strong illumination therefore the sympathetic dark reaction would seem to be least in the case of the red fibres.

Subjective colours of the same class as those shown by Benham's top, but not nearly so conspicuous, have long been known. Helmholtz* mentions that if a rotating disk with black and white sectors is looked at fixedly, each white sector appears to be reddish along its leading border and bluish along its rear border. He also remarks that these colours are more easily seen upon a disk covered with two spiral bands, black and white, of equal breadth. From these and other observations, Helmholtz concludes that when a point of the retina is exposed to rapid alternations of white light and of darkness, causing successive states of increasing and decreasing excitation, the moment of maximum excitation is not the same for all colours. It has, however, been shown above that in analogous cases the red originates in a portion of the retina which has not been exposed to the direct action of light, while the blue originates in a portion where light has not ceased to act. Helmholtz's supposition therefore does not apply—at least to the class of colours at present under consideration.

I have not made any attempt to account for the more feeble colours exhibited by the two intermediate groups of lines in Benham's top, nor for the changes which occur when the speed of rotation is increased. These effects no doubt result, at least in part, from modifications of the phenomena already discussed. But for the present I am compelled to discontinue the experiments on account of the disagreeable and probably injurious effects which they produce upon the eyes.

"On the Effect of Pressure in the Surrounding Gas on the Temperature of the Crater of an Electric Arc. Correction of Results in former Paper." By W. E. Wilson, F.R.S., and G. F. Fitzgerald, F.R.S. Received November 30,—Read December 17, 1896.

In May, 1895, a preliminary paper by one of the authors was read at the Royal Society, in which is described the apparatus used for these experiments, and the results which were then obtained.

The primary object of this research was to determine, if possible, whether the temperature of the crater in the positive carbon varies when the pressure in the surrounding gas is changed.

It has been suggested that the temperature of the crater is that of

* 'Phys. Optik,' § 23.

boiling carbon. The most modern determinations give this temperature of the crater as about 3300—3500 C.*

If this is the true boiling point of carbon, it is then clear that solar physicists must find some other substance than solid carbon particles to form the photospheric clouds in the sun, as the temperature of this layer is most probably not below 8000 C.,† unless, indeed, the pressure in the solar atmosphere is sufficient to raise the boiling point of carbon to about this temperature (see p. 381). It is in order to throw some light on this subject that these experiments were undertaken.

The gas used in our first experiments was nitrogen, and we found that the radiation from the crater fell off in a most remarkable manner whenever the pressure was raised in the box surrounding the arc. This falling off was not due to any very large extent to visible cloud or smoke, and the crater seemed so much reduced in temperature as to glow with only a red heat. This seemed to show that the temperature of the crater depends on how much it is cooled by the surrounding gas, and not on its being the temperature at which the vapour of carbon has the same pressure as the surrounding atmosphere.

It was found that we were limited to pressures not exceeding about 20 atmos., as at this pressure we could not withdraw the negative carbon sufficiently to see into the crater without the arc breaking. We were then only able to obtain a current from a battery of accumulators which had an E.M.F. of 110 volts. Since then we obtained a Crompton dynamo which could give 300 volts and 15 amperes, and which was driven by a turbine.

From the great difficulty of obtaining a sufficient quantity of pure nitrogen under pressure, we obtained a 20 ft. cylinder of air compressed to 120 atmos. With this we tried a series of experiments, and these at first seemed to corroborate our former ones, in which we used nitrogen, but we found that at any rate some of the radiation, and possibly a great deal of it, was cut off by the formation of what appeared to be red fumes of NO_2. We found no absorption from this cause so long as the pressure was nearly atmospheric, but at about 100 lbs. pressure this gas was formed with great rapidity, and undoubtedly cut off a great deal of the radiation. We easily confirmed our belief in the presence of this gas by its well known absorption spectrum.

