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

CONDUCTED BY

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> "Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

VOL. XXIV.—FIFTH SERIES.
JULY—DECEMBER 1887.

LONDON:
TAYLOR AND FRANCIS, RED LION COURT, FLEET STREET.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua ..... Admiratio generat quæstionem, quæstio investigationem investigatio inventionem."—*Hugo de S. Victore.*

——"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas,
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

ALERE FLAMMAM.

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ERRATUM.

Page 195.—In equation (49) of Sir W. Thomson's paper, *for* $\mathrm{L}f(y)+\mathrm{MF}(y)$ *read*

$$\frac{1}{2\sqrt{(m^2+q^2)}}\Big[\epsilon^{y\sqrt{(m^2+q^2)}}\int_0^y dy\,\epsilon^{-y\sqrt{(m^2+q^2)}}\big(\mathrm{L}f(y)+\mathrm{MF}(y)\big)$$

$$-\epsilon^{-y(m^2+q^2)}\int_0^y dy\,\epsilon^{y\sqrt{(m^2+q^2)}}\big(\mathrm{L}f(y)+\mathrm{MF}(y)\big)\Big]$$

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[FIFTH SERIES.]

JULY 1887.

I. *Conduction of Heat in Liquids.—Historical Treatment.*

By C. Chree, *M.A., Fellow of King's College, Cambridge*[*].

POSSIBLY the earliest experiment of any note on the conduction of heat in liquids is due to Despretz[†]. He supplied hot water at intervals of five minutes to a metal vessel in contact with the upper surface of water contained in a vertical cylinder, and observed the readings of a series of thermometers at different depths, some in the axis and others at or near the cylindrical surface. He specially observed the temperatures in the stationary condition when the heat supplied just balanced that lost by the apparatus. He found that the temperature in the axis of the cylinder then followed precisely the same law as in a metal bar of small cross section heated at one end to a constant temperature; *i. e.* if v denote the temperature at depth x, he found $v \propto e^{-qx}$, where q depends on the material and dimensions of the cylinder. By comparison of two cylinders of the same material he found $\log q \propto (\text{diameter})^{-\frac{1}{2}}$, a relation which also holds in the case of metal cylinders. Near the surface of the water the temperature did not vary much over a horizontal cross section, but at depths of the same order as the diameter of the cylinder the variation was very considerable. As a single experiment occupied Despretz for sixty hours, and the method seems

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[*] Communicated by the Author.

[†] *Comptes Rendus*, 1838, p. 933; and *Ann. de Chim. et de Phys.* 1839, lxxi. p. 216.

scarcely fitted to supply accurate numerical results, the interest attaching to his observations is chiefly historical.

A slight modification of Despretz's method was applied by Herr Paalzow[*], who sought to determine whether there was any connexion between the conductivities for heat and for electricity possessed by various liquids. He observed the stationary temperatures finally indicated by four thermometers at different depths below the surface of the liquids, where the heat was applied. The formulæ he employed do not seem likely to give anything directly proportional to the conductivity, but his observations were sufficient to prove that no relation exists between the conductivities for heat and for electricity. For instance, water and sulphuric acid conducted heat very similarly, the former being, according to Paalzow, slightly the better conductor.

In the article on Heat, in the *Encyclopædia Britannica*, Sir W. Thomson quotes with approval the value obtained by Mr. J. T. Bottomley for the conductivity of water. The method employed was a slight modification of Despretz's, hot water being cautiously poured on the top of a board, of diameter slightly less than that of a vertical wooden cylinder containing water on which the board floated. The method seems open to the same objections as Despretz's, and the comparison of other fluids with water would involve considerable difficulties. Bottomley found for the absolute conductivity of water in the C.G.S. system ·002; but, as will be seen, this differs somewhat widely from subsequent and more reliable results.

In the 'Philosophical Transactions' for 1869, p. 637, is a paper on our subject by the late Prof. Guthrie. As the memoir is readily accessible, it is sufficient to say that his apparatus consisted of two equal hollow metal cones, which could be placed with their bases horizontal at small determinate distances apart. The liquid to be examined was introduced between the bases, where it was maintained by the capillary forces at its surface. A current of hot water was circulated through the upper cone, and the temperature of the air in the lower cone was determined by the height of a column of coloured fluid in a tube communicating with the interior of the cone.

The deductions made by Prof. Guthrie from his experiments are of an extremely fallacious nature, and as they are not infrequently quoted it seems desirable to examine them briefly.

In his first series of experiments a hot current was suddenly

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[*] Pogg. *Ann.* cxxxiv. 1868, p. 618.

instituted in the upper cone, and the time-interval observed when an alteration in the level of the coloured fluid became apparent. This he denotes by t, and calls the "time-of-heat penetration." He denotes by T the temperature of the lower cone, by dT the excess of temperature of the current in the upper cone, and by δ the thickness of the liquid layer. He found that t diminishes when dT is increased, and is not directly proportional to δ. He thence unwarrantably assumed the conductivity to increase with the temperature. Possibly a clear idea of his views will best be given by a statement he makes on page 649:—"If we had found that the time t . . . had been the same for heat of all temperatures, we should of course expect to find the time t proportional to the thickness δ." A very slight acquaintance with the mathematical theory is sufficient to show that this is incorrect. In fact there should be an immediate effect gradually increasing; and the interval, prior to its detection, depends on the delicacy of the apparatus and the fineness of the observer's senses.

In a second series of experiments he obtained what he terms a "measure of the resistance" of the conductivity. First, the bases of the cones were placed in contact, and the level of the coloured fluid read off at equal intervals of time subsequent to the commencement of a hot current in the upper cone. The cones were then separated to a certain distance, a liquid introduced, and the experiment repeated. The difference between the readings in the two cases he considered a direct measure of the quantity of heat stopped by the liquid, and this he supposed to measure the thermal resistance, *i. e.* the reciprocal of the conductivity. On page 658 he says:—" . . . the specific heat of the liquid in these contact experiments is of no influence;" and on the following page he shows how to deduce numerical results from the observations.

The theory is altogether faulty, and the numerical results, though doubtless accurate measures of something, are totally misleading. I have been unable to find any means of obtaining probable results from the data supplied.

There are two points, however, of great importance that Prof. Guthrie's experiments seem to establish. Apart from conduction proper or convection-currents, there are still two ways in which heat might travel from the upper cone to the lower. This might take place by radiation; and that a certain amount of heat is so transferred is unquestionable. That it is comparatively small may be deduced from the fact that a decided interval (several seconds) elapsed after the heat was applied before any movement of the coloured fluid could be detected. Prof. Guthrie was evidently much influenced by ideas derived from Prof. Tyndall's experiments on radiant heat, and seemed ready to attach at least sufficient importance to this agency. He assured himself, however, that the insertion in the liquid of a thin film of Swedish filtering-paper, which would have a great influence on radiant heat, produced scarcely any effect in his experiment.

The other possible disturbing cause is diffusion. Guthrie, however, painted the base of his upper cone with a soluble aniline dye, but observed no trace of colour in the liquid near the lower cone, and thence concluded that no sensible part of the phenomenon was due to this agency. These results are of fundamental importance from their bearing on the methods of subsequent observers.

Herr Lundquist[*], in a paper which I have been unable to meet with, obtained, by the same method as Ångstrom employed for metals, absolute values for the conductivity, k, of several liquids. The following results, in which the units are centimetre, gramme, and minute, are given in a paper (presently to be considered) by Herr Weber, who considers Lundquist's method very accurate but very tedious.

	Temperature, Centigrade.	k, Lundquist.	k, Weber.
Water	40·8	·0937	·0953
Salt-solution, of density 1·178,	43·9	·0897	·0901
Sulphate of zinc, of density 1·382,	45·2	·0952	·0872

For the last liquid Lundquist took as the specific heat the value ·77, while the correct value according to Weber is ·697, and the corrected value of k would be ·0862. Weber determined k at only two temperatures, the highest being 23°, and deduced the third column of the preceding Table by assuming the conductivity to vary uniformly with the temperature. Thus the agreement of his results and Lundquist's does not, as will more fully appear subsequently, form so convincing a proof of their accuracy as might be thought at first sight.

Herr Winkelmann[†] has investigated the conductivity of a considerable number of liquids. His apparatus consisted essentially of two brass cylinders, one enclosing the other. The inner was completely closed, save for a small hole at the centre of its upper surface in which was fastened a glass tube. This tube passed through a corresponding hole in the

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[*] *Upsala Universitets Arsskrift*, 1869, p. 1.

[†] Pogg. *Ann.* cliii. p. 481.

upper surface of the outer cylinder, and could be fixed there in such a position that the distance between the surfaces of the two cylinders was everywhere the same. The tube was then bent twice at right angles, and terminated in a beaker of mercury. The height of the mercury in the tube varies with the temperature of the air in the inner cylinder, which may thus be determined. The liquid to be examined was enclosed between the cylinders, and communicated with the exterior only through a small funnel in the upper surface of the outer cylinder, which was also originally filled with the liquid. This little arrangement kept the space between the cylinders always filled.

The experiment consisted in plunging the apparatus into ice-cold water and observing the height of the mercury at subsequent intervals of time. Denoting the temperature of the enclosed air before immersion by τ_0, and at a time t after immersion by τ, Winkelmann uses the formula $vt = \log(\tau_0/\tau)$, where v is the "velocity of cooling." He assumes that the temperature of the enclosed liquid is the same at equal distances from the surface of the outer cylinder; that this outer cylinder is at the same zero-temperature as the surrounding ice-water; and that the inner cylinder, the liquid layer touching it, and the enclosed air are always of one temperature. He then takes for the temperature of the liquid the formula

$$u = \tau_0 e^{-vt}(\alpha x + \beta x^2 + \gamma x^3);$$

where x is the distance from the outer cylinder, and α, β, γ are constants determined from the conditions assumed at the bounding surfaces of the liquid.

That results based on so many assumptions should not be altogether satisfactory is not surprising; and Winkelmann found, from observations with three different apparatus, that the value of the conductivity supplied by his formulæ rose as the thickness of the liquid layer was increased.

He had previously found that the outer cylinder became surrounded by a layer of water decidedly above the freezing-point, and employed a stirrer to promote circulation. He attributes the discrepancy in the results obtained from the three double cylinders to the fact that the stirrer, while maintaining the curved surface of the outer cylinder at 0°, did not remove the heated water from its top or bottom. Thus, denoting the true conductivity by K, while p stands for the ratio of the top and base to the entire surface of the outer cylinder of the apparatus which gave for the apparent conductivity the value k, he uses the equation

$$\mathrm{K} = k + npv,$$

where n depends only on the nature of the liquid. From the three double cylinders, for each of which p is known, he thus gets three equations. Any two of these give a value for n, and thus three values may be deduced for K. According to Winkelman, these values agreed marvellously for nearly every liquid examined. The following Table contains the apparent conductivities k_1, k_2, k_3 deduced from the three apparatus, in which the thicknesses of the liquid layers were ·2, ·26, and ·5 centim. respectively. K denotes the true conductivity according to Winkelmann.

	k_1.	k_2.	k_3.	K.
Water	·0624	·0697	·0850	·0924
Salt-solution	·0658	·0869	·1261	·1605
Alcohol	·0294	·0359	·0650	·0904
Carbon bisulphide	·0357	·0446	·0826	·1186
Glycerine	·0404	·0413	·0435	·0449

The units are the centimetre, gramme, and minute, and the results apply to a temperature of from 10° to 18°.

A theory in which the corrections required are so large can hardly be considered satisfactory, and in Wiedemann's *Annalen*, Bd. x., it meets with a very severe criticism from Herr Weber. He attributes the discrepancy in the results given by the three apparatus to convection-currents, which he says would, in the case of the liquids of least internal friction, become very prominent unless the thickness of the liquid layer were small. He considers the results given by the first apparatus to be moderately good, but those given by the other two and Winkelmann's corrections to be useless. In the same number (p. 668) there is an elaborate reply by Winkelmann, who shows that Weber's remarks as to the effects of internal fluid friction are scarcely borne out by the facts; but he does not succeed in proving his method to be satisfactory.

The next observer, Herr Beetz*, has dealt with a large number of liquids, including various solutions of different strengths. His apparatus consists of a long cylindrical glass whose axis is vertical, enclosed in a coaxial glass of slightly greater diameter fused to the inner glass near its mouth. The

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* Wied. *Ann.* vii. p. 435 (1879); and *Sitzungsberichte der math.-phys. Classe der k. b. Acad. der Wissenschaften*, Heft i. p. 86 (1879).

liquid is introduced between the two glasses, the interval between which can communicate with the exterior by a small lateral aperture, some distance below the junctions of the glasses but above a fixed level, to which the enclosed liquid is brought. In the mouth of the inner glass is a cork holding fast the stem of a thermometer with a long bulb. The inner glass is filled with mercury up to a fixed level slightly lower than the level of the liquid to be examined, and the thermometer is fixed so that its entire bulb is surrounded by the mercury. The distance between the curved surfaces of the glasses is small compared to the radius of the inner glass, which in turn is small compared to the length of the glasses or of the bulb of the thermometer. Thus fairly good results may be expected from a mathematical treatment which regards the liquid as existing between two infinite circular cylinders.

Beetz's main observations consist of two series, one at a low, the other at a higher temperature. In the former the whole apparatus was immersed in an ice-bath and removed when the enclosed thermometer indicated 1° C. It was then wiped dry, and, on the thermometer indicating 2°, plunged into a bath of water at 20°. The bath was kept full to the top, so that the apparatus was always immersed to the same depth. The moment when the thermometer indicated 4° was taken as a starting-point, and the times of rising successive intervals of 2° carefully noted. In the second series the apparatus was immersed in a water-bath of over 45°, and at 44° plunged into the water-bath of 20°, the times of cooling successive intervals of 2° being reckoned from the moment when the thermometer stood at 40°. For the velocity v of cooling or heating, Beetz assumed the formula

$$vt = \log(\tau_0/\tau)\ ;$$

where τ_0 is the difference between the temperatures of the enclosed thermometer and the bath at the instant from which the time is reckoned, whilst τ is the difference at time t. He supposed the conductivity of the liquid to be proportional to a quantity C given by $10^{-5}\mathrm{C} = v \log e$. He found that the above formula did not apply to the interval immediately subsequent to the immersion of the apparatus, and that towards the end of the experiment it again failed. For the intervals corresponding to the limiting values 6°–14° in the first series of experiments, and 36°–28° in the second, it answered very well. The discrepancy in the later stage of the experiment seemed due to the gradual collection round the apparatus of a layer of water differing in temperature from the rest of the bath.

That Beetz's formula is incorrect has been clearly pointed out by Weber*. He looks on the liquid as bounded by two infinite cylinders and supposes the thermometer, the surrounding mercury, the inner glass vessel, and the inner surface of the liquid to be at one temperature. He thence deduces for the temperature-difference the equation

$$\tau = \Sigma \mathrm{A} e^{-\frac{k}{\rho c} m^2 t},$$

where t is the time, k the conductivity of the liquid, ρ the density and c the specific heat of mercury, while A is a constant. The summation extends to all values of m given by the equation

$$mr_1 \frac{\mathrm{J}_0(mr_1)\mathrm{Y}_0(mr_2) - \mathrm{J}_0(mr_2)\mathrm{Y}_0(mr_1)}{\mathrm{J}_1(mr_1)\mathrm{Y}_0(mr_2) - \mathrm{J}_0(mr_2)\mathrm{Y}_1(mr_1)} = \frac{2\rho c}{\rho_1 c_1};$$

where r_1, r_2 are the radii of the surfaces bounding the liquid, whose density is ρ_1 and specific heat c_1, and J, Y stand as usual for the two solutions of the Bessel's equation of the degree indicated by the suffix. Supposing $r_2 - r_1$ small compared to r_1, it is easily proved that the least root of the above equation is much smaller than the next. This explains why, except at the commencement of the heating or cooling, the retention of but a single term, viz. $\tau = \mathrm{A} e^{-\frac{k}{\rho c} m^2 t}$, gives a fairly satisfactory result.

