VII. *Some researches on flame. By Sir* Humphry Davy, *LL.D. F.R.S. V.P.R.I.*

Read January 16, 1817.

I have described in three papers which the Royal Society have honoured with a place in their Transactions, a number of experiments on combustion which show that the explosion of gaseous mixtures can be prevented or arrested by various cooling influences, and which led me to discover a tissue permeable to light and air, but impermeable to flame, on which I founded the invention of the wire gauze safe lamp now generally used in all collieries in which inflammable air prevails, for the preservation of the lives and persons of the miners In a short notice published in the third number of the Journal of Science and the Arts, edited at the Royal Institution, I have given an account of some new results on flame, which show that the intensity of the light of flames depends principally upon the production and ignition of solid matter in combustion, and that the heat and light in this process are in a great measure independent phenomena. Since this notice has been printed, I have made a number of researches on flame: and as they appear to me to throw some new lights on this important subject, and to lead to some practical views connected with the useful arts, I shall without any farther apology, present them to the Royal Society.

That greater distinctness may exist in the details, I shall treat of my subjects under four heads. In the first I shall discuss the effects of rarefaction, by partly removing the pressure of the atmosphere upon flame and explosion. In the second, I shall consider the effects of heat in combustion. In the third, I shall examine the effect of the mixture of gaseous substances not concerned in combustion upon flame and explosion. In the fourth, I shall offer some general views upon flame, and point out certain practical and theoretical applications of the results.

I. *On the effect of rarefaction by partly removing the pressure of the atmosphere upon flame and explosion.*

The earlier experimenters upon the BOYLEAN vacuum observed that flame ceased in highly rarefied air: but the degree of rarefaction necessary for this effect, has been differently stated. Amongst late experimenters, M. de GROTTHUS has examined this subject. He has asserted that a mixture of oxygene and hydrogene ceases to be explosive by the electrical spark when rarefied sixteen times, and that a mixture of chlorine and hydrogene cannot be exploded when rarefied only six times, and he generalises by supposing that rarefaction, whether produced by removing pressure or by heat, has the same effect.

I shall not begin by discussing the experiments of this ingenious author. My own results and conclusions are very different from his; and the cause of this difference, will I think be obvious in the course of these inquiries. I shall proceed in stating the observations which guided my researches.

When hydrogene gas slowly produced from a proper mixture was inflamed at a fine orifice of a glass tube, as in the experiment called the philosophical candle, so as to make a jet of flame of about $\frac{1}{6}$ of an inch in height, and introduced under the receiver of an air pump containing from 200 to 300 cubical inches of air, the flame enlarged as the receiver became exhausted; and when the gage indicated a pressure between 4 and 5 times less than that of the atmosphere was at its maximum of size, it then gradually diminished below, but burned above till the pressure was between 7 and 8 times less, when it became extinguished.

To ascertain whether the effect depended upon the deficiency of oxygene, I used a larger jet with the same apparatus, when the flame to my surprise burned longer, and when the atmosphere was rarefied ten times, and this in repeated trials. When the larger jet was used, the point of the glass tube became white hot, and continued red hot till the flame was extinguished. It immediately occurred to me, that the heat communicated to the gas by this tube, was the cause that the combustion continued longer in the last trials when the larger flame was used; and the following experiments confirmed the conclusion. A piece of wire of platinum was coiled round the top of the tube, so as to reach into and above the flame. The jet of gas of $\frac{1}{6}$ of an inch in height was lighted, and the exhaustion made; the wire of platinum soon became white hot in the centre of the flame, and a small point of wire near the top fused: it continued white hot till the pressure was 6 times less, when it was 10 times it continued red hot at the upper part, and, as long as it was dull red, the gas though extinguished below, continued to burn in contact with the hot wire, and the combustion did not cease until the pressure was reduced 13 times.

It appears from this result, that the flame of hydrogene is extinguished in rarefied atmospheres, only when the heat it produces is insufficient to keep up the combustion, which appears to be when it is incapable of communicating visible ignition to metal, and as this is the temperature required for the inflammation of hydrogene at common pressures, it appears that its *combustibility* is neither diminished nor increased by rarefaction from the removal of pressure.

According to this view with respect to hydrogene, it should follow that amongst other combustible bodies, those which require least heat for their combustion, ought to burn in more rarefied air than those that require more heat, and those that produce much heat in their combustion ought to burn, other circumstances being the same, in more rarefied air than those that produce little heat: and every experiment I have made confirms these conclusions. Thus olefiant gas which approaches nearly to hydrogene in the heat produced by its combustion, and which does not require a much higher temperature for its inflammation, when its flame was made by a jet of gas from a bladder connected with a small tube furnished with a wire of platinum, under the same circumstances as hydrogene, ceased to burn when the pressure was diminished between 10 and 11 times: and the flames of alcohol and of the wax taper which require a greater consumption of heat for the volatilization and decomposition of their combustible matter, were extinguished when the pressure was 5 or 6 times less without the wire of platinum, and 7 or 8 times less when the wire was kept in the flame. Light carburetted hydrogene, which produces, as will be seen hereafter, less heat in combustion than any of the common combustible gases, except carbonic oxide, and which requires a higher temperature for its inflammation than any other, had its flame extinguished, even though the tube was furnished with the wire when the pressure was below $\frac{1}{4}$.

The flame of carbonic oxide which, though it produces little heat in combustion, is as inflammable as hydrogene, burned when the wire was used, the pressure being $\frac{1}{6}$.

