PHILOSOPHICAL

TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXXXIII.

 PART I.

LONDON:

PRINTED BY RICHARD TAYLOR, RED LION COURT, FLEET STREET.

MDCCCXXXIII.

IV. *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, Petersburgh, &c. &c.*

Read January 10th and 17th, 1833.

§ 7. *Identity of Electricities derived from different sources.*

265. THE progress of the electrical researches which I have had the honour to present to the Royal Society, brought me to a point at which it was essential for the further prosecution of my inquiries that no doubt should remain of the identity or distinction of electricities excited by different means. It is perfectly true that CAVENDISH *, WOLLASTON †, COLLADON ‡ and others, have in turn removed some of the greatest objections to the acknowledgement of the identity of common, animal and voltaic electricity, and I believe that philosophers generally consider these electricities as really the same. But on the other hand it is also true, that the accuracy of WOLLASTON's experiments has been denied §, and that one of them, which really is no proof of chemical decomposition by common electricity (309. 327.), has been that selected by several experimenters as the test of chemical action (336. 346.). It is a fact, too, that many philosophers are still drawing distinctions between the electricities from different sources; or at least doubting whether their identity is proved. Sir HUMPHRY DAVY, for instance, in his paper on the Torpedo ||, thought it probable

* Phil. Trans. 1776, p. 196.

† Ibid. 1801, p. 434.

‡ Annales de Chimie, 1826, p. 62, &c.

§ Phil. Trans. 1832, p. 282, note.

|| Phil. Trans. 1829, p. 17. "Common electricity is excited upon non-conductors, and is readily carried off by conductors and imperfect conductors. Voltaic electricity is excited upon combinations

that animal electricity would be found of a peculiar kind; and referring to that, in association with common electricity, voltaic electricity and magnetism, has said, "Distinctions might be established in pursuing the various modifications or properties of electricity in these different forms, &c." Indeed I need only refer to the last volume of the Philosophical Transactions to show that the question is by no means considered as settled *.

266. Notwithstanding, therefore, the general impression of the identity of electricities, it is evident that the proofs have not been sufficiently clear and distinct to obtain approbation from all those who were competent to consider the subject; and the question seemed to me very much in the condition of that which Sir H. DAVY solved so beautifully,—namely, whether voltaic electricity in all cases merely eliminated, or did not in some actually produce, the acid and alkali found after its action upon water. The same necessity that urged him to decide the doubtful point, which interfered with the extension of his views, and destroyed the strictness of his reasoning, has obliged me to

of perfect and imperfect conductors, and is only transmitted by perfect conductors or imperfect conductors of the best kind. Magnetism, if it be a form of electricity, belongs only to perfect conductors; and, in its modifications, to a peculiar class of them [1]. Animal electricity resides only in the imperfect conductors forming the organs of living animals, &c."

* Phil. Trans. 1832, p. 259. Dr. DAVY, in making experiments on the torpedo, obtains effects the same as those produced by common and voltaic electricity, and says that in its magnetic and chemical power it does not seem to be essentially peculiar,—p. 274; but he then says, p. 275, there are other points of difference; and after referring to them, adds, "How are these differences to be explained? Do they admit of explanation similar to that advanced by Mr. CAVENDISH in his theory of the torpedo; or may we suppose, according to the analogy of the solar ray, that the electrical power, whether excited by the common machine, or by the voltaic battery, or by the torpedo, is not a simple power, but a combination of powers, which may occur variously associated, and produce all the varieties of electricity with which we are acquainted?"

At p. 279 of the same volume of Transactions is Dr. RITCHIE's paper, from which the following are extracts: "Common electricity is diffused over the surface of the metal;—voltaic electricity exists within the metal. Free electricity is conducted over the surface of the thinnest gold leaf as effectually as over a mass of metal having the same surface;—voltaic electricity requires thickness of metal for its conduction," p. 280: and again, "The supposed analogy between common and voltaic electricity, which was so eagerly traced after the invention of the pile, completely fails in this case, which was thought to afford the most striking resemblance," p. 291.

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[1] Dr. RITCHIE has shown this is not the case, Phil. Trans. 1832, p. 294.

ascertain the identity or difference of common and voltaic electricity. I have satisfied myself that they are identical, and I hope the proofs I have to offer, and the results flowing from them, will be found worthy the attention of the Royal Society.

267. The various phenomena exhibited by electricity may, for the purposes of comparison, be arranged under two heads; namely, those connected with electricity of tension, and those belonging to electricity in motion. This distinction is taken at present not as philosophical, but merely as convenient. The effect of electricity of tension, at rest, is either attraction or repulsion at sensible distances. The effects of electrical currents may be considered as 1st, Evolution of heat; 2nd, Magnetism; 3rd, Chemical decomposition; 4th, Physiological phenomena; 5th, Spark. It will be my object to compare electricities from different sources, and especially common and voltaic electricities, by their power of producing these effects.

I. *Voltaic Electricity.*

268. *Tension.*—When a voltaic battery of 100 pairs of plates has its extremities examined by the ordinary electrometer, it is well known that they are found positive and negative, the gold leaves at the same extremity repelling each other, the gold leaves at different extremities attracting each other, even when half an inch or more of air intervenes.

269. That ordinary electricity is discharged by points with facility through air; that it is readily transmitted through highly rarefied air; and also through heated air, as for instance a flame; is due to its high tension. I sought, therefore, for similar effects in the discharge of voltaic electricity, using as a test of the passage of the electricity either the galvanometer or chemical action produced by the arrangement hereafter to be described (312. 316.)

