THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

MARCH 1885.

XX. *On the Seat of the Electromotive Forces in the Voltaic Cell.*
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To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE accompanying tract is an expansion of a paper drawn up as the opening of a discussion on Contact Electricity by Section A of the British Association at the Montreal Meeting. It is in five portions or chapters. The first of these [printed this month] is mainly historical; in the second and third portions theoretical views are considered, and my own are given at some length in accordance with the kind advice of Sir W. Thomson; the two remaining chapters are occupied with cognate thermoelectric phenomena, with the theory of the simple voltaic cell, which singularly enough is pretty complicated and very little understood, and with a discussion on the size of atoms. Although the paper is being printed in the annual volume of the Association, it has been suggested that a further publication of it might be useful in order that Physicists may have a better and more leisurely opportunity of contributing to the discussion of a matter which must have occupied the attention of every one more or less, which branches out into several interesting and important developments, and which, as a matter of controversy, has remained singularly long unsettled. I will therefore ask you to find room for it in your Journal, in the hope that the discussion may be there conveniently continued and properly closed.

I am, Gentlemen,

Your obedient servant,

University College, Liverpool,
December 1884.

OLIVER J. LODGE.

Phil. Mag. S. 5. Vol. 19. No. 118. March 1885. M

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1. The subject chosen for the present discussion illustrates in a remarkable way the need for such conversations. It is scarcely credible, at the present rate of progress, that eighty-four years after the discovery of the voltaic pile, opinion should still be utterly divided as to the seat of the main E.M.F. in it. I venture to hope that it may now be decided, and a substantial agreement arrived at with respect to it. My business is to open the discussion; but it so happens that for some seven or eight years I have believed myself to see more or less clearly to the root of this particular matter, and a laborious review of the literature of the subject has only strengthened my conviction. Having, therefore, strong and definite views I can hardly help letting them appear; and without assuming prematurely that these views are agreed with, they may yet serve as a link with which to connect the facts and the multifarious observations thereon.

In the course of my reading on the subject I have found only two great and epoch-making papers, that of Volta in 1801, and that of Sir William Thomson in 1851. The other contributions are some of them keen, like those of Faraday and Clerk Maxwell; some of them laborious, like those of Hankel and Ayrton and Perry; but none contain anything essentially and powerfully new except those two: unless, indeed, we include in the subject the immensely important phenomena of Seebeck and of Peltier, and Faraday's fundamental law of electro-chemical decomposition.

Volta* showed that when two metals were put into contact and separated, the insulated one was charged with electricity sufficient to make gold leaves diverge. He also stated that the contact force between any two metals was independent of intermediate metals, so that the metals could be arranged in a definite numerical series; and he gave the first series of the kind:—

* Volta, *Gehler's Wörterbuch*, iv. p. 616. See also a carefully edited version, *Annales de Chim.* 1 ser. xl. p. 225 (1801).

Moreover, he started a hypothesis to account for the action, a sort of impulsion or attraction of electricity by matter—an idea subsequently elaborated by Helmholtz. Fabroni* objected to Volta's explanation of his experiment. He denied contact force, and considered that the electricity was developed by chemical action.

Then the fight began, and lasted on and off some half-century. On the one side were Volta, Davy, Pfaff, Péclet, Marianini, Buff, Fechner, Zamboni, Matteucci, and Kohlrausch. On the other were Fabroni, Wollaston, Parrot, CErsted, Ritchie, Pouillet, Schönbein, Becquerel, De la Rive, and Faraday.

It was not all fighting: part of it resulted in a more thorough investigation of voltaic phenomena; and very often the original point of dispute was lost sight of, and Volta's fact itself was doubted in the eagerness to disprove Volta's explanation. The experiments of Pfaff and Péclet†, however, fairly well established the correctness of his observation; and Kohlrausch showed how, by means of a Daniell's cell combined with a condenser, to measure Volta forces absolutely, thus inventing a method which has been employed with modifications by Hankel, by Gerland, by Clifton, by Ayrton and Perry, by Von Zahn, and by most other experimenters on the subject‡.

* Fabroni, *Journal de Physique de l'Abbé Rozier*, xlix. p. 348.

† Péclet on the Contact of Good Conductors: *Comptes Rendus*, 1838, p. 930; Pogg. *Ann.* xli. 1839, p. 346; *Ann. de Chim.* 1842 and 1841, 3 ser. ii. p. 233.

Pfaff, Letter to Gay Lussac: *Ann. de Chim.* 2 ser. xli. p. 236 (1829).—Pfaff: For and against the production of Electricity by Chemical Processes, as a consequence of some experiments on the E.M.F. of liquids and metals: Pogg. *Ann.* xli. 1840, pp. 110 and 197.—Pfaff: Experimentum crucis in favour of the Contact Theory: Pogg. *Ann.* liii. 1841, p. 303. This crux is on p. 306, and consists in substituting ZnSO_4 for H_2SO_4 in a Grove cell, and showing that the current through a thin wire galvanometer is stronger than before. This, he says, leaves no further shift or evasion (*Ausflucht*) for the chemical theory. It is a fact we have grown accustomed to, but it is rather surprising, that the E.M.F. given by ZnSO_4 should be even higher than that given by H_2SO_4 . A convenient "*Ausflucht*" could nevertheless be provided for the chemical theory by pointing out that the combustion heat $\text{Zn}, 2\text{NO}_3$ is greater than $\text{Zn}, \text{SO}_4 - \text{H}_2\text{SO}_4 + 2(\text{H}, \text{NO}_3)$, if indeed the fact be so. Another shift is to talk about basic sulphate and the sourness of ZnSO_4 ; another is to use the word "*dissociation*."

‡ Kohlrausch's method consisted in bringing the plates of the two metals close together, connecting them by a wire for an instant, separating them, and putting one into connection with a Dellman electrometer, the other with the earth. The observation is repeated with a Daniell in the connecting wire, first one way, then the other. Thus three equations are obtained, $M, M' = ka$, $D + M/M' = k\beta$, $D - M/M' = k\gamma$;

whence

$$M/M' = \frac{B - \gamma D}{B + \gamma}, \quad \text{or} \quad \frac{a}{\beta - a} D.$$

(Pogg. *Ann.* vols. lxxv. p. 88, lxxxii. pp. 1 and 40, and lxxxviii. p. 465. 1851 and 1853.) He gets his results much lower than later experimenters; only $\frac{1}{2}$ a volt for Zn Cu, and $\cdot 58$ for Zn/Pt.

Both the plates are insulated by silk threads. The fixed wire, *d*, with which the raised plate comes into contact, leads to a Dellman electrometer. The connections are arranged for determining the "electroscopic tension" on the poles of an open battery, to see if it is the same as the E.M.F. (See Pogg. *Ann.* 1848, vol. lxxv. pp. 88 and 220.)

This apparatus he also used to measure the Volta effect between two metals, his classical memoir on the subject being in Poggendorff's *Annalen*, 1851, vol. lxxxii. p. 1. Later he improved the condenser, bringing it into the form shown in the following figure.

Fig. 2.—Kohlrausch's Later Form of Condenser.
 (See Pogg. *Ann.* 1853, vol. lxxxviii. p. 464.)

Wherever electrostatic methods were employed, and where the electroscope was the instrument of research, contact theorists had it all their own way; and it was only by apparent effort and twisting of experiments that chemical theorists could maintain their ground. But when electric currents were dealt with, and the galvanometer used, then the chemists had their turn, and they showed most conclusively that no mere contact could maintain a current unless heat disappeared or chemical action occurred: a fact obvious enough to us to-day on the principles so laboriously and finally established by Joule. By means of the galvanometer, the contact theory was so belaboured by Faraday that it ultimately seemed to give up the ghost, and the chemical doctrines triumphed. So much so, that Volta's original fact, in spite of the evidence which had been accumulated, was again doubted; and one finds in text-books culled from this period statements that Volta must have had wet fingers, or that he rubbed the plates together, or that there was moisture in the air. Also hints are given that films existed on the plate, that squeezed coats of varnish or lacquer might produce some electricity, and so on. It was pointed out, moreover, by De la Rive*, how minute a trace of chemical action could produce how much electricity, and how little electricity could affect an electroscope. But it is to be noted that any chemical action caused by damp on the plates, or moisture in the air, would be of the nature of local action, and local action is not a satisfactory producer of manageable electricity. Sir Humphry Davy is very clear on this head. He shows that chemical action need produce no electricity, instancing the burning of iron nitre on charcoal, potash and acid in a crucible or an electroscope, &c.; a plate of zinc placed on mercury and separated is found positive, but if left long enough to amalgamate, the compound shows no signs of electricity. Davy's views are singularly advanced, and are worth quoting†.

* De la Rive: *Traité d'électricité*, ii. p. 776; *Annales de Chimie*, xxxix. p. 311 (1828).