The flame of sulphuretted hydrogene, the heat of which is in some measure carried off by the sulphur produced by its decomposition during its combustion in rare air, when burned in the same apparatus as the olefiant and other gases, was extinguished when the pressure was $\frac{1}{7}$.

Sulphur, which requires a lower temperature for its combustion than any common inflammable substance, except phosphorus, burned with a very feeble blue flame in air rarefied fifteen times, and at this pressure the flame heated a wire of platinum to dull redness, nor was it extinguished till the pressure was reduced to $\frac{1}{20}$.*

Phosphorus, as has been shown by M. VAN MARUM, burns in an atmosphere rarefied 60 times; and I found that phosphuretted hydrogene produced a flash of light when admitted into the best vacuum that could be made, by an excellent pump of NAIRN'S construction.

The mixture of chlorine and hydrogene inflames at a much lower temperature than that of hydrogene and oxygene, and produces a considerable degree of heat in combustion; it was

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* The temperature of the atmosphere diminishes in a certain ratio with its height, which must be attended to in the conclusions respecting combustion in the upper regions of the atmosphere, and the elevation must be somewhat lower than in arithmetical progression, the pressure decreasing in geometrical progression.

There is, however, every reason to believe, that the taper would be extinguished at a height of between 9 and 10 miles, hydrogene between 12 and 13, and sulphur between 15 and 16.

therefore probable that it would bear a greater degree of rarefaction, without having its power of exploding destroyed; and this I found in many trials is actually the case, contrary to the assertion of M. de GROTTHUS. Oxygene and hydrogene in the proportion to form water, will not explode by the electrical spark when rarefied eighteen times, but hydrogene and chlorine in the proportion to form muriatic acid gas, gave a distinct flash of light under the same circumstances, and they combined with visible inflammation when the spark was passed through them, the exhaustion being to $\frac{1}{24}$th.

The experiment on the flame of hydrogene with the wire of platinum, and which holds good with the flames of the other gases, shows, that by preserving heat in rarefied air, or giving heat to a mixture, inflammation may be continued when, under common circumstances, it would be extinguished. This I found was the case in other instances, when the heat was differently communicated: thus, when camphor was burned in a glass tube, so as to make the upper part of the tube red hot, the inflammation continued when the rarefaction was 9 times, whereas it would only continue in air rarefied 6 times, when it was burned in a thick metallic tube which could not be considerably heated by it.

By bringing a little naphtha in contact with a red hot iron, it produced a faint lambent flame, when there remained in the receiver only $\frac{1}{30}$ of the original quantity of air, though without foreign heat its flame was extinguished when the quantity was $\frac{1}{6}$.

I rarefied a mixture of oxygene and hydrogene by the air pump to about eighteen times, when it could not be inflamed by the electric spark. I then heated strongly the upper part of the tube till the glass began to soften, and passed the spark, when a feeble flash was observed not reaching far into the tube, the heated gases only appearing to enter into inflammation. This last experiment requires considerable care. If the exhaustion is much greater, or if the heat is raised very slowly,* it does not succeed; and if the heat is raised so high as to make the glass luminous, the flash of light, which is extremely feeble, is not visible: it is difficult to procure the proper degree of exhaustion, and to give the exact degree of heat; I have, however, succeeded three times in obtaining the results, and in one instance it was witnessed by Mr. BRANDE.

To elucidate the enquiry still farther, I made a series of experiments on the heat produced by some of the inflammable gases in combustion. In comparing the heat communicated to wires of platinum by flames of the same size, it was evident, that hydrogene and olefiant gas in oxygene, and hydrogene in chlorine, produced a much greater intensity of heat in combustion, than the other gaseous substances I have named burned in oxygene: but no regular scale could be formed from observations of this kind. I endeavoured to gain some approximations on the subject by burning equal quantities of different gases under the same circumstances, and applying the heat to an apparatus by which it could be measured. For this purpose a mercurial gas holder was furnished with a system of stop cocks, terminating in a strong tube of platinum having a minute aperture. Above this was fixed a copper cup filled with olive oil, in which a thermometer was placed. The oil was heated to 212° to prevent any differences in the communication of heat by the condensation of aqueous vapour;

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* The reason will be obvious from what is stated in page 55.

the pressure was the same for the different gases, and they were consumed as nearly as possible in the same time, and the flame applied to the same point of the copper cup, the bottom of which was wiped after each experiment.

The results were as follows:

The flame from olefiant gas raised the thermometer to	270°
——— hydrogene - - -	238
——— sulphuretted hydrogene - -	232
——— coal gas - - -	236
——— gaseous oxide of carbon - -	218

The quantities of oxygene consumed (that absorbed by the hydrogene being taken as 1) would be, supposing the combustion perfect, for the olefiant gas 6, for the sulphuretted hydrogene 3, for the carbonic oxide 1. The coal gas contained only a very small proportion of olefiant gas; supposing it to be pure carburetted hydrogene, it would have consumed 4 of oxygene. Taking the elevations of temperature, and the quantities of oxygene consumed as the data, the ratios of the heat produced by the combustion of the different gases, would be for hydrogene 26, for olefiant gas 9.66, for sulphuretted hydrogene 6.66, for carburetted hydrogene 6, for carbonic oxide 6[*].