270. The voltaic battery I had at my disposal consisted of 140 pairs of plates four inches square, with double coppers. It was insulated throughout, and diverged a gold leaf electrometer about one third of an inch. On endeavouring to discharge this battery by delicate points very nicely arranged and approximated, either in the air or in an exhausted receiver, I could obtain no indications of a current, either by magnetic or chemical action. In this, how-

ever, was found no point of discordance between voltaic and common electricity; for when a Leyden battery (291.) was charged so as to deflect the gold leaf electrometer equally, the points were found equally unable to discharge it with such effect as to produce either magnetic or chemical action. This was not because common electricity could not produce both these effects (307. 310.), but because when of such low intensity the quantity required to make the effects visible (being enormously great (371. 375.),) could not be transmitted in any reasonable time. In conjunction with the proofs of identity hereafter to be given, these effects of points also prove identity instead of difference between voltaic and common electricity.

271. As heated air discharges common electricity with far greater facility than points, I hoped that voltaic electricity might in this way also be discharged. An apparatus was therefore constructed (Plate II. fig. 5.), in which A B is an insulated glass rod upon which two copper wires, C, D, are fixed firmly; to these copper wires are soldered two pieces of fine platina wire, the ends of which are brought very close to each other at *e*, but without touching; the copper wire C was connected with the positive pole of a voltaic battery, and the wire D with a decomposing apparatus (312. 316.), from which the communication was completed to the negative pole of the battery. In these experiments only two troughs, or twenty pairs of plates, were used.

272. Whilst in the state described, no decomposition took place at the point *a*, but when the side of a spirit-lamp flame was applied to the two platina extremities at *e*, so as to make them bright red-hot, decomposition occurred; iodine soon appeared at the point *a*, and the transference of electricity through the heated air was established. On raising the temperature of the points *e* by a blowpipe, the discharge was rendered still more free, and decomposition took place instantly. On removing the source of heat, the current immediately ceased. On putting the ends of the wires very close by the side of and parallel to each other, but not touching, the effects were perhaps more readily obtained than before. On using a larger voltaic battery (270.), they were also more freely obtained.

273. On removing the decomposing apparatus and interposing a galvanometer instead, heating the points *e* as the needle would swing one way, and removing the heat during the time of its return (302.), feeble deflections were

Fig. 4.

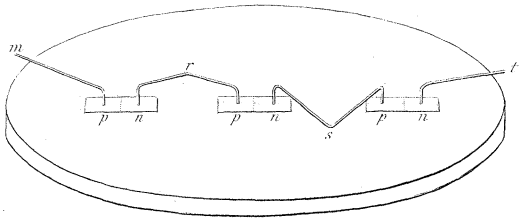
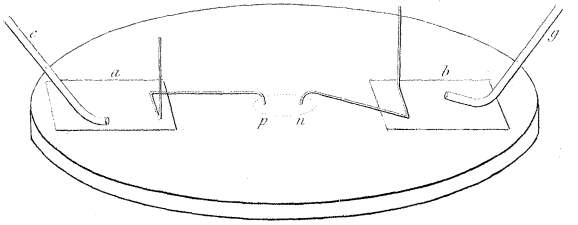


Fig. 2.



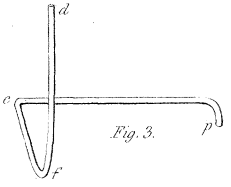


Fig. 3.

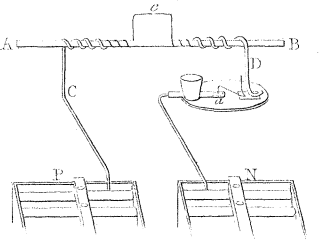


Fig. 5.

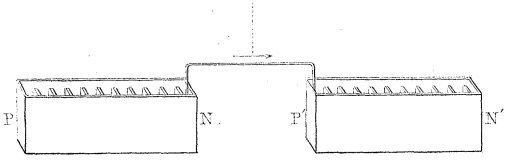


Fig. 1.

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soon obtained: thus also proving the current through heated air; but the instrument used was not so sensible under the circumstances as chemical action.

274. These effects, not hitherto known or expected under this form, are only cases of the discharge which takes place through air between the charcoal terminations of the poles of a powerful battery, when they are gradually separated after contact. Here the passage is through heated air exactly as with common electricity, and Sir H. DAVY has recorded that with the original battery of the Royal Institution this discharge passed through at least four inches of air *. In the exhausted receiver the electricity would *strike* through nearly half an inch of space, and the combined effects of rarefaction and heat was such upon the inclosed air as to enable it to conduct the electricity through a space of six or seven inches.

275. The instantaneous charge of a Leyden battery by the poles of a voltaic apparatus is another proof of the tension, and also the quantity, of electricity evolved by the latter. Sir H. DAVY says †, "When the two conductors from the ends of the combination were connected with a Leyden battery, one with the internal, the other with the external coating, the battery instantly became charged, and on removing the wires and making the proper connexions, either a shock or a spark could be perceived: and the least possible time of contact was sufficient to renew the charge to its full intensity."

276. *In motion;* i. *Evolution of Heat.*—The evolution of heat in wires and fluids by the voltaic current is matter of general notoriety.

