LIX. Experimental researches in electricity.—Third series

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To cite this article: Michael Faraday D.C.L. F.R.S. M.R.I. (1833) LIX. Experimental researches in electricity.—Third series, *Philosophical Magazine Series 3*, 3:17, 353-363, DOI: [10.1080/14786443308648200](https://doi.org/10.1080/14786443308648200)

To link to this article: <http://dx.doi.org/10.1080/14786443308648200>

Published online: 01 Jun 2009.

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With respect to the explosion of the shell being audible at a great depth, the experiment at Geneva is not any proof; for if I recollect aright, that was *superficial* sound, not perpendicular *sound*,—a difference of considerable consequence.

Palace Yard, Sept. 23, 1833.

T. R. F.

LIX. *Experimental Researches in Electricity*.—*Third Series*.
By MICHAEL FARADAY, D.C.L. F.R.S. M.R.I. *Fullerian*
Prof. Chem. Royal Institution, Corr. Mem. Royal Acad. of
Sciences, Paris, Petersburg, &c. &c.

[Concluded from p. 262.]

III. *Magneto-Electricity*.

343. *Tension*.—THE attractions and repulsions due to the tension of electricity have been well observed with that evolved by magneto-electric induction. M. Pixii, by using an apparatus, clever in its construction and powerful in its action*, was able to obtain great divergence of the gold leaves of an electrometer†.

344. *In motion*: i. *Evolution of Heat*.—The current produced by magneto-electric induction, can heat a wire in the manner of ordinary electricity. At the British Association of Science at Oxford, in June of the present year, I had the pleasure, in conjunction with Mr. Harris, Professor Daniell, Mr. Duncan, and others, of making an experiment, for which the great magnet in the museum, Mr. Harris's new electrometer (287.), and the magneto-electric coil described in my first paper (34.), were put in requisition. The latter had been modified in the manner I have elsewhere described‡, so as to produce an electric spark when its contact with the magnet was made or broken. The terminations of the spiral, adjusted so as to have their contact with each other broken when the spark was to pass, were connected with the wire in the electrometer, and it was found that each time the magnetic contact was made and broken, expansion of the air within the instrument occurred, indicating an increase, at the moment, of the temperature of the wire.

345. ii. *Magnetism*.—These currents were discovered by their magnetic power.

346. iii. *Chemical Decomposition*.—I have made many endeavours to effect chemical decomposition by magneto-electricity.

† *Annales de Chimie*, l. p. 322.

† *Ibid.* li. p. 77.

‡ *Phil. Mag. and Annals*, N.S. 1832, vol. xi. p. 405.

city, but unavailingly. In July last I received an anonymous letter (which has since been published *,) describing a magneto-electric apparatus, by which the decomposition of water was effected. As the term "guarded points" is used, I suppose the apparatus to have been Wollaston's (327. &c.), in which case the results did not indicate polar electro-chemical decomposition. Signor Botto has recently published certain results which he has obtained†; but they are, as at present described, inconclusive. The apparatus he used was apparently that of Dr. Wollaston, which gives only fallacious indications (327. &c.). As magneto-electricity can produce sparks, it would be able to show the effects proper to this apparatus. The apparatus of M. Pixii already referred to (343.) has however, in the hands of himself‡ and M. Hachette§, given decisive chemical results, so as to complete this link in the chain of evidence. Water was decomposed by it, and the oxygen and hydrogen obtained in separate tubes according to the law governing volta-electric and machine-electric decomposition.

347. iv. *Physiological Effects*.—A frog was convulsed in the earliest experiments on these currents (56.). The sensation upon the tongue, and the flash before the eyes, which I at first obtained only in a feeble degree (56.), have been since exalted by more powerful apparatus, so as to become even disagreeable.

348. v. *Spark*.—The feeble spark which I first obtained with these currents (32.), has been varied and strengthened by Signori Nobili and Antinori, and others, so as to leave no doubt as to its identity with the common electric spark.