† Davy, Bakerian Lecture, 1806 (see Phil. Trans. 1807, p. 39):—"As the chemical attraction between two bodies seems to be destroyed by giving one of them an electrical state different from that which it naturally possesses . . . so it may be increased by exalting its natural energy. Thus, while zinc is incapable of combining with oxygen when negatively electrified in the circuit even by a feeble power, silver easily unites to it when positively electrified. . . . Among the substances that combine chemically, all those, the electrical energies of which are well known, exhibit opposite electrical states. . . . In the present state of our knowledge it would be useless to attempt to speculate on the remote cause of the electrical energy, or the reason why different bodies after being brought into contact should be found differently electrified; its relation to chemical affinity is, however, sufficiently evident. May it not be identical with

Quite detached from any connection with the controversy, because at that time quite unintelligible to all but one or two here and there, two papers appeared in 1851 by the President of this Section, which were the triumph and apotheosis of the chemical theory of the source of the current in the voltaic cell*. In one of these papers (that on "Electrolysis") it is irrefutably established, on the basis of the conservation of

it, and an essential property of matter?"—Page 44: "The great tendency of the attraction of the different chemical agents by the positive and negative surfaces in the Voltaic apparatus seems to be to restore the electrical equilibrium. . . . The electrical energies of the metals with regard to each other or the substance dissolved in the water, seems to be the cause that disturbs the equilibrium, and the chemical changes the cause that tends to restore the equilibrium; and the phenomena most probably depend on their joint agency." He then gives a very Voltaic account of the action of the pile—much in agreement with Sir Wm. Thomson; and endeavours to reconcile chemical and contact theorists by pointing out how essential a part chemical action plays in the production of a current, a most clear-sighted thing to do at that date. One more sentence may be quoted from this remarkable paper, though it is not quite so striking as the preceding. —Page 49: "These ideas are evidently directly in contradiction to the opinions advanced by Fabroni, and which in the early stage of the investigation appeared extremely probable, viz. that chemical changes are the primary cause of the phenomena of Galvanism. Before the experiments of M. Volta on the electricity excited by mere contact of metals were published, I had to a certain extent adopted this opinion; but the new fact immediately proved that another power must necessarily be concerned, for it was not possible to refer the electricity exhibited by the opposition of metallic surfaces to any chemical alterations, particularly as the effect is more distinct in a dry atmosphere, in which even the most oxidizable metals do not change, than in a moist one, in which many metals undergo oxidation."

* Sir W. Thomson:—1. "On the Mechanical Theory of Electrolysis and the Applications of the Principle of Mechanical Effect to the Measurement of Electromotive Forces in Absolute Units," *Phil. Mag.* December 1858. Reprint of Mathematical and Physical Papers, vol. i. pp. 472 and 490. 2. "On the Dynamical Theory of Heat, part vi. Thermoelectric Currents:" *Proc. R. S. Edin.* Dec. 1851; *Trans. R. S. Edin.* 1854; *Math. and Phys. Papers*, vol. i. pp. 232 and 316.

Helmholtz also clearly applied the conservation of energy to Voltaic circuits in his memoir *Die Erhaltung der Kraft*, read before the Physical Society of Berlin, 23 July, 1847. In this powerful memoir Prof. Helmholtz sails placidly through a great part of physics, applying to various phenomena the then new principle of the conservation of energy. He regards all action as occurring at a distance, and shows, as is well known, that, on this hypothesis, central forces are the necessary and sufficient condition of conserved energy. This part may now be regarded as superseded; but in the more special portions, among other things, he develops the mechanical theory of the E.M.F. of voltaic cells, of thermoelectric piles, and of magneto-machines; anticipating in many respects the somewhat later though independent work of Sir W. Thomson on these subjects.

Prof. Helmholtz's memoir is easily accessible through a translation, by [John] T[yn]dall, which appeared, in May 1853, in the "new series" of 'Scientific Memoirs,' issued by Taylor and Francis.

energy, that, making exception of such irreversible effects as are not readily brought into calculation, and allowing for certain possible reversible effects to be investigated thermoelectrically, the E.M.F. of a cell is not only dependent on the chemical action going on, but is calculable numerically in absolute measure on purely chemical data supplied provisionally by Dr. Andrews. It is proper to say, however, that this brilliant theory is avowedly based on the laborious and acute experimental work of Joule on the conservation of energy in the voltaic circuit*.

In the other of the two papers (that on "Thermo-electricity") it was shown that, from the fact that a current absorbed or generated heat at a metallic junction, an E.M.F. was necessarily situated there—in other words, that the Peltier effect necessitated the previously discovered Seebeck one.

The establishment of the conservation of energy, by Joule, for ever placed beyond doubt the fact that the energy of the electric current produced by a battery was due to, and was the equivalent of, the chemical actions going on there; but it was supposed, and is still supposed (though, as I venture to think, quite erroneously), to leave untouched the question as to the precise seat of the E.M.F. in a battery.

However that may be, the success of the chemical theory of the electric current naturally caused it to be still more certainly assumed that the apparent contact-force of Volta could also be accounted for by accidental chemical action, and that without some chemical action somewhere no Volta effect could be produced. This, also, I believe to be quite false; provided always that the phrase "chemical action" be used in its ordinary sense as meaning combination, and that the word "action" be not explained away as meaning anything whatever.

2. The triumph of the chemical theorists with regard to the Volta effect was, however, shortlived, for, from 1860, the invention of the quadrant electrometer put into the hands of electrostatic experimenters a far more refined and delicate instrument than could have been thought possible a few years before; and the illustrious inventor of that instrument himself for ever put the truth of Volta's phenomenon beyond doubt, by the most simple and beautiful device of suspending a charged torsion-arm over a zinc-copper junction. By comparing the deflection so produced with that caused by a

* Joule:—"On Heat evolved in Metals and during Electrolysis," *Phil. Mag.* [3] xix. p. 260 (1841); "On the Electric Origin of the Heat of Combustion," *ibid.* xx. p. 98, and xxii. p. 204; "On the Heat disengaged in Chemical Combination," *Phil. Mag.* [4] iii. p. 481. See also Reprint of Joule's papers by the Physical Society of London (Taylor & Francis).

Daniell cell, an absolute measure of the so-called contact-force was made; and it was shown that, uniting the copper and zinc by a drop of water, instead of by a metal, no deflection was produced. It was also shown that the deflection was greatest when the zinc was clean and the copper oxidized*.

But Sir William Thomson went further than this: he sounded a theoretic note, and in a sentence revived the whole controversy about the seat of power in the pile. The sentence is this:—"For nearly two years I have felt quite sure that the proper explanation of voltaic action in the common voltaic arrangement is something very near Volta's, which fell into discredit because Volta or his followers neglected the principle of the conservation of force. I now think it quite certain that two metals dipped into one electrolytic liquid will (when polarization is done away with) be at the same potential." And then he goes on to one of those brilliant and extraordinary speculations characteristic of no one else, and applies this apparent contact-force to determine a lower limit to the size of atoms—an application obviously of transcendent interest, and of more importance than all the previous outcome of contact discussions put together †.

The whole subject now acquired a fresh interest, and the new series of experimental determinations of contact-force began.

3. Hankel's and Gerland's measurements belong to this period in point of date (1861-69), though in method and motive they probably are the outcome of the earlier period‡. Hankel uses a modified Kohlrausch method and a Bohnenberger or

* Proc. Lit. and Phil. Soc. Manchester. Letter from Prof. W. Thomson to the president, Dr. Joule, Jan. 21, 1862: "New Proof of Contact-Electricity." See reprint of papers on Electrostatics and Magnetism, p. 317.

† "There cannot be a doubt that the whole theory is simply chemical action at a distance. Zinc and copper connected by a metal wire attract each other from any distance, so do platinum plates coated with oxygen and hydrogen respectively. I can now tell the amount of the force, and calculate how great a proportion of chemical affinity is used up electrolytically before two such disks come within any specified small distance down to a limit within which molecular heterogeneity becomes sensible. This of course gives a definite limit for the size of atoms."—*Letter to Dr. Joule*, 1862, cited above. See also Thomson and Tait, *Nat. Phil.*, Part II. Appendix F.

‡ Hankel, *Electr. Untersuchungen*, *Abh. der königl. Sächs. Gesellschaft, math.-phys. Klasse*, 1861 and 1865. See also Pogg. *Ann.* cxv. p. 57, and cxvi. pp. 286 and 440; cxxxi. p. 607.

Gerland, "On the E.M.F. between Water and some Metals," *Pogg. Ann.* cxxxiii. 1868, p. 513; cxxxvii. 1869, p. 552. In his second paper, to get over the air effect, Gerland joins two metals through a galvanometer, and then dipping them into a liquid, observes the first swing of the needle. He also compensates the E.M.F. by Poggendorff's method. He thus determines the value of

$$M/M' + M'/\text{Liquid} + \text{Liquid } M.$$

Hankel electroscope; his special merit is the determination of metal-liquid contact-forces without introducing blotting paper, glass, or fingers, as the earlier experimenters had done.

Fig. 3.—Hankel's Arrangement for observing the Volta Effect between a Metal and a Liquid.

The liquid, L, is in a funnel-tube, A, B; C is a copper plate, and M the metal under observation. First, p touches C for an instant, then C is raised and made to touch p' , which leads to a Hankel electrometer.

This gives $\text{Cu}/\text{M} + \text{M}/\text{L} = k\alpha$.

Then the liquid is run out of the funnel, a plate of the metal M is placed on its mouth, and the experiment repeated.

This gives $\text{Cu}/\text{M} = k\beta$.

To eliminate k substitute a plate of zinc for M, and get

$\text{Cu}/\text{Zn} = k\gamma$.

Then, finally, $\text{M}/\text{L} = \frac{\alpha - \beta}{\gamma} \text{Cu}/\text{Zn}$.

In all these expressions air contact-forces are, as usual, neglected. But it is very tempting to try if, by increasing the number of such equations, one cannot calculate some metal/air contact-forces. Thus the special case when M is copper gives one more equation. We can then take zinc instead of M, and can also make the condenser-plate of zinc instead of copper, and so on; but we get no forwarder, fresh unknowns appear as fast as additional equations, and some of the equations are liable to degenerate into identities.

So far back as 1824 Becquerel* attempted the investigation of metal-liquid contacts; and Buff† made some measurements in 1842, but his results are scarcely likely to be reliable considering the poor experimental resources of that date.

Professor Clifton employed Kohlrausch's method in 1877, using a Thomson electrometer and a Clark cell as standard of E.M.F. He has only published a preliminary paper‡, in which he overlooks minutiae such as change of contact-

* Becquerel, *Ann. de Chimie*, 1824. He put the liquid in a copper capsule on the plate of an electroscope, and connected it with the condenser plate by his fingers.

