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assure you that as long as the art of writing has been practised no letter has ever been written with such an ink. Dealing now again in my ozone business, I found out the other day that all manganese salts, be they dissolved or solid, are decomposed by ozone, hydrate of peroxide of manganese being produced and the acid set at liberty. Now to come round again to my ink, I must tell you that these lines are written with a solution of sulphate of manganese. The writing being dry, the paper is suspended within a large bottle, the air of which is strongly ozonized by means of phosphorus. After a few minutes the writing becomes visible, and the longer you leave it exposed to the action of ozone the darker it will become. Sulphurous acid gas uniting readily with the peroxide of manganese to form a colourless sulphate, the writing will instantly disappear when placed within air containing some of that acid; and it is a matter of course that the writing will come out again when again exposed to ozonized air. Now all this is certainly mere playing; but the matter is interesting in a scientific point of view, inasmuch as dry strips of white filtering paper drenched with a weak solution of sulphate of manganese furnish us with rather a delicate and specific test for ozone, by means of which we may easily prove the identity of chemical, voltaic and electrical ozone, and establish with facility and certainty the continual presence of ozone in the open air. I have turned brown my test-paper within the electrical brush, the ozonized oxygen obtained from electrolysed water and the atmospheric air ozonized by phosphorus. The quantity of ozone produced by the electrical brush being so very small, it requires of course some time to turn the test-paper brown.

As it is rather inconvenient to write with an invisible ink, I will stop here; not however before having asked your kind indulgence for the many blunders and faults which my ozone bottle will no doubt bring to light before long.

Yours most truly,

Bâle, July 1, 1847.

C. F. SCHENBEIN.

XXXII. *On the Decomposition of Water by Platinum and the Black Oxide of Iron at a white heat, with some observations on the theory of Mr. Grove's Experiments.* By GEORGE WILSON, M.D.*

THE remarkable discovery recently made public by Mr. Grove, that water in certain circumstances, when raised to a white heat, is resolved into its constituent gases, has na-

* Communicated by the Chemical Society; having been read March 15, 1847.

turally excited much attention. It furnished the unexpected confirmation of the truth of an opinion expressed by James Watt so far back as 1783, that if steam could be made red hot [white hot] so that all its latent heat should be converted into sensible heat, either the steam would be converted into permanent air, or some other change would take place in its constitution*.

In the greater number of Mr. Grove's experiments, water was raised in temperature through the medium of platinum; and it became a question accordingly, as Sir John Herschel and my friend Dr. Lyon Playfair suggested, how far the decomposition of water observed was owing to the mere heat of the metal, how far to the peculiar surface-influence, or so-called catalytic force, which has been so long recognized as possessed by platinum and the other noble metals. Dr. Playfair also referred to the fact, "that many bodies at high temperatures exhibited a great affinity for oxygen, which they did not possess at lower temperatures; as, for instance, silver, gold, and even platinum itself, which metals absorb oxygen when intensely heated, and give it out again on cooling. If the experiments had been tried in tubes of quartz or silica, they would not have been open to the objection which the use of so peculiar a metal as platinum appeared to involve†."

There was indeed one form of Mr. Grove's experiment not liable to the exception urged against those where platinum was used. He found it quite possible to decompose steam by sending Leyden-jar discharges through it, and refers the decomposition solely to the heat evolved by the electric spark. The same view has been suggested as not improbable by Faraday, in relation to the decomposition of water in the liquid form by electric discharges‡. With great diffidence, however, I would remark, that the spark decomposition of water cannot be regarded as an *experimentum crucis*. Although the electric spark cannot decompose steam electrolytically, we may not at once infer that it cannot decompose it in another way. I have no wish to assert that it can, but it is possible that it may, and a crucial experiment should be unexceptionable. Again: the spark discharge of a Leyden jar exerts a great disruptive force, and acts topically with much violence. There is reason moreover to believe that mechanical agitation or disturbance of a chemical compound can in many cases cause the separation of its elements. It may seem an extravagant idea to suppose that oxygen may be torn or detached from hydrogen by the action of a dis-

* Phil. Trans. 1783, p. 416.

† Athenæum for September 19th, 1846, p. 966.

