V. *On the Effect of surrounding Media on Voltaic Ignition.*

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In the Philosophical Magazine for December 1845, I pointed out a striking difference between the heat generated in a platinum wire by a voltaic current, according as the wire is immersed in atmospheric air or in hydrogen gas, and in the Bakerian Lecture for 1847 I have given some further experiments on this subject, in which the wire was ignited in atmospheres of various gases, while a voltameter enclosed in the circuit yielded an amount of gas in some inverse ratio to the heat developed in the wire. It was also shown, by a thermometer placed at a given distance, that the radiated heat was in a direct ratio with the visible heat.

Although the phenomenon was apparently abnormal, there were many known physical agencies by which it might possibly be explained, such as the different specific heats of the surrounding media, their different conducting powers for electricity, or the varying fluency or mobility of their particles which would carry off the heat by molecular currents with different degrees of rapidity.

The investigation of these questions will form the subject of this paper.

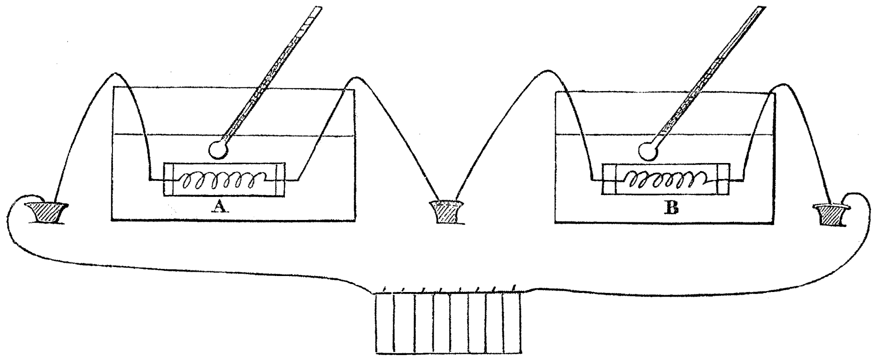
An apparatus was arranged, see fig. 1. Two glass tubes A and B, of 0·3 inch internal diameter and 1·5 inch length, were closed with corks at each extremity; through the corks the ends of copper wires penetrated, and joining these were coils of fine platinum wire, one-eightieth of an inch diameter and 3·7 inches long when uncoiled. Tube A was filled with oxygen, tube B with hydrogen, and the tubes thus prepared were immersed in two separate vessels, in all respects similar to each other, and containing each three ounces of water. A thermometer was placed in the water in each vessel; the copper wires were connected, so as to form a continued circuit, with a nitric acid battery of eight cells, each plate exposing eight square inches of surface. Upon the circuit being completed the wire in the tube containing oxygen rose to a white heat, while that in the hydrogen was not visibly ignited; the temperature of the water, which at the commencement of the experiment was 60° Fahr. in each vessel, rose in five minutes in the water surrounding the tube of hydrogen from 60° to 70°, and in that containing oxygen from 60° to 81°*.

Before I enter into a further detail of experiments, I would remark upon the extraordinary character of this result. The same current or quantity of electricity

* After the publication of the Bakerian Lecture, my experiment on the peculiar effect of hydrogen on the ignited wire was noticed in a paper by M. Matteucci, which though I had it in my hand shortly after its

passes through two similar portions of wire immersed in the same quantity of liquid,

Fig. 1.



and yet, in consequence of their being surrounded by a thin envelope of different gases, a large portion of the heat which is developed in the one portion appears to have been annihilated in the other. Similar experiments, varying the gas in one tube while hydrogen was retained in the other, gave the following results. In five minutes the thermometer rose—

1st.	In the hydrogen. From 60° to 69°·5.	In the associated nitrogen. From 60° to 81°·5.
2nd.	In hydrogen. From 60° to 70°·5.	In carbonic acid. From 60° to 80°.
3rd.	In hydrogen. From 60° to 70°.	In carbonic oxide. From 60° to 79°·5.
4th.	In hydrogen. From 60° to 70°·5.	In olefiant gas. From 60° to 76°·5*.

On a different day I tried the following experiments; all the circumstances were the same, excepting that the battery was in more energetic action, for which reason I have not tabulated them with the others.

publication, I regret to say I did not read with the attention it deserved. I have read it since the experiments in this paper were commenced, and I see that I am now executing a task assigned to me by my friend. M. Matteucci, for a different object, makes a somewhat similar experiment to the one given above, which however differs from mine in the material point, that he operated first on one gas and then on the other, and thus did not compare the effects produced by the same quantity of electricity. I cannot quite agree in the conclusions deduced by him from this and the other experiments he cites, but I will not here contest them, as it would lead me away from the main point of this paper.