It will be useless to reason upon this ratio as exact, for charcoal was deposited both from the olefiant gas and coal gas during the experiment, and much sulphur was deposited from the sulphuretted hydrogene; and there is great reason to believe, that the capacities of fluids for heat increase with their temperature. It confirms, however, the general con-

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[*] These results may be compared with Mr. Dalton's new System of Chemical Philosophy; they agree in showing that hydrogene produces more heat in combustion than any of its compounds.

clusions, and proves that hydrogene stands at the head of the scale, and gaseous oxide of carbon at the bottom. It might at first view be imagined that, according to this scale, the flame of carbonic oxide ought to be extinguished by rarefaction, at the same degree as that of carburetted hydrogene; but it must be remembered, as I have mentioned in another place, that carbonic oxide is a much more combustible gas. Carbonic oxide inflames in the atmosphere when brought into contact with an iron wire heated to dull redness, whereas carburetted hydrogene is not inflammable by a similar wire, unless it is heated to whiteness so as to burn with sparks.

II. *On the effects of rarefaction by heat on combustion and explosion.*

The results detailed in the preceding section are indirectly opposed to the opinion of M. de Grotthus, that rarefaction by heat destroys the combustibility of gaseous mixtures. Before I made any direct experiments on this subject, I endeavoured to ascertain the degree of expansion which can be communicated to elastic fluids by the strongest heat that can be applied to glass vessels. For this purpose I introduced into a graduated curved glass tube some fusible metal. I heated the fusible metal and the portion of the tube containing the air included by it, under boiling water for some time. I then placed the apparatus in a charcoal fire, and very gradually raised the temperature till the fusible metal appeared luminous when viewed in the shade. At this time the air had expanded so as to occupy 2.25 parts in the tube, it being 1 at the temperature of boiling water. Another experiment was made in a thicker glass tube, and the heat was raised until the tube began to run together; but though this heat appeared cherry red, the expansion was not to more than 2.5, and a part of this might perhaps have been apparent only, owing to the collapsing of the glass tube before it actually melted. It may be supposed that the oxidation of the fusible metal may have had some effect in making the expansion appear less; but in the first experiment the air was gradually brought back to its original temperature of boiling water, when the absorption was scarcely sensible. If M. Gay Lussac's conclusions be taken as the ground work of calculation, and it be supposed that air expands equally for equal increments of temperature, it would appear that the temperature of air capable of rendering glass luminous must be 1035° Fahrenheit.*

M. de Grotthus describes an experiment in which atmospheric air and hydrogene, expanded to four times their bulk over mercury by heat, would not inflame by the electric spark. It is evident, that in this experiment a large quantity of steam or of mercurial vapour must have been present, which, like other inexplosive elastic fluids, prevents combustion when mixed in certain quantities with explosive mixtures; but though he seems aware that his gases were not dry, yet he draws his general conclusion, that expansion by heat destroys the explosive powers of gases, principally from this inconclusive experiment.

I introduced into a small graduated tube over well boiled mercury, a mixture of two parts of hydrogene and one of

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* The mode of ascertaining temperatures as high as the point of fusion of glass by the expansion of air, seems more unexceptionable than any other. It gives for the point of visible ignition nearly the same degree as that deduced by Newton from the times of the cooling of ignited metal in the atmosphere.

oxygene, and heated the tube by a large spirit lamp till the volume of the gas was increased from 1 to 2.5. I then, by means of a blow-pipe and another spirit lamp, made the upper part of the tube red hot, when an explosion instantly took place.

I introduced into a bladder a mixture of oxygene and hydrogene, and connected this bladder with a thick glass tube of about $\frac{1}{6}$ of an inch in diameter and three feet long, curved so that it could be gradually heated in a charcoal furnace; two spirit lamps were placed under the tube where it entered the charcoal fire, and the mixture was very slowly pressed through: an explosion took place before the tube was red hot.

This experiment shows that expansion by heat, instead of diminishing the combustibility of gases, on the contrary, enables them to explode apparently at a lower temperature, which seems perfectly reasonable, as a part of the heat communicated by any ignited body must be lost in gradually raising the temperature. I made several other experiments which establish the same conclusions. A mixture of common air and hydrogene was introduced into a small copper tube, having a stopper not quite tight; the copper tube was placed in a charcoal fire; before it became visibly red an explosion took place, and the stopper was driven out.

I made various experiments on explosions by passing mixtures of hydrogene and oxygene through heated tubes; in the beginning of one of these trials, in which the heat was much below redness, steam appeared to be formed without any combustion. This led me to expose mixtures of oxygene and hydrogene in tubes, in which they were confined by fluid fusible metal to heat; and I found that by carefully applying a heat between the boiling point of mercury, which is not sufficient for the effect, and a heat approaching to the greatest heat that can be given without making glass luminous in darkness, the combination was effected without any violence, and without any light: and commencing with 212°, the volume of steam formed at the point of combination appeared exactly equal to that of the original gases. So that the first effect in experiments of this kind is an expansion, afterwards a contraction, and then the restoration of the primitive volume.

If when this change is going on, the heat be quickly raised to redness, an explosion takes place; but with small quantities of gas the change is completed in less than a minute.

It is probable, that the slow combination without combustion, already long ago observed with respect to hydrogene and chlorine, oxygene and metals, will happen at certain temperatures with most substances that unite by heat. On trying charcoal, I found that at a temperature which appeared to be a little above the boiling point of quicksilver, it converted oxygene pretty rapidly into carbonic acid, without any luminous appearance, and at a dull red heat, the elements of olefiant gas combined in a similar manner with oxygene, slowly and without explosion.

The effect of the slow combination of oxygene and hydrogene is not connected with their rarefaction by heat, for I found that it took place when the gases were confined in a tube by fusible metal rendered solid at its upper surface; and certainly as rapidly, and without any appearance of light.

