

Note on some Photographs of Lightning and of "Black" Electric Sparks

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of agreement as to multiplicity he thought might be caused by their eyes being directed towards different parts of the sky when a multiple flash occurred ; the one who happened to be looking towards the flash might be conscious of only one impression, whereas the others in directing their eyes would receive the flashes on different parts of the retina. In some cases as many as three distinct flashes (occurring at intervals of about ten seconds) traversed the same path, and a number of the discharges presented a beaded or striated appearance. The beads seemed to remain after the main flash had faded, and this might account for the bands shown in Dr. Hoffert's photograph.

Mr. C. V. Boys, in referring to multiple flashes, said that although his statements made in the discussion of Mr. Whipple's paper on April 13th were not readily accepted, yet no one who watched the recent storm could doubt their existence.

Prof. S. P. Thompson thought the order of the flashes on the photograph may have been the reverse of that supposed ; for he observed that the bands of light extended on both sides of the (so-called) primary flash, whereas the outside of the third flash was quite dark.

Mr. E. W. Smith noticed many cases of "sympathetic discharge" in which a flash in the north seemed to precipitate another in the north-west within a few seconds ; and in this he was corroborated by Mr. Gregory, who viewed the storm from a different locality.

Mr. C. V. Burton thought the heating of the air by the first spark of a multiple flash might give rise to the tributaries to the succeeding sparks.

Mr. A. W. Ward mentioned a long flash observed at Cambridge which passed from the zenith and struck some farm-buildings at a distance, and he was particularly impressed by the considerable time occupied in its progress.

XXVIII. *Note on some Photographs of Lightning and of "Black" Electric Sparks.* By A. W. CLAYDEN*.

DURING the thunderstorm on the night of June 6 I exposed several plates in the hope of securing photographs of lightning. Three of these gave results.

* Read June 22, 1889.

One was exposed to two flashes, not counting such as did not cross the field of view. These two flashes show complicated and beautiful structure. One of them is a multiple flash, distinctly seen as double by both my wife and myself. An enlargement of this shows curious flame-like appendages pointing upwards from every angle. The other flash is a broad ribbon. The images of the masonry in the left-hand corner (which are necessarily slightly out of focus) show three positions of the camera. They are sharp, hence the camera did not move during the existence of a flash; and the directions of those movements which did occur do not in any way correspond to the movements (if such there were) which would have been required to produce the ribbon-like effect from a linear flash.

A second plate shows four flashes, and the camera moved much more than in the first case. None of these flashes are ribbons. Development showed the plate to be overexposed.

The third was exposed to six flashes; that is to say, I judged that six of them crossed the field of view. There were many others between times, which were either in the clouds or occurred in other parts of the sky. One flash, I remarked at the time, must be "right down the middle of the plate." Development showed this plate to be very much overexposed, and the image required careful nursing. I was much surprised to see nothing but one triple flash in the corner. I supposed that I must have mistaken the plate, and was about to throw it away, but on carefully searching for the above-mentioned vertical flash, I found its image was reversed, printing as a black flash with a white core. Subsequent observation showed other dark flashes; and the enlargement of part of the plate shows that there are indications of white cores to each of them.

Now the connexion between this reversal and overexposure was very striking. Hence it occurred to me that the black flashes might be due to a sort of cumulative action, not to the excessive brightness of the individual flashes, but rather to the excessive action produced by the superposition of the glare from an illuminated white cloud upon the normal image of the flash.

To test this I endeavoured to obtain the same effect with

the sparks from a small Wimshurst machine ; but, under the conditions in which I worked, I could not get a longer spark than one inch.

I first photographed a series of brilliant sparks, using two large Leyden jars. These gave normal images, very dense, and shaded off at the margins, although the focus, as shown by the knobs of the machine, was good.

Next I tried less brilliant sparks from the machine with its ordinary small jars. These gave similar images, but less dense.

Then I repeated both experiments, and before developing the plates exposed them to the diffused light from a gas-flame. The brilliant sparks then yielded images which may either be called normal with a reversed margin, or reversed with a normal core. The fainter sparks were completely reversed.

One plate of bright sparks was exposed to the gas-light, so that different parts were acted upon for different times. The reversal seems to spread inwards as the exposure to diffused light is increased.

One plate of faint sparks was only half of it exposed to diffused light. The result is that on that part the sparks are reversed, while on the other they are normal.

Finally I photographed a number of sparks in a series across the plate, and placed a sheet of white cardboard behind them to do duty for the white background of cloud. Some of the first sparks impressed on the plate show reversed images.

