
Calcareous earth,	Fixed alkali,
Volatile alkali,	Magnesia,
Magnesia.	Volatile alkali.

But he ought to have placed the different bodies under the head of fixed air, the same as under the acids, for the change of affinity is owing to the fire which the calcareous earths and the magnesia, in the state of lime, possessed, and which Dr. Black does not so much as acknowledge it does possess, not having a chemical knowledge of fire, or its affinity.

But a still stronger proof that alkaline salts have a more powerful attraction for fire than for fixed air is this: expose the dry caustic alkaline salt, or lime, to dry fixed air, and they will not attract it, nor part with their fixed fire; but add fixed air and water to the caustic salt, or lime, both acting together, their joint attraction will expel the fire, though neither can do it by themselves.

* Lime, from the fire which it possesses, will assist in reducing metals; i. e. I have found a calx will sooner be reduced by being mixed with lime than with calcareous earths.

Also, Mr. Cavendish burnt the pure air of the atmosphere, and the pure air of the laboratory, into the nitrous acid; that azote had nothing to do in this experiment, I have fully proved in the sequel.

Therefore,

‘ It is the very same with the vitriolic acid and iron.— The earth of iron attracts so strongly its fixed fire, that the vitriolic acid cannot rob the earth of iron of it; but, if the acid’s attraction be assisted with water, the earth of iron having a strong attraction for the water, their joint powers will decompose the iron, its earth attracting the water and the acid, whilst the fixed fire is expelled as inflammable air.

‘ If the concentrated vitriolic acid be applied to the iron, it will not act upon it, because the attraction which the earth of the iron retains for the phlogiston, is stronger than that which the acid has for the earth. But, if the vitriolic acid is diluted with water, they both act together; the acid attracts the phlogiston and the earth, whilst the water attracts the earth.

‘ That the earths of metals have a very strong attraction for phlogiston may be strikingly found in many instances. Silver so strongly retains this attraction, that the vitriolic acid will not act upon it; but apply the nitrous acid, and it will attract its phlogiston or concentrated fire, and fly off with it in the form of nitrous air.— Gold has so strong an attraction for its phlogiston, that the nitrous acid will not dissolve it. It is well known that the action of two acids, the marine and the nitrous, is required at the same time, or that the marine be dephlogisticated.

‘ We find that metals resist combustion in the same manner as they do the action of acids. Gold will not burn though exposed to the strongest fire; it may sublime, but will not consume, because gold strongly retains its fire. We shall shew in the *aurum fulminans* that its attraction for the concentrated fire, or phlogiston of the volatile alkali is so very powerful, that, being exposed to
 the

Therefore, impreſt with this theory, I have been performing ſome experiments with this electrical fire, making it as intense as poſſible, in order to repel the body's fixed fire,

the ſlighteſt degree of warmth, it will ruſh into the alkali, and decompoſe it into actual fire.*

Upon the ſame principle, the nitrous acid (as is ſhewn in my publications on this ſubject) will act upon eſſential oils; and, owing to the attraction of gold and of the nitrous acid for concentrated fire, they will ruſh into the alkali and the oils, and ſet looſe in the efferveſcence ſuch a quantity of actual fire as to produce ignition; the volatile alkali at the ſame time exploding, as it does in the combuſtible ſalt lately diſcovered by the French chemiſts, which is formed of the nitrous acid and the volatile alkali.

It is evident that the nitrous acid and the earth of gold act in conſequence of their attraction for the fixed fire of oils and alkali, for, in the proceſs, the nitrous acid becomes what chemiſts call phlogiſticated. However, the proceſs will not ſucceed without the acid be highly dephlogiſticated; and no other calx but the earth of gold will do, which (as has been already ſhewn) has ſo great an attraction for phlogiſton, that the gold is reduced after the exploſion of the *aurum fulminans*. It was from this attraction that Mr. Scheele found the earths of gold and ſilver to be reduced by only gently throwing the rays of light on them.

The calces of metals have a very ſtrong attraction for phlogiſton; and we are acquainted with no bodies in nature capable of greater attraction than acids, nor any that can decompoſe the metals but them. In reſpect to gold, it has been already obſerved, that the influence

* Mr. Scheele found the earth of gold to have ſuch an attraction for the volatile alkali that it would actually attract it from the vitriolic acid, decompoſing the vitriolic ammoniac. And the volatile alkali is known to be a phlogiſtic body, forming inflammable air with heat. See his Eſſays.

fire, as actual, or free fire. From the analogy of nature's great agent, viz. pure air; as its fire is not only most easily set loose, and by

of two acids is required. Nay, it is evident from Mr. Scheele's experiments, that the calx of gold can decompose the vitriolic ammoniac, or take from it its phlogiston. When this is done by combustion, it is by setting loose their phlogiston as actual fire. If gold is soluted in *aqua regia*, and another metal added to the solution, the calx of gold having a superior attraction for its phlogiston, will rob the metal of it, and will be precipitated with its metallic splendour. Silver is next in attraction for phlogiston; then mercury and copper. They are well known to precipitate each other according to their different degrees of attraction for phlogiston.

' To explain these phenomena, Mr. Lavoisier says, it is not owing to these bodies having an attraction for phlogiston, agreeably to the gradation just mentioned, but to their having a less attraction for dephlogisticated air than the baser metals.

' This cannot be so, for the calces of lead and mercury part with their dephlogisticated air much easier than the other metals.

' Let us attend to a solution of gold in the dephlogisticated marine acid. If the gold is precipitated by an alkali, the acid will be found in its phlogisticated state, or as common marine acid. Take a quantity of this solution, and add essential oils to it, or any other phlogistic bodies you please, and you will find the oil attract the calx of the gold from the acid, which will receive its phlogiston again, and be precipitated in its metallic form. How can this be explained on the theory of Mr. Lavoisier? The oil which is formed of his carbone ought, according to his explanation, to have united with the dephlogisticated air of the calx of the gold, and to have formed fixed air; yet there is not an atom of fixed air formed in the process. Moreover, dephlogisticated air could not have been imbibed again by the acid, as it is left in its phlogistic state.

by that means acts upon the combustible body's fire, but also as it possesses an acid which aids

‘ Another phenomenon is discoverable from this experiment, namely, that the calx of gold has so strong an attraction for its phlogiston, that it will leave its acid and be united to its phlogiston in this low temperature of heat.

‘ When the above facts are maturely considered, we find every thing militates against Mr Lavoisier's explanation. Besides, it is well known that the oils and pure air will not unite but in the temperature of oils burning. Gold, from its affinity to its phlogiston, resists calcination; for, if the calx be precipitated even the acetous acid will dissolve it.

‘ Mr. Lavoisier's doctrine of fixed air has nothing to support it. If I drop oil, or any phlogistic body, into the marine acid, either in its dephlogisticated, or phlogisticated state, they will form only marine acid air and inflammable air, not one atom of fixed air. Will any chemist be hardy enough to contradict the evident conclusion which follows from this experiment? It is just the same with the other acids. With these carbonic bodies they form only acid and inflammable airs. When metals are dissolved in acids, they form a calx, a snug body in which to secrete the pure air of the acids; but when oils are dissolved, they ought to form fixed air in the greatest abundance. Hence, agreeably to this doctrine, we should thus have a quicker process for obtaining fixed air than by the solution of calcareous earths in acids.

‘ Now here we have a regular chain of phenomena clearly corresponding with this theory. But let us enquire how our modern hypotheses agree with it.

‘ Mr. Lavoisier, some time after I had endeavoured to set aside the doctrine of phlogiston being an element, from a conviction of its insufficiency to account for the above phenomena, went to the opposite extreme, not even allowing any kind of fixed fire in those processes of metallic reduction and formation of nitrous air, asserting that, in the first, it is merely imbibing oxygen gas, and, in the other, the nitrous acid losing its oxygen gas.

‘ The

aids the combustion, by uniting with the combustible body; it struck me to substitute
a body

'The first thing that strikes us in this doctrine is this; if you take an ounce of the concentrated vitriolic acid diluted with two ounces of water, and apply it to iron, inflammable air only is generated. If you apply the same quantity of the concentrated nitrous acid with an equal quantity of water, nitrous air only is generated.

'To explain these phenomena, it is said, that the water, in the first process, is decomposed, and, in the second, the nitrous acid. Now, I should think that every chemist must allow that it is the acid in both these processes that dissolves the metal. How then can it be conceived that, in one process, the acid should be the calcining body, and the water in the other? If the solutions are examined, the soluting bodies will be found to be the acids in both; for, by adding an alkali, the acid unites to it, and precipitates the earth in both the processes; but water will not dissolve iron by itself.

'The explanation seems evidently to be this (as has been elsewhere shewn): the nitrous acid as is well known, having a greater attraction for phlogiston, rapidly seizes upon it, and from its greater volatility, flies off with it as nitrous air. On the contrary, the vitriolic acid, having a less attraction for it, will not decompose iron without the aid of water, for the earth of which, water has so strong an attraction, that when the water, aided with a great degree of heat, acts upon the iron in the form of steam, it will decompose iron without the acid.

'That inflammable air should be formed when water is added to iron, either along with the vitriolic acid, or by steam, appears to be from this cause: all airs, in entering their aerial form, have a strong attraction for water, the same as the neutral salts have in entering the crystalline form, called, with great propriety, the water of composition. Of this we have a striking instance, as I have before shewn, in nitrous vapour, which is perfectly condensable, unless it passes through water,
and

a body for it, which possessed both fixed fire and an acid. Both sulphur and phosphorus immediately presented themselves as fit bodies for the experiments; I therefore mixed these bodies with the metals most easily, capable of being burnt, as copper, iron, &c.— Pure air also possesses water, which aids its agency in making bodies burn, by entering the burning body as the calx of metals; seen by both

and then it gets the water of composition for its aerial form, and becomes permanent nitrous air. Therefore, in the formation of inflammable air from iron, the water and heat seize upon the phlogiston of the metal, with a small proportion of the vitriolic acid, and they form inflammable air. And if a metal is reduced by fire alone, and then calcined by acids, or by water and pure air, as in the calcination of iron by these bodies, its phlogiston, being separated, will either take the form of an oily scum, or the state of inflammable air. Then we must suppose that the fire has penetrated the metal, being closely attracted by it, so that, being set loose, or parted from the calx, it will appear in its fixed state united with a part of the metal in the state of phlogiston, and which, when burned, turns again into actual fire,

‘ If the vitriolic acid without water is aided by heat, it will equally decompose iron, forming it into an air similar to the nitrous, viz. the vitriolic acid air.

‘ But, as aerial forms are apt to elude the senses, and consequently not to be so easily understood, let us take more substantial bodies, such, for instance, as the different kinds of æthers; these will place the doctrine in so clear a light that the dullest capacity can hardly mistake it.

‘ The vitriolic æther is made by adding the vitriolic acid to spirit of wine, by slow degrees, in the coolest atmosphere; for, I have found that, if the acid is added too rapidly, so as to generate a great degree of heat, they only form a vitriolic acid air and the phlogisticated vitriolic

both fixed air and water, being required in decomposing lime, setting its fire free.— Therefore water would aid these processes; but then, as our theorists have been endeavouring to prove that water is compounded of pure air, we must not make use of it in the experiments.

I burnt pure sulphur and iron, without the agency of pure air, into a vitriolated iron; this I proved many years ago: and also, that I formed from them the vitriolic acid air, and inflammable air. But, knowing that the most intense fire is to be procured from the electrical fire, and, knowing that the intensity is essential to the combustion, or separating by repulsion, the fire which combustible bodies possess. I therefore exposed sulphur and iron to an intense application of electrical fire, and it answered my most sanguine expectation. By heat those bodies are easily dissolved into a mass. I got a small cylinder of iron, better than an inch

olic acid†. But, by uniting them in the atmosphere of a cold cellar, they form the vitriolic æther, the acid and the spirit of wine being so united, that they form the neutral body, æther, the acid disappearing, being covered, saturated, or, more properly speaking, neutralized, with the phlogiston of the spirit of wine.

† Mr. Scheele has written an admirable Essay on *Æther*, in which he clearly proves that the acids unite with the spirit of wine. Speaking of the vitriolic æther, p. 301, he says, "this, I think, will be quite sufficient to prove that the vitriolic acid is a constituent part of the vitriolic æther." And he proves just the same of the other æthers formed of the other acids and ardent spirit.

in length, and about half an inch in diameter; having closed it at one end, I filled it with this dissolved mass of sulphur and iron, about one part of the former, and three of the latter. After having near filled it, I closed the other end of the cylinder, then drilled two holes at each end through the iron cylinder, and through a part of the mass, so that I introduced two golden wires into them; bringing their points near together: the drilled holes being considerably larger than the wires, I poured melted glass into them, so as to insulate all the parts of the golden wires in the mass, except their points. Then, by drawing through the mass the strongest electrical fire, and also by examining that part of the mass through which the fire passed; I found it clearly, beyond all dispute vitriolated.—Copper answers, I think, better than iron for the experiment, and I generally found the internal side of the iron cylinder vitriolated. Sometimes I have thought that the experiment would have answered better if the sulphur and the metals were placed in the cylinder without being melted, the former in powder, and the latter in filings. Sometimes, in these experiments, substituting phosphorus instead of sulphur.

After I heard of Mr. Cuthbertson's experiments on metals, by electricity in the air pump, *in vacuo*, and in the different airs, I performed

performed them in this way. The metals that were to be calcined, were placed in a strong metallic case, with a lining of silk, or other electric bodies, between the metal case and the metals. By this means I was able to perform the explosion repeatedly, upon the same metal. I have also repeated, in Mr. Cuthbertson's way, my experiments on sulphur and the metals.

I have varied these experiments, sometimes making a mixture of different metals with sulphur or phosphorus, as equal parts of copper, mercury, lead, tin, sulphur, or phosphorus; and sometimes iron, or barytes, in the place of one of the above metals.— And also, exposing the above ingredients in a most intense hot crucible. But, two things ought to be attended to in this experiment, the ingredients should not be added till the crucible is red hot at the bottom.— The theory of which, is this. When the ingredients unite together in a mass, they produce so great a heat, that it sets free a great part of their fixed fire: for, bodies which contain a great quantity of fixed fire, when chemically united together, a part of this fire is set loose; as they require more fire to saturate them when separated than when united. And, the quicker they are melted and united together, the better, as more heat is produced, and the greater the repulsion; and, in consequence, the greater separation of free

† a

fire

fire. But, as both the vitriolic acid air and inflammable air are set loose in these experiments, proved by my Letter to Dr. Priestley, Lavoisier, &c. in the year 1788. Therefore, by having the pneumatic apparatus to carry off those airs, it will favour the process; as, when they are confined they hinder the combustion.

Upon hearing that Mr. Cuthbertson had performed some electrical experiments upon the different metals, I was anxious that he should repeat mine upon sulphur and the metals; I therefore wrote to a correspondent of his, the beginning of November 1797, desiring that he would request him to do it, and he immediately wrote to Mr. Cuthbertson; I have calcined mercury, having a small proportion of lead, or tin in it, both, *in vacuo*, in fixed air, or azote, by giving them the strongest agitation in a phial, by a mill.

Dr. Priestley agrees with me, for we both are clear that, in burning pure and inflammable airs, there is an acid in the residuum; but the French chemists have denied it, though clearly in their experiment there was azote generated. But they conducted the process by making repeated explosions; so that, if there was any acid generated, it was constantly exposed to the influence of fire, which might unite with the acid and be discharged with it.

To see whether fire would have any influence

fluence upon the acid, I took a tube of glass whose diameter was very small; into this tube I introduced a golden wire which nearly filled it: having placed it near half way into the tube, just at its point I melted the glass around it, so that it was impervious to air and water; then, at the other end of it I dropt into the tube a few drops of the nitrous acid, and introduced another golden wire; and when the acid was all concentrated, in the middle, between the two gold wires, I melted the glass round the last introduced wire in the same manner as the first. Now here I had the acid placed between the two wires, and the glass closely melted near the point of the wires, so that there was no passage for the acid. Upon passing, for a long time together, the electric fire through the acid, I found, upon examination, it was principally dissipated. That the passages were thoroughly closed upon the wires, and that the acid could not pass through any opening, I was certain, by examining each end of the tube, there being not the least acid. Therefore, it must have united with the fire and passed with it through the body of the glass; the best examination of the acid is to see what proportion of alkali it will saturate before and after the operation.

In Count Rumford's experiments, on the explosion of gunpowder, we see how very

penetrating the nitrous acid is when united to fire, fully explained in my Letter; a circumstance never thought of by our aerial chemists in explaining the phenomena? and, it is clear that the explosions of combustible bodies, as gunpowder, &c. their force depending upon the fire set loose, are by uniting itself with the bodies of the compound, which gives the fire this great mechanical force.— In gunpowder the body which the fire principally unites with, is the nitrous acid: and this theory fully explains the phenomena of bodies exploding. Mr. Kirwan says in his *Essay on phlogiston*, p. 31. “If sulphur be digested in oil of turpentine, and then slowly distilled for ten, or twelve days, it will be converted into vitriolic acid, according to Homberg. *Mem. Par.* 1703. It must be remembered, that if this experiment be not cautiously conducted, it is very dangerous.”

It is most extraordinary that the clearest and strongest facts are passed over; while the credulous world runs to new experiments, made splendid to dazzle weak minds. Now how can the French theory explain this experiment? here are two combustible bodies gently burned, or decomposed of their fixed fire by distillation. Sulphur, they allow, contains no oxygen gas, nor does the oil of turpentine: from whence then came the combustion and the vitriolic acid. The oils contain, according to Mr. Lavoisier,

12 oz. 5 gros. 5 grs. of charcoal, and 3 oz. 2 gros. 67 grs. of hydrogen. Here evidently those two combustible bodies, from two circumstances, were ignited. First, the sulphur from the vitriolic acid it contains, has a strong attraction for the high concentrated fire of the essential oils; the attraction, or union of which, sets part of their fire loose; this appears clear from one of the mineral acids, viz. the nitrous, when its attraction is not blunted by being united with fire (as the vitriolic acid is in the state of sulphur) will, when united with the essential oils, so forcibly attract them, that, in their chemical union, they will produce combustion; that is, they will generate so great a temperature of heat and produce that repulsion, as to repel the essential oil's fixed fire from its chemical combination, so as to be formed into free fire. Secondly, the sulphur and essential oils from the heat which they receive in their distillation, it will aid the combustion.—Therefore these two causes will sufficiently account for their combustion, and the vitriolic acid being in the residuum.

Now, to attend to the explanation according to their theory; from whence comes all oxygen gas, not only to form the vitriolic acid, but to attract all the charcoal and inflammable air of the essential oils: and what comes of all the fixed air which ought to have been generated in the process. Even
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to suppose that the oil might contain water, (but that it does not even agreeably to Mr. Lavoisier's theory) and more particularly from the most concentrated oils, being the only oils which will answer for the experiment: but to allow that it does, what came of all the inflammable air the water is supposed to contain, as it ought to have been left in the residuum? But, more striking than all this! from whence came all the fire set loose during the distillation?

Can any but the most *blind, absurd, and prejudiced* chemists reconcile this simple experiment to the French theory? I here, as I have done before, call upon them to explain this simple experiment. I call upon them in the name of *candour, honor, and justice*, fairly to investigate and answer this little tract, or otherwise to acknowledge their errors. If the public do not insist upon it they, neither do justice to science, themselves, or me. This publication, I flatter myself, will fully elucidate Count Rumford's enquiries into "heat and light." See his letter in the Phil. Trans. for the year 1797, p. 215.

By agitating mercury, and tin, or lead, in distilled water (See my Thoughts on Air)—And, also, if there is the least moisture the metals are calcined. If it proceeded from a decomposition of the water, inflammable air ought to have been generated, which is not the case.

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The repulsion of fire is as great and general a law in nature as gravitation. It is to this repulsion of the particles of fire to each other, that bodies are specifically lighter at the equator than at the poles; for, at the surface of the earth it is highly saturated with free fire, and also the bodies which gravitate to it: so as that both their gravitating matters lose part of their influence, being counteracted by the repulsive fire. Bodies which contain a quantity of fire in a loose state, as water, become heavier after parting with this fire, as in the state of ice: for, while it contained this fire it repelled the fire which the earth possess; so that the earth's attraction for the gravitating particles of the water was not so great. Therefore, speaking philosophically, we should say, such a body gravitates to such a degree; its composition being so many points of repulsion. Thus water contains an equal quantity of gravitating matter with ice, but, a greater quantity of fire; therefore it is lighter.

Oil contains a great quantity of repulsive matter, therefore it is light; but the fire it contains is more fixed than it is in water.— Fire when fixed, loses a great deal of its repulsive quality; for, accordingly as it is chemically attracted it loses its repulsive quality. Thus, in sulphur, the vitriolic acid strongly attracts its fire; and though it con-

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tains a great quantity, yet, from its being so strongly combined, it is very heavy. Thus airs contain an immense quantity of fire, or repulsive matter; therefore, after their combustion, their gravitating matter ought to be heavier, as a great quantity of fire escapes; but then the fire escapes with a part of the gravitating matter, as in firing inflammable and pure airs.

Fire repels accordingly as it is concentrated together in its free state. Thus, in combustions, when very violent, or active, what an amazing force it has. Any person who has seen an house on fire must have observed its amazing force and powers. In Papin's digester, when a great quantity of fire is attached to the water, for when let loose what an immense activity and force it has? Thus, in steam, the greater the proportion of fire it contains, the greater its repulsion, or force, as in Mr. Betancour's experiments. But I do not believe to the same degree he speaks of.*

That

* The repulsion appears clear from an investigation of all the phenomena wherein fire is concerned.— Thus, in the electric fire, two electrified pith balls repel each other. But if two balls are electrified negatively, they will equally repel each other. Upon this principle the balls being electrified negatively, or not having their equal saturation of electricity, which, all bodies in nature possess; in consequence the natural electricity of the atmosphere, will be attracted to the vacuum in the balls, or

That experiment of a bar of iron receiving a certain quantity of the sun's rays upon it, heating it to such a point: but, if all those rays be collected together by a convex glass, or mirror, and then the iron receives them, it will be heated to a greater degree. Here the same quantity of rays receive a greater heating power, from having their repelling power increased, by being concentrated.

The theory is this, fire heats from its motion, and that motion is owing to the parti-

to its minus electricity; for this clear and simple reason. As the balls possess less electricity than the air, in consequence, the air next the surface of the balls, its natural electricity will be less repelled, and, in consequence accumulate there, or form an atmosphere of *plus* electricity around the balls; the result of which is, the balls, from having an electrical atmosphere around them, will repel each other. As the electrical fire is universally distributed through all bodies; each particle of the fire repels its neighbour, in consequence, when a body loses its natural saturation, the repulsion from that body will be taken off, and, consequently, the neighbouring fire will accumulate around it to restore the equilibrium.

An electrical conductor, electrified positively, I have found the atmosphere around it in different states of electricity. The first column beyond the conductor's atmosphere, I have found negatively electrified, from the conductor repelling the air's natural saturation of fire. Beyond that again, positive electricity, from the column of air beyond the repulsion of the conductor's electricity; the electrical fire accumulating from the column of air next to the electrical conductor, being electrified negatively, in consequence repelling its neighbouring column less, producing, in consequence, a greater accumulation, or the plus state.

cles of fire repelling each o'her. Therefore, by being concentrated together, that motion is increased, from the repulsion being increased. Thus the sun's rays have more heat, comparatively, than a culinary fire; from this cause, their motion is greater, the fire particles from a culinary fire are not so repelled, in consequence, they do not give the same degree of light: and, moreover, the colour of bodies is owing to the power they have of reflecting, or repelling fire.—When the reflection is strong they are of a white colour, red the next, and violet the least. So bodies burning assume different colours, according to the intensity of fire: and, in burning sulphur, though it consists of an amazing quantity of fire, yet its colour is blue; owing to this, the acid of the sulphur has a strong attraction for the fire, therefore, they partly escape together, and the vitriolic acid retards its repulsive powers, in consequence its rapid motion.—Thus, in bodies burning in the pure air of the laboratory, how much stronger their light and heat are; or in lamps, by placing a glass so as to increase the current of atmospherical air, how it also increases their light and heat.

As we have observed, the colour of bodies is owing to the particles of fire, according as they are repelled: and as colour is a sensation, that sensation acts according to the force with which the retina is struck; if with
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great force bodies appear white, if with little force violet. Just the same of sound; sound is conveyed to the ear by the air, as air contains a great quantity of fire: therefore, it is highly elastic, and, accordingly as this fixed fire is struck, fixed fire is repelled, viz. that next the body which strikes it; but then its fixed fire strikes and repels its neighbour, and so till it reaches the ear or tympanum. It is from this cause that lightning, or gunpowder, which contains a great quantity of fire, therefore repels the fixed fire of the atmosphere, and produces such considerable reports as thunder and the roaring of artillery. Nature having united the fixed fire of the atmosphere, with an acid and water, into an elastic aerial form; therefore, from that elasticity, it is particularly adapted to convey sound. Thus bodies which are the most sonorus are metallic bodies, also containing a quantity of fixed fire; therefore, bodies susceptible of repulsion.— And what shews the wonderful power of the Deity; the animal organs, viz. the eye and ear, are both susceptible of the same distinct sensations, there being a direct coincidence between the prismatic colours and the notes of music; the optic and auditory nerves being equally susceptible of similar sensations. But we may as well say that the fixed fire of the air is sound as that the rays of light, or fire, are coloured. No, they have nothing
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to do with either colour, or sound, they only give an impression to the nerves, which gives that impression to the brain. That nature impresses it with such and such sensations; and these sensations are so distinctly marked, that they do not run into one another indiscriminately, but from such an impulse the fire receives, it will give the sensation of one colour, and such another, &c. And the same of sound, such a percussion of the air shall give such a note, and such another, &c. and these different sensations being so similar. How simple, great, and elegant is this!

There is great difference between light and sound; the former is from the pure fire, or rays of light striking the optic nerve; the latter from fire united to matter, striking the auditory nerve: and that by means of an agent, the drum, or tympanum of the ear. That these sensations are from impressions which the nerves received is certain; for, if by rubbing my eye in the dark with my finger, by giving an impression to the optic nerve, I can give a very vivid sensation of light and colours; the same if I put my finger into my ear and move it quickly; I can give a sensation of sound: therefore, as there could no coloured rays of light come in the dark from my fingers: colours must be owing to the impulse by which the rays of light, or fire strike upon the retina. The way to give these sensations to your eye by
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your finger is, to close the eye and rub the internal canthus with it, being that part of the eye next the nose.

As the picture of the object is painted upon the bottom of the eye, inverted, a great deal of discussion with philosophers has taken place; why, we do not see objects inverted. If the sensation were conveyed to the brain, by this picture, and also the colours, we really would see them inverted. But, the picture, as represented upon the retina, does not impart the sensation as a coloured picture, but only each particle of light coming from the object, gives such and such a stimulus, in consequence, such and such sensations are conveyed to the brain. If it was not so, how comes it that in a delirium, patients see objects with the same distinct vision as when real. Now this can only take place from the disease giving a stimulus to the optic nerve, not the stimulus of a real coloured picture being painted upon the retina. Or upon writing, or reading a great deal (for, greatly I have laboured for the cause of truth and science, yet little has been my reward) I have found little coloured images floating before my eyes; here the senses were clearly not delirious.

