



# LXVII. Further experiments on the electricity of steam

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allowed the steam to expand in a very great degree before it issued into the air, and caused it to be discharged in the state of low-pressure steam; but no diminution of electricity could be perceived in the jet, when thus attenuated; so that the electrical development does not appear to depend upon the degree of violence with which the steam comes in contact with the atmosphere.

The entire absence of negative electricity seemed to preclude the possibility of the phænomena arising from expansion, and the only remaining supposition appeared to be, that the condensation which took place in the jet, set free the electricity which the steam had absorbed in the process of evaporation. This supposition had been previously rendered probable, when it was discovered that the upper and most opaque part of the jet yielded the most electricity, although I was at first inclined to attribute that circumstance to the dampness of the steam, in that part of the jet, rendering it a better conductor, and causing it to part more readily with its electricity. Experiments were next, therefore, commenced to ascertain the effect of insulating the boiler, and wholly condensing the steam; but these require repetition before they can be much relied upon. The great difficulty is to effect insulation amidst so much moisture, but I have no doubt that with a little perseverance this object will be accomplished, and I trust I shall be able to furnish, in time for insertion in the next Number of the *Philosophical Magazine*, such further results as will set the question at rest.

I am, yours, &c.

Newcastle upon-Tyne, Nov. 18, 1840. WM. GEO. ARMSTRONG.

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LXVII. *Further Experiments on the Electricity of Steam.*

By H. L. PATTINSON, Esq., F.G.S.

*To the Editors of the Philosophical Magazine and Journal.*

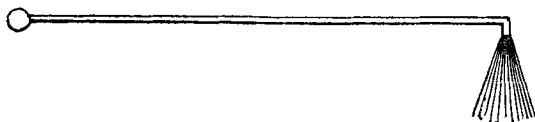
GENTLEMEN,

SINCE my last letter to you, dated the 19th ult. (published at p. 375 of this volume), relative to the electricity of steam issuing from two boilers at Cramlington Colliery, the subject has been further pursued both by myself and others, and sparks have been obtained from the steam of various boilers, in every direction. The mode of operating has generally been that described in my letter, viz. suffering the steam to escape from the safety-valve of the boiler tried, and testing its electricity by holding in it a shovel or an iron rod, the operator standing upon an insulating stool. Sometimes the indications have been very slight, and sometimes there

has hardly been any appearance of electricity in the steam ; but in such cases the trials have been generally made under unfavourable circumstances, and from all that has yet been done, the presumption is certainly that steam is always more or less electrical. It is not, however, always electrical to the same extent under the same pressure, as I shall presently show.

Mr. Armstrong was the first to experiment with a locomotive engine-boiler (one used on the Newcastle and North Shields railway), from which he obtained very striking results. The directors of the Newcastle and Carlisle railway, through their secretary, Mr. Adamson, gave me permission to experiment upon the boilers of the locomotive engines on that line, and I now beg to lay before you the results I have obtained. In preparing for, and performing these experiments, I have, as before, been assisted and accompanied by Mr. Henry Smith, and I have received the most willing and efficient aid from Mr. Anthony Hall of Blagdon, the mechanical engineer on the railway.

1. A copper rod, half an inch in diameter, and five feet long, was provided, made hollow for lightness ; this was terminated at one end by a two-inch ball, and at the other (which was bent at a right angle) by ten or twelve sharp-pointed wires, spread out in every direction to collect the electricity more perfectly from the steam.



2. The Wellington locomotive engine, immediately after coming to the station with passengers, was first tried. At this time the steam was blowing forcibly out of the safety-valve, at a pressure of fifty-two pounds per inch. On holding the pointed conductor in this current of steam, with its points downwards, the individual holding it standing at the time on an insulating stool, sparks three to four inches long were given off from his person to the boiler. The sparks were largest when the valve was held down a minute or two and then suddenly lifted, so as to suffer a large volume of steam to escape with great rapidity. By this management the sparks were frequently four inches long, and occasioned considerable pain to the person on the stool, even when given from a brass ball held in his hand. The sparks were largest when the points of the conductor were held in the steam about two feet above the valve ; but larger sparks were ob-

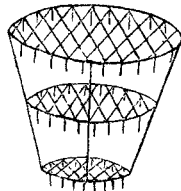
tained when it was held much higher; and indeed sparks were obtained by holding the conductor entirely out of the cloud of steam, and at a distance from it, for the air in the wooden shed in which we operated became speedily electrical throughout. The electricity was positive.