Coupling these experiments with the observations as to the overexposure of the "dark-flash" plate, and with the fact that all dark-flash plates I have seen show symptoms of considerable exposure, I submit that there is at least a good case for this theory of cumulative or repeated action producing the reversal. The partial reversal of the bright sparks seems to correspond with the bright core to some dark flashes; and the complete reversal of the less brilliant sparks to the absence of any such core from the less conspicuous portions of a dark flash.

There is certainly one difficulty yet to be got over, and that is the crossing of a dark flash by a bright one. However, I have some experiments* in view which I hope may throw some

* Since writing the above communication I have made a number of further experiments, which I hope to describe in detail at some future

light upon this also. In my own negative the point of crossing seems to be extra bright.

Meanwhile I must apologize to the Society for bringing forward these notes in such an immature and hastily constructed condition. My excuse must be that the photographs of electric sparks were only taken the day before yesterday, and today's meeting is the last of the session.

In the discussion which followed, Mr. W. N. Shaw exhibited a photograph taken during the same storm, which is particularly rich in dark flashes branching outwards from an intensely bright one. In some places the bright line has dark edges, and in one part a thin bright line runs along the middle of an otherwise dark portion of the flash. In answer to Mr. Inwards, Mr. Shaw said the plate was exposed about half a minute, and the former thought that under those conditions the appearance of the plate did not contradict Mr. Clayden's hypothesis.

Speaking of the same photograph, Prof. Perry considered that Mr. Clayden's observations would explain the result, for a bright flash required more exposure to diffused light to reverse it than a faint one did.

Prof. Ramsay reminded the meeting that Prof. Stokes's "oxides of nitrogen" explanation was still a possible one; and Mr. C. V. Burton asked whether they may be due to faint sparks cutting off light from brightly illuminated clouds, just as a gas-flame absorbs light from a brighter source.

In reply, Mr. Clayden thought the "oxides of nitrogen" hypothesis improbable, and said his experiments did not enable him to answer Mr. Burton's question. As regards Mr. Shaw's plate, he believed the diffused light from the clouds would be

time. But perhaps I may be allowed to say at once that I have succeeded in imitating the phenomenon of a bright image crossing a dark one. The experiments point to the conclusion that diffused light acting upon a plate can reverse previously impressed images of electric sparks, but is powerless to affect any such impressions which may be made afterwards. Similar results are obtained whether the source of the diffused light is a gas-flame, a lamp, or a series of sparks. I do not at present offer any theoretical explanation of these facts, but they are in themselves sufficient from a meteorological point of view. "Dark" flashes of lightning have no existence in nature, but are caused by the exposure of the plate to an illuminated sky *after* the passage of the flash. This illumination may be due to subsequent flashes, the more recent of which will give normal images possibly crossing the reversed ones.

sufficient to reverse the fainter tributary flashes, although it was insufficient to reverse the primary one.

From data obtained when the ribbon-flash was taken he had made some calculations, which gave the height of the clouds about 1000 yards and the ribbon-flash 1300 yards long and 100 yards wide.

XXIX. *Expansion with Rise of Temperature of Wires under Pulling Stress.* By J. T. BOTTOMLEY, M.A., F.R.S., F.C.S.*

[Plate V.]

It is probably well known to the members of the Physical Society that, at the instance of the British Association and with the assistance of a money grant from that body, very interesting secular experiments on the elasticity and ductility of wires were commenced some years ago in Glasgow. In the tower of the Glasgow University buildings certain wires are hung in pairs for comparison. One of each pair carries a heavy load about half the breaking weight of the wire; the other carries about one tenth of the breaking weight. Certain marks are put on the wires; and the object of the experiment is to find whether the heavily loaded wire seems, on comparison with the lightly loaded wire, to go on running down incessantly, or whether it comes asymptotically to a fixed length for a given temperature, ceasing to experience further permanent elongation.

The observations of the last few years show that the elongation due to further pulling out has, to say the least, become exceedingly small, so small that it is extremely difficult to observe it; and at the Aberdeen meeting of the British Association I pointed out that a great difficulty is introduced into the making of deductions from these observations through the impossibility of controlling the temperature of the tube in which the wires are placed. If, for example, there is any difference as to expansion with temperature of the same wire when lightly and when heavily loaded, a cause of disturbance would be introduced which it would be excessively difficult to allow for. It seemed therefore absolutely essential to make direct experiments on this point. The

* Read June 22, 1889.