That the eye does not distinguish colours, from these colours being painted upon the retina, appears, from many circumstances: I shall mention one. I have long been in
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the habit of using green glasses; upon my first using them every thing appeared very green through them; but, by habit, that greenness went off and I could distinguish colours. Now there could only pass through these glasses, according to the old doctrine, green rays, and that colour must have been painted upon the retina; therefore, this colour ought never to have gone off. But, according to my doctrine; as I suppose, the colours depend upon the force with which bodies reflect the rays of light upon the retina: therefore, from long habit, the retina receiving all its rays through this green glass, it was accustomed to receive the rays of light of a less impulse; in consequence, it adapted itself to distinguish the different impulses of light, under this depressed, or impaired impulse*.

* It is from the repulsion of the particles of light; they pass by each other without impeding each other as other bodies do, as in the light of two candles. See Count Rumford's experiments in the Phil. Trans.

MR. CAVENDISH,

I have made your Letter and this part of the work separate; for this reason.—My former works clearly evincing the truth of my system; therefore, your opposition to it can have no apology; for the truth must have appeared to every impartial reasoner, without the aid of these new experiments, which are beyond all controversy.

A LETTER

A LETTER

TO

HENRY CAVENDISH, ESQ.

SIR,

IN the year 1788, I wrote a letter to you and others upon the extreme errors and fallacy of the French system, and also upon your two experiments, the firing inflammable and pure airs, and passing the electric spark through pure and atmospherical airs; proving, to the satisfaction of candour and common sense, your very erroneous conclusions from these experiments: which letter you have never been able to refute.— I now address to you another upon the same subjects, but I do not expect more candour from you towards this letter than the former; I am sensible there is a most powerful and illiberal combination formed against me and my system. But, Sir, whatsoever be the arts, influence, and conspiracy, time will unravel the whole.

Though this combination is supported by so numerous a body; a phalanx who flatter themselves their names can command every thing, yet truth will and must prevail. If your opinions, experiments, and conclusions are just, then I am willing to stand condemned as censuring you unjustly; and in that censure, as being too confident of my own opinion. But I am not like you and your confederates, who skulk from investigation. I do here seriously call upon the public to arraign us both

at their tribunal, and to pass their sentence according to their justice. But, in fixing their judgment, I hope, they will carefully weigh the facts *pro* and *con*.

No doubt, Sir, you will call this letter presumptuous; and if I do not prove the French theory to be *egregiously* false, and also, if called upon, shew that every mean, illiberal, and shameful artifice has been made use of to repress fair investigation, I will agree with you that it is presumptuous. When a man believes he has truth and justice on his side, when his opponents dare not openly refute, but take every method that cunning and art can invent to suppress fair investigation, after they have been publicly called upon; then under those circumstances, Mr. Cavendish, I think, that I neither do justice to science, truth, nor myself, if I do not state my grievance to the world. I acknowledge that my language is harsh and pointed; but Sir, I appeal to your behaviour for its justification: I am aware of the great influence of this overbearing combination, I know that it is great and mighty, and, like many tyrants, has its janisaries (the herd of reviewers) to strangle its adversaries.

As to these gentry, I think they must feel themselves really disarmed after my last heavy charges, which they were unable to answer, therefore these charges still stand in all their force. But I am afraid, I am paying their feelings too great a compliment, for they are entrenched in brass and ignorance, and deride the shafts of honor, justice, and truth: they now do not comment upon or analyze my publications, but condemn *en masse*, as they found they were failing in unknown seas beyond their weak powers to fathom. But, Mr. Cavendish, I must apologize for ranking you in such company, and also to myself in con-
descending.

descending to answer their illiberality. I know their judgment does not weigh an atom with the man of science; but their writings have this bad effect, they can pass that character upon a book as prevents its being read, particularly abroad; and when these opinions are echoed by such men as you, Sir. I see Sir Charles Blagden has resigned his secretaryship to the royal society, and Mr. Tennant is elected into the council.

What must we judge from this? Is the former glad to be quit of his office, and the latter brought there to second you in your schemes, of rejecting or admitting chemical papers, according as they refute or defend your supposed "*illustrious*" experiments? Sir, this is an age of trifling experimenters, men that have no genius, who expect immortality from their mechanical labours in their laboratory, though merely the result of chance. And when they come to philosophise upon those accidental experiments, they cannot carry their ideas beyond their laboratory; in consequence, when their gigantic systems are applied to the various phenomena of nature, they form inconsistencies and absurdities beyond the imagination to conceive, being the grossest puerilities: these facts I publicly and openly declare to you and the public, and pledge myself for the truth, as being clearly demonstrated in this letter, besides my other publications.

This combination is not confined to England, it also extends to France. Dr. Priestley has wrote two papers lately, clearly refuting their hypothesis, and directly calling upon them for an answer.—Certainly he stands as high as Mr. Fourcroy himself in the public estimation: but behold they are *mute*, they dare not enter upon investigation, though they have resumed their chemical pursuits. What
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must the candid world think? they principally rest their absurd hypothesis upon inflammable and pure airs forming water. Dr. Priestley, and many others, have performed that experiment in a far more accurate and just way, and an acid is in the residuum along with water; this the French theorists knew; but to counteract this experiment, Messrs. Fourcroy, Vanquelin, and Sequin, performed it upon a large scale, and therefore not an accurate one: where they say no acid was produced but an air generated, formed of the condensed acid and inflammable air, as Dr. Priestley sufficiently proved after me. But why was not Mr. Lavoisier and the other leading members of the French school present at this supposed decisive experiment; an experiment which they dare not repeat, nor will they admit of any contradiction. Their behaviour to M. La-Marck was also illiberal in the extreme, and unprecedented as gentlemen and philosophers; though I have received worse treatment. But can you flatter yourself that this behaviour will reign for any time? no Sir, "*For truth is mighty and must always in the end prevail.*"

You, Sir, have, I believe, been lately much in your laboratory in hoping to find experiments to defend this extraordinary system. But I suppose you cannot find any, or otherwise you would not patronise such trifling chemical papers, as the Phil. Trans. have lately produced: for a long time they were *mute* upon chemical subjects, and, I think it would have been more political to have continued so. These chemical papers, I have fully shewn in this letter, to be futile and puerile.

Our aerial philosophers seem to have got into the greatest errors concerning the doctrine of combustion, supposing it is conducted by attraction: but combustion is clearly the separating or breaking down the formation

tion of bodies, and not attracting or building up new ones. Thus fire enters into the integral substance of all combustible bodies, which bodies are those which possess the greatest quantity of fire; therefore when those bodies are destroyed by fire, or have their fixed fire set loose as actual, the composition of them is entirely broken down; from two causes. First, As the fire made an integral part of the bodies, and secondly, as the free fire's great principle is repulsion: therefore, as the fire is let loose, all the component parts of the burning body are repelled or forced from their chemical combinations by the repulsatory principle of fire; and unless chemists will introduce this great repulsatory principle of free fire into combustion, they never can account for the phenomena; for this great repulsion of fire is as certain an agent as chemical attraction.— Therefore our late chemical theories teaching that the air acts in combustion by attracting the supposed elements, carbone, inflammable air, or phlogiston, from bodies when burning, is erroneous, as the air acts in combustion as the agent: it being a combustible body formed of fire, fixed air, and water, and its fire being slightly attracted to the fixed air and water, is therefore easily set loose in the combustion, and then acts upon burning bodies as nature's great agent in setting loose the combustible bodies' fire; and in respiration, putrefaction, and other processes, this fixed fire is easily attracted from its union with the fixed air and water of the pure air uniting to the blood in its fixed state.

But Sir, had that able electrical experimenter Mr. Cuthbertson's experiments been given to the public in the transactions, it would have been a paper that would have done honor to the Society. But no, they were experiments which cut up your boasted theory by the roots. Mr. Cuthbertson has calcined different metals, by passing the electric spark through them both, *in vacuo*, in fixed air, and azote.

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From whence comes the calcination, Mr. Cavendish? here is no oxygen-gas in the process.— Sir, The explanation is obvious and clear, the electric fire was so intense as to set the fixed fire free, which gave the metal its ductility.* Will it not also set the fixed fire free which neutralizes the fixed air and water into atmospherical air, leaving the fixed air and water in the residuum?— This, Sir, both Dr. Priestley and Mr. Bergman have proved, and the true explanation of your experiments, with the electric fire, is, that when the pure air of atmospherical air is united with the pure air of the laboratory, both their acids will be actually condensed in the experiments; this I have proved.

Why does Mr. Keir delay the publication of his experiments on metallic solutions? Those who have seen the second part, the first being published in the Phil. Transactions, say, that he has detected the grossest errors in Mr. Lavoisier's statements: the experiments of men anxious to establish a theory, should always be received with great caution, and repeated. That those statements were grossly erroneous I have proved in my different publications. See my Letter, and Chemical Essays. Then

* The doctrine of the repulsion of the particles of fire to each other being now clearly established; and the electrical fire passing through the metals being so very intense, see Dr. Van Marum's experiments, where he melted a considerable body of iron at one discharge; he also entirely calcined twenty-four inches of leaden wire, three eighths of an inch in diameter, by one explosion, therefore this immense heat of the electrical fire, will act strongly in repelling the fixed fire of the metals, and set it free.

In my Letter, page 41.

† We cannot help making this remark here, that it is astonishing, that the most eminent chemists who have adopted the singular

Then I must again repeat, why, Mr. Cavendish, does not Mr. Keir produce his experiments which shew the *most gross and palpable errors* in Mr. Lavoisier's? No, that would endanger their grand system: but will the world be deluded in this way! If these

‘gular theory of the composition of the acids, should not have more minutely enquired into it, before they gave us such different opinions upon the same experiments. Mr. Methetic supposes, no nitrous acid enters into nitrous air, because, he says, “though nitrous air is obtained from a solution of mercury in nitrous acid, almost all the acid is found in the solution.”

‘Upon the other hand, Mr Kirwan says, p. 38, “when a metal, or any phlogisticated substance is dissolved in the nitrous acid, this substance attracts the acidifying principle of the nitrous acid, and its phlogiston is attracted by the nitrous basis; and thus by a double affinity, the nitrous acid is in part decomposed, and nitrous air formed.”

‘This supposed acidifying principle, according to him, is fixed air; according to Mr. Lavoisier, is dephlogisticated air; but according to Mr. Methetic's experiment, it is almost the whole of the nitrous acid employed in the solution. But I say, according to truth, it is all the nitrous acid, but what was taken up by the phlogiston in forming nitrous air.’

‘Mr. Lavoisier, who first formed this theory, from observing the effects produced in a solution of the nitrous acid and mercury, says, “100 grains of dry nitrous acid, consists of 64 grs. of dry nitrous air, united to 36 grains of pure air, deprived of its specific fire.” Mr. Kirwan, seeing in some measure the insufficiency of this composition, says, “In my opinion, 100 grains of pure, dry, colourless, nitrous acid, contain 38, 17 grains of fixed air as its acidifying principle, 57,06 of nitrous basis, and 4,77 of phlogiston united to the nitrous basis.”— Both of them make the nitrous air above half the weight of the basis; therefore, upon examining the acid after the solution, we should have expected to have found, according to them, the other part of the constituent bodies; agreeable to Mr. Lavoisier, 36 of the pure air; and agreeable to Mr. Kirwan, 38,17 grains of fixed air. But unfortunately for these opinions, we find almost the whole of the pure nitrous acid. We certainly should have expected, in adopting such an extraordinary hypothesis,

these experiments had been in support of it, the public would have had them many years ago.— But Sir, You *see and know*, that this boasted system is absurd in the extreme, and that mine stands upon the solid basis of immutable truth.

I could mention many other extraordinary similar facts, but I shall tire my reader; I therefore will enter upon my strictures on the different chemical papers in the Phil. Transactions.

‘ hypothesis, so contrary to what chemistry had ever taught or suggested, that they should have examined it more carefully. I leave my learned reader to make his own reflections, they appear so obvious; I shall make no other comments, the errors of the theory must appear in the most striking light.

In my Chemical Essays page 72.

‘ Dr. Priestley says, vol III. p. 299, “ Mr. Metheric found p. 146, though nitrous air is obtained from a solution of mercury in nitrous acid, *almost all the acid* is found in the solution.”— And Dr. Fordyce found *almost all the acid* in the solution of zinc in the vitriolic acid. Then can we have a doubt but both solutions are from the same cause; and not suppose the one from a decomposition of the water, and the other from a decomposition of the acid. The nitrous acid has a strong attraction for the metals, the same as it has for calcareous earths; and if I add as much of the acid as to neutralize the earth and metals, and no more; and, if upon examination of them after the saturation, I find all the acid, only allowing a little for what the nitrous air took up from the metal; can chemists form the most vague conjecture that all the acid is decomposed in the metallic saturation, as all the acid is found entire, and the same in both the saturations? But still more forcibly to contradict so very *absurd* an opinion; by passing the electric spark through nitrous air, Dr. Van Marum reduced three-fourths of it into the nitrous acid.—(See a full explanation of this in my Letter, p. 32.) What led to this *extraordinary hypothesis*, was the experiments of Mr. Lavoisier upon mercury, and which I have shewn can likewise *only* be accounted for by my hypothesis.— See page 56 of these Essays.—These are a small part of my observations upon the subject. See particularly my letter.

It requires no great observation to see, that all the exertions and objects of our present chemical philosophers are, to defend the absurdities of the French theory; seeing I have opposed it by such strong arguments, proving incontestibly its very weak, unstable, principles: therefore there appears clearly a general combination to support its tottering basis; honor, candour, and justice are all to be sacrificed in the contest. That these observations are just, will clearly appear from the following strictures.

Observations on DR. PEARSON'S Experiments.

WITH SOME OTHER REMARKS.

Dr. Pearson has written a most extraordinary paper, in the Phil. Transactions, endeavouring to shew that water is decomposed by passing the electric fire through it. I must make this observation, that such futile experiments and reasoning do not deserve any answer; however, I shall not honor them with a full investigation, but only make some cursory remarks.

In the first place I must make this general observation, that Dr Pearson's experiments and manner of reasoning are always loose and unsatisfactory. Upon the Dutch experiments being published, some of the ablest electricians and chemists endeavoured to repeat them, but when performed in an accurate manner, they found that they could produce neither inflammable nor pure airs: Dr. Pearson thinks himself more lucky.— But must not my reader be astonished when he finds these experiments were performed with a brass conductor, of a large surface, and that there was a sensible erosion or calcination of the metal, so
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that the instrument would only do for a short time; and moreover, that the water through which the electric fire passed, was exposed to the open air.

In page 146 he says, "By repeated discharges there is an impression made in the brass tube, in the part where the discharge passes through it, and at last a small hole is made in that part. On this account the same mounted tube cannot serve for producing a large quantity of gaz."

Now, are Dr. Pearson and his friend Mr. Cavendish ignorant, that water being so intensely heated, as it must be in this experiment, would calcine metals and produce inflammable air; and also that when water is heated, it will expel the pure air it contains, and also that water which has got its air expelled, will attract pure air from the atmosphere?* The water became opaque in the experiment, clearly shewing, (see my experiments) that as it parted with its air, that the bodies which it held in solution were precipitated. The Dr. says, he did not perform the experiments with water which had got all its air expelled by former experiments, leaving that to others.

But I shall ask, *even his candour or yours, Mr. Cavendish*, if that would not have been the only just way of performing the experiment, as being water which had given out all its pure air? for performing it with water which had not given out all its air by the experiment, only proves that

* Dr. Pearson says, there was "Always left a residue of at least one fourth of its bulk on passing through it the electric spark, even when water was used, which had been free from air by boiling or the air pump." Then can there be a question, Mr. Cavendish, from whence the pure air proceeded, as it contained so great a proportion of azote? if it had been from the decomposition of water it would have contained no azote according to your own hypothesis.

the electrical fire acts as common fire in expelling the air from water, the same as steam of water in calcining metals, and forming, by that means, inflammable air. But we wanted not a paper of the Phil. Trans. to prove to us that the electrical fire acts as common fire, for all the world knew before that it would melt and calcine metals, Dr. Van Marum melting a considerable body of iron at one discharge of the battery.

And what a quantity of electrical fire was required to produce a small quantity of gas! for the Dr. says, " My calculation also, that 35 to 40000 " discharges were requisite to produce one cubical " inch of gaz from water, containing its usual " quantity of common air."

Dr. Pearson's paper then is a most puerile performance. But I shall give him acknowledgment for this sagacity, that he knew the only proper way would have been to have proved that the same water would have done for the experiment, again and again, and that he would accordingly try this part of it with the greatest attention; as the supposed decomposition of water greatly rested upon it. But he is obliged to say, " It may now be " expected, that I should have made the experi- " ments with this apparatus on distilled water " freed from its air, not only by long boiling, or " the air pump, but by passing through it several " hundred electrical discharges. It would also " have been, to some persons, more satisfactory, if " the experiments had been made upon a larger " scale, so as to have produced the combustion of " a much larger quantity of gaz, and consequently " have produced a greater quantity of water."

The Dr. has been performing these experiments nearly two years, for he says, " From my journal " of the numerous experiments, made during the " course

“ course of nearly two years, I shall select those
 “ which will serve to explain the nature of the
 “ process, and show the power of the plate elect-
 “ rical machines; and I shall particularly relate
 “ those experiments which afforded the most useful
 “ results concerning the nature of the gaz ob-
 “ tained.”

Therefore we have a right to suppose those experiments were selected to prove this absurd doctrine of water being composed of pure and inflammable airs, for, upon this idea rests the whole fabrick of the French theory. But I must again observe, Mr. Cavendish, was it necessary in this experiment to have one of the metal conductors formed of a metal that is so easily calcineable.— If these airs proceeded from the decomposition of water, why might not two golden wires have done; the one to carry and the other to receive the electric fire; as they perform the circuit as well as any other metals, but he knew they would not.— In short I might make fifty other objections to these experiments; but I have given them more attention already than they deserve. †

Mr. Cavendish, I before addressed a letter to you and others; and I hope, satisfactorily proving your fallacious theories to every *rational* chemist. I then told you that your *supposed* great discovery of nitrous acid being formed of pure air and azote, by the electric spark, was fallacious; that the cause of the nitrous acid being formed in that experiment, was, from the pure air of the atmosphere, and the factitious pure air of the chemists being burnt so intensely as to precipitate both their acids in a condensed state: but, that in common com-

† It certainly must be a dying cause when they can bring only such futile, weak, experiments to defend it.

bustion,

bustion, their acids are left in an aerial state, as fixed air; and that atmospherical air is necessary in the process, as its fire is more easily set loose by the electric spark, fully discovered by the electric spark burning it into fixed air, proved by Priestley, and Bergman. Mr. Kirwan says, "It is impossible to deny all credit to those who asserted that lime water was precipitated by taking the electric spark in common air, though it did not succeed with him (Mr. Cavendish) either from his using an instrument of different power from that used by others, or air phlogisticated by a different process"

§ Now the pure air of atmospheric air's fire being easily set loose, acts upon the artificial pure air of chemists, so that there is such an intense combustion as to precipitate both their acids into a concentrated state, as the nitrous acid; so that azote has nothing to do in the process †

† In my letter, page 126.—'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 very 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 the chemists: the latter not at all supporting vegetable life, and likewise animal life not being able to injure it completely, as animals died in it when it was better by the nitrous test, than the purest atmospherical air; (see my thoughts on 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 decompose respirable airs, and according to the intensity of the fire, the air will be accordingly decomposed. ' If

§ I need not inform my reader that I have proved, by fifty experiments, that pure airs are formed of an acid, fixed fire, and water, so as to form an aerial neutral salt.

I see

I see, upon the continent, a German chemist, M. Gitting, has got into the same mistake in the burning of phosphorus, for he found that atmospherical air was necessary to phosphorus burning in a certain temperature. But, Sir, the true explanation is, from the pure part of the atmospherical air, being necessary in the process, as its fire is more easily set loose, and not from the impure part of it; this I have fully shewn in my publications.

Sir,

‘ 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 decomposed into an acid and water.

‘ Now the electric heat is most intense, it being capable of producing a phenomenon that we cannot imitate by any chemical process. Metals are only melted by heat; the electric 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-eighths of an inch in diameter, by one explosion. This shews a degree of heat we cannot imitate. Our heat that we can apply, requires a long continued application, but this is instantaneous. Therefore when empyreal air is acted upon by this wonderful heat, we should expect it would decompose the air to an acid and water.

‘ This theory is most strikingly shewn, in firing inflammable air 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.

Sir, for the sake of justice, truth, honor, and science; do not, by your great influence, embarrass and overpower candid investigation. I shall ask you and some others a plain and obvious question. How comes it that the oxygen-gas, or pure air made in the laboratory, will not support animal life? Mr. Scheele, Dr. Priestley, and others, in breathing it, could not reduce it so far as to bring it lower than the standard of pure air. Hear, O common sense! the reason assigned for it

by

‘ 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. This he imputed to the impurities of
 ‘ that air: however, there is more of it decomposed 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 all along (in my publications) shewn 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 a laboratory, 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 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 decompose the airs.

‘ This

by our wonderful French chemists, that it produces such an inflammation upon the lungs as to stop re-

‘ This artificial empyreal air, we have likewise all along shewn, 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! (see my thoughts on a. r.)

‘ 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 decomposed, producing the nitrous acid; but the acid is not produced from the decomposition of the phlogificated air. For take only the empyreal part of the atmospherical 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 phlogificated 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 phlogificated 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 decomposing them. Thus in animal respiration, I can decompose 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 fully injured it.

‘ But even inflammable air may be breathed and decomposed; 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 shewn us long ago, that the empyreal part of the atmosphere may be all decomposed by the electric spark.

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

piration;

piration; can these men, with their puerile reasons, pretend to be philosophers! Every physician must have seen the highest inflammation produced upon the lungs in diseases, and yet the patient still breathes and lives. ¶

But this vital pure air, which is so necessary to life, shall, in an excess of it, kill, in a few minutes. Examine a mouse which has died in it, and see its lungs if they are at *all inflamed*. No, they will be found in the *identical same state as if it had been respiring azote*. When any combustible body burns in it, as Dr. Priestley says, "It burns with cracklings as if it was full of some combustible matter."

The common air of the atmosphere, I have proved by incontestible experiments, is formed of fixed air, fire, and water, but the pure air or oxygen-gas of the laboratory, of the mineral acids, water and fixed fire, and, in consequence, it retains its fire so strongly that the blood finds a difficulty of decomposing it; so that an animal will be a longer time in decomposing a given quantity of it than a quantity of pure air of the atmosphere that has identically the same quantity of oxygen-gas in it, nay twice as long as Dr. Priestley acknowledges, and it even leaves it in as pure a state

¶ I attended a patient who had a red hot iron run into his lungs, which produced the highest state of inflammation that could be conceived, and yet the patient survived it. I think I may hazard the idea that it would be a greater one than the breathing of oxygen-gas. Inflammation of the lungs is attended with the greatest pain. Was Mr. Scheele's and Dr. Priestley's lungs, upon breathing this air, in pain? No, upon leaving off breathing this air they were perfectly well. It must be a curious kind of inflammation that kills so *immediately*, and disappears so *immediately*.

as the atmosphere.* Listen again, you *profound* chemists. An animal shall die in it *precisely* at the time it brings its standard to atmospherical air. (see both the experiments of Priestley and Scheele) Would it not, you profound conjurors, at that moment become perfectly salutary? otherwise, when
the

* In my Chemical Essays, p. 39. ‘ As the forming fixed air
‘ into dephlogisticated air, is certainly a process by which we may
‘ accurately judge of the formation of dephlogisticated air, I
‘ have paid a particular attention to it; but I will not give the
‘ minutiae of the experiments, as many have done. If the pub-
‘ lic, after the usage I have received, gets a detail from me, they
‘ may be satisfied. To other philosophers the Philosophical
‘ Transactions are open for their long history of dry experiments:
‘ *but I hope the day of reckoning and retribution will come.* That
‘ fixed air, when soluted in water, will, by the action of the sun,
‘ form pure air, has been proved by Dr. Priestley and others.—
‘ As my theory supposes that fixed air and water, united to the
‘ rays of the sun will form pure or respirable air; therefore to
‘ shew whether my theory, or Mr. Lavoisier’s is just, I made a
‘ number of experiments. I took fresh distilled water, and ex-
‘ posed it to a strong heat, without getting any kind of air from
‘ it; I then added to it a quantity of fixed air, which it readily
‘ absorbed, marking the quantity; after that, I exposed it to the
‘ rays of a hot sun. But as transparent bodies are well known
‘ to admit the rays of light to pass through them, without ar-
‘ resting or stopping their passage, I added an extraneous body;
‘ any will answer, silk thread, or a dead leaf, dried straws, &c.
‘ which have an attraction for the air; and by this means I
‘ found a great quantity of pure air rise to the top of the de-
‘ canter which contained the water; after that I expelled all the
‘ air from the water by heat, and I obtained rather a larger
‘ volume of pure air, than of the fixed air used; and not an
‘ atom of the latter. I found that two things were to be at-
‘ tended to in those bodies, which were added to the water, to
‘ assist the process; viz. they ought not to be transparent, and
‘ ought to have a kind of an electrical repulsion to water, so that
‘ the air may stand upon them in distinct globules: I found glass
‘ bodies, for these reasons, improper, and likewise linen thread
‘ which had a great attraction for the water.

‘ It

the animal was brought into the open air, it would necessarily *die from its inflammation*. How comes it that animals die immediately (the same as in the most noxious air) in Dr. Priestley's dephlogisticated nitrous air, though it will allow a candle to burn in it with such an enlarged flame? The reason is, as I have shewn, its fire is so fixed and concentrated

‘ It is remarked by philosophers, that green animalcules appear when the water gives out air in the greatest abundance.— This appears to be from their arresting the sun’s rays, and attracting the air in the water, the green seeming to be the best colour; and which nature indeed, makes use of in the great vegetable world, in attracting the rays of the sun. When these green animalcules appear in the water, by gently adding fixed air, you may continue on, for a long time, the process of generating pure air.

‘ The globules of air, when they are seen standing upon the silk, &c. appear little at first, but gradually grow bigger; the silk reflecting the rays, and by that means they enter the globe, warming it, and saturating the air, growing larger, and as it were generating or forming pure air, by the fixed air attracting the rays, and saturating itself, by neutralizing them along with the water. We see most of the saline productions of nature are formed of acids, an alkaline salt, (which I suppose fixed fire) and water, and forming regular crystals. In the vegetable kingdom, most of the bodies belonging to it are formed of acids, fire, water, and earth; in bitumens, oils, &c. a more concentrated fire is necessary with acids, fire, water, and earths; and the different concentrations, combinations, and proportions of these bodies make the different bodies upon the earth, without running into the wild speculative opinion, that all bodies are formed of airs. No airs are formed of them.’

From the GENTLEMAN’S MAGAZINE, for December, 1794.