* I should perhaps remark, that several test experiments were tried to ascertain the working of the apparatus; thus, the same gas was placed in both tubes, and the results given by the thermometer were found to be accurately the same in both vessels. The tubes were also changed with reference to the containing vessels and to the contained gases. The water was always agitated to render its temperature uniform previously to reading off, &c. &c.

In oxygen associated with coal gas the thermometer rose in five minutes—

In oxygen.	In coal gas.
From 60° to 82°.	From 60° to 76°.

In hydrogen associated with coal gas the thermometer rose in five minutes—

In hydrogen.	In coal gas.
From 60° to 77°.	From 60° to 82·5°.

From this it would appear that coal gas should be placed, as to its cooling effect on the ignited wire, between hydrogen and olefiant gas.

On another day sulphuretted hydrogen associated respectively with oxygen and hydrogen was tried; the wire in the sulphuretted hydrogen was at first ignited to a degree somewhat inferior to that in oxygen, but the gas was rapidly decomposed; sulphur being deposited on the interior of the vessel and the intensity of ignition gradually decreased, so as ultimately to be scarcely superior to the ignition in hydrogen: indeed the gas by this time had become nearly pure hydrogen. The following were the effects on the thermometer in five minutes, all being arranged as before.

In oxygen.	In sulphuretted hydrogen.
From 60° to 86°.	From 60° to 76°.

In hydrogen.	In sulphuretted hydrogen.
From 60° to 79°.	From 60° to 81°·5.

This result would place sulphuretted hydrogen between hydrogen and coal gas; but as the gas was rapidly decomposed, the greater part of the experiment was made with hydrogen containing small quantities of sulphur combined, and not with sulphuretted hydrogen. I therefore think that proto-sulphuret of hydrogen, or the gas which consists of equivalent ratios of the two elements, would be much further removed from pure hydrogen; probably it would be about equal in its cooling effect to carbonic acid or carbonic oxide.

In phosphuretted hydrogen the platinum wire is destroyed by combining with the phosphorus the instant it reaches ignition, so that its relation to the other gases could not be ascertained.

Protoxide and deutoxide of nitrogen are, as I have observed in the Bakerian Lecture, decomposed by the ignited wire; they, as well as atmospheric air, are, as nearly as may be, equal in their effect to their elements separately.

In the vapour of ether the ignited wire is extinguished nearly as completely as in hydrogen; I have not yet tried its comparative effect, but should judge it to be nearly the same as coal gas or olefiant gas.

In my former experiments* the following was the order of the gases, testing the intensity of ignition by the inverse conducting power of the wire, as measured by the amount of gas in a voltameter included in the circuit.

* Philosophical Transactions, 1847, p. 2.

Gases surrounding the wire.	Cubic inches of gas evolved in the voltameter per minute.
Hydrogen	7·7
Olefiant gas	7·0
Carbonic oxide	6·6
Carbonic acid	6·6
Oxygen	6·5
Nitrogen	6·4

Assuming that in the present experiments the heat in the water is a correct indication of the intensity of ignition in the wire, the order is the same in both series of experiments. Hydrogen is however so far removed from both oxygen and nitrogen in its effects upon the ignited wire, that in order more accurately to ascertain the relative position of the latter two gases, I made a few further experiments on them as contrasted with each other, and not with hydrogen. I first repeated my former experiment on these two gases, varying it only by changing the circumstances in the manner suggested by the present experiments, which on account of the vessel containing the wire being immersed in a given quantity of water, instead of being exposed to the external atmosphere, would occasion greater equality in the surrounding cooling effects, and would give me the opportunity of combining both methods in one experiment.

I filled both tubes A and B with oxygen, and included a voltameter in the circuit; in two minutes 3·43 cubic inches of hydrogen were evolved in the voltameter, and the thermometer in each cell had risen from 60° to 63°. A similar experiment with nitrogen gave in two minutes 3·4 cubic inches of hydrogen, and the thermometer rose from 60° to 63°.