‡ Researches in Electricity, 3rd series, paragraph 337.

ruptive force on the molecules of water, as if chemical affinity were but a kind of mechanical cohesion, which may be overcome by division. On the other hand, however, it must not be forgotten, that we are now acquainted with a large number of fulminating compounds, which can be decomposed by friction, by a touch, or a stroke. These compounds are all fragile, and water is a very stable combination; but fragility and stability are but terms of degree, in relation to stability of union: and if it shall appear that a feeble mechanical force can overcome a small intensity of affinity, it will be acknowledged as quite possible that a powerful mechanical agency may overcome a great one. We have no means perhaps of making an unexceptionable experiment as to the decomposing power of mechanical force; for we cannot bring it into play without calling into action other agencies. If we touch, or rub, or strike a fulminate, for example, we cause the evolution of heat, and add its decomposing power to that of the mechanical impulse. It would be a mere *petitio principii*, however, to assume that the heat produced alone effects the decomposition observed. It seems to me, therefore, that the decomposition of steam by the electric spark furnishes a more complex problem for solution than the action of white-hot platinum on the same compound does; and that the experiments made with the metal are more likely to throw light on those tried with the spark, than to be explained by them.

Whilst thinking over these difficulties, and the objections to Mr. Grove's conclusions suggested by Herschel and Playfair, I had occasion to perform the familiar class-experiment of burning iron wire in oxygen. I observed with an interest I had not felt previously, although I had carelessly noticed the phenomenon before, that bubbles of apparently permanent gas rose from the globules of white-hot oxide of iron as they fell into the water. It seemed to me possible that this gas might be a mixture of oxygen and hydrogen separated by the influence of the metallic oxide, acting as platinum did in Mr. Grove's experiments. It was certain, moreover, that if this should prove to be the case, it would supply a powerful argument in favour of that gentleman's conclusion, which seems, in spite of all the objections noticed, in the highest degree probable, namely, that heat, apart altogether from the medium through which it is applied, can resolve water into its elements.

As the following experiments were made solely in the hope of substantiating Mr. Grove's view, which unfortunately, however, they leave exactly as they found it, I trust that gentleman will not consider their publication an interference with his researches. I was led to try them incidentally, and

abandoned them as soon as I found I could render Mr. Grove no assistance by means of them.

It would be difficult to conceive a more rapid and effectual way of raising a body to a white heat than that afforded by the combustion of iron in oxygen. I took for granted also (as it afterwards appeared, too hastily) that the metal could not but be saturated with oxygen and converted into a definite oxide, which would be chemically indifferent to each of the elements of water, and if it decomposed it at all, would reject both its constituents. The convenient way, moreover, in which the globules of oxide detach themselves and fall into the water, and the rapidity with which the whole process goes on, make it a very easy matter to collect in considerable quantity whatever gases are evolved. A stoppered bottomless jar of the ordinary construction for the iron-wire experiment, and of 291 cubic inches' capacity, was made use of in the following trials. Eighteen experiments were made with it, and from 100 to 110 grains of fused globules were obtained from each combustion. A test-tube, with a funnel fixed into it by a perforated cork, and filled with water, was arranged so as to receive the gas. In some experiments it was placed within the oxygen jar, so that the coil of wire when introduced hung close to it, a piece of tin plate being arranged so as to guide the globules within the edge of the inverted funnel. In the greater number of trials however the tube and funnel were placed outside of the vessel containing the oxygen, and an inclined plane of tin plate was so placed as to carry the globules past the edge of the jar, and within the mouth of the funnel. No difference of result was observed in experiments made in both ways, but the latter arrangement was preferred as more convenient, and as enabling more oxygen to be employed at each trial.

In all the experiments, permanent gas was evolved when the fused globules fell into the water. This statement is to be considered as applying to each combustion considered as a whole; for individual globules were frequently observed to give off no gas at all, or to evolve so very little, that it might be air separating from the water, in which it had previously existed in solution. The quantity of gas obtained at each combustion varied greatly. Sometimes as much as a cubic inch was procured, more frequently only half that quantity, and occasionally less. The globules from thick coils of wire gave off a larger volume of gas than those from thin ones.

Portions of the gas were transferred to a Grove's eudiometer over water, and exposed to a white-hot platinum wire. They did not kindle or detonate, nor were they sensibly diminished in volume. Other portions were subjected to

electric sparks and discharges in a syphon eudiometer over water, with the same negative results; but when air or oxygen was mingled with the gas, it exploded sharply with heated platinum or the electric spark. When a match was applied to the open end of a tube containing the unmingled gas, it burned rapidly with a pale blue flame, but did not explode. The gas given off during the action of the fused globules on water was not then a mixture of oxygen and hydrogen.