TO SIR JOSEPH BANKS.

SIR,

Carlisle, Oct. 15:

‘ I am much obliged to you for your politeness in saying, “ I shall readily receive any paper you are pleased to send me; and, if the doctrines it contains are not, in my opinion, contradicted

ted that animals cannot decompose it, being formed of the pure nitrous acid and phlogiston; nay, Dr. Priestley formed it of such a high proportion of phlogiston, or fixed fire, that it exploded: I have
formed

“ by the result of experiments already made, or which may be
“ tried after receiving it, for the purpose of bringing them to
“ the test of reason and truth, I will most readily present it to the
“ Royal Society.” ‘ I, therefore, send you the experiments, in
order that you may have them tried, and see whether they are
just or not.

‘ After the numerous experiments which I have made (and published in my different Works) by exposing water impregnated with fixed air to the influence of the sun, and thereby producing *pure* air; and likewise after the experiments of Dr. Priestley, in which water produced air by distillation; a very important question occurs, viz. to what cause are these productions of pure air to be attributed?

‘ To elucidate these phenomena, I made the following experiments. I took snow-water, which had been previously distilled several times, taking care to prevent its contact with the atmosphere, so that it would yield no air, either by the process of exposure to the sun, or by distillation. I then impregnated it with a small quantity of fixed air; after that, I added a little earth which had been precipitated from spring-water by the process of boiling; then corking them carefully in a bottle, and shaking them occasionally till the earth was soluted by the fixed air and water. After that, I put the solution into a still, and let it undergo the same process of distillation which Dr. Priestley speaks of in his last publication on this subject; and I got from it a quantity of air, partly pure and partly azote. And, as the airs were generated, the water deposited the earth which it had held in solution. And by repeated distillations it would yield no more air till fixed air was added to it again, and which soluted again the precipitated earth. And I found that if the solution be exposed to the light of the sun for sometime previous to the distillation, the experiment will be assisted.

‘ I have likewise found that water, which had undergone the action of the sun, and had produced air, as in the experiments of Sir Benjamin Thompson, will do equally as well for the above experiment as snow-water: for, after it has boiled, it will be found to possess no air, its power having been exhausted by

formed it of such a high proportion of phlogiston, that it has allowed the flame of a candle to extend to twice its usual volume, that is, more of the fixed fire or phlogiston of the air was burning at the same time,

Now

the air which it had already generated by the action of the sun. But, if fixed air be added to this water, it will again solute the earth which had been precipitated from the water by the process. And, if this solution be either distilled, or exposed to the sun, pure air and azote will be again generated, the earth precipitated, and the fixed air disappear. †

‘ Now I think, Sir Joseph Banks, these experiments require no comment; it is unnecessary to say from what cause these phenomena proceed. But I shall say nothing upon that head; I only send you the experiments that they may be repeated, and their validity ascertained.

‘ The Dutch chemists have repeated an experiment of mine, in which, from exposing sulphur and iron to heat, I found that inflammable and vitriolic acid airs were generated: their results are similar to mine. But I have carried the experiment farther than they have done. By exposing the sulphur and iron to a greater, more rapid, and more continued heat, I have produced a greater combustion, and formed them into a vitriolated iron. But this process requires attrition as well as great heat; which I contrived to give it by a heated iron or glass pebble moving it rapidly at the time. Now, as the sulphur and iron were pure, and perfectly dried, there being no water, acid, nor pure air, in the process; and as great heat and flame were generated, therefore I think philosophers will agree with me, that the sulphur and iron were decomposed of their fixed fire, of that fixed fire which formed the one into sulphur and the other into a metal, as the residuum was a vitriolated iron.

‘ I should hope that what I have said in my Chemical Essays must sufficiently prove, that in Dr. Fordyce’s late experiments, published in the Philosophical Transactions, the vitriolic acid was

† Will it be believed, Mr. Urban, that this experiment, from which a knowledge of the origin and formation of the atmosphere may be deduced, could not obtain a *reading* before the most learned and respectable Society in the world, which, for a series of years, has made the study of air one of its most peculiar and most interesting objects? And without deigning to assign a reason, though much importuned.

the

Now I think this view of the phenomena ought clearly to explain them. Nitrous air will sooner set fire to phosphorus than the artificial pure air, for

the principal calcining body, aided by the water; and that, when he added the alkaline salt to the solution, it attracted the acid from the calx. But to prove it more clearly—

‘ If a calcareous earth be soluted in the vitriolic acid and water, and precipitated by the same alkaline salt, or *kali purum*, it will be precipitated as lime; and, in both these solutions, the acid is required to be mixed with water. But, if the calcareous earth be precipitated with the mild alkali, it will be thrown down as calcareous earth. And, that the calcareous earth was soluted or acted upon by the acid, is clear from its fixed air being expelled; and it is equally clear that this case is similar when metals are soluted or calcined by acids and water; for, their phlogiston is expelled; the water in one process going to the formation of fixed air, and in the other to that of inflammable. But, if any doubt still remains, that the acid and not the water is the calcining body, let the solution be exposed to a strong heat (which is one of Dr. Priestley’s experiments) the calx will be precipitated; and if carefully examined, being previously well washed in water, in order to wash away any acid that does not make a part of the calx, it will be found to be formed of an acid and the earth of the calx, together with a saturation of water. What must we think of that theory which considers water as the calcining body when the vitriolic and marine acids are used, and the nitrous acid when that is used in the process? But I have found that, if the dephlogisticated marine acid be employed in the process of calcining metals, even though it be mixed with water, marine acid air is produced, and not inflammable air; which is owing to the marine acid having a part of its phlogiston taken from it; therefore, it attacks the phlogiston of the metal with more force or violence, and consequently greater heat is produced, and they form the marine acid air. The nitrous acid does the same, having likewise a strong attraction; for phlogiston (a well-known fact) will attack the phlogiston of the metal with violence and force, producing so great a degree of heat as to form the nitrous air, which is an acid one, the same as the marine acid air; for, they both turn the vegetable juices red, being airs containing more of the acid and less of the phlogiston, which forms the inflammable air principally; though all these airs have water for their basis.

‘ And

for its acid acts in decomposing the fixed fire of the phosphorus; and all airs formed from the nitrous acid do the same. Phosphorus will sooner be set on fire by the weight of the atmosphere being

* And farther, to render this doctrine still more clear. If the nitrous acid be not strong, and if the solution be made in a vessel surrounded by a freezing mixture, the acid being gently added to the metal, they will only produce a phlogisticated air. This is the case when tin is added to a neutral solution of tin in the nitrous acid; it is calcined, a calx is thrown down, and an imperfect nitrous air produced, which is so much phlogisticated as to have its acid neutralized so as not to affect the vegetable juices.

* Or, in the solution of zinc in the nitrous acid, if the acid be gently added in a freezing situation, they will generate an inflammable air that will explode. By producing as little effervescence as possible, and consequently little heat, the acid gets fully saturated with phlogiston.

* The action of the acids upon metals is exactly the same as that of fixed air, or aerial acid on lime, which is an earth saturated with fire; but fire more loosely concentrated than in metals. And it is worthy of remark, that the aerial acid will not expel the fire of the lime without the aid of water, but by their joint influence or attraction for the earth of the lime they will precipitate the fire; exactly as the vitriolic acid and water will expel the fire in acting upon the metals; but the metallic fire is expelled in a fixed state as inflammable air. For a more full elucidation of this doctrine I must refer to my former publications.

* If the action of acids upon phlogiston is managed in a gentle way, without producing great heat and effervescence, they will unite without forming airs. As, for instance,

* If the volatile vitriolic acid be added to iron nails (which is one of Dr. Higgins's experiments, see p. 49 of his last publication) they will generate no air, but the acid and the phlogiston of the iron will form a sulphur; which arises from this, the acid being phlogisticated, its activity for additional phlogiston is partly blunted, and it unites to it in a very gentle manner. But I have found, if this mixture be made in a vessel exposed to great heat, that both inflammable and vitriolic acid airs will be generated. And it is from the same cause that the tin produces so high a phlogisticated air when fresh tin is added to a solution of tin in the nitrous acid; for, the acid leaves the calx to attack
the

being taken from it, as its volatile parts are sooner evaporated. Inflammable air will also assist its burning by dissolving it.

But one of the most striking of the French experiments, is,* that fixed air and water will make it burn better than atmospherical air. I have fully shewn (see my Chemical Essays) that phosphorus has a great attraction for fixed air and water, and that it is this attraction makes it burn in atmospherical air, from its attraction for its fixed air and water, and also from its attraction for acids and water; therefore it will burn in acid air, as the French chemists have shewn. It burned in fixed air and water, so well, that they say, "Which light increased to a greater degree of brilliancy than would have been exhibited in atmospherical air."

But this phenomenon was not owing, as they supposed, to the pure air of the water which the fixed air took along with it in passing through the water; for, if the fixed air pass through mercury the same combustion will take place, provided the phosphorus is exposed to a little water along with fixed air. By their joint influence, they penetrate the phosphorus and set its fire loose; and they will

the phlogiston of the fresh tin; but it leaves the one to attack the other in so gentle a manner, that little heat or effervescence is produced, and the acid gets its full saturation before it is sufficiently aerilized, to produce an air that will admit of a candle burning in it with an enlarged flame. But, if this experiment be made in a vessel surrounded by a freezing mixture, it will get so full a saturation of phlogiston as to form inflammable air which explodes. Surely this is a connected chain of facts which cannot be misunderstood. I am, Sir, your most obedient, humble servant,

‘ ROBERT HARRINGTON.’

* See the present French chemical publication, where there is a number of experiments upon the burning of phosphorus.

both be found to be imbibed by the phosphorus producing the phosphoric acid; and, as a death blow to our French chemists, no charcoal or inflammable air is produced. The true explanation is this, phosphorus is formed from the phosphoric acid and fire, which are very loosely combined, so that they are easily set loose by the joint powers of water and fixed air, which being attracted by the dry concentrated phosphoric acid, in consequence, becomes fluid.

That fire and the phosphoric acid form phosphorus is sufficiently proved by experiments.— See my thoughts on air, where, by exposing the phosphoric acid with a small proportion of water to a long continued heat, I formed the acid into phosphorus; and this shews how all the pure airs of the laboratory are formed; only the nitrous acid when united to the calx of lead, attracts a far less concentration, forming what they call oxygen-gas; which, “Burns as if it was full of combustible matter.” But the phosphoric acid, with the calx of lead, form inflammable air.

But the pure air of the atmosphere does not burn with these cracklings. To make the experiments, take the same proportion of pure air made from nitre, and mix it with an equal proportion of azote, as they say atmospherical air is formed of, and this air will burn with those cracklings which are partial explosions; and also, if breathed by an animal it will not be able to leave it in an impure state; nay vegetables immediately die in it. But in the pure air of the atmosphere, none of these phenomena take place. It is hard, after what I have said, still to have these *absurd doctrines* to expose. As our present aerial philosophers catch at any thing ever so insignificant: they may say, that, in the respiration of pure air from nitre, why an
R
animal

animal cannot perfectly injure it, is owing to the fixed air produced; but Dr. Priestley found, that, when a mouse breathed it over lime water, it made no difference.*

Observations

* In Natural Philosophy, vol. 5.—“ 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 $5\frac{1}{2}$ 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 $6\frac{1}{2}$, 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.

“ It appears from this experiment, that the air confined by lime water was both diminished and phlogisticated exactly like that which had been confined by common water, by the respiration of mice of equal size, in the same time. The diminution indeed was, at first, a small matter greater in the air confined by the lime water; because the common water did not imbibe the fixed air so readily; but this made no apparent difference with respect to the mice, and the next day the two portions of air were found to be as nearly as possible of the same dimensions and of the same degree of purity.

“ 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

Observations on Dr. W. Henry's Paper.

Now, Mr. Cavendish, as you command the key to the chemical department of the Phil. Trans. let us consider the next paper that you have patronized; it equally bears the same marks of puerility as Dr. Pearson's. Indeed after what has been said, and at this advanced period of the investigation of airs, it appears most extraordinary. But Sir, I will pay your sagacity that compliment that you must see the extreme futility and absurdity of these papers, yet they answer the purpose of that policy you have adopted in your combination against my plain and rational theory. But to the examination. Dr. Austin, who certainly was one of the first experimenters, has clearly proved from his experiments, that, in passing the electric spark through the heavy inflammable airs, they were expanded to twice their bulk, and from thence he concluded that, the charcoal which formed a part of these airs was decomposed into the light inflammable air and azote; for, in firing these heavy inflammable airs with pure air, before the passing the electric spark through them, there was a considerably greater quantity of fixed air in the residuum, than after the electric spark had passed through them; therefore he formed this opinion of charcoal, being formed of the light inflammable air and azote: indeed, under the errors of the French theory he could form no other conclusion. But hear what Dr. William Henry says in the Phil. Trans.

Dr. Henry says that, Dr. Austin had not examined the residuums with sufficient attention after firing this heavy electrified inflammable air

“ it lived in it till it was thoroughly phlogisticated. This may
 “ deserve a farther investigation. I should have put other mice
 “ into what remained of the dephlogisticated air.”

with pure air :—“ For in one case the remaining
 “ gas was inflammable, and in another that it sup-
 “ ported combustion *like vital air.*” Certainly
 then this last residuum had all its inflammable and
 charcoal consumed in the combustion; therefore
 must have been fired with its full proportion of pure
 air, and, in consequence, if his theory had been
 true, would have formed the same quantity of fixed
 air. But our aerial chemists have formed the most
 erroneous ideas of all their experiments. Dr.
 Priestley only observed this fact very lately, though
 if he had attended to my publications he might
 have observed it long ago. He says, in his experi-
 ments on the generation of air from water,

“ The reason why, in my former experiments, I
 “ always procured more or less acid, must have been
 “ that, without any intention, or suspecting that
 “ any thing depended upon it, I must have had
 “ some surplus of dephlogisticated air. M. LAVOIS-
 “ SIER I also perceive to have taken it for granted,
 “ as I did; that after either of our processes, any
 “ surplus of either of the two kinds of air would
 “ only have remained unsaturated, and have been
 “ found in the residuum.

Therefore by Dr. Henry's employing a greater
 proportion of pure air than Dr. Austin did, no
 doubt he might produce more fixed air than the
 Dr. as the greater quantity of fire set loose by the
 heavy inflammable air being electrified, would, in
 consequence, set loose a greater quantity of the fire
 which neutralized the fixed air of the pure air; but
 as there was a greater proportion than the inflamma-
 ble air required for its combustion; its combustion
 would not be so complete as to form the whole of
 its pure air's acid into its condensed state, but leave
 part of it in the state of fixed air.

And

And when Dr. Priestley and I used too little pure air, we found that the condensed acid united to the unburnt inflammable air, and formed with it azote; but even in firing the light inflammable air from metals with too great a proportion of pure air, they sometimes have found a proportion of fixed air in the residuum.

But this heavy inflammable air though electrified, and in consequence possessing a greater proportion of fire, is, nevertheless, far from the high state of concentrated fire that light inflammable air possesses, therefore, will still leave a part of the pure air's acid in its aerial state as fixed air.

But let Dr. Henry examine the residuum in Dr. Austin's experiments, and he will not find any carbone in them, and probably some condensed acid; and, in consequence, less fixed air.

Indeed it clearly appears that our aerial chemists will always be in the dark in all their experiments and explanations till they adopt my theory, and then nothing can be more easy and rational.

Dr. Austin's supposition that *charcoal, that wonderful French element*, is a compound body formed of light inflammable air and azote, throws such a ridicule upon their theory, that, I wonder they had not directed their shafts at it before.

Now, Dr. Henry, from what I have said, the reader must be master of your explanations, Dr. Austin's and mine: and let us see which is the true one. We all agree that, there is a greater quantity of light inflammable air in the heavy inflammable air, after its being electrified: then, what produces it? is the question. I suppose that the heavy inflammable airs are formed of concentrated fire; but, of a less proportion of fire than the light inflammable airs, and by the electric fire they receive a greater proportion. For, do not they reduce
metals

metals in a greater proportion, giving them a greater quantity of its fire? For, as Dr. Austin says,— P. 402. “ When the electrified air was fired with
 “ oxygenous gas, it was found that more oxygen
 “ was required for its saturation than before the
 “ action of the electric fluid; which proves that,
 “ by this process, an actual addition was made of
 “ combustibile matter.”

† I will pay Dr. Henry the compliment of having clearly ascertained that water is necessary to the formation of this light inflammable air in this experiment, p. 408 “ If the dilatation of the carbonated
 “ hydrogenous gas arose from the decomposition
 “ of water, the effect should cease when this fluid
 “ is previously abstracted. To ascertain whether
 “ this consequence would really follow, I exposed
 “ a portion of the gas, for several days before
 “ electrization, to dry caustic alkali. On attempting its expansion, I found that it could not
 “ be carried beyond one-sixth the original bulk of
 “ the gas. By 160 very strong explosions it attained this small degree of dilatation, but 80
 “ more produced not the least effect; though the
 “ former number would have been amply sufficient to have dilated the gas, in its ordinary
 “ state, to more than twice its original volume.—
 “ A drop or two of water being admitted to this
 “ portion of gas, the expansion went on as usual;
 “ and I may here observe, that when a little water
 “ gained admission into the tube along with the
 “ gas, in any experiment, which often happened

† What can be more rational than this idea, that a heavy inflammable air may be expanded by imbibing fire? Do not we see it in all bodies? If water is formed into a heavy vapour, by expanding, it will become a lighter body; only in the former case the heat becomes an integral part of the body.

“ before

“ before I had acquired sufficient expertness in
 “ transferring the air from water to mercury, the
 “ dilatation went on with remarkable rapidity”

But then if this light inflammable air was owing to the water being decomposed, certainly we should have either found the pure air entire, or have found with the supposed charcoal of the inflammable air, fixed air, after the electrization. But let us quote what Mr. Henry says upon this subject: “ The change in the lime water was very
 “ trifling; but my friend Mr. Rupp, who witnessed this as well as several of the other experiments, and who is much conversant in the observation of chemical facts, was satisfied that, after a while, he saw small flocculi of a precipitate on the surface of the mercury.”

And will not lime water absorb inflammable air? Mr. Cavendish, I here seriously call upon you, Sir; can you suppose there was any fixed air formed in this experiment. Dr. Henry says, “ But Dr. Austin did not observe that any precipitation was occasioned in lime water by agitating it with the electrified gas.”

And Dr. Henry says, in his experiments, it was very trifling; but his friend Mr. Rupp thought he observed some small flocculi of a precipitate on the surface of the mercury. Good heavens! are such vague opinions to be brought in support of an hypothesis! Dr. Henry does not hazard his opinion, but he brings his friend Mr. Rupp. The precipitation of lime water is a clear distinct process, so obvious to every one, that it is impossible to mistake; particularly so large a quantity of fixed air as must be generated in this process, according to their theory. As the light inflammable air generated was so considerable; therefore the fixed air, according to this theory, must have also been considerable.—

Inflammable

Inflammable bodies will precipitate lime from lime water, as alcohol; but this precipitation is as lime, and not as calcareous earth, this might account for Mr. Rupp's *supposed* flocculi; or also the acid generated in the process, might unite with the calcareous earth, and account for the phenomena. But since our *aerial philosophers* are so ardently and enthusiastically led away by the French *mania*, let us see if inflammable air is not sometimes generated where there is no charcoal. M. Monge found that the electric spark would turn fixed air into inflammable air; and this, according to their theory, he supposes from a decomposition of water: but, as there was no pure air generated the other supposed constituent of water, he supposes it must have combined with the mercury.— But Mr. Monge unfortunately had not such a friend as Mr. Rupp, with his keen vision to discover that the mercury was calcined in the operation, which we know it must have been if the mercury had imbibed it. Yet, true it is, the same phenomena will take place if the fixed air is electrified over water. §

Now, Sir, attend to my explanation: this electric fire will give the calces of metals such a quantity of fire as to reduce them; if passed through the phosphoric acid, will, from the same cause, form it into inflammable air, (and also common fire will do the same with the phosphoric and the calx of lead) and, if passed through the

§ Besides as the electric fire only passed from one conductor to the other: and only passing through the fixed air would not act upon the mercury: also how could they suppose if pure air had been set loose in the process it could act on the mercury? But all these reflections are foreign to our *aerial philosophers*; their reasonings, according to a person in the Gentleman's Magazine, "it must and does."

nitrous

nitrous acid will give it such a quantity of fire as to form it into an air that will allow bodies to burn in it with a most brilliant and enlarged flame.— Also, if passed through the caustic volatile alkali, will give it such a quantity of fire as to form it into inflammable air. Likewise fixed air is formed into inflammable air by fire; for, if pure limestone is exposed to a strong heat, it will discharge inflammable air, owing to calcareous earth having so strong an attraction for its air, that it will not part with it till it has got so strong an impregnation of fire as to form inflammable air.* But if fixed air is united to the earth of mercury, as when a solution of mercury in the acids is precipitated by the fixed alkali, the acid unites to the alkaline salt, and the fixed air to the mercury. || See Dr. Priestley, Vol. 6, Sect. vii. p. 162. [This

* It cannot be from decomposing water, for here is no metal to attract the other component part, pure air.

|| In my Letter, page 86.—‘ The phosphoric acid is obtained from burning phosphorus, and was one leading experiment which made Mr. Lavoisier adopt his theory of the composition of acids, from seeing a quantity of dephlogisticated air imbibed.— But as we observed before, unfortunately for this theory, which of itself directly overturns it, the phosphoric acid when united with the calx of lead, only produces inflammable air, and if the process is pushed by a strong fire, the calx will be reduced.— Here then the phosphoric acid (agreeably to their mode of reasoning) is formed of phlogiston only. What then becomes of the dephlogisticated air which was imbibed in the process of burning the phosphorus? but the vitriolic acid may be made into phosphorus, with the aid of animal bones and inflammable air. In short, in the course of this Letter, it will be found that there is but one general acid in nature, and that they may be formed into one another.

‘ The vegetable acid, which when highly concentrated with phlogiston will form sulphur (see Priestley) and when fermented in vegetables, produces fixed air; with calcareous earth, produces only phlogisticated air, this acid then agreeably to their reasoning, is formed of phlogisticated air.

But

This calx of mercury, by heat, will part with its fixed air as pure air; and that there is a near connection between pure and inflammable air is clear; for Dr. Priestley, in the same process, sometimes formed his nitrous dephlogisticated air (as he calls it) into an air that would admit of bodies burning in it with an enlarged flame, and sometimes so enlarged as to burn all at once, the same as inflammable air.

Now if this air is attended to it will clearly expound the whole mystery; if the acid vapour is slightly mixed with phlogiston, or has got a slight impregnation,

‘ But the most striking, is the acid called fixed 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, dephlogisticated air, and inflammable air: just agreeably to this regular gradation, proving that the fire combines with the acid and earth, first expelling the fixed air that is loosely combined 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 agreeably to their explanation, the fixed air is formed of all these airs. That the calcareous earth is not impregnated with phlogiston, is proved from its forming pure dephlogisticated air with the nitrous acid, and from its being capable of being all formed into phlogisticated air with the vegetable acid: here, agreeably to the new aerial proofs, it cannot certainly be supposed to be all pure phlogiston.— Therefore it surely cannot admit of any other explanation, than the one which my theory gives it.

‘ 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 decomposed by an alkali. If exposed to fire, it will yield empyreal air and no charcoal; but it cannot bear the explanation that Mr. Kirwan gives of it, viz. the fixed air being decomposed, for if the fire is not pushed, you will get the dephlogisticated air from it, and the calx will not be reduced. Nay, to shew that the calx is not phlogisticated after this process, agreeably to Dr. Priestley’s

impregnation, it will only form nitrous air; but if this nitrous air is exposed to iron it will corrode the iron, taking its phlogiston from it, so as to form the air that will allow a candle to burn in it with an enlarged flame.‡ But sometimes, in the same process (see Dr. Priestley) it got such a considerable proportion of phlogiston or fixed fire as to explode all at once. And that these phenomena, according to my theory, are clear; for, attending to the different impregnations, it will burn differently; sometimes the flame of the candle will be smaller, sometimes larger: that is, suppose a candle whose flame in burning in atmospheric air should be the size of half an inch; in the nitrous air (we will suppose in ten inches of this air) its flame will sometimes extend to one inch of the air, sometimes to two inches of the air, and so on, and sometimes it will extend to the whole ten inches; it burning the air all at once.§ And that it is not the same inflammable air which comes from metals, is easily ascertained by putting

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. My theory here appears so rational, that when this dephlogisticated air is burned, it will be decomposed: that is, its fire will be separated from the fixed air, both being produced in the process of burning.'

‡ There appears a great absurdity in their theory. The nitrous air, by corroding iron and forming it into a calx, two thirds of the air is turned into that state as to admit a candle to burn in it as vigorously as the purest air, or what they call oxygen-gas: but it ought to have been reversed; as the iron is calcined all the oxygen-gas ought to have gone to the formation of the calx.

§ If this nitrous air stands long enough over the iron; the calx of iron will attract its phlogiston, forming the volatile alkali, and the air will be left with so small a saturation as to form a kind of azote.

it to the different tests; but it is precisely an air formed of the nitrous acid, with a strong impregnation of phlogiston or fixed fire. See my thoughts on air.

Now, Mr. Cavendish, you must know that in 1781, in my first publication, I clearly proved that water was a necessary ingredient in all airs, forming a kind of base to them, therefore, I will appeal to every candid and unprejudiced man: if, in this experiment of Dr. Henry's, where water was necessary to the forming of the heavy inflammable air into the light inflammable air by electricity (and, as I have shewn, even heat itself will form the heavy inflammable airs into the light;) if it does not clearly prove that the water enters into the new formed light inflammable air, pure, and undecomposed; for if it was decomposed there must have been either pure air or fixed air found in it; and that there was neither is *perfectly clear*, from Dr. Austin's experiments, and *even* Dr. Henry's; and as heat will also form the heavy inflammable airs into light inflammable airs, therefore, they themselves, *if they have any candour*, must admit that heat will not decompose water. §

Truth,

§ In Dr. Bewley's Treatise on air, p. 165.—“ Inflammable
 “ airs burn in dephlogisticated airs with an intenser propor-
 “ tioned to their purity, or to the quantity of their concentra-
 “ ted fire; and they will accordingly decompose dephlogisti-
 “ cated air. If they possess a great quantity of fire, as the in-
 “ flammable air from metals, they will entirely decompose it,
 “ and form the nitrous acid and water; but if they do not pos-
 “ sess 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 inflammable airs, which are formed by pas-
 “ sing through hot earthen tubes, a greater quantity of fire, either
 by

Truth, honor and justice, will call out shame! if this combination is not given up. For, I do not believe there ever was so illiberal and extensive an one formed. As to that part of the paper which treats of the formation of the volatile alkali; I have, in my writings, clearly proved that alkalis are formed of fixed fire, therefore it will clearly elucidate all that part of the phenomena.