3. The steam in the boiler was now gradually run down to see how the electrical condition would vary with the pressure. At forty pounds per inch the sparks became much less, the largest not reaching three inches. At thirty pounds the largest spark did not reach two inches; at twenty pounds it became barely an inch; at ten pounds not more than from one-fourth to one-half of an inch; and at five pounds per inch pressure the spark was hardly perceptible. But if at any pressure the valve was held down a few minutes so as to suffer the steam to accumulate and then suddenly opened, there was always a great increase, for an instant, of the electrical effects.

4. Another boiler, that of the Lightning engine, which had also just come in from a trip, and had its steam blowing off forcibly, at a pressure of fifty pounds per inch, was now tried in exactly the same way as the Wellington. On holding the pointed conductor in the steam, whether regularly blowing off at the valve or escaping with great rapidity from the sudden lifting of the valve, it did not yield a spark more than one-fourth of an inch long. We then blew a quantity of water out of the boiler of the Lightning until it barely covered the tubes inside, and on afterwards testing its steam blowing off at fifty pounds per inch, the spark was found increased to nearly two inches in length. The steam of the Lightning was, however, much less electrical than the steam of the Wellington at the same pressure, under all the circumstances of our experiments.

5. The strong current of steam and water issuing from the boiler of the Lightning when the water was blown out of it as just stated, was tested for electricity, but no indications could be perceived whatever.

6. A very large conductor had been provided, made of zinc two-inch tubing, in this way,—three rings were made of this tubing, respectively three feet, two feet, and one foot diameter. These rings were attached to each other a foot and a half apart by side pieces, so as to form a hollow frustum of a cone, three feet high, with ends three feet and one foot diameter respectively. The inside of this cone was laced across with copper wire, and the whole bristled with pointed wires in every direction. By means of a long



iron bar, placed upright in a cask of rosin (both to insulate it and to serve as a foot), and a horizontal arm projecting from it, made to slide up and down on the vertical bar, the large conductor could be placed in any part of the cloud of steam issuing from the valve, and the electricity given off could be conveyed from it in any direction. Care was taken to round off all parts of this conductor, so as to avoid sharp points and angles as much as possible. On trying this large conductor in the current of steam from the Wellington, we were disappointed to find that it did not yield a longer spark than the small pointed copper rod with which we had previously experimented. The spark was larger in volume, but it did not possess greater intensity. It never struck through more than three inches of space, but its effect upon the person when taken was very violent and painful. Our intention was to have ascertained the rate at which large jars could be charged from the steam, in order to form some idea of the quantity of electricity given off; but the evening had become very damp, and the air was so moist, that we could not procure sufficient insulation, and were obliged to relinquish the attempt.

7. When the large conductor was held in the cloud of steam with its lower part or apex about two feet above the valve, it gave off numerous and powerful sparks; but if at this time the points of the small conductor were placed by a person connected with the ground in the steam below the large conductor a foot above the valve, the electricity given off by the large conductor was very materially diminished.

8. By means of screws, the entire engine (the Wellington) was raised off the rails and placed upon blocks of baked wood, so as to insulate it entirely. The steam being now blown off at the valve, the boiler and engine became strongly electrical with negative electricity; points placed upon any part of the engine exhibiting the peculiar star of the negative element, and threads suspended from the engine being repelled by excited sealing-wax. The steam was at the same time strongly positive, and when a point connected with the conductor held in the steam was brought near a point attached to the insulated boiler, the pencil upon the former and star upon the latter were beautifully decisive as to the electrical states of each.

9. I repeated Volta's experiment by placing a hot cinder upon the cap of a gold-leaf electrometer, and projecting a few drops of water upon it, when the leaves diverged strongly with negative electricity. I observed, that when the cinder was very hot, and the production of the steam consequently very rapid, the electricity given out was always most powerful.