Weber contents himself with proving the falsity of Beetz's equation. It is not, however, difficult to obtain an approximate value for the least value of m, and thus to deduce from Beetz's observations fairly satisfactory values for the conductivity. Thus, supposing $m(r_2 - r_1)$ small, we may put

$$\mathrm{J}_0(mr_2) = \mathrm{J}_0(mr_1) + m(r_2 - r_1)\mathrm{J}_0'(mr_1),$$

where

$$m\mathrm{J}_0' \equiv \frac{d}{dr}\mathrm{J}_0,$$

and use a similar result for $\mathrm{Y}_0(mr_2)$.

Also $\mathrm{J}_0' = -\mathrm{J}_1$, and $\mathrm{Y}_0' = -\mathrm{Y}_1$;

thus the equation reduces to

$$m^2 r_1(r_2 - r_1) = \frac{2\rho c}{\rho_1 c_1},$$

whence

$$\tau = \tau_0 e^{-\frac{nk}{\rho_1 c_1} t},$$

where n is constant for the apparatus. We should thus get $k \propto \rho_1 c_1 \mathrm{C}$; *i. e.* we have only to multiply Beetz's numbers by the specific heat of unit volume of the liquid considered to

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* Wied. *Ann.* Bd. x. pp. 480–490.

obtain values of a moderate degree of accuracy. In every case, however, a certain amount of error will exist, for glass is a very bad conductor; and so the inner glass vessel, however thin, will vary in temperature throughout its thickness, and the thermometer thus will not give the temperature of the inner layer of the liquid. The connexion between the outer and inner glasses, and the divergence of the apparatus from the form assumed in the mathematical investigation, have also some effect.

The question of convection-currents was carefully considered by Beetz. He introduced some lycopodium-seed into the liquid under investigation and watched it during the experiment through a microscope. He had no difficulty in detecting currents travelling down the one glass surface and up the other. Experimenting with water at the lower temperature he gradually thickened it with meal till no currents appeared, but found scarcely any change in his value for the conductivity. He even boiled the water and meal and let it cool, so as to form a thick paste, without producing much effect. He thus concluded that at low temperatures the convection-currents were negligible. With our amended formula for the conductivity his conclusion would scarcely be justified; bnt without exact knowledge of the change in the density and specific heat it is impossible to deduce trustworthy results. At higher temperatures Beetz found convection-currents to play a decided part. As the thickness of his liquid layer was the same as in Winkelmann's smallest apparatus, these facts go far to justify Weber's objections to Winkelmann's method.

Beetz examined a very large number of solutions of different strengths, in all cases stating the density; thus, where the specific heats are known, his results can be easily modified according to the amended formula. His two series of observations answered to mean temperatures of about 11° and 32° respectively, and thus measures of the increase of the conductivity with the temperature might be deduced. As there is a comparatively small change in the specific heat of unit volume of most liquids for a temperature-variation of 21°, the numbers actually given by Beetz would suffice. The values so deduced are, however, generally several times greater than those obtained by more trustworthy methods, which points distinctly to an increase of convection-currents with the temperature. Beetz introduced in the liquids colouring-matter variously affecting radiant heat, but could detect no change in the conductivity. He is thus at one with Guthrie in this matter.

The following Table gives a summary of Beetz's results for such of the liquids and solutions as I can find the specific heats of. The first column of relative values is that deduced by Beetz's formula, the second that deduced by mine, taking the conductivity of water as 100.

	Density.	Conductivity relative to water; Beetz's formula.	Conductivity relative to water, amended.
NaCl solution	1·11	103·1	98·1
"	1·2	104·6	97·9
Sulphuric-acid solution..	1·083	99·1	96·0
" " ...	1·496	97·6	77·0
" " ...	1·806	90·8	
Glycerine	1·228	82·1	61·0
Ether	·724	112·3	42·3
Alcohol	·804	87·0	41·9
Chloroform	1·488	113·0	39·1
Carbon disulphide	1·272	123·9	39·0
Olive-oil..................	·915	64·2	28·2
Benzine	·691	99·1	26·0

These results belong to a temperature of about 11° C. Similar results, answering to a temperature of 32° C., could in like manner be deduced from Beetz's experiments; but, as already mentioned, they would be less trustworthy, owing to the existence of convection-currents.

Some very careful experiments on conduction have been carried out by Herr Weber*, who has also made some important criticisms on previous methods. I shall consider his theory and experiments in connection with a very powerful paper by Herr Lorberg†. This is based entirely on Weber's work, and gives a more exact mathematical treatment of the subject. It also attempts to show that Weber's deductions from his observations require amendment.

Weber's apparatus consists of two plates of copper, about 8 centim. each in radius, whose faces are very accurately plane. During the experiment both plates are exactly horizontal, with their centres in one vertical line, and the lower supports the upper by means of three equal symmetrically disposed small pieces of some badly-conducting substance. These keep the plates at a distance of ·231 centim. apart. The upper plate is about 1·023 centim. in thickness, and the lower about half as much. The liquid is introduced and maintained between the plates, much as in Prof. Guthrie's apparatus; only for some of the more mobile liquids Weber had to attach a small glass rim to the lower plate, forming,

* Wied. *Ann.* x. pp. 103, 304, 472; and xi. p. 347.

† *Ibid.* xiv. pp. 291 & 427.

with the upper plate, a closed vessel. Two wires of different metals have their one junction fused to the upper surface of the upper copper plate, and their other junction kept in ice. This supplies a thermoelectric current, which traverses a reflecting galvanometer, whose readings supply data for the calculation of the temperature of the hot junction. When the apparatus has been for some time at a fixed temperature, as indicated by the constancy of the galvanometer-reading, the lower plate is suddenly placed on a horizontal block of ice at 0°; a screen of metal, maintained at 0°, is at once put over the apparatus; and the time noted. The weight of the apparatus presses out the melted water, and so keeps the surface of the ice-plate horizontal and its temperature at 0°. After about two and a half minutes, readings of the galvanometer are begun, and repeated at regular short intervals. The law of cooling of the hot junction is thus obtained; and the dimensions and other properties of the apparatus being known, the conductivity of the liquid may be calculated.

Weber's mathematical treatment is not satisfactory, as he started with an unwarrantable assumption, so as to lighten the work, which presents serious difficulties. He first attempts to show that, practically, the upper copper plate is at one temperature throughout. Let u denote the temperature at time t, ρ the density, c the specific heat, k the internal and h the external conductivity of the liquid layer, whose thickness is Δ; and let the same letters, with suffix 1, refer to the upper copper plate. Then, in treating the plate, Weber at once assumes that at its lower surface

$$u_1 = u \quad \text{and} \quad k_1 \frac{du_1}{dx} = k \frac{u}{\Delta}.$$

The former relation may be correct, and Lorberg considers that it follows from Poisson's theory of heat. Further, most, if not all, writers on the present subject have assumed that there is no discontinuity in temperature in passing from any one medium to another in contact with it. This is a matter of great importance when the conduction takes place through thin layers, and any uncertainty on the point is much to be regretted. The results of direct experiments, however, have been contradictory; and several high authorities hold a contrary opinion. The second equation is, as Lorberg has shown, undoubtedly erroneous; thus, Weber's proof that the copper plate is isothermal is little better than a pure assumption. Granting, however, the first equation, Lorberg shows conclusively that either copper plate is very nearly at one temperature throughout.

Weber next considers the liquid layer separately; and his work, though of a somewhat approximate nature, is fairly satisfactory. His conclusions are practically in accord with those deduced by Lorberg from a very able and complete treatment. Lorberg considers the case of n horizontal disks of the same radius, but of different materials, one above another, all originally at one temperature. The lowest is suddenly put on an ice-plate, and the whole surrounded by an atmosphere at 0°. The problem is very complicated, especially as Lorberg is driven to attach different meanings to the same letter in different parts of the work, and has to use an elaborate system of suffixes. He applies the results to three disks—copper, water, copper—in order, but complicates matters still further by inverting his previous notation.

The conclusion come to both by Weber and Lorberg is that, when cooling has lasted a comparatively short time, the temperature of the hot junction is given with sufficient accuracy by a single term,

$$u_1 = \mathrm{A}e^{-\mu^2 t}.$$

According to the former,

$$\mu^2 = \frac{k}{\rho c} q^2;$$

while the latter gets

$$\mu^2 = \frac{k}{\rho c} q^2 + \frac{2}{\mathrm{R}} \frac{h}{\rho c},$$

where R is the radius of either plate. Neglecting smaller terms, Lorberg's equation for q is, in the present notation,

$$\frac{\rho_1 c_1}{\rho c} \Delta_1 \tan q\Delta = 1 - \frac{\Delta^2}{q^2} \frac{\rho c}{\rho_1 c_1} \frac{h_1}{k} \left(\frac{2}{\mathrm{R}} + \frac{1}{\Delta_1} \right) + \frac{\Delta^2}{q^2} \frac{h}{k} \frac{2}{\mathrm{R}};$$

while Weber's differs only in leaving out the term in h.

In Lorberg's equation (25), from which the above equation is taken, there are several terms of a lower order. These are deduced from his general equation (22), with which I entirely agree. I further coincide with the approximate equation given above, but after carefully working through all the algebra, which Lorberg leaves the reader to supply, am unable to agree entirely with these secondary terms. The discrepancy, however, would affect the value of q only to a very small extent. Lorberg finally gives

$$q^2 = (\cdot 231)^{-2} \left\{ \cdot 2539 - \cdot 1472 \frac{h}{\rho c} \right\}$$

$$= 4\cdot 758 - \text{term in } h;$$

while Weber has

$$q^2 = 4{\cdot}778.$$

Lorberg's work, with the arithmetical coefficients of which I do not entirely agree, would indicate that the term in h, itself supposed to be a very small quantity, is negligible without sensible error. Thus, Lorberg's more exact treatment would alter Weber's values for the conductivity by less than $\frac{1}{2}$ per cent., which is far less than the probable experimental error. In my opinion Lorberg, in not a few cases, uses more figures after his decimal point than circumstances warrant. He is also entirely dependent on Weber's measurements of the apparatus, which are hardly likely to possess the extreme accuracy he supposes. It would, for instance, be difficult to determine the distance between the copper plates exact to one-hundredth of a millimetre; but an error of this amount would produce as great an effect in the value of the conductivity as Lorberg's corrections do.

I shall now examine somewhat closely Weber's and Lorberg's numerical deductions from the former's experiments. When dealing with water at a low temperature, Weber took observations every ten seconds. He denotes by x_n the $(n+1)$th galvanometer-reading; thus x_n and x_{n+6} denote readings at an interval of one minute. A fall of 1° in the hot junction corresponds to a decrease of about 17 divisions in the galvanometer-reading. The Table on p. 14 is part of a table given by Weber*, along with an extension by Lorberg †.

The first reading corresponds to a temperature 15°·59 in the hot junction, and the last to 3°·56.

The application of this table follows very simply from the formula

$$u = \mathrm{A}e^{-\mu^2 t}.$$

Thus, assuming $u \propto x$, and counting t in minutes, we get

$$\mathrm{M}\mu^2 = \log_{10}\frac{x_n}{x_{n+6}} \quad \text{and also} \quad = \log_{10}\frac{x_n - x_{n+6}}{x_{n+6} - x_{n+12}},$$

where

$$\mathrm{M} \equiv \log_{10} e = {\cdot}43429\,\ldots.$$

If, then, the mathematical theory were in all respects satisfactory, the mean values in the third and fourth columns should agree. The fourth column is due to Lorberg, and did not come under Weber's attention. The latter, however, points out that in the third column there is, on the whole, a

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* Wied. *Ann.* x. p. 308.

† Wied. *Ann.* xiv. p. 442.

Time.	$\log_{10} \frac{x_n}{x_{n+1}}$.	$\log_{10} \frac{x_n}{x_{n+6}}$.	$\log_{10} \frac{x_n - x_{n+6}}{x_{n+6} - x_{n+12}}$.
h ′ ″			
3 2 0	·02551	0·16611	0·17507
10	·02838	·16567	·17068
20	·02958	·16579	·17784
30	·02563	·16354	·17327
40	·02969	·16248	·16745
50	·02732	·16208	·17072
3 0	·02507	·16199	·17532
10	·02850	·16337	·17933
20	·02733	·16031	·17173
30	·02457	·15916	·17422
40	·02929	·16024	·17216
50	·02723	·15822	·16399
4 0	·02640	·15609	·15589
10	·02544	·15628	———
20	·02618	·15529	Mean... 0·17136
30	·02565	·15265	
40	·02727	·15502	
50	·02510	·15571	
5 0	·02664	·15618	
10	·02445	———	
20	·02354	Mean...0·15980	
30	·02802		
40	·02796		
50	·02557		
6 0	———		
	Mean... ·02668		

gradual decrease in $\log \frac{x_n}{x_{n+6}}$. This would indicate that μ^2, and so also k, which varies as μ^2, diminishes as the temperature falls. Thus, strictly, k should have been treated as depending on the temperature; however, the variation is not very rapid, and so Weber supposed it legitimate to take the mean value supplied by the third column in calculating the conductivity of water at the mean temperature of the experiment. This gives $\mu^2 = \cdot 3680$, and thence $k = \cdot 0768$, corresponding to a temperature of about 4°·1, according to Weber's formulæ.

Lorberg considers the fourth column to give a correct value for μ^2, and explains the difference between it and the third column by a theory presently to be considered. From this one experiment he finds, by the method of least squares, $\mu^2 = \cdot 39556$, with a mean error of ·00067, and thence deduces $k = \cdot 08317 - \cdot 0046\, h$. If, as he supposes, h be certainly less than ·006, the last term is of no practical importance.

From the mean of a considerable number of experiments, Weber finds $k = \cdot 0745$ at 4°·1 and $= \cdot 0857$ at 23°·67; thence he assumes $k=k_0(1+\alpha u)$, where $\alpha=\cdot00786$ and k_0 refers to 0° C. Lorberg, applying his own method of calculation to one of Weber's observations at the higher temperature, found $\mu^2=\cdot42877$; whence $\alpha=\cdot005$. Lorberg employs his own value for α to show that the divergence between his and Weber's values for $M\mu^2$ in the first experiment are much too great to be due to the change in k. Employing Weber's value for α, which seems fairer, I find that only about one half the divergence could be accounted for. Lorberg further considers the fluctuations in Weber's table of values for $M\mu^2$ to be considerably beyond the limits of errors of observation, and finds that they cannot be explained by supposing k, k_1, &c. to be quadratic functions of the temperature. The following is the theory he propounds to account for all the phenomena:—

It is, he says, very unlikely that the metal cover of the apparatus remains at 0°, but, on the contrary, it will after a short time assume a temperature θ. This obviously will modify the temperature of the apparatus, and will, he supposes, produce in the upper copper plate a constant excess τ over the temperature given by the mathematical theory. Thus the temperature measured by the galvanometer is really $\tau+u$; and, the deflection varying as the temperature, we should have

$$x = s + Ae^{-\mu^2 t},$$

where s is a constant given by

$$s(1-e^{-\mu^2})=x_{n+6}-e^{-\mu^2}x_n \equiv g_n, \text{ say.}$$

The formula $\log\frac{x_n-x_{n+6}}{x_{n+6}-x_{n+12}}$ obviously eliminates s and gives μ^2 directly. Lorberg then forms a table of values of g_n, and from them deduces $s=8\cdot27$. He also has a table of values of $\log\frac{x_n-x_{n+12}}{x_{n+6}-x_{n+18}}$, and of a corresponding quantity g_n'. This gives a value for μ^2 agreeing well with his other value, and gives $s=8\cdot24$. He points out, what is unassailable, that a given error in the scale-reading would produce a much smaller effect on Weber's quantities than on his own. Assuming $\cdot4$ of a scale-division as a superior limit to the error, he points out that this would not account for the fluctuations in Weber's table. It would, however, correspond to an error of $\cdot011$ in the value of $\log\frac{x_n-x_{n+6}}{x_{n+6}-x_{n+12}}$, and of $\cdot7$ in the value of g_n. Lorberg thus considers the fluctuations in his own tables remarkably small and fully accounted for by errors of reading.