I often observe in papers, that they are finished with a short note implying something very decisive in favour of their theory. See Dr. Henry's,†

Now, Dr. Henry, by dissolving phosphorus in any gas, and passing the electric spark through it, I have turned the phosphorus into the phosphoric acid, even in azote gas, also in the pure vapour of water, and no inflammable air was generated:

“ by heat, or the electric spark, and by enlarging the surface of
 “ the tube through which the vapour had to pass; all the in-
 “ flammable airs, by having more concentrated heat thrown into
 “ them, will act accordingly upon the dephlogistated air.—
 “ And also, by a proper adjustment of the proportion of the
 “ inflammable airs to the dephlogistated 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 hypo-
 “ thesis) will act upon the dephlogistated 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 of fire is thrown
 “ into them, they will likewise explode.”

† He says.—“ Since this paper was written I have extended
 “ the inquiry to phosphorated hydrogenous gas, which expands
 “ equally with carbonated hydrogen; loses its property of inflaming
 “ when brought into contact with oxygenous gas; and affords
 “ evident traces of a production of phosphorous or phosphoric
 “ acid.”

It appears very extraordinary that Dr. Henry should accuse Dr. Austin of employing too little dephlogisticated air in his experiments; when, in reality, most of them were performed with a greater proportion of this air, than was sufficient to saturate the inflammable airs: for, a great quantity of pure air was in the residuum. See experiments seventh, eighth, ninth, and particularly the tenth; therefore he very justly concludes with saying, "It is evident that the quantity of dephlogisticated air used in this experiment is more than sufficient to combine with this inflammable air". Indeed nothing can be clearer than his experiments, and they were made upon a more accurate and just plan than Dr. Henry's.— Dr. Henry does not answer the different experiments and arguments of Dr. Austin, but omits the greatest part of them. Dr. Austin says, vol. 80, p. 70. "I have already taken notice of the formation of fixed air from nitrous ammoniac, which is now well known to contain nothing, but the phlogisticated, light inflammable, and dephlogisticated airs. This salt, heated in close vessels, yields dephlogisticated nitrous air in great abundance, mixed with a small proportion of fixed air. I have often repeated this experiment with nitrous ammoniac, which indicated no trace of fixed air either with lime water, or with acids, before its decomposition; but, when the salt was decomposed by heat, I always found lime water rendered turbid by the generated air; and, upon adding an acid to the turbid lime water, have observed air bubbles to be produced in it."

Now, Sir, how can you explain this? which can only be done in this way; the different acids are all formed from one: therefore they may be all formed into one another. But

But to prove, beyond all controversy, my theory. If the *pure volatile alkali* is thrown into a red hot crucible, previously filled with the pure dephlogisticated or atmospherical air, the volatile alkali will burn; but, instead of turning the dephlogisticated air into water, it turns it into *fixed air*. This experiment is best performed with a crucible in the form of a bottle, with a bladder fixed to its mouth containing the volatile alkali; and when the crucible gets red hot at the bottom, the alkali is then to be put into the crucible; or it may be performed without the bladder.

Now, Mr. Cavendish, I appeal to common sense, that the solution of these phenomena can only be from my theory; and bodies burning in dephlogisticated air, change it either into a condensed acid and water, or the aerial acid and water, according to the degree of its combustion. If very intense, it takes from the pure air totally its aerial form, and when not so intense, leaves it in its aerial form as fixed air: in proof of which I have all along brought the most conclusive experiments and arguments, in my publications.— I shall here name only one, as I am tired with going over so repeatedly the same ground. If the red precipitate, which yields only the purest oxygen-gas, be heated with the filings of iron, which yields the purest inflammable air; the purest fixed air is produced, and in the greatest quantity. The cause of which is this: these airs are generated very slowly, therefore the combustion of the airs takes place in a gentle manner, and in consequence, the heat is not sufficient to make that active combustion so as to condense the dephlogisticated air's acid, but leaves it in its aerial form. O! when will men's candour open their eyes?

And

And in these experiments of Dr. Austin, no light inflammable air (the same as the inflammable air from iron) was generated; but only the heavy inflammable air got such a great addition of fire, by the electric spark, so as to expand it to twice its bulk, and in consequence of its containing more fire or combustible matter, it required more pure air for its combustion.

I must again repeat Dr. Henry's mentioning, in that vague way, the lime water being affected; for, in this very experiment, he supposes 44 measures of fixed air being absorbed by the lime water. Would it not then have *shewn* evident and *clear* signs of precipitation, without the aid of Mr. Rupp? But this absorption was the inflammable air.

*Observations on DR. WELLS'S Paper, with
other Remarks.*

Now, Sir, let us consider another curious paper upon the colour of the blood and see if it will shew any more sagacity, Dr. Wells's observations and experiments betray the same puerile defence of the French system. He begins with saying; "Dr. Priestley is, I believe, the only person who has hitherto attempted to shew by what means common air brightens the colour of the blood which has been for some time exposed to it."

What must I say to such declarations? if Dr. Wells had read the Gentleman's Magazine, or my other publications, he might have saved himself the trouble of this performance; he then would see the futility of his arguments. For, I have, I hope, satisfactorily proved to every man who is open to candour, that the blood receives the fixed fire of the air, and from thence it receives

ceives its red colour : he will then see the subject fully investigated, and, I flatter myself, very differently handled from his vague suppositions, even though he is patronized by you, Sir. ||

|| The Dr. may triumphantly ask, can he be supposed to look for Philosophical papers, upon such interesting subjects, in the Gentleman's Magazine, when there is a royal society instituted for such papers? what will those men say to this, who have hindered my papers from being received there? But I must here introduce a character drawn by that able satirist in his Pursuits of Literature,—Part II. p. 19. “I termed the Commentators on Shakespear out of mere pleasantry, “black-letter dogs.” But if among these scholars, or in any other description of the learned, there should be found A MAN, who, with the grace of exterior accomplishment, or the fulsome semblance of it; with the gifts of fortune, and the rank of a gentleman; with a strong devotion to literature without remission and almost without example; with acuteness of mind and extensive classical erudition, who, I say, should so far forget himself as to practise arts which would disgrace the meanest retainer to learning:—If SUCH A MAN should be found, with fair professions, and obliging attentions, similar of friendship, but at the bottom, false, hollow, designing and malicious; who, jealous of every little advancement or lucky discovery, even of a professional artist, should strive to depress the efforts of struggling laborious merit, or to blast the rival ingenuity of his learned contemporaries; who should inflict a wound with more than Parthian dexterity, and yet be studious of frequenting the company of men of character to countenance his own; and finally, who should collect and scatter around him the *virus lunare*, the vaporous drops that hang in any region of infection, that the objects of their influence may feel the blast of the enchanter, and know not whence it comes. If, I say, SUCH A MAN should be found, I shall not name him, and it is not for him to lay bare his own conscience by a foolish, appropriating indiscretion. I have only sketched out at present such a character *in prose*; and all I shall say further is, may He, if SUCH A MAN exist, strive to wipe out such actions by more than literary contrition, and deeply feel and know that he has lived, throughout the course of a life, not inconsiderable in its duration, under a fatal error, and a wretched abuse of time, learning, talents and accomplishments.”

The Dr. must not expect me to follow him closely through his vague suggestions, all of which are foreign to the subject: but I will refute him with his own words. He says, p. 422. " Now this
 " increased reflection of light, in the reddened
 " pieces, could not arise from any change in the
 " reflective power of their surfaces; for bodies
 " reflect light from their surfaces in proportion to
 " their density and inflammability; and neither of
 " these qualities, in the reddened pieces of cras-
 " samentum, can be supposed to have been aug-
 " mented by common air, or a solution of a
 " neutral salt in water."

Now, Sir, in another place, in answer to Dr. Priestley, you say, " It may be urged, that, since
 " the neutral salts, and the different alkalis, when
 " saturated with fixed air, produce the same effect
 " as common air upon the colour of blood, if
 " common air acts by attracting phlogiston, those
 " other bodies must have a similar operation.—
 " But surely it cannot be thought that the mild
 " volatile alkali, which has been supposed by che-
 " mists to superabound with phlogiston, can yet
 " attract it from blood."

Then, Sir, I retort upon you your own arguments; can you say that the volatile alkali is not phlogiston or *inflammable*? no, you acknowledge it, and I believe all chemists are *now* giving up Mr. Lavoisier's *absurd doctrine*, that the fire which comes from the combustion of inflammable bodies *all* comes from the air, *the absurdity of which I have shewn in such strong colours*, that they are forced to relinquish it. See my treatise on air.

I have clearly shewn in my works, that pure air is formed of fixed fire, an acid and water. And also that the common alkaline salts are formed of fixed fire. But, if my experiments do not satisfy you,

you, if you will look at the experiments of Dr. Girtanner* you will there see that the fixed alkalis are so combustibile, or inflammable, that the experimenter supposed (according to the French

* Dr. Duncan, in his chemical news for the year 1796, says, " Dr. GIRTANNER has long employed himself in analysing some bodies hitherto considered as simple, and thinks he has obtained the following results.

" 1. Phosphorus consists of azote and hydrogene, like ammoniac.

" 2. The fixed alkalis consist of carbone and azote, soda containing more azote. potash more carbone.

" 3. Sulphur consists of carbone and hydrogene.

" 4. The fluoric acid has a compound, probably triple base.

" 5. Arsenic seems to consist of carbone, azote and hydrogene.

" The proofs of these assertions are soon to be published at length in Gren's Journal of Natural Philosophy."

You see into what contradictions the experiments of our theorists lead them to: phosphorus is said to consist of hydrogene and azote, then in burning, it imbibes a great quantity of pure air, this pure air then you say forms with azote, the nitrous acid, and with hydrogene water; then in burning phosphorus the nitrous acid ought to be produced and not the phosphoric: and in burning the fixed alkali both fixed air and the nitrous acid ought to be produced: also, in burning sulphur, fixed air and water ought to be produced, and no vitriolic acid; so of the other bodies, for I am tired with the absurdities. Thus the analysis of the same bodies in different ways, what very different results they give. And in burning the pure volatile alkali in pure air; as the alkali according to you, Mr. Cavendish, is formed of inflammable air and azote; the pure air, instead of being burned into fixed air, as I have proved just before, it ought to have been burned to the nitrous acid and water, according to your experiments. But those experiments of yours, of turning pure air and azote to the nitrous acid, by the electric fire, I have shewn to be erroneous, and you have never dared to contradict me.— And how comes the nitrous ammoniac to be aerilized into dephlogisticated and fixed air?— See Dr. Austin, just quoted, p. 38. How can you explain this? In short, the whole French theory is so absurd and ridiculous, it makes one lose all patience in the contradicting of it. O chemistry; chemistry, how long wilt thou be shackled with this French aerial philosophy.

theory) that alkaline salts are formed of pure charcoal.

Now, Sir, you must know this theory supposes that the air, in respiration, takes an immense quantity of charcoal from the blood when it turns it red; then how can you reconcile this wonder of wonders, that charcoal, in the state of alkaline salts, can make it red! And also, as the volatile alkali makes blood red, yet you say that the process of the air is also to take inflammable air from the blood, and which, you acknowledge the volatile alkali is formed of. To what absurdities do our French theories lead us; that by the pure air in respiration taking charcoal and inflammable air in such quantities from the blood, the blood becomes red; and also, by adding these same bodies to black blood it becomes red. But you say that the red colour is owing to the pure air imbibed by the blood: but, Sir, are the fixed and volatile alkali pure air; no, they acknowledge they are the very bodies which the air takes from the blood in this reddening process. But know this, Sir, that these alkaline bodies are fixed fire, and that either fixed or free fire reddens the blood.— And let us now consider what authority the French chemists have for asserting that any pure air is imbibed by the blood; why Mr. Lavoisier says the pure air of the atmosphere disappears, and there is less fixed air expired than in any other process of injuring the air. But why, Sir, ought we not to suppose that part of the fixed air is imbibed by the blood from the motion of the lungs, and the quantity of moisture in the process? If a quantity of fixed air and pure air are equally agitated with the same quantity of moisture and motion as in respiration, you will find the water imbibe the fixed air and not the
pure

pure air. This is an experiment which may be easily tried out of the body; and I also found that the condensed water imbibed the fixed air and not the pure air. But even to allow you this opinion, must you not say, Sir, that when this pure air is imbibed into the blood it must be turned to fixed air and water, as it must there meet with the inflammable air and charcoal of the blood (according to your hypothesis) there not being so much as an intervening membrane to obstruct the union; and if it was not by attraction, how should it penetrate the membrane of the lungs, and what bodies could attract it but the inflammable air and charcoal, according to your system. But, Sir, to the test of experiments, I shall not give you my own, though I have made many; but Mr. Scheele who bears the first of characters, whose experiments are simple and clear; not like the pompous and complex experiments of the French, which are calculated to embarrass and obscure the truth. But before I give you the illustrious Scheele's experiments let me just hint to you a simple truth.

According to your theory the black globules have charcoal and inflammable air taken from them, and attract pure air in their stead, and they become larger and red.

Now, Sir, I should suppose, if our *aerial* philosophers will allow of reason, that the globules should become very light, as they parted with these gross bodies and received pure air; which not only made up the deficiency of the condensed charcoal and inflammable air in point of bulk, but even encreased it, as they imbibed such a quantity of pure air in its aerial state, so as to increase beyond their former bulk, that the globules should become very light, but they are nevertheless specifically

cifically heavier than water, as they sink in it; and, according to your assertion, which is certainly just, being the doctrine of the great Newton, "That bodies reflect light from their surfaces in proportion to their density and inflammability."

Then, must it not be a very singular doctrine? for, the Dr. says, "This increased reflection must, consequently have arisen from some change in their internal parts." And this change to be from those inflammable bodies *charcoal and inflammable air*, being discharged from the body and to have received pure air in their stead; which, according to you, is the ascending principle, and so far from being an inflammable body, attracts inflammable bodies as an acid attracts an alkali: and also from their density; then can you suppose the blood would become more dense from parting with solid bodies, as charcoal? which vegetables, you say, are formed of, and inflammable air; of which, you say, the volatile alkali is principally formed, receiving pure air, in its aerial state, in their room.

You see, Mr. Cavendish, this *French theory* which you are so enamoured of, when investigated, into what absurdities it leads you. But, Sir, I assert that your attachment to it is from being interested; as, *you say*, you are the author of the two great experiments, viz. of producing water by firing pure air and inflammable air; and by *passing the electric spark through pure air and azote, producing the nitrous acid*, which are supposed to be the pillars of the theory; but the fallacy of these experiments I have satisfactorily shewn. But as your friend, Dr. Pearson, from them, gives you the name of illustrious, you cannot bear the idea of their fallacy being detected, in consequence your illiberal opposition to my plain truths.

I must

I must here make this remark, that when men are prejudiced in favour of a particular hypothesis, the most glaring absurdities are adopted, and the most obvious and clear facts are overlooked. Mr. Gough, in the Manchester Memoirs, vol. 4th, shews, by some experiments, that vegetation reduces pure air to fixed air; a fact, which, I fully proved in the year 1785. See my treatise on air.

The illustrious Scheele has given a number of clear and satisfactory experiments, made with such beautiful simplicity that they cannot possibly be misconstrued, very different from the pompous apparatus of Mr. Lavoisier; the result of those experiments clearly establish this important fact, that insects and vegetation turn the whole of the pure air into the aerial acid; but that animals of a higher order turn only a part of the pure air into the aerial acid. He says, in his experiments on air and fire, page 150. "Whereas insects and
 "vegetables so greatly change the air (atmospheric)
 "rical) that one fourth of it becomes aerial acid,
 "I was therefore desirous of knowing whether
 "*empyrean air is not that which is here changed into*
 "*aerial acid, because just the same bulk of air had*
 "been converted into aerial acid, which empyrean
 "air occupied it."

He then gives a number of experiments which clearly establishes the fact. I have made the same beautiful and simple experiments over mercury, and diversifying them, so that there cannot remain the least dispute. And another experiment of this great man's, which, I think, immediately establishes my hypothesis, is, that the cause why less aerial acid appears in the respiration of the higher orders of animals, is owing to the agitation of the air

with

with the moisture in the lungs, absorbing a part of the fixed air.*

He says, p. 152. " It is known that blood newly
 " taken, and exposed to common Air, acquires on
 " its surface a fine florid red; and that the parts
 " below, after coming in contact with Air grow
 " likewise red: does the Air perhaps here undergo
 " an alteration? I filled one third of a matras with
 " blood recently taken from a bullock; and tied
 " it carefully up with a bladder, and shook it fre-
 " quently: eight hours after this, I found in this
 " air neither aërial acid, nor any diminution of it
 " in bulk; but the flame of a candle was imme-
 " diately extinguished in it. This experiment
 " was made during winter; whence it is clear,
 " that the above effects cannot be ascribed to pu-
 " trefaction; since the same blood, six days after,
 " was still found to be fresh, and all putrefactions
 " produce aërial acid.—I was now desirous of
 " knowing how empyreal air, by itself, would act
 " upon animals and vegetables.

" I put two ounces of saltpetre in a small glass
 " retort, on burning coals, and fixed to the orifice
 " a large softened bladder; (No. 35.) then I
 " left the saltpetre so long boiling, that I had
 " obtained in the bladder three-fourths of a quart
 " of empyreal air: I tied up the bladder, re-
 " moved it from the retort, and fixed a tube to its
 " orifice; and having quite emptied my lungs
 " from air, I began to breath the air out of the
 " bladder (No. 84) This succeeded so well, that
 " I was able to take *forty* inhalations before it be-
 " came troublesome to me; and lastly, exhaled all

* That difficulty and laborious breathing of animals, when struggling for life, will even act upon azote. Dr. Priestley's experiments shew, even that air in laborious breathing, is received into the system.

“ the air from my lungs into the bladder. The
 “ air seemed not much to be diminished, and
 “ having filled a glass with it, a burning candle
 “ introduced into it continued to burn.

Now here it clearly appears that the agitation made the blood absorb all the fixed air. But that it was owing to the agitation, I ascertained by taking a quantity of black crassamentum and placing it in a glass, in a quantity of atmospherical air; after they had stood a few hours, upon passing up a quantity of lime water, a fourth part of the air was absorbed. Blood I found in this experiment did not act so rapidly upon pure air (oxygen-gas) as upon atmospherical air, according to the pure air it contains, this clearly accounts for the fact why animals cannot live in it so well as in atmospherical air; also, vegetables do not thrive well in this pure air.

P. 148. “ I shut up some flies in a phial, into
 “ which I had laid a paper whereon I put some
 “ honey: some days after they all were dead; but
 “ they had not absorbed any air: the milk of lime
 “ diminished its bulk of about one-fourth part,
 “ and the rest extinguished a candle.”

Therefore the illustrious philosopher, after making many experiments, says, p. 150. “ These
 “ are remarkable circumstances; that air is not
 “ considerably absorbed by animals endowed with
 “ lungs, contains very little aërial acid, and, how-
 “ ever, extinguishes the flame of fire; whereas
 “ insects and vegetables so greatly change the air,
 “ that one fourth of it becomes aërial acid.”

I could wish to have these experiments of the great Scheele repeated by others, their simplicity and beauty are particularly interesting. I cannot help expressing great dissatisfaction at seeing the experiments now made, are those of the French
 H school,

school; to support it they strain every principle, and we have shewn, in this work, what weight they ought to have; I can only say, *Magna est veritas et prevalebit.* But even the experiments made to establish it just prove the contrary: to exemplify this I will cursorily observe some experiments of Mr. Gough's, upon vegetation, in the Manchester Memoirs, and also Mr. Cruikshanks's, in Nicholson's Journal: these experiments prove the same as Mr. Scheele's; that vegetation turns pure air into fixed air. Mr. Gough says,— Page 491, “It is evident from this experiment, “that the putrefactive fermentation, or an emission of *gas* from their substance, destroys the “vegetative faculty of peas; from which we may “venture to conclude, that it has, sooner or later “the same effect on all other seeds exposed to its “influence.”

But as if this French theory infatuated people's senses; though Mr. Gough's experiments clearly shewed all the pure air was turned to fixed air, and that when the vegetables gave out any gas, they were, instead of undergoing the vegetating process, actually in the putrefactive process, and after that would never vegetate again. Yet, according to his French principles, he is of opinion, that, in vegetation, pure air is imbibed, and fixed air emitted, so as directly to correspond; the same quantity of pure air absorbed, directly is the same quantity emitted; just the same erroneous absurdities that they have adopted in animal respiration. Mr. Cruikshanks says, in Nicholson's Journal, p. 338, “To be more certain of the nature of the “change which the pure air undergoes in this “process, the experiment was repeated as follows: “January 23. A quantity of barley, soaked in “water for two days, was introduced into a jar “containing

" containing 46-measures of very pure oxygen
 " gas, and inverted over mercury. At the end
 " of three days the barley began to grow, and
 " this process continued for ten days, although
 " very slowly: the column of gas remained ex-
 " actly of the same height, so that it had under-
 " gone no apparent diminution or increase:—
 " the barley being withdrawn, the air in the jar
 " was examined, and found to consist of carbonic
 " acid gas, mixed with only 1-50th of its bulk of
 " oxygen gas. The barley was partly converted
 " into malt, the quantity of oxygen being insuf-
 " ficient to produce this change upon the whole."

Now, can experiments be more plain, simple, and just! it is impossible to mistake them.

And can it be supposed but that an insect acts upon pure air the same as other animals, receiving fixed fire which supports its animal heat?— Then it clearly receives no pure air, as it is all turned to fixed air. But can any one deny that insects have all the principles of animal life, the same as any other animals; nay insects have comparatively more energy or principles of life than any other animals; attend to their amazing activity and quickness of motion; and how wonderful that so small an animal as the bee shall have the same temperature of animal heat as the elephant, and that so small a body shall retain its heat. Their organs being every way the same, only the insects breathe by their surface, *and therefore there is no agitation of the air for the moisture to imbibe the fixed air.* Therefore, under this review of the circumstances, as the only difference is in this, their not having lungs to agitate the air, we certainly have a right to assert that the phenomena is owing to this cause, and to no other.

* Now, Mr. Cavendish, can you, after this review of the phenomena, still persist in your errors? if you do, must not the impartial world clearly see that a selfish interest actuates you. Consider, the longer you are in acknowledging these plain truths, the greater must be your condemnation; and that acknowledgement must be a public one.

But though Dr. Wells has made so extraordinary a declaration that, "Dr. Priestley is, he believes, the only person who has hitherto attempted to shew by what means common air brightens the colour of the blood."

Yet, in my first publication, in the year 1781, I there, I hope, clearly established that the chyle, united with the animal lymph, attract the fixed fire of the air forming round them a ring of that active repulsion to the rays of light; so as to give them that bright scarlet colour, swimming in the serum with a flat surface, the same as oil in water, forming a repulsive quality: they are also oily, unctuous, and combustible like oil, the dark spot in their middle is the solid particle of lymph and

* But as animals die in dephlogisticated air (See Dr. Priestley's experiments page, 26 of this Letter)—Is it not most wonderful, as they ought to have lived so much better in this air, being nothing but pure air, it therefore should have attracted so much more rapidly the *supposed* charcoal and inflammable air of the blood, and, in consequence, impart its fire to the blood, and also have entered in a greater proportion into it. But, every chemist allows that the blood acts slowly upon it. Then how can it, in the name of common sense, inflame the lungs. But, can any rational man let his prejudice, or theories be ever so great, suppose that an inflammation can be induced instantaneously upon the lungs, so as to produce death, as in the breathing of this air. For, positively death ensues before they can bring it below the standard of atmospherical air; but if the animal is brought to breathe atmospherical air, the inflammation instantly vanishes, and it is perfectly well. O wonderful!!!

chyle,

chyle, which being formed of an animal vegetable lymph and an acid; has a superiour attraction for the pure air's fixed fire than its acid and water have. And that this fixed fire being so loosely attached to this animal vegetable lymph, is, by the mechanical action of the vessels and muscles, so propelled against each other, and against the sides of the vessels, that their loose fire is set loose as animal heat during the circulation, and they are brought to the lungs to get their fire renewed again. So, as in inflammatory complaints, part of these globules shall have lost so much of their fire as to form that buffy coat upon the surface of the blood; also seen in the blood of pregnant women from the office of the placenta where no inflammation takes place: in which the maternal blood imparts its fixed fire to the blood of the infant.

Now, as in both these states of the human body, the blood shews the same appearances, viz. this buffy coat; we have a right to construe from the same cause, the globules having lost their fixed fire.* But a full history of all these phenomena is connected with my history of animal and vegetable life; which I have not given to the public, owing to the shameful behaviour I have received; tho' I have had it by me for above these twenty years.

When we reflect upon the food of those animals which live upon vegetable food, the sheep for instance, the acescent grass, nay, the very acescent turnip, which is its most fattening food; and shall not only make it so immensely fat, which fat our *learned* theorists say is formed of inflammable air and carbone. And all its discharges being not of an acid quality; but of the most *high* state of alkales-

* *The fire in the one case producing the increased degree of animal heat, as fever; in the other being attracted in its fixed state to the infant's blood, for its life and animal heat.*

gency and putrescency, abounding so much with the volatile alkali; nevertheless supporting so high a temperature of animal heat: and all this from the *acescent* turnip and the action of the atmospheric air in the lungs. And this process, they say, is from the air receiving the blood's inflammable air (which they allow the volatile alkali is principally formed of) and its carbone, which is proved by Dr. Girtanner, alkaline salts are formed of; and that it gives the blood in their stead, pure air, which they say, acids are principally formed of.

Now, I appeal to common sense under this review, if any doctrine can shew greater absurdity? O science!!! But let us consider these phenomena under my theory. The *acescent* turnip mixing with the animal lymph in the receptacle of the chyle is thrown into the circulation and passes the lungs, attracting the fixed fire of the air which neutralizes the acid, and, by this constant application, it becomes highly alkalescent; forming fat, the volatile alkali, animal lymph, &c. Turning from the *acescent* vegetable, to the alkaline animal. Nay, if this alkalescent animal matter is debarred of its animal circulation, it will become highly putrid and alkalescent. But, in going through this putrid process, the air imparts its fixed fire to it.— And, when the animal is dead, that is, wants its circulation to set this fire of the air loose as actual heat, it therefore becomes fixed, forming the volatile alkali. I flatter myself that I have shewn the absurdity in strong colours, the supposing that all the fire in burning inflammable bodies comes from the air; but that the principal part comes from the burning body: then, as the volatile alkali is a combustible body, containing so great a quantity of inflammable air,

it must have received it from the air. § No, they say the air receives it in such quantities, from the blood, that if the process stops but for five minutes, death ensues. The body, which in burning, acts the same upon the air as the blood in respiration

§ I do not know a more decisive fact, shewing, that the air gives out its fixed fire when injured, and that this fixed fire forms the alkali; then, the effect which the air has upon that animal, vegetable body, cheese. Examine a cheshire or any other cheese, when it is new, and when it is very old, the latter state being so immensely strong abounding with the volatile alkali. Nay, in time, it shall become animate, and creep away — And it is amazing how quick it injures the air; and this is done, they say, by imparting phlogistic bodies to the air; viz. the volatile alkali, at least what they allow the volatile alkali, is principally formed of inflammable air, and also that phlogistic body carbon. I think, if our philosophers were to live upon this phlogistic cheese for a time, or were to have their nose held over it for some time, it would bring them to their senses; the olfactory nerve being so near to their head; almost equal to the change from a sound to a rotten egg, with which I was for feeding our prejudiced theorists, in order to bring them to their senses. But I think either would do.