M. de Grotthus has stated, that, if a glowing coal be brought into contact with a mixture of oxygene and hydrogene, it only rarefies them, but does not explode them; but this depends upon the degree of heat communicated by the coal: if it is red in day light and free from ashes, it uniformly explodes the mixture; if its redness is barely visible in shade, it will not explode them, but cause their slow combination: and the general phenomenon is wholly unconnected with rarefaction, as is shown by the following circumstance. When the heat is greatest, and before the invisible combination is compleated, if an iron wire heated to whiteness be placed upon the coal within the vessel, the mixture instantly explodes.

Light carburetted hydrogene, or pure fire-damp, as has been shown, requires a very strong heat for its inflammation; it therefore offered a good substance for an experiment on the effect of high degrees of rarefaction by heat on combustion. I mixed together one part of this gas and eight parts of air, and introduced them into a bladder furnished with a capillary tube. I heated this tube till it began to melt, and then slowly passed the mixture through it into the flame of a spirit lamp, when it took fire and burned with its own peculiar explosive light beyond the flame of the lamp, and when withdrawn, though the aperture was quite white hot, it continued to burn vividly.

That the compression in one part of an explosive mixture produced by the sudden expansion of another part by heat, or the elestric spark, is not the cause of combination, as has been supposed by Dr. Higgins, M. Berthollet, and others, appears to be evident from what has been stated, and it is rendered still more so by the following facts. A mixture of hydro-phosphoric gas (bi-phosphuretted hydrogene gas) and oxygene, which explode at a heat a little above that of boiling water, was confined by mercury, and very gradually heated on a sand bath: when the temperature of the mercury was 242°, the mixture exploded.

A similar mixture was placed in a receiver communicating with a condensing syringe, and condensed over mercury till it occupied only $\frac{1}{5}$ of its original volume. No explosion took place, and no chemical change had occurred, for when its volume was restored, it was instantly exploded by the spirit lamp.

It would appear, then, that *the heat* given out by the compression of gases is the real cause of the combustion which it produces, and that at certain elevations of temperature, whether in rarefied or compressed atmospheres, explosion or combustion occurs, i. e. bodies combine with the production of heat and light.

III. *On the effects of the mixture of different gases in explosion and combustion.*

In my first Paper on the fire-damp of coal mines, I have mentioned that carbonic acid gas has a greater power of destroying the explosive power of mixtures of fire-damp and air than azote, and I have ventured to suppose the cause to be its greater density and capacity for heat, in consequence of which it might exert a greater cooling agency, and prevent the temperature of the mixture from being raised to that degree necessary for combustion. I have lately made a series of experiments with the view of determining how far this idea is correct, and for the purpose of ascertaining the general phenomena of the effects of the mixture of gaseous substances upon explosion and combustion.

I took given volumes of a mixture of two parts of hydrogene and one part of oxygene by measure, and diluting them with various quantities of different elastic fluids, I ascertained at what degree of dilution the power of inflammation by a strong spark from a Leyden phial was destroyed. I found that for one of the mixture inflammation was prevented by

Of Hydrogene, about	8
Oxygene	9
Nitrous oxide	11
Carburetted hydrogene	1
Sulphuretted hydrogene	2
Olefiant gas	½
Muriatic acid gas	2
Silicated fluoric acid gas	⅚

Inflammation took place when the mixtures contained of

Hydrogene	6
Oxygene	7
Nitrous oxide	10
Carburetted hydrogene	¾
Olefiant gas	⅓
Sulphuretted hydrogene	1½
Muriatic acid gas	1½
Fluoric acid gas	¾

I hope to be able to repeat these experiments with more precision at no distant time; the results are not sufficiently exact to lay the foundation for any calculations on the relative cooling powers of equal volumes of the gases, but they show sufficiently, if the conclusions of M. M. De la Roche and Berard be correct, that other causes, besides density and capacity for heat, interfere with the phenomena. Thus nitrous oxide, which is nearly $\frac{1}{3}$ denser than oxygene, and which, according to De la Roche and Berard, has a greater capacity for heat in the ratio of 1.3503 to .9765 in volume, has lower powers of preventing explosion; and hydrogene, which is 15 times lighter than oxygene, and which in equal volumes has a smaller capacity for heat, certainly has a higher power of preventing explosion; and olefiant gas exceeds all other gaseous substances in a much higher ratio than could have been expected from its density and capacity. The olefiant gas I used was recently made, and might have contained some vapour of ether, and the nitrous oxide was mixed with some azote, but these slight causes could not have interfered with the results to any considerable extent.

Mr. Leslie, in his elaborate and ingenious researches on heat, has observed the high powers of hydrogene of abstracting heat from solid bodies, as compared with that of common air and oxygene. I made a few experiments on the comparison of the powers of hydrogene, in this respect, with those of carburetted hydrogene, azote, oxygene, olefiant gas, nitrous oxide, chlorine, and carbonic acid gas. The same thermometer raised to the same temperature, 160°, was exposed to equal volumes (21 cubic inches) of olefiant gas, coal gas, carbonic acid gas, chlorine, nitrous oxide gas, hydrogene, oxygene, azote, and air, at equal temperatures, 52° Fahrenheit.