277. ii. *Magnetism.*—No fact is better known to philosophers than the power of the voltaic current to deflect the magnetic needle, and to make magnets according to *certain laws;* and no effect can be more distinctive of an electrical current.

278. iii. *Chemical decomposition.*—The chemical powers of the voltaic current, and their subjection to *certain laws,* are also perfectly well known.

279. iv. *Physiological effects.*—The power of the voltaic current, when strong, to shock and convulse the whole animal system, and when weak to affect the tongue and the eyes, is very characteristic.

280. v. *Spark.*—The brilliant star of light produced by the discharge of a

* Elements of Chemical Philosophy, p. 153. † Ibid. p. 154.

voltaic battery is known to all as the most beautiful light that man can produce by art.

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281. That these effects may be almost infinitely varied, some being exalted whilst others are diminished, is universally acknowledged; and yet without any doubt of the identity of character of the voltaic currents thus made to differ in their effect. The beautiful explication of these variations afforded by CAVENDISH's theory of quantity and intensity requires no support at present, as it is not understood to be doubted.

282. In consequence of the comparisons that will hereafter arise between wires carrying voltaic and ordinary electricities, and also because of certain views of the condition of a wire or any other conducting substance connecting the poles of a voltaic apparatus, it will be necessary to give some definite view of what is called the voltaic current, in contradistinction to any supposed peculiar state of arrangement, not progressive, which the wire or the electricity within it may be supposed to assume. If two voltaic troughs P N, P′ N′, fig. 1. be symmetrically arranged and insulated, and the ends N P′ connected by a wire, over which a magnetic needle is suspended, the wire will exert no effect over the needle; but immediately that the ends P N′ are connected by another wire, the needle will be deflected, and will remain so as long as the circuit is complete. Now if the troughs merely act by causing a peculiar arrangement in the wire either of its particles or its electricity, that arrangement constituting its electrical and magnetic state, then the wire N P′ should be in a similar state of arrangement *before* P and N′ were connected, to what it is afterwards, and should have deflected the needle, although less powerfully, perhaps to one half the extent which would result when the communication is complete throughout. But if the magnetic effects depend upon a current, then it is evident why they could not be produced in *any* degree before the circuit was complete; because prior to that no current could exist.

283. By *current*, I mean anything progressive, whether it be a fluid of electricity, or two fluids moving in opposite directions, or merely vibrations, or, speaking still more generally, progressive forces. By *arrangement*, I understand a local adjustment of particles, or fluids, or forces, not progressive. Many other reasons might be urged in support of the view of a *current* rather than

an *arrangement*, but I am anxious to avoid dilating unnecessarily upon what can be supplied by others at the moment.

II. *Ordinary Electricity.*

284. By ordinary electricity I understand that which can be obtained from the common machine, or from the atmosphere, or by pressure, or cleavage of crystals, or by a multitude of other operations; its distinctive character being that of great intensity, and the exertion of attractive and repulsive powers, not merely at sensible but at considerable distances.

285. *Tension.* The attractions and repulsions at sensible distances, caused by ordinary electricity, are well known to be so powerful in certain cases, as to surpass, almost infinitely, the similar phenomena produced by electricity, considered as of other kinds. But still those attractions and repulsions are exactly of the same nature as those already referred to under the head *Tension*, *Voltaic electricity* (268.); and the difference in degree between them is not greater than often occurs between cases of ordinary electricity only. I think it will be unnecessary to enter minutely into the proofs of the identity of this character in the two instances. They are abundant; are generally admitted as good; and lie upon the surface of the subject: and whenever in other parts of the comparison I am about to draw, a similar case occurs, I shall content myself with a mere announcement of the similarity, expanding only upon those parts where the great question of distinction or identity is still controverted.

286. The discharge of common electricity through heated air is a well-known fact. The parallel case of voltaic electricity has already been described (272, &c.).

287. *In motion.* i. *Evolution of heat.* The heating power of common electricity when passed through wires or other substances, is perfectly well known. The accordance between it and voltaic electricity is in this respect complete. Mr. Harris has constructed and described* a very beautiful and sensible instrument on this principle, in which the heat produced in a wire by the discharge of a mere spark of common electricity is readily shown, and to which I

* Philosophical Transactions, 1827, p. 18. Edinburgh Transactions, 1831. Harris on a New Electrometer, &c. &c.

shall have occasion to refer for experimental proof in a future part of this paper (344.).

288. ii. *Magnetism.* Voltaic electricity has most extraordinary and exalted magnetic powers. If common electricity be identical with it, it ought to have the same powers. In rendering needles or bars magnetic, it is found to agree with voltaic electricity, and the *direction* of the magnetism, in both cases, is the same; but in deflecting the magnetic needle, it has been found deficient, so that sometimes the power has been denied altogether, and at other times distinctions have been hypothetically assumed for the purpose of avoiding the difficulty*.

289. M. COLLADON, of Geneva, considered that the difference might be due to the use of insufficient quantities of common electricity in all the experiments before made upon this point; and in a memoir read to the Academie des Sciences in 1826†, describes experiments, in which, by the use of a battery, points, and a delicate galvanometer, he succeeded in obtaining deflections, and thus establishing identity in that respect. MM. ARAGO, AMPERE, and SAVARY are mentioned in the paper as having witnessed a successful repetition of the experiments. But as no second witness of these effects has come forward, MM. ARAGO, AMPERE, and SAVARY, not having themselves published (that I am aware of,) their acceptance of the results, and as others have not been able to obtain the effects, M. COLLADON's conclusions have been by some doubted or denied; and an important point with me was to establish their accuracy, or remove them entirely from the received body of experimental evidence. I am happy to say that my results fully confirm those by M. COLLADON, and I should have had no occasion to describe them, but that they are essential as proofs of the accuracy of the final and general conclusions I am enabled to draw respecting the magnetic and chemical action of electricity, (360. 366. 367. 377. &c.).