Lest heat dissociation might cause an apparent increase in the amount of NO_2, we tried heating some of this gas in a flask. We observed that when hot the brown fumes became golden yellow, and

* Wilson and Gray, 'Roy. Soc. Proc.,' vol. 58; Violle, 'Journ. de Phys.,' 3rd series, vol. 2, 1893, p. 545.

† Wilson and Gray, 'Phil. Trans.,' A, vol. 185, 1894.

the absorption bands nearly disappeared, so that the heating could *not* have been the cause of the apparently enormous production of NO_2 at high pressure.

We next tried whether oxygen blown into the arc would burn up the carbons, but found it did not do so to any serious extent, and so tried the arc in a compressed atmosphere of this gas.

The arc burned very nicely indeed in the oxygen, the carbons keeping a good shape, and a very steady crater. The oxygen was, however, so contaminated with nitrogen that at high pressure enormous quantities of NO_2 were again formed, so that we could not proceed further with the radiation experiments. The arc was a bright blue bead, about the size of a pea, and the spectrum was a beautiful banded one.

From these results we concluded that the reduction of radiation, and red-hot appearance of the crater in the former experiments in nitrogen, were due to its being contaminated with oxygen and to the large quantities of NO_2, which were formed by the arc when under pressure.

We next tried the arc in hydrogen. The gas was obtained as pure, but contained hydrocarbons as an impurity, possibly from having been compressed into a cylinder which had previously been charged with coal-gas.

The arc in hydrogen at atmospheric pressures was a long, thin flame, that moved as far up the carbons as possible; especially on the negative carbon it walked up a cm. along the cone. It went so far that it fuzed the copper ring that held the negative carbon, and we had to replace it by an iron wire lashing. It was very unsteady, and trees of soot and a deposit of hard graphitic carbon formed on this positive carbon as if there were electrolysis of the hydrocarbon, and carbon were electro-negative compared with hydrogen. This growth took place all round the crater, while there was no tendency for anything to grow on the negative carbon.

The arc was only 5—6 mm. wide, and sometimes over 2 cm. long. There was a green outer flame, with a bright red line not a mm. wide down the middle of it. Where it impinged on the negative carbon there was a bright red flame from the middle of the bright spot on the carbon. The outer greenish part seemed to give much the same spectrum as the green cone in a Bunsen burner, while the red flame and line was undoubtedly glowing hydrogen. As we saw the C and F hydrogen lines very distinctly, the red C line being dazzlingly bright and not nearly so wide as in a coil spark at atmospheric pressure whenever the image of the red part of the arc was thrown on the slit of the spectroscope, the appearance was quite like that of a solar prominence.

The end of the positive carbon was pitted into a number of craters

as the arc was very unsteady, and when the pressure was raised it was almost impossible to keep an arc going, partly because the arc broke when it was elongated the least bit, and partly because a complete lantern of soot trees grew all round the crater, and seemed to short-circuit the arc from time to time.

The arc being very unsteady, no satisfactory reading of the voltage and current was possible. At from 60 to 80 lbs. pressure the voltage varied from 60—80, and the amperes kept continually varying from 15—20. At 40 lbs. with 20 amperes the volts varied from 50—60. The crater was not well developed, so that the radiation observation, even at low pressures, was not very satisfactory, while at high pressures the arc was too short to see into the crater at all, and the lantern of soot trees hid a considerable length, 3 or 4 mm. of the negative carbon besides. The radiomicrometer gave 440 divisions with a good arc in air, and 380 with the moderately good crater in hydrogen. But this difference is no greater than would often occur with a good and moderately good crater, so that there is not any proof of a difference of temperature due to cooling power of hydrogen. These experiments showed us that it was quite hopeless to get any measures of radiation under pressure with hydrogen.