It is quite true that the greatest deviation of g_n from its mean value is only ·6; but as this mean value is only 2·7, a greater accuracy would seem desirable. Also, in $\log \frac{x_n - x_{n+6}}{x_{n+6} - x_{n+12}}$ one deviation is as much as ·015, which exceeds Lorberg's limit. This quantity is further so much of the nature of an average, that large deviations could hardly be expected. I have constructed a table of values of $\log \frac{x_n - x_{n+5}}{x_{n+5} - x_{n+10}}$; and though the mean value deduced for $M\mu^2$ is nearly the same as Lorberg's, the numbers fluctuate from ·1875 to ·1522, and so represent a deviation much above what Lorberg allows to be possible. I have also calculated s after Lorberg's method from the formula

$$s(1 - e^{-\frac{m\mu^2}{6}}) = x_{n+m} - x_n e^{-\frac{m\mu^2}{6}},$$

putting $m = 2$, 3, 5, 8, and 9, and obtained values of s steadily increasing from 7·82 to 8·36. A study of Weber's table also shows that in several cases, allowing Lorberg's full error, we should get

$$x_n - x_{n+1} < x_{n+1} - x_{n+2},$$

which would absolutely make μ^2 a negative quantity. Weber himself says he had observed the galvanometer-zero to alter as much as 1·2 scale-divisions per minute, and complains of fluctuations occurring, especially in his experiments on water. There is finally somewhat of a dilemma. Lorberg's method would indicate that μ^2 remained constant during the whole range of each experiment, and so was independent of the temperature; but the values he obtains from the two experiments at different temperatures are different, and so would require μ^2 to increase with the temperature.

The value Lorberg finds for s would give τ about $\frac{1}{2}°$, and this, he thinks, might escape detection. I agree with him in thinking it unlikely that the cover and its enclosed atmosphere remain at zero temperature throughout, but think it equally unlikely that τ should be a constant. Weber gives only one specimen table at each temperature for his experiments on water; thus Lorberg availed himself of all the data at his disposal, as he limited his criticism to water. This seems, however, rather a slender foundation to build an elaborate theory on, especially as the value for k deduced after Weber's method from the typical experiment at the lower temperature exceeded by fully 3 per cent. the mean deduced from all the experiments of that series.

I have applied Lorberg's method to the other specimen tables given by Weber, which supply the observations taken in a single experiment on each of the three fluids glycerine, benzine, and mercury. Employing s and τ as in Lorberg's tables on water, my results are as follows:—

	k. After Weber.	k. After Lorberg.	s.	τ.
Glycerine	·0402	·043	14·0	$\overset{c}{0}$·8
Benzine	·0200	·0235	23·6	1·4
Mercury	·9094	·925	9·5	0·5

I have also tried by an approximate method what variation of the conductivity with the temperature would account for the variations in these tables, and got $a=$·0045 for glycerine and =·038 for benzine. By actual experiment at a higher temperature, Weber got for glycerine $a=$·00423; he does not give its value for benzine or mercury. Thus, in the case of glycerine, Weber's results seem at least consistent; yet Lorberg's values are quite distinct, and agree with the data quite as well as in the case of water, though they require τ to have a value which could hardly escape detection. As to benzine, the change in the conductivity could hardly be big enough to explain the phenomena, though the decrease of $M\mu^2$ in the table seems very regular. As Lorberg's theory would require $\tau=1^{\circ}{\cdot}4$, it can scarcely be considered more satisfactory.

There is still one point in Weber's experiments that calls for remark. He observed that even when the liquid originally occupied the space between the copper plates so fully as to have its surface very decidedly convex outwards, before the experiment ended the surface would become concave. This he ascribed to the diminution in the volume of the liquid consequent on the fall of temperature. It is to be noticed that the area of the plates and the distance between them must, for the same reason, be diminished; so that what is observed is a difference-effect. The rate of expansion of most liquids is, however, much greater than that of copper or of the material separating the plates; thus the contraction of the liquid would produce nearly its full effect. A consideration of the actual rate of expansion of the liquids examined would scarcely lead one to expect an effect so marked as seems actually to have occurred, and rather suggests that a good deal of evaporation may have taken place. In the more volatile liquids this would naturally occur, and as modifying not only the quantity of fluid but also the surface conditions, would affect the accuracy of the theory. The diminution of the area of the plates and of the distance between them would be too small to introduce any sensible error.

The following Table gives Weber's results, along with those given by Lorberg for water, and the values I have obtained by Lorberg's method for glycerine and benzine. In each case Weber's value is that placed first. The temperature is about 4° C. when not otherwise stated. The units are centimetre, gramme, and minute.

	Density.	*k.*	Conductivity, from Weber's own figures, taking water as 100.
Water at 4°·1	......	·0745	......
Corrected by Lorberg	......	·0831	......
Water at 23°·6	......	·0857	......
Corrected by Lorberg	......	·0911	......
Sulphate-of-copper solution	1·160	·0710	95·3
Sulphate-of-zinc solution	1·134	·0711	95·4
,, ,, ,,	1·272	·0698	93·7
,, ,, ,, at 4°·5	1·362	·0691	92·7
Last solution at 23°·44	......	·0776	
Salt in water at 4°·4	1·178	·0692	92·9
,, ,, at 26°·28	......	·0809	
Glycerine at 6°·25	1·220	·0402	53·9
Corrected by Lorberg's method	......	·0430	
Glycerine at 25°·2	......	·0433	
Alcohol	0·795	·0292	39·2
Bisulphide of carbon	1·271	·0250	33·6
Ether	0·728	·0243	32·6
Olive-oil	0·911	·0235	31·6
Chloroform	1·485	·0220	29·5
Citron-oil	0·818	·0210	28·2
Benzine	0·701	·0200	26·8
Corrected by Lorberg's method	......	·0235	
Mercury at 4°·5	......	·9094	1220·7
Corrected by Lorberg's method	......	·9250	
Mercury at 17°	......	·9720	

Herr Christiansen[*] has compared the conductivities of several liquids by means of a comparatively simple piece of apparatus. It consists of three horizontal copper plates separated by glass props. The plates are each 13·13 centim.

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[*] Wied *Ann.* xiv. p. 23.

in diameter and ·9 centim. in thickness. In the cylindrical surface of each is a hole, which contains the bulb of a very flat thermometer graduated to fifths of a degree. The thermometer-stems project horizontally from the plates, which we shall denote by I., II., and III. in descending order. By means of small holes in I. and II., which can be closed by copper plugs, liquids may be introduced between I. and II. and also between II. and III. The lower plate (III.) is supported on a metal vessel kept at a constant low temperature; while on I. is placed a second metal vessel, through which a stream of hot water is maintained. The experiment consists in observing the readings T_1, T_2, T_3 of the thermometers in I., II., and III., when all have become stationary.

Let e_1, e_2 denote the thickness of the upper and lower layers of liquid, and K_1, K_2 the conductivities of these liquids. Also let e_0 and K_0 denote similar quantities for the middle plate II.; and suppose S the cross section, A the cylindrical surface, and h the external conductivity of this same plate. Then Christiansen gives two approximate formulæ. The first is

$$\frac{K_2}{K_1} = \frac{e_2}{e_1}\,\frac{T_1-T_2-\delta}{T_2-T_3-\delta},$$

where

$$\delta \equiv \frac{K_1 e_0}{K_0 e_1}(T_1-T_2)$$

is a small quantity. The second is

$$\frac{K_1}{K_2} = \frac{e_1}{e_2}\,\frac{T_2-T_3}{T_1-T_2}\left\{1+\frac{Ahe_2}{SK_2}\,\frac{T_2-T_0}{T_2-T_3}\right\}.$$

In the first the thermometers are supposed to indicate the arithmetical mean of the temperatures of the upper and lower surfaces of their respective plates, which are not assumed equal. Any variation with the temperature in the conductivity of either liquid is neglected. The second formula supposes the temperatures of plates I. and III. to be the same throughout. It, however, allows for a variation in the conductivity of either liquid, after the law

$$K = K_0(1+\alpha u),$$

where u is the temperature, and makes no assumption as to the value of α. In it K_1 refers to a temperature $\frac{1}{2}(T_1+T_2)$, and K_2 to $\frac{1}{2}(T_2+T_3)$. In both formulæ it is assumed that contiguous layers of any two materials have always the same temperature, and the external conductivity of the liquids is neglected. Christiansen also obtains a formula depending on the cooling of the apparatus from which to obtain absolute measures of the conductivity. It is, however, based on a rather imperfect mathematical treatment; and no clear idea is given of how the experimental work may be carried out. Some more delicate method of determining the temperatures would be desirable. Since no results seem to have been actually obtained by this formula, further explanation is unnecessary.

The formula actually employed is the second one of the two given above. Thus Christiansen's results theoretically apply to a perfectly determinate temperature; and if the relation between the conductivity and the temperature be linear, they are not open to the charge of implicitly neglecting the variation of the conductivity.

The method has some obvious advantages. It seems very simple in execution, and allows the conductivities of any two liquids to be compared directly through a single experiment. Further, as the temperature is kept stationary, the readings can be taken with great accuracy, and no errors consequent on slight mistakes in measuring time-intervals can come in. For the same reason, much less objection could be taken to the assumption that no discontinuity of temperature exists in passing across the common surface of a liquid and metal. There is at least one rather serious objection, however. In the actual experiments there existed a difference of from 11° to 19° between the temperatures of the two fluid layers—a difference varying with the nature of the fluid. Thus the conductivities do not refer to any standard temperature; and if the variation of the conductivity with the temperature be as large as found by Weber, the comparison instituted is somewhat misleading.

With one exception, the upper fluid was air; while different liquids were successively introduced between the two lower plates, and in neither layer was the mean temperature the same in any two experiments. In one experiment only was the air replaced by a liquid, leading to a direct comparison between the conductivities in glycerine and olive-oil. The value obtained for the ratio was 1·845 : 1; while that deduced from the separate experiments with air was 1·87 : 1. Considering the complete difference in the temperatures to which the conductivities refer, this coincidence is not so convincing as Christiansen seems to think. No doubt the difficulty as to the variation in temperature could be surmounted by experiments in which both layers are filled with the same fluid. This would enable the dependence of the conductivity on the temperature to be directly determined for each fluid separately. This being found, a single comparison between any two fluids would give the ratio of their conductivities at any required temperature. Christiansen seems to have seen the necessity for determining the variation of conductivity in air, but made no attempt to investigate it for any of the liquids. From several experiments, in which both intervals were occupied by air, he found, assuming

$$k = k_0\{1 + \gamma t\},$$

where k_0 refers to temperature 0° and k to temperature t, the value of γ to be ·001504. He does not, however, apparently consider the result sufficiently trustworthy to be applied to the actual corrections.

Another point requiring criticism is the extreme thinness of the fluid layers. Thus, the lower was usually ·1909 centim. and the upper only ·0214. In the experiments with air in both intervals these were equal, being ·0214 centim. in one set and ·0754 in another. The mere measurement to so great a degree of accuracy would be by no means easy, and the plates would require to be extremely smooth and accurately plane. An error in the thickness would produce an exactly proportionate error in the conductivity. Then the temperatures of the plates separated by the thin (or air) layer differed by from 10° to nearly 25°. Thus the air would be very far removed from a uniform medium; and unless the law connecting the conductivity and the temperature be strictly linear, a very serious error might be introduced. There would also be a great deal of radiation across the fluid layers, and the effects of diffusion even would require to be examined. There is also the question whether there is not a species of internal radiation, distinct from ordinary conduction, in the case of a gas or liquid, at least when the variation in temperature is so extremely rapid as occurs here.

To obtain absolute values of the conductivities from Christiansen's results, the value of the conductivity in air is required. The values obtained from the Kinetic Theory of Gases by the formulæ of Maxwell and Clausius almost agree, and their mean is in C.G.S. units ·0000490. The rate of increase with the temperature is $\gamma = \cdot 00183$ according to Clausius, and twice as much according to Maxwell. Almost identical values for the conductivity have been obtained experimentally by Graetz and also by Kundt and Warburg, and the former found $\gamma = \cdot 0018$. Somewhat different values have, however, been found by other observers, *e. g.* Winkelmann, who got a decidedly different value for γ. He and Graetz indulge in mutual criticisms, and neither seems to put the accuracy of his results above suspicion.

The following table, mainly taken from Christiansen's paper, gives a summary of his results. K denotes the ratio of the conductivity of the liquid mentioned, at the temperature $\frac{1}{2}(T_2+T_3)$, to that of air at $\frac{1}{2}(T_1+T_2)$; while K′ is the value found for the liquid by Weber in his experiments at low temperatures, *i. e.* about 4°. I have calculated the last column, assuming for the conductivity of air at 0° the value ·000049* in C.G.S. units, and taking $\gamma=\cdot 0018$; k refers to the temperature $\frac{1}{2}(T_2+T_3)$, and belongs to the liquid mentioned opposite, the unit of time being one minute.

	$\frac{1}{2}(T_1+T_2)$.	$\frac{1}{2}(T_2+T_3)$.	K.	K/K′.	k.
Water	22·32	11·67	21·09	283	·0645
„	33·51	15·78	20·87	......	·0650
Alcohol	24·88	13·65	7·82	268	·0240
Glycerine	23·94	11·57	12·64	314	·0387
„	34·85	15·46	12·49	......	·0390
Olive-oil	26·90	13·71	6·73	288	·0207
„	39·55	18·69	6·66	......	·0210
Citron-oil	26·17	13·14	6·52	310	·0201

It seems worth noticing that in each case the results are consistent with an increase in the conductivity as the temperature rises.

Herr Graetz† has carried out a considerable number of experiments by a novel method. By means of a simple arrangement the liquid is forced under a determinate constant pressure through a straight horizontal pipe of very small radius. The pipe is immersed in a bath, through which flows water of a constant low temperature T_0. The ends of the pipe are let into corks fitting in apertures in the sides of the bath. The corks also fit into the mouths of two larger tubes, one of which brings the liquid to the pipe, while the other carries it away. The entering liquid is warmed in a bath, and has its temperature T_1 determined by means of a fixed thermometer just before it reaches the mouth of the pipe. A similar thermometer gives the temperature U of the liquid just after leaving the pipe. The quantity of liquid traversing the pipe per minute is also observed.

Graetz's two papers treat of two different series of experiments, which differed, however, only in the pipes employed and the velocity with which the liquids traversed them. In the earlier series the pipe was of copper, whose dimensions are

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* Wullner, Wied. *Ann.* iv. p. 321, gives ·000045 as the correct theoretical value; values obtained by experiment seem mostly to exceed that employed above.

† Wied. *Ann.* xviii. p. 79, and xxv. p. 337.

variously stated. In one place its diameter is said to be ·06 centim., while in another place the radius of the outer surface is said to be ·06 centim. and to bear to the radius of the inner surface the ratio 3 : 2. The length is variously stated as 9·8 or 8·9 centim. A layer of warmer water tended to gather round the pipe, but was dispersed by means of a brush driven by electricity. Graetz assumed that, through the action of the brush, the outer surface of the pipe was always kept at the same temperature T_0 as the surrounding fluid.

After some preliminary calculations of a rough nature, Graetz proceeds to a complete mathematical treatment. He shows that no sensible error is introduced by supposing the pipe of one temperature throughout. He then assumes Poiseuille's law, that in a pipe of radius R, from which $\pi R^2 \alpha$ is the quantity of liquid issuing in unit time, the velocity at a distance r from the axis is $2\alpha\left(1-\frac{r^2}{R^2}\right)$.

Employing this expression in his differential equation, he found, by a satisfactory process, that the mean temperature of the issuing liquid is given by the equation

$$U = T_1 \Sigma p_i e^{-\frac{k}{\varrho c}\frac{\pi l \mu_i{}^2}{2V}},$$

where k, ρ, c have their usual meaning, l is the length of the pipe, V the volume of liquid issuing per minute, and μ_i the root of a certain equation. The two first roots are $\mu_1 = 2{\cdot}7043$ and $\mu_2 = 6{\cdot}50$, and the terms answering to the other roots are negligible. The coefficients p_1 and p_2 are also numerical quantities, which can be calculated from the mathematical theory. In his first paper, however, Graetz thought it easier to deduce them from three observations on copper-sulphate solution, in which the liquid traversed the pipe under three different pressures. The values so deduced were $p_1 = {\cdot}91264$ and $p_2 = {\cdot}01249$; and these were employed in calculating the conductivities of all the other liquids. In most cases the liquid traversed the pipe under at least two different pressures; and the results obtained appear to agree remarkably well. These results were uniformly considerably larger than Weber's; and believing them to be true, Graetz indulges in a good deal of criticism, and accepts Lorberg's corrections as tending in the right direction. In his second paper, however, he is able to determine p_1 and p_2 directly from his mathematical theory; and it turns out that $p_1 = {\cdot}81747$ and $p_2 = {\cdot}0325$.