But, upon the other hand, it is universally known, that oranges when first pluckt from the tree are very sour or acid.— But, by keeping, they become perfectly sweet. Now, from whence does this proceed. The sour orange is constantly acting upon the air injuring it; this is, according to the French theorists, by imparting charcoal and inflammable air to the air, and imbibing the aciescent principle or pure air in their stead. But then the effect ought to be reversed, the orange should have been turned from sweet to sour; the alkaline principle being taken from it, and the aciescent principle given to it. But very unfortunately, for them, all these processes reverse their theory.

Mr Cruikshanks supposes, that sugar is formed principally from oxygen-gas; then how comes the bee's honey, as it receives no pure air by respiration? No, the sugar comes from fixed fire, neutralizing its acid. It is from this, that it is, a vegetable of a hot climate. And he also supposes that sugar contains less oxygen-gas than acids, but here the orange becomes from sour to sweet by the air, reversing his opinion. — *But they err in*

all

tion does, is spirits of wine; they both, it is said, turn the air to fixed air and water, giving it inflammable air and carbone in the same proportion as the blood does in respiration: essential oils do the same. And, our *wise* theorists say, that the blood gives the air these bodies in the concrete state of an oily body: also they agree with me that blood receives its stimulating power from the blood. Then they say this is done by discharging these stimulating bodies, containing fixed fire, to the air from the blood, and receiving pure air in their stead; which, they say, is the acescent principle.

Dr. Beddoes and others, seriously argue that the inflammatory hectic, in consumption, is brought on by pure air, the acescent principle; therefore, they order their patients to abstain from acids and vegetable food: and to use alkalescent animal food. Good heavens! is the French theory to teach physicians, in the latter end of the eighteenth century, that alkalescent food cools the blood, and acids or acescent food heat it. That is, to get drunk with vinegar, and cool themselves with spirits of wine. Nay, they actually prescribe the breathing of impure air in consumptions, and living upon alkalescent food. ¶ But then as respiration

all their explanations; for how can the acescent principle be necessary to the forming of sugar? sugar which is a phlogistic body, and which rum is made from. And is it not that saccharine quality in vegetables, that produces ardent spirits; and is not the malting of barley necessary to its being distilled into a spirit? But, all these processes, according to our aerial philosophers, are, from the vegetable receiving pure air, the acidifying principle. No, clearly beyond all doubt, it is from the vegetable receiving the fixed fire of the air that it produces spirits, which are demonstrably formed of fixed fire.

¶ A gentleman told me that, the reason why our *aerial* chemists will not adopt my theory, is, that it develops their absurdities in so glaring a light, that, they have not resolution to stand it.

and

and the burning of inflammable bodies are the same process, what becomes of all the actual fire set loose in respiration? why, they say, it is imbibed by the blood, and there lulled into inactivity. But how comes the fine membrane of the lungs not to be scorched in the process; as, the fire, even according to them, must pass in its actual state through it.—Nay, place the lungs in the actual fire which comes from the burning of essential oils; will it not burn them, ye profound *philosophers*? In short, black is white: you may as well support the one hypothesis as the other. *Oh, blind, prejudiced man!* All bodies which contain fixed fire are stimulating; and here they make the office of the lungs to be, to discharge phlogistic bodies from the blood, and receive acescent bodies (which are sedative) in their stead. Then if the lungs' office is to discharge fixed fire, from whence comes animal heat. *But, as I have all along shewn, nothing can be so absurd as the French theory, in supposing that all the fire in combustion comes from the pure air. I will just propose this obvious experiment to every man of common sense; and I call upon you, Mr. Cavendish, Dr. Black, and all the abettors of the French theory, to answer it. Let two pieces of iron be ground together in vacuo, and they will produce a great quantity of heat and fire: and, if ground long enough, part of their surfaces will be formed into a calx in the form of a powder. This clear and obvious experiment, of itself, perfectly destroys the French system. The experiment equally answers if they are ground in azote, fixed air, or any other: you chemists reflect upon this.†* But

† I believe there is not so shameful an instance in the annals of literature, as the treatment I have received; though they have pilfered my ideas and experiments, yet they never once mention my name. In the Monthly Magazine, when speaking of the
1
French

But I will mention another experiment more adapted to our present discussion.

Take a thin piece of red crassamentum cut immediately of the surface, and then cut an equal thin piece of the black crassamentum; after drying them well, then burn them in a certain proportion of pure air, and you will find the red crassamentum injure the air more than the black, and, in consequence, yield a greater proportion of fire.

Now, as the red globules had yielded all their inflammable airs and carbone to the atmospheric air, and the black globules none; how comes in this case, the red globules to be more combustible, and to have more of these bodies to give to the air? If any philosopher will examine the red globules and the black globules with a microscope, by the unctuous feel, by combustion, by the taste, the former having a more active saltish taste, by their repulsion when swimming in the serum; I am pretty certain he will say the red ones have more phlogiston or fixed fire than the

French theory, its error in supposing all the heat in burning bodies comes from the air, gives the credit to Mr. Beddoes; the proving that the heat comes from the combustible body. Dr. Beddoes, I call upon you to know what part of your works ever suggested the idea: but you have carried the French theory to a more laughable inconsistency and extremity than any other writer. See your theory upon breathing factitious airs in consumption, with my observations upon it. Now Sir, in this same magazine, when you thought that the French were complimenting our government, in assisting your plans, of introducing the breathing the factitious airs in diseases, you soon rectified the mistake; therefore, honor and justice equally called upon you to have rectified the other mistake. All my writings were forcible and clear, proving that the greatest part of the fire came from the combustible body. See my treatise on air.

other.*

other. * It is just the same with oil. By exposing the purest oil to the air, it injures the air the same as the blood. And every man knows that it becomes, in consequence, more rancid, and more inflammable: and will any *serious aerial* philosopher say that this is done by giving the air its inflammable bodies, carbone and inflammable air?— Then how, in the name of common sense! can oil become more inflammable by having previously given to the air its inflammable bodies, carbone, and inflammable air, and receiving the acedent principle, pure air? As by its exposure to the air it injured it; therefore, then, according to the French theory it must have been undergoing a partial combustion. But, after this exposure, it is more inflammable; that is, it had more carbone and inflammable air to give to pure air, than it had before exposure. Know this, and not any thing can be clearer, that, in consequence of exposure, it receives from the air its fixed fire; and, in consequence, becomes more rancid and hot, and produces more fire in combustion. † If I have proved

* If we dare mention any old authorities against our present *aerial* chemists. The great Gaubius was of opinion that the red globules were more phlogistic. Indeed every physician must know, that blood which possesses the most of these red globules is the most inflammatory; that is, implying the most plethoric state and the most liable to the inflammatory diathesis. But all *reason* is now set aside by our *aerial* experimenters, in short, black is white. *O ye learned conjurors!*

† If oil is exposed in close vessels, to this fire, it will become equally rancid, as it does in the open air. Pray, Mr. Cavendish, can it have lost its carbone and inflammable air and got pure air in close vessels, or can it have received any thing but fire in this process; can you have passed through life so many years and not know that the fat of meat becomes more tasty, salt and pungent by the fire? If, Mr. Cavendish, you are still ignorant

proved any thing (See my treatise on air) it is, that combustion is a process which sets loose the fixed fire of bodies as well as the fixed fire of the pure air. That the pure air is necessary from having its fire more loose, and therefore acts as the agent in setting loose the combustible bodies' fire; and the more fire the combustible body has, in consequence, it will require more air to set it loose. Can any thing be more easy, more rational, more simple than this theory and these experiments, far from the obscurity of the French theory, and their pompous parade of experiments. In short, having the trouble so often to argue upon so obvious and clear a fact, it becomes very unpleasant. What will future ages say of the candour and judgment of my opponents?

Another question introduces itself here; how comes combustion to take place in the lungs, under so weak a temperature of animal heat? They assign some silly reason; for the frivolity of our French chemists receives any kind of reasoning; that inflammable air in its aerial state, possessing its latent caloric, cannot unite with the pure air. But along with the carbone, it is united with the pure air, they both being in their condensed state, as oil. Will oils burn with pure air in this low temperature of animal heat? † But
to

whether fire makes blood more rancid, ask your tallow chandler when he scorches his fat, what is the consequence? He will teach you a little philosophy, at least, a little common sense.

† This idea, of bodies not uniting with one another, owing to their latent heat, is contradicted by every experiment. Will not fixed air unite with alkaline air? marine acid air, also with alkaline air? and pure air with nitrous air? in short, all the acid airs. Why fixed air will not unite with lime without water, I proved, is owing to the fire of lime being so attracted to the
earth

to shew their extreme errors; will not oils when out of the body, injure pure air: and there is no heat generated. Nay, so far from being a process of combustion, the oil generates, as we before proved, a greater power of inflammability from it by possessing more fixed fire. And this oil when burned gives out a great quantity of actual fire, and injures the air. That when no combustion takes place, the air is injured, but there is no fire. Then as the oil becomes more combustible from it, or has more fire to give to the air when it is burned, does it not clearly appear, beyond all dispute, it must have received this fire from the air. And to corroborate this theory; if the oil is exposed to fire, in close vessels, it will attract it, and become rancid, the same as it does in exposure to the air. Now, in this process, it could neither part with inflammable air nor carbone, nor receive pure air. *But positively could only receive fire.*

If the red globules are burnt in atmospherical air, the residuum is a charcoal. Our wise theorists say, this process is by giving charcoal to the air; but the residuum is charcoal. If black globules are exposed to the atmosphere, according to the French theorists, a similar process to combustion takes place; but, instead of their turning to charcoal, they form red globules, which, in consequence, became more combustible than the

earth that the water or fixed air cannot expel it singly. Just so the fixed fire of iron, the vitriolic acid or water cannot expel it singly, without their action is united, or else aided by fire, as steam or the acid made hot. These, facts in this case, are unfortunate for our *wise* theorists, as indeed every thing militates against them; for fire is necessary to their action upon each other, instead of hindering their union; and, is not the fire in the steam or vapour latent? Then how comes it from this latent fire to act upon the iron, for the water will not without the fire?

black

black globules. If that important process of respiration was a burning process, would not the Greenlanders' blood, who live upon whale's oil, be formed of the same residuum, as when oil is burned out of the body, namely, pure water, and this to resist this piercing cold climate. Bread is the food of man; If you burn it, it turns to a charcoal. But, wonderful, the same burning process in the lungs turns it to the red globules.— And if the air, in the burning took from it charcoal, how came the residuum to be charcoal?

You see what a striking set of absurdities your theory leads you to. But, in inflammatory fevers, do we not observe through the whole system, its temperature increased? See a fat man after a severe fever; all his fat, or oil, is consumed, his red globules all broke down into a loose watery blood, during the disease, and the blood puts on a strong buffy coat. What can this be owing to? the great characteristic of the fever is an excess of animal heat, and the oil and red globules of the system, are, in the same proportion, exhausted. That these bodies did not discharge themselves in their compound state we are certain, for the animal evacuations shew no such bodies: oil is a body which will not mix with water, therefore, it might easily be detected in the evacuations: also, the red blood from its colour.

If men, instead of confining themselves to their experiments in the laboratory, had endeavoured to digest those which they have already made, and to form some fixed principles to proceed upon, by enquiring whether the apparent result of those experiments agreed with the phenomena of nature, they would not run into such wild errors and inconsistencies. Atmospheric air supports putrefaction, part of it becoming
fixed

fixed air in the process, and the blood becomes alkalescent, or putrid. Then, you say, it is by attracting those alkalescent bodies from the blood, viz. fixed and inflammable air; which your advocates say alkalis are formed of. So the blood becomes alkalescent by having its alkalescent bodies taken from it. Astonishing! but the wonder is not half over; for, you say, those bodies turn the air into fixed air. Now, Mr. Henry has shewn, in his translation of Mr. Lavoisier's works, that fixed air resists putrefaction, bodies already putrid became sweet by it. And, as you say, a part of the pure air is absorbed by the blood, being attracted thro' the membranes of the lungs, it must certainly there form fixed air, as there is not even an intervening membrane to obstruct its uniting with the charcoal and inflammable air of the blood. Then how comes an animal's blood, when dead, to putrefy in the lungs? nay, let us take blood out of the lungs, it certainly ought never to putrefy, as it changes the atmospherical air around it into fixed air; and also imbibing air which it must turn to fixed air. But, I have not done with you yet. Sir, it is a fact which all physicians know, that alkalis make blood more fluid, and that acids thicken it. Now, Sir, the blood, by passing through the lungs, becomes more fluid; a fact equally well known to physicians, and this must be done, according to you, by attracting alkalis from it, and imparting to it acids, or the aerial acid, fixed air! What think you of your boasted theory?

But let us analyze the French theory of respiration, as given by Mr. Sequin. Mr. Lavoisier, he says, "proved that four-fifths of the pure air breathed, is turned into fixed air." Then one-fifth was either absorbed by the blood; or, what
Mr.

Mr. Sequin thinks much more probable, had entered into the composition of water. Mr. Sequin adduces the experiments of M. M. Cigna, Priestley, and Hamilton, by whom, this latter opinion is supported. Here, then, they suppose no air enters the blood in its aerial state. But, our philosophers finding this opinion, in contradiction to the phenomena, they think, though they have no experiment to prove it, that the pure air is attracted by the blood. But if it is attracted by the blood, it must be by the inflammable air and carbone of the blood; in consequence, they must either form fixed air or water. Mr Sequin supposes the latter, for he does not hold the absurd doctrine that the pure air and inflammable, or carbone, can join together, though a membrane intervenes, without it is done by attracting each other through it; if so, they must either form fixed air or water in or out of the blood. But some sagacious theorists say, that pure air penetrates this membrane itself, and enters the blood as pure air, and when in the blood, does not form with it either fixed air or water.

You conjurors, will not animal membranes resist the passage of air; how comes then the pure air to penetrate it, if it is not done by attraction, what bodies are there to attract it but inflammable air and carbone? Can your *extreme credulity* suppose that when the air is *in* the blood, it can resist these attractions, when air that is out of the blood cannot, though a membrane intervenes; for shame with your puerile absurdities.

Mr. Sequin says, that venous blood is impregnated with inflammable air, to prove which, he

§ The membrane of the lungs is impervious to air, the same as the bladder.

brings

brings an experiment of Dr. Priestley's, that red blood, by exposure to inflammable air, became black. But the Dr. confesses, not so black as when exposed to fixed air or nitrous air. But, I will inform Mr. Sequin, black blood will turn red from imbibing inflammable air; will it not turn red also from being mixed with condensed inflammable air in the state of the volatile alkali, and also red from alkaline salts, which, they say, are carbone. That blood which is red, from attracting the fixed fire of the atmosphere, upon attracting the high concentrated fire; as inflammable will loose a little of its rich scarlet. For, as I have all along proved, that the concentration of fire in atmospherical air is more similar to the concentration of fire in alkaline salts, than in phlogiston. But here they have no experiment to prove that the blood contains carbone. And as they themselves allow, that these combinations seem difficult; for we know that the application of a burning body is required to inflame inflammable air, and a heat of near 150 deg. of Reaumur to burn carbone in the open air: and they are obliged to say. "Although these combinations
 "at first seem difficult, for we know that the ap-
 "plication of a burning body is required to in-
 "flame hydrogen gas, and a heat of near 150 deg.
 "of Reaumur to burn carbone in open air; yet, in
 "the lungs, they take place readily without either
 "of those agents; because the hydrogen is not
 "disengaged in the state of gas, and therefore not
 "prevented, by its attraction for caloric, from
 "uniting with oxygen in the ordinary temperature,
 "and because the carbone is held in a very divided
 "state by the hydrogen."

How easily our *aerial* chemists account for every thing. Mr. Cavendish, I must here posi-
 K tively

tively stop a little to tell you a story. A gentleman, a French theorist, like yourself; was firing inflammable and pure airs, in close vessels. After one of his experiments he observed at the bottom of the vessel, a body formed of burnt hair. He was agitated, pleased, and gratified, and told me seriously, that he found out the process by which nature generates hair, and produced his specimen describing the process. I smiled, he was angry, and said it was no more improbable, or irrational, than their producing water, and began entering into a long and learned discussion; saying, that these burnings and explosions were performed upon the surface and the heads of animals, and in consequence, hair generated: but *I laughed, and granted that it was no more improbable, than their forming water*, and said his reasoning was so modern and aerial. He grew angry, proposed a repetition of the experiments in my presence. With elevated confidence, he prepared his apparatus and cloathed his head with his elaboratory wig, and looking me in the face, saying, now you shall see. I burst out a laughing, immoderately; he stormed, I gravely took off his wig and pointed to the place where it was sensibly burnt. Conviction struck him; he hung his head, and I left him with saying; such are the *aerial* experiments and principles of modern chemists.

Then, the *aerial* reason why inflammable air and carbone burn in the low temperature of animal heat, is owing to these bodies not possessing caloric, which they do in their gas state. Pray, Mr. Cavendish (for I have not yet done with you) charcoal has none of this gas in its state of charcoal, why does it not burn in this low temperature? Oils, ether, and many other bodies may be formed into inflammable air; why do they

they not burn in their condensed state, in the same temperature?

But even the inflammable air, if exposed to water, will be condensed by it, and it is still obstinate in burning. Nay, the pure inflammable air, which you say water is formed of, will not burn when it has lost its caloric. An experiment of the illustrious Scheele, *who had no littleness about him.*

Mr. Scheele, by placing iron in distilled water, obtained an inflammable air in its condensed state, which, as Mr. Scheele says, "it is an oily scum which swims upon the top of the water."

Now, Mr. Cavenish, will this condensed inflammable air or charcoal inflame with pure air, in this state of condensation; or low in this temperature? Does not conviction flash your absurdities in the face? What a herculean labour have I, it is like cleaning the Augean stable!

Dr. Crawford's opinion, that, the venous blood requires more heat to raise its temperature than the arterial blood, I directly assert, is erroneous, (See my treatise on air) where I shew the errors of his experiments. I must again put this question to every physician; from whence comes the heat in an inflammatory fever, which often continues such a length of time, and reduces the fattest patient to the leanest? In their doctrine, the animal heat is said to reside in the venous blood, possessing a little more than the arterial, and it receives that in passing the lungs, and it is lost by the blood once circulating through the animal, and comes to the lungs to receive a fresh supply.†

Now

† Dr. Crawford, from whom they take this erroneous opinion, only supposes the difference to be 11.5 to 10. But see what Mr. Sequin thinks. "To explain this, he has recourse to those experiments of Dr. Crawford, which prove that the capacities for containing

Now, in my theory, the globules of the blood are formed of lymph and fixed fire; and also the fat or oil of the animal is formed of more condensed fixed fire. That the bear in Greenland, with this

containing caloric in arterial and venous blood, are nearly as 11, 5 to 10; that is to say, if it require a quantity of caloric, represented by 11.5, to heat a pound of arterial blood from Zero to 30°, it shall only require a quantity as 10, to heat a pound of venous blood from Zero to 30°."

Is it possible this difference between the heat of the arterial and venous blood could produce the increased heat in an inflammatory fever. But will it account for the animal heat? Suppose a man's blood was in this proportion, even to be near half of his solids, then the blood in the circulation would be able to give to the system scarce one degree of heat. But then, how comes it that instead of animal heat being not one degree above the circumambient air, it shall often be above one hundred in frosty weather. From whence then is the source, as the lungs give so very little to the arterial blood; how must a fly support its animal heat?

The calculation, according to Dr. Lower, that the blood in an ordinary man, is 20 lbs. therefore this blood must give out an immense quantity of heat; indeed to keep the other nineteen parts of the animal in the heat of ninety-six, in returning to the heat, for the blood is estimated to be in the proportion of one 20th of the weight of the whole, according to Dr. Moulton. Then, how is it possible to suppose that this small quantity of heat which the arterial blood possesses above the venous (viz. 11.5 to 10.) could not only supply animal heat in Iceland; but, in inflammatory fevers to increase it to such an excess. But let us attend to local inflammations, we will suppose an inflammation in the foot; the increased circulation according to them, must make the carbonated hydrogenous gas of the venous blood enter the arterial, and increase the heat from 11.5 to 10: then O wise philosophers, the inflamed foot could only receive a little more than one degree of heat above the usual temperature; after that was gone where is the reservoir to replenish it again? the blood must go to the lungs before this heat can be renewed. Pray let me ask them if, under this review, we could have either animal heat or general or local inflammations? Why, I will confidently say, that this *wonderful reservoir* would not supply animal heat enough for your little finger, Mr. CAVENDISH.

immense

immense store of fixed fire, in the form of fat, shall be able to sleep over the winter; but, in the spring his fat is gone, being decomposed, and going to the support of his animal heat, which, though low in his sleep, is still considerably superior to the atmosphere. And another phenomenon it beautifully accounts for; if an animal body is laid in water, it will, in time, become a species of fat. The cause is this, not being exposed to the air, it cannot attract as much fixed fire as to make it putrify, or alkalescent; and that process does take place, and it gradually receives so much as to form a fat or oily substance, a kind of wax or spermaceti. Therefore, in a fever all this fixed fire of the animal is set loose as actual heat; nature having such a reservoir of fixed fire, the fever will be able to support itself for so long a time. To prove that this theory is true, examine a patient who has died of an inflammatory fever; if all his body is not principally exhausted of its fixed fire: and, the bodies which contain this fire are not discharged in their compound state, we are certain; for we should easily detect them.

Now, Mr. Cavendish, all these facts no man can dispute, and they are so obvious that no one can mistake them. But I am not so sanguine as to suppose they will be admitted. No, I know the combination that is against me, and that they can command the public opinion. But I know this. *Magna est veritas et prevalibit*; and when truth does triumph, beware of her sentence: I shall certainly demand of the public that retribution be made, and such behaviour shewn in its proper colours. Ignorance shall be no apology; for, it is impossible, after my writings, that the most common intellects can mistake them. The gentlemen with their "little blue backed books,
" and

“ and the gentleman with the great blue backed “ book”* are, probably, indifferent to censure.— Indeed they are below notice, being only mere agents. But you and many others, Sir, whom I shall some day mention, I hope are not. If my doctrine had received a fair and candid investigation, it would have been well. But, the low arts to suppress it, which have been employed, betray a meaness, which I supposed impossible with philosophers. But, Sir, the day of retribution *must* and *shall* come. Now let me seriously call upon the public not to let their judgment be biased, but seriously to think for themselves. What must future ages suppose, when the truth was so clearly placed before them, that they did or would not see it? but were led by a set of designing men and their mercenary agents. Let reason assume her dignity, and shew the same spirit and sense that our forefathers possessed. Are our studies to investigate nature, or to exalt the characters of a few individuals, whose conduct will one day be shewn in its proper colours.

I flatter myself, I have made it clearly appear to every impartial person that no oxygen-gas is received into the blood by respiration; but it is turned, in this process, into fixed air, and that a part of the fixed air is imbibed. Then let us

* Nicholson's Chemical Journal's principal object is, to keep up the absurdities of the French theory. One cannot help remarking its extreme partiality for this system, and its defenders. In Count Rumford's paper, on the explosion of gunpowder, only a part is given. But, in Dr. Pearson's, the whole, nay, even a considerable part which the royal society rejected giving to the public: it is a journal formed of all the trifling chemical papers that can be collected in favour of the French system, while he is yet perfectly mute to any thing in its opposition.— But it is a most puerile performance.

reason

reason a little further upon it; though, I must own, I ought to ask pardon of my candid intelligent reader in enlarging so much upon what appears so obvious and clear: but, I must apologize, having so artful and powerful a combination against me; aiming to overpower, both reason, common sense, and justice.

The office of the lungs is, according to them, to take charcoal and inflammable air from the blood. Then let us see what those bodies are:— by burning oil, which the Greenlanders *live* upon, the air, they say, performs the same office as it does in the lungs; taking from the oil both its charcoal and inflammable air; and leaving the residuum in a state of *water*. But, as water is not capable of nourishing the body; the oil's nourishment must have been from the charcoal and inflammable air. Then dame nature acts *a most wasteful and extraordinary* part, contrary to her usual œconomy; she obliges the stomach and other functions of the body to digest such a quantity of nourishment, that if it is not constantly discharged, even if the lungs stop but for five minutes, death is the consequence; besides consuming unnecessarily a quantity of food. Wonderful, O you *aerial* chemists! But, as they suppose, the food, by passing the lungs, becomes more stimulant, according to Dr. Goodwin.— How comes this stimulus to be increased by taking those bodies from it, which spirits of wine are said to be formed of; as the burning of spirits of wine: the same phenomena are produced, the residuum is water, and the air is turned to fixed air. Then, by taking those stimulating bodies from the blood and turning it into water, the heart is stimulated by this water to do its office; which, if the blood had gone to it with all its charcoal

charcoal and inflammable air or spirits, it would not have stimulated the heart, in consequence the animal would have died. But, as I have forced them into this opinion, that all the fire does not come from the oxygen-gas, but that the greatest part comes from the burning body; then, how can it be supposed that the blood, by losing charcoal and inflammable air, bodies which are composed principally of fixed fire, should become more stimulant, from that cause producing animal heat.

Pray, you wonderful aerial philosophers, does not fire stimulate the animal body?

Mr. Cavendish, you and other philosophers, are to be taught the facts resulting from common sense; ask a child if fire will burn? yes, a burnt child dreads the fire. If you had been aware of that, you would not have persisted in this foolish doctrine, after what I had before said. Then does not fixed fire stimulate? Ask a child if brandy will not intoxicate? here let the child be your teacher! Then how comes the blood in the lungs, if the pure air does not act upon it, to kill in five minutes? as in this low temperature, viz. animal heat, charcoal, and inflammable air cannot act upon it, they require a red heat. The reason they assign is, that the charcoal and inflammable air do not possess caloric, therefore they will unite in this low temperature. But, you vague *theorists*, will charcoal burn in this low temperature of heat, or will inflammable air when condensed? Will spirits of wine, the most combustible body we know of, do it? though these bodies possess, according to Mr. Lavoisier, none of this caloric.— Then *how* can you suppose an animal is killed by its lungs being stopped. If these *wonderful agents*, charcoal and inflammable air, had exuded through
the

the membrane of the lungs into the air vessels, in an aerial state, and not having air to carry them off; then upon an animal, being suffocated, we should have found the lungs filled with these bodies: but, even in that case, why should they kill, as the lungs might expire them: and, you say, that these bodies are discharged in their condensed state; but, in suffocation, are they found in the lungs, and how could they kill the animal?

