

stage, it probably became more violent, from an accession of air, and a communication with the atmosphere, so as to produce the luminous explosion. Not having gunpowder enough to account for the phenomena that occurred, I have hazarded this novel opinion with much diffidence, but with the concurrence of the Gentleman who favoured me with his assistance in examining the apprentices, and agreed with me in his judgment of the case.

It is said that some flashes of lightning were seen that night in this neighbourhood, without thunder; but the apprentices had not observed it, and two or three other persons, who supposed they had seen lightning about the time the explosion happened, were afterwards convinced that they had mistaken for it the light caused by the fire in the ship. They heard no thunder. Besides, it does not appear that the ordinary effects of lightning on a ship agree with the phenomena that occurred on this occasion.

I have since heard that many years ago a ship laden with coals from Sunderland took fire, and was burnt at Portsmouth or Plymouth; and that the damage was commonly supposed to have been caused by the sulphur of the coal taking fire, as had been often noticed in the heaps of waste round coal-pits. The production and nature of carbureted hydrogen gas were then little known, but it probably was the cause of this ship being burnt.

Bishop Wearmouth, June 21, 1816.

H. PEMBERTON.

VI. Curious Galvanic Experiments. By Mr. Porrett.

(To Dr. Thomson.)

DEAR SIR,

Tower, June 6, 1816.

The conversation which took place at your house the other evening on the subject of voltaic arrangements, brought to my recollection two experiments that I made about 18 months since, which appear to me to be interesting, the second one especially. I did not then publish them, conceiving that at some future time, when leisure and inclination should coincide, I might follow up with some advantage the new facts which these elementary experiments disclosed; but as it continues to be very uncertain when this coincidence will take place, and still more so whether I should be able to do any thing like justice to the subject, I will no longer be induced to delay giving the information which is now in my power, by the hope (probably fallacious) of hereafter making it more complete. The experiments are, therefore, quite at your service for publication, in case you think proper to honour them with a place in your *Annals*. They are as follow:—

Exper. 1.—A small battery of 50 pairs, of $1\frac{1}{4}$ inch double plates, was put into action by diluted muriatic acid. The wires from its two extremities were immersed in water, the decomposition of which of course took place rapidly. The battery was left thus disposed until its action on the water had nearly ceased to be visible. At this period the greater part of the liquid contained in the cells of the battery was withdrawn by means of a syringe, only about $\frac{1}{4}$

of the original quantity being left. The effect of this abstraction was a renewal of the rapid decomposing action on the water, which the battery exercised at the commencement of the experiment. I ascertained that this extraordinary effect was not occasioned by the increased distance between the surfaces of the liquid in the cells, by constructing a battery with very deep cells, from which I could withdraw as much liquid as before, but without uncovering the plates, an operation which I found to be quite inefficacious. It appears to me, therefore, that the explanation of this fact must be sought for in the action of the atmosphere upon the humid surfaces of the metallic plates.

Exper. 2.—I took an ounce medicine phial, and with a red-hot rod of iron cut it in a horizontal direction, so as to form the lower part into a small jar. I threw away the upper part, and divided the small jar into two equal parts in the direction of its length, so as to make a vertical section of it. The two halves of the jar were then pressed together in their original position, having first interposed a piece of moistened bladder. All the parts of the bladder which protruded beyond the outside of the jar were then cut away; and when this was completed, melted sealing-wax was run down the outer edge of the bladder, and thus the two halves of the glass vessel were firmly united. By this means the inside of the glass jar was divided into two cells, by the bladder interposed between them.

One of these cells having been filled with water, and left for several hours, was found to have retained the water. The bladder, therefore, was not sufficiently porous to allow the water to filtrate through it. The cell filled with water was now positively electrified, with a battery of 80 pairs of $1\frac{1}{4}$ inch double plates, and a few drops of water were put into the empty cell, so as to cover the bottom of it. This small quantity of water was then negatively electrified. The phenomena which ensued were exceedingly curious and instructive. Independent of the decomposition of a small part of the water, which of course took place in the usual manner, the principal part of it obeyed the impulse of the voltaic current *from the positive to the negative wire*, first overcoming the resistance occasioned by the compact texture of the bladder, so as in about half an hour to have brought the water in both cells to the same level, and afterwards overcoming the additional resistance occasioned by the gravitation of the water, by continuing to convey that fluid into the negative cell, until its surface in that cell was upwards of $\frac{3}{4}$ of an inch higher than in the positive cell. A much greater difference of level might doubtless be obtained by operating with a larger apparatus, and for a longer time; but the results are perfectly conclusive when the experiment is performed on the small scale in which I tried it.

I have repeated the above experiment several times, and invariably found the liquid, whatever it was, descend on the side posi-

tively electrified, and ascend on that negatively electrified, chemical changes at the same time going on, as in the celebrated transfer experiments of Sir H. Davy; but those experiments could not show the mechanical action of the voltaic current, consequently only the chemical action was observed in them. To render the mechanical action evident, it is an indispensable condition that there should be interposed between the positively and negatively electrified liquids a body which, although porous, is yet sufficiently compact to prevent filtration taking place in common circumstances. Bladder answers this condition. I do not think, however, that it does so as well as filtering paper that has been prepared in the following manner, suggested to me by my very ingenious friend Mr. Wilson, of Guy's Hospital:—Spread the white of an egg thinly upon filtering paper; then immerse the paper into boiling water, so as to coagulate the albumen; it is then well adapted for these experiments. Thick paper of a very compact texture would probably do without this preparation; but I cannot state this positively, not having tried it.

I think that by the above experiment I have demonstrated the existence of a power not before noticed in the voltaic current, namely, that of conveying fluids through minute pores not otherwise pervious to them, and of overcoming the force of gravity.

Is not this electro-filtration, jointly with electro-chemical action, in constant operation in the minute vessels and pores of the animal system?

I wish that some person well versed in the sciences of anatomy, chemistry, and electricity, would answer this question. I am not qualified to attempt its solution, being a stranger to the first mentioned science, and possessing but a moderate knowledge of the other two; and it appears to me that only a proficient in all should venture to propose any new physiological opinions, but I cannot help thinking that an affirmative answer to the above question is capable of a good defence.

To those who may be inclined to repeat the preceding experiment, it may be useful to mention that by letting fall from a dropping tube a little sulphuric acid into the cells of the battery occasionally, its action is prolonged, without the trouble of renewing the liquid in the cells, or the inconvenience of disturbing the whole arrangement, the partial action of this dense acid on the plates is prevented by stirring the liquid afterwards with a little stick.

I remain, with much respect, dear Sir,

Yours very truly,

R. PORRETT, jun.

VII. *Prize proposed by the Royal Medical Society of Edinburgh.*

“What are the chemical changes produced in the air by the growth of plants, and do they on the whole purify or deteriorate the atmosphere?”

A set of books, or a medal of five guineas value, shall be given