† Buff, Liebig's *Ann. Chem. u. Pharm.* 1842. He made the lower plate of his condenser the metal to be examined; on it he placed glass, and then filter paper soaked in the liquid, which he connected with the metal plate by a wire of the same metal.

‡ Clifton, *Proc. Royal Soc.* xxvi. p. 299 (1877).

force by time and adventitious circumstances. When he touches on theory he agrees with Thomson. Professor Clifton has examined the Volta effects for the substances ordinarily used in batteries with great care, and has probably elicited the maximum of accuracy possible to his method. He gives the E.M.F. of numerous virgin cells in which no current has circulated.

Ayrton and Perry in 1876 devised in Japan a very ingenious but somewhat unwieldy modification of Kohlrausch's method, and with the help of students carried out a most extensive and laborious series of determinations of metal/metal, metal/liquid, and liquid/liquid contacts*.

Fig. 4.—Ayrton and Perry's Apparatus for measuring the Volta effect with all sorts of substances.

The substances are arranged on the lower platform, as, for instance, the metal and liquid shown in the figure at P and L. The platform, A B, is capable of rotation through 180° on its railway, R. 3 and 4 are carefully insulated gilt plates fixed to a bar which can be raised and lowered. The experiment consists in lowering these plates close to the surfaces to be tested, and connecting them with each other for a short time; then raise them, rotate the platform through 180° , lower again, and connect them with a quadrant-electrometer.

* Ayrton and Perry, Brit. Assoc. Glasgow, 1876. No abstract printed. Part I., Proc. Royal Soc. 1877 or 1878, is a preliminary account. Part II. describes a metallic voltaic cell of magnesium and platinum and mercury, also some experiments on electrolytes of high resistance. Part III., Phil. Trans. 1880, is the complete account of their published electroscopic experiments.

Fig. 5.—End view of Ayrton and Perry's Apparatus.

On the appearance of Clifton's paper the year after, they issued a strongly worded claim* in respect both of priority and completeness—a claim which seems to me well established, for their results are the most comprehensive yet obtained, and the energy needed to devise, construct, and use such an apparatus as the one they depict must have been immense. A convenient summary of their numbers is to be found in Everett's 'Units,' second edition. The main result achieved by them is the experimental establishment of the summation law for all substances (this is not to be confused with Volta's summation or series law, which is only applicable to metals), viz. that the total E.M.F. of a closed circuit of any number of substances may be reckoned by adding up the Volta forces observed electrostatically for every pair of substances in contact. This law is, it seems to me, for reasons given later (§. 7), very probable theoretically; but still it was quite essential to have it experimentally established, especially as Ayrton and Perry point out that it is often called in question without good ground. The establishment of this law is, I say, perhaps their main work in this matter, besides the

* Ayrton and Perry: Letter published in 1877 by Meiklejohn (Yokohama).

observation of the Volta effect for various difficult substances, especially liquids and liquids.

Clifton arrives at the same conclusion with regard to summation, and gives handy diagrams, reproduced in Jenkins's 'Electricity,' of the contact-force at the different junctions. My own opinion is that the intended and obvious significance of these diagrams is theoretically wrong, but they embody certain experimental results conveniently, and they can be interpreted properly.

Both Clifton and Ayrton and Perry appear to believe in the great constancy of the value Zn/Cu . Clifton gives it as .8516 volt ("Quelle précision!" somewhat sarcastically ejaculates Pellat, who himself finds it to vary between .63 and .92). Ayrton and Perry assert that it is more constant than a Daniell. I believe that both Professor Clifton and Professors Ayrton and Perry have made several experiments besides those recorded in their communications to the Royal Society; but as they have not been published, I can give no account of them.

Among the Thèses presented to the Faculty of Science in Paris in 1881 we find an important memoir by Pellat*, which reviews the whole position very clearly, and records a series of determinations of Volta force among metals, determinations which are evidently the most accurate and satisfactory yet made. He adopts the capital experimental method of neutralizing the charge of a condenser by a Poggendorff or compensation method, and thus converts Kohlrausch's into a null method, for which a very sensitive electroscope is all that is needed. The plates of the two metals set face to face are connected, not directly, but by a greater or less length of a graduated wire conveying a current; and the position of the slider on the wire is adjusted by continually separating the plates and testing until no charge at all is found. The step of potential on the wire is then precisely equal to the "contact-force" between the plates; for this would have caused a charge in a similar but uncompensated condenser, and the step of potential on the wire has neutralized it.

Compensation methods of a sort had been used before by

* Thèses présentées à la Faculté des Sciences de Paris, pour obtenir le Grade de Docteur-ès-Sciences physiques, par M. H. Pellat, Professeur de Physique au Lycée Louis le Grand, No. 461; Juin 22, 1881. See also *Journal de Physique*, 1881, xvi. p. 68, and May 1880: "Différence de potential des couches électriques qui recouvrent deux métaux en contact."

Fig. 6.—Diagram of Pellat's Method.

Légende.

KL, compensateur.

C, curseur.

R, rhéostat.

P, piles Daniell fournissant le courant du compensateur.

 p, p' , plateaux du condensateur (p fixe, p' mobile). m , interrupteur.

F, feuille d'or.

A et A', plateaux attractifs de l'électroscope.

 b, b , batteries de 100 volts chacune pour charger les plateaux A A'.

The diagram, fig. 6, pretty well explains itself. The contact at m is broken the instant before p' is raised.

Fig. 7.—Pellat's Condenser.

Fig. 7 shows how this was done in practice. Pulling the string, A, raises a sliding-pin with a shoulder, g , fixed to it a millimetre below the bar, B. The pin thus first breaks contact near H, and then raises the hinged bar BB, to which the upper plate, M, is attached. The upper plate, being always in connection with a battery, requires no special insulation. The lower plate is carefully supported by a Thomson insulator, I. The lower plate was considerably smaller than the upper one, and it was further protected from stray inductions by a guard-screen. Screws, c, c , served to regulate the distance between the plates.

Gerland and by Thomson. Gerland applied compensation to determine the E.M.F. of two metals dipped into a liquid, and Thomson applied it to the divided-ring experiment, thus making it very analogous to that of Pellat*.

Pellat also adopts Sir William Thomson's view that the Volta effect is due to a true contact-force between the metals, and that it represents a real difference of potential between them when in contact; at the same time he is careful to point out that no rigorous proof can be given of this, and that all that is really and certainly measured electrostatically is the difference of potential between what he calls the electric coats (*les couches électriques*), or what may be more simply called the air-films, on the two touching metals.

The following is a summary of some of Pellat's measurements, gold being the metal with which all are compared, and the numbers being given in volts.

“Difference of Potential of the Electric Coats which cover a Metal and Standard Gold metallically connected, and both in Air.”

Metals used.	E.M.F. in volts.		Metals used.	E.M.F. in volts.	
	With very clean but scarcely scratched surface.	With surface strongly scratched by rubbing with emery.		With very clean but scarcely scratched surface.	With surface strongly scratched by rubbing with emery, or in some of the last cases with cloth or filter-paper.
Zinc	·85	1·08	Iron	·29	·38
Lead	·70	·77	Brass	·29	·37
Tin	·60	·73	Copper	·14	·22
Antimony ...	·44	·49	Platinum ...	—·03	+·06
Nickel	·38	·45	Gold	—·04	+·07
Bismuth.....	·36	·48	Silver	—·06	+·04
Steel	·29	·44			

* I find indeed that Sir W. Thomson completely anticipated Pellat in the application of galvanic compensation to the measurement of Volta effects; for in ‘Nature,’ April 14, 1881 (vol. xxiii. p. 567), is printed an account, given in brief at the Swansea meeting of the British Association (see Trans. of Sects. 1880, p. 494), which relates how the divided-ring experiment naturally developed into more complete compensation with slide resistances, and that an extensive series of measurements were made on this plan in the years 1859–61 with results quite in agreement with those published by Hankel in 1862. Other experiments were made since 1861 with results confirmatory of those of Pfaff, 1821, showing the Volta effect to be independent of the surrounding gas. The description of all these experiments was therefore withheld till something new should be obtained by the method, and was not published until Pellat's paper had

4. Meanwhile some experimenters, starting with a belief in the chemical origin of the Volta effect, had made experiments supposed to support this view. Mr. J. Brown, of Belfast, in 1878, repeated Thomson's divided-ring experiment *, as well as Kohlrausch's condenser-experiment, in other gases than air; and found a very decided difference, and even a reversal of sign, when sulphuretted hydrogen was substituted for air. The metals Brown used were copper and iron, and he obtained a 1-centimetre deflection in the direction indicating iron + in air; while in SH_2 he obtained a 3-centimetre deflection, indicating that iron was —. On readmitting air, the deflection again reversed, and so on until the copper coated itself with a blue film of sulphide, when the deflection became undecided, owing, as Brown supposes, to "the cessation of chemical action."

Fig. 8.—Mr. J. Brown's Arrangement for observing the Volta Effect in different Gases by Sir William Thomson's Method of a bimetallic ring with an electrified needle hanging over it.

appeared in the *Journal de Physique*, May 1880. Fig. 10 sufficiently exhibits Sir William's arrangement. In a postscript are described a few additional experiments of the same kind as those published in 1881 by Schulze-Berge, in which a platinum plate is soaked for a certain time in dry hydrogen or oxygen, and then used in the Volta condenser. The observation is made that merely soaking a plate in gas is more effective than electroplating it with the same gas with an E.M.F. of a volt.

* J. Brown, *Phil. Mag.* August 1878, Feb. 1879, and March 1881; see also *Brit. Assoc. Trans. of Sect.* 1881, and 'Electrician,' vol. vii. p. 165.