This experiment accords with my previous one as to the voltameter test, but indicates no difference in oxygen and nitrogen with the thermometer test; I therefore in the following three experiments associated nitrogen with oxygen in the apparatus, fig. 1. All things being disposed as with the experiments on hydrogen associated with other gases, in five minutes the thermometer rose—

	In the oxygen.	In the associated nitrogen.
Exp. 1st.	From 60° to 71°·5.	From 60° to 73°.
2nd.	60° to 77°.	60° to 76°.
3rd.	60° to 75°.	60° to 76°.
Mean . .	60° to 74°·5.	60° to 75°.

The battery had increased somewhat in power after the first experiment, but as both wires formed part of the same circuit in each experiment, the variations in battery power do not affect the comparative results. The second experiment gives a variation in the position of oxygen and nitrogen with reference to the first and third experiments, but the gases so nearly approach in their cooling effects, that these slight differences are not much to be relied upon; however I applied a further test. I associated in turn oxygen and nitrogen with carbonic acid; the following were the results. In five minutes the thermometer rose—

	In oxygen.	In carbonic acid.
Exp. 1st.	From 60° to 75°.	From 60° to 75°.
2nd.	60° to 76°.	60° to 75°.

	In nitrogen.	In carbonic acid.
Exp. 1st.	From 60° to 74°.	From 60° to 73°.
2nd	60° to 73°.	60° to 72°·5.

The battery had in the last experiment a little decreased in power; the oxygen and nitrogen both produced a less cooling effect than the carbonic acid, but the oxygen came nearer to it than the nitrogen, thus according with the previous experiments. Upon the whole it would appear that oxygen produces a somewhat greater cooling effect on the ignited wire than nitrogen, but these gases may, for the purposes of this paper, be fairly regarded as equal. Atmospheric air produces a similar effect to oxygen and nitrogen separately, though I am inclined to think that a slight chemical change takes place when atmospheric air is exposed to the ignited wire, and that nitrous acid is formed; for if litmus paper be held over a voltaically ignited platinum wire in the air, a slight but very perceptible tinge of red marks the portion of it immediately over the wire.

With the view of ascertaining whether the specific heat of the surrounding media were the cause of the phenomenon, I proceeded to try the effect of the wire carrying a voltaic current on different liquids; all things being disposed as in the previous experiments, and three ounces of water being associated respectively with the same quantity of the following liquids. The thermometer rose in five minutes—

In water, from 60° to 70°·3.	In spirit of turpentine.	60° to 88°.
In water, from 60° to 70°·3.	In sulphuret of carbon	60° to 87°·1.
In water, from 60° to 69°.	In olive oil	60° to 85°.
In water, from 60° to 70°·1.	In naphtha	60° to 78°·8.
In water, from 60° to 70°·5.	In alcohol sp. gr. 0·84	60° to 77°.
In water, from 60° to 68°·5.	In ether	60° to 76°·1.

I do not much rely on the last experiment,—the battery was in more feeble action; and though each of the above results is the mean of three experiments, yet the variations in the results of the different experiments with ether being considerable (while in the others they were very trifling), lead me to place no great dependence on it. The rapidity of evaporation and the readiness of ebullition of the ether require that a larger quantity should be used; but as this for the purpose of comparison would have required all the experiments to be repeated with different quantities of liquid, I have not thought it worth while to go through the series a second time. It will be observed, that the effects with the above liquids are by no means in direct relation with their respective specific heats; but in order to bring the results of the experiments with liquids into comparison with those with gases, I now associated a gas with a liquid, viz. hydrogen with water. All things being disposed as before, the tube A was filled with hydrogen gas, the tube B with water, both being immersed in three ounces of water. The thermometer rose in five minutes—

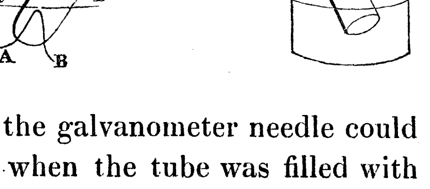
In hydrogen.	In water.
From 60° to 75°·5.	From 60° to 72°

This experiment of itself conclusively negatives the possibility of specific heat alone accounting for the phenomenon under consideration; and though, doubtless, specific heat must have some influence on the cooling effects of different gases and liquids, yet in the former it is apparently of very trifling import in comparison with the real physical cause of the differences, whatever that may be.

Supposing, as is stated by Faraday*, that gases possess feeble conducting powers for voltaic electricity, and supposing hydrogen, from its close analogy in chemical character to the metals, to possess a greater conducting power than the other gases, this would account for its peculiar effect on the ignited wire, as a certain portion of the current, instead of forcing its way through the wire, would be carried off by the surrounding gas. In order to ascertain this I arranged the following experiments.