290. The plate electrical machine I have used is fifty inches in diameter; it has two sets of rubbers; its prime conductor consists of two brass cylinders connected by a third, the whole length being twelve feet, and the surface in contact with air about 1422 square inches. When in good excitation, one

* DEMONFERRAND's Manuel d'Electricité dynamique, p. 121.

† Annales de Chimie, xxxiii. p. 62.

revolution of the plate will give ten or twelve sparks from the conductors, each an inch in length. Sparks or flashes from ten to fourteen inches in length may easily be drawn from the conductors. Each turn of the machine, when worked moderately, occupies about $\frac{4}{5}$ths of a second.

291. The electric battery consisted of fifteen equal jars. They are coated eight inches upwards from the bottom, and are twenty-three inches in circumference, so that each contains one hundred and eighty-four square inches of glass, coated on both sides, independent of the bottoms, which are thicker glass, and contain each about fifty square inches.

292. A good discharging train was arranged by connecting metallically a sufficiently thick wire with, first, the metallic gas pipes of the house, then with the metallic gas pipes belonging to the public gas works of London; and finally, with the metallic water pipes of London. It was so effectual as to carry off instantaneously electricity of the feeblest tension, even that of a single voltaic trough, and was essential to many of the experiments.

293. The galvanometer was one or the other of those formerly described (87. 205.), but the glass jar covering it and supporting the needle was coated inside and outside with tinfoil, and the upper part (left uncoated, that the motions of the needle might be examined,) was covered with a frame of wire-work, having numerous sharp points projecting from it. When this frame and the two coatings were connected with the discharging train (292.), an insulated point or ball, connected with the machine when most active, might be brought within an inch of any part of the galvanometer, yet without affecting the needle within by any ordinary electrical attraction or repulsion.

294. In connexion with these precautions, it may be necessary to state that the needle of the galvanometer is very liable to have its magnetic power deranged, diminished, or even inverted by the passage of a shock through the instrument. If the needle be at all oblique in the wrong direction to the coils of the galvanometer, when the shock passes, effects of this kind are sure to happen.

295. It was to the retarding power of bad conductors, that I first looked with the hope of being able to make common electricity assume more of the characters and power of voltaic electricity, than it is usually supposed to have.

296. The coating and armour of the galvanometer were first connected with the discharging train (292.); the end B (87.) of the galvanometer wire was connected with the outside coating of the battery, and then both these with the discharging train; the end A of the galvanometer wire was connected with a discharging rod by a wet thread four feet long; and finally, when the battery had been positively charged by about forty turns of the machine, it was discharged by the rod and the thread through the galvanometer. The needle immediately moved.

297. During the time that the needle completed its vibration in the first direction and returned, the machine was worked, and the battery charged; and when the needle in vibrating resumed its first direction, the discharge was again made through the galvanometer. By repeating this action a few times, the vibrations soon extended to above 40° on each side of the line of quiescence.

298. This effect could be obtained at pleasure. Nor was it varied, apparently, either in direction or degree, by using a short thick string, or even four short thick strings in place of the long fine thread. With a more delicate galvanometer, an excellent swing of the needle could be obtained by one discharge of the battery.

299. On reversing the galvanometer communications so as to pass the discharge through from B to A, the needle was equally well deflected, but in the opposite direction.

300. The deflections were in the same direction as if a voltaic current had been passed through the galvanometer, i. e. the positively charged surface of the electric battery coincided with the positive end of the voltaic apparatus (268.), and the negative surface of the first with the negative end of the latter.

301. The battery was then thrown out of use, and the communications so arranged that the current could be passed from the prime conductor, by the discharging rod held against it, through the wet string, through the galvanometer coil, and into the discharging train (292.), by which it was finally dispersed. This current could be stopped at any moment, by removing the discharging rod, and either stopping the machine or connecting the prime conductor by another rod with the discharging train; and could be as instantly renewed. The needle was so adjusted, that whilst vibrating in moderate and small arcs, it required time equal to twenty-five beats of a watch to pass in one

direction through the arc, and of course an equal time to pass in the other direction.

302. Thus arranged, and the needle being stationary, the current, direct from the machine, was sent through the galvanometer for twenty-five beats, then interrupted for other twenty-five beats, renewed for twenty-five beats more, again interrupted for an equal time, and so on continually. The needle soon began to vibrate visibly, and after several alternations of this kind, the vibration increased to 40° or more.

303. On changing the direction of the current through the galvanometer, the direction of the deflection of the needle was also changed. In all cases the motion of the needle was the same in direction as that caused by the use of an electric battery or a voltaic trough (300.).

304. I now rejected the wet string, and substituted a copper wire, so that the electricity of the machine passed at once into wires communicating directly with the discharging train, the galvanometer coil being one of the wires used for the discharge. The effects were exactly those obtained above (302.).