In 1881 he observed a time-change (decrease) of the Volta effect at a copper-zinc junction, and reckoned that at the first instant after cleaning the potential-difference would be as high as .9 Daniell, "which," he says, "agrees with J. Thomsen's determination of the difference of the heats of combustion of zinc and copper and oxygen." He here gives a hint of holding a heterodox notion which I do not find in any other of his writings, and which I believe he abandoned, even if he ever really held it.

In 1879 Brown tried a copper-nickel divided ring, substituting HCl for air, and here also succeeded in obtaining a reversal of sign. He also arranged a divided ring of wet blotting-paper, and showed that there was a difference of potential when the two halves were touched with a zinc-copper couple (which is not remarkable); but he then goes on to draw a moral, and to say that the slit of the divided ring corresponds to the air-film, and the wet paper to the moisture-film in the ordinary Volta-condenser experiment. The film of moisture on the zinc plate is thus shown to have a + charge, and that on the copper a negative. If it be objected that the better the plates fit, the better the manifestation of contact E, it is to be replied that it is not to be supposed that there is no air between them anyhow (says Brown). Probably, he says, gas produces the difference of potential only so far as it forms a film on the surface. When a metal and a liquid are experimented on, it is probably really a two-fluid cell, the other fluid being that condensed on the surface of the metal.

Brown thus goes strongly for the activity of the films, or condensed air-sheets, which certainly exist on the surface of solids, and which may play an important part in the matter; but he supposes that these films act by corroding or attacking the plates, and that such a film is necessarily existent between surfaces nominally in contact if any Volta effect is to be produced; so that if the metal faces really and truly touched all over, they would show no charge when separated. Moreover, he lays it down that the potential difference is only observed while chemical action is going on; but that so soon as it ceases, from any cause, at once the Volta effect ceases too. In all this I entirely differ from him; but his experiments are very interesting and much to the point.

They cannot, however, be regarded as settling the question—the very important and fundamental question—as to whether the Volta effect depends on the atmosphere or medium surrounding the plates, or whether it is an absolute effect depending on contact alone. Experiments on this point are absolutely discordant; and it seems to be one of those points

which it is very difficult to settle by direct experiment. For if by using a chemically active gas instead of air, you get a positive result or change in the Volta effect, the answer from the other side is: "Yes, of course, because your plates are corroded and coated with sulphide or chloride, or something whose contact-forces come in and modify everything." If, on the other hand, you get a negative result when you substitute some inert gas like hydrogen for air, then it is objected that you have not removed the air-film which the plates had contracted from long standing in the air; and if you answer indignantly that you did, and that your hydrogen was perfectly pure, it is replied with a sneer, "Oh yes, it is not so easy to get pure hydrogen as you seem to think."

Moreover, suppose a positive effect on changing the gas *was* established; what then? Nothing is settled except that the metal/air contact-force is proved to be somewhat different from the metal/gas contact-force. There seems to be really no way of knocking contact-force on the head experimentally, and this probably because it is a reality: *there really is* a contact-force at every junction of dissimilar substances; and the E.M.F. of a circuit, whether it be inductive or conductive, is always the sum of such contact-forces. I do not say that the contact-force at any given locality has the value ordinarily assigned to it as the result of experiment.

The earliest attempt made to examine the question as to whether the Volta effect depended on the atmosphere was made by Pfaff* in 1829, who used dry and damp air, oxygen, nitrogen, hydrogen, carburetted hydrogen, and carbonic acid, and he found that there was no difference so long as no visible chemical action occurred; but it must be noted that the opposing faces of his plates were varnished. De la Rive, on the other hand, asserted that there was no Volta effect in the slightly rarefied air then known as "vacuum."

In recent times Pellat has investigated the subject, and has come to a conclusion in agreement with Pfaff, viz. that the differences are very small. The metals used by Pellat were copper and zinc, and the gases were air dry and damp, dry oxygen, dry nitrogen, dry and pure hydrogen, dry and pure carbonic acid. He finds slight variations, but exceedingly slight, and such as Pfaff, Exner, and Brown could hardly have detected. He says:—"Au surplus il est fort probable que, si quelques-uns des auteurs précédents avaient tenté les expériences que j'ai faites au sujet des gaz, ils auraient trouvé des résultats négatifs; les faibles variations produites par le changement des propriétés du gaz que j'ai pu mettre nette-

* Pfaff, *Ann. de Chim.* 2 ser. xli. p. 236. The metals he employed were copper, tin, and zinc.

ment en évidence, grâce à la précision des mesures, sont au-dessous des erreurs expérimentales de leurs méthodes, ou à peine supérieures dans les cas les plus favorables."

Fig. 9.—Pellat's Apparatus for experimenting in different Gases and at different Pressures.

The movable plate is now the lower one, and it is pulled down by an electromagnet, E , a little way against the springs, B , which tend to drive it up against the screw-tops, C . Contact is automatically broken at q the instant before separation. The bell-jar has 35 litres capacity, the diameter of each plate being 15 centimetres. It must be impossible to employ anything like pure gases in a bell-jar enclosing such a bulky mass of heterogeneous material; and the pressure was found not to go below 2 or 3 centimetres of mercury. However, he has since made a smaller arrangement of 1 litre capacity, with plates 9 centimetres diameter, and, what is more important, with the electromagnet outside, and nothing inside but glass, mica, and metal. In this the pressure goes down to a millimetre. But even this is not all that could be wished. Moreover the experiments described had been made with the larger apparatus.

In all the above gases he has also studied the effect of varying the pressure. Lowering the pressure slightly *increases* the observed difference of potential; but the change lags a little behind the pressure-variation. Damp and dry air behave in the same way. In oxygen the effects of pressure are rather better marked. Nitrogen gives nearly the same numbers as air; but after it has been in for some time, the numbers are slightly lower than at first. Hydrogen gives a little greater effect than even rarefied air; rarefying hydrogen does not alter it much. Carbonic acid gives the same numbers as rare air or dense hydrogen. As for liquids, plates wetted with alcohol give the same result as if immersed in plain air.

Von Zahn* also tried a condenser in various gases, and found no difference; but when he tried a platinum-zinc condenser in the highly rarefied air now known as vacuum with some melted sodium in a branch tube to absorb all the oxygen, the Volta effect was diminished, and only represented a potential difference of half a Daniell. I am not clear whether sodium can be trusted to ultimately absorb every trace of oxygen; but I should judge it would take a very long time: and as to rarefaction, dividing the numbers of molecules in a vessel by a million or two leaves them quite numerous enough to accomplish anything they want.

Sir W. Thomson has also made experiments in different gases with negative results†. These experiments are not described in detail, but they were made with the apparatus shown in fig. 10.

The views of Ayrton and Perry on the subject of the effects of atmosphere underwent modification between their first paper and their third. In their first paper they say they have good reason to believe that there is no great difference of potential between a metallic or liquid surface and the air in contact with it

Clerk Maxwell, in a letter to the 'Electrician,' ‡ pokes fun at them for this, saying:—"A statement like this, coming from men whose scientific energy is threatening to displace the centre of electrical development, and to carry it quite out of Europe and America to a point much nearer Japan, is worthy of all attention, even without an explicit statement of their 'good reason.' But Mr. J. Brown has shown (Phil. Mag. August 1878) by the divided-ring method of Sir W. Thomson, that whereas copper is negative with respect to iron in air, it is positive with respect to iron in H_2S . It would

* Memoir quoted below.

† Thomson, Brit. Assoc., Trans. of Sect., Swansea (1880), p. 494.

‡ 'Electrician,' April 26, 1879.

Fig. 10.—Sir William Thomson's Compensation arrangement for measuring Volta-force in Air and Gases by a null method, devised and used in 1856, published in 1880.

The upper plate of the condenser is uninsulated, and slides up and down from one fiftieth to a quarter or half an inch from the lower insulated plate, which is permanently joined to the electrometer. The upper plate is put in communication with the lower one momentarily through the slide resistance, and is then raised, the position of the slide being adjusted till no deflection ensues.

appear, therefore, that the reason why the results of metals by the ordinary 'contact-force' experiments harmonize so well with the comparison by dipping both metals in water or an oxidizing electrolyte, is not because the E.M.F. between a metal and a gas or an electrolyte is small, but because the properties of air agree to a certain extent with those of ordinary electrolytes. For if the active component of the electrolyte is sulphur, the results are quite different; and the same kind of difference occurs when hydrogen sulphide is substituted for air."

In their third paper*, therefore, we find Ayrton and Perry's views changed, and they clearly state that their experiments, like all those based on inductive methods, leave the question of air-contacts quite undecided.

They then go on to say:—"One way of determining the E.M.F. of contact in volts between a substance and air, and a way we hope shortly to employ, is to repeat all these exact contact-experiments in different gases [it is not quite true that this would give the results required, because it would only give differential effects; very important to observe, no doubt, but not the same as observing the actual contact-force of air or of gas]. . . . We shall thus ascertain whether the contact-difference of potentials of a substance and a gas differs much for different gases. Qualitative experiments in this direction have already been made with very interesting results by Mr. Brown of Belfast, but his experiments differ from ours in not being quantitative." [Only, as their experiments do not yet exist, Mr. Brown's have still some value.]

In vol. xii. of Wiedemann's *Annalen* † I find some interesting experiments by Schulze-Berge on contact-force between metals and gases. He uses a condenser and different gases, but the plates of his condenser are both of the same metal, and he coats one of the plates with a film of the gas, say chlorine, or hydrogen, or ozone, and leaves the other covered with air. To measure the potential-difference he employs the compensation method of Pellat, and his arrangement seems fairly satisfactory. But he does not explicitly enter on the question as to the seat of E.M.F. in the Volta experiment (except in a controversy with Professor Exner). He assumes that a contact-force between metals and metals and between metals and liquids has been established, and he wishes to extend it to the contact of a metal and a gas.