IV. *Thermo-Electricity*.

349. With regard to thermo-electricity, (that beautiful form of electricity discovered by Seebeck,) the very conditions under which it is excited are such as to give no ground for expecting that it can be raised like common electricity to any high degree of tension; the effects, therefore, due to that state are not to be expected. The sum of evidence respecting its analogy to the electricities already described, is, I believe, as follows:—*Tension*. The attractions and repulsions due to a certain degree of tension have not been observed. *In currents*: i. *Evolution of Heat*. I am not aware that its power of raising temperature has been observed. ii. *Magnetism*. It was discovered, and is best recognised, by its magnetic powers.

* Lond. and Edinb. Phil. Mag. and Journ. 1832, vol. i. p. 161.

† Ibid. 1832, vol. i. p. 441.

‡ *Annales de Chimie*, li. p. 77.

§ Ibid. li. p. 72.

iii. *Chemical Decomposition* has not been effected by it. iv. *Physiological Effects*. Nobili has shown* that these currents are able to cause contractions in the limbs of a frog. v. *Spark*. The spark has not yet been seen.

350. Thus only those effects are weak or deficient which depend upon a certain high degree of intensity; and if common electricity be reduced in that quality to a similar degree with the thermo-electricity, it can produce no effects beyond the latter.

V. Animal Electricity.

351. After an examination of the experiments of Walsh †, Ingenhousz ‡, Cavendish §, Sir H. Davy ||, and Dr. Davy ¶, no doubt remains on my mind as to the identity of the electricity of the torpedo with common and voltaic electricity; and I presume that so little will remain on the minds of others as to justify my refraining from entering at length into the philosophical proofs of that identity. The doubts raised by Sir H. Davy have been removed by his brother Dr. Davy; the results of the latter being the reverse of those of the former. At present the sum of evidence is as follows:—

352. *Tension*.—No sensible attractions or repulsions due to tension have been observed.

353. *In motion*: i. *Evolution of Heat*; not yet observed: I have little or no doubt that Harris's electrometer would show it (287. 359.).

354. ii. *Magnetism*.—Perfectly distinct. According to Dr. Davy **, the current deflected the needle and made magnets under the same law, as to direction, which governs currents of ordinary and voltaic electricity.

355. iii. *Chemical Decomposition*.—Also distinct; and though Dr. Davy used an apparatus of similar construction with that of Dr. Wollaston (327.), still no error in the present case is involved, for the decompositions were polar, and in their nature truly electro-chemical. By the direction of the magnet, it was found that the under surface of the fish was negative, and the upper positive; and in the chemical decompositions, silver and lead were precipitated on the wire connected with the under surface, and not on the other; and when these wires were either steel or silver, in solution of common salt, gas (hydrogen?) rose from the negative wire, but none from the positive.

356. Another reason for the decomposition being electro-

* *Bibliothèque Universelle*, xxxvii. p. 15. † *Phil. Trans.* 1773, p. 461.

‡ *Phil. Trans.* 1775, p. 1.

§ *Ibid.* 1776, p. 196.

|| *Ibid.* 1829, p. 15. ¶ *Ibid.* 1832, p. 259.

** *Ibid.* 1832, p. 260.

chemical is, that a Wollaston's apparatus constructed with *wires*, coated by sealing-wax, would most probably not have decomposed water, even in its own peculiar way, without the electricity rising high enough in intensity to produce sparks in some part of the circuit; whereas the torpedo was not able to produce sensible sparks. A third reason is, that the purer the water in Wollaston's apparatus, the more abundant is the decomposition: and I have found that a machine and wire points which succeeded perfectly well with distilled water, failed altogether when the water was rendered a good conductor by sulphate of soda, common salt, or other saline bodies. But in Dr. Davy's experiments with the torpedo, *strong* solutions of salt, nitrate of silver, and superacetate of lead were used successfully, and there is no doubt with more success than weaker ones.

357. iv. *Physiological Effects*.—These are so characteristic, that by them the peculiar powers of the torpedo and gymnotus are principally recognised.

358. v. *Spark*.—The electric spark has not yet been obtained, or at least I think not; but perhaps I had better refer to the evidence on this point. Humboldt, speaking of results obtained by M. Fahlberg, of Sweden, says, "This philosopher has seen an electric spark, as Walsh and Ingenhousz had done before him at London, by placing the gymnotus in the air, and interrupting the conducting chain by two gold leaves pasted upon glass and a line distant from each other*." I cannot, however, find any record of such an observation by either Walsh or Ingenhousz, and do not know where to refer to that by M. Fahlberg. M. Humboldt could not himself perceive any luminous effect.