The stimulus of spirits of wine must have come from charcoal and inflammable air; as, in burning of it the residuum is water; and, the air, according to them, can only take charcoal and inflammable air from it. Therefore the fire and stimulating quality of the spirits of wine, must come from its charcoal and inflammable air; for, when the air takes them from it, the residuum is water. In burning oils, the residuum is water; and as oil affords the greatest nourishment, it must have been from its charcoal and inflammable air.—Therefore, the two great offices of respiration, according to our wise philosophers, are to take from the blood both its stimulating and nourishing parts; turning it into a state of water. Behold your wonderful theory dissected. Mr. Lavoisier says, that water consists of hydrogen and oxygen gasses; and that these gasses, with the addition of charcoal, or carbone, form alkohol or spirits. Also, he says, it appears that one pound of olive oil, consists of 12 oz. and 5 grs. of charcoal and 3 oz. 2 gros. 67 grs. of hydrogen. Therefore, clearly, beyond a doubt, these bodies, charcoal and inflammable air, according to their doctrine, are the stimulus and nourishment of the animal system; and, that the office of the lungs is to take both the stimulus and nourishment from the blood, reducing it to water. These, O you wise chemists, are the *clear*

and positive deductions from your theory, beyond all dispute; the gross absurdity of which, I will not debase my pen by controverting. It could only be in the way I have named their doctrine in the *Gent. Mag.* for 1792. vol. LXII. page 815.* [Now,

* ‘ This refutation of Mr. Tenant’s hypothesis was in the hands of Sir Joseph Banks, together with the *Reflections on Phlogiston, or fixed Fire*, which I am now publishing in the *Gentleman’s Magazine*, so early as the 14th of March; and, very soon after, they were in the hands of Mr. Cavendish, for the purpose of being read before the Royal Society; an honour which I have been disappointed of. This is not the first time that I have found it necessary to be very careful of dates; and, I hope I shall be pardoned for mentioning this circumstance here, as I wish it to be generally known, that I had refuted Mr. Tenant’s experimental hypothesis a considerable time before it was done by a writer in the correspondence of the *Monthly Review* for March last. The singular, and I hope I may be allowed to call it, absurd, idea, that fixed air is formed of charcoal and pure air, originates from an experiment in which it appears that, when charcoal is burnt in a given quantity of oxygen-gas, there is a greater weight of fixed air left in the residuum than the original weight of the oxygen-gas. But Dr. Bewley has proved that part of the fixed air comes from the charcoal, which, it is well known, possesses fixed air. But, to shew more forcibly the different modes of reasoning of our present philosophical chemists, let us examine Mr. Kirwan’s explanation of Homberg’s experiment in *Mem. Par.* 1703: “ If sulphur be digested in oil of turpentine, and then slowly distilled for ten or twelve days, it will be converted into vitriolic acid.” Mr. Kirwan says, that the vitriolic acid is formed from the fixed air in the oil of turpentine being decomposed, its pure air leaving its carbone, and uniting to the sulphur.

‘ Here, according to Mr. Kirwan, the oil contains, as a component part, so much fixed air as to be sufficient to form the sulphur into the vitriolic acid. But, wonderful to relate! if this same oil of turpentine is burnt in oxygen gas, the fixed air which is left is considerably less in weight than the oxygen gas; but charcoal, which they say possesses no fixed air, leaves the fixed air heavier than the pure air. But, as a proper explanation of the experiment, let us attend to Dr. Bewley. “ Here it appears that two high phlogistic bodies, by the action of heat, without either dephlogisticated air or acids, will have their phlogiston

Now, Sir, I publicly declare to you and to the world, there is not a more clear fact in philosophy, than that my theory is true, in supposing that the blood receives from the air, its fixed fire, which neutralized the fixed air of the pure air, and, in consequence

giston 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 decompose 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.” See Bewley’s Treatise on Air, p. 60.

‘But Dr. Bewley having shewn the fallacy of their arguments in respect to the composition of fixed air, when speaking of pure air being formed by exposing pure water impregnated with fixed air to the rays of the sun, concludes in this jocular strain: “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 surprize when I tell him, that Mr. Lavoisier seriously proposes a manufactory to obtain charcoal by the decomposition of fixed air?— See his Elements, p. 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 alcohol 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 be turned into good British spirits; which would render

consequence, becomes a stimulus to the heart, and, in short, to the whole animal. From muscular action, and the beating of the arteries, this fixed fire is decomposed into animal heat; and, in consequence, the blood is brought again to the lungs to get it replenished with fixed fire.

Now,

that article still cheaper. But, alas! this, I am afraid, will still be one of the chemical desiderata; 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."

'Had our chemico-aërial philosophers not stuck so closely to their experiments in bottles and glasses, crucibles and gun-barrels, but looked into nature, and tried if their theory, would account for, or be confirmed by, other phenomena they would have foreseen and avoided the dance of absurdities into which it has led them.

But to follow Dr. Bewley's facetious manner, Mr. Lavoisier, in his Elements, p. 106, says, It appears that one pound of olive oil consists of 12 oz. 5 gros, 5 grains, of charcoal, and 3 oz. 2 gros, 67 grains, of hydrogen." Now, wood in being charred well, by the action of the fire, gives out nearly the same proportion of hydrogen, or inflammable air. Then wood and olive oil are the same body. But, to be more exact, by heating charcoal red-hot you may make it imbibe the same proportion of the purest hydrogen from metals, about the fourth of its weight, chemically attracting it with great avidity, then they ought to form olive oil; but would Mr. Lavoisier, or his adherents, wish for such oil to dress their sallads with? Most of our aliments are supposed, by these new theories, to be principally formed of carbone, or charcoal; but I am afraid it would be found difficult of digestion. Mr. Tennant, in his decomposition of fixed air, formed charcoal that would not burn, it being principally the earth of the lime, or, in other words, charcoal upon a par with the sallad-oil of Mr. Lavoisier.

Yet such are the absurdities of our modern theories. But, as Mr. Kirwan, in his Essay upon Phlogiston, p. 52, says, "It is impossible to deny all credit to those who asserted that lime-water was precipitated by taking the electric spark in common air,

Now, I call upon all the philosophers, teachers of chemistry, &c. no longer to patronize this absurd French theory, so opposite to common sense. *But if they are still deaf to reason, candour, and justice, I hope the impartial world will pass a just, public, and fair censure upon them.* I have called upon them to relinquish or defend their absurdities. I have challenged, coaxed, and irritated them to it; but all to no purpose. They well knowing that to defend them, would bring them to public notice; and their absurdities would be condemned, laughed at, and ridiculed.—Therefore, they presume, upon their supposed *great* names, their pompous apparatus, and their artful combination.

But I positively declare, that all their boasted experiments in their laboratory, are only to be rationally explained upon my theory; calling upon them to produce *even* one against it; for, when closely examined by their own, there are the same absurdities, inconsistencies, and errors, as I have just shewn that there is in its explaining the phenomena of respiration.

But let us now consider the process of vegetation, its action upon the air, and see if it will not shew the same errors in their theory. Vegetables act upon air the same as animals do; reducing it to fixed air. We know that vegetables cannot live without air, the same as animals. And, according to our wise theorists, it is by giving to

air, though it did not succeed with Mr. Cavendish, either from his using an instrument of different power from that used by others, or air phlogisticated by a different process." Now, in order to conclude this long note, permit me to ask Mr. Lavoisier, if he can possibly suppose the electric spark to be charcoal?—Yet, according to his theory, it must be charcoal by turning pure air into fixed air.

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the air their charcoal; and, they say, that charcoal is the food of vegetables, it being this body they receive from manures. And it is this body that they say the air receives from them, so, the vegetables are just the agent to transmit this body to the air, then how are they nourished? I suppose they will say a part is retained by the vegetables. But let us see if that is the case.

If you take a mustard seed and examine the charcoal it contains and then take one and place it in cotton and moisten it with the purest distilled water, it will grow most luxuriantly, producing a number of the same seeds; here is a great accumulation of charcoal, then, how could it have received it? Why, our wise theorists say, it is done by giving charcoal to the air. It being like Fortunatus's purse, the more was given out of it, it became the fuller.

Can we have a more convincing argument; that the air acts upon the vegetable, by giving it its fixed fire: for, how hot and spicy this vegetable is, and how quick it grew! Then, doubtless, all this hot spicy oil which both the vegetable and its seeds possess, either came from the air; or from the water, they containing a fine green oil. And, we know that in order that vegetables should possess this fine green oil, they must be exposed both to the air and sun. And, certainly, beyond a doubt, receiving their fire which the vegetable consolidates into its substance. For, it is well known that, that part of the vegetable which is covered or excluded from the air and sun, becomes white and blanched, losing its fine green oil which gives it that green colour. Therefore, demonstrably as this green oil is got from the air and sun, it must be formed of fixed fire, and not of carbone or inflammable air; as the air could not give the
vegetable

vegetable these bodies; nay, if it is not, they say, constantly taking these bodies from it, the vegetable dies; and the sun could not give it charcoal, &c. Then does not this immediately prove, that this oil consists of fixed fire, and when it is burnt in the open air, the air's fixed fire being set loose, sets also loose the oil's fixed fire? In consequence, the air is left as aerial acid, having lost the fixed fire in both processes, viz. vegetation and combustion.

And another observation I must make upon this vegetable: as it has nothing but the distilled water, no earthly mineral substance to enter its circulation, it therefore receives all its nourishment from the air; and which, we have shewn is principally fire, either fixed or free: therefore, these circumstances sufficiently account for its hot pungent taste and quality, and its growing so quickly. Why all the hot aromatics grow in hot climates, is owing to fire abounding so much there; while the more acedent vegetables grow in colder climates.—Let the world be no longer imposed upon by a set of designing men; a set of superficial observers, experimenters, and reasoners, but think for themselves.

It is very extraordinary how the philosophical world is blinded by the authorities of those to whom it is attached. Dr. Priestley had fallen into the error that azote was the proper air for vegetable life; but, which opinion was clearly confuted, both by the great Scheele and myself. Yet Mr. Gough pertinaciously adheres to it; apparently because Dr. Priestley was of that opinion. Yet, even his own experiments directly confute it. He says, p. 493. “Hence it appears, that the
“putrefactive fermentation destroys the vegeta-
“tive power of seeds surrounded by *azote* or covered

“ vered by water : consequently the presence of
 “ *oxygene* is necessary for preventing this destruc-
 “ tive process ; which it does by producing
 “ another, that may be called the *vegetative fer-*
 “ mentation.”

But then, according to that blind partiality, he supposes that vegetation, after the first period, differs as the vegetable gets older ; and, the experiment to prove this, is, a slip of spearmint, the bottom of it was placed in river water, with all its pure air, and also exposed to the air ; but the top was placed in azote, and exposed to the light. He says, “ The leaves began to wither in a few days.”

My reader will smile, no doubt, at his proof.— The leaves falling off, “ A number of fresh shoots
 “ appeared in their places, both under the water
 “ and under its surface.” But, Mr. Gough, do you not know that water, with vegetables in it, will produce pure air, when exposed to the light ; and also, that water contains a great quantity of pure air ? Therefore, this active vegetable will receive oxygen-gas sufficient to make some fresh shoots. But if azote was the proper air for vegetables, how came the leaves to wither so immediately. If you were exposed to an atmosphere that made your limbs drop of, would you say that was the proper air for animal life ; and which immediately killed infants, as it does the infant vegetable ? the fresh shoots appearing, only proves that the slip had an internal circulation.

But, Sir, take two vegetables ; both placed in the earth, and even in water, with one of their upper surfaces in atmospherical air, and the other in pure azote, both over mercury, and then you will clearly see that the one is the proper pabulum of vegetable life, and the other directly noxious

to it. Vegetables require pure air, and act the same upon it as animals do, receiving its fixed fire. But their blood and juices have not that strong attraction for fixed fire which animal blood and juices have. Discovered by the one becoming putrid so much sooner than the other. †

The sheep is an animal, whose pasturage enriches the ground considerably more than any other animal. Then, how is this to be accounted for by the French theory; as, according to it, the ground must be incorporated with more charcoal: and, as the office of the sheep's lungs is to give the air charcoal, from whence then could the sheep give the ground the charcoal? Also, it is well known that, if the grass which the ground produces, was regularly ploughed into it again, the poorest soils would, by this management, become the richest.

How is this accounted for? Vegetables, you say, give charcoal to the air. Nay, if it is not constantly doing it the vegetable dies. Then, undoubtedly, the soil ought to have been exhausted of its charcoal; even though the vegetables were ploughed into it again. Is it not well known that, exposing the earth to the air, by what the farmers call fallowing, the ground becomes richer? Then this, according to your theory, must be by the air receiving charcoal from the earth. But attend to my explanation.

† That charcoal contains aerial acid, as well as inflammable air, appears, from an experiment of Mr. Scheele's, 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 tied to the mouth, the alkali, on the other hand, loses its causticity and effervesces with acids."

The sheep being the most enriching animal for ground, is owing to its fleecy coat in resisting the air's taking from it its animal heat; in consequence this heat is retained in the body, and forms fat or oil; and its manure is, in consequence, so much more phlogistic, or possessing more fixed fire. Also, as vegetables act upon the air the same as animals, receiving its fixed fire; and the sun likewise giving them fixed fire; when they are ploughed into the earth again, it must receive much additional fire. Vegetables are well known to possess a degree of vegetable heat above the circumambient air: in short, vegetables are only animals of much lower degree. And do we not know, that the different species of both imperceptibly run into each other; naturalists not being able sometimes to distinguish the gradation.—Oils afford the greatest nourishment both to animals and vegetables, and are they not formed of fixed fire? Also, the best manures are those which possess the most fixed fire; and, likewise, which consist of bodies that have the greatest attraction for the air's fixed fire. In fattening animals they often give them food made acedent, which attracts the air's fixed fire.

Chemists, seeing the extreme absurdity in which I have placed Mr. Lavoisier's theory of caloric, upon Dr. Black's doctrine of latent heat; have, therefore, run into the opposite extreme, and say, all the fire comes from the burning body. They think, that they can reconcile this change with the rest of Mr. Lavoisier's system: one would suppose I had not to contend with rational men, but children. They actually cannot carry their ideas to, or reason upon the most obvious and clear facts. If in burning metals, as there escapes such an immense body of actual fire, which, according to this new doctrine, must

must have resided in the metal; therefore must not this immense body of fixed fire have had some essential office to do in the metal, to change its properties, and to alter its chemical principles. Can you, Mr. Cavendish, reconcile their absurdity? We see earths by being exposed to a strong fire, formed into glass: if fixed fire is used as alkaline salts, it will aid the process; are we to commit a greater absurdity by saying this alkaline salt is pure charcoal, according to some late experiments.

If I add alkaline salt to the calx of lead it will form it into a glass; if the volatile alkali or alkaline air, &c. it will form it into a metal; even with those calces of lead which have given out all their pure air, these phenomena will take place. † Then by burning the metal the volatile alkali will be consumed as actual fire, and the metal becomes a calx again. If in burning nitre and lead a great deal of actual fire is let loose and the metal becomes a calx; here the fire, they allow, comes from the metal. ‡ If I burn nitre with charcoal, all the fire must come from the charcoal; but if I burn the nitrous acid and essential oils, then all the fire must come from the oils; or in burning bodies in the dephlogisticated marine acid air the same phenomena take place: then let us enquire how the different combustions are conducted. According to this new theory, the nitrous acid, and essential oils, must burn in this manner:

† Here it clearly appears, that fire of a high concentration forms metallic earths into a metal, and fire of a less concentration into glass. Even fire will produce the same, if strongly urged to the calx of lead (without addition) it will form the calx into a metal, but if not strongly urged more of it will become glass.

‡ And in Mr. Cuthbertson's experiments, by the active fire of electricity, the metal will be burned into a calx *in vacuo* in fixed air or in azote, here there was no oxygen-gas in the process.

the pure air of the nitrous acid must attract the charcoal and inflammable air of the essential oils, and upon the oils loosing their carbone and inflammable air, their fire also becomes loose and actual; so that, instead of inflammable bodies or phlogiston and caloric repelling one another, according to Dr. Crawford, it is the reverse: for, as bodies part with the one they part with the other, and as pure air imbibes the one from the burning body, the other also leaves it.

Poor Dr. Crawford! if thou hadst been alive, thou wouldst have seen how our *aerial* philosophers mangle every thing, even thy erroneous doctrine; the doctrine upon which they founded their own. Essential oils, burning in air, leave a residuum of water: here then essential oils consist of charcoal, inflammable air, and caloric. But let us take pure carbone and pure inflammable air separately, and burn them; charcoal and pure air likewise burn: then charcoal consists of carbone and caloric, inflammable air and pure air burn in the most violent manner, then inflammable air consists of inflammable air, and an immense quantity of caloric; condensed inflammable air forms an oil, then it also possesses an immense quantity of fire.

Now let us carry *this wise aerial* doctrine to respiration and putrefaction. In respiration, the air attracts even through the membrane of the lungs, they say, these gross bodies, charcoal and inflammable air, from the blood, but no heat is set loose; the lungs shewing no more heat than the rest of the body. In putrefaction, also, the air imbibes from the blood or animal flesh, carbone and inflammable air, but no fire takes place, seen by placing a thermometer in a cadaverous body.— But still more wonderful! Mr. Cavendish; I will place your absurd theories in a still stronger light, *if possible.*

possible. The black globules of the blood, by giving to the air their carbone and inflammable air produce no fire or heat. But, if these globules which are now red, be burned in the air, they will give more charcoal and inflammable air to the pure air, than they would if burned as black globules; and, in the last process give out an amazing quantity of fire; far more fire than they would have given out if burned as black globules. Then, must not this conclusion strike every man of *common sense and reason*, that the black globules receive caloric from the air in respiration and putrefaction; and, as the pure air becomes fixed air and water, in respiration, it must be loosing its fixed fire and water which neutralized them into that fire air, called pure air.

For if pure air is made fixed air and water, by charcoal, and inflammable air, received from the blood, in respiration, being to its full saturation (for the red globules will not injure the air any more); how, in the name of common sense, have the red globules more charcoal and inflammable air to give to the air when burned, than they had before they passed the lungs. What can be clearer: have I not incontestibly proved that fixed air and water, applied to the rays of the sun, become pure air, the fire neutralizing the aerial acid? will not the volatile and fixed alkalies neutralize acids! *and have I not proved them to be fixed fire?* They say that the charcoal and inflammable air, which the blood gives to the air in respiration, are in the state of condensed hydro-carbonated air: but, will this condensed hydro-carbonated air, burn in the temperature of animal heat? Nay, in a far lower degree of heat bodies putrefy; which is, according to them, a process of combustion. But, had philosophers been anxious to investigate truth, they

they would have known that, this hydro-carbonated air is made from ether and essential oils, by heat.* Then will these oils and ether burn in pure air, in these temperatures?

I say, expressly, that there is not one experiment of theirs but which can be explained upon my system, and by no other; and I have repeatedly called upon them to prove, if they can, to the contrary; they have never been able to do it, but skulk behind their pompous apparatus and supposed great names.

That fire entering into bodies, in large proportions, certainly must make a part of those bodies. Thus, in sulphur, phosphorus, &c. For, when those bodies are burned, an acid appears (even when no oxygen-gas is in the process) as the vitriolic, phosphoric, acids: and, as we see alkaline salts are principally formed of fixed fire, which we *must* know neutralizes acids; therefore, as the acid of pure air is neutralized, no acid appearing till it has been burnt, and, in consequence, lost its fire, the same as when sulphur was burnt; is it not clear it is from the same cause? Which is demonstrable: as, by exposing fixed air and water to the rays of the sun, we can make a pure air, of the same kind as the pure air of atmospherical air; not so high a concentrated air as the pure air of the laboratory. Also, this pure air of the laboratory is made from an acid, being neutralized by fire in the process, as we have explained.

Mr. Lavoisier was under a necessity, from his doctrine, of calling pure air the acidifying principle: he, therefore, called it oxygen-gas. For, according to his doctrine, those phlogistic bodies, sulphur, phosphorus, and charcoal, were all turned

* See the late French publications where both ether and the essential oils were formed into their hydro-carbonated air by heat.

to an acid, by being compounded with it, viz. the vitriolic, phosphoric, and carbonic acids.— But then our late philosophers, seeing that in many processes, where pure air is imbibed, their doctrine says no acid is generated: as, in the firing of pure and inflammable airs, they say, water and no acid is generated. But, that there is an acid generated, in this process, is fully proved by my former experiments and publications; also, by Dr. Priestley's two late publications (which are principally taken from mine) and more particularly from the new experiments named in this letter. But let us consider this doctrine of their's more closely, and see what absurdities it leads to. Pure air, according to my doctrine, is formed of an acid, water, and a concentration of fire, something similar to alkaline salts; and when made from nitre, of its acid and alkali; but, upon heats being applied, the acid being fixed to the alkaline salt, is not therefore so soon aerilized, as it is when by itself: but, as it has a strong attraction for fire, discovered from its being so easily evaporized, also from the explosion of gunpowder, it being from its attraction for fire, therefore, flies off with it, and produces those great mechanical powers seen in Count Rumford's experiments.— Then, as the acid, along with the water which the nitre contains, attracts a great quantity of fire, and, as the acid attracts fire, it loses its attraction for the alkali, seen from heated nitre, that even the vegetable acid can set the nitrous acid free from the alkali; but then it is set free in its phlogisticated state, or a state in which it possesses a quantity of fixed fire, or that actual fire which it has fixed in the process. And, as the nitre is exposed to a great heat in the process; the nitrous acid, and the water, receive such a quantity as to be aerilized;

lized; but, their attraction for the alkali makes them receive a small proportion of it, so as to form that neutral body called the pure air of the laboratory: but I have shewn it is not so good an air as the pure air of the atmosphere, as that is formed from the fire, or solar rays of the sun, fixed air and water; therefore, an air not of such a high concentration of fixed fire, as the pure air of the laboratory; the latter not answering so well for either animal or vegetable life, but burns with cracklings as if it was full of some combustible matter, as Dr. Priestley has well observed. Fixed air and water, by being exposed to the sun, will form pure air; which I have proved in the most unexceptionable manner. See my former writings. Therefore, when this neutral air is burned with inflammable bodies, an acid must always appear: if the fire is very intense it will take from the air its aerial form, the residuum being water, and an acid; as in the burning of inflammable and pure airs. Or, when sulphur or phosphorus are burnt in pure air; as, they are formed of the vitriolic and phosphoric acids, with a high concentration of fixed fire; the intensity of the burning being so considerable, along with the dry acids, being robbed of their fixed fire, or phlogiston by combustion, will attract, forcibly, both the fixed air and water of the pure air; otherwise, how should these acids be in a fluid state, after the process, if they had not imbibed water from the pure air: for, even a quantity of water may be got from them after the process, and yet they will be still fluid. Therefore, when in the state of sulphur, or phosphorus, no one can suppose they contain water, but, are the dry acids united to fixed fire.

Why this phlogistic neutral aerial body, called pure air, is necessary to combustion, is, that its
fire

fire is so loose that it is easily set free as actual fire; and therefore acts as an agent in setting loose the fixed fire of the sulphur and phosphorus. But, the atmosphere's air's fixed fire is more loosely combined than the pure air of chemists; therefore phosphorus burns in the former in a lower temperature than the latter: also, the former supports animal and vegetable life so much better than the latter. For, it is owing to this loose concentration of its fire; that Dr. Priestley and Mr. Bergman found, even the electrical fire passing through it would set it loose, and leave the fixed air and water it was combined with.*

Then, as we see clearly that pure air is formed of an acid, there must always be an acid in the residuum, when pure air is burned. And, that the acid comes from the pure air, is certain, from the experiments of Dr. Girtanner, viz. alkaline salts, being burnt in pure air, turn it into fixed air; and, I suppose even our *credulous* chemists will not say that the acid of fixed air could come from the alkaline salts. If they do, they are not deserving of serious argument; I give them up to the French philosophical mania. † It is just the same process in respiration; the black globules, being formed of a lymph and a condensed acid, have a greater attraction for the fixed fire of the atmospherical air than its own acid and water, and, in consequence, they attract it: for, condensed acids have a stronger attraction for fixed fire, than when in their aerial state.

* And it is from this that Mr. Cavendish found atmospherical air, necessary, in the process of decomposing pure airs into the nitrous acid, and, not from the azote, as I have fully proved.

† The exception, they say, is in burning pure air and inflammable air; water being the residuum. But why this exception? is not this being the only one, a strong argument, that, an acid is in the residuum upon burning these airs.

As nitrous air is immediately decomposed by the nitrous acid. But, the black globules cannot so well decompose the pure air of chemists, its fixed fire being more closely attracted; and these black globules, then attracting this loose fire of the atmosphere, it gives them a kind of repulsive fire, which surrounds them; making them so active in repelling the rays of light, and also a high refractory power: so that this fire being so loose that even the mechanical attrition they meet with in the circulation, sets their fire loose as actual. But, they say, this repulsive vesicle is given by the acidifying principle, pure air; and likewise this refractory power: but acids immediately decompose the red globules even into their state of whitish lymph. Dr. Wells says, p. 423. "If a small quantity of a concentrated mineral acid be applied to a piece of dark crassamentum, the parts touched by it, will, for an instant, appear florid; but the same acids, added to a solution of the red matter in water, do nothing more than destroy its colour."

The true explanation is this; the high concentrated acid, being dropt upon the black globules, sets all the fixed fire of the globules loose, instantaneously; which, in becoming actual, gives that florid appearance only for a moment. Mineral acids do the same with essential oils. The nitrous acid sets such a quantity of their fire loose as to produce ignition. Then, well may the concentrated acids set the loose fire of the globules free: but if the acids are diluted with water, they produce no such phenomena, setting the fire of the black globules so gently loose, that no floridness appears. By violently agitating the black globules, you produce the same floridness, by setting loose their fixed fire as actual. And it is easily set loose in combustion, and then it acts upon the burning bodies

as nature's great agent in setting loose the combustible body's fixed fire: and, in respiration, putrefaction, and other processes, this fixed fire is easily attracted from its union with the fixed air and water of the pure air.

It really makes one smile at the manner of arguing which our *great theorists* make use of. Dr. Wells seriously tells us that both acids and alkalis act upon the globules by increasing their opacity, and that the latter makes blood more florid, from that cause. But, he says, that acids make blood more opaque, and yet destroy the colour altogether. And, there is not a clearer fact than this, that alkalis make black blood florid, and that acids make florid blood black: but wonderful that both act by increasing the blood's opacity, he says; and from the opacity of the blood being increased, the blood becomes florid and red; indeed, Mr. Cavendish, you certainly have great honor in rejecting my papers and admitting these and others.—Physicians all agree that acids coagulate or thicken blood, that alkalis make it more fluid, this has all along been their doctrine, but our present theorists overturn every thing with their *aerial* wisdom, which is truly as light in the scale of reason as air itself, and your "illustrious" name Mr. Cavendish, will not, I believe, add to the weight.