This great divergence from the values employed of course completely invalidates the conclusions drawn from the first paper. After this it might seem unnecessary further to consider that paper, but for Graetz's statement that it will still be useful for giving comparative values of the conductivity, and that it may throw some light on the trustworthiness of the method. On page 90 is given the formula from which the calculations are said to be derived. In it μ_1 is incorrectly given as 2·4043. In five out of six of the following tables of data l is given as 9·8 centim. These tables give a summary of the data obtained in the experiments on six different liquids, the number of experiments on each varying from five to thirteen. I may safely assert that in most, if not all, of these experiments it will be found that the values found by Graetz for the conductivity are very far from satisfying his equation, whether l be 9·8 or 8·9. Singularly enough, however, taking $l=8·9$, if the index of e be multiplied by ·7 the equation will in most cases be very nearly satisfied; and in some cases a still better result is obtained by multiplying the second term, which is comparatively very small, by 10. I have calculated out seventeen of the experiments, including the three from which p_1 and p_2 were calculated, and in only one did this emendation fail nearly to satisfy the equation. Putting $l=9·8$ centim. will be found to place Graetz's results in a worse light even than 8·9. As the index of e consists of a series of factors, three or four of which are constants, it would seem probable that Graetz calculated the value of the constant part once for all. One of these factors is π, and it seems quite possible that this may have been put equal $\frac{22}{7}$, and the 7 may have been overlooked. If this explanation be correct the decimal place also must have been misplaced, which might easily occur.

The product kl occurs in the index of e; thus, as the second term is very small, an error of the above nature, or in the value of l, would not seriously have affected the relative values of the conductivity provided the coefficient p_1 had been correct. A glance at the formula, however, will show that since p_1 was incorrectly taken, no trustworthy results can be obtained without repeating the entire calculation. The results deduced when the correct values of p_1 and p_2 are inserted do not seem very consistent, and it is scarcely worth while considering them here.

In his second paper Graetz employs the amended coefficients, and attributes the errors in the values obtained experimentally to his having made the liquids traverse the pipe at velocities exceeding the limits to which Poiseuille's law could be applied. This explanation seems utterly inconsistent with observations he makes on the results obtained by varying the velocity. Commencing with a very rapid current, the value given for k by his theory is distinctly said to increase as the velocity is diminished, and this to a very decided amount*. For a considerable diminution in the velocity the value found for k is then almost constant, and this value is assumed by Graetz to be the correct one. For very slow currents, again, the value appears to diminish. There were also certain practical difficulties in employing very slow currents. In these the difference between U and T_0 is so small that any slight fluctuation in the former has a serious effect on the results; further, the difference between U and T_1 is so great that it is hard to say to what temperature the value found for k should be assigned. But the main difficulty consists in determining U correctly. With moderately fast currents the liquid threads of different temperature in the pipe mix on issuing into the much wider tube, and the reading of a thermometer in this tube is nearly independent of its position. With slow currents, however, the threads do not mix properly; and even the introduction of a Z-shaped tube at the end of the pipe failed to effect a thorough mixture.

Graetz points out that, in his first paper in the differential equation employed, it is tacitly assumed that k is independent of the temperature. He does not, however, succeed in removing this objection unless two very doubtful points be conceded. The first is that the temperature of the liquid in the pipe depends only on the distance from the axis; and the second that the higher value of the conductivity in the warmer layers is exactly counteracted by the increase in the velocity in capillary tubes with the temperature. The effect of gravity would render doubtful the first assumption, and the second seems decidedly improbable. The point is of considerable importance; for, according to the theory, there must have been in some of the experiments a difference of 20° C. between the temperature of the entering liquid in the axis and at the surface of the pipe.

In the second series of experiments the material of the pipe was platinum, which was selected as being less exposed to chemical action. Graetz also takes l as the length of the pipe exposed to the water in the bath; the ends, which were fixed into the corks, being excluded. Thus, in the various experiments l is stated variously as from 6·145 to 7·71 centim. In the first series of experiments one is left to conclude that l was the entire length of the pipe; and it seems highly questionable what a correct use of the theory requires. In fact the formula employed assumes l to be the length of the column of

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* See pages 346 & 347 of Graetz's second paper.

the liquid, which is surrounded by a metal cylinder at T_0°; the liquid before entering the cylinder having a uniform temperature T_1, and leaving with a mean temperature U. Now if Graetz be correct in supposing that the metal cylinder is so good a conductor as to be of the temperature T_0 throughout its entire thickness when surrounded by water of temperature T_0, it seems obvious that the ends of the pipe cannot preserve a totally different temperature. For instance, at the end where the liquid enters, the part of the pipe imbedded in the cork seems at least as likely to have the temperature T_0 as the temperature T_1; and there can be little doubt that in reality the temperature is intermediate between the two, and varies from point to point of the pipe. Since l occurs as a factor of k in the formula, any error in l produces an exactly proportional error in k in the opposite direction. If the above view be correct, l is uniformly given too small, and so the value found for k is always too large; but the error in l would differ from one experiment to the next. Thus the values obtained for k would not lead to correct measures of relative conductivities even.

In the tables of results Graetz does not directly give the volume issuing from the pipe per minute; but he gives the value of a quantity t, which is defined some pages previously as the time in seconds required to pass 100 cubic centim. of the fluid through the pipe. Thus we should have V, given by the equation $V=\frac{100\times 60}{t}$. The substitution, however, of this result and the other data in the equation does not seem to satisfy it even approximately; so probably t has some meaning to which the paper affords no obvious clue.

The absolute values given in the second paper are much less than in the first, and, as a rule, slightly smaller even than Weber's, though referring to a higher temperature. Graetz seemed disposed to assign this difference to the radiation between Weber's plates; but numerical results deduced from an application of Stefan's law of radiation to the data given by Weber indicated that not more than 2 per cent. of the effect could have been due to this cause. He thus attributes the difference to Weber's neglecting the loss of heat by external conduction from the liquid surface.

On the whole I do not think Graetz's method very satisfactory, and am doubtful if much weight should be attached to his results. There are a good many points in the theory open to criticism; and the formula obtained is of such a nature that a small error in the value given for the constant p_1 by the theory would produce a large error in the value deduced for the conductivity.

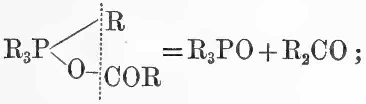
In the following Table, which embodies the results of Graetz's second paper, k is the conductivity at the temperature T, and α denotes the rate of increase of k with the temperature. Graetz, however, does not claim great accuracy in the values of α.

	Liquid density.	T.	k.	α.
Alcohol	0·8204	13·40	·0327	
Glycerine	1·210	13·35	·0382	·012
,,	1·20	27·14	·0444	
Turpentine-oil	0·8692	12·63	·0195	·0067
,,	0·864	22·73	·0208	
Ether	0·729	13·63	·0227	
NaCl solution	1·153	13·87	·0674	·0057
,,	1·1506	25·41	·0717	
$KClO_3$ solution	1·026	14·14	·0698	·0078
,,	1·024	27·10	·0769	
Carbon disulphide	1·2728	13·31	·0160	
Petroleum	0·7899	12·66	·0213	·011
,,	0·784	22·73	·0237	

II. *On the Action of Heat on the Salts of Triethylbenzylphosphonium.* *By* N. Collie, *Ph.D., Lecturer on Chemistry at the Ladies College, Cheltenham**.

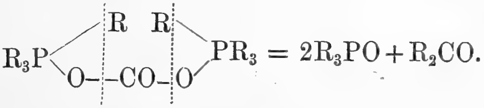
In a former paper with another chemist† I drew attention to the manner in which phosphonium salts decompose when subjected to the action of heat; a decomposition which differed from that which takes place when the corresponding compound ammonium and sulphine salts are heated.

It was found that all phosphonium oxysalts yield as the chiet product of decomposition, when subjected to the action of heat, the oxide of the tertiary phosphine, and usually a ketone at the same time is formed; in the case of a monobasic acid,



$$R_3P\begin{matrix}\diagup R\\ \diagdown O-COR\end{matrix} = R_3PO + R_2CO\,;$$

or in the case of a dibasic acid,



$$R_3P\begin{matrix}\diagup R & R\diagdown\\ \diagdown O- & -CO- & O\diagup\end{matrix}PR_3 = 2R_3PO + R_2CO.$$

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* Communicated by the Author.

† Letts and Collie, Phil. Mag. Aug. 1886, p. 183.

This difference from the corresponding nitrogen and sulphur compounds was explained by the reducing action of the tertiary phosphine.

In the former paper, already alluded to, the action of heat only on the Tetrethylphosphonium salts was investigated; and as in some cases the results were of a somewhat complicated nature and not perfectly simple of explanation, mixed phosphonium salts, containing both aromatic and fatty radicals, were chosen for examination, as it was thought that they might throw some additional light on the general way in which these phosphorus compounds decompose.

Chloride of Triethylbenzylphosphonium.

This salt has been previously prepared by Hofmann[*] by heating a mixture of benzylidene chloride with triethylphosphine and alcohol to 120°–130° C. A simpler method was, however, employed, viz. treating triethylphosphine with excess of chloride of benzyl, when complete combination occurs, and the chloride of triethylbenzylphosphonium is produced,

$$P(C_2H_5)_3+C_7H_7Cl=P(C_2H_5)_3(C_7H_7)Cl.$$

It was found necessary, however, to use an excess of the chloride of benzyl in the reaction, on account of the heat which is evolved when the combination occurs, which causes the volatilization of some of the triethylphosphine. After all reaction ceases the mixture solidifies, on cooling, to a mass of crystals. On treatment with water the excess of benzyl chloride easily separates from the aqueous solution of the triethylbenzylphosphonium chloride, which can then be evaporated to a small bulk and allowed to crystallize. As Hofmann gives no description of the salt, and only prepared the platinochloride in a state fit for analysis, some of the salt was carefully purified and its properties determined.

(*a*) 2·041 grms. of salt, dried *in vacuo* over sulphuric acid, lost 0·137 grm. = 6·71 per cent.

(*b*) 0·446 grm. dry salt took 19·0 cubic centim. decinormal nitrate-of-silver solution = 15·12 per cent. Cl.

(*c*) 0·283 grm. dry salt took 11·8 cubic centim. decinormal nitrate-of-silver solution = 14·80 per cent Cl.

(*d*) 0·363 grm. dry salt gave 0·842 grm. CO_2 and 0·2915 grm. H_2O,

$$C=63·26, \quad H=8·92.$$

	Calculated for $(C_7H_7)(C_2H_5)_3PCl, H_2O$.	Found. (*a*)
H_2O	6·85	6·71

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[*] *Ann.* Spl. I. p. 323.

	Calculated for $(C_7H_7)(C_2H_5)_3PCl$.	Found.		
		(*b*)	(*c*)	(*d*)
C …………	63·80	…	…	63·26
H …………	9·00	…	…	8·92
Cl …………	14·52	15·12	14·80	…

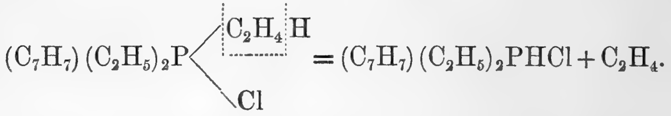
The salt itself is a white crystalline substance scarcely deliquescent, and dissolves in a very small quantity of water; it is soluble in alcohol, but is precipitated in the crystalline form on addition of dry ether. The dry salt melts at 178°–180° C. without decomposition. The watery solution of the salt gives with chloride of platinum a yellow voluminous precipitate, already described by Hofmann.

When the chloride of triethylbenzylphosphonium is subjected to the action of heat, it first fuses, and at a much higher temperature decomposes, yielding gaseous and solid products; and although the decomposition takes place at a very high temperature it is unattended by any charring. When fourteen grams of pure salt were heated in a distilling-flask (with an especially wide delivery-tube), the decomposition did not take place until the temperature had risen above 360° C.; during the experiment 1200 cubic centim. of gas were evolved, and the distillate was a solid mass of crystals. Some of the gas was subjected to analysis. (7·0 cubic centim. gas needed 21·5 cubic centim. oxygen for combustion, yielding 14·0 cubic centim. of CO_2, the ratio being approximately 1 : 3 : 2 proved it to be ethylene.) The remainder of the ethylene was absorbed by bromine, giving a liquid b.p. 130°–134° C., which gave on analysis numbers agreeing with those needed by dibromide of ethylene, proving beyond doubt the composition of the gas. The solid distillate was redistilled; it boiled without decomposition between 225°–230° C. A chlorine determination gave the following numbers:—0·622 grm. salt took 29·9 cubic centim. decinormal nitrate-of-silver solution = 17·06 per cent. Cl. Theory for $(C_7H_7)(C_2H_5)_2PHCl$, C=16·39 per cent. The salt is soluble in water, giving an acid solution; mixed with caustic soda an oil separates, which possesses all the properties of a tertiary phosphine; it fumes strongly in the air; possesses a most penetrating smell, which differs, however, from that of triethylphosphine; it also gives a red crystalline compound with bisulphide of carbon. The phosphine can be separated from the solution of caustic soda by distillation in an atmosphere of hydrogen by means of a current of steam. Several grams of the phosphine were thus obtained. When dried over caustic soda and fractionally distilled, the whole passed over between 250°–255° C., and an analysis proved it to consist of diethylbenzylphosphine.

0·232 grms. gave 0·627 CO_2 and 0·192 H_2O.

	Calculated for $(C_7H_7)(C_2H_5)_2P$.	Found.
C	73·33	73·70
H	9·44	9·19

These results show that, when the chloride of triethylbenzylphosphonium is heated, it decomposes in the following way :—



$$(C_7H_7)(C_2H_5)_2P\begin{cases} C_2H_4\,H \\ Cl \end{cases} = (C_7H_7)(C_2H_5)_2PHCl + C_2H_4.$$

And the decomposition is nearly quantitative ; for fourteen grams of salt should give, according to the above equation, about 1300 cubic centim. of ethylene gas, and the amount found was 1200 cubic centim.

Bromide of Triethylbenzylphosphonium.

By saturating a solution of the hydrate with hydrobromic acid a neutral solution was obtained. This was evaporated to a small bulk and allowed to stand over sulphuric acid. Long needle-shaped crystals were deposited, which were not perceptibly deliquescent.

0·400 grm. dry salt took 13·75 cubic centim. decinormal nitrate-of-silver solution =27·50 per cent. Br.

	Calculated for $(C_7H_7)(C_2H_5)_3PBr$.	Found.
Br	27·68	27·50

Eight grams of the salt were heated ; no decomposition began till the temperature had risen to above 360° C., when gas was evolved, and the salt rapidly turned brown ; so much charring eventually took place that the flask containing the salt became almost full of charcoal. The small quantity of distillate was strongly acid from free hydrobromic acid, and contained toluene, and the hydrobromates of triethylphosphine and diethylbenzylphosphine. The gas produced consisted chiefly of ethylene ; but there was present also some inflammable hydrocarbon not absorbable by bromine. Evidently only a small amount of the salt decomposes in a manner similar to the chloride, the larger quantity being totally decomposed.

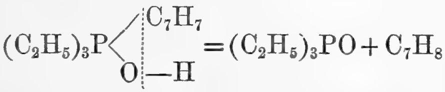
Hydrate of Triethylbenzylphosphonium.

The hydrate was prepared by two methods :—(1) by the action of silver oxide on the chloride in aqueous solution ; (2) by the action of barium hydrate on the sulphate.