Again, Sir John Leslie, in his dissertation on the progress of mathematical and physical science, prefixed to the seventh edition of the *Encyclopædia Britannica*, Edinb. 1830, p. 622, says, "From a healthy specimen" of the *Silurus electricus*, meaning rather the *gymnotus*, "exhibited in London, vivid sparks were drawn in a darkened room;" but he does not say he saw them himself, nor state who did see them; nor can I find any account of such a phænomenon; so that the statement is doubtful†.

359. In concluding this summary of the powers of torpedinal electricity, I cannot refrain from pointing out the enormous absolute quantity of electricity which the animal must

* Edinburgh Phil. Journal, ii. p. 249.

† Mr. Brayley, who referred me to these statements, and has extensive knowledge of recorded facts, is unacquainted with any further account relating to them.

put in circulation at each effort. It is doubtful whether any common electrical machine has as yet been able to supply electricity sufficient in a reasonable time to cause true electro-chemical decomposition of water (330. 339.), yet the current from the torpedo has done it. The same high proportion is shown by the magnetic effects (296. 371.). These circumstances indicate that the torpedo has power (in the way probably that Cavendish describes,) to continue the evolution for a sensible time, so that its successive discharges rather resemble those of a voltaic arrangement, intermitting in its action, than those of a Leyden apparatus, charged and discharged many times in succession. In reality, however, there is *no philosophical difference* between these two cases.

360. The *general conclusion* which must, I think, be drawn from this collection of facts is, that *electricity, whatever may be its source, is identical in its nature*. The phænomena in the five kinds or species quoted, differ not in their character, but only in degree; and in that respect vary in proportion to the variable circumstances of *quantity* and *intensity** which can at pleasure be made to change in almost any one of the kinds of electricity, as much as it does between one kind and another.

Table of the experimental Effects common to the Electricities derived from different Sources.

	Physio- logical Effects.	Magnetic Deflec- tion.	Magnets made.	Spark.	Heating Power.	True chemical Action.	Attraction and Re- pulsion.	Discharge by Hot Air.
1. Voltaic Electricity }	×	×	×	×	×	×	×	×
2. Common Electricity }	×	×	×	×	×	×	×	×
3. Magneto- Electricity }	×	×	×	×	×	×	×	
4. Thermo- Electricity }	×	×	?		?			
5. Animal Electricity }	×	×	×	?		×		

* The term *quantity* in electricity is perhaps sufficiently definite as to sense; the term *intensity* is more difficult to define strictly. I am using the terms in their ordinary and at present accepted meaning.

§ 8. *Relation by Measure of common and voltaic Electricity.*

361. Believing the point of identity to be satisfactorily established, I next endeavoured to obtain a common measure, or a known relation as to quantity, of the electricity excited by a machine, and that from a voltaic pile; for the purpose not only of confirming their identity (378.), but also of demonstrating certain general principles (366. 377, &c.), and creating an extension of the means of investigating and applying the chemical powers of this wonderful and subtile agent.

362. The first point to be determined was, whether the same absolute quantity of ordinary electricity, sent through a galvanometer, under different circumstances, would cause the same deflection of the needle. An arbitrary scale was therefore attached to the galvanometer, each division of which was equal to about 4° , and the instrument arranged as in former experiments (296.). The machine (290.), battery (291.), and other parts of the apparatus were brought into good order, and retained for the time as nearly as possible in the same condition. The experiments were alternated so as to indicate any change in the condition of the apparatus and supply the necessary corrections.

363. Seven of the battery jars were removed, and eight retained for present use. It was found that about forty turns would fully charge the eight jars. They were then charged by thirty turns of the machine, and discharged through the galvanometer, a thick wet string, about ten inches long, being included in the circuit. The needle was immediately deflected five divisions and a half, on the one side of the zero, and in vibrating passed as nearly as possible through five divisions and a half on the other side.