The times required for cooling to 106° were for

	′	″
Air - - - -	2	
Hydrogene - - -		45

	′ ″
Olefiant gas	1.15
Coal gas	55
Azote	1.30
Oxygene	1.47
*Nitrous oxide	2.30. 2.53
*Carbonic acid gas	2.45
Chlorine	3.6

It appears from these experiments, that the powers of elastic fluids to abstract or conduct away heat from solid surfaces, is in some inverse ratio to their density, and that there is something in the constitution of the light gases, which enables them to carry off heat from solid surfaces in a different manner from that in which they would abstract it in gaseous mixtures, depending probably upon the mobility of their parts.[†] The heating of gaseous media by the contact of fluid or solid bodies, as has been shown by Count RUMFORD, depends principally upon the change of place of their particles; and it is evident from the results stated in the beginning of this section, that these particles have different powers of abstracting heat analogous to the different powers of solids and fluids. Where an elastic fluid exerts a cooling influence on a solid surface, the effect must depend principally upon the rapidity with which its particles change their places: but where the cooling particles are mixed throughout a mass with other gaseous particles, their effect must princi-

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[*] These two last results were observed by Mr. FARADAY of the Royal Institution, (from whom I receive much useful assistance in most of my experiments), when I was absent from the Laboratory.

[†] Those particles which are lighest must be conceived most capable of changing place, and would therefore cool solid surfaces most rapidly: in the cooling of gaseous mixtures, the mobility of the particles can be of little consequence.

pally depend upon the power they possess of rapidly abstracting heat from the contiguous particles; and this will depend probably upon two causes, the simple abstracting power by which they become quickly heated, and their capacity for heat which is great in proportion as their temperatures are less raised by this abstraction.

Whatever be the cause of the different cooling powers of the different elastic fluids in preventing inflammation, very simple experiments show that they operate uniformly with respect to the different species of combustion, and that those explosive mixtures, or inflammable bodies, which require least heat for their combustion, require larger quantities of the different gases to prevent the effect, and *vicè versa*; thus one of chlorine and one of hydrogene still inflame when mixed with eighteen times their bulk of oxygene, whereas a mixture of carburetted hydrogene and oxygene in the proper proportions for combinations, one and two, have their inflammation prevented by less than three times their volume of oxygene.

A wax taper was instantly extinguished in air mixed with $\frac{1}{10}$ of silicated fluoric acid gas, and in air mixed with $\frac{1}{6}$ of muriatic acid gas; but the flame of hydrogene burned readily in those mixtures, and in mixtures in which the flame of hydrogene was extinguished, the flame of sulphur burned.

There is a very simple experiment which demonstrates in an elegant manner this general principle. Into a long bottle with a narrow neck introduce a lighted taper, and let it burn till it is extinguished; carefully stop the bottle, and introduce another lighted taper, it will be extinguished before it reaches the bottom of the neck: then introduce a small tube containing zinc and diluted sulphuric acid, and at the aperture of which the hydrogene is inflamed; the hydrogene will be found to burn in whatever part of the bottle the tube is placed: after the hydrogene is extinguished, introduce lighted sulphur; this will burn for some time, and after its extinction, phosphorus will be as luminous as in the air, and, if heated in the bottle, will produce a pale yellow flame of considerable density.

In cases when the heat required for chemical union is very small, as in the instance of hydrogene and chlorine, a mixture which prevents inflammation will not prevent combination, i. e. the gases will combine without any flash. This I witnessed in mixing two volumes of carburetted hydrogene with one of chlorine and hydrogene; muriatic acid was formed throughout the mixture, and heat produced, as was evident from the expansion when the spark passed, and the rapid contraction afterwards, but the heat was so quickly carried off by the quantity of carburetted hydrogene that no flash was visible.

In the case of phosphorus, which is combustible at the lowest temperature of the atmosphere, no known admixture of elastic fluid prevents the luminous appearance; but this seems to depend upon the light being limited to the solid particles of phosphoric acid formed; whereas to produce flame, a certain mass of elastic fluid must be luminous; and there is every reason to believe, that when phosphuretted hydrogene explodes in very rare air, it is only the phosphorus which is consumed. Any other substance that produces solid matter in combustion would probably be luminous in air as rare, or in mixtures as diluted, as phosphorus, provided the heat was elevated sufficiently for its combustion. I have found that this is actually the case with respect to zinc. I threw some zinc filings into an ignited iron crucible fixed on the stand of an air pump under a receiver, and exhausted until only $\frac{1}{60}$ of the original quantity of air remained. When I judged that the red hot crucible must be full of the vapour of zinc, I admitted about $\frac{1}{60}$ more of air, when a bright flash of light took place in and above the crucible, similar to that which is produced by admitting air to the vapour of phosphorus in vacuo.

The cooling power of mixtures of elastic fluids in preventing combustion must increase with their condensation, and diminish with their rarefaction; at the same time, the quantity of matter entering into combustion in given spaces, is relatively increased and diminished. The experiments on flame in rarefied atmospherical air, show that the quantity of heat produced in combustion is very slowly diminished by rarefaction, the diminution of the cooling power of the azote being apparently in a higher ratio than the diminution of the heating powers of the burning bodies. I endeavoured to ascertain what would be the effect of condensation on flame in atmospheric air, and whether the cooling power of the azote would increase in a lower ratio, as might be expected, than the heat produced by the increase of the quantity of matter entering into combustion, but I found considerable difficulties in making the experiments with precision. I ascertained, however, that both the light and heat of the flames of the taper, of sulphur and hydrogene, were increased by acting on them by air condensed four times; but not more than they would have been by an addition of $\frac{1}{5}$ of oxygene.