1st. Into the closed end of a bent tube, fig. 2, a loop of platinum wire, A B, and two separate platinum wires C D, were hermetically sealed, the extremities of the latter being approximated as closely as possible, and the interval between them being close to and immediately over the apex of the loop. The tube was filled with hydrogen, and the wire A B connected with a voltaic battery of sufficient power to raise it to as high a degree of ignition as it would bear without fusion; C and D were now connected with the poles of another battery, a delicate galvanometer being interposed in the circuit. Not the slightest effect on the galvanometer needle could be detected, and a similar negative effect took place when the tube was filled with atmospheric air.

Fig. 2.

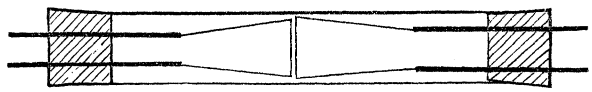


2nd. Parallel portions of platinum wire were now arranged in close proximity (see fig. 3.), and so that each might be ignited to a full incandescence by separate insulated batteries. When surrounded by atmospheres, both of atmospheric air and of hydrogen and fully ignited, not the slightest conduction could be detected, across the interval between the wires, with ten cells of the nitric acid battery, and being enabled

* Experimental Researches, §§ 272, 441 and 444.

by the kindness of Mr. GASSIOT to repeat this experiment with his battery of 500 well-insulated cells of the nitric acid combination, air did not conduct when the ignited wires were approximated to the $\frac{1}{50}$th of an inch; on approaching them nearer they came within striking distance, were instantly fused, and the galvanometer needle, which had up to this time been perfectly stationary, was whirled rapidly round.

Fig. 3.



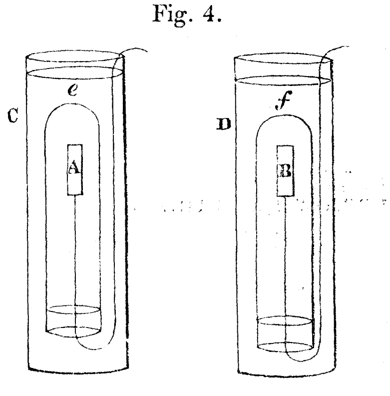
I think I am entitled to conclude from this, that we have no experimental evidence that matter in the gaseous state conducts voltaic electricity; probably gases do not conduct Franklinic electricity, as the experiments which would seem *primâ facie* to lead to that conclusion, are explicable as resulting from the disruptive discharge.

In FARADAY's experiment two wires were approximated in the flame of a spirit-lamp, and a slight conduction across the interval in the flame was observed. This conduction might have been due to certain unconsumed particles of carbon existing in the flame, or possibly to the flame itself; according to Dr. ANDREWS, flame, even that of pure hydrogen gas, conducts voltaic electricity*.

I now endeavoured to ascertain whether any specific inductive effect of the hydrogen might have an influence: parallel wires of platinum and parallel coiled copper wires were placed in atmospheres of hydrogen and of atmospheric air, one of which parallel wires conveyed the current, and the other wire was connected with a delicate galvanometer. I could detect no difference in the arcs of deflection of the needle at the instant of meeting or breaking contact, whether the wires were in atmospheres of hydrogen or of atmospheric air; nor when parallel platinum wires with their surrounding atmospheres of gas were immersed in a given quantity of water, could I detect any difference in the resulting heat, whether the current passed in the same or in a different direction through each wire.

My next object was to ascertain whether, in cases of ordinary ignition, the same apparent annihilation of heat took place in hydrogen gas as with voltaic ignition. Two iron cylinders A B, fig. 4, each weighing 390 grains, were attached to long iron wires bent back in the form shown in the figure. The cylinders were placed together in a crucible of fine sand, and the whole heated to an uniform white heat. The cylinders were now taken out of the sand, placed at the surface of equal portions of water in the vessels C and D; two inverted tubes *e*, *f*, the one of hydrogen, the other of atmospheric air, were placed over them, and the whole quickly immersed in the water, and retained by a little contrivance, which I need not particularize, in the position shown in the figure. The

Fig. 4.