Its freedom from all but a trace of oxygen was ascertained in other ways. To one portion of the gas standing over water nitric oxide was added, but no ruddy fume or yellow coloration showed itself. When phosphorus was introduced into the gas, in one instance it did not smoke, but in the greater number of cases it fumed for a brief period, and occasioned an amount of contraction barely perceptible. The gas appeared to be nearly pure hydrogen. To ascertain if it certainly were so, a portion of it was carefully dried, by chloride of calcium, and transferred to a eudiometer over warm mercury. Dry oxygen was then added, and the mixture exploded. When the whole had cooled, the walls of the eudiometer appeared dimmed by a very thin layer of moisture, but the quantity of gas operated on was too small to admit of visible drops being produced. Another portion of the gas was mixed with half its volume of oxygen and fired by the electric spark. The contraction which followed explosion varied in different experiments, but was frequently such as to leave not more than one-twentieth part of the mixed gases unconsumed. Phosphorus smoked in this residue for a short time, showing that excess of oxygen had been made use of, and left a minute volume of gas which was not diminished by caustic potash, and must have been nitrogen.

It seemed possible that the trace of carbon present even in malleable iron might affect the quality of the gas resulting from the action of the globules of oxide on water, and that carburetted hydrogen, carbonic oxide or carbonic acid might be produced. It seemed desirable to know whether the latter were present or not, as the oxygen might have gone to form them. It was impossible to be certain that carbonic acid was absent, for the gas from the globules being necessarily collected over water, the temperature of which was low, carbonic acid would be retained in solution by that liquid. All that I can say on this point is, that lime-water was not rendered muddy or in the slightest degree opalescent by the gas. It was several times detonated with oxygen over lime-water, but the latter remained quite transparent, so that neither carbonic oxide nor carburetted hydrogen can have been present. In short, the gas evolved from water by the white-

hot globules of oxide of iron, was hydrogen mingled with a small quantity of air, previously no doubt in solution in water.

As only the hydrogen, then, of the water decomposed was obtained, it became necessary to account for the absence of the oxygen. I was tempted for a moment to think it possible that the black oxide of iron might have changed into the red oxide of the same metal, by combining with the oxygen not obtained in the elastic form: *ex. gr.* thus $2 \text{Fe}_3 \text{O}_4 + \text{O} = 3 \text{Fe}_2 \text{O}_3$.

But the proto-peroxide of iron is known to be a very stable compound, little if at all prone to become the peroxide; and it seemed more likely that unoxidized iron might be present in the fused globules, which occasioned the evolution of hydrogen when it came in contact with water. To ascertain this point, portions of the globules were dissolved in dilute muriatic and sulphuric acids, and were found in most cases to evolve hydrogen. Some specimens of the globules gave off not a trace of gas when they dissolved, and must have consisted of the definite oxide; a point of interest in connection with the fact already mentioned, that globules were frequently observed to drop into water without any bubbles of gas rising from them.

The volume of hydrogen however given off in some of the trials, when the product of combustion was placed in acid, was very considerable. A graduated gas jar was filled with dilute sulphuric acid, and inverted over a small capsule containing 100 grains of the crushed globules, which was placed in a basin also containing dilute acid. By this arrangement the gas was collected and measured at the same time, without risk of mixing with air, or necessity for watching the process, which is a slow one. 100 grains treated in this way gave off 16 cubic inches of hydrogen, corresponding to 9 grains of iron. The experiment was accidentally stopped at this point whilst the gas was still rising in undiminished quantity.