10. I then insulated an iron pan, twelve inches diameter and two inches deep, and attached to it a pith-ball electrometer, with balls three-eighths of an inch diameter, and threads five inches long, and also attached to the pan a metallic wire, the pointed extremity of which was placed about one-twentieth of an inch distant from the point of another wire connected with the ground. The iron pan was then filled with cinders, very hot, from a wind-furnace, and on projecting upon them a few ounces of water, steam was evolved with great rapidity, and at the same moment the pith balls diverged to the distance of an inch, and sparks passed between the metallic wires. This was several times repeated.

These experiments enable us, I conceive, to give a clear explanation of the electrical phenomena presented by steam. There is no doubt whatever, as Dr. Faraday conjectures in his note to Mr. Armstrong's paper in your last Number, "that this evolution of electricity by vaporization is the same as that already known to philosophers on a much smaller scale." The electricity appears to originate at the instant of vaporization, and the steam as it collects within the boiler is electrified with positive electricity, the water and metallic boiler being at the same time negative. In this condition the electricity of both is latent, like the electricity of the two plates of an excited electrophorus; but the instant steam is suffered to escape, its positive electricity, being carried off along with it, and out of the influence of the equivalent quantity of negative electricity in the boiler, becomes free, and hence the steam is electrical with positive electricity. The same thing takes place with the boiler, in which negative electricity is set at liberty as the steam escapes, and which becomes evident on insulating the boiler.

When steam much mixed with water, or what engine-men call "wet steam," escapes from a boiler, it evidently cannot be very highly electrical, for the negative water will tend to neutralize the positive steam, and this may perhaps in some measure account for the increased effect in the Lightning on lowering the water within its boiler, and for the increase of intensity in every boiler, observed when the valve has been forcibly held down and is suddenly opened; but it does not seem sufficient to account *entirely* for these variations of intensity, nor for the difference of intensity in different boilers at the same pressure. It is therefore probable that chemical action between the metal of the boiler and the water has something to do with exalting the electrical condition of the steam at the moment it is generated; but this part of the subject certainly requires further investigation. By far the most powerful ef-

fects up to this time have been obtained from locomotive engines, in which water is heated in contact with brass tubes. How far this may influence the production of electricity, further experiments must determine. It is certainly somewhat curious to consider the splendid locomotive engines we see daily in the light of enormous electrical machines; but this they undoubtedly are; the steam is analogous to the glass plate of an ordinary machine, the boiler to the rubbers; and a conductor properly exposed to the escaping steam gives out torrents of electricity.

I am, Gentlemen,

Your obedient Servant,

Bentham-Grove, Gateshead,  
November 21, 1840.

H. L. PATTINSON.

LXVIII. *On the Motion of a small Sphere vibrating in a resisting Medium.* By the Rev. J. CHALLIS, Plumian Professor of Astronomy in the University of Cambridge\*.

IN the London and Edinburgh Philosophical Magazine for September, 1833 (vol. iii. p. 186.), I have given a solution of the problem of the resistance to the motion of a ball-pendulum vibrating in the air, by making use of the principle of the conservation of *vis viva*, and assuming that for slow vibrations the motion of the air surrounding the ball is the same as if the fluid were incompressible. I have given another solution in the Cambridge Philosophical Transactions (vol. v. part ii. p. 200.), by adopting the above assumption without using the principle of the conservation of *vis viva*; and in the latter solution it is not taken for granted, as in the other, that the same considerations apply to fluid motion directed to or from a moving centre, as to motion to or from a fixed centre. The two methods lead to the same result. In 1835, M. Plana published at Turin a Memoir (for a copy of which I am indebted to the kindness of the author) containing a solution of the problem in question, the same in principle as that of Poisson in vol. xi. of the *Mémoires* of the Paris Academy of Sciences†, with the difference of treating separately the motions in a compressible and an incompressible fluid, and so obviating some objections to which Poisson's reasoning appeared liable. M. Plana adverts to my communication in the Philosophical Magazine, and subjoins a translation of it, but is unwilling to admit the correctness of the principle of the method I have employed, apparently for no other reason than that it leads to a result differing from his own.

\* Communicated by the Author.

† Poisson's memoir is also inserted in the *Connaissance des Temps* for 1834.