* Philosophical Magazine, vol. ix. p. 176.

temperature of the water at the commencement of the experiment was 60° FAHR. In four minutes the water surrounding the hydrogen had risen to 94°, and became stationary there, while that surrounding the air had only reached 87°; in ten minutes the water surrounding the hydrogen had sunk to 92°·5, while that surrounding the air had risen to 93°, which was the highest temperature it reached; thus the respective maxima were 94° and 93°; but considering the greater time which the water surrounding the air required to attain its maximum temperature, and that being during this time at a temperature above that of the surrounding atmosphere, it must have lost something of its acquired heat, we may fairly consider the maxima to be the same, and that the difference of effect in the two gases had reference solely to the time occupied in the transference of the heat. In a second experiment the results were similar, the maximum being in this experiment 92·5 in hydrogen, and 91 in air*.

As far as ordinary ignition is concerned, hydrogen has been shown by the experiments of LESLIE and DAVY to produce a more rapid cooling effect than air; and the above experiment having shown that it does not alter or convert into any other force the actual amount of heat given off, my next step was to inquire whether this rapidity of cooling effect of the hydrogen would account for the effects observed with voltaic ignition. Although the two classes of effects were apparently very different, it might be that the improved power of conduction arising from the rapid cooling effect of the hydrogen might, by enabling the current to pass more readily, carry off the force in the form of electricity, which if the wire offered more resistance (as it would when more highly ignited) would be developed in the form of heat. By employing the same medium, but impeding the circulation of the heated currents in one case, while their circulation was free in the other, some light might be expected to be thrown on the inverse relation of the conducting power to the heat developed. The following experiment was therefore tried.

In the apparatus represented in fig. 1, tube A was uncorked, so as to allow free passage for the water, while tube B was filled up with fine sand soaked with water, and then corked at both ends; the current was passed and the following was the result. In the vessel containing tube A, the thermometer rose in five minutes from 52° to 60°, and in that containing tube B from 52° to 60° also; during a second five minutes, the thermometer rose in the vessel containing A from 60° to 67°, and in the vessel containing B from 60° to 67° also.

I tried another analogous experiment: a coil of platinum wire was placed in a very narrow glass tube one-sixth of an inch diameter; this was hermetically sealed at one end, and the other drawn into a very narrow aperture, little more than sufficient to allow the platinum wire to pass, and filled with water (it was necessary to leave a small aperture to prevent the bursting of the tube by the expansion of the heated water); in the other vessel a similar coil of platinum wire was placed, but without

* Iron wire produces a similar effect to platinum wire in the voltaic experiments.

any glass tube at all. The circuit having been completed as before, the thermometer rose in five minutes—

In the water without the tube, from . . . . 60° to 87°.
In the water containing the tube, from . . . . . . 60° to 86°.

Here the difference, slight as it was, was against what theory would have led one to anticipate; the exact equality however of the previous experiment, and the close approximation of the results in this one, afford no conclusive information as to the point under consideration, though the negative result rather tends against the view which would assimilate the effects of voltaic to those of ordinary ignition.

As another method of attaining the object before mentioned, viz. the inverse relation of the conducting power of the wire to the heat developed in it, I tried the following experiment. A platinum wire of one foot long and $\frac{1}{80}$th of an inch diameter was ignited in air by ten cells of the battery, a voltameter being included in the circuit; the amount of hydrogen given off by the voltameter was one cubic inch in forty-four seconds: half the wire was now immersed in water of the temperature of 60° Fahr.; by this means the intensity of ignition of the other half was notably increased; the voltameter now yielded one cubic inch in forty seconds: two-thirds of the wire immersed, gave one cubic inch in thirty-seven seconds; and five-sixths immersed, gave one cubic inch in thirty-five seconds. The heat of the portion of wire not immersed in water had in the last experiment nearly reached the point of fusion of the platinum. By this result it appears that the increased resistance to conduction of the ignited portion is not equal to the increased conducting power of the cooled portion of the same wire.

With a view of seeing how far the cooling effect upon the ignited wire might be due to the greater or less fluency or mobility of the particles of the different media surrounding it, I have looked into the papers of Faraday* and of Graham†. In the experiments of the former, it appears that the escape of different gases at a certain pressure through capillary tubes, or the velocities of revolution of vanes or floats surrounded by different gases, was in some inverse ratio to the density of such gases; and the experiments of the latter show that the effusion or escape of gases through a minute aperture in a plate, takes place with velocities inversely as the square root of their specific gravities. In Graham's experiments, however, when the escape took place through capillary tubes, the results seemed subject to no ascertained law, though the compounds of carbon with hydrogen passed through with greater facility than other gases.