305. Instead of passing the electricity through the system, by bringing the discharging rod at the end of it into contact with the conductor, four points were fixed on to the rod; when the current was to pass, they were held about twelve inches from the conductor, and when it was not to pass, they were turned away. Then, except with this variation, operating as before (302.), the needle was soon powerfully deflected, and in perfect consistency with the former results. Points afforded the means by which COLLADON, in all cases, made his discharges.

306. Finally, I passed the electricity first through an exhausted receiver, so as to make it there resemble the aurora borealis, and then through the galvanometer to the earth; and it was found still effective in deflecting the needle, and apparently with the same force as before.

307. From all these experiments, it appears that a current of common electricity, whether transmitted through water or wire, or rarefied air, or by means of points in common air, is still able to deflect the needle; the only requisite being, apparently, to allow time for its action: that it is, in fact, just as magnetic in every respect as a voltaic current, and that in this character therefore no distinction exists.

308. Imperfect conductors, as water, brine, acids, &c. &c. will be found far more convenient for exhibiting these effects than other modes of discharge, as by points or balls; for the former convert at once the charge of a powerful battery into a feeble spark, or rather continuous current, and involve little or no risk of deranging the magnetism of the needles (294.).

309. iii. *Chemical decomposition.* The chemical action of voltaic electricity is characteristic of that agent, but not more characteristic than are the laws under which the bodies evolved by decomposition arrange themselves at the poles. Dr. WOLLASTON showed* that common electricity resembled it in these effects, and "that they are both essentially the same;" but he mingled with his proofs an experiment having a resemblance, and nothing more, to a case of voltaic decomposition, which however he himself partly distinguished; and this has been more frequently referred to by others, on the one hand, to prove the occurrence of electro-chemical decomposition, like that of the pile, and on the other to throw doubt upon the whole paper, than the more numerous and decisive experiments which he has detailed.

310. I take the liberty of describing briefly my results, and of thus adding my testimony to that of Dr. WOLLASTON on the identity of voltaic and common electricity as to chemical action, not only that I may facilitate the repetition of the experiments, but also lead to some new consequences respecting electro-chemical decomposition (376. 377.).

311. I first repeated WOLLASTON's fourth experiment†, in which the ends of coated silver wires are immersed in a drop of sulphate of copper. By passing the electricity of the machine through such an arrangement, that end in the drop which received the electricity became coated with metallic copper. One hundred turns of the machine produced an evident effect; two hundred turns a very sensible one. The decomposing action was however very feeble. Very little copper was precipitated, and no sensible trace of silver from the other pole appeared in the solution.

312. A much more convenient and effectual arrangement for chemical decompositions by common electricity, is the following. Upon a glass plate, fig. 2, placed over, but raised above a piece of white paper, so that shadows may not interfere, put two pieces of tinfoil *a*, *b*; connect one of these by an

* Philosophical Transactions, 1801, p. 427, 434. † Ibid. 1801, p. 429.

insulated wire *c*, or wire and string (301.) with the machine, and the other *g*, with the discharging train (292.) or the negative conductor; provide two pieces of fine platina wire, bent as in fig. 3, so that the part *d*, *f* shall be nearly upright, whilst the whole is resting on the three bearing points *p*, *e*, *f*; place these as in fig. 2; the points *p*, *n* then become the decomposing poles. In this way surfaces of contact, as minute as possible, can be obtained at pleasure, and the connexion can be broken or renewed in a moment, and the substances acted upon examined with the utmost facility.

313. A coarse line was made on the glass with solution of sulphate of copper, and the terminations *p* and *n* put into it; the foil *a* was connected with the positive conductor of the machine by wire and wet string, so that no sparks passed: twenty turns of the machine caused the precipitation of so much copper on the end *p*, that it looked like copper wire; no apparent change took place at *n*.

314. A mixture of half muriatic acid and half water was rendered deep blue by sulphate of indigo, and a large drop put on the glass, fig. 2, so that *p* and *n* were immersed at opposite sides; a single turn of the machine showed bleaching effects round *p*, from evolved chlorine. After twenty revolutions no effect of the kind was visible at *n*, but so much chlorine had been set free at *p*, that when the drop was stirred the whole became colourless.

315. A drop of solution of iodide of potassium mingled with starch was put into the same position at *p* and *n*; on turning the machine, iodine was evolved at *p*, but not at *n*.

316. A still further improvement in this form of apparatus consists in wetting a piece of filtering paper in the solution to be experimented on, and placing that under the points *p* and *n*, on the glass: the paper retains the substance evolved at the point of evolution, by its whiteness renders any change of colour visible, and allows of the point of contact between it and the decomposing wires being contracted to the utmost degree. A piece of paper moistened in the solution of iodide of potassium and starch, or of the iodide alone, with certain precautions (322.), is a most admirable test of electro-chemical action; and when thus placed and acted upon by the electric current, will show iodine evolved at *p* by only half a turn of the machine. With these adjustments and the use of iodide of potassium on paper, chemical action is

sometimes a more delicate test of electrical currents than the galvanometer (273.). Such cases occur when the bodies traversed by the current are bad conductors, or when the quantity of electricity evolved or transmitted in a given time is very small.

317. A piece of litmus paper moistened in solution of common salt or sulphate of soda, was quickly reddened at *p*. A similar piece moistened in muriatic acid was very soon bleached at *p*. No effects of a similar kind took place at *n*.

318. A piece of turmeric paper moistened in solution of sulphate of soda was reddened at *n* by two or three turns of the machine, and in twenty or thirty turns plenty of alkali was there evolved. On turning the paper round, so that the spot came under *p*, and then working the machine, the alkali soon disappeared, the place became yellow, and a brown alkaline spot appeared in the new part under *n*.