When freshly precipitated oxide of silver is added to a solution of the chloride in water a strongly alkaline solution is obtained, but at the same time there is slight reduction of the silver oxide. The second method for the preparation of the hydrate is free from this objection ; and it was also tried in order to see whether a decomposition occurred similar to that noticed during the preparation of the hydrate of tetrabenzylphosphonium, viz. decomposition of the hydrate at the moment of production (if *hot* and *concentrated* solutions be used) into phosphine oxide and a hydrocarbon,

$$(C_7H_7)_4PHSO_4 + Ba(OH)_2 = (C_7H_7)_3PO + C_7H_8 + BaSO_4 \, ;$$

while with *cold dilute* solutions the tetrabenzylphosphonium hydrate and barium sulphate are formed. This does not seem to be the case with the hydrate of triethylbenzylphosphonium; both boiling and concentrated solutions were used, but not a trace of toluene or of an oxide of a tertiary phosphine could be detected. The hydrate prepared by either of the above methods forms a strongly alkaline and caustic liquid, which will absorb carbon dioxide readily, and precipitate metals from their solutions in the same way as other phosphonium hydrates do. When perfectly dry by long standing over phosphoric anhydride *in vacuo*, it crystallizes in long and very deliquescent needles. When it was heated above 100° C. it melted, and was almost at once completely decomposed. No gas was evolved, and the distillate consisted of two layers ; the upper one insoluble in water, and, from its general properties and an analysis made, proved to be toluene ; the lower was nearly pure oxide of triethylphosphine. The decomposition which occurs, therefore, when the hydrate of triethylbenzylphosphonium is heated is as follows :—



$$(C_2H_5)_3P\!\left\langle\begin{array}{l}C_7H_7\\ O\,—H\end{array}\right. = (C_2H_5)_3PO + C_7H_8$$

and, as there was no gas produced, evidently no oxide of a mixed phosphine could have been formed.

Carbonate of Triethylbenzylphosphonium.

Several attempts were made to prepare the normal carbonate, but without success, and this salt does not seem capable of existence: at all events it cannot be obtained by any of the following processes:—

(1) By the action of barium carbonate on the sulphate.

(2) By the action of silver carbonate on the chloride.

(3) By dividing a solution of the hydrate into two portions, and saturating one half with carbon dioxide and then adding the other half to it.

In each of these cases a strongly alkaline solution was obtained, which, on evaporation to dryness and subsequent heating, invariably decomposed with the greatest ease into the same substances; viz. toluene, oxide of triethylphosphine, and carbon dioxide. As these substances could not easily be produced from the normal carbonate unless water had acted on the salt,

$$\big((C_2H_5)_3(C_7H_7)P\big)_2CO_3 + H_2O = 2C_7H_8 + 2P(C_2H_5)_3O + CO_2,$$

several careful experiments were performed in order to ascertain whether this was the case or not. Some more carbonate was prepared by the last-named method, and the solution was concentrated *in vacuo* over sulphuric acid, and finally allowed to remain with phosphoric anhydride in a vacuum desiccator for two months. It had then dried up to a mass of brittle white crystals, which remained constant in weight and on analysis gave the following numbers:—

0·223 grm. salt gave 0·532 grm. CO_2 and 0·190 H_2O.

	Calculated for		
	$\big((C_2H_5)_3(C_7H_7)P\big)_2CO_3$.	$(C_2H_5)_3(C_7H_7)PHCO_3$ $(C_2H_5)_3(C_7H_7)POH$.	Found.
C	67·78	65·32	65·07
H	9·20	9·27	9·46

These numbers show that the normal carbonate is not formed by the foregoing process, and that the salt is probably a mixture of the acid carbonate with the hydrate; and this is conceivable, as it has been already shown that no carbonate of the tetrabenzylphosphonium exists*.

The action of heat on the dry salt also points to the same conclusion; for complete decomposition occurred at a temperature of 130° C., and toluene, oxide of triethylphosphine, and carbon dioxide were the only substances produced.

$$(C_2H_5)_3(C_7H_7)PHCO_3 = (C_2H_5)_3PO + C_7H_8 + CO_2,$$

$$(C_2H_5)_3(C_7H_7)POH \quad = (C_2H_5)_3PO + C_7H_8.$$

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* Letts and Collie, Trans. Roy. Soc. Edin. vol. xxx. part 1, p. 199.

No ketone could be detected, and the gas evolved was completely absorbed by caustic soda.

Many other experiments were made with carbonate prepared by the other methods mentioned, but always with the same results.

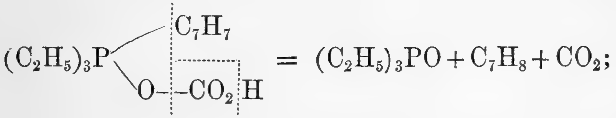
Acid Carbonate of Triethylbenzylphosphonium.

This salt can be easily prepared by saturating a solution of the hydrate with carbon-dioxide gas; the solution is faintly alkaline to litmus-paper, and can be evaporated over the water-bath without decomposition; but to be obtained in the crystalline state it must be allowed to remain *in vacuo* with sulphuric acid for a considerable time.

0·312 grm. of salt gave 0·705 CO_2 and 0·249 H_2O.

	Calculated for $(C_2H_5)_3(C_7H_7)PHCO_3$.	Found.
C	62·22	61·62
H	8·51	8·86

This salt decomposes completely, when heated to 130° C., into toluene, carbon dioxide, and oxide of triethylphosphine. 10 grams of the dry salt gave 800 cubic centim. of carbon-dioxide gas, which was completely absorbed by caustic soda. The theoretical amount required by the following equation is 827 cubic centim.



$$(C_2H_5)_3P\begin{cases} C_7H_7 \\ O\!-\!CO_2H \end{cases} = (C_2H_5)_3PO + C_7H_8 + CO_2;$$

thus showing that the decomposition was complete.

The extreme ease with which this acid carbonate decomposes, and the apparent non-existence of the normal salt, show a very marked diminution in the alkalinity of the hydrate of triethylbenzylphosphonium as compared with the hydrate of tetrethylphosphonium. The normal carbonate of the latter is a stable substance, but the introduction of the benzyl group into the molecule in the place of an ethyl group seems sufficient to make a considerable difference; while, if all the ethyl groups be replaced by benzyl groups, as in the hydrate of tetrabenzylphosphonium, the alkalinity of the hydrate becomes so feeble that it is no longer capable of fixing carbon dioxide at all.

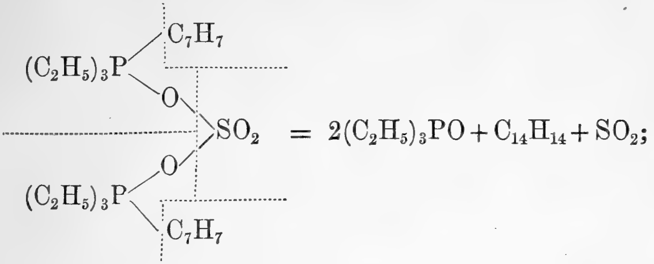
Sulphate of Triethylbenzylphosphonium.

This salt was prepared by the action of sulphate of silver on the chloride; the small quantity of dissolved sulphate of silver was removed by sulphuretted hydrogen, and the acid solution was then carefully neutralized with a small quantity of the hydrate of triethylbenzylphosphonium. On concentrating this solution by evaporation on a water-bath, a small amount of decomposition was noticed, a trace of a free tertiary phosphine being produced, and, what seemed remarkable, the phosphine was not the triethylphosphine but the diethylbenzylphosphine, which possesses a very different smell from the triethylphosphine, and can therefore by this property be easily distinguished from it. The solution, as it became concentrated, was distinctly acid to litmus-paper, and was therefore further concentrated by allowing it to stand over sulphuric acid *in vacuo*. The syrup eventually solidified to a mass of deliquescent crystals.

These were transferred to a distilling-flask, and heated; they soon fused: decomposition began at about 230° C., and then proceeded rapidly. A gas was evolved which was recognized as sulphur dioxide by its smell, solubility in water, and other properties. The distillate was partly crystalline, and also smelt strongly of sulphur dioxide; it was treated with water (in which the crystals were insoluble) and warmed, in order to volatilize the sulphur dioxide and traces of toluene, which were also noticed. The crystals were eventually separated from the aqueous solution, which contained only the oxide of triethylphosphine, and, after washing with water and recrystallizing several times from hot dilute alcohol, they were obtained as white glistening leaflets: m.p. 119° C. (0·111 grm. substance gave 0·381 CO_2 and 0·0666 H_2O. $C=93{\cdot}61$ per cent., $H=6{\cdot}66$ per cent. Stilbene, $C_{14}H_{12}$, contains $C=93{\cdot}33$ per cent. and $H=6{\cdot}66$ per cent.) The melting-point of stilbene is given as 124° C., and this hydrocarbon unites directly with bromine, giving a dibromstilbene, m.p. 237° C. Some of the crystals, m.p. 119° C., were accordingly treated with bromine; a slightly yellow crystalline compound was obtained, which, when heated to 230° C., grew brown and half melted, but did not melt completely till the temperature had risen to 240° C. There is little doubt, therefore, that the hydrocarbon is stilbene.

The mother liquors, from which the stilbene was obtained, yielded on evaporation another crystalline substance, which possessed a much lower melting-point, and seemed from its appearance to be dibenzyl, as it melted at 60°–63° C., contained neither phosphorus nor sulphur, and burnt with a very smoky flame. It was unfortunately present only in very small quantities; and, although several attempts were made to isolate it in a state of purity, they were without success.

The decomposition which the sulphate undergoes when heated is therefore probably as follows :—



$$\left.\begin{array}{l}(C_2H_5)_3P\left\langle\begin{array}{l}C_7H_7\\ O\end{array}\right.\\ (C_2H_5)_3P\left\langle\begin{array}{l}O\\ C_7H_7\end{array}\right.\end{array}\right\rangle SO_2 \;=\; 2(C_2H_5)_3PO + C_{14}H_{14} + SO_2;$$

and differs from the action of heat on the sulphate of tetrethylphosphonium, as no sulphide of a tertiary phosphine is formed.

The toluene and stilbene noticed are no doubt produced by the decomposition of the dibenzyl,

$$2C_{14}H_{14} = C_{14}H_{12} + 2C_7H_8.$$

This decomposition has already been noticed, when the chloride of tetrabenzylphosphonium is decomposed by heat*.

Acetate of Triethylbenzylphosphonium.

In order to prepare this salt, barium acetate was added carefully to a solution of the sulphate in water. During the concentration of the aqueous solution over the water-bath, a smell of diethylbenzylphosphine was noticed. The acetate is extremely deliquescent and difficult to obtain in the crystalline state. When it was heated, no change occurred till the salt had reached a temperature of 200° C.; decomposition then began rapidly, and liquid distilled, but no gases were evolved, and towards the end of the experiment the contents of the flask had to be heated to a temperature above 300° C. On washing out the distillate from the condenser with a little water, it separated into two layers, the upper one being insoluble in water. The aqueous solution was acid in its reaction with litmus-paper, and gave with caustic soda a trace of free triethylphosphine; on distillation it seemed to be almost completely composed of water and oxide of triethylphosphine; there was mixed with it, however, a small quantity of some substance which possessed a very high boiling-point, and yielded some triethylphosphine when again distilled.

The liquid, insoluble in the water, contained no free phosphine, and, on being subjected to fractional distillation, yielded first a little toluene, but consisted almost entirely of

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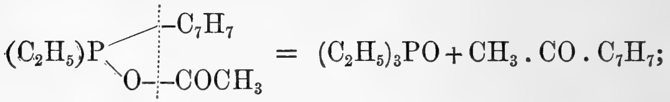
* Letts and Collie, *loc. cit.* p. 213.

some substance boiling between 200°–220° C. It did not seem to be attacked when boiled with strong caustic potash, but traces of acetate of potash were detected in the aqueous solution. It was not changed by hydrochloric acid, and had a pleasant aromatic smell. It gave a crystalline compound with bisulphite of sodium, and on oxidation with chromic acid a mixture of acetic and benzoic acids was produced. The substance is evidently therefore the methylbenzylketone, b.p. 215° C.; and probably mixed with it is a trace of the acetate of benzyl, which gives the acetate of potash when boiled with caustic potash. The chief decomposition which the acetate undergoes when heated is therefore as follows:—



$$(C_2H_5)P\begin{matrix} \vdots -C_7H_7 \\ O-\vdots-COCH_3 \end{matrix} = (C_2H_5)_3PO + CH_3 \,.\, CO \,.\, C_7H_7;$$

while a small quantity of the salt splits up, yielding triethylphosphine and the acetate of benzyl,

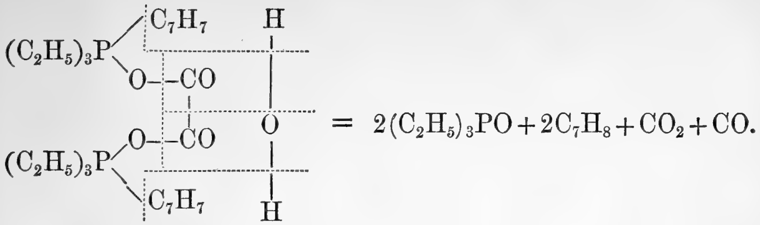
$$(C_2H_5)_3(C_7H_7)P \,.\, O \,.\, COCH_3 = (C_2H_5)_3P + C_7H_7O \,.\, COCH_3.$$

Oxalate of Triethylbenzylphosphonium.

When the chloride of triethylbenzylphosphonium is treated with oxalate of silver, this salt is produced, together with the chloride of silver. The insoluble silver salts were separated by filtration, and the solution of the oxalate concentrated to a small bulk over the water-bath. During the evaporation a slight smell of the diethylbenzylphosphine was noticed, and the solution, which had been neutral, became slightly acid in its reaction with litmus paper. The concentrated solution, when allowed to stand over sulphuric acid *in vacuo*, soon solidified to a mass of radiating crystals. Eight grams of these were subjected to the action of heat. Decomposition began at 170° C., and gas was rapidly evolved; the temperature was gradually raised to 250° C., when the whole of the salt had distilled, and no charring had occurred.

The distillate was completely composed of toluene and oxide of triethylphosphine, and not a trace of any other substance could be detected. The gases evolved were first treated with caustic potash solution, when about one half dissolved; the remainder was not attacked by bromine; it burnt with a blue flame, and gave carbon dioxide alone when exploded with oxygen gas.

It is difficult to explain the formation of only toluene and oxide of triethylphosphine, and the absence of triethylphosphine, unless water has taken part in the reaction.

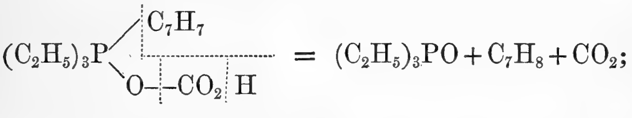


$$\begin{array}{l}(C_2H_5)_3P\begin{cases}C_7H_7 \quad H\\ O-CO\end{cases}\\ \qquad\qquad\qquad\quad O\\ (C_2H_5)_3P\begin{cases}O-CO\\ C_7H_7 \quad H\end{cases}\end{array} = 2(C_2H_5)_3PO+2C_7H_8+CO_2+CO.$$

Probably the water is present in the crystals as water of crystallization, and several attempts were made to estimate the amount, but owing to the deliquescent nature of the salt, and the difficulty of separating the mother liquor from the crystals, no constant numbers could be obtained.

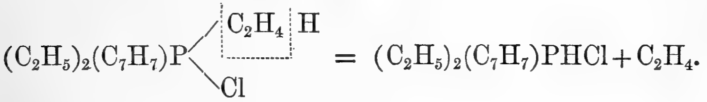
In conclusion, it is seen from the foregoing experiments, that when the salts of triethylbenzylphosphonium are subjected to the action of heat, they differ in their mode of decomposition from the corresponding tetrethylphosphonium compounds. This is due probably to several causes, the chief being the less stable nature of the salts, owing probably to the introduction of the benzyl group; they decompose therefore more easily, and in the majority of cases the hydrocarbon radical detached from the molecule is the benzyl. The non-existence of the normal carbonate is worthy of note; and the different decomposition suffered by the sulphate, when heated, as compared with the sulphate of tetrethylphosphonium (which latter salt yields almost equal amounts of the oxide and sulphide of triethylphosphine).

Perhaps, however, the real point of interest is that when the triethylbenzylphosphonium is united with oxyacids, the radical separated from the phosphorus is invariably the benzyl*



$$(C_2H_5)_3P\begin{cases}C_7H_7\\ O-CO_2 \quad H\end{cases} = (C_2H_5)_3PO+C_7H_8+CO_2;$$

while with the chloride and bromide the radical detached is the ethyl in the form of ethylene gas,



$$(C_2H_5)_2(C_7H_7)P\begin{cases}C_2H_4 \quad H\\ Cl\end{cases} = (C_2H_5)_2(C_7H_7)PHCl+C_2H_4.$$

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* It was remarkable though, that, during the evaporation of several of the oxysalts, they should have given traces of a free phosphine, which was apparently the diethylbenzylphosphine.