364. The other seven jars were then added to the eight, and the whole fifteen charged by thirty turns of the machine. The Henley's electrometer stood not quite half as high as before; but when the discharge was made through the galvanometer, previously at rest, the needle immediately vibrated, passing *exactly* to the same division as in the former instance. These experiments with eight and with fifteen jars were repeated several times alternately with the same results.

365. Other experiments were then made, in which all the battery was used, and its charge (being fifty turns of the machine,) sent through the galvanometer: but it was modified by being passed sometimes through a mere wet thread, sometimes through thirty-eight inches of thin string wetted by distilled water, and sometimes through a string of twelve

times the thickness, only twelve inches in length, and soaked in dilute acid (298.). With the thick string the charge passed at once; with the thin string it occupied a sensible time, and with the thread it required two or three seconds before the electrometer fell entirely down. The current therefore must have varied extremely in intensity in these different cases, and yet the deflection of the needle was sensibly the same in all of them. If any difference occurred, it was that the thin string and thread caused greatest deflection; and if there is any lateral transmission, as M. Colladon says, through the silk in the galvanometer coil, it ought to have been so, because then the intensity is lower and the lateral transmission less.

366. Hence it would appear that *if the same absolute quantity of electricity pass through the galvanometer, whatever may be its intensity, the deflecting force upon the magnetic needle is the same.*

367. The battery of fifteen jars was then charged by sixty revolutions of the machine, and discharged, as before, through the galvanometer. The deflection of the needle was now as nearly as possible to the eleventh division, but the graduation was not accurate enough for me to assert that the arc was exactly double the former arc; to the eye it appeared to be so. The probability is, that *the deflecting force of an electric current is directly proportional to the absolute quantity of electricity passed, at whatever intensity that electricity may be*.*

368. Dr. Ritchie has shown that in a case where the intensity of the electricity remained the same, the deflection of the magnetic needle was directly as the quantity of electricity passed through the galvanometer†. Mr. Harris has shown that the heating power of common electricity on metallic wires is the same for the same quantity of electricity whatever its intensity might have previously been‡.

369. The next point was to obtain a *voltaic* arrangement producing an effect equal to that just described (367.). A platina and a zinc wire were passed through the same hole of a draw-plate, being then one eighteenth of an inch in diameter; these were fastened to a support, so that their lower ends

* The great and general value of the galvanometer, as an actual measure of the electricity passing through it, either continuously or interruptedly, must be evident from a consideration of these two conclusions. As constructed by Professor Ritchie with glass threads (see *Philosophical Transactions*, 1830, p. 218, and *Quarterly Journal of Science*, New Series, vol. i. p. 29.), it apparently seems to leave nothing unsupplied in its own department.

† *Quarterly Journal of Science*, New Series, vol. i. p. 33.

‡ *Plymouth Transactions*, page 22.

projected, were parallel, and five sixteenths of an inch apart. The upper ends were well connected with the galvanometer wires. Some acid was diluted, and, after various preliminary experiments, that adopted as a standard which consisted of one drop strong sulphuric acid in four ounces distilled water. Finally, the time was noted which the needle required in swinging either from right to left or left to right: it was equal to seventeen beats of my watch, the latter giving one hundred and fifty in a minute. The object of these preparations was to arrange a voltaic apparatus, which, by immersion in a given acid for a given time, much less than that required by the needle to swing in one direction, should give equal deflection to the instrument with the discharge of ordinary electricity from the battery (363. 364.); and a new part of the zinc wire having been brought into position with the platina, the comparative experiments were made.

370. On plunging the zinc and platina wires five eighths of an inch deep into the acid, and retaining them there for eight beats of the watch, (after which they were quickly withdrawn,) the needle was deflected, and continued to advance in the same direction some time after the voltaic apparatus had been removed from the acid. It attained the five-and-a-half division, and then returned swinging an equal distance on the other side. This experiment was repeated many times, and always with the same result.