319. On combining a piece of litmus with a piece of turmeric paper, wetting both with solution of sulphate of soda, and putting the paper on the glass, so that *p* was on the litmus and *n* on the turmeric, a very few turns of the machine sufficed to show the evolution of acid at the former and alkali at the latter, exactly in the manner effected by a volta-electric current.

320. All these decompositions took place equally well, whether the electricity passed from the machine to the foil *a*, through water, or through wire only; by *contact* with the conductor, or by *sparks* there; provided the sparks were not so large as to cause the electricity to pass in sparks from *p* to *n*, or towards *n*; and I have seen no reason to believe that in cases of true electro-chemical decomposition by the machine, the electricity passed in sparks from the conductor, or at any part of the current, is able to do more, because of its tension, than that which is made to pass merely as a regular current.

321. Finally, the experiment was extended into the following form, supplying in this case the fullest analogy between common and voltaic electricity. Three compound pieces of litmus and turmeric paper (319.) were moistened in solution of sulphate of soda, and arranged on a plate of glass with platina wires, as in fig. 4. The wire *m* was connected with the prime conductor of the machine, the wire *t* with the discharging train, and the wires *r* and *s* en-

tered into the course of the electrical current by means of the pieces of moistened paper; they were so bent as to rest each on three points, *n, r, p*; *n, s, p*, the points *r* and *s* being supported by the glass, and the others by the papers: the three terminations *p, p, p* rested on the litmus, and the other three *n, n, n* on the turmeric paper. On working the machine for a short time only, acid was evolved at *all* the poles or terminations *p, p, p*, by which the electricity entered the solution, and alkali at the other poles *n, n, n*, by which the electricity left the solution.

322. In all experiments of electro-chemical decomposition by the common machine and moistened papers (316.), it is necessary to be aware of and to avoid the following important source of error. If a spark passes over moistened litmus and turmeric paper, the litmus paper (provided it be delicate and not too alkaline,) is reddened by it; and if several sparks are passed, it becomes powerfully reddened. If the electricity pass a little way from the wire over the surface of the moistened paper, before it finds mass and moisture enough to conduct it, then the reddening extends as far as the ramifications. If similar ramifications occur at the termination *n*, on the turmeric paper, they *prevent* the occurrence of the red spot due to the alkali, which would otherwise collect there; sparks or ramifications from the points *n* will also redden litmus paper. If paper moistened by a solution of iodide of potassium (which is an admirably delicate test of electro-chemical action,) be exposed to the sparks or ramifications, or even a feeble stream of electricity through the air from either the point *p* or *n*, iodine will be immediately evolved.

323. These effects must not be confounded with those due to the true electro-chemical powers of common electricity, and must be carefully avoided when the latter are to be observed. No sparks should be passed, therefore, in any part of the current, nor any increase of intensity allowed by which the electricity may be induced to pass between the platina wires and the moistened papers, otherwise than by conduction; for if it burst through the air, the effect referred to ensues.

324. The effect itself is due to the formation of nitric acid by the combination of the oxygen and nitrogen of the air, and is, in fact, only a delicate repetition of CAVENDISH's beautiful experiment. The acid so formed, though small in quantity, is in a high state of concentration as to water, and pro-

duces the consequent effects of reddening the litmus paper, or preventing the exhibition of alkali on the turmeric paper, or, by acting on the iodide of potassium, evolving iodine.

325. By moistening a very small slip of litmus paper in solution of caustic potassa, and then passing the electric spark over its length in the air, I gradually neutralized the alkali, and ultimately rendered the paper red; on drying it, I found that nitrate of potassa had resulted from the operation, and that the paper had become touch-paper.

326. Either litmus paper or white paper moistened in solution of iodide of potassium, offers therefore a very simple, beautiful, and ready means of illustrating CAVENDISH's experiment of the formation of nitric acid from the atmosphere.

327. I have already had occasion to refer to an experiment (265. 309.) by Dr. WOLLASTON, which is insisted upon too much, both by those who oppose and those who agree with the accuracy of his views respecting the identity of voltaic and ordinary electricity. By covering fine wires with glass or other insulating substances, and then removing only so much matter as to expose the point, or a section of the wires, and by passing electricity through two such wires, the guarded points of which were immersed in water, WOLLASTON found that the water could be decomposed even by the current from the machine, without sparks, and that two streams of gas arose from the points, exactly resembling, in appearance, those produced by voltaic electricity, and, like the latter, giving a mixture of oxygen and hydrogen gases. But Dr. WOLLASTON himself points out that the effect is different from that of the voltaic pile, inasmuch as both oxygen and hydrogen are evolved from *each* pole; he calls it "a very close *imitation* of the galvanic phenomena," but adds, that "in fact the resemblance is not complete," and does not trust to it to establish the principles, correctly laid down in his paper.