I condensed air nearly five times, and ignited iron wire to whiteness in it by the voltaic apparatus, but the combustion took place with very little more brightness than in the common atmosphere, and would not continue as in oxygene, nor did charcoal burn much more brightly in this compressed air than in common air. I intend to repeat these experiments, if possible, with higher condensing powers: they show sufficiently that, (for certain limits at least) as rarefaction does not diminish considerably the heat of flame in atmospherical air, so neither does condensation considerably increase it; a circumstance of great importance in the constitution of our atmosphere, which at all the heights or depths at which man can exist, still preserves the same relations to combustion.

It may be concluded from the general law, that at high temperatures, gases not concerned in combustion will have less powers of preventing that operation, and likewise, that steam and vapours, which require a considerable heat for their formation, will have less effect in preventing combustion, particularly of those bodies requiring low temperatures, than gases at the common heat of the atmosphere.

I have made some experiments on the effects of steam, and their results were conformable to these views. I found that a very large quantity of steam was necessary to prevent sulphur from burning. Oxygene and hydrogene exploded by the electric spark when mixed with five times their volume of steam; and even a mixture of air and carburetted hydrogene gas, the least explosive of all mixtures, required a third of steam to prevent its explosion, whereas $\frac{1}{5}$ of azote produced the effect. These trials were made over mercury; heat was applied to water above the mercury, and 37.5 for 100 parts was regarded as the correction for the expansion of the gases, It is probable that with certain heated mixtures of gases, where the non-supporting or non-inflammable elastic fluids are in great quantities, combination with oxygene will take place, as in the instance mentioned, page 63, of hydrogene and chlorine, without any light, for the temperature produced will not be sufficient to render elastic media luminous; and there are no combustions, except those of the compounds of phosphorus and the metals, in which solid matters are the result of combinations with oxygene. I have shown in the paper referred to in the introduction, that the light of common flames depends almost entirely upon the deposition, ignition and combustion of solid charcoal; but to produce this deposition from gaseous substances demands a high temperature. Phosphorus, which rises in vapour at common temperatures, and the vapour of which combines with oxygene at those temperatures, as I have mentioned before, is always luminous, for each particle of acid formed must, there is every reason to believe, be white hot; but so few of these particles exist in a given space that they scarcely raise the temperature of a solid body exposed to them, though, as in the rapid combustion of phosphorus, where immense numbers are existing in a small space, they produce a most intense heat.

In all cases the quantity of heat communicated by combustion, will be in proportion to the quantity of burning matter coming in contact with the body to be heated. Thus, the blow-pipe and currents of air operate. In the atmosphere, the effect is impeded by the mixture of azote, though still it is very great: with pure oxygene compression produces an immense effect, and with currents of oxygene and hydrogene, there is every reason to believe, that solid matters are made to attain the temperature of the flame. This temperature, however, evidently presents the limit to experiments of this kind, for bodies exposed to flame can never be hotter than flame itself; whereas in the voltaic apparatus there seems to be no limit to the heat, except the volatilization of the conductors.

The temperatures of flames are probably very different. Where, in chemical changes, there is no change of volume, as in the instance of the mutual action of chlorine and hydrogene, prussic gas (cyanogen) and oxygene, approximations to their temperatures may be gained from the expansion in explosion.

I have made some experiments of this kind by detonating the gases by the electrical spark in a curved tube containing mercury or water; and I judged of the expansion from the quantity of fluid thrown out of the tube: the resistance opposed by mercury, and its great cooling powers, rendered the results very unsatisfactory in the cases in which it was used; but with water, cyanogen and oxygene being employed, they were more conclusive. Cyanogen and oxygene, in the proportion of one to two, detonated in a tube of about $\frac{2}{5}$ of an inch in diameter, displaced a quantity of water which demonstrated an expansion of fifteen times their original bulk. This would indicate a temperature of above 5000° of Fahrenheit, and the real temperature is probably much higher; for heat must be lost by communication to the tube and the water. The heat of the gaseous carbon in combustion in this gas, appears more intense than that of hydrogene; for I found a filament of platinum was fused by a flame of cyanogen in the air which was not fused by a similar flame of hydrogene.

IV. *Some general observations, and practical inferences.*

The knowledge of the cooling power of elastic media in preventing the explosion of the fire-damp, led me to those practical researches which terminated in the discovery of the wire-gauze safe-lamp; and the general investigations of the relation and extent of these powers, serves to elucidate the operation of wire-gauze and other tissues or systems of apertures permeable to light and air, in intercepting flame, and confirms the views I originally gave of the phenomenon.

Flame is gaseous matter heated so highly as to be luminous, and that to a degree of temperature beyond the white heat of solid bodies, as is shown by the circumstance, that air not luminous will communicate this degree of heat.* When an attempt is made to pass flame through a very fine mesh of wire-gauze at the common temperature, the gauze cools each portion of the elastic matter that passes through it, so as to reduce its temperature below that degree at which it is luminous, and the diminution of temperature must be proportional to the smallness of the mesh and the mass of the metal. The power of a metallic or other tissue to prevent explosion, will depend upon the heat required to produce the combustion as compared with that acquired by the tissue; and the flame of the most inflammable substances, and of those that produce most heat in combustion, will pass through a metallic tissue that will interrupt the flame of less inflammable substances, or those that produce little heat in combustion. Or the tissue

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* This is proved by the simple experiment of holding a fine wire of platinum about the $\frac{1}{20}$ of an inch from the exterior of the middle of the flame of a spirit lamp, and concealing the flame by an opaque body, the wire will become white hot in a space where there is no visible light.