We finally tried an atmosphere of carbon dioxide. We used a cylinder of liquid CO_2, which was connected to our arc box by a copper tube and stop valve. The arc burned fairly well in this gas, and, except for the difficulty of getting a sufficiently long arc at pressures above 150 lbs., some pretty satisfactory measures of radiation were obtained. We found that whenever the pressure was suddenly reduced, there was a fog formed in the box, which cut off the light enormously. Also by looking down the steel tube, which is closed at its end by a lens, we could see powerful convection currents in the gas which scattered a lot of light. At high pressure the refraction due to these currents prevented any sort of an image of the crater being formed while the pressure was varying. While the pressure was steady a good image could be formed. This tube is nearly 3 ft. in length, and only $\frac{1}{2}$ in. in bore, and it would naturally take time for the gas to settle down throughout its length. We propose to have this tube removed, and the aperture in the box closed by a strong piece of plain glass, and to form an image of the carbons by a lens placed at a suitable distance outside. This we expect will remove the difficulty arising from these convection currents.

The result of all these experiments so far is that it would require more evidence than we have been able to get, to affirm that either the temperature of the crater of the arc is raised or lowered by pressure. We got some very concordant observations, which showed the temperature to be lowered with pressure, and in which at the time we could see no evidence of absorption by fog, but then, at other

times, there was undoubtedly absorption from this cause. We certainly got no evidence that there is any appreciable increase of temperature. When the arc was started in the gas at a low pressure and then the pressure was raised, the radiation at the low pressure was greater than at a high pressure; but when the arc was started first in the gas at high pressure, and then the pressure reduced, the radiation was rather higher in the gas at high pressure. From all this we concluded that the greater part of the differences we were observing were due to the absorption of the light in the long tube already mentioned, which increased the longer the arc was kept burning, and was probably greater at high than at low pressures. The best observations were made with variations of pressure from 15 up to 100 lbs. per sq. in., and there seems very little evidence of much change of radiation with this change of from 1 up to between 6 and 7 atmos.

The whole question is surrounded with great difficulty. If the carbon be really in equilibrium with its own vapour at the temperature of the crater and at the pressure of the surrounding atmosphere, some relation must exist between the change in pressure and change in temperature of the crater. If we knew the latent heat of volatilisation of carbon, we should be able to calculate the change of temperature from the well-known thermodynamic formula

$$\frac{\delta T}{T} = \frac{\Delta v}{\lambda} \cdot \delta p.$$

Δv can certainly be approximately determined on the supposition that the absolute temperature of the crater is fifteen times the absolute temperature of the freezing point, *i.e.*, 3800. We thus get for gaseous carbon $\Delta v = 10^4$, *q.p.*, at this temperature. For 1 atmos. $\delta p = 10^6$, *q.p.*, so that

$$\frac{\delta T}{T} = \frac{10^{10}}{\lambda}.$$

Hence, unless the latent heat of carbon be enormously great compared with that of other substances, $\delta T/T$ will be considerable. If λ be as great as the latent heat of vaporisation of carbon given by Trouton's law, *i.e.*, about 4000 calories, or $16{\cdot}8 \times 10^{10}$ ergs, $\delta T/T$ would be about $\frac{1}{17}$, and δT would be nearly 220° C. for each atmosphere, and a change of pressure of about 18 atmos. would raise the temperature of the crater to that estimated for the sun. The corresponding increase of radiation would be very great, for the radiation varies, at least approximately, as the fourth power of the absolute temperature. This would lead one to expect that the radiation would be nearly doubled for each 4 atmos. added. Such an increase as this certainly does not take place, so that we may conclude that either the temperature of the crater is not that of boiling carbon,

or else that the latent heat of volatilisation of carbon is very considerably greater than that calculated from Trouton's law. Even though this latent heat were as great as the heat of combustion of C to CO_2, *i.e.*, 7770, there would be an increase of about 70 per cent. in the radiation for an increased pressure of 6 atmos. Such an enormous latent heat is unprecedented, and yet our experiments would, almost certainly, have shown such an increased radiation as this. So far, therefore, the experiments throw considerable doubt on the probability that it is the boiling point of carbon that determines the temperature of the crater. It might be questioned whether there is energy enough in the current to do all this work, but upon an extravagant estimate of the amount of carbon volatilised in the crater, it appears that there is more than a hundred times as much energy supplied by the current as would be required for volatilising the carbon, even though its latent heat were as great as the heat of combustion of C into CO_2.

