

The electro-chemical system, as established by Berzelius, places oxygen at the head of what are called the electro-negative elements, and puts chlorine in a positive relation to that body. It is however a well-known fact, that in a great number of cases, chlorine, with respect to its electro-negative activity, very much surpasses oxygen. Chlorine is capable of expelling oxygen from a great number of oxides; chlorine eliminates bromine and iodine from the bromides and iodides, whilst pure oxygen seems not to act upon those compounds. In spite of these exalted electro-negative powers possessed by chlorine, it is apt to produce with oxygen a series of acids in which chlorine is said to act the part of an electro-positive principle. I ask, how is it possible that in some cases chlorine proves to be a more electro-negative body, and in another instance a more positive one than oxygen? It seems to me that the facts alluded to are very little in accordance with the principles of the electro-chemical system; nay, that they imply a direct contradiction to them, provided chlorine be taken for a simple substance. But if we consider it as oxymuriatic acid, all the anomalies spoken of disappear, and we can easily account for the reactions produced by chlorine. The oxygen disengaged, for instance, during the reaction of chlorine upon potash, comes from the oxygenized muriatic acid, and not from the potash; and the decomposition of the former is effected by the strong affinity which potash has for muriatic acid. Those who maintain that there is some truth in the principles of the electro-chemical system, must, I think, be inclined to readopt the views of Berthollet, in order to save their endangered theory.

Before concluding, I shall take the liberty to add some general considerations regarding the subject I have treated of. By regarding chlorine as an elementary substance, one of the greatest theoretical changes that ever took place in the history of chemistry was effected. Indeed, by Davy's theory, oxygen lost, if I may say so, its royal dignity amongst the elements; and at any rate a very powerful rival was set up in chlorine. Oxygen was no more the body exclusively enjoying the privilege of being the generator of acids, the supporter of combustion, &c.; that privilege was also claimed for the upstart. But if there should be found good reasons for giving up Davy's hypothesis and readopting the old doctrine, we could hardly help restoring oxygen to its ancient dignities, and considering it again as an agent that has not its equal amongst the rest of the elementary bodies. To speak without metaphor, oxygen would become again one of the centres of chemistry, as it formerly was, and as most likely hydrogen

may be. Indeed both substances bear so much the stamp of simplicity, they exhibit so decided a chemical antagonism towards each other, both of them, conjointly with their remarkable product "water," act throughout the domain of our science so extensive and important a part, that we can hardly help suspecting them to be active in some way or other in most, if not in all chemical reactions, and seeing in oxygen and hydrogen the hinges upon which the whole chemical world turns. The theory of Davy, seducing and plausible as it appears at first sight, has possibly proved a check, rather than a spur, to the development of chemistry, on account of its having changed, perhaps, the true point of view from which oxygen ought to be looked at. For if oxygen should happen to act that all-important part which Lavoisier and the chemists of the last century assigned to that element, it is not difficult to see that the views of Davy are calculated to retard the progress of theoretical chemistry rather than to accelerate it.

I need not say, that the considerations I have taken the liberty to submit to you have been entered into with the view only of drawing the attention of philosophers towards a subject which seems to me to be of considerable theoretical importance, and worthy of our study.

C. F. SCHÖNBEIN.

XXXI. *On the Existence of an Equivalent Relation between Heat and the ordinary Forms of Mechanical Power.* By JAMES P. JOULE, Esq.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE principal part of this letter was brought under the notice of the British Association at its last meeting at Cambridge. I have hitherto hesitated to give it further publication, not because I was in any degree doubtful of the conclusions at which I had arrived, but because I intended to make a slight alteration in the apparatus calculated to give still greater precision to the experiments. Being unable, however, just at present to spare the time necessary to fulfil this design, and being at the same time most anxious to convince the scientific world of the truth of the positions I have maintained, I hope you will do me the favour of publishing this letter in your excellent Magazine.

The apparatus exhibited before the Association consisted of a brass paddle-wheel working *horizontally* in a can of water. Motion could be communicated to this paddle by means of

weights, pulleys, &c., exactly in the manner described in a previous paper*.