371. Hence, as an approximation, and judging from *magnetical force* only, at present (376.), it would appear that two wires, one of platina and one of zinc, each one eighteenth of an inch in diameter, placed five sixteenths of an inch apart, and immersed to the depth of five eighths of an inch in acid, consisting of one drop oil of vitriol and four ounces distilled water, at a temperature about 60°, and connected at the other extremities by a copper wire eighteen feet long and one eighteenth of an inch thick (being the wire of the galvanometer coils), yield as much electricity in eight beats of my watch, or in $\frac{8}{130}$ ths of a minute, as the electrical battery charged by thirty turns of the large machine, in excellent order (363. 364.). Notwithstanding this apparently enormous disproportion, the results are perfectly in harmony with those effects which are known to be produced by variations in the intensity and quantity of the electric fluid.

372. In order to procure a reference to *chemical action*, the wires were now retained immersed in the acid to the depth of five eighths of an inch, and the needle, when stationary, observed; it stood, as nearly as the unassisted eye could decide, at $5\frac{1}{2}$ division. Hence a permanent deflection to that extent

might be considered as indicating a constant voltaic current, which in eight beats of my watch (369.) could supply as much electricity as the electrical battery charged by thirty turns of the machine.

373. The following arrangements and results are selected from many that were made and obtained relative to chemical action. A platina wire one twelfth of an inch in diameter, weighing two hundred and sixty grains, had the extremity rendered plane, so as to offer a definite surface equal to a circle of the same diameter as the wire; it was then connected in turn with the conductor of the machine, or with the voltaic apparatus (369.), so as always to form the positive pole, and at the same time retain a perpendicular position, that it might rest, with its whole weight, upon the test paper to be employed. The test paper itself was supported upon a platina spatula, connected either with the discharging train (292.), or with the negative wire of the voltaic apparatus, and it consisted of four thicknesses, moistened at all times to an equal degree in a standard solution of hydriodate of potassa (316.).

374. When the platina wire was connected with the prime conductor of the machine, and the spatula with the discharging train, ten turns of the machine had such decomposing power as to produce a pale round spot of iodine of the diameter of the wire: twenty turns made a much darker mark, and thirty turns made a dark brown spot penetrating to the second thickness of the paper. The difference in effect produced by two or three turns, more or less, could be distinguished with facility.

375. The wire and spatula were then connected with the voltaic apparatus (369.), the galvanometer being also included in the arrangement; and a stronger acid having been prepared, consisting of nitric acid and water, the voltaic apparatus was immersed so far as to give a permanent deflection of the needle to the $5\frac{1}{3}$ division (372.), the fourfold moistened paper intervening as before*. Then by shifting the end of the wire from place to place upon the test paper, the effect of the current for five, six, seven, or any number of the beats of the watch (369.) was observed, and compared with that of the machine. After alternating and repeating the experiments of comparison many times, it was constantly found that this standard current of voltaic electricity, continued for eight beats of the watch, was equal, in chemical effect, to thirty turns of the machine; twenty-eight revolutions of the machine were sensibly too few.

376. Hence it results that both in *magnetic deflection* (371.)

* Of course the heightened power of the voltaic battery was necessary to compensate for the bad conductor now interposed.

and in *chemical force*, the current of electricity of the standard voltaic battery for eight beats of the watch was equal to that of the machine evolved by thirty revolutions.

377. It also follows that for this case of electro-chemical decomposition, and it is probable for all cases, that the *chemical power, like the magnetic force* (366.), *is in direct proportion to the absolute quantity of electricity* which passes.

378. Hence arises still further confirmation, if any were required, of the identity of common and voltaic electricity, and that the differences of intensity and quantity are quite sufficient to account for what were supposed to be their distinctive qualities.

379. The extension which the present investigations have enabled me to make of the facts and views constituting the theory of electro-chemical decomposition, will, with some other points of electrical doctrine, be almost immediately submitted to the Royal Society in another series of these Researches.

Royal Institution, Dec. 15, 1832.

NOTE.—I am anxious, and am permitted, to add to this paper a correction of an error which I have attributed to M. Ampere in a former series of these Experimental Researches. In referring to his experiment on the induction of electrical currents (78.), I have called that a disc which I should have called a circle or a ring. M. Ampere used a ring, or a very short cylinder made of a narrow plate of copper bent into a circle, and he tells me that by such an arrangement the motion is very readily obtained. I have not doubted that M. Ampere obtained the motion he described; but merely mistook the kind of mobile conductor used, and so far I described his *experiment* erroneously.