There is another considerable difficulty in the theory of the temperature of the crater being that of boiling carbon arising from the slowness of evaporation. The crater on mercury is dark, but then it volatilises with immense rapidity and the supply of energy by the current being more than 100 times that required merely for evaporation, there seems very little reason why even a considerable difference in latent heat should make any sensible difference in the rate of evaporation of mercury and carbon, especially as, at the same temperature, the diffusion of carbon vapour is nearly three times as fast as that of mercury vapour and the temperature immensely higher.

We would, in conclusion, call attention to a cause of opacity in the solar atmosphere that is illustrated by the effect of convection currents in the long tube we were observing at high pressures; these convection currents behaved just like snow, or any other finely divided transparent body immersed in another of different refractive index. Light trying to get through is reflected backwards and forwards in every direction, until most of it gets back by the way it came. The consequence was that even the electric arc light was unable to penetrate the tube at high pressure, when these convection currents were active. The only light that came out of the tube was the feeble light outside, which was returned to us by reflection at the surfaces of these convection currents. In a similar manner we conceive that any part of the solar atmosphere which is at a high pressure, and where convection currents, or currents of different kinds of materials, are active, would reflect back to the sun any radiations coming from below, and reflect to us only the feeble radiations coming from interplanetary space. In his paper on "The Physical Constitution of the Sun and Stars" ('Roy. Soc. Proc.,' No. 105, 1868), Dr. Stoney called attention to an action of this kind that might be due to clouds of transparent

material, like clouds of water on the earth, but in view of the high solar temperature it seems improbable that any body, except, perhaps, carbon, could exist in any condition other than the gaseous state in the solar atmosphere; so that it seems more probable that sun-spots are due, at least partly, to reflection by convection streams of gas, rather than by clouds of transparent solid or liquid particles.

"Influence of Alterations of Temperature upon the Electrotonic Currents of Medullated Nerve."* By AUGUSTUS D. WALLER, M.D., F.R.S. Received December 14,—Read December 17, 1896.

(Abstract.)

The effects of a rise of temperature upon electrotonic currents may be briefly stated as follows:—

1. The ordinary electrotonic currents, A and K, are temporarily diminished or abolished at about 40°.

2. At about 30° of a rising temperature the K current is increased without notable alteration or with actual diminution of the A current.

3. On returning from 40° towards the normal (15° ± 2°) temperature, the A and K currents reappear. K is increased and A is diminished, so that the previous normal inequality A > K is diminished, or actually reversed to A < K. In all cases the quotient A/K is diminished; in some cases it actually falls below unity.

[The negative variation is temporarily abolished at about 40°; a positive gives place to a negative variation in consequence of a raised temperature to 40°.]

The above three statements are illustrated by Experiments 2366, 2322, and, from the examination of their records, it will be clear that there is here no question of the effects being due to alterations of resistance. A and K are tested for alternately, and the deflection by 0·001 volt is taken at intervals of about ten minutes. [Other examples of a similar character are given in the 'Proceedings of the Physiological Society' for November, 1896, and a record of temporary diminution of the negative variation is given in fig. 12 (Experiment 777), 'Phil. Trans.,' 1897.]

* In all the experiments referred to in this communication, the polarising current is by one Leclanché cell (the resistance in its circuit being about 100,000 ohms). The nerve lies upon four unpolarisable electrodes fixed at intervals of 12 mm., serving as leading-in electrodes to the polarising current and leading-out electrodes to the electrotonic current. On the galvanometer records, the anelectrotonic deflection A reads upwards, the katelectrotonic deflection K reads downwards; after-anelectrotonic and after-katelectrotonic deflections A′ and K′ read respectively downwards and upwards (there being under the conditions of experiment no marked homodromous after-katelectrotonic deflection).