III. *On the Potential of the Electric Field in the neighbourhood of a Spherical Bowl, charged or under influence.* By Dr. J. Nieuwenhuyzen Kruseman*.

[Plate I.]

THE contents of this paper were suggested by Sir W. Thomson's memoir, "Determination of the Distribution of Electricity on a circular segment of plane or spherical conducting surface, under any given influence."

It has been my aim to find the potential at every point of space in those cases in which Sir W. Thomson has given the distribution of electricity. I found it possible to expand the desired function as a series of spherical harmonics, and afterwards to bring the expansion into a finite form. The result, however, may be stated and proved by a very simple method which I shall now proceed to describe.

§ 1. When a very thin circular plate is charged with electricity to a given potential V_0, the equipotential surfaces are spheroids, and the potential at every point of the surrounding space is given by the equation

$$V=\frac{2V_0}{\pi}\sin^{-1}\frac{2c}{s_1+s_2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where c represents the radius of the plate, and s_1 and s_2 the longest and shortest lines drawn from the point under consideration to its circumference. Now the angle figuring in this formula has a simple geometrical meaning. For, let in fig. 1 the plane of the plate represent a plane through the point P and the centre O of the disk normal to the latter. This plane cuts the circumference of the disk at the points A and C. Thus we have

$$AC=2c, \quad PA=s_1, \quad PC=s_2.$$

Bisecting the angle APC by AD, drawing the perpendicular DE on AC in the plane of the disk, it is easily seen that

$$\sin EPD=\frac{2c}{s_1+s_2}.$$

The double of this angle EPD is the greatest angle which any chord of the plate subtends as seen from P. Since it frequently occurs in the following considerations, I venture to

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* Abstract from a paper in *Verslagen en Mededeelingen der Koninklyke Academie van Wettenschappen.* Communicated by the Author.

designate it by the more or less appropriate term of *amplitude of the disk with regard to P.*

Accepting this term, the following verbal expression may be given to formula (1) :—

When a thin circular disk is charged to potential π, the potential at every point of space is numerically equal to the amplitude of the disk with regard to that point.

Of course it is supposed that no other electrical charges are present in the field.

Now by Thomson's theory of electrical images, from every function V, which satisfies Laplace's equation ($\Delta^2 V=0$), another function which satisfies the same equation may be derived by the following process of inversion. A point within the space where $\Delta^2 V=0$ is connected by a straight line with the centre of any spherical surface of radius a ; let the distance be ρ ; on the same radius another point is chosen at a distance r from the centre, both distances being connected by the relation $\rho r=a^2$. This point is the image of the former. A function having the value $\frac{a}{r}f$ at the image, while the particular value of the function V at the original point is f, satisfies Laplace's equation.

§ 2. We shall apply this theorem to a function analogous to the potential of a charged disk, but distinct from it in certain particulars. Instead of the amplitude of a disk we shall consider the amplitude of a segment of a spherical surface, *i. e.* the amplitude of a bowl. This amplitude may be defined thus : For nearly all points of space it is identical with that of a disk having the same boundary ; there exists only a difference for points situated in the space between the bowl and the plane of its rim. For those points the amplitude of the bowl is 2π minus the amplitude of the rim ; thus it is greater than π, much in the same way as the solid angle subtended at the same points by the bowl exceeds 2π.

In the case of a disk the potential attains a maximum value for points on the disk ; the function now under consideration has its maximum on the bowl ; the differential-quotients of both functions are identical as to their *absolute value,* but they differ in *sign* for points within the space between the bowl and the plane of its rim. Thus our new function satisfies Laplace's equation throughout space ; it is discontinuous with its differential-quotients only on the bowl, where it has two values, those for outer and inner points supplementing each other to 2π. The equipotential surfaces are still spheroids ; but to those which cut the segment belong two distinct potentials.

The thus defined function we are to invert, taking for centre and radius of the inversion the centre and radius of the sphere, of which the bowl forms a part, so that this latter becomes its own image.

The function obtained by the inversion will be discontinuous for points on the bowl, since the original function is discontinuous for the same points; the inner and outer values have only interchanged.

And now finally we may imagine a function the value of which at any point is the algebraical sum of the values of both the original and inverted functions. It is evident that this new function has the following properties:—

1st. It is continuous for points on the bowl, as well as for all other points.

2nd. On the bowl it has the constant value 2π.

3rd. Its differential-quotients of the first order are continuous for all points of space, those situated on the bowl making the only exception.

4th. It satisfies Laplace's equation.

5th. It vanishes at infinity.

Now a function having these properties fulfils all the conditions necessary and sufficient for the potential of the bowl.

Thus the first part of the problem mentioned at the head of this article is solved for the case that the bowl is charged to potential 2π. Had it been charged to any other potential V_0, we should have had of course to multiply our function by the factor $\frac{V_0}{2\pi}$.

The algebraic expression of the function is then

$$V=\frac{V_0}{\pi}\left\{\sin^{-1}\frac{2c}{s_1+s_2}+\frac{a}{r}\sin^{-1}\frac{2c}{t_1+t_2}\right\},$$

where t_1 and t_2 are related to the image of the considered point as s_1 and s_2 are to the point itself.

But from a figure it is seen at once that

$$\frac{s_1}{t_1}=\frac{s_2}{t_2}=\frac{r}{a}.$$

Thus we have finally

$$V=\frac{V_0}{\pi}\left\{\sin^{-1}\frac{2c}{s_1+s_2}+\frac{a}{r}\sin^{-1}\frac{r}{a}\frac{2c}{s_1+s_2}\right\}. \quad . \quad (2)$$

With respect to the angles that appear in this formula it follows from the foregoing considerations that, since the first must be taken obtuse when the considered point lies in the space between the bowl and the plane of its rim, the second is obtuse when the image of the point lies within the same space. In all other cases the angles are acute.

§ 3. As an application of (2) we may calculate the capacity of the bowl by writing down the potential at the centre of the sphere, which obviously is equal to the charge of the bowl divided by the radius of curvature.

We find thus

$$\mathrm{V}=\frac{\mathrm{V}_0}{\pi}\left\{\sin^{-1}\frac{c}{a}+\frac{c}{a}\right\},$$

or, writing 2α for the amplitude of the bowl with regard to the centre,

$$\mathrm{V}=\frac{\mathrm{V}_0}{\pi}\{\alpha+\sin\alpha\}=\frac{\mathrm{E}}{a}.$$

Thus

$$\mathrm{C}=\frac{\mathrm{E}}{\mathrm{V}_0}=\frac{a(\alpha+\sin\alpha)}{\pi},$$

the same as the value given in Watson and Burbury's 'Electricity' (p. 142).

The same authors treat (§ 141) of the effect on the potential of making a small hole in a spherical or infinite plane conductor.

The result they arrive at needs a correction. From our formula (2) it follows easily that, when c is small compared with a, s_1 and s_2 the potential is given by the formula,

$$\mathrm{V}=\frac{\mathrm{V}_0 a}{r}+\frac{\mathrm{V}_0}{6\pi}\cdot\frac{c^3(r^2\sim a^2)}{s^3a^2},$$

where s represents the distance of the point from the centre of the hole, while terms of higher order than the third are neglected.

It follows that the system is *not* equivalent to a complete sphere charged to potential V_0, together with an additional charge on the aperture.

Our equation gives without difficulty the charge induced on the bowl by a quantity E of electricity concentrated at any point in its neighbourhood. For, if we bring a charge $-\mathrm{V}_0 a$ in the centre of the sphere, the bowl being formerly charged to V_0 and insulated, the potential of the bowl will become null, while the distribution of its electricity remains unchanged. At any point P where the potential had the value V it will now be $\mathrm{V}-\mathrm{V}_0\,.\frac{a}{r}$; thus V represents the potential of the induced charge at the point P when the bowl is electrically connected with the earth and in its centre of curvature a charge $-V_0a$ is concentrated.

When we now call to mind the reciprocal property of Green's function, we know at the same time the potential at the centre when the inducing charge is at P, and from that we may derive at once the induced charge of the bowl by multiplying by a.

Writing E for $-V_0a$, we find thus for that charge

$$Q=-\frac{E}{\pi}\left\{\sin^{-1}\frac{2c}{s_1+s_2}+\frac{a}{r}\sin^{-1}\frac{r}{a}.\frac{2c}{s_1+s_2}\right\}^*.$$

§ 4. As a last application of (2) we proceed now to find the distribution of the electricity on the insulated and charged bowl.

To this effect we choose two points situated on the same radius, equally distant from the bowl. When their distance from it is taken infinitely small (δr), then these points are each other's images. The outer point we designate by A, the inner one by B. For the amplitudes of the bowl with regard to the point where it is cut by the line AB, we write u and $2\pi-u$, u being the outer and $2\pi-u$ the inner value.

We have thus,

$$\text{for the amplitude at A, } u+\frac{\delta u}{\delta r}\delta r,$$

$$\text{,, ,, ,, B, } 2\pi-\left(u-\frac{\delta u}{\delta r}\delta r\right).$$

The inversion gives,

$$\text{for the function at A, } \frac{a}{a+\delta r}\left\{2\pi-u+\frac{\delta u}{\delta r}\delta r\right\},$$

$$\text{,, ,, ,, B, } \frac{a}{a-\delta r}\left\{u+\frac{\delta u}{\delta r}\delta r\right\}.$$

Thus for the potential, when the bowl is charged to potential 2π,

$$\text{at A, } 2\pi-\delta r\left\{\frac{2\pi-u}{a}-2\frac{\delta u}{\delta r}\right\},$$

$$\text{at B, } 2\pi+\delta r\left\{\frac{u}{a}+2\frac{\delta u}{\delta r}\right\}.$$

For the surface-densities on the convex and concave sides

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* Compare this result with Theorem II. Chapter iii. in Maxwell's 'Electricity.'

of the bowl we find then,

$$4\pi\sigma = -\left\{2\frac{\delta u}{\delta r} - \frac{2\pi - u}{a}\right\},$$

$$4\pi\sigma' = -\left\{2\frac{\delta u}{\delta r} + \frac{u}{a}\right\}.$$

Now we have

$$u = 2\sin^{-1}\frac{2c}{s_1+s_2}, \quad ds_1 = \frac{s_1}{2a}\,\delta r, \quad ds_2 = \frac{s_2}{2a}\,\delta r.$$

Thus

$$\frac{\delta u}{\delta r} = -\frac{2c}{a\sqrt{(s_1+s_2)^2 - 4c^2}}.$$

And, finally, multiplying by $\frac{V_0}{2\pi}$ to make the potential on the bowl V_0 instead of 2π:

$$4\pi\sigma = \frac{V_0}{\pi a} + \frac{V_0}{\pi a}\left\{\frac{2c}{\sqrt{(s_1+s_2)^2 - 4c^2}} - \sin^{-1}\frac{2c}{s_1+s_2}\right\},$$

$$4\pi\sigma' = \frac{V_0}{\pi a}\left\{\frac{2c}{\sqrt{(s_1+s_2)^2 - 4c^2}} - \sin^{-1}\frac{2c}{s_1+s_2}\right\}.$$

Assuming polar coordinates, taking the centre of the bowl for pole and its centre of curvature for origin, then

$$s_1 = 2a\sin\frac{a+\eta}{2}, \quad s_2 = 2a\sin\frac{a-\eta}{2},$$

$$\sin 2u = \frac{2c}{s_1+s_2} = \sqrt{\frac{1+\cos a}{1+\cos\eta}},$$

$$\tan 2u = \frac{2c}{\sqrt{(s_1+s_2)^2 - 4c^2}} = \sqrt{\frac{1+\cos a}{\cos\eta - \cos\alpha}}.$$

Substituting these values, we have

$$4\pi\sigma' = \frac{V_0}{\pi a}\left\{\sqrt{\frac{1+\cos\alpha}{\cos\eta - \cos a}} - \tan^{-1}\sqrt{\frac{1+\cos\alpha}{\cos\eta - \cos\alpha}}\right\},$$

and for $4\pi\sigma$ the same value with the addition of $\frac{V_0}{\pi a}$.

These are Thomson's expressions for the density (Reprint, p. 185).

§ 5. To solve the second part of our problem, we shall make use of the following theorem :—

When two functions, being so related that by inversion their values interchange, are inverted once more with regard to an arbitrary sphere, they will remain each other's inverted functions with respect to the image of the first sphere.

Or otherwise: When a function is inverted several times in succession the resulting function is independent of the order in which the inversions are effected.

This theorem may easily be proved with the aid of a simple figure (fig. 2) and a very small amount of calculation. Even without proof it will readily be accepted, being rather obvious.

We apply it to the potential of the spherical bowl, which we suppose to be a part of the spherical surface, represented (in section) by fig. 2.

The potential of A consists of two parts, which are each other's inverted functions with regard to the sphere; they will continue to be so related after an inversion with P for centre, and the tangent C from P to the sphere for radius. The first term is the amplitude of the bowl at A multiplied by the factor $\frac{V_0}{2\pi}$. By the inversion the lines drawn from A to the rim of the bowl become circular arcs through P B (the image of A with respect to the sphere), and points of the rim of a new bowl, the image of the first with respect to P: this of course forms a part of the same sphere, the latter remaining unchanged after the inversion. It is this new bowl with which we presently shall have to deal. From the elementary theory of the reciprocal radii, it follows that the angles formed by the circular arcs, referred to above, are the same as those between the lines of which they are the inversion. For shortness' sake I would venture to call the greatest of these the circular amplitude of the new bowl with respect to P and B. I shall designate it by the symbol 2θ.

The first term of the inverted potential now becomes

$$V_0 \cdot \frac{2\theta}{2\pi} \cdot \frac{b}{\mathrm{PB}}.$$

The second term is the inverted function of that represented by the first term with respect to the sphere. Thus it has at the point B the value that the first term has at B′, multiplied by $\frac{a}{\mathrm{OB}}$. We may write for it

$$V_0 \cdot \frac{2\theta'}{2\pi} \cdot \frac{b}{\mathrm{PB'}} \cdot \frac{a}{\mathrm{OB}};$$

where $2\theta'$ represents the circular amplitude of the new bowl with respect to P and B′. In the new system the bowl has not a constant potential, but for the point C on the bowl it is

$$V_0 \cdot \frac{b}{PC}.$$

However, by concentrating at P a quantity of electricity $-V_0 b$, the potential on the bowl becomes null; and at B we have, writing E for $-V_0 b$,

$$V = \frac{E}{PB} - \frac{E}{\pi}\left\{\frac{\theta}{PB} + \frac{a\theta'}{PB'.OB}\right\}. \quad . \quad . \quad . \quad (3)$$

This accordingly is the expression for the potential at any point B in the electric field, determined by a charge E at P, the bowl being connected by a thin conducting wire with the earth. What still remains to be done is to express θ and θ' as functions of any parameters, which are capable of defining the positions of P and B with reference to the bowl and to each other.

This purely geometrical problem requires for its solution rather lengthy calculations. At least I have not been able to compress them within narrower limits than is done in the following paragraph.

§ 6. The problem before us may be stated thus:—

Being given in space a circle and two arbitrary points; draw all possible circles through the two points and a point of the given circumference: it is required to find the greatest angle enclosed between two of these circles.

We shall solve this problem by the method of the reciprocal radii, of which the theory of electrical images is the extension. The figure defined in the statement of the problem we shall transform by the said method into fig. 1.

To this effect we assume a coordinate system with B for origin, and the line drawn from B to P for positive z axis. We suppose the circle to be given as the section of a sphere through B, and a plane.

Let the equation of the plane be

$$\alpha x + \beta y + \gamma z - p = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

with the condition

$$\alpha^2 + \beta^2 + \gamma^2 = 1;$$

that of the sphere be

$$(x-u)^2 + (y-v)^2 + (z-w)^2 = V^2, \quad . \quad . \quad . \quad (5)$$

with the condition

$$u^2 + v^2 + w^2 = V^2.$$

To invert this figure with B for centre and PB=R for radius, the following substitutions are necessary:—

$$x=\frac{R^2}{\rho^2}\xi,\quad y=\frac{R^2}{\rho^2}\eta,\quad z=\frac{R^2}{\rho^2}\zeta;$$

where ξ, η, and ζ are the coordinates of the inverted point x, y, z, and ρ^2 stands for

$$\xi^2+\eta^2+\zeta^2.$$

The plane (4) becomes a sphere,

$$\left(\xi-\frac{\alpha R^2}{2p}\right)^2+\left(\eta-\frac{\beta R^2}{2p}\right)^2+\left(\zeta-\frac{\gamma R^2}{2p}\right)^2=\frac{R^4}{4p^2}; \quad . \quad (4\text{A})$$

and the sphere (5) a plane,

$$u\xi+v\eta+w\zeta=\frac{R^2}{2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (5\text{A})$$

The point P remains unchanged, while B becomes the point at infinity. The quantities to be found presently are:—

(1) The radius a of the circle determined by (4A) and (5A).