Metallic iron, then, was certainly present in many of the globules, and of this I had direct ocular demonstration. On crushing some of them in a mortar, they were found to separate into a shell of pulverizable oxide, and a core of iron which formed a nearly spherical pellet. In one case 50 grains of the globules were crushed, the pellets separated, and the residue placed in diluted sulphuric acid. It did not evolve a trace of hydrogen in the course of twenty-four hours. The pellets were then added to the same acid, and gave off 12 cubic inches of gas = 13.6 per cent. of iron in the globules*. The shell of oxide is frequently imperfect or perforated, so

* In none of the experiments was the thermometer or barometer specially observed, as minute accuracy was not aimed at.

that water or any other liquid penetrates to the iron core, and is subject to its influence. When this becomes known, it need not surprise us that most of the globules should rapidly decompose water. After observing this fact, I tried the effect of thin and thick coils of wire, and found that the latter invariably gave off the greater volume of gas. When the coil is so thin that the metal all oxidizes, no gas is evolved at all. A thick coil indeed furnishes a striking mode of illustrating to a class the principle of Lavoisier's mode of decomposing water, and forms a beautiful addition to the iron-wire experiment.

From these observations then, it would seem that white-hot oxide of iron cannot decompose water in the way white-hot platinum does. But before any conclusion can be drawn from this fact inimical to Mr. Grove's views, or favourable to the opinion that a specific property of the platinum has more to do with the decomposition of water than its mere temperature has, we should require to know how far the two white-hot bodies are to be considered as at the same temperature. In Mr. Grove's experiments, platinum is raised to as high a heat as it can bear without fusing. It must then be elevated to a temperature much above that necessary to make iron white hot, or to fuse its oxide, for our forges can melt iron and its oxides, but do not fuse platinum. It may also be remarked, that bright as the light emitted by burning iron is, it falls short in intensity of that given off by platinum on the verge of fusion. It seems accordingly probable, that during the combustion of iron in oxygen the temperature never rises high enough to confer upon the resulting oxide the power of decomposing water. The question admits of direct decision, by ascertaining whether oxide of iron, heated by the oxy-hydrogen blowpipe to as high a temperature as fusing platinum, acquires the power of decomposing water without appropriating to itself either of its elements. But it would have been an interference with Mr. Grove's own researches to have made experiments of this kind, and I have accordingly left the question undecided.

Meanwhile the experiments I have recorded are of some little interest, as at least showing that not only a white heat, but a high white heat, is essential to the successful performance of Mr. Grove's experiments. Unfortunately, we have not at present any method of measuring high temperatures which admits of ready application or secures great accuracy. "White heat" is in fact a vague expression for a range of temperature, of the extremes in either direction or extent of which we have no very precise knowledge. The power of the eye to measure the relative intensities of the light evolved

by white-hot bodies is very limited, and varies greatly in different individuals. But the experiments I have recorded seem to supply the means of so far at least defining the white heat requisite for the separation of the elements of water, inasmuch as they show that it must at least exceed the temperature necessary for the fusion of malleable iron or its black oxide. If, moreover, the decomposing powers of the electric spark be solely referable to its temperature, we seem entitled to conclude, from the experiments I have detailed, that the heat of the smallest spark that can decompose water is at least equivalent to that of fusing platinum. They appear also to warrant another conclusion. It was suggested by Dr. Leeson and by Mr. Hunt, that the bursting of steam-boilers might occasionally be owing to the metal they consist of becoming white-hot and decomposing water like platinum, with the rejection of both its elements*. This ingenious suggestion seemed to myself, before making experiments with iron, likely to prove just; but as fusing white-hot iron appears unable to decompose water, otherwise than by combining with its oxygen, it is impossible that the walls of a boiler can ever be raised to a temperature sufficiently high to enable them to separate the elements of water in the way platinum does.

I may now be permitted to make some comments on the rationale of the results obtained by Mr. Grove. That gentleman, if I understand him aright, considers the decomposition of water by white-hot platinum not only, as assuredly it is, a remarkable and unexpected result, but as evidencing on the part of heat a power to produce opposite or dissimilar chemical effects in the same circumstances. He is reported in the *Athenæum* (Sept. 19th, 1846, p. 966) to have "announced his discovery that all the processes by which water may be formed are capable of decomposing water" (p. 966). If by this statement be simply meant, that heat combines oxygen and hydrogen into water, and decomposes water into these gases, it will be admitted to be a just conclusion; but it may be questioned, I think, whether Mr. Grove's experiments add anything to our knowledge of the power of heat to effect chemical changes, except in so far as they supply an additional very remarkable example of its twofold analytical and synthetical agency, which has been so long recognised. Hydrogen, which as a gas is probably the vapour of a very volatile metal, may be compared with mercury, also a volatile substance. If mercury and oxygen be heated together to the temperature of 662° F., they combine and form the red oxide of the metal. If this resulting oxide be raised to a low red heat,