* Phil. Trans. 1880.

† Schulze-Berge, "Ueber die Elektricitäts-erregung beim Contact von Metallen und Gasen," *Ann. der Phys. u. Chem.* xii. p. 293 (1881).

Believing firmly in the existence of films of condensed gas at the surface of a solid, which films require time for their formation or removal, he deems it sufficient to soak one of the plates of his condenser in the gas to be examined, and then to take it out and measure the difference of potential between it and the other plate coated only with a film of air. Tried thus, ozone rendered gold, platinum, and brass negative as against the corresponding air-covered plate. Hydrogen rendered its platinum strongly positive ; but its influence on gold was slight, and on brass uncertain. Chlorine and bromine made platinum negative, and ammonia made brass positive.

It may be readily objected that what the soaking with gas accomplished was not the formation of a film of gas, but a film of actual chloride, oxide, or other combination. Against this is to be urged the fact that after removal from the gas the effect diminished with time, and the plates gradually returned to nearly their former state. He tried if he could remove the gas film from one of the plates by exhaustion under an air-pump, and the plate so treated exhibited a difference when taken out and compared with an ordinary plate ; but he was cautious enough to repeat the experiment, leaving the plate under the bell-jar for the same time and not exhausting. The same difference appeared, so he attributes it to possible grease.

This is the right sort of way to make experiments ; and if everybody experimented with proper care there would be vastly fewer papers published, and science would progress on the whole faster.

At present it feels to me overladen with a mass of publication, mostly of necessity by men of not absolutely the first order, much of it with no sort of clearness or insight, but rough, crude, and ill-digested. A man makes a number of experiments ; he does not stop to critically examine and weigh them, and deduce from them their meaning, nor indeed does he often take the trouble to examine whether any definite meaning can in their then shape be drawn from them ; but he rushes with them into print, producing a memoir of wearisome length and sometimes extreme illiterateness of style.

Some one else then has the trouble of wading through the heap to see whether any fragments of value may perchance be imbedded in it, and probably he is unable to come to much definite conclusion, because he cannot be in so good a position for criticism of the experiments as the original author was. He therefore writes a paper pointing out defects and errors in the communication. Others take up the same line,

the original man replies, and so there is a controversy, and nothing is really settled at all. Finally, some one else independently goes over the whole ground from some distinct point of view, makes a few well-planned, clear, and decisive experiments, describes them in a compact and readable form, and there results a definite gain to science. But how much better would it have been if this last paper had been the only one published! Unless a man is an experimental genius of the highest order, it is necessary for him to think for far more time than he experiments, if he wishes to advance and not to lumber his science. If it be objected, as indeed it may with great truth be, that one man's life and capacity are not sufficient for this in the present state of knowledge, the objection constitutes a strong argument in favour of the proposition that the time has come for an organization of science and a more definite division of labour.

To return to the experiments of Herr Schulze-Berge, one is not able to say after all that they are very satisfactory, for they do not distinctly settle any question. The general conclusion he draws from them is the apparently safe one that the contact-force between a metal and a gas is not in general the same as between a metal and air. Even this is not absolutely safe, however, because it might conceivably be that an air-gas contact-force caused all the difference. Granting that this is unlikely, the experiments are in favour of a contact-force between metals and air or gas; but they do not establish the fact any more strongly than, if so strongly as, Mr. Brown's experiments had already done. The weak point in both is the possible corrosion of the plates and formation of films of alloys or compounds, which may be the real source of the observed difference of potential.

And against the existence of a contact-force between metals and various gases, the experiments of Pellat and others are to be remembered, which resulted in the conclusion that a condenser made of two different metals showed nearly the same Volta effect, whether the atmosphere surrounding the plates was air or hydrogen.

If it be assumed that the experiments of Brown and Schulze-Berge establish their point, and that Pellat's apparatus for different gases (fig. 9) is satisfactory (rather a large assumption), I am unable to reconcile the discrepancy, except by suggesting that Pellat did not take sufficient pains to remove the condensed-air sheet originally on both his plates. It is of course just possible that the *difference* between the potentials of the two metals might be the same in two gases though the absolute potential of both was different, but it is improbable.

In this connection I must notice also a rather long memoir* by Dr. W. von Zahn, published in 1882, which reviews the whole subject, and describes an elaborate series of measurements made with an apparatus something like what one might suppose Ayrton and Perry's to become if it were arranged for use in different gases and *in vacuo*. He refers with admiration to Pellat's work in the preface; and I do not suppose imagines that his own numerical determinations can compare with Pellat's for accuracy where they overlap, seeing that he only makes use of a sort of combination of Kohlrausch's and Hankel's methods, with a Hankel electrometer as a measuring instrument†. He has tried, however, a large number of substances as well as ordinary metals, such as powdered antimony, iron and nickel reduced by hydrogen, many kinds of carbon, Fe_3O_4 , manganite, pyrolusite, copper oxide, lead "hyperoxide," iron glance, and other minerals. He has measured the Volta effect in various gases and at different pressures, and finds, like Pellat, that it does not appreciably vary.

He has also examined the effect of temperature on the Volta effect, though he appears to think that this ought to bear some close relation to the phenomenon of Seebeck, a natural mistake many years ago when made by Avenarius, whom it led most happily, though fortuitously, to the true, and by him experimentally verified, law of E.M.F. in a thermoelectric circuit‡. However, Zahn finds that experiment lends no support to this view, and says that a larger series of results must be obtained before basing a theory on them. Von Zahn is a confirmed contact theorist, and he victoriously assails several experiments supposed to be distinctly in favour of a chemical view of the Volta effect. He says he publishes his results because of the extraordinary discoveries being propounded by Professor Exner (such as that a thermopile will not work in a vacuum)§,

* *Untersuchungen über Contactelektricität*, von Dr. W. v. Zahn: Leipzig, Teubner, 1882.

† A Hankel electrometer is a modification of Bohnenberger's, in which a battery with middle to earth is substituted for the dry pile; the plates on either side of the gold leaf are minutely adjustable, and the motions of the gold leaf are read by a microscope. It is sometimes preferred to a quadrant for its small capacity and dead quick motion; it can be made very sensitive, but it can hardly be a satisfactory measuring instrument. Pellat used it, but only as an electroscop.

‡ Avenarius, "Die Thermoelektricität, ihrem Ursprunge nach, als identisch mit der Contactelektricität betrachtet," Pogg. *Ann.* cxix. 1863. See also Pogg. *Ann.* cxxii., where he proceeds to calculate Volta effects from thermoelectric data.

§ I have been unable to find this extraordinary statement in Exner's works, but it is quoted again by Ayrton and Perry, *Phil. Mag.* 1881, p. 49. Exner seems to have said that the thermoelectric power of bismuth-

and because of the vague and unsatisfactory views of physicists in general on the matter (*e. g.* of Hart); but I am bound to say that, so far as I can judge, Dr. von Zahn's own experiments are not of that conclusive and decided character that one had hoped for from his start, and he signally fails to sum up the facts in a neat and crisp manner. He adheres to the contact view, but his adhesion scarcely seems to me to be based on strong evidence; and in fact his theoretical views seem a little superficial considering the date at which he writes; so that one may admit pretty well all he says about contact and not chemical action as the cause of the Volta effect without being deeply committed to any specially true or specially false position*. His best experiment, and a very crucial one if only it could be perfectly performed, is the attempt to measure the Volta effect in an absolute vacuum. A pair of zinc and platinum plates are sealed up in a brass and glass vessel in such a way that one of them is capable of moving up and down, and thus of varying its distance from the other. Gravity is employed to separate the plates, the whole vessel being inverted. The vessel is filled with dry nitrogen and exhausted for some days, occasional heat, P_2O_5 , and melted sodium, being employed to improve the vacuum.

The result is that the Volta effect is very decidedly "too small," going down to half a Daniell, so far as the measurements made by his not entirely unobjectionable method can be trusted; but he does not seem to think that this is much of an argument either way, and, not finding any further change after some days, he did not pursue the investigation further by letting in some air and seeing whether the old value is restored, though he perceives clearly that this would be a crucial experiment. This abstinence is so remarkable that it

antimony is destroyed by immersing the pile in pure nitrogen; and Young of Princetown takes the trouble to examine whether it is so experimentally (see *Phil. Mag.* x. 1880, p. 450), and finds the thermoelectric power of metals the same at one millionth atmosphere as at 1. This, however, would be altogether inconclusive if experiment were needed to settle it.

* I am afraid this is not peculiar to Dr. von Zahn. It may be that the German writers on the subject are too busy accumulating facts to care much about their precise theoretical bearing; but I notice a very loose and unsatisfactory way of putting forward secondary matters as if they were the real points at issue, and of never really getting to the heart of the matter. It is singular that the four questions or heads under which that eminent writer, Professor Wiedemann, proceeds to discuss the attempts which have been made to settle the question of the seat of E.M.F. are such that if a categorical answer to each were, by supernatural means, vouchsafed to us, we should be, I believe, none the wiser (Wiedemann, *Elek.* vol. ii. p. 985, new edition).

seems necessary to quote his own grounds for it, and I do so in a footnote*.

Fig. 11.—Von Zahn's Apparatus for experimenting in different Gases at different Pressures and Temperatures, and also in high Vacua.