This doctrine of all the fire coming from the air in combustion, *even* from the supposed condensed air in their nitrous acid, is very extraordinary. But to shew the extreme fallacy of this doctrine, I have all along proved; when the vitriolic acid and water are applied to iron it is calcined and an immense quantity of inflammable air is generated. They say that these phenomena are owing to water being robbed of its supposed pure air, it being attracted by the metal forming a calx, and the in-

flammable air is set at liberty; then the water must so possess such a quantity of calor as to form it into inflammable air. Nay, even the other supposed component part of water which enters the calx must carry an immense quantity of caloric along with it: for the marine acid, they say, from attracting this supposed pure air from the calx of lead, receives along with it such a quantity of caloric, that bodies burn in this marine acid with a far greater degree of heat than in pure air in its aerial state.* But in burning lead it is calcined and an immense quantity of fire comes from it; this fire, or caloric, they say, is from the air being *condensed* in its being attracted into this calx. Nay, what an immense quantity of fire comes from the burning of inflammable air and pure air; but, the water, (the residuum) from their explanation of these phenomena has more fire to give back again when it is decomposed. Can you, Sir, as a philosopher, a man of sense, and a rational man, sanction such gross absurdities? No, I have forced the world to relinquish this doctrine of caloric. Then, as an immense quantity of fire does come from the burning of pure inflammable airs, are we not authorized to suppose, that this fire formed an essential part of these airs? That the water could not possess all this caloric, I think *we may directly assert*; then, it must have come from the metal.— That the vitriolic acid did not possess it, appears equally certain; for, upon adding the water to it, previous to its being applied to the iron, a great heat came from them; † and, that great heat alone will

* Also the *aurum flammans*, fulminates, they say, from the fire of the condensed pure air in the calx, the fire of which is so very intense.

† As both phlogistions and ante phlogistions, built this hypothesis

will reduce either the calx of mercury or lead, is well known. I say, in the Gentleman's Magazine, Vol. LXII. part 2, p. 908.*

Now,

thesis upon Black's doctrine of latent heat; they must both fall to the ground: for, the phlogistions say, as pure air attracts the phlogiston, its latent heat, or caloric is repelled, and becomes actual, according to Dr. Crawford. But, if black is black, and not white, phlogiston is not an element, according to Stahl, but fixed fire; and enters into the composition of bodies, forming an essential part of them.

* ' We find that those bodies which possess a high saturation of fire, viz. combustible bodies, when they have parted with it, will attract a more moderate saturation; thus, for instance; metals, by being burned, having lost their full saturation, will attract fire in a looser state, or in the same state that lime, or the caustic alkaline salts do; for, if a metal is dissolved in nitrous acid, the acid will first attract its phlogiston, forming nitrous air. But, if this metal is precipitated by another, possessing an inferior attraction for phlogiston, it will be precipitated with its metallic splendour; and, if with the caustic alkaline salt, it will be precipitated with its fixed fire.

' In burning vegetables which possess phlogiston, they, upon parting with it, attract fresh fire in the state of an alkaline salt. It was for this reason that the old chemists considered alkaline salts as being formed from incineration, and they procured it by burning vegetables, which vegetables they knew did not possess it before their calcination; for, instead of an alkaline, they are generally possessed of an acid, or at least an acescent quality.

' As we have always supposed alkaline salts to be principally formed of fixed fire of a less concentration than what is termed phlogiston, we shall give further proofs of it, in order to corroborate the theory. Metallic earths, after calcination, possess so much fixed fire in an alkalescent state, and of the quality, or disposition to actual fire in this state, that they act as fluxes in forming glass; and every chemist knows that alkaline salts are the great flux made use of in vitrifying bodies, or turning them into glass. We find that inflammable air, oils, and other high phlogistic bodies, form metallic earths into metals; but, alkaline salts of a less saturation of fire, form them into glass. And as the same bodies are capable of entering into both states, so, if oils are applied, they will form the metallic state; but, if alkalis are applied, they will form the vitrified state.

' And

Now, here we see alkalis, which, I think, I may be allowed to call fixed fire, will either form lead into a glass, or metal. This fixed fire, in the state of iron, by having water applied to it in the form

‘ And, as in the metals, some of the metallic earths which have a strong attraction for fire will be reduced by it alone, and so it is by the vitrifying earths; those that have a strong attraction for fire will be formed into glass by its power alone; but those which are refractory will require alkaline salts the same as the metals; many requiring phlogiston to reduce them.

‘ There is a near resemblance between metals and glass; from a loose earthy state they both become hard and compact; and even cast-iron, which is a species of iron imperfectly reduced, not having its full saturation of fire or phlogiston, will crack and break like glass.

‘ Still further and more powerfully to corroborate my theory. If oils are applied to the calces they will recover their metallic form; but, if alkalis, they will be vitrified. And, in reducing a calx of lead by fire alone, I have formed part into lead, and part into glass. And of the following fact I am well convinced: in reducing metals we generally find a part refractory, which cannot be made into metal; as in lead for instance. That there is such a considerable waste in the earth of lead called litharge, is owing to its having received an alkaline saturation of fire, and being thereby prevented from taking the metallic saturation; the acid of the air, combining with the earth so as to aid the fire, becomes alkalescent. And for this reason it is that we find this refuse of the metals more adapted to act as a flux. I have likewise found this refractory earth to go further than the other in the saturation of an acid; and that it is capable of precipitating the other less alkalized earth when dissolved in an acid. And it is owing to the same cause that lime cannot be made to imbibe the same quantity of fixed air it before possessed, as part of the earth has received a certain quantity of fire producing an alkalescent state. Lime, from the fire it possesses, being soluble in water, and having, like salts, a bitter taste, becomes of an alkalescent quality.

‘ But I should suppose that the following will be admitted as an *experimentum crucis*, to prove that the earths of metals, during their reduction, imbibe fixed fire in the state of what has been denominated phlogiston. If you reduce a calx with oil or charcoal, it is supposed to imbibe phlogiston from those substances;

form of steam, will be aerilized into fixed fire, as inflammable air; so, also, when iron is calcined with the aqueous vitriolic acid, a great heat is generated in the process, which heat is necessary to its being formed into an air. But, if iron is applied to cold water, the inflammable air only appears on the surface of the water in its condensed state; being an oil, as Scheele observes. || And, I must again repeat, that, as Dr. Black's doctrine of latent heat, which Mr. Lavoisier had introduced into this theory, must be given up:—this caloric must have been an integral part of these airs, and must have formed an essential part of the qualities of them. Combustion will take place without oxygen-gas, as I proved, from the burning of sulphur

stances; but, to vitrify it, you add an alkaline salt. There is supposed to be something mysterious in the part which the charcoal or oil acts in the process; but in the second, as every chemist knows, the alkali is imbibed, and enters into the glass.—For that the process is similar is evident from this, that the calx, when vitrified, parts with the acid it had received; if from the air, in the state of fixed air.

'It hath been satisfactorily shewn by experiments, that oils and alkalis will become volatile. Mr. Lavoisier found that the vegetable acid and alkali, which he calls the acetite of potash, will, if distilled, become the volatile alkali, p. 270 of his Nomenclature. The volatile alkali having acquired a higher saturation of fixed fire than the fixed alkali will form metallic earth into their metallic chrystallization, while the fixed alkali will form them into the vitrified chrystallization.'

|| Here is a great difference between the fire which forms the phlogiston into inflammable air, and the fixed fire which is an integral part of its compound. The oily scum which is found upon water, in which iron is placed, will burn equally as well as inflammable air. Nothing can be more simple than this experiment: if the steam of water pass through charcoal, as the steam carries to it both water and fire, it will form the two component parts of charcoal, viz. phlogiston and fixed air, into inflammable air, and the aerial acid. The simplicity of my theory immediately recommends it.

and

and iron: then, it is almost unnecessary to say, that this inflammable air, the phlogiston of Stahl, is formed of high concentrated fixed fire, with water which forms its aerial basis. Nothing can be more obvious. But, I ask you, how can you explain it otherwise? I call upon you to shew.

Mr. Scheele says, p. 166. "I have a ready method of finding out *whether the water contains empyreal air or not*: I take, for instance, one ounce of the water, and let fall into it about four drops of a solution of vitriol of iron, and add two drops of a solution of salt of tartar somewhat diluted with water. It yields immediately a dark green precipitate; which, in a couple of minutes changes into *yellow*, if the water contains empyreal air; but if the water be previously boiled and grown cold without the access of free air, or, if the water be distilled lately, the precipitate retains its green colour and does not turn yellow, unless about an hour after; and never changes into yellow, if it be kept in full glasses so that the free air can have no access to it."

All my quotations from Scheele are from his experiments on air and water. This is a question much agitated, viz. in the calcination of iron, with an acid and water, what is it that the calx imbibes in order to its becoming a calx. I have all along shewn that both the vitriolic acid and water are necessary to the calcination, to decompose the earth of iron of its phlogiston; the same as both the aerial acid and water, are required to rob the calcareous earth of its fire, when in the state of lime. This elegant and simple experiment of Mr. Scheele's, I think, will greatly help us in the elucidation of the phenomena.—When the alkali attracted the acid of the vitriol of iron, the iron being robbed of one of its
soluting

soluting bodies, attracts the pure air of the water, which turns it from green to yellow. But, according to the French theory, the earth of iron had attracted its full saturation of pure air from water. Why then does it attract the pure air of the water? And, why does the pure air of the water change its colour from a green to a yellow? How comes it that the calx of iron, when calcined in the open air, is always yellow; whilst its other calces are green. The calx of iron made by the steam of water, is always heavier than the calx made by the air; and it will never become the colour of rusty iron, if ever so long exposed to the air.— From this cause, it is calcined by the hot water, which forms a different coloured rust than the air forms; and, as the calcination from the air, is from iron imbibing its acid, therefore, there is a less quantity of the acid than the water, required to saturate the calx; the former being a stronger body than the latter, therefore forms a lighter calx. The iron still continuing green (in Mr. Scheele's experiments) the water shews that the calx of iron attracted the water to supply the place of the vitriolic acid; but, when there is pure air in the water, it attracts it in preference, and turns yellow. [This, I flatter myself, is incontrovertibly a rational explanation of the phenomena.] And the proportion of pure air the calx imbibes is very considerable. Therefore, undoubtedly, when the vitriolic acid was united to the calx; it was not fully saturated with pure air, as it imbibes pure air immediately as the acid leaves it: and it will not imbibe this pure air while it contains the acid. and in water which has no pure air, the calx will not imbibe pure air by decomposing the water, as it remains green; and if it did, inflammable

air, according to them, would be generated, which is not the case.

The experiments of Lemery are well known to every philosopher: "Having mixed 25 lbs. of sulphur with an equal weight of iron filings, and having kneaded the mixture together by a little water, into the consistence of a paste, he put it into an iron pot, covered it with a cloth, and buried the whole a foot under ground. In eight or nine hours the earth swelled, grew warm, and cracked, hot sulphureous vapours were perceived, a flame which dilated the crack was observed, and a subterraneous fire producing a volcano in miniature was lighted up, the experiment has been frequently repeated; it is to be observed that too much or too little water will equally prevent inflammation. About twenty-five years ago, the experiment of imitating a volcano was tried at PARIS, upon a larger scale; whether by the directions of the late Duke of Orleans, or of the Count Laurigaes I do not recollect. The necessary ingredients were deposited under a mount, raised for the purpose, in a garden. A perfect volcano was thence exhibited, to the no small alarm of the neighbourhood."

Now, Mr. Cavendish, how can we reconcile this experiment with your extraordinary, and I have no hesitation in saying, absurd doctrine: from whence proceeded all the fire? according to Mr. Lavoisier, neither sulphur, nor iron, possesses any fire: and, as to the water, all its fire is dissipated when its two supposed composing bodies, viz. inflammable and pure airs, united. I say, you wise *aerial* philosophers, from whence comes the fire? here is no pure air required in the experiment, the ingredients being buried many feet under ground. This I will prophecy. that future
ages

ages will wonder and ridicule the present age's ignorance, and credulity, in such absurdities, being seriously advanced. Must not every one who has an atom of sense see, that, in this experiment, the fire came from the sulphur and iron, the water having a great attraction for the earth of iron, the vitriolic acid of the sulphur, and the sulphur also for the earth of iron: here is such a fermentation produced, as to set their phlogiston, or fixed fire, loose as free fire. This explanation is just, as the sulphur is turned into the vitriolic acid, and the iron into a calx.

What a number of experiments and singular constructions our aerial chemists have made; the vitriolic acid and water being applied to iron, calcine it and generate inflammable air. Another strong mineral acid, the nitrous, with water applied to iron, calcine it, and nitrous air is produced. In the first process, they say, the water is the calcining body, in the next, they say, the nitrous acid. Only reflect upon such a different explanation in the same process; you credulous chemists! it being almost impossible to yield to such absurdities, to what does it lead you? See page 22 of this Letter.

Candles clearly elucidate this subject; when they are first made, they readily decompose the air, attracting its fixed fire, and reducing it to fixed air; which they continue to do for some months. After that time they no longer act upon the air, but evaporate their oil or combustible matter into the atmosphere: in consequence, it is well known that a candle will not burn well when it is first made, but gradually improves as long as it acts upon the air injuring it. But, after that period it loses; and, if it is kept for a long time it will not burn at all, having lost all its inflammable matter.

matter. Now, Mr. Cavendish, according to your system it should have been best at the first; as the air acting upon it, and receiving its charcoal and inflammable air, being a process of combustion, therefore, when it came to that period when the air would no longer receive the candle's charcoal and inflammable air, it should have lost its inflammability. But it is, at this period, in its perfection, or highest state of inflammability.— And, after that, when it has really given out its phlogiston to the air, it injures it not; but the candle's inflaming quality is discharging itself, so that in time it will not burn. What will phlogistons say to this?

You see, Mr. Cavendish, that the strongest facts may be obtained by acute observation in the commonest departments; without having recourse to your pompous apparatus, which is only calculated to embarrass truth.*

If

* The British Critic, upon reviewing my Chemical Essays (it being the first time that this learned Critic had exercised his liberal powers upon my theory) made his remarks upon three points. The first learned observation, was, upon the catch word being wrong, in the first page; and from this circumstance, he arrogated to himself a degree of penetration in being able to detect the error. To such trifles as these I fully allow his competency. But, his next remark was not so lucky, for he there was travelling upon unknown ground. I had shewn the fallacy of our aerial philosophers, in supposing the pure air, or oxygen-gas of the laboratory, and the pure air of atmospherical air being the same. Here the Critic triumphed, directly asserting I was wrong, without any explanation. But denouncing, from his *little* critical chair, my condemnation.— The next was an illiberal insinuation against the validity of my experiments; his verdict being pronounced without having tried them. Now, Mr. Critic, allow me to make an observation or two upon your supreme judicature. As to your first bright observation I own you were right, that the catch word was wrong. Correcting the press myself, and my mind being absorbed with
with

If philosophers, without stuffing their laboratories with their complicated pompous apparatus, more calculated for the pageantry of a pantomime, than for true chemical discoveries, had attended to the facts which are constantly offering themselves to our observation; they would have seen the extreme futility of their boasted system.— Thus the manure of a stable, when piled together in a great mass, what a prodigious heat it produces. Also, hay, if stacked too green. Now the heat produced in these two bodies, could not come from the air: for, the larger the mass, the more considerable the heat, and it is generated principally in the centre of the mass, where air could not have access to it.

Now, Sir, I have long been thinking of the body which possesses the greatest quantity of this wonderful charcoal and inflammable air, which, according to you, both *kills, nourishes, and stimulates* the animal; and coal is the body, as the burning of an ounce of coal will injure a greater

with the profound subject, such errors may be found; for indeed I am not anxious about them, otherwise I do not know what our gentlemen with their "little blue backed books" would do if it was not for these trifles; therefore, I think they answer the purpose of a fly cage, to attract all the noxious insects. And every spacious room should have such a cage.— For, if I had had a few more catch words misplaced, this *learned* fly might not have been so illiberal, as they would have formed the basis of his criticism. But, let me give a little advice to you and the rest of your gentry. You are not supposed to be a body of men who are at all conversant, or have a critical knowledge of those very abstruse subjects: but, if you were to frequent the resort of men of learning, you might there glean enough to make up your monthly budget; here the ignorant part of the public, who form their opinions upon your *decrees*, might form them more justly. But I am afraid there is too much intrigue with booksellers and authors for you to act so candidly; being the mere mercenary tools of others.

quantity

quantity of pure air than the same weight of any other body, and you say the coal injures the air by giving it charcoal and inflammable air, and in pure coal very *few ashes* remain. Then, Mr. Cavendish, coal ought to be the most nourishing food an animal can subsist upon; superior to oil, as it injures the air in a greater proportion than it does: that is, it gives it more of these nourishing and stimulating bodies, viz. charcoal and inflammable air. Now, Mr. Cavendish, by putting your theory to the test, if you were to try to live upon coals. No, Sir, I should be sorry your experiments should kill you, I can only say, "go to bed, go to bed" and dream no more of such idle, ridiculous, and foolish absurdities. But, awake; with your eyes open to candour, truth and justice.

It appears the most *extraordinary doctrine*, that, in respiration the lungs discharge those phlogistic bodies, viz. inflammable air and charcoal, and receive pure air, the acescent principle. The food of animals, being the acescent vegetables, while the animal is so high a phlogistic body, and having a constant temperature of 96 deg. of heat; also, its discharges are so phlogistic. But let us put this doctrine to the test. 'Inflammable air, which every one acknowledges possesses a large quantity of phlogiston by being exposed to black blood, it is absorbed by it so as to lose its inflammability, and the blood turns red. Now let us examine the red blood. By exposing it to injured air, or foul air it turns black; and Dr. Priestley mentions an instance where the air became better, and was diminished after it by nitrous air. (Priestley, Vol. iii. p. 77.) The application of fixed air to blood equally turns it black. In short all phlogisticated bodies, oils, spirits, salts, &c. turn black
blood

blood red. All acids and other dephlogisticated bodies, even water, turn red blood black; nay blood turns black *in vacuo* where no phlogiston can get to it, but only has liberty to exhale its own (as related by Father Beccaria) and the change is too immediate to suppose it is from putrefaction. But as strong a fact as any, is, that animal phlogiston turns blood immediately red; this is seen very strikingly in the animal urine when taken in its most rich state as discharged from a person under a high fever. For when the phlogiston is rapidly set loose and discharged, then it will have the strongest effect, turning the blackest blood immediately red. That urine is composed of phlogiston no one can doubt, from its possessing such a quantity of volatile ammoniac salts, and from its volatile pungent smell. The saliva has the same effect, which is another phlogistic animal juice.— Besides the rays of the sun have the same effect. But water has just the opposite effect. All these facts are so very striking they must make us entertain little doubt.

But, I will finish with my observations upon the cameleon, which is *well known* to live almost without food; there being well attested facts of a single fly subsisting it for a month: nay, it can subsist without that fly. See its natural history.— Therefore, I suppose, Mr. Cavendish, you will allow these two things. First, that it is an animal which requires very little food. Secondly, that it can have very little charcoal and inflammable air to give to the air in respiration, and, that it might, in consequence, do without lungs; as, according to you, these *noxious, nourishing, and stimulating* bodies, viz. charcoal and inflammable air, would not *possibly* be in such abundance as to require their discharge, the wonder being how the animal could
subsist

subfist upon fo little nourifhment. But *know*, Mr. Cavendifh, that it has a greater proportion of lungs than any other animal; it being comparatively almoft *all cheft*; nay, nature not fatisfied with that, has given it a fingular power of throwing into its cellular membrane, over all its body, a great quantity of air, which muft be expofed to its blood and juices, fwelling and enlarging all its furface, and, as it fubfides, or forces out the air, loaded with your charcoal and inflammable air, it takes frefh air in again.* Now you muft fay, Mr. Cavendifh, this fly poffeffs fo much of thefe nourifhing and ftimulating bodies, that, if nature had not given it this fingular and extraordinary faculty of making its body *in a manner all lungs*; it would not have been able to have difcharged, in the courfe of a month, all the nourifhing and ftimulating bodies which this fly poffeffs, befides, the quantity which the cameleon would require for its nourifhment and animal heat.

But, Mr. Cavendifh, let us fee how thefe phenomena agree with my theory. As it has little food to attract and decompound the air, therefore, that which it has to form its blood ought to be as much expofed to the air as poffible, in order to attract as much of the air's fire to fupport its animal heat. And that its blood does attract a

* Chambers' dictionary fays, "The trunk of the body is properly all breaft, for the creature has no belly, its ribs being continued to the *ilia*." This evidently fhewing that its principal organ is its lungs, and having no belly, fhews it requires very little food; it having, in a manner, no organs to digeft it.— "The thicknefs of its body is not to be determined, as the creature alters that at pleafure whilft it more or lefs inflates its body; and this inflation not only goes through the whole body, but into the legs and tail." How clearly nature's defign is to throw into this animal as much air as poffible, as it has peculiar organs for the purpofe.

greater

greater proportion of fire, is proved; for, its surface being so phlogistic, as its skin (another singularity of the animal) changes its colour into such vivid and varied hues.

But I see, in the Monthly Magazine, that Mr. Gren has some experiments to prove that no pure air is received into the blood by respiration. But, nailed to this infatuated French theory, he supposes that the animal dies from suffocation, the lungs not discharging these phlogistic bodies* So this acetent vegetable food produces so much phlogiston that the animal requires the lungs to be acting every moment, and, if they stop their function but for five minutes, death ensues. Also, it requires its animal heat of 96 to be constantly discharging its phlogiston, as free fire; besides, its discharges are so phlogistic. Nay, the Cameleon requires more than any other animal, all these discharges of phlogiston which it received from the fly, O Wonderful!

I also see in the Monthly Magazine, an account of experiments that vegetables decompose pure air into fixed, and also decompose fixed air again; such hocus pocus experiments and opinions are so grossly absurd that they deserve no answer.

Men, under the influence of the French theory, are still introducing most singular doctrines, misconstruing the most obvious experiments. See an account of M. De Saffure, jun. experiments in the Monthly Magazine for the month of April, 1798. It says, "In the same valuable number we find an Essay by M. DE SASSURE, jun. on the question, "Is the formation of carbonic acid essential to vegetation?" From several ingenious experiments on vegetation in atmospheric air, mixed

* The absurdities of which opinion see p. 72 of this Letter.
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with different proportions of carbonic acid, and in atmospheric air deprived of carbonic acid, Mr. De S. has deduced the following laws:

1. That plants, like animals, are continually forming carbonic acid while vegetating, either in the light or shade.

2. That like animals, they form this carbonic acid, by means of the oxygen of the atmosphere; and that the reason why the formation of this acid is not always manifest, is its being immediately decomposed.

3. That the presence, or rather the elaboration of carbonic acid, is necessary to vegetation in the light.

4. That light is favourable to vegetation, by contributing to the decomposition of carbonic acid.

5. That plants, while vegetating in the light, can support a dose of carbonic acid so strong as to destroy them when in the shade."

The vegetable juices having a far less attraction for the fixed fire of the atmosphere, therefore do not decompose its pure air into fixed air; but, only take a part of its fixed fire, bringing it nearer to the state of azote. It is for this reason that vegetables require an airy situation, the vegetable receiving a part of the air's fixed fire, and fresh air is immediately applied by the wind, so that it may receive a part of its fixed fire; but not so much as to form the air into fixed air; which is easily ascertained by receiving a current of air, which is constantly passing through a tube, wherein are placed a number of vegetables, and examining this air by lime water. But, if a vegetable is placed in close vessels, then it will not grow luxuriantly (not having the free current of the atmosphere to give to it its superabundant fire) and it will receive all the air's fixed fire, forming it
to

to fixed air. Also, vegetables having a quantity of fixed air, mixed along with the atmospherical air, they are vegetating in, their watery juices will imbibe it, as water would do.* And even as I have all along supposed, this fixed air circulating in the vegetables, mixed with their juices, may receive such a quantity of fire from the sun as to be discharged as air; in the same manner as fixed air is formed into pure air, when united with water by the sun's rays. And also, when it is floating in a moist atmosphere, as I have shewn.— And that pump water, &c. which only contains fixed air, will by exposure to the sun, produce pure air, is well known: but, upon the water's generating the pure air, the water turns green, from a number of animalcules, as Dr. Ingenhoufz and others have shewn. And when the water is thus incorporated with these green animalcules, and having given out its pure air, if you go on impregnating it with fixed air, it will continue to generate pure air. Therefore when water, which before exposure to the sun, only gave out fixed air; after exposure gives out pure air; and at the time when the pure air is discharged from the water, great numbers of green animalcules appear: allowed by every one. Then these green animal-

* It is the same in the animal body; from great inanition the inhaling vessels of the surface will imbibe, from the circumambient air, a considerable quantity of matter. See Dr. Watson's Essays. A jockey, in the course of a short time, how considerably heavier he got, though he had only drank a glass of wine. But, in a state of health, or when the vessels were properly filled, the system would have discharged matter, instead of receiving it. Therefore, what vegetables do in this confined unvegetating state, is no argument what they do in their healthy state in the open air; they respire by their leaves, therefore, in great inanition, a similar state to the jockey, they will imbibe air and moisture from the atmosphere.

cules must act upon the fixed air by arresting the sun's rays, appears clear; for, silk does the same, otherwise, if these animals acted at all upon the air, it would be by decomposing pure air into fixed air, and not fixed air into pure air.

And, I should hope, Mr. Cavendish, you will not assert that these animalcules differ so far from all other animals; that instead of discharging charcoal, as you suppose, by the air, they really imbibe it; for after the process, no charcoal is generated.

That light, or rays of the sun, are necessary to vegetation, for those plants which do not receive them, are blanched, losing that fine vegetable aromatic oil. Which is, from this cause, the rays of light receiving that impulse or motion, penetrate the vegetable, and enter into its substance.— But heat, or fire, which has not that impulse, does not enter so well into the vegetable, so as to unite with its substance; it is just the same with the metallic earths, or calces. By exposing the calces of silver, mercury, gold, manganese, &c. to the rays of the sun (See Scheele, p. 77); the calces were reduced, forming metals; but, if they were exposed to the same heat as those rays produced, no reduction would take place; the fire not having that impulse of motion as light, to penetrate the calces and enter into its substance, so as to reduce them. And, that it is heat alone which reduces metals, is certain; for, if you apply a greater degree of heat, which heat shall pass through black bodies before it arrives at the calces; it will nevertheless reduce them.

Out of the numerous arguments I have brought in my different publications, to prove that alkaline salts are fixed fire, I shall quote one in my Chemical Essays. "In forming the calcareous earths into lime, in attending to the process accurately,

we may observe different phenomena; at first the fixed air and water are expelled, and the earth saturated with loose fire. But if you push the process farther, the fire will, instead of taking this loose saturation, become more fixed, penetrating the earth, so as to become alkalescent: and in this state the lime becomes light and spungy, having lost part of its earth, and will not do for mortar, the workmen rejecting it, as being useles; for it will not split and fall into powder with water. I have, by carrying on the process for a long time, made it almost an alkaline salt, turning it perfectly mild, in respect to its causticity, and having the properties of alkalis in most of its qualities."†

Many of the leading philosophers, after the publication of my Treatise on Air, seeing in what a ridiculous light I had placed their doctrine, and how untenable their own principles were; immediately fled to the French standard, as the only one which could, in any manner, make any head against my system; and hoping, by their numbers and great names, to suppress investigation, reason, and truth.‡ But, *magna est veritas et prævalebit.*

Now

† 'I have proved, in my Thoughts on Air, that the causticity of lime does not depend upon the absence of its fixed air, but upon its saturation of loose fire; which fire, when it becomes active by water, &c. burns or consumes bodies. Now, this experiment proves it: for, when the fire is so pushed in the process, as to be more fixed in the earth, so that moisture, &c. cannot expel it, the lime then becomes mild;—and it will bear no other explanation. The idea of its wanting its acid to neutralize the earth, is quite inadequate to the phenomena; for it ought in this case to have become more caustic at the latter part of the process, instead of its becoming perfectly mild, and of an alkalescent quality.