328. This experiment is neither more nor less than a repetition, in a refined manner, of that made by Dr. PEARSON in 1797*, and previously by MM. PAETS VAN TROOSTWYK and DEIMAN in 1789 or earlier. That the experiment should never be quoted as proving true electro-chemical decomposition, is sufficiently evident from the circumstance, that the law which regulates the trans-

* NICHOLSON's Journal, 4to, vol. i. pp. 241, 299, 349.

ference and final place of the evolved bodies (278. 309.) has no influence here. The water is decomposed at both poles independently of each other, and the oxygen and hydrogen evolved at the wires are the elements of the water existing the instant before in those places. That the poles, or rather points, have no mutual decomposing dependence, may be shown by substituting a wire, or the finger, for one of them, a change which does not at all interfere with the other, though it stops all action at the changed pole. This fact may be observed by turning the machine for some time; for though bubbles will rise from the point left unaltered, in quantity sufficient to cover entirely the wire used for the other communication, if they could be applied to it, yet not a single bubble will appear on that wire.

329. When electro-chemical decomposition takes place, there is great reason to believe that the quantity of matter decomposed is not proportionate to the intensity, but to the quantity of electricity passed (320.). Of this I shall be able to offer some proofs in a future part of this paper (375. 377.). But in the experiment under consideration, this is not the case. If, with a constant pair of points, the electricity be passed from the machine in sparks, a certain proportion of gas is evolved; but if the sparks be rendered shorter, less gas is evolved, and if no sparks be passed, there is scarcely a sensible portion of gases set free. On substituting solution of sulphate of soda for water, scarcely a sensible quantity of gas could be procured even with powerful sparks, and almost none with the mere current; yet the quantity of electricity in a given time was the same in all these cases.

330. I do not intend to deny that with such an apparatus common electricity can decompose water in a manner analogous to that of the voltaic pile; I believe at present that it can. But when what I consider the true effect only was obtained, the quantity of gas given off was so small that I could not ascertain whether it was, as it ought to be, oxygen at one wire and hydrogen at the other. Of the two streams one seemed more copious than the other, and on turning the apparatus round, still the same side in relation to the machine gave the largest stream. On substituting solution of sulphate of soda for pure water (329.), these minute streams were still observed. But the quantities were so small, that on working the machine for half an hour I could not obtain at either pole a bubble of gas larger than a small grain of sand. If the

conclusion which I have drawn (377.) relating to the amount of chemical action be correct, this ought to be the case.

331. I have been the more anxious to assign the true value of this experiment as a test of electro-chemical action, because I shall have occasion to refer to it in cases of supposed chemical action by magneto-electric and other electric currents (336. 346.) and elsewhere. But, independent of it, there cannot be now a doubt that Dr. WOLLASTON was right in his general conclusion; and that voltaic and common electricity have powers of chemical decomposition, alike in their nature, and governed by the same law of arrangement.

332. iv. *Physiological effects.*—The power of the common electric current to shock and convulse the animal system, and when weak to affect the tongue and the eyes, may be considered as the same with the similar power of voltaic electricity, account being taken of the intensity of the one electricity and duration of the other. When a wet thread was interposed in the course of the current of common electricity from the battery (291.) charged by eight or ten revolutions of the machine in good action (290.), and the discharge made by platina spatulas through the tongue or the gums, the effect upon the tongue and eyes was exactly that of a feeble voltaic circuit.

333. v. *Spark.*—The beautiful flash of light attending the discharge of common electricity is well known. It rivals in brilliancy, if it does not even very much surpass, the light from the discharge of voltaic electricity; but it endures for an instant only, and is attended by a sharp noise like that of a small explosion. Still no difficulty can arise in recognising it to be the same spark as that from the voltaic battery, especially under certain circumstances. The eye cannot distinguish the difference between a voltaic and a common electricity spark, if they be taken between amalgamated surfaces of metal, at intervals only, and through the same distance of air.

334. When the battery (291.) was discharged through a wet string placed in some part of the circuit away from the place where the spark was to pass, the spark was yellowish, flamy, having a duration sensibly longer than if the water had not been interposed, was about three fourths of an inch in length, was accompanied by little or no noise, and whilst losing part of its usual character had approximated in some degree to the voltaic spark. When the electricity retarded by water was discharged between pieces of charcoal, it was

exceedingly luminous and bright upon both surfaces of the charcoal, resembling the brightness of the voltaic discharge on such surfaces. When the discharge of the unretarded electricity was taken upon charcoal, it was bright upon both the surfaces, (in that respect resembling the voltaic spark,) but the noise was loud, sharp and ringing.

335. I have assumed, in accordance, I believe, with the opinion of every other philosopher, that atmospheric electricity is of the same nature with ordinary electricity (284.), and I might therefore refer to certain published statements of chemical effects produced by the former as proofs that the latter enjoys the power of decomposition in common with voltaic electricity. But the comparison I am drawing is far too rigorous, to allow me to use these statements without being fully assured of their accuracy; and I have no right to suppress them, because, if accurate, they establish what I am labouring to put on an undoubted foundation, and have priority to my results.

336. M. Bonijol of Geneva * is said to have constructed very delicate apparatus for the decomposition of water by common electricity. By connecting an insulated lightning rod with this apparatus, the decomposition of the water proceeded in a continuous and rapid manner even when the electricity of the atmosphere was not very powerful. The apparatus is not described; but as the diameter of the wire is mentioned as very small, it appears to have been similar in construction to that of Wollaston (327.); and as that does not furnish a case of true polar electro-chemical decomposition (328.), this result of M. Bonijol does not prove the identity in chemical action of common and voltaic electricity.

