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X. *On the Heat produced by Chlorine, and on a singular Effect produced by Lightning.* By JOHN MURRAY, F.L.S. M.W.S. 8c. 8c.

To Dr. Tilloch.

SIR, — DR. Hare, of Philadelphia, is stated to have found that though the thermometer immersed into a volume of chlorine gas is not affected, the sensation of heat on the skin announces it to be 90° to 100° F. This however I believe only takes place on opening the vessel in contact with atmospheric air.

This phænomenon is very ingeniously, and I believe justly, ascribed to a chemical action between the gas and the insensible perspiration of the skin.

Allow me to add that the same phænomena are manifested when a volume of nitrous gas is opened into the atmosphere: the thermometer rises only a fraction, but the sensation of heat on the hand is equal to that superinduced by chlorine.

My feelings deceive me if the same thing does not occur in the case of muriatic acid gas: the quantity which I collected over the mercurial cistern was contained in a small cylinder, and would not admit the immersion of my whole hand.

I have been much interested in reading an account of “a singular effect produced by a stroke of lightning on the 3d July last, communicated by Professor Pictet to the Helvetic Society.” The lightning is represented as having done no damage to the house, but “it perforated a piece of white iron with *two holes* of an inch in diameter, and five inches distant; and, what was very remarkable, the *burs at the edges of the holes were in opposite directions.*” Professor Pictet draws the conclusion, that either two electric currents had moved simultaneously, and in opposite directions, or that, after it had perforated the metal and moving five inches along it, had penetrated the plate in an opposite direction. I must confess, however, that *to me* the latter opinion seems to violate the principles of probability.

You may remember, sir, that some years ago you were good enough to insert in the Philosophical Magazine a communication of mine which detailed experiments *precisely similar* in the phænomena presented.

It may not be irrelevant to advert to two of them. A wire of the thickness of a knitting-needle rested on the table, one end being in contact with the exterior coating of the charged Leyden jar, a card rested on the wire, and a patch of tinfoil was placed over the further end of this wire: another card succeeded, and a similar patch of tinfoil with a card and wire above, terminated

minated the arrangement. The patches of tinfoil, and of course the ends of the wires which terminated below and above them, were separated from each other about two inches. When the electric discharge was made by placing one of the balls of the insulated discharging rod in contact with the further end of the upper wire, &c. the under patch of tinfoil exhibited an *indent upward*; and the upper patch an *indent downward*: they were frequently perforated, and the burs similarly directed. If I used *two wires below*, parallel to each other, say $\frac{3}{4}$ ths inch apart, and *one wire above*, then had I two indents—or two burs directed upwards, and one indent or one bur downwards; and on the other hand, when I employed *two wires above and one below*, the phænomena were reversed, there being *two burs or indents from above*, and *one only from below*.

In this detail there is described another phænomenon not less interesting. I coated uniformly with *China ink* the ball of the rod connected with the internal metallic surface of the Leyden jar, as well as that of one of the balls of the jointed discharger, and placed a vertical card between these balls. When I made the discharge, the card exhibited the usual appearance of a bur raised on both sides. But independently of this, I found a *circular portion of the China ink displaced from both of these balls, and an indent in each of them*. Each ball was an *inch distant* from the respective surfaces of the interposed card, and the discharge was a powerful one. It was on these that I founded the opinion that Mons. Moll's experiment was only a modification of mine; and I have transmitted to Professor Moll of Utrecht a note of these experiments, agreeably to his request to me.

It is highly satisfactory for me to receive, after the lapse of a few years, the verification of these experiments, sealed as it were by the signet of celestial fire.

I have the honour to be, sir,

Your obliged and humble servant,

May 15, 1822.

J. MURRAY.

XI. *A Reply to Mr. JOHN MURRAY, "on the Apparatus for restoring the Action of the Lungs in apparent Death."* By Mr. JOHN MOORE, Jun.

To Dr. Tilloch.

SIR, — **I**s it not singular that Mr. Murray should have been so displeased with me, for the notice I took of his machine for producing artificial respiration, when he has given an invitation to the public to completely investigate it; requesting them

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