The upper plate, M, is attached by glass rods to a sledge, H, which slides on vertical steel rods, G, being pulled up by a string. On reaching its highest point it comes into contact with an insulated platinum wire, S, which communicates with a Hankel electrometer through one of the insulated exits, R. The bottom plate is supported on the ring, E. T is the stopcock for exhaustion and change of gas. Fig. 2 shows the appendage to the bottom plate for warming it by a current of steam. Fig. 3 shows the apparatus used for high vacua. The two plates are zinc and platinum, and the platinum is arranged to fall by its own weight when the whole thing is inverted. The diagram shows it in its highest position, and just going to fall back into its dotted position. The friction of its guide-rod seems not wholly satisfactory.

* "Andere etwas bessere Beobachtungsreihen gaben ähnliche Werthe. Bei allen war die Kleinheit von C. (the apparent Volta effect Zn/Pt) auffallend. Nun stellt ja dieser Werth, wie oben besprochen, nicht die Potentialdifferenz Zink-Platin dar, für seine Kleinheit muss aber eine anderweite Ursache vorliegen. Dieselbe kann ich zunächst nur darin finden, dass die Zinkplatte bei Anfertigung und weiterer Behandlung des Apparates sichtbar angelaufen ist (auf der einen Hälfte sogar blaue Färbung angenommen hat).

"Es liesse sich allerdings vermuthen, dass diese offenbar zu geringe

Zahn goes on to describe an experiment with bright sodium *in vacuo* instead of zinc, the sodium having been long kept melted in a laterally connected bulb before being introduced into position. He finds the sodium strongly positive to copper; but there can be nothing crucial about this experiment, I imagine, for metal in contact with glass may so easily give rise to disturbing electrifications.

I believe he must have employed the best vacuum of any experimenter on this subject, and that he has therefore gone most near to the proof of what I cannot help believing will be found to be the truth, viz. that the Volta effect in an absolute vacuum or perfectly inert gas (old air-sheets *et hoc genus omne* having been thoroughly removed) is very small. But if it be the case, as I believe it is, that the effect is almost independent of the *quantity* of oxygen present, so long as it is present, the difficulty of making the experiment so as to be sure of the absence of even the last few thousand or million oxygen molecules is almost overwhelming. The question of the dependence of Volta force on atmosphere remains thus undecided; and all the evidence which I can adduce in favour of such dependence is this incipient decrease observed by von Zahn, the little too-mixed-up observation of Mr. Hart (described later), the measurements of Schulze-Berge, and the more decided experiments of Brown. It may indeed be readily held that the weight of experimental evidence tends the other way, since most experimenters on the subject (Pellat, Schultze-Berge, von Zahn, and I may add Sir W. Thomson) have left off just as pure contact theorists as they began. I would attempt an experiment myself, save that I am so profoundly impressed with the difficulty of making one in which no fault or loophole can be found, and which will by every one be deemed satisfactory and final; so I prefer to base my views on a general survey and on fairly conclusive reasoning, rather than on a crucial but almost impossible experiment.

5. Perhaps this is now the place to refer to the somewhat erratic series of papers by Professor Franz Exner, of Vienna*.

Differenz in der wirklich wesentlichen Verminderung von Feuchtigkeit und Sauerstoff gesucht werden müsste, so dass der Apparat nach dem Oeffnen eine stärkere Spannung zeigen würde. Dies wäre dann wirklich ein *experimentum crucis* zu Gunsten der chemischen Theorie. Diese Entscheidung vorzunehmen wird aber erst dann nothwendig sein, wenn nach längerer Zeit, wo das eingeschmolzene Natrium noch mehr alle Reste von Sauerstoff beseitigt haben wird, eventuell noch Wiederhitzen und dergl. der jetzige Zustand des Apparates unverändert wieder gefunden sein wird."—*Von Zahn's Memoir*, p. 48.

* Exner, *Sitzb. der Akad. der Wissensch. Wien*: July 1878, "On the Nature of Galvanic Polarization;" July 1879, "On the Cause of the Production

He sets himself to disprove the existence of contact-force in the most straightforward and obvious manner, and to establish the fact that there is no electrical evolution without definite and actual chemical action. To this end he announces the following propositions:—(1) that two metals in a chemically indifferent medium show no electricity; (2) that the potential-difference of two connected metals in air is exactly half the difference of their heat-combustion energies; and (3) that two pieces of the same metal produce contact-electricity as soon as they are put into chemically different atmospheres.

The experiments by which he supports these assertions have, every one of them, been elaborately and severely criticised by Beetz, Hoorweg, Julius, Schulze-Berge, Von Zahn, Ayrton and Perry, Pellat, and Wiedemann; and his numerical determinations of contact-force appear to be unique*.

It is not necessary for me to enter into a discussion on the merit of his experiments, inasmuch as the mere fact of the existence of so great a body of hostile opinion is sufficient to show that they are not of a kind best qualified to produce conviction. The theoretical views which led Professor Exner to formulate his second statement above, that the potential-difference of two connected metals is equal to half the difference of their heats of combustion per equivalent, are, I am sorry to say, quite unintelligible to me. They depend on the hypothetically necessary existence of films of oxide, between which and the metal there is supposed to be a considerable difference of potential. Perhaps a few quotations from Professor Exner's first paper on contact-electricity will render his position clearer†. His views are but little really different

of Electricity by the Contact of Heterogeneous Metals;" December 1879, "On the Theory of Inconstant Galvanic Elements;" May 1880, "On the Theory of Volta's Fundamental Experiment;" July 1880, "On the Theory of Galvanic Elements;" November 1880, "On the Nature of Galvanic Polarization;" July 1882, "On some Experiments relating to Contact Theory."

* Beetz, Wiedemann's *Annalen*, xii. p. 290; Hoorweg, *ibid.* xi. p. 133 (1880), and xii. p. 90; Julius, *ibid.* xiii. pp. 276 and 296; Schulze-Berge, *ibid.* xv. p. 440, as well as xii. pp. 307 and 319; Von Zahn, p. 41 and Preface, of his Memoir; Ayrton and Perry, *Phil. Mag.* 1881, p. 43; Pellat, *Paris Thèses*, No. 461, p. 17; Wiedemann, *Elektricität*, ii. pp. 992-995.

† I quote from Mr. J. Brown's translation (*Phil. Mag.* Oct. 1880) of a paper in Wiedemann's *Annalen* of the same year, with some abbreviations:—"An investigation concerning the nature of galvanic polarization has led me to a quite distinct view of the origin of the so-called contact-electricity, a view which will be supported by experiments following. I have shown that the original cause of the polarization-current is to be sought for, not at the contact of the electrodes with ions liberated on

from those of De la Rive and other older "chemical theorists;" but they are (especially in later papers) expressed in so definite and decided a manner that they have excited a sharper controversy than vaguer and more hesitating writings could. This indeed may be regarded as their special merit. The main objection which can be taken to them relates to the quantitative statements: these are vigorously made, but they seem unwarranted by facts accumulated by all other observers, though indeed some of his own experiments certainly seem to support them. It has also been objected that he misinterprets some of his experiments.

He has got hold of the notion that the heat of combustion has some sort of relation to the Volta effect, and there I am heterodox enough to agree with him. But what the relation is, and how it acts, and what sort of potential-difference you ought to expect in accordance with theory, concerning all these things I am utterly at variance with him; and I deem it prudent not to attempt to represent views which I am unable to understand, because it is unlikely that I should do them justice.

them, but in the recombination of the latter; and the E.M.F. of the current so produced is measured by the heat-value of this combination, just as the E.M.F. of any galvanic cell is measured by the heat-value of the chemical process going on in it. With a so-called contact-action the existence of the polarization-current, and obviously of every other current, has nothing whatever to do. The idea then suggested itself to seek for the cause of the production of electricity in the experiment of Volta, not in the contact of two metals, but in previous chemical actions of the surrounding media on their surfaces. I have expressed the opinion that so-called contact-electricity is produced by the oxidation of the metal in contact by the oxygen of the air, just as in galvanic cells it is evolved by oxidation of zinc. If the supposition prove true, and it has proved true, the E.M.F. of his metal in contact in air must be measured and expressed by their heats of combustion."

He then points out how all Volta tension series are in oxidation order, and relates approvingly De la Rive's view that metals in air were attacked not only by water-vapour but by dry oxygen, and that electricity is produced by any kind of chemical action in proportion to the intensity of the chemical affinity. Then he gives his numerical theory and supporting experiments; and finally concludes:—"I believe we are entitled to say that no *Scheidungskraft* exists at the contact of two metals." The following must take the place of Volta's law of the evolution of electricity:—"The difference of electric potential between two metals in contact is measured by the algebraic sum of the heat-value of the chemical action going on at each."

In his theory and experiments, and all through the rest of the paper, he considers the difference of potential equal to *half* the difference of heat-values, so the above last statement must be a slight numerical slip.

The above extracts are among the most favourable. It would be easy to select passages from this, and from his other memoirs on the subject, of a more surprising character.

Professor Exner, to strengthen his position, adduces a large number of very simple experiments (such as connecting first one Daniell and then two Daniells to an electrometer, and observing that in the second case the deflection is double the first), and from them he obtains equations proving algebraically that $Zn/Cu = 0$. Considered as conundrums these equations are ingenious, but it is a waste of time seriously to discuss them as Herr Julius has done in an elaborate manner. To suppose that such everyday experiments as these are in direct contradiction of the contact theory is scarcely complimentary to the great men who have held, and who still hold, that view.

Dr. C. G. Knott, in 1879*, examined the contact-force between plates of the same metal at different temperatures, using the condenser or Kohlrausch method. He found that iron, copper, zinc, and probably tin, were negative when hot to the same metals cold; and the effect increases uniformly with temperature. But it is permanent, remaining after the hot plates have cooled down; hence it must be due to oxidation.

A slow oxidation proceeds with time alone. Time-curves are logarithmic like cooling-curves, and the most oxidizable metal varies most quickly both for time-variation and temperature-variation. There seems to be a surface-condition of a metal proper to each temperature which no polishing can change, for it establishes itself in a few seconds after cleaning, and only changes with temperature.