being the same, and impermeable to all flames at common temperatures, the flames of the most combustible substances, and of those which produce most heat, will most readily pass through it when it is heated, and each will pass through it at a different degree of temperature. In short, all the circumstances which apply to the effect of cooling mixtures upon flame, will apply to cooling perforated surfaces. Thus, the flame of phosphuretted hydrogene at common temperatures, will pass through a tissue sufficiently large not to be immediately choaked up by the phosphoric acid formed, and the phosphorus deposited.* A tissue of 100 apertures to the square inch, made of wire of $\frac{1}{60}$, will at common temperatures intercept the flame of a spirit lamp, but not that of hydrogene; and when strongly heated, it will no longer arrest the flame of the spirit lamp. A tissue which will not interrupt the flame of hydrogene when red hot, will still intercept that of olefiant gas, and a heated tissue which would communicate explosion from a mixture of olefiant gas and air, will stop an explosion from a mixture of fire-damp, or carburetted hydrogene.

The ratio of the combustibility of the different gaseous matters are likewise to a certain extent as the masses of heated matter required to inflame them.† Thus an iron

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* If a tissue containing above 700 apertures to the square inch be held over the flame of phosphorus or phosphuretted hydrogene, it does not transmit the flame till it is sufficiently heated to enable the phosphorus to pass through it in vapour. Phosphuretted hydrogene is decomposed in flame, and acts exactly like phosphorus.

† It appeared to me in these experiments, that the worst conducting and best radiating substances required to be heated higher for equal masses to produce the same effect upon the gases; thus, red hot charcoal had evidently less power of inflammation than red hot iron.

wire of $\frac{1}{40}$ of an inch heated cherry red, will not inflame olefiant gas, but it will inflame hydrogene gas; and a wire of $\frac{1}{8}$, heated to the same degree, will inflame olefiant gas; but a wire of $\frac{1}{500}$ must be heated to whiteness to inflame hydrogene, though at a low red heat it will inflame bi-phosphuretted gas; but wire of $\frac{1}{40}$ heated even to whiteness will not inflame mixtures of fire-damp.

These circumstances will explain, why a mesh of wire so much finer is required to prevent the explosion from hydrogene and oxygene from passing, and why so coarse a texture and wire is sufficient to prevent the explosion of the fire-damp, fortunately the least combustible of the known inflammable gases.

The general doctrine of the operation of wire-gauze cannot be better elucidated than in its effects upon the flame of sulphur. When wire-gauze of 600 or 700 apertures to the square inch is held over the flame, fumes of condensed sulphur immediately come through it, and the flame is intercepted; the fumes continue for some instants, but as the heat increases they diminish, and at the moment they disappear, which is long before the gauze becomes red hot, the flame passes; the temperature at which sulphur burns being that at which it is gaseous.

Another very simple illustration of the truth of this view is offered in the effect of the cooling agency of metallic surfaces upon very small flames. Let the smallest possible flame be made by a single thread of cotton immersed in oil, and burning immediately upon the surface of the oil: it will be found to be about $\frac{1}{30}$ of an inch in diameter. Let a fine iron wire of $\frac{1}{180}$ be made into a circle of $\frac{1}{10}$ of an inch in diameter and brought over the flame. Though at such a distance, it will instantly extinguish the flame, if it be *cold:* but if it be held above the flame, so as to be slightly heated, the flame may be passed through it without being extinguished. That the effect depends entirely upon the power of the metal to abstract the heat of flame, is shown by bringing a glass capillary ring of *the same* diameter and size over the flame; this being a much worse conductor of heat, will not extinguish it even when *cold.* If its size however be made greater, and its circumference smaller, it will act like the metallic wire, and require to be heated to prevent it from extinguishing the flame.*

Suppose a flame divided by the wire-gauze into smaller flames, each flame must be extinguished in passing its aperture till that aperture has attained a temperature sufficient to produce the permanent combustion of the explosive mixture.

A flame of sulphur may be made much smaller than that of hydrogene, that of hydrogene smaller than that of a wick fed with oil, and that of a wick fed with oil smaller than that of carburetted hydrogene; and a ring of cool wire which instantly extinguishes the flame of carburetted hydrogene, only slightly diminishes the size of a flame of sulphur of the same dimensions.

Where rapid currents of explosive mixtures are made to act upon wire-gauze, it is of course much more rapidly heated; and therefore the same mesh which arrests the flames

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* Let a small globe of metal of $\frac{1}{20}$ of an inch in diameter made by fusing the end of a wire be brought near a flame of $\frac{1}{30}$ in diameter, it will extinguish it when cold at the distance of its own diameter; let it be heated, and the distance will diminish at which it produces the extinction; and at a white heat it does not extinguish it by actual contact, though at a dull red heat it immediately produces the effect.

of explosive mixtures at rest, will suffer them to pass when in rapid motion; but by *increasing* the cooling surface by diminishing the size, or increasing the depth of the aperture, all *flames*, however rapid their motion, may be arrested. Precisely the same law applies to explosions acting in close vessels: very minute apertures when they are only few in number will permit explosions to pass, which are arrested by much larger apertures when they fill a whole surface. A small aperture was drilled at the bottom of a wire-gauze lamp in the cylindrical ring which confines the wire-gauze; this, though less than $\frac{1}{18}$ of an inch in diameter, passed the flame and fired the external atmosphere, in consequence of the whole force of the explosion of the thin stratum of the mixture included within the cylinder driving the flame through the aperture; though, had the whole ring been composed of such apertures separated by wires, it would have been perfectly safe.

