

*As the following letter on the new Chemistry from the Rev. of Dr. Priestley, can have been seen by very few of our readers, we trust its insertion in the Mirror will not be unacceptable. It is copied from a late number of the Monthly Magazine.*

*To the Editor of the Monthly Magazine.*

Sir,

HAVING undertaken to examine the principles of the new theory of chemistry, I wish to excite as much attention as possible to the subject; and as your publication goes into the hands of all lovers of literature in England, I beg leave to make use of it, in order to state, in a general way, what appears to me to be the strongest objection to the system, which has now reigned triumphant about twelve years, very few persons besides myself being advocates for the old doctrine of phlogiston. I have already published two pamphlets on the subject, and I intend to continue the controversy till I have collected all the evidence that shall be sufficient to decide the question; and if in the interim I see reason for so doing, I shall publicly acknowledge my conversion to the doctrine that I now controvert, and shall even take a pride in so doing. In the meantime, having heard what has been advanced by some very able advocates for the new system, in answer to my first pamphlet, I think I am pretty well apprized of all that can be said with respect to those experiments that are yet before us. But in time something more decisive may be produced. In reply to all that I have yet heard on the subject, I would observe,

1. When a metal viz. iron is dissolved in the vitriolic acid, the antiphlogistians say, that the inflammable air which is procured does not come from the iron, but from the water, which is decomposed in the process. But, according to their theory, water consists of two principles, hydrogen gas and oxygen, and therefore, if the hydrogen be set at liberty, in the form of inflammable air, there ought to remain an additional quantity of oxygen in the vessel; and I ask where this is to be found? They say in the scale of the iron. But I answer, that this scale exhibits no appearance whatever of its containing any oxygen, and the acid attached to it yields less dephlogisticated air on being subjected to a red heat, than the same quantity of the acid that was employed in the process. And if this scale be afterwards exposed to the test of a burning lens in atmospheric air, it is far from making any addition to it, that this air is diminished.

It is acknowledged by my opponents, that after the solution the acid in the vessel will not saturate more alkali than it would have done before. Since, then, this additional quantity of oxygen which the new theory supposes, cannot be found, either in the form of an acid, or of dephlogisticated air, what evidence is there of its existence? And is not the probability greatly in favour of the inflammable air coming from the iron rather than from the water, and that by the loss of this principle it becomes a scale? If this be the case, metals are compound substances, and water, as far as we yet know, a simple one; whereas, according to the new theory, metals are simple substances, and water the compound.

2. When steam is applied to red hot iron, inflammable air is procured, and the iron receives an addition of about one half of its former weight, such is the something with what the forge-men in England call *fiery oxide*, and with the *scale of iron* in a blacksmith's shop. This substance the antiphlogistians say is an oxide of iron, supposing that the water is decomposed by passing in contact with it when the hydrogen is separated in the form of inflammable air, and the oxygen remains united to it. But I ask, what is the evidence of this substance, containing any oxygen, when iron neither is reduced to an acid, nor exhibits in the form of oxygenous gas, or dephlogisticated air? I think that the addition to the iron is mere water, and when it is heated in inflammable air, the iron is revived, and the water set at liberty.

Another evidence of a solid substance, like this, containing oxygen, is its oxygenating, or as I call it, dephlogisticating, the marine acid. But though this substance is completely dissolved in this acid, no sign of oxygenation appears. Indeed, some very slight sign, barely perceptible, sometimes appears on the formation of scales of iron, to which (being formed in the open air) it is probable that a small quantity of oxygen may adhere. But if this small quantity be dissolved, I ask why is not more discovered, when more water is present? The agent is the same, and it has much more matter to act upon.

They say that this fiery oxide is a partial oxide of iron, and common rust of iron a complete oxide. But since iron receives a much greater addition to its weight by becoming fiery oxide, than by being converted into rust, and all the addition is allowed to be pure oxygen, the former ought to contain more of this principle than the latter. Hence, fiery oxide is incapable of becoming rust. 'Tis contrary to fact, it is asserted; but let the observation of the fact decide between us.

I further observe, that when any solid substance, containing oxygen or dephlogisticated air, is heated in inflammable air, a quantity of fixed air is formed, by the union of the oxygen from the substance and the inflammable air in the vessel. This is the case when sulphur is revived in this circumstance, but not so when fiery oxide is used, nothing but water being found in the vessel. Also, when iron, or any substance containing phlogiston, is heated in dephlogisticated air, fixed air is produced. This the antiphlogistians say comes from the phlogiston in the iron. But the phlogiston in the iron employed is not one hundredth part of the weight of the fixed air produced, if the phlogiston could be separated, and decomposed in the process, which it cannot.

That fixed air may be produced by the union of dephlogisticated and inflammable air, I further prove by heating together *red precipitate*, which yields only dephlogisticated air, and *filings of iron* which give only inflammable air; when both are in a copious production of the pure fixed air. This, however, I am informed my opponents deny. With me the experiment has never failed. Let others judge between us. If this be the fact, here is a copious production of what the antiphlogistians call the *carbonic acid* without any *carbon*, which they say is the only source of it.

But the argument which my opponents urge with the greatest confidence, is drawn from the supposed composition of water, viz. from dephlogisticated and inflammable air mixed in a certain proportion to each other: I say, however, that when these two kinds of air are fired together, they produce either the nitrous acid, or phlogisticated air, which is known to be capable, by decomposition of forming nitrous acid. When the inflammable air is more than sufficient to form nitrous acid, the phlogisticated air is produced. This I demonstrate, by firing the two kinds of air, in a close tube of glass or copper.

To this my opponents have objected, that when I produce any acid, it must have come from the decomposition of the phlogisticated air, which I had not been careful to exclude in the process. But I am confident that, in my last method of making the experiment, there was not the least sensible quantity of phlogisticated air present, and that, in all the cases, the acid produced was a hundred times more than the phlogisticated air could have formed. Whenever their flame burned strongly, so as to exhaust more dephlogisticated air than they wished,

the water they produced was not free from acidity, and whenever it was, they found a surplus of phlogisticated air, which agrees with my experiments.

That dephlogisticated and inflammable air, united in certain circumstances, can form phlogisticated air, I have shown in several processes. Inflammable air, exposed to rust of iron combined by mercury, becomes in time wholly phlogisticated air, and the rust is converted into a black powder, which no doubt is the same thing with plumbago. This substance, therefore, is iron superoxidized with phlogiston.

On the whole, I am as far as ever from seeing any evidence of either the composition or decomposition of water; but on the contrary, much and very satisfactory evidence against it. I write, however, with no other view than to promote a full and fair discussion of the subject, which is certainly of considerable importance in chemistry. I am, Sir, your's, &c.

Northumberland, J. PRIESTLEY.

Dec. 20 1797.