The paddle moved with great resistance in the can of water, so that the weights (each of four pounds) descended at the slow rate of about one foot per second. The height of the pulleys from the ground was twelve yards, and consequently, when the weights had descended through that distance, they had to be wound up again in order to renew the motion of the paddle. After this operation had been repeated sixteen times, the increase of the temperature of the water was ascertained by means of a very sensible and accurate thermometer.

A series of nine experiments was performed in the above manner, and nine experiments were made in order to eliminate the cooling or heating effects of the atmosphere. After reducing the result to the capacity for heat of a pound of water, it appeared that for each degree of heat evolved by the friction of water, a mechanical power equal to that which can raise a weight of 890 lbs. to the height of one foot, had been expended.

The equivalents I have already obtained are,—1st, 823 lbs., derived from magneto-electrical experiments†; 2nd, 795 lbs., deduced from the cold produced by the rarefaction of air‡; and 3rd, 774 lbs. from experiments (hitherto unpublished) on the motion of water through narrow tubes. This last class of experiments being similar to that with the paddle-wheel, we may take the mean of 774 and 890, or 832 lbs., as the equivalent derived from the friction of water. In such delicate experiments, where one hardly ever collects more than half a degree of heat, greater accordance of the results with one another than that above exhibited could hardly have been expected. I may therefore conclude that the existence of an equivalent relation between heat and the ordinary forms of mechanical power is proved; and assume 817 lbs., the mean of the results of three distinct classes of experiments, as the equivalent, until still more accurate experiments shall have been made.

Any of your readers who are so fortunate as to reside amid the romantic scenery of Wales or Scotland, could, I doubt not, confirm my experiments by trying the temperature of the water at the top and at the bottom of a cascade. If my views be correct, a fall of 817 feet will of course generate one de-

* Phil. Mag. vol. xxiii. p. 436. The paddle-wheel used by Rennie in his experiments on the friction of water (Phil. Trans. 1831, plate xi. fig. 1) was somewhat similar to mine. I employed, however, a greater number of "floats," and also a corresponding number of stationary floats, in order to prevent the rotatory motion of the water in the can.

† Phil. Mag. vol. xxiii. pp. 263, 347. ‡ Phil. Mag. May 1845. p. 369.

gree of heat; and the temperature of the river Niagara will be raised about one-fifth of a degree by its fall of 160 feet.

Admitting the correctness of the equivalent I have named, it is obvious that the *vis viva* of the particles of a pound of water at (say) 51° , is equal to the *vis viva* possessed by a pound of water at 50° plus the *vis viva* which would be acquired by a weight of 817 lbs. after falling through the perpendicular height of one foot.

Assuming that the expansion of elastic fluids on the removal of pressure is owing to the centrifugal force of revolving atmospheres of electricity, we can easily estimate the absolute quantity of heat in matter. For in an elastic fluid the pressure will be proportional to the square of the velocity of the revolving atmospheres; and the *vis viva* of the atmospheres will also be proportional to the square of their velocity; consequently the pressure will be proportional to the *vis viva*. Now the ratio of the pressures of elastic fluids at the temperatures 32° and 33° is 480:481, consequently the zero of temperature must be 480° below the freezing-point of water.

We see then what an enormous quantity of *vis viva* exists in matter. A single pound of water at 60° must possess $480^{\circ} + 28^{\circ} = 508^{\circ}$ of heat, in other words, it must possess a *vis viva* equal to that acquired by a weight of 415036 lbs. after falling through the perpendicular height of one foot. The velocity with which the atmospheres of electricity must revolve in order to present this enormous amount of *vis viva*, must of course be prodigious, and equal probably to the velocity of light in the planetary space, or to that of an electric discharge as determined by the experiments of Wheatstone.

I remain, Gentlemen,

Oak Field, near Manchester,
August 6, 1845.

Yours respectfully,
JAMES P. JOULE.

XXXII. *An Examination of Dr. Dalton's New Method of Measuring the Water of Crystallization contained in different varieties of Salts.* By SAMUEL HOLKER, Bury*.

THE subject of this paper was suggested to me by my friend Mr. Davies, Professor of Chemistry at the Royal Medical School, with whom I have for some time devoted myself to the study of chemistry. I may observe as a rather remarkable circumstance, that an inquiry of so much interest and importance, commenced by Dr. Dalton, and displaying

* Read at the Manchester Literary and Philosophical Society, February 20, 1844; and communicated by the Author.