In the same paragraph I have stated that M. Ampere says the disc turned “to take a position of equilibrium exactly as the spiral itself would have turned had it been free to move;” and further on I have said that my results tended to invert the sense of the proposition “stated by M. Ampere, *that a current of electricity tends to put the electricity of conductors near which it passes in motion in the same direction.*” M. Ampere tells me in a letter which I have just received from him, that he carefully avoided, when describing the experiment, any reference to the direction of the induced current; and on looking at the passages he quotes to me, I find that to be the case. I have therefore done him injustice in the above statements, and am anxious to correct my error.

But that it may not be supposed I lightly wrote those passages, I will briefly refer to my reasons for understanding them in the sense I did. At first the experiment failed. When re-made successfully about a year afterwards, it was at Geneva, in company with M. A. de la Rive: the latter philosopher described the results*, and says that the plate of copper bent into a circle which was used as the mobile conductor “sometimes advanced between the two branches of the (horse-shoe) magnet, and sometimes was repelled, *according to the direction of the current in the surrounding conductors.*”

I have been in the habit of referring to Demonferrand's *Manuel d'Électricité Dynamique*, as a book of authority in France; containing the general

* *Bibliothèque Universelle*, xxi. p. 48.

results and laws of this branch of science, up to the time of its publication, in a well arranged form. At p. 173, the author, when describing this experiment, says, "The mobile circle turns to take a position of equilibrium as a conductor would do in which the current moved in the *same direction* as in the spiral;" and in the same paragraph he adds, "It is therefore proved that a current of electricity tends to put the electricity of conductors, near which it passes, in motion in the *same direction*." These are the words I quoted in my paper (78.).

Le Lycée of 1st of January 1832, No. 36, in an article written after the receipt of my first unfortunate letter to M. Hachette, and before my papers were printed, reasons upon the direction of the induced currents, and says, that there ought to be "an elementary current produced in the same direction as the corresponding portion of the producing current." A little further on it says, "therefore we ought to obtain currents, moving in the *same direction*, produced upon a metallic wire, either by a magnet or a current. M. Ampere was so thoroughly persuaded that such ought to be the direction of the currents by influence, that he neglected to assure himself of it in his experiment at Geneva."

It was the precise statements in Démonferrand's *Manuel*, agreeing as they did with the expression in M. de la Rive's paper, (which, however, I now understand as only meaning that when the inducing current was changed, the motion of the mobile circle changed also,) and not in discordance with anything expressed by M. Ampere himself where he speaks of the experiment, which made me conclude, when I wrote the paper, that what I wrote was really his avowed opinion: and when the Number of the *Lycée* referred to appeared, which was before my paper was printed, it could excite no suspicion that I was in error.

Hence the mistake into which I unwittingly fell. I am proud to correct it, and do full justice to the acuteness and accuracy which, as far as I can understand the subjects, M. Ampere carries into all the branches of philosophy which he investigates.

Finally, my note to (79.) says that the *Lycée*, No. 36, "mistakes the erroneous results of MM. Fresnel and Ampere for true ones," &c. &c. In calling M. Ampere's results erroneous, I spoke of the results described in, and referred to by the *Lycée* itself; but now that the expression of the direction of the induced current is to be separated, the term *erroneous* ought no longer to be attached to them.

April 29, 1833.

M. F.

LX. Reviews, and Notices respecting New Books.

Elements of Plane and Spherical Trigonometry, with its Applications to the Principles of Navigation and Nautical Astronomy; by J.

R. YOUNG*: to which are added some original Researches in Spherical Geometry, by T. S. DAVIES, Esq. F.R.S.L. & E., F.R.A.S., &c.

OUR old-fashioned elementary works on mathematical science, though less erudite in appearance than their modern substitutes, were characterized by some excellences which we rarely find in recent treatises on the same class of subjects. One of these excellences was, the close connexion that was constantly kept in view

* We were gratified to learn, since this review was written, that this distinguished mathematician has been elected *Professor of Mathematics in the Royal Belfast Institution*.