The cooling effects of gases on the ignited wire are decidedly not in any ratio with their specific gravities; thus, carbonic acid on the one hand, and hydrogen on the other, produce greater cooling effects than atmospheric air; and olefiant gas, which closely approximates air, and is far removed from hydrogen in specific gravity, much more nearly approximates hydrogen, and is far removed from air in its cooling effect.

* Quarterly Journal of Science, vol. iii. p. 354.

† Philosophical Transactions, 1846, p. 573.

Upon the whole, we may conclude, from the experiments detailed in this paper, that the cooling effect of different gases, or rather the difference in the cooling effect of hydrogen and its compounds from that of other gases, is not due to differences of specific heat; it is not due to differences of specific gravity; it is not due to differences of conducting powers for electricity; it is not due to the character of hydrogen in relation to its transmission of sound, noticed by LESLIE, for reasons which I have before given*; it is not due to the same physical characters of mobility which occasion one gas to escape from a small aperture with greater facility than another; but it may be, and probably is, affected by the mobile or vibratory character of the particles by which heat is more rapidly abstracted. I at one time thought that the effect might have relation to the combustible character of the gas, and that the electro-negative gases were in respect to it contra-distinguished from the electro-positive or neutral gases, but the experience I have obtained from the experiments detailed here induces me to abandon that supposition.

I incline to think, that, although influenced by the fluency of the gas, the phenomenon is mainly due to a molecular action at the surfaces of the ignited body and of the gas. We know that in the recognised effects of radiant heat, the physical state of the surface of the radiating or absorbing body exercises a most important influence on the relative velocities of radiation or absorption; thus, black and white surfaces are, as every one knows, strikingly contra-distinguished in this respect: why may not the surface of the gaseous medium contiguous to the radiating substance exercise a reciprocal influence? why may not the surface of hydrogen be as black, and that of nitrogen as white to the ignited wire? This notion seems to me the more worthy of consideration as it may establish a link of continuity between the cooling effects of different gaseous media and the mysterious effects of surface in catalytic combinations and decompositions by solids such as platinum. Epipolic actions will, I feel convinced, gradually assume a much more important place in physics than they have hitherto done; and the further development of them appears to me the most probable guide to the connection by definite conceptions of physical and chemical actions.

The difference of the cooling effect of hydrogen, and of those of its compounds, where it is not neutralized by a powerful electro-negative gas, from all other gases, is perhaps the most striking peculiarity of the phenomena I have described. The differences of effect of all gases other than hydrogen and such compounds are quite insignificant when compared with the differences between the hydrogenous and the other gases. There are some phenomena which I have before observed, and which were, at the time I noticed them, inexplicable to me; but they now appear dependent on this physical peculiarity of hydrogen. Thus, if a jet of oxygen gas be kindled in an atmosphere of carburetted hydrogen, the flame is smaller than when the converse effect takes place. The voltaic arc between metallic terminals is also much

* Philosophical Transactions, 1847.

smaller in hydrogen gas than in nitrogen, though both these gases are incapable of combining with the terminals; indeed to obtain an arc at all in hydrogen is scarcely practicable.

DAVY has, in his Researches on Flame, given several experiments which are similarly explicable; but though noting the results, he nowhere, as far as I am aware, attributes them to any specific peculiarity of hydrogen.

Of the phenomenon which I have examined in this paper, I first published an account in connection with some experiments on the application of voltaic ignition to lighting mines, and it does not appear impossible that the experiments now detailed may ultimately find some beneficial application in solving the problem of a safety-light for mines. A light which is just able to support itself under the cooling effect of ordinary atmospheric air would be extinguished by air mixed with hydrogenous gas.

I am far from pretending to have devised any means of fulfilling these conditions, and yet supplying an efficient light; I merely throw it out as a suggestion for consideration, knowing that there are no additions to our knowledge which are not ultimately valuable in their practical application; and that a suggestion, however vague,—a new point to those whose minds may be occupied with the subject, may lead them to results which he who makes the suggestion is unable to attain.

P.S. Since this paper was communicated I have received a paper from Dr. ANDREWS of Belfast, who published as early as 1840, in the Proceedings of the Royal Irish Academy, experiments similar to those of mine first published in 1845. My experiments were made in the same year as those of Dr. ANDREWS, but as I withheld their publication, Dr. ANDREWS is fully entitled to priority. Had I known of his experiments earlier, I should have recited them in the first part of this paper.