(2) The normal h from P on (5A).

(3) The distance b from the centre of the circle to the foot-point of h.

Setting, for brevity's sake,

$$\alpha u+\beta v+\gamma w=P,$$

we find without much difficulty,

$$a^2=\frac{R^4}{4p^2V^2}\{V^2-(p-P)^2\}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

$$h=\frac{R}{2V}(R-2w), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

$$b^2=\left\{\frac{\alpha R^2}{2p}+(l-h)\frac{u}{V}\right\}^2+\left\{\frac{\beta R^2}{2p}+(l-h)\frac{v}{V}\right\}^2 \\ +\left\{\frac{\gamma R^2}{2p}+(l-h)\frac{w}{V}-R\right\}^2;$$

or, after some reduction,

$$b^2=\frac{R^4}{4p^2}-\frac{R^2}{4p^2V^2}(2pw-PR)+R^2-\frac{\gamma R^3}{p}. \quad . \quad . \quad . \quad . \quad (8)$$

It may be remarked that the perpendicular h will have the positive or negative sign according as it lies with B on the same or the opposite side of the plane.

By these formulæ, a, h, and b are expressed as dependent on quantities artificially introduced by the use of a coordinate system. We shall now proceed to get rid of those lines and angles wholly alien to the problem, and to substitute for them parameters naturally belonging to the figure in question. We adopt as such:—

The perpendicular p from B on the plane of the circle.
The perpendicular p_1 from P on the same plane.
The perpendicular q from the centre of the sphere (5) on this plane.
The perpendicular q_1 on this same plane from the centre of a sphere through P and the circumference.
The radius c of the circle.
The distance R from B to P.

It is easily seen that these six independent quantities are sufficient to define the figure.

The following relations exist between the old and the new parameters :—

$$p_1 = p - \gamma \mathrm{R},$$
$$q = p - \mathrm{P},$$
$$c^2 = \mathrm{V}^2 - q^2,$$
$$q - q_1 = \frac{\mathrm{R}(2w - \mathrm{R})}{2p_1}.$$

With the aid of these we transform the formulæ (6), (7), and (8) into the following :—

$$a^2 = \frac{\mathrm{R}^4 c^2}{4p^2(c^2+q^2)}, \quad \ldots\ldots\ldots \tag{6A}$$

$$h^2 = \frac{4p^2 p_1^2 (q-q_1)^2}{4p^2(c^2+q^2)}, \quad \ldots\ldots\ldots \tag{7A}$$

$$b^2 = \frac{c^2\mathrm{R}^4 - 4p^2 p_1^2 (q-q_1)^2 + 4\mathrm{R}^2 p p_1 q q_1 + 4 p p_1 \mathrm{R}^2 c^2}{4p^2(c^2+q^2)}. \tag{8A}$$

And, finally, writing κ^2 for the common factor, $4p^2(c^2+q^2)$,

$$\kappa a = \mathrm{R}^2 c,$$
$$\kappa h = 2 p p_1 (q - q_1),$$
$$\kappa^2 (b^2 - a^2 + h^2) = 4\mathrm{R}^2 p p_1 (q q_1 + c^2).$$

When we now return to fig. 1, and suppose it to be the inverted figure, we see that

$$h = \mathrm{PF}, \quad b = \mathrm{OF}, \quad a = \mathrm{OC};$$

and, as before, we set

$$\mathrm{PA} = s_1, \quad \mathrm{PC} = s_2.$$

Thus

$$s_1^2 = (b+a)^2 + h^2, \quad s_2^2 = (b-a)^2 + h^2,$$
$$s_1^2 + s_2^2 = 2(a^2 + b^2 + h^2),$$
$$s_1^2 + s_2^2 - 4a^2 = 2(b^2 - a^2 + h^2),$$
$$2s_1 s_2 = 2\sqrt{(a^2+b^2+h^2)^2 - 4a^2b^2} = 2\sqrt{(b^2-a^2+h^2)^2 + 4a^2h^2}.$$

Therefore

$$2s_1s_2\kappa^2=8\mathrm{R}^2\sqrt{p^2p_1^2[(qq_1+c^2)^2+c^2(q-q_1)^2]};$$

or

$$2s_1s_2\kappa^2=8\mathrm{R}^2\sqrt{p^2p_1^2(q^2+c^2)(q_1^2+c^2)},$$

and

$$[(s_1+s_2)^2-4a^2]\kappa^2=8\mathrm{R}^2\{pp_1(qq_1+c^2)+\sqrt{p^2p_1^2(q^2+c^2)(q_1^2+c^2)}\}.$$

And, because

$$\tan\theta=\tan\mathrm{EPD}=\frac{2a}{\sqrt{(s_1+s_2)^2-4a^2}},$$

we have

$$\tan^2\theta=\frac{\mathrm{R}^2c^2}{2\{pp_1(qq_1+c^2)+\sqrt{p^2p_1^2(q^2+c^2)(q_1^2+c^2)}\}}. \quad (9)$$

The radical must always be taken with the positive sign; p and p_1, analogous to q and q_1, have the same or the contrary sign according as they fall on the same or the opposite side of the plane on which they are normal.

Equation (9) may be written in another form, and thereby considerably simplified. To this effect we introduce the angle APC, that we designate by 2ϕ, and the line $\mathrm{PE}=s$. We see easily that

$$\mathrm{PE}=s=\sqrt{s_1s_2},$$

$$s_1s_2\sin 2\phi=s^2\sin 2\phi=2pc,$$

$$s^2=s_1s_2=2p\sqrt{q^2+c^2},$$

$$s_1s_2\cos 2\phi=s^2\cos 2\phi=2pq.$$

Now t and ψ being quantities analogous to s and ϕ, but having reference to the point B, we find finally, by substitution in (9),

$$\tan^2\theta=\frac{2\mathrm{R}^2c^2}{s^2t^2\cos 2(\phi-\psi)+s^2t^2},$$

or

$$\tan\theta=\pm\frac{\mathrm{R}c}{st\cos(\phi-\psi)}. \quad \dots\dots \quad (10)$$

An expression of remarkable simplicity.

§ 7. The same formula may be made use of to find an expression for θ'. For R, t, and ψ must only be substituted the quantities R′, t', ψ' which have the same relation to B′, the image of B, that R, t, and ψ have to B itself.

But (compare fig. 3)

$$\frac{t_1'}{t_1}=\frac{t_2'}{t_2}=\frac{t'}{t}=\frac{\mathrm{OB}'}{a}=\frac{a}{\mathrm{OB}},$$

and

$$2\pi-2\psi'=2\psi-\alpha;$$

as may be seen from the similar triangles,

$$\mathrm{OBT_1}\sim\mathrm{OT_1B'},$$

and

$$\mathrm{OBT_2}\sim\mathrm{OT_2B'}.$$

α has the same signification as above.

ψ and ψ' must be reckoned positive whenever the points they are related to lie with O on the same side of the plane of the rim.

Now equation (3) becomes

$$V=\frac{E}{R}-\frac{E}{\pi}\left\{\frac{1}{R}\tan^{-1}\frac{\pm cR}{st\cos(\phi-\psi)}\right.$$
$$\left.+\frac{a}{R'.\mathrm{OB}}\tan^{-1}\frac{\pm cR'.\mathrm{OB}}{a.s.t\cos(\phi+\psi-\alpha)}\right\}. \qquad (11)$$

The apparent want of symmetry of this formula with respect to the points P and B vanishes when we consider that

$$\mathrm{PB'.OB}=\mathrm{P'B.OB},$$

which relation is immediately proved by looking at fig. 3.

§ 8. Formula (11) contains the general solution of the second part of the problem mentioned at the head of this paper. But to make it of general application, we have to explain how to decide in each given case on the signs to be given to $\tan\theta$ and $\tan\theta'$; or, what amounts to the same, whether the angles figuring in the equation are acute or obtuse. Now, in our considerations on the potential of the isolated bowl, from infinite space a finite part was singled out. For points within that limited space the angle in the first term of the potential was obtuse: the angle in the second term was obtuse when the *image* of the considered point was situated within the space, originally limited by the bowl and the plane of the rim, which by inversion becomes a space bounded by the bowl and part of the spherical surface that may be constructed through the rim of the new bowl and the point P. Since in the original figure the point at infinity is external to the space above defined, in the transformed figure the point P (the image of the point at infinity) is always external.

Thus we have the following rule to decide whether the angles in formula (11) are to be taken acute or obtuse; in other words, whether the upper or under sign is to be used.

Construct a spherical surface through one of the given points and the rim of the bowl. A part of this surface and the bowl determine a limited space, which does not contain the chosen point; when the *other* point lies within that space the angle in the *first* term is obtuse; when its *image* is within it the *second* angle is obtuse; in all other cases the angles are acute (compare fig. 4).

§ 9. For the particular case that the bowl becomes a plane disk, B′ the image of B is with B on the same perpendicular and at the same distance from the plane of the disk. Moreover

$$\frac{a.}{\mathrm{OB}} = 1, \qquad \alpha = 0.$$

Thus

$$\mathrm{V} = \frac{\mathrm{E}}{\mathrm{R}} - \frac{\mathrm{E}}{\pi}\left\{\frac{1}{\mathrm{R}}\tan^{-1}\frac{\pm c\,.\,\mathrm{R}}{s\,t\cos(\phi-\psi)} + \frac{1}{\mathrm{R}'}\tan^{-1}\frac{\pm c\,.\,\mathrm{R}'}{s\,t\cos(\phi+\psi)}\right\}. \qquad (12)$$

This is the solution of the problem that Sir W. Thomson had in view when he said, at the end of the cited memoir, "It would be interesting to continue the analytical investigation far enough to determine the electric potential at any point in the neighbourhood of a disk electrified under influence."

The same formula holds for the case of an infinite plane with a circular aperture. The difference lies in the space above defined, as is shown in figs. 5 and 6.

Formula (11) may, of course, be applied to the case of a spherical surface without any aperture, and the known formulæ are then easily found.

§ 10. Perhaps it will be not uninteresting to give the expansions of formula (2) in spherical harmonics. I shall not, however, show how to get the series; but I shall write the equation down, and afterwards demonstrate its truth. It takes the following forms:—

$$\left.\begin{aligned} &\text{for } r<a,\ \mathrm{V}' = \frac{\mathrm{V}_0}{\pi}\sum_{n=0}^{n=\infty}\left(\frac{r}{a}\right)^{n}\left\{\frac{\sin n\alpha}{n} + \frac{\sin(n+1)\alpha}{n+1}\right\}\phi_n(\theta);\\ &\text{for } r>a,\ \mathrm{V} = \frac{\mathrm{V}_0}{\pi}\sum_{n=0}^{n=\infty}\left(\frac{a}{r}\right)^{n+1}\left\{\frac{\sin n\alpha}{n} + \frac{\sin(n+1)\alpha}{n+1}\right\}\phi_n(\theta); \end{aligned}\right\} \quad (13)$$

where ϕ_n is a zonal harmonic, the coefficient of x^n in the expansion of $(1+x^2-2x\cos\theta)^{-\frac{1}{2}}$.

I shall presently show that the function defined by equations (13) has the following properties:—

(1) $\Delta^2 V' = 0, \qquad \Delta^2 V = 0.$

(2) for $r = a, \quad \theta < \alpha, \qquad V' = V = V_0.$

(3) for $r = a, \quad \theta > \alpha, \qquad V' = V.$

(4) for $r = a, \quad \theta > \alpha, \qquad \left(\frac{\delta V'}{\delta r}\right) = \left(\frac{\delta V}{\delta r}\right).$

Since every term is a spherical harmonic, the first-named property is evident. To prove the exactness of the other statements, we take as a starting-point the function

$$(1 + z^2 - 2z \cos \theta)^{-\frac{1}{2}}$$

of the complex argument z. This function has two points of discontinuity, namely,

$$z = e^{\pm \iota \theta},$$

whose moduli are equal to unity. We may write thus

$$(1 + M^2 e^{2\iota\alpha} - 2Me^{\iota\alpha} \cos \theta)^{-\frac{1}{2}} = \Sigma\, M^n e^{n\iota\alpha} \phi_n \text{ for } M < 1,$$

and

$$(1 + M^2 e^{2\iota\alpha} - 2Me^{\iota\alpha} \cos \theta)^{-\frac{1}{2}} = \frac{1}{M} \Sigma \left(\frac{1}{M}\right)^n e^{n\iota\alpha} \phi_n \text{ for } M > 1.$$

Since the function is continuous for $M = 1$ (unless $\theta = \alpha$), and since both expressions become identical in that case, we have generally

$$(1 + e^{2\iota\alpha} - 2e^{\iota\alpha} \cos \theta)^{-\frac{1}{2}} = \sum_0^\infty e^{n\iota\alpha} \phi_n,$$

with the only condition $\theta \gtrless \alpha$. Or

$$\frac{\cos \frac{\alpha}{2} - \iota \sin \frac{\alpha}{2}}{\sqrt{\cos^2 \frac{\alpha}{2} - \cos^2 \frac{\theta}{2}}} = \sum_0^\infty e^{n\iota\alpha} \phi_n;$$

and, multiplying by $e^{\iota\alpha}$,

$$\frac{\cos \frac{\alpha}{2} + \iota \sin \frac{\alpha}{2}}{\sqrt{\cos^2 \frac{\alpha}{2} - \cos^2 \frac{\theta}{2}}} = \sum_0^\infty e^{(n+1)\iota\alpha} \phi_n.$$

Separating the real and imaginary parts of these formulæ, we must make a distinction between the cases $\theta < \alpha$ and $\theta > \alpha$. We find, For $\theta < \alpha$:

$$\sum_0^\infty \phi_n \cos n\alpha = \frac{\sin\frac{\alpha}{2}}{2\sqrt{\cos^2\frac{\theta}{2} - \cos^2\frac{\alpha}{2}}}, \quad . \quad . \quad . \quad (a)$$

$$\Sigma\, \phi_n \sin n\alpha = \frac{\cos\frac{\alpha}{2}}{2\sqrt{\cos^2\frac{\theta}{2} - \cos^2\frac{\alpha}{2}}}, \quad . \quad . \quad . \quad (b)$$

$$\Sigma\, \phi_n \cos(n+1)\alpha = \frac{-\sin\frac{\alpha}{2}}{2\sqrt{\cos^2\frac{\theta}{2} - \cos^2\frac{\alpha}{2}}}, \quad . \quad . \quad . \quad (c)$$

$$\Sigma\, \phi_n \sin(n+1)\alpha = \frac{\cos\frac{\alpha}{2}}{2\sqrt{\cos^2\frac{\theta}{2} - \cos^2\frac{\alpha}{2}}}. \quad . \quad . \quad . \quad (d)$$

For $\theta > \alpha$:

$$\Sigma\, \phi_n \cos n\alpha = \frac{\cos\frac{\alpha}{2}}{2\sqrt{\sin^2\frac{\theta}{2} - \sin^2\frac{\alpha}{2}}}, \quad . \quad . \quad . \quad (a')$$

$$\Sigma\, \phi_n \sin n\alpha = \frac{-\sin\frac{\alpha}{2}}{2\sqrt{\sin^2\frac{\theta}{2} - \sin^2\frac{\alpha}{2}}}, \quad . \quad . \quad . \quad (b')$$

$$\Sigma\, \phi_n \cos(n+1)\alpha = \frac{\cos\frac{\alpha}{2}}{2\sqrt{\sin^2\frac{\theta}{2} - \sin^2\frac{\alpha}{2}}}, \quad . \quad . \quad . \quad (c')$$

$$\Sigma\, \phi_n \sin(n+1)\alpha = \frac{\sin\frac{\alpha}{2}}{2\sqrt{\sin^2\frac{\theta}{2} - \sin^2\frac{\alpha}{2}}}, \quad . \quad . \quad . \quad (d')$$

Integrating (a) and (c) between the limits 0 and α, (a') and (c') between the limits α and π, we have the further formulæ :—

For $\theta < a$:

$$\Sigma \phi_n \frac{\sin na}{n} = \cos^{-1}\left(\frac{\cos\frac{a}{2}}{\cos\frac{\theta}{2}}\right) + \frac{\pi}{2}, \quad . \quad . \quad . \quad . \qquad (e)$$

$$\Sigma \phi_n \frac{\sin(n+1)a}{n+1} = -\cos^{-1}\left(\frac{\cos\frac{a}{2}}{\cos\frac{\theta}{2}}\right) + \frac{\pi}{2}. \quad . \quad . \quad . \qquad (f)$$

For $\theta > a$:

$$\Sigma \phi_n \frac{\sin na}{n} = \Sigma \phi_n \frac{\sin(n+1)a}{n+1} = \sin^{-1}\left(\frac{\sin\frac{a}{2}}{\sin\frac{\theta}{2}}\right). \qquad (e' \,\&\, f')$$

By addition of (e) and (f) we see at once that

$$V = V' = V_0$$

for $\theta < a$, $r = a$.