* *Athenæum*, Sept. 19th, p. 966.

it is decomposed into mercury and oxygen. In like manner, if hydrogen and oxygen be raised together to the temperature of 660° F.*, they unite and form water. If the resulting water be raised to a white heat, it is resolved into hydrogen and oxygen. Both metals (?) present the same phenomena. At one temperature (nearly the same in both cases) combination with oxygen occurs; at a higher temperature, decomposition of the oxide happens. Many other examples might be given in illustration of the same fact. Such cases, however, do not seem to warrant a conclusion as to heat exhibiting anything like a polarity of force, by which I understand the manifestation in opposite directions of opposite powers of equal intensity. At all events, if the opposite effects of different *intensities* of the same agent be considered equivalent to a polarity of action, it is difficult to see what force may not be called a polar one. The decomposing and combining power of heat of different intensities, seems exactly comparable to the opposite effects of different intensities of mechanical impulse.

If two pieces of smooth glass are laid together and struck gently or compressed slightly, they unite or cohere. If the united pieces are thereafter exposed to a sharp blow or to great compression, the union is dissolved, or they are shattered to fragments. Here the same force effects mechanical synthesis and mechanical analysis. But in these contrasted actions, as seems to be the case also in Mr. Grove's experiments, the results are occasioned by a difference in degree of intensity of the same power, not as in the opposite effects of a polarizing force like electricity, by a difference in the kind of power which appears, whatever be its intensity. There is one form, indeed, of Mr. Grove's experiment which at first sight does not appear to admit of the explanation proposed in reference to the other trials—I allude to the decomposition of steam by the electric spark, which is well known to have the power of combining hydrogen and oxygen into water. A similar experiment was made in perhaps a still more instructive form in the latter part of last century by Beccaria†, Pearson and Van Troostwyk, and more recently by Wollaston‡, in his well-known decompositions of water with guarded poles. In certain of these trials it was found that Leyden jar discharges sent through water, decomposed it till the accumulation of permanent gas left the wires bare; after which the first spark that passed recombined the gases into water, which again covered the wire, when decomposition could

* Graham's Elements, 1st edit. p. 259.

† Lettere dell' *Elettrecismo*, quoted in Lardner's Electricity, vol. i. p. 78.

‡ Faraday's Electrical Researches, series 3, paragraph 328.

anew be obtained. Here, to appearance, the same agent acting with the same intensity, alternately decomposed and re-composed water. For argument's sake, let it be acknowledged that the heat alone of the spark was the cause of chemical change. Nevertheless it may be questioned, whether it acted with equal intensity in both cases. The electric spark must be conceived, according to the results already given, to be at first at a high white heat, and whilst retaining this temperature we may believe it to possess a power of disuniting the elements of water, and of preventing their union. But as soon as the spark falls to the temperature of 660° F., it loses its power of decomposing water, and, on the other hand, acquires a power of uniting hydrogen and oxygen. Although therefore the spark is always *furnished* of the same intensity, its action may change, and even be reversed, as its intensity diminishes. Moreover, even when the spark is white-hot, it is only the amount of matter directly in its track that will be raised to a white heat. Contiguous portions will have their temperature much lower, so that in the case of hydrogen and oxygen, at some little distance from the route of the spark, the temperature will be 660° F., and there combination will begin, and ultimately extend through the whole mass of gas.

In like manner, when a platinum wire is made white-hot in a mixture of hydrogen and oxygen, it causes their combination. Here we may suppose that union occurs as soon as the temperature of the metal rises to 660° F., and before it acquires a white heat. Or if we were to arrange matters so that the wire should be made white-hot in a vacuum and hydrogen, and oxygen afterwards admitted to it, still union of the gases should happen; for although the wire might prevent combination immediately around itself, at no great distance where the temperature was below 700° F. it would compel union. In all such experiments the combining effect of heat will be much more manifest than its decomposing power; not that perhaps the former is in reality greater than the latter, but because flame is propagated through a mixture of hydrogen and oxygen by a series of combustions. The hot wire or the electric spark kindles only the portions of gas immediately adjacent to it, but the combustion of those sets fire to the molecules contiguous to them, and these in their turn to their neighbours, till all are made to burn. Thus the flame travels after the original cause of combustion has ceased to operate directly, and the momentary action of a small spark, or the transient heat of a red-hot capillary wire may suffice to fire an infinitely large mass of hydrogen and oxygen. There is no provision for a similar propagation of decomposition through water or steam when either is made white-hot; the

absolute amount accordingly of disunion of the elements of water occasioned is very small.