337. At the same page of the Bibliotheque Universelle, M. Bonijol is said to have decomposed *potash*, and also chloride of silver, by putting them into very narrow tubes and passing electric sparks from an ordinary machine over them. It is evident that these offer no analogy to cases of true voltaic decomposition, where the electricity only decomposes when it is *conducted* by the body acted upon, and ceases to decompose, according to its ordinary laws, when it passes in sparks. These effects are probably partly analogous to that which takes place with water in Pearson's or Wollaston's apparatus, and may be due to very high temperature acting on minute portions of matter; or they may be

* Bibliotheque Universelle 1830, tome xlv. p. 213.

connected with the results in air (322.). As nitrogen can combine directly with oxygen under the influence of the electric spark (324.), it is not impossible that it should even take it from the potassium of the potash, especially as there would be plenty of potassa in contact with the acting particles to combine with the nitric acid formed. However distinct all these actions may be from true polar electro-chemical decompositions, they are still highly important, and well worthy of investigation.

338. The late Mr. BARRY communicated a paper to the Royal Society * last year, so distinct in the details that it would seem at once to prove the identity in chemical action of common and voltaic electricity, but that, when examined, considerable difficulty arises in reconciling certain of the effects with the remainder. He used two tubes, each having a wire within it passing through the closed end, as is usual for voltaic decompositions. The tubes were filled with solution of sulphate of soda, coloured with syrup of violets, and connected by a portion of the same solution, in the ordinary manner; the wire in one tube was connected by a *gilt thread* with the string of an insulated electrical kite, and the wire in the other tube by a similar *gilt thread* with the ground. Hydrogen soon appeared in the tube connected with the kite, and oxygen in the other, and in ten minutes the liquid in the first tube was green from the alkali evolved, and that in the other red from free acid produced. The only indication of the strength of the atmospheric electricity is in the expression, "the usual shocks were felt on touching the string."

339. That the electricity in this case does not resemble that from any ordinary source of common electricity, is shown by several circumstances. WOLLASTON could not effect the decomposition of water by such an arrangement, and obtain the gases in *separate* vessels, using common electricity; nor have any of the numerous philosophers, who have employed such an apparatus, obtained any such decomposition, either of water or of a neutral salt, by the use of the machine. I have lately tried the large machine (290.) in full action for a quarter of an hour, during which seven hundred revolutions were made without producing any sensible effects, although the shocks that it would then give must have been far more powerful and numerous than could have been taken, with any chance of safety, from an electrical kite-string; and by reference

* Philosophical Transactions, 1831, p. 165.

to the comparison hereafter to be made (371.), it will be seen that for common electricity to have produced the effect, the quantity must have been awfully great, and apparently far more than could have been conducted to the earth by a gilt thread, and at the same time only have produced the "usualshocks."

340. That the electricity was apparently not analogous to voltaic electricity is evident, for the "usual shocks" only were produced, and nothing like the terrible sensation due to a voltaic battery, even when it has a tension so feeble as not to strike through the eighth of an inch of air.

341. It seems just possible that the air which was passing by the kite and string, being in an electrical state sufficient to produce the "usual shocks" only, could still, when the electricity was drawn off below, renew the charge, and so continue the current. The string was 1500 feet long, and contained two double threads. But when the enormous quantity which must have been thus collected is considered (371. 376.), the explanation seems very doubtful. I charged a voltaic battery of twenty pairs of plates four inches square and with double coppers, very strongly, insulated it, connected its positive extremity with the discharging train (292.), and its negative pole with an apparatus like that of Mr. Barry, communicating by a wire inserted three inches into the wet soil of the ground. This battery thus arranged produced feeble decomposing effects, as nearly as I could judge answering the description Mr. Barry has given. Its intensity was, of course, far lower than the electricity of the kite-string, but the supply of quantity from the discharging train was unlimited. It of course gave no shocks to compare with the "usual shocks" of a kite-string.

342. Mr. Barry's experiment is a very important one to repeat and verify. If confirmed, it will be, as far as I am aware, the first recorded case of true electro-chemical decomposition of water by common electricity, and it will supply a form of electrical current, which, both in quantity and intensity, is exactly intermediate between those of the common electrical machine and the voltaic pile.

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, 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 com-

* Annales de Chimie, l. p. 322. † Ibid. li. p. 77.

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

§ 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.

plete 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. 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‡,

* Bibliotheque Universelle, xxxvii. 15.

† Philosophical Transactions, 1773, p. 461.

‡ Phil. Trans. 1775, p. 1.

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-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

* Philosophical Transactions, 1776, p. 196.

† Ibid. 1829, p. 15.

‡ Ibid. 1832, p. 259.

§ Ibid. 1832, p. 260.

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 phenomenon; 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 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 suc-

* 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.

cessive 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.

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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 phenomena 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.

	Physiological Effects.	Magnetic Deflection.	Magnets made.	Spark.	Heating Power.	True chemical Action.	Attraction and Repulsion.	Discharge by Hot Air.
1. Voltaic Electricity....	×	×	×	×	×	×	×	×
2. Common Electricity ..	×	×	×	×	×	×	×	×
3. Magneto-Electricity ..	×	×	×	×	×	×	×	
4. Thermo-Electricity ..	×	×	?		?			
5. Animal Electricity....	×	×	×	?		×		

§ 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.

* 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.

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 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.

* 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.

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 $\frac{8}{150}$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 $5\frac{1}{3}$ 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,

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

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.) 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,
15*th Dec.* 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'Electricité Dynamique*, as a book of authority in France; containing the general 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 DEMONFERRAND'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.

* Bibliotheque Universelle, xxi. p. 48.