Mr. S. Lavington Hart, in 1881†, describes a mercury-dropper where the mercury is contained in a funnel, and is connected with an electrometer by an iron rod dipping into it. The drops form inside an iron inductor, and they fall negatively charged. Mr. Hart so far ignores any Volta force that he considers the arrangement as an inversion of Lippmann's electrometer, the advancing drops being oxidized. It can plainly be regarded, however, as a mere Fe/Hg contact arrangement; and that is what I suppose it to be. He makes two interesting modifications: the first is to replace the air round the dropping mercury by coal-gas; the electrical effect is then zero. This is interesting, because the exuding drops of mercury, unlike most pieces of metal, expose to the coal-gas a virgin surface which has probably contracted no condensed air-sheet: only coal-gas is a rather sophisticated substance for

* Knott, Proc. R. S. Edinb. 1879-80, No. 105, p. 362.

† Hart, Brit. Assoc. York, p. 555; and Phil. Mag. November 1881, ser. 5, xii. p. 324.

it to be first exposed to. If the experiment is regarded as sufficiently direct and simple, this fact lends support to the view that Volta forces depend on the medium surrounding the metals.

The second modification is to bring an earth-connected iron bar close to the drops, and to show that it reduces the deflection. Mr. Hart thinks it reduces the oxidation by proximity; and certainly, provided the obvious action of a mere electrostatic screen has been considered and provided against, the action by proximity is very remarkable. A similar effect has been observed and more fully worked out by Pellat, in a paper published in 1882*. Pellat says that if he places two metallic surfaces parallel to one another and very close together (say half a millimetre more or less, variations from 12 to $\cdot 1$), each metal undergoes a slight alteration of the properties of its superficial coat. The alteration takes some minutes to produce, increases with time, but tends to a limit. When the influencing metal is removed, the other returns gradually to its primitive state. Lead and iron produce the largest influence effects; copper, gold, and platinum give smaller but distinct effects; zinc produces hardly any, unless it be put within a hundredth of a millimetre or so. Pellat does not attempt to account for this interesting phenomenon further than by suggesting some possible connection with the smell of metals.

Mr. Hart's theoretical views are at first sight somewhat analogous to my own, though they are by no means the same. He considers the case of two metals immersed in liquid electrolytes, and dismisses air by calling it a gaseous electrolyte. He believes zinc and copper in contact to be at the same potential, and throws the variation of potential on the air between them†. He considers the electrical effect brought about by the electro-negative ion oxygen combining with the zinc and charging it negatively, while some electro-positive ion combines with the copper and charges it positively, "though not unless the two metals are in sufficient proximity to overcome electrolytic diffusion" (whatever that may mean as applied to this case). He thinks his mercury-dropping experiment in coal-gas is conclusive as to the equality of potential of metals in contact. This, I fear, is rather rapid induction.

* *Comptes Rendus*, xciv. (1882) p. 1247: "Influence of Metals on one another at a Distance."

† The diagrams of potential which Mr. Hart gives of cells were given more fully by Prof. Exner in his paper on the Theory of Galvanic Elements (1880). Mr. Hart's views are, in fact, rather similar to some of the more reasonable ones of Prof. Exner.

I do not see how it follows on his own hypothesis that his arrangement is virtually a reversed Lippman electro-meter.

Sir W. Thomson's dropping arrangements, or voltaic cells, in which gravity does the work instead of chemical action, are so well known that it is scarcely necessary to do more than refer to them. Mr. Hart's mercury-dropper is scarcely a modification of the copper-filing dropper shown in fig. 12. Sir William also shows how to couple up such cells in series*, and how to construct a mechanical replenisher on the Volta principle (fig. 13).

Fig. 12.—Thomson's Gravitation Voltaic Cell.

a copper filings. *c* receiver.
b inductor—zinc. *d* copper funnel.

The copper filings drop negatively charged against electrical forces. If you join *c* and *d* by a copper wire, you can get a current flowing wholly through and with copper.

Fig. 13.—Thomson's Voltaic Induction Machine.

One of the inductors, *T*, is lined with one metal, the other with another, and the two connected. The carrier-wheel is rotated, and the contact-springs, *A*, *A'*, become oppositely charged. By afterwards charging the inductors with a Daniell cell, and comparing the deflection now produced in an electrometer connected to *A*, *A'* with what it was before, measurements of Volta effect can be obtained; or of course it can be made a null method.

6. In order to give this historical sketch more completeness, it may be as well to record rapidly such other memoirs as I have been able to get acquainted with: it is in the highest degree probable that several are omitted, but I hope no very important ones. Prof. Wiedemann's collection of views and memoirs bearing on the subject is at the end of the second volume of the new edition of his *Elektricität*.

* Electrostatic Reprints, p. 325.

Edlund has published a long paper* in which he investigates experimentally the Peltier effect; he points out clearly at the end that there is no relation between the Peltier and Volta effects; and he suggests that this is because of the contact-force between the metals and the gas or air in which they are, the fact of such contact-force being, he thinks, sufficiently established by gas-batteries and galvanic polarization†.

Majocchi, in a paper printed in *Phil. Mag.* xxx. p. 97, regards the E.M.F. of contact as due to the "adhesion" of the two metals for each other: pretty much the same idea as Sir W. Thomson's chemical action at a distance, an idea which makes the energy of the Volta effect Zn/Cu depend on, and be calculable from, the combination-heat of zinc and copper in making brass. I must return to this matter later, because it is important in itself and crucial as regards theory.

Gassiot‡ made an experiment intended to show that there could be a difference of potential excited between metals by proximity without actual contact, or at any rate without metallic contact. Grove§ also made a similar experiment.

Hoorweg||, and also Nobili¶, have a theory that all galvanic currents are really thermoelectric.

In the article "Electricity" in the *Encyc. Brit.* p. 99, Professor Chrystal gives some clear general considerations regarding the seat of E.M.F. and the opposing views which are held with regard to it. He is judicious in his attitude with regard to them; but the mere statement of the position in so

* Edlund: *Pogg. Ann.* cxxxvii. p. 474; cxl. p. 435; cxliii. pp. 404, 534. See also *Phil. Mag.* [4] xxxviii. p. 263; xliii. pp. 81, 213, 264, especially p. 273.

† Sundell investigates the E.M.F. of alloys in contact with copper, employing Edlund's method, and finds, like him, that for alloys as well as for simple metals the Peltier corresponds with the Seebeck force. The peculiar language used in this and the preceding paper may easily cause it to be imagined that they have found Volta force to agree with Peltier. In fact, Sundell is so quoted in Watts's 3rd Suppl. p. 708. Von Zahn quotes Edlund in the same sense; and indeed it is probable that Edlund himself at first thought he was investigating Volta forces thermoelectrically (*Pogg. Ann.* cxlix. p. 144).

‡ Gassiot, *Phil. Mag.* xxv. 1844, p. 283.

§ Grove, 'Literary Gazette,' Jan. 21, 1843; Wiedemann, *Elek.* ii. p. 988.

|| Hoorweg, *Wied. Ann.* ix. p. 552, 1880; xi. p. 233; and xii. p. 75.

¶ Prof. Wiedemann notes as interesting that in 1828 Nobili held a notion that all galvanic currents are thermoelectric, thus vaguely anticipating the modern thermodynamic theory of E.M.F. See *Wied. Elektricität*, ii. p. 985, and Nobili, *Bibl. Univ. de Genève*, xxxvii. p. 118. But Prof. Hoorweg seems bitten with the same idea in recent times, and in 1879-80 writes long papers in proof that all current-energy is due to absorption of heat at junctions!

clear a form is in itself a powerful argument for the views held by Maxwell*.

Prof. Fleeming Jenkin, in the last edition of his 'Electricity and Magnetism,' p. 216, endeavours to reconcile the contact and chemical theories. According to the chemical theory, the E.M.F. of a cell $= \Sigma(J\theta\epsilon)$; according to the contact-theory it is $C/L + L/Z + Z/C$. On these undoubted facts he proceeds to found a number of statements which are true†, though scarcely simple; in fact they perhaps rather tend to complicate what may be held to be a simple matter.

Schönbein, in a letter to Faraday published in the *Philosophical Magazine* for 1838‡, throws out a remarkable sugges-

* Although this article is, or ought to be, easily accessible to everybody, there is one important suggestion in it which it is as well to quote, viz. that contained in the following sentence:—"We are so ignorant of the nature of the motion which is the essence of the electric current that the very form in which we have put the question [as to the locality of E.M.F.] may be misleading. If this motion be in the surrounding medium, as there is great reason to believe it to be, it would not be surprising to find that speculations as to the exact locality of the E.M.F. *in the circuit* were utterly wide of the mark." Prof. Willard Gibbs suggested something of the same sort at Montreal, though in a rather vaguer form. I do not myself feel any doubt that a precise location can be given to the E.M.F., notwithstanding that much of the current energy exists in the medium. The most complete attention to the distribution of energy in circuits which has yet been bestowed on the subject has been given by Prof. Poynting in his remarkable memoir (*Phil. Trans.* 1884), and he therein locates the E.M.F. of a battery exactly where I do myself.

† Except, indeed, a doubtful statement at the end of Number 2 and an erroneous bit of reasoning at the end of Number 4, though the conclusion drawn is correct.

‡ Schönbein, *Phil. Mag.* vol. xii. pp. 225 and 311. The two most striking sentences are here extracted:—

"Before closing my letter, allow me to communicate to you in a general manner the view which I have taken of the subject in question. In the first place, I must tell you that I am by no means inclined to consider mere contact in any case as the cause of the excitement of even the most feeble current. I maintain, on the contrary, in accordance with the principles of the chemical theory, that any current produced in a hydro-electric voltaic circle is always due to some chemical action. But as to the idea which I attach to the term 'chemical action,' I go further than you and M. de la Rive seem to go; for I maintain that any tendency of two different substances to unite chemically with one another must be considered as a chemical action, be that tendency followed up by the actual combination of those substances, or be it not; and that such a tendency is capable of putting electricity into circulation."