‡ I see in a late publication, an opinion of mine given to Dr. Latham; which I had repeatedly published long before. That part of the electrical fire which is observed in a spark, comes from

Now let us consider what all their boasted discoveries amount to; why, that the animal lungs are a coal pit. The air receiving by them, from the blood, the very identical same bodies which colliers draw from their coal pits, viz. coal and inflammable air. Let this *great discovery* be handed down to posterity, as the important result of the experiments of our modern chemists. † Fame, let thy trumpet sound to future ages this glorious *truth*, of the 18th century. Let poets paint some emblematical fiction, personating the philosophers and the surprising coal pit.

Imprest with this *great, wonderful, and surprising* discovery, my imagination painted to me the scene in Macbeth; the witches, and Hecate with her "Vap'rous drop profound" caught from "the corner of the moon," dancing round the cauldron: emblematical of you, philosophers, and the coal pit; with your profound magic art dancing around it, and with the light *airy* step of a French cotillon: each philosopher *throwing* into the cauldron some experiments and suggestions; and the

from the air; the air being, in consequence, injured. To prove which, I shewed the spark taken *in vacuo*. has not any luminous brilliancy. But these things are trifles, my name was never to be mentioned, for that was their great policy. Dr. Priestley, I have shewn, has taken many of his opinions from mine, but never once hints at my name.

† From calculating the quantity of pure air turned to fixed air, in the course of twenty-four hours, by one man; and the proportion, they say, of charcoal, which pure air requires to its forming fixed air; we are brought to this data, that many pounds of charcoal must be discharged by every man's lungs in the course of twenty-four hours: but, as coal consists of both charcoal and inflammable air, they say, then it is nearer the body which the lungs discharge, so that many pounds of it must be passed through the tender vessels of the lungs in twenty-four hours, without either injuring, or blackening them. O Wonderful process, you wise aerial chemists, it is a most curious coal pit!

foremost

foremost of these *aerial* beings, which seemed loaded with the heaviest burthen, and which even made the pot to boil over, was the two experiments of burning inflammable air and oxygen gas, forming water; and oxygen gas and azote forming the nitrous acid with the electrical fire. Upon these experiments being thrown in, the *air* rung with acclamation; even a number of snarling, barking, curs, added to the noise. At last a majestic figure appears, called truth; having in one hand reason, and in the other common sense, from whom a little puff of wind arose and dissipated the whole. They vanished into airy nothing, "And left not a wreck behind."

Observations upon Count RUMFORD's Paper:

I shall now make a few remarks upon Count Rumford's Paper, in the Phil. Trans. He certainly deserves every compliment from the public for his useful researches; but I must beg leave to make a few strictures upon his paper, on firing gunpowder

Now, let us consider what gunpowder is formed of: the strongest, according to Dr. Watson, is nitre eighty parts, charcoal fifteen, sulphur five, of these ingredients nitre makes the largest proportion. We shall see, in the sequel, that one pound of nitre is composed, according to Mr. Lavoisier, 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." —

The

The Count has clearly shewn that the force of firing gunpowder is considerably stronger than Mr. Robin's, who only made it one thousand, times greater than the mean pressure of the atmosphere; while he makes it equal to fifty thousand. But, the gunpowder, in the Count's experiment, had not all its force, as it was fired in a close vessel and had not liberty to expand in the atmosphere; for, by this expansion, all the nitre and charcoal are burnt; also the sulphur is more thoroughly ignited, and their fixed fire set loose. To prove this, if you fire gun-powder in pure air, its force will be considerably stronger, than when fired in atmospherical air. The experiment is to take a large decanter, with a bladder fixed to its mouth, and place in the bottom of it a piece of hot iron, and then, gradually, drop upon the iron, repeatedly, small grains of the powder from the bladder. By this means you will be able to burn a great quantity of powder, and, you will find, according to the purity of the air, in the decanter, the powder will fire with different forces. By firing in this manner, very strong powder, particularly that made with dephlogisticated marine acid, and an alkali, in the purest air, all the fixed fire of the powder will be set loose, viz. that from the alkali, which is the principal part; and also, that of the charcoal and sulphur: so that there only will remain an acid vapour, with a little of the ashes from the charcoal. When the residuum is to be examined accurately; either a piece of heated silver, or the electric spark, are the best to fire the gunpowder.

Now let us consider the way in which Mr. Lavoisier's theory explains it. *The supposed caloric*, in the nitrous acid, is attracted by the charcoal, forming fixed air. But, he himself is obliged

obliged to acknowledge. Mr. Lavoisier says, p. 453. of his Nomenclature, "I have tried some kinds which have produced almost double the effect [meaning the force of the explosion] of ordinary gunpowder, although they give out a sixth part less of gas during deflagration."

As the sulphur's attraction for this pure air could only set the supposed caloric loose, for they generate no air, but condense air. Then of what service is the alkali? none according to them; while every one who is acquainted with the composition of gun-powder, knows that all its force depends upon the quantity of the alkali it contains. Then, according to these wonderful theorists we should find, after the operation, all the azote, or nitrous air, which the pure air of the acid was combined with; the alkali entire, united with the vitriolic acid, and an immense quantity of fixed air, which should be formed from the pure air of the acid and the charcoal. They inform us that the great heat generated in forming phosphorus, or sulphur, into the phosphoric or vitriolic acids, comes from the pure air being condensed in the operation. Now, wonderful, that this already condensed air, which they suppose, in this mineral acid, should possess all this fire, so as to cause so wonderful an explosion: nay, upon parting with its supposed azote or basis, they ought to be aerilized. Can we believe a theory so preposterous and absurd, which supposes that all these phlogistic bodies, which disappear in the process, do not contribute an atom of fire to the combustion; it all proceeding from the condensed pure air in the nitrous acid. How hard it is, in this prejudiced trifling age, I should have such absurdities so repeatedly to combat. But I shall consider, more particularly, Count Rumford's experiments. He

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says,

says, p. 233. " But what will become of this theory, and of all the suppositions upon which it is founded, if I shall be able to prove, as I hope to do in the most satisfactory manner, that the force of fired gunpowder, instead of being 1000 times, is at least 50,000 greater than the mean pressure of the atmosphere?"

Now he sees the impropriety of attributing this immense force to the generating of airs, according to Mr. Lavoisier's theory. But, which he seems to adopt; as the general principles of it are as necessary to establish his own, which is that " To arise principally from the elasticity of the *aqueous vapour* generated from the powder in its combustion." In order to find sufficient water for this theory, he says, it is even generated in the process. But they do not allow that any of these bodies contain inflammable air, to generate it: to carry their supposition so far, as, that the alkali contains it along with azote; then we should have this azote also set loose, after the process, so as to add to these wonderful generation of airs. But, Mr. Lavoisier does not, *even hint* at such a preposterous idea. The late experiments of Dr. Girtanner shew, that the alkali, according to their theory, is formed of pure carbone. Then the alkali would form with the pure air of the acid's; fixed air, in the combustion. Good heavens! what a generation of airs there ought to be in this combustion. But two necessary questions intrude themselves: from whence comes all the caloric to produce the immense fire in the combustion, and also to aerilize all these aerial bodies? and, if these airs are observed after the process? The first question they answer by saying, that all this wonderful caloric is *in the condensed pure air*, in the nitrous acid. Astonishing! As to the other
other

other question, they admit these airs are not generated. But the Count himself, with all his attachment to their theory, says, "it is dangerous to admit the action of an agent (viz. caloric) whose existence is not yet clearly demonstrated."

But what proves clearly that the charcoal is not essential in this fulminating process, is, that nitre will fulminate with an alkali and sulphur, without the charcoal; therefore, Mr. Lavoisier's doctrine is directly contradicted: for here is no charcoal to unite with the supposed pure air of the acid. The true explanation is this, the nitrous acid being united with the alkali, along with charcoal and sulphur; the charcoal as being a very combustible substance, being ground into a fine powder, and mixed with the other ingredients; upon fire being applied, it immediately takes fire; and as the sulphur is the next body for being susceptible of combustion, it also takes fire, and the heat generated is so considerable as to give that activity to the nitrous acid, that it penetrates the alkali, setting its fire loose as actual. This acid, when applied to that phlogistic body, essential oil, will of itself, without the aid of heat, or pure air, set such a quantity of their fire loose as to generate combustion. If the alkali is united with the dephlogistic marine acid, the least attraction, without heat, will make them explode, as we have just observed; or, if a powder be formed of nitre, an alkaline salt, and sulphur, and heated to a certain degree, they will explode: a well known experiment. Here, in these processes, they can only be explained upon my hypothesis; the fire, set loose from the alkali, unites with the nitrous, or marine acids, and flies off with them; causing the explosion. Heat of itself has no explosive force; it is owing to its being united to

bodies as acids, water, &c. That it has any mechanical powers, is proved from this; form a hollow globe of gold, and heat it as intense as possible, the heat will not expand or burst the globe. The Count says, that the explosion is owing to water being formed into a caloric vapour. But it is surprising, that when an hypothesis is formed, the most obvious and clear facts pass unnoticed. The nature of this wonderful elastic vapour he has shewn us. He says, p. 226. " Upon examining the vent-plug and the
 " pin, they were both found to be much corroded
 " and damaged; though I had taken the precau-
 " tion to harden them both before I made the ex-
 " periment.

" I afterwards repeated the experiment with a
 " simple vent, made very narrow, and lined with
 " gold to prevent its being corroded by the acid
 " vapour generated in the combustion of the gun-
 " powder; but this vent was found, upon trial, to
 " be as little able to withstand the amazing force
 " of the inflamed gunpowder as the others. It
 " was so much, and so irregularly corroded, by the
 " explosion in the first experiment, as to be ren-
 " dered quite unserviceable."

Let me ask the Count this question, will the vapour of water corrode gold? But here he himself allows it to be an acid vapour, and, will not this circumstance also inform our obstinate theorists, that the acid is not decomposed, but passes entire: and, I appeal to the Count's experiments if this clear elastic acid vapour is not the cause of the explosion.* If the explosion de-

* If it was the water in the salts that made the explosion, the moistening the powder would assist the explosion; but every one knows it would counteract it, and, as to the French experiments of M. De Betancour, I have no great opinion of them.

depended upon the decomposition of the acid, it could only generate fixed air, or water, according to their theory. But, if the Count will explode the fulminating powder, formed of nitre, sulphur, and an alkali; the same acid vapour he will find escape, corroding the gold: or, if the metal be examined, after its corrosion, it will be found united to the nitrous acid. These are plain, obvious and simple truths, which, it is impossible to mistake; and, I call upon the Count either to accede, or invalidate this explanation of the phenomena. He also says, p. 248. " This substance, (meaning the residuum after the explosion) " which was of a black colour, or rather of a dirty " grey, which changed to black upon being expo- " sed to the air, had a pungent, acrid, alkaline " taste, and smelt like liver of sulphur. It attrac- " ted moisture from the air with great avidity — " Being moistened with water, and spirit of nitre " being poured upon it, a strong effervescence en- " sued, attended by a very offensive and penetra- " ting smell. Nearly the whole quantity of " matter of which the powder was composed, " seemed to have been transformed into this sub- " stance; for the quantity of elastic fluid which " escaped upon removing the weight, was very " inconsiderable; but this substance was *no longer* " *gunpowder*; it was not even inflammable."

Now here it clearly appears that the nitre had lost its acid, as it effervesced so strongly with fresh spirit of nitre. But, according to the French theorists, the acid must have been decomposed, and its pure air generate with the carbone, fixed air; and also its base, azote, must have been set loose: then what an immense volume of airs this fixed air and azote must have formed, being equal in weight to the acid and charcoal; according

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to this theory (which, the more it is investigated the more absurd it appears) the Count says, "The quantity of elastic fluid, which escaped, upon removing the weight, was very inconsiderable."

But let us see how far it agrees with my theory. Upon the explosion, the fixed fire of the alkali, charcoal, and sulphur escaped, and united itself with the acid of the nitre, and the sulphur (for since the first days of chemistry, it has all along been taught that acids, particularly the nitrous, has the strongest attraction for fire, either fixed or free) therefore part of the fire united with the acids, and which produced the explosion; but as they were confined from escaping, they were forced, by the immense weight, to enter the mass again.† And when fresh nitrous acid was added to the mass, the old acids, united with the fire, escaped, producing "a very offensive and penetrating smell."‡ I think I need not comment upon their theory, which says, all the fire comes from the condensed pure air in the nitrous acid; for here is the pure air condensed; and air, in its condensation they say gives out all its fire or caloric, as in the burning of sulphur and phosphorus. Yet, in the nitrous acid, when this pure air escapes from its condensation, that it should give out such an immense quantity of fire again, is truly laughable: nay, that it shall now aerilize, both its base, which they say is azote,

† As according to Dr. Darwin's experiments; upon air being strongly mechanically compressed, it will give out heat: so of the nitrous acid in this process, by the immense mechanical force exerted upon the nitrous acid vapour. part of its heat, or fire will be forced from its chemical combination with the acid; and part of the acid will be forcibly condensed into the alkali again; but, in a phlogisticated state, that is, still retaining a quantity of fire.

‡ Mr. Scheele has observed, that when nitre has been much heated, even the vegetable acid can set its nitrous acid loose.

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and also itself, with the charcoal, forming fixed air. And this, they say, is proved from Dr. Black's doctrine of latent heat. Can we believe rational men could have produced such a theory. *O you great aerial philosophers!* You are true inhabitants of Swift's flying island. See my Letter in the Gentleman's Magazine, for August, 1796. I say in my Treatise on Air, p. 18 and 49.

Now let us consider more particularly the circumstances, half an ounce of oxygen-gas, in being condensed into the phosphoric acid, is supposed (agreeably to their theory) in its condensation, to produce heat equal to melt 34 oz. of ice: but, when a less quantity of oxygen-gas is already condensed, and hath given out the heat, which it does in its condensation, that it shall produce nearly an equal quantity of heat, so as to melt 32 oz. 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 oz. 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 and phosphoric acids. 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 with it more 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 philosophize upon this experiment, agreeably to the Drs. Black's and Crawford's theories, the

the wonder will be still greater; for in the experiment of burning nitre and sulphur, the 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 oz. of ice, had disappeared instead 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.*

‘ 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 this science persuaded, that these attractions are one of the most singular phenomena in nature,

* It is wonderful how this French theory has been so generally adopted; and even, from their own confession, how inadequate it is to account for the phenomena. Mr. Lavoisier says, in his Elements, p. 106. “ By the foregoing experiments, that
 “ quantity of charcoal should melt 76.18723 lbs. of ice, and
 “ the quantity of hydrogen in a pound of the oil should melt
 “ 62.15053 lbs. The sum of these two gives 138.33776 lbs.
 “ of ice, which the two constituent elements of the oil would
 “ have melted, had they separately suffered combustion, whereas
 “ the oil really melted 148.88330 lbs. which gives an excess of
 “ 10.54554 in the result of the experiment above the calculated
 “ result, from data furnished by former experiments.” The reason is this, the oil contains specifically more fire than charcoal.

have

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;

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even fifty times more considerable, than came from it in the former experiment; 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 decomposition 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. This now is 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 acid. Such absurd doctrines need no comment.

' 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

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 alkalis 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 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

* "Dr. Crawford in his explanation of the nitrous acid de-flagrating 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. To such ridiculous explanations are they obliged to have recourse in support of a wrong theory. I think such powerful phenomena are not to be accounted for by such slight causes."

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 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 shewn, that the heat in these phenomena is chemically united, the same as in alkalis and acids.'

I have now taken a review of all the chemical papers in the Trans. except Mr. Tennant's, upon the combustion of the diamond. And here, Mr. Cavendish, you must excuse my not entering into any discussion of it. For, to endeavour to treat with serious argument, that absurdity of absurdities, viz. that the diamond is pure charcoal, would be an equal absurdity, though it has had the sanction of the Phil. Trans. I have related a similar absurdity which this theory of yours has given birth to. See page 75.

The best of all manufactories would be burning charcoal into diamonds which is only chrySTALLIZING it; for, Mr. Cavendish, your chemical powers certainly can do it. The great Newton supposed that the diamond is a combustible body, from its refraction of the rays of light; and that the refraction of light is from bodies possessing fixed fire, is clearly proved, from the repulsive principle of fire; a principle so certain, that it alone more clearly proves the diamond's composition,

position, and is an evidence superior to all the vague rhapsodies and romances of the French theory. That all bodies which contain fixed fire should be formed of either carbone or inflammable air, is truly ridiculous; but, that according to the quantity of fixed fire bodies possess, they will, accordingly act upon the air. If not highly concentrated, they will leave the acid of the air in the state of fixed air, or its aerial state; as, in the burning of the fixed alkalies. See experiments of Dr. Girtanner. But, the more highly concentrated, as is the volatile alkali, its fire will be so intense as to decompose the air into a condensed acid and water; as in the firing of inflammable air, which the volatile alkali may be formed into.

The philosophers of the day adopt the new theory in explaining all the phenomena, forcing it in head and shoulders, without questioning its validity; but even, if true, how could it rationally account for this particular phenomenon? viz. Count Rumford, in his *Essays*, when speaking of food says, "In heating vegetables in water, the water adds to the nourishment." Then it must be by being decomposed by the process of heating. If it was decomposed by *this simple process* how is it performed? Is the inflammable air attracted by the vegetable in this *low temperature of heat*, below boiling? Then what becomes of the pure air? Or if the pure air is attracted what becomes of the inflammable air? The body which attracts the pure air is charcoal, then they ought to have formed fixed air; as to the inflammable air there was nothing to attract it but pure air which it was already united to. But our philosophers have decreed it to be so, therefore, "*it must and does.*" The advantage resulting from the process is this; the vegetables and water by the action of the fire, are
so

to united, or digested, that all their parts are more assimilated to enter into chyle; also another essential circumstance is, these vegetable bodies, receive, in the process, such a quantity of the fire as animalizes them the more. Thus barley, from the process of malting and brewing, receives such a quantity of fire from the air in malting, and from the fire, in brewing, as considerably to alter its nature and qualities. But, in the Count's calculations he forgets how much more easily a Bavarian foldier, from habit and from the climate, is supported with food, than an English foldier.*

* But pray, Mr. Cavendish, explain this experiment of Dr. Priestley's, in Vol. 4th, p. 36: also the note in page 33 of this Letter, upon the phosphoric acid and the calx of lead producing inflammable air; but they ought to have produced dephlogisticated air according to your theory.

In this Letter I have just examined your theory, principally in one department of nature, viz. respiration; but I am able to prove the same absurdities in any of the rest.

NOW, SIR, BEFORE I CONCLUDE, LET ME CALL UPON YOU IN THE MOST SOLEMN, AND SERIOUS MANNER, EITHER PUBLICLY TO REFUTE, OR ACKNOWLEDGE MY SYSTEM. BUT, IF YOU STILL CONTINUE *mute*, I THINK YOUR MOST PREJUDICED FRIENDS CAN BE AT NO LOSS FOR THE CAUSE.

I AM

Your most obedient and humble Servant,

ROBERT HARRINGTON.

Carlisle, JUNE 10, 1798.

FINIS.

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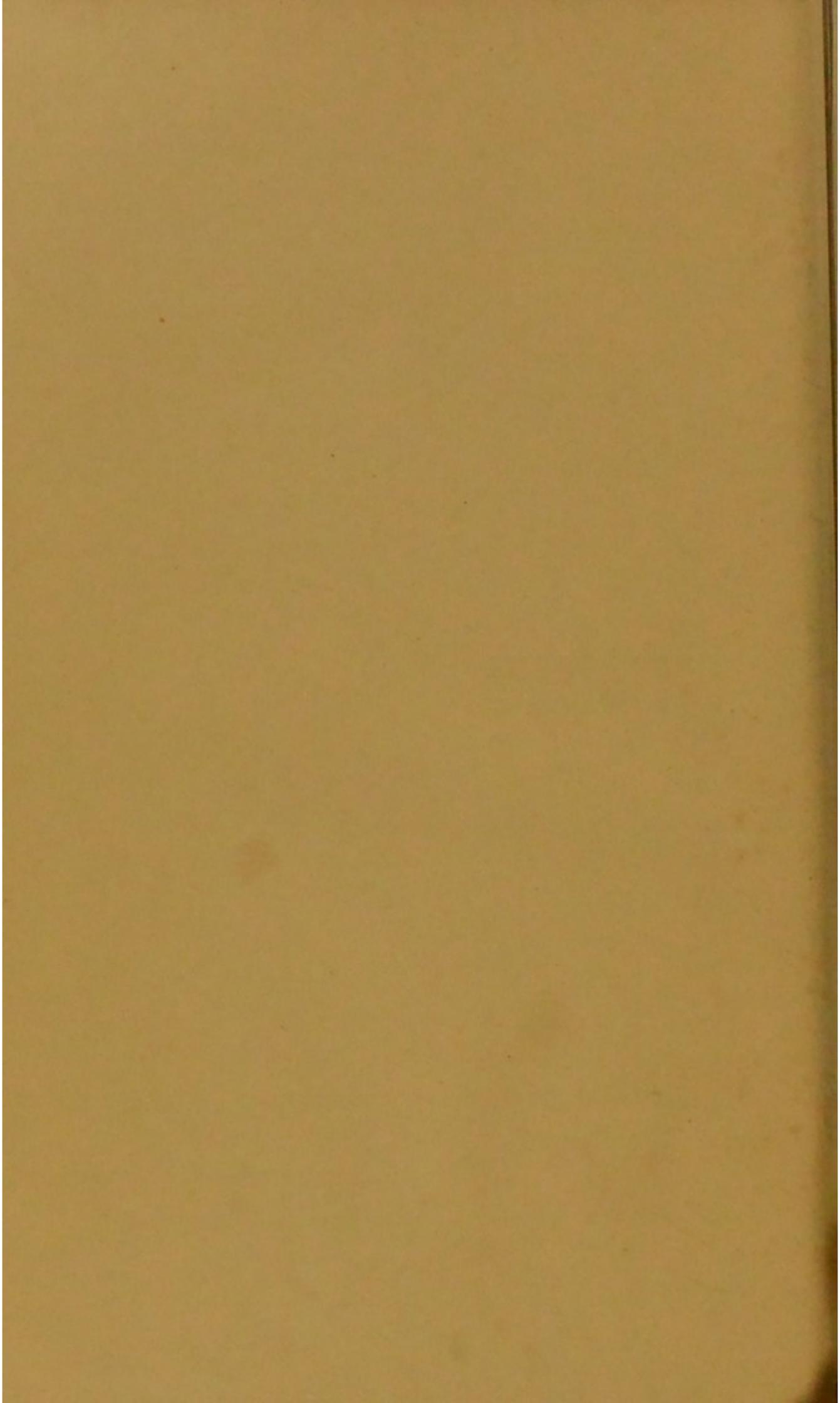
AN
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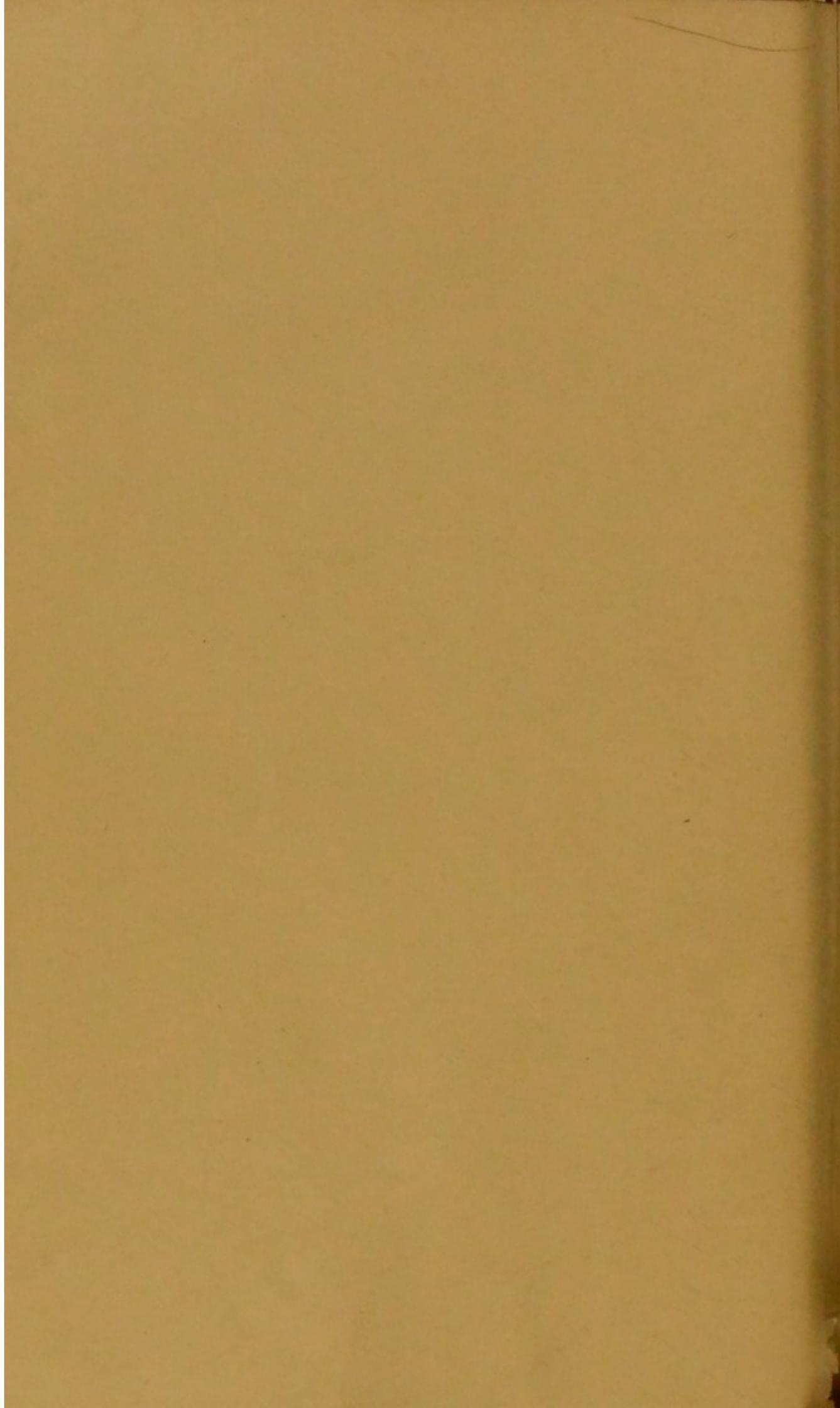
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BY ROBERT HARRINGTON, M. D.

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[Dec. 1798.]







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