Nothing can demonstrate more decidedly than these simple facts and observations, that the interruption of flame by solid tissues permeable to light and air, depends upon no recondite or mysterious cause, but to their cooling powers, simply considered as such.

When a light included in a cage of wire-gauze is introduced into an explosive atmosphere of fire-damp at rest, the maximum of heat is soon obtained, the radiating power of the wire, and the cooling effect of the atmosphere, more efficient from the mixture of inflammable air, prevents it from ever arriving at a temperature equal to that of dull redness. In rapid currents of explosive mixtures of fire-damp, which heat common gauze to a higher temperature, twilled gauze, in which the radiating surface is considerably greater, and the circulation of air less, preserves an equal temperature. Indeed the heat communicated to the wire by combustion of the fire-damp in wire-gauze lamps, is completely in the power of the manufacturer, for by diminishing the apertures and increasing the mass of metal, or the radiating surface, it may be diminished to any extent.

I have lately had lamps made of thick twilled gauze of wires of $\frac{1}{40}$, sixteen to the warp, and thirty to the weft, which being rivetted to the screw, cannot be displaced; from its flexibility it cannot be broken, and from its strength cannot be crushed, except by a very strong blow.

Even in the common lamps the flexibility of the material has been found of great importance, and I could quote one instance of a dreadful accident having been prevented, which must have happened had any other material than wire-gauze been employed in the construction of the lamp: and how little difficulty has occurred in the practical application of the invention, is shown by the circumstance, that it has been now for ten months in the hands of hundreds of common miners in the most dangerous mines in Britain, during which time not a single accident has occurred where it has been employed, whilst in other mines, much less dangerous, where it has not yet been adopted, some lives have been lost, and many persons burned.*

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* Plates of different forms of this lamp are annexed. (Pl. V.) They are applicable to all purposes in which explosions or inflammations are to be guarded against, whether from fire-damp, or carburetted hydrogene, coal gas, vapours of spirits, or of ether. And by the introduction of glass cylinders within the wire-gauze cylinder *above* the flame, the wick may be made very large, and it burns on the principle of the Liverpool lamp.

The facts stated in Section II. explain why so much more heat is obtained from fuel when it is burnt quickly; and they show that in all cases the temperature of the acting bodies should be kept as high as possible, not only because the general increment of heat is greater, but likewise, because those combinations are prevented which at lower temperatures take place without any considerable production of heat: thus, in the Argand lamp, the Liverpool lamp, and in the best fire-places, the increase of effect does not depend merely upon the rapid current of air, but likewise upon the heat preserved by the arrangements of the materials of the chimney, and communicated to the matters entering into inflammation.

These facts likewise explain the methods by which temperature may be increased, and the limit to certain methods. Currents of flame, as it was stated in the last section, can never raise the heat of bodies exposed to them, higher than a certain degree, their own temperature; but by compression, there can be no doubt, the heat of flames from pure supporters and combustible matter may be greatly increased, probably in the ratio of their compression. In the blow-pipe of oxygene and hydrogene, the maximum of temperature is close to the aperture from which the gases are disengaged, i. e. where their density is greatest. Probably a degree of temperature far beyond any that has been yet attained may be produced by throwing the flame from compressed oxygene and hydrogene into the voltaic arc, and thus combining the two most powerful agents for increasing temperature.

The circumstances mentioned in this Paper, combined with those noticed in the Pape on flame printed in Mr. BRANDE's

Journal of Science and the Arts, explain the nature of the light of flames and their form. When in flames pure gaseous matter is burnt, the light is extremely feeble: the density of a common flame is proportional to the quantity of solid charcoal first deposited and afterwards burnt. The form of the flame is conical, because the greatest heat is in the centre of the explosive mixture. In looking steadfastly at flame, the part where the combustible matter is volatilized is seen, and it appears dark, contrasted with the part in which it begins to burn, that is where it is so mixed with air as to become explosive. The heat diminishes towards the top of the flame, because in this part the quantity of oxygene is least. When the wick increases to a considerable size from collecting charcoal, it cools the flame by radiation, and prevents a proper quantity of air from mixing with its central part; in consequence, the charcoal thrown off from the top of the flame is only red hot, and the greater part of it escapes unconsumed.

The intensity of the light of flames in the atmosphere is increased by condensation, and diminished by rarefaction, apparently in a higher ratio than their heat, more particles capable of emitting light exist in the denser atmospheres, and yet most of these particles in becoming capable of emitting light, absorb heat; which could not be the case in the condensation of a pure supporting medium.

The facts stated in Section I. show that the luminous appearances of shooting stars and meteors cannot be owing to any inflammation of *elastic* fluids, but must depend upon the ignition of solid bodies. Dr. HALLEY calculated the height of a meteor at ninety miles, and the great American meteor which threw down showers of stones, was estimated at seventeen miles high. The velocity of motion of these bodies must in all cases be immensely great, and the heat produced by the compression of the most rarefied air from the velocity of motion must be probably sufficient to ignite the mass; and all the phenomena may be explained, if *falling stars* be supposed to be small solid bodies moving round the earth in very eccentric orbits, which become ignited only when they pass with immense velocity through the upper regions of the atmosphere, and if the *meteoric bodies* which throw down stones with explosions be supposed to be similar bodies which contain either combustible or elastic matter.

Cobham-hall, Kent,
January 8, 1817.