If allowance, however, be made for the apparent difference in extent of effect which heat shows in uniting and in disuniting the elements of water, the phenomena otherwise seem referable solely to the intensity of the temperature to which hydrogen and oxygen are exposed. The opposite processes might go on simultaneously, union or disunion being determined simply by the different temperatures to which different portions of the gases were raised. At least it seems not improbable that if a mixture of steam and of hydrogen and oxygen were exposed to electric discharge, decomposition of the steam and combination of the hydrogen and oxygen might be effected by the same spark, provided the volume of steam were not large. In the track of the spark decomposition would occur, so long as a white heat prevailed. When the temperature fell, combination would happen where the spark had passed, if it had not already commenced in the neighbourhood of its direct route. Similar remarks apply *mutatis mutandis* to the action of a hot platinum wire on a mixture of steam with oxygen and hydrogen.

It may be objected to this view, that Mr. Grove decomposes steam in his eudiometer, and obtains a permanent bubble of gas, consisting of hydrogen and oxygen. The bubble however obtained in this way is very small, and could not probably be greatly increased. Mr. Grove has not mentioned how large a volume of hydrogen and oxygen he could obtain in the same eudiometer, by alternately boiling the water till the steam produced caused the liquid to fall below the wire, and allowing the steam to condense till the water rose above the metal. But I venture to say that no large volume of permanent gas could be procured by this process if the same eudiometer were employed many times successively. The combining action of the wire might not take effect on the hydrogen and oxygen when their quantity was small, and they were diluted through a large volume of steam, for in virtue of the law of diffusion, the molecules of hydrogen and oxygen would be separated from each other by molecules of water-vapour; but when the latter diminished in bulk, it seems impossible to doubt that kindling of the gases would occur.

Mr. Grove's experiments then do not appear to prove that heat of the same intensity is able in the same circumstances to form water and to decompose it. When therefore it is stated that water can be produced by the processes that disunite its elements, the word 'process' can only be understood to signify that the general arrangement in both cases is the

same, not that the intensity of the agent called into play, or its mode of action is identical. If this could be affirmed, we should be able to announce as a general proposition, that manifestations of the same force absolutely identical as to quality, quantity and intensity, could produce totally opposite results, which would be tantamount to affirming that unlike effects may flow from the same cause, without any alteration in the qualities or conditions of the latter.

The last observation I would make refers to the curious fact noticed by Mr. Grove, namely, that when a platinum wire is heated white-hot in steam, "in a few seconds a small bubble of gas is formed; but if the action be continued for a week, it does not increase in quantity*."

Are we to suppose that the wire is at the same time decomposing water around itself, and producing water at a little distance, undoing in one place what it effects in another, so that no permanent accumulation of gas is allowed to take place? This is possible, but I think not likely. The observation made by Mr. Grove seems sufficiently explicable, on the supposition that as soon as the wire is completely enveloped in steam, the thermo-circulatory currents which the high temperature occasions in the vapour prevent it from remaining long enough in contact with the wire to become heated white-hot. The steam probably circulates endlessly around the wire without a trace of decomposition occurring in it. It seems not unlikely indeed that in Mr. Grove's experiments with his eudiometer it was not steam that yielded the hydrogen and oxygen obtained, but the last film of water below the wire, which could not escape from the metal, but tended rather, in consequence of its expansion, to rise towards it, and was thus compelled to acquire a white heat, and to break up into its elements. If this view be correct, an arrangement where a white-hot wire or sheet of platinum foil was kept grazing the surface of water, might be found to effect a continuous decomposition of the liquid in question.

It is no objection to this view that an electric spark decomposes steam readily, for the duration of the spark is so short, that there is no time for the production of thermo-currents, nor any possibility of the steam escaping from the powerful topical action of the discharge. The spark may be compared to fulminating silver, whose action is instantaneous and violent, but quite local,—the heated platinum to gunpowder, the effect of which is cumulative and more general.

* *Athenæum*, Sept. 19th, 1846, p. 966.