And on page 314 he explains this last phrase, which he has elsewhere called a "current of tendency," thus:—

"As what I term a current of tendency is no doubt in some cases nothing but that electrical state which the Voltaists consider to be the effect of their 'force electromotive,' or of contact, it appears to me that, from some of the facts above stated, a specific and most important conclusion regarding the theory of the pile can be drawn. Even if we

tion with regard to "chemical tendency" as the possible source of a current, or rather of "force electromotive." His language and ideas are in many respects old-fashioned and erroneous; he uses such phrases as "a current of tendency," he supposes currents with no electrolytic power to exist, and of course is not troubled about energy considerations. But I feel little doubt that, had he lived later, he would have held that while *currents* were due to chemical action, *electromotive force* was due to "chemical tendency;" and this is pretty exactly my own view of the matter.

I have only just discovered this Schönbein letter; and I have also found some paragraphs in Faraday which more in detail and with fair distinctness express what I believe to be the true view. (See §§ 893-900, Exp. Res. vol. i. *)

grant to the Voltaists our current of tendency to be the effect of mere contact, the facts alluded to prove that such a current does not possess a sensible degree of electrolyzing-power; consequently that the chemical effects of the common voltaic arrangements have nothing to do with current-electricity excited by contact."

* Extract from Faraday's 'Experimental Researches,' vol. i. :-

"(893) The use of *metallic contact* in a single pair of plates and the cause of its great superiority above contact made by other kinds of matter, become now very evident. When an amalgamated zinc plate is dipped into dilute sulphuric acid, the force of chemical affinity exerted between the metal and the fluid is not sufficiently powerful to cause sensible action at the surfaces of contact, and occasion the decomposition of water by the oxidation of the metal, although it is sufficient to produce such a condition of the electricity (or the power upon which chemical affinity depends), as would produce a current if there were a path open for it; and that contact would complete the conditions necessary, under the circumstances, for the decomposition of water.

"(894) Now the presence of a piece of platina touching both the zinc and the fluid to be decomposed opens the path required for the electricity. Its *direct communication* with the zinc is effectual, far beyond any communication made between it and that metal (*i. e.* between the platina and zinc) by means of decomposable conducting bodies, or, in other words, *electrolytes*, as in the experiment already described [that of decomposing iodide of potassium without metallic contact by interposing it on blotting-paper between the platinum and the zinc of a simple voltaic cell], because, when *they* are used, the chemical affinities between them and the zinc produce a contrary and opposing action to that which is influential in the dilute sulphuric acid; or if that action be but small, still the affinity of their component parts for each other has to be overcome, for they cannot conduct without suffering decomposition; and this decomposition is found *experimentally* to react back upon the forces which in the acid tend to produce the current, and in numerous cases entirely to neutralize them. Where direct contact of the zinc and platina occurs, these obstructing forces are not brought into action, and therefore the production and the circulation of the electric current, and the concomitant action of decomposition are then highly favoured.

"(895) It is evident, however, that one of these opposing actions may be dismissed and yet an electrolyte be used for the purpose of completing

Various observations regarding the E.M.F. of different cells are made in the series of papers still appearing in the *Philosophical Magazine* by Dr. Alder Wright and Mr. C. Thompson, "On the Determination of Chemical Affinity in terms of E.M.F."

For much discussion of contact-electricity and for some interesting statements of the views of Marianini, Davy, and others, refer to 'Experimental Researches,' vol. ii. p. 20 &c. From what is there said, it appears that Karsten and Marianini held a modified contact theory, placing the E.M.F. at the metal-fluid junction; and that Becquerel admitted as a possibility the efficiency of chemical attraction, as distinct from combination, something in the same way as Schönbein.

Professor Tait in his 'Thermodynamics' lends his powerful support to the contact view of the activity of the pile as taught by Sir W. Thomson.

Some work has been done in the direction of observing reversible heat-effects at metal-liquid junctions, notably by Joule, Thomson, and Bosscha. We shall have occasion to refer to this work later.

Joule in 1841 sent currents through several dilute acid voltmeters with different electrodes, and measured the excess or reversible heat $H - RC^2$ generated in the whole cell; with the result that the excess of heat observed is that due to the observed back E.M.F. of the cell, minus that concerned in the decomposition of water. A table of his results is given by Chrystal, *Encyc. Brit.* p. 91. For Maxwell on the same subject see 'Elementary Electricity,' p. 146.

Thomson (Math. and Phys. Papers, pp. 496, 503) says that, of two decomposition-cells, one with zinc cathode, the other with platinum cathode, the former showed the most heat when the same current was sent through both. Separation

the circuit between the zinc and platina immersed separately into the dilute acid; for if in the above experiment the platina wire be retained in metallic contact with the zinc plate, and a division of the platina be made elsewhere, then the solution of iodide placed there, being in contact with platina at both surfaces, exerts no chemical affinities for that metal; or, if it does, they are equal on both sides. Its power, therefore, of forming a current in opposition to that dependent upon the action of the acid in the vessel is removed, and only its resistance to decomposition remains as the obstacle to be overcome by the affinities exerted in the dilute sulphuric acid.

"(896) This becomes the condition of a single pair of active plates where *metallic contact* is allowed. In such cases, only one set of opposing affinities are to be overcome by those which are dominant in the vessel; whereas, when metallic contact is not allowed, two sets of opposing affinities must be conquered (894)."

ting the electrodes by a porous cell, zinc cathode showed more heat than zinc anode, but platinum anode more heat than platinum cathode*. He speaks of the local heat developed at a tin surface, and shows that it is greater where hydrogen is liberated than where tin is dissolved; and suggests thermal observations on four dilute-acid voltmeters in one circuit with zinc and platinum electrodes, arranged according to the permutations—zinc-zinc, zinc-platinum, platinum-zinc, and platinum-platinum. Thomson attributes the extra heat at an electrode to opposing chemical affinities which have to be overcome—a doctrine of “chemical resistance.”

Bosscha examines and develops all these matters in a series of interesting papers published about 1857†. He attributes the development of local heat, at a cathode against which hydrogen is liberated, to the change of hydrogen from the nascent condition to the ordinary one—in other words, to the energy of the molecular combination H, H . He finds the electromotive forces exhibited by this local generation of heat at the surface of different metals in acid to have the following values in volts:—

Pt	Fe	Cu	Sn	Hg	Zn
·45	·49	·64	·86	1·2	1·2

One more memoir I must mention before closing this historical sketch and discussion thereon, a valuable communication by Bouty to the *Journal de Physique* ‡, “On Thermoelectric Force at Contact of Metals and Liquids, and on the Peltier Effect thereat.” He finds the Peltier coefficient at a junction of copper with salts of copper eighty times as great as at an iron-zinc junction, and eleven times as great as bismuth-copper. He also measures the metal-liquid thermoelectric E.M.F. at different temperatures, and shows that Thomson’s thermodynamic formula

$$J\Pi = \frac{TdE}{dT}$$

is perfectly true and in agreement with experiment in these cases also. He endeavours to see if this Peltier, or, as we had better call it for distinction, Joule or Bouty effect, can be calculated from the energies of combination. After tabulating his results alongside of heats of oxidation and heats

* Showing, I suppose, that while zinc attracts oxygen much and hydrogen not at all, platinum attracts hydrogen more than oxygen.

† Bosscha, *Pogg. Ann.* vols. ci., ciii., cv., cviii.

‡ Bouty, *Journal de Physique*, 1879, viii. p. 341; ix. pp. 229 and 306, especially p. 306.

of solution, he decides that it is hopeless, and that we must give up trying to establish a relation between these quantities. Chemical action, he concludes, only disturbs the effect by altering the surfaces, and by developing parasitic heats. They may mask, but they do not produce, the true Bouty phenomenon, which he believes is probably physical.

The difficulties of making these measurements are exceedingly great; and, notwithstanding the ingenuity and skill displayed, it seems to me possible that some error or unexpected source of disturbance may have modified the results. So far as I know, they have not yet been repeated; and I can hardly regard the experimental method used as perfectly safe*.

[To be continued.]

XXI. *Some Principles and Results of Harmonic Motion.*

By PLINY EARLE CHASE, LL.D.†

WHEN two bodies, in relative motion, come into contact, pressure begins to act between them to prevent any parts of them from jointly occupying the same space. . . . Any force in a constant direction acting in any circumstances, for any time great or small, may be reckoned on the same principle; so that what we may call its whole amount during any time, or its '*time-integral*,' will measure, or be measured by, the whole momentum which it generates in the time in question" (Thomson and Tait, '*Natural Philosophy*,' i. sections 294, 297).

No relative motions can be more important, and no contact more complete, than those which exist between the all-pervading luminiferous æther and the sun. In the conversion of tangential luminous waves into spherical vibrations, gravity is acting in a constant direction towards the sun's centre. The amount of its activity upon each æthereal particle, μ , or its time-integral during each cyclical period, t , is measured

* I find that a method exactly like that used by Bouty was suggested by Clerk Maxwell, '*Elementary Electricity*,' p. 146.

† From the Journal of the Franklin Institute, January 1885. Communicated by the Author.

This paper has been written, in part, to meet a desire which has been expressed by some members of the Royal Astronomical Society for an introduction to the article on "*Harmonic Motion in Stellar Systems*" (Phil. Mag. Sept. 1884). The author will feel under obligation to any readers who will inform him of other points which need further elucidation.