The differentiation of (13) gives, for $r = a$,

$$\left(\frac{\delta V'}{\delta r}\right)_{r=a} = \frac{V_0}{\pi a} \sum_0^\infty \left[\sin na + \sin(n+1)a - \frac{\sin(n+1)a}{n+1}\right] \phi_n.$$

$$\left(\frac{\delta V}{\delta r}\right)_{r=a} = \frac{V_0}{\pi a} \sum_0^\infty \left[\sin na + \sin(n+1)a - \frac{\sin na}{na}\right] \phi_n.$$

Thus, for $\theta > a$, we have

$$\left(\frac{\delta V}{\delta r}\right) = \left(\frac{\delta V'}{\delta r}\right) = -\frac{V_0}{\pi a} \sin^{-1}\left(\frac{\sin\frac{a}{2}}{\sin\frac{\theta}{2}}\right).$$

The mere inspection of the series (13) shows that for $r = a$ $V = V'$, and that V vanishes at infinity.

The function defined by the series has the stated properties, and accordingly it satisfies all the conditions required for the potential in the neighbourhood of a bowl charged to potential V_0.

For $\theta < a$, we find, moreover,

$$\sigma' = -\frac{1}{4\pi}\left(\frac{\delta V'}{\delta r}\right)_{r=a} = \frac{V_0}{4a\pi^2}\left\{\frac{\cos\frac{a}{2}}{\sqrt{\cos^2\frac{\theta}{2}-\cos^2\frac{a}{2}}} + \cos^{-1}\left(\frac{\cos\frac{a}{2}}{\cos\frac{\theta}{2}}\right) - \frac{\pi}{2}\right\},$$

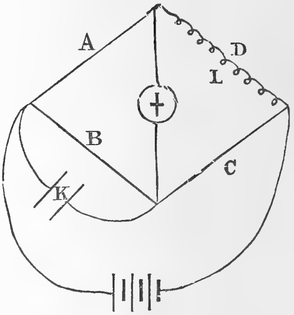
$$\sigma = -\frac{1}{4\pi}\left(\frac{\delta V}{\delta r}\right)_{r=a} = \frac{V_0}{4a\pi^2}\left\{\frac{\cos\frac{a}{2}}{\sqrt{\cos^2\frac{\theta}{2}-\cos^2\frac{a}{2}}} + \cos^{-1}\left(\frac{\cos\frac{a}{2}}{\cos\frac{\theta}{2}}\right) + \frac{\pi}{2}\right\}.$$

When we introduce α and θ instead of $\frac{\alpha}{2}$ and $\frac{\theta}{2}$, Thomson's formulæ for the density reappear. It is to be noticed, however, that in this paragraph these formulæ have been proved without making use of the theory of electrical images.

IV. *On a Modification of a Method of* Maxwell's *for Measuring the Coefficient of Self-induction. By* E. C. Rimington[*].

In 'Electricity and Magnetism,' § 778, vol. ii., Maxwell gives a method of comparing the coefficient of self-induction of a coil with the capacity of a condenser, the connexions for which are practically as follows:—D is the resistance, possessing self-induction; the armatures of a condenser, of capacity K, are connected to the ends of the arm B. There are keys in the battery and galvanometer-circuits. An ordinary balance is obtained by depressing the battery-key before the galvanometer-key; then AC=BD. The resistances B and D are next adjusted so that no momentary deflection shall exist on the galvanometer when its circuit is made before that of the battery. Then L=KBD.

Fig. 1.



This method is very inconvenient, as it necessitates a double adjustment. One of the resistances, generally C, has to be adjusted so as to give an ordinary balance; then, in order to obtain no deflection when the galvanometer-circuit is closed,

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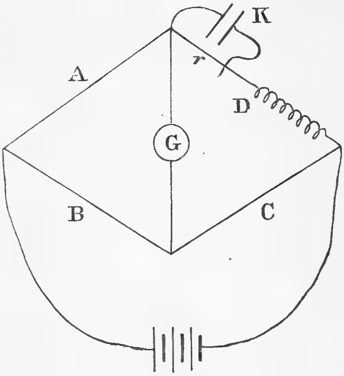
[*] Communicated by the Physical Society: read May 14, 1887.

first the resistance B will have to be altered, as D is generally the resistance of the coil. This necessitates a fresh adjustment of C, and so on.

The first modification of this method I made was by putting a resistance possessing no self-induction in the arm D in series with the coil, as in fig. 2, a slider moving over this resistance and the condenser being connected as shown.

Fig. 2.

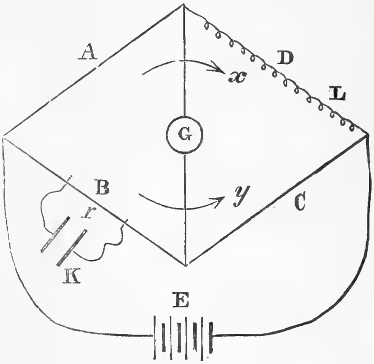


An ordinary bridge-balance is first obtained, and the slider is then adjusted until there is no throw on the galvanometer, when its circuit is closed before that of the battery. Then

$$L = Kr^2,$$

where r is the resistance between the armatures of the condenser. This method was found to be insensitive, as, unless K were large, r had to be made high. It will not do to make K too large with an ordinary mirror-galvanometer, on account of a sort of double throw being obtained, the effect of self-induction being more rapid than that of the condenser, and the galvanometer not ballistic enough. I then adopted the following modification, which answers very well.

Fig. 3.



The arm B is a resistance on which two sliders move, one of them being connected to each armature of the condenser K.

Obtain a permanent balance; then AC=BD.

Now adjust sliders until there is also a balance when galvo.-circuit is closed first. Let x be the current flowing in the arms A and D when it has attained its permanent value, and let y be that in the arms B and C. Let r be the resistance between the sliders when both balances are obtained. Let the battery and galvanometer circuits be both closed, and let the former be broken. The quantity of electricity which passes through the galvanometer due to the self-induction in D

$$=\frac{Lx}{C+D+\frac{G(A+B)}{G+A+B}}\times\frac{A+B}{G+A+B}$$

$$=\frac{LxB}{B(C+D)+G(B+C)}, \text{ since } AC=BD.$$

The quantity which passes through the galvanometer, due to the discharge of the condenser,

$$=Kyr\frac{r}{A+B+\frac{G(C+D)}{G+C+D}}\times\frac{C+D}{G+C+D}$$

$$=\frac{Kyr^2C}{B(C+D)+G(B+C)}, \text{ since } AC=BD.$$

Now these quantities obviously pass through the galvanometer in opposite directions; and if there is no throw, they must be equal. Therefore

$$\frac{LxB}{B(C+D)+G(B+C)}=\frac{Kyr^2C}{B(C+D)+G(B+C)},$$

or

$$L=Kr^2\frac{y}{x}\cdot\frac{C}{B}.$$

Now $\frac{y}{x}=\frac{D}{C}$; $\therefore\ L=Kr^2\frac{D}{B}$.

If $r=B$, we have Maxwell's method, and

$$L = KBD.$$

Of course it is not necessary to use two sliders; one armature of the condenser can be connected either to the junction of A and B or to the junction of B and C, and the other to the slider; but by having B composed of two slide-resistances, the smaller one being equal to the resistance of one of the coils of the larger, and employing a slider on each, a much greater range of adjustment can be obtained.

Suppose r to be slightly out of adjustment by an amount δ. Then the quantity which passes through the galvanometer, or

$$q=\frac{K(r+\delta)^2Cy-BLx}{B(C+D)+G(B+C)}.$$

Now $y=x\frac{D}{C}$;

$$\therefore q=x\frac{K(r+\delta)^2D-BL}{B(C+D)+G(B+C)},$$

and since $BL = Kr^2D$,

$$q = x\frac{2Kr\delta D}{B(C+D)+G(B+C)}.$$

If p be the fractional error in r, $\delta = pr$;

$$\therefore\ q = x\frac{2Kr^2Dp}{B(C+D)+G(B+C)}.$$

Now

$$x = \frac{E}{\rho + \frac{(A+D)(B+C)}{A+B+C+D}} \times \frac{C}{C+D}$$

$$= E\frac{C}{\rho(C+D)+D(B+C)},$$

since $AC = BD$; and $Kr^2D = BL$;

$$\therefore\ q = \frac{2ELpBC}{\{\rho(C+D)+D(B+C)\}\{B(C+D)+G(B+C)\}}$$

$$q = \frac{2Elp}{\left\{\rho\left(1+\frac{D}{C}\right)+D\left(1+\frac{B}{C}\right)\right\}\left\{C+D+G\left(1+\frac{C}{B}\right)\right\}}.$$

If we wish to make q as large as possible for a given value of p, we must make C small and consequently B also small. Of course B cannot be less than r, and will generally be a given resistance, depending on the apparatus we are employing. If B is given, differentiating the expression for q with respect to C, and equating to zero, we obtain

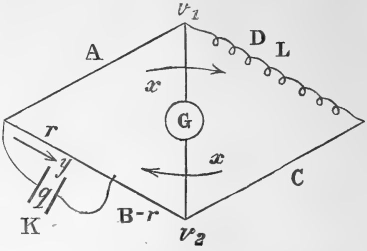
$$C = \sqrt{\frac{BD(G+D)(\rho+B)}{(G+B)(\rho+D)}}.$$

If ρ is small compared to the other resistances, this gives

$$C = B\sqrt{\frac{G+D}{G+B}}.$$

This method can be made much more sensitive by adopting the principle employed by Professors Ayrton and Perry in their admirable instrument the Secohmmeter. A commutator is introduced into the battery and galvanometer-circuits, which by its revolution puts on the battery, the galvanometer-circuit being closed, breaks the latter, or short-circuits the galvanometer and then breaks the battery-circuit, afterwards closing the galvanometer-circuit, repeating this cycle of operations during each revolution. In this method neither the speed of the commutator nor the fraction of the cycle during which the battery and galvanometer are on together need be known, as is the case with the secohmmeter.

To find the conditions under which a telephone may replace the galvanometer.



In order that the galvanometer may be replaced by a telephone it is necessary that the current through it at any moment shall be zero. Consequently v_1 must equal v_2 always. Therefore the resistance of G may be anything. Let it be infinite, and let x be the current in A, D, C, and B$-r$, y the current in r, and q the charge on the condenser at any moment t after the battery-circuit is broken.

Let x_0 be the current in D, y_0 the current in r, and q_0 the charge on the condenser at the moment of breaking the battery-circuit, and suppose the break is not prolonged by a spark at the contact.

Then

$$(\mathrm{C}+\mathrm{D})x+\mathrm{L}\frac{dx}{dt}=v_1-v_2=0, \quad . \quad . \quad . \quad . \quad (1)$$

$$(\mathrm{A}+\mathrm{B}-r)x-ry=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$x+y=-\frac{dq}{dt}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

From (2),

$$y=x\,\frac{\mathrm{A}+\mathrm{B}-r}{r};$$

and therefore from (3),

$$x\,\frac{\mathrm{A}+\mathrm{B}}{r}=-\frac{dq}{dt}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Now from (1),

$$\frac{dx}{x}=-\frac{\mathrm{C}+\mathrm{D}}{\mathrm{L}}\,dt,$$

$$\int_{x_0}^{x}\frac{dx}{x}=-\frac{\mathrm{C}+\mathrm{D}}{\mathrm{L}}\int_0^t dt;$$

or

$$\log\frac{x}{x_0}=-\frac{\mathrm{C}+\mathrm{D}}{\mathrm{L}}\,t,$$

$$x=x_0e^{-\frac{\mathrm{C}+\mathrm{D}}{\mathrm{L}}t}.$$

Again, since the resistance of G may be anything, let it equal zero; then the condenser is discharging through a resistance equal to

$$\frac{r(\mathrm{A}+\mathrm{B}-r)}{\mathrm{A}+\mathrm{B}}=\frac{r\{\mathrm{B}(\mathrm{C}+\mathrm{D})-\mathrm{C}r\}}{\mathrm{B}(\mathrm{C}+\mathrm{D})},$$

since

$$\mathrm{AC}=\mathrm{BD}.$$

Therefore

$$q=q_0 e^{-\frac{t}{\mathrm{K}\frac{r\{\mathrm{B}(\mathrm{C}+\mathrm{D})-\mathrm{C}r\}}{\mathrm{B}(\mathrm{C}+\mathrm{D})}}}=q_0 e^{-\frac{t}{\mathrm{K}}\cdot\frac{\mathrm{B}(\mathrm{C}+\mathrm{D})}{r\{\mathrm{B}(\mathrm{C}+\mathrm{D})-\mathrm{C}r\}}},$$

and

$$-\frac{dq}{dt}=q_0\frac{\mathrm{B}(\mathrm{C}+\mathrm{D})}{\mathrm{K}r\{\mathrm{B}(\mathrm{C}+\mathrm{D})-\mathrm{C}r\}}e^{-\frac{t}{\mathrm{K}}\cdot\frac{\mathrm{B}(\mathrm{C}+\mathrm{D})}{r\{\mathrm{B}(\mathrm{C}+\mathrm{D})-\mathrm{C}r\}}}.$$

Also

$$q_0=\mathrm{K}y_0r \quad \text{and} \quad \frac{\mathrm{A}+\mathrm{B}}{r}=\frac{\mathrm{B}(\mathrm{C}+\mathrm{D})}{\mathrm{C}r}.$$

Substituting these values in (4), we obtain

$$x_0\frac{\mathrm{B}(\mathrm{C}+\mathrm{D})}{\mathrm{C}r}e^{-\frac{\mathrm{C}+\mathrm{D}}{\mathrm{L}}t}=y_0\cdot\frac{\mathrm{B}(\mathrm{C}+\mathrm{D})}{\mathrm{B}(\mathrm{C}+\mathrm{D})-\mathrm{C}r}\cdot e^{-\frac{\mathrm{B}(\mathrm{C}+\mathrm{D})}{\mathrm{K}r\{\mathrm{B}(\mathrm{C}+\mathrm{D})-\mathrm{C}r\}}t}.$$

Also

$$\frac{x_0}{y_0}=\frac{\mathrm{C}}{\mathrm{D}};$$

therefore

$$\frac{1}{r}e^{-\frac{\mathrm{C}+\mathrm{D}}{\mathrm{L}}t}=\frac{\mathrm{D}}{\mathrm{B}(\mathrm{C}+\mathrm{D})-\mathrm{C}r}e^{-\frac{\mathrm{B}(\mathrm{C}+\mathrm{D})}{\mathrm{K}r\{\mathrm{B}(\mathrm{C}+\mathrm{D})-\mathrm{C}r\}}t}.$$

This equation must hold for every value of t; hence

$$\frac{1}{r}=\frac{\mathrm{D}}{\mathrm{B}(\mathrm{C}+\mathrm{D})-\mathrm{C}r}, \quad \ldots\ldots \quad (\mathrm{A})$$

and

$$\frac{\mathrm{C}+\mathrm{D}}{\mathrm{L}}=\frac{\mathrm{B}(\mathrm{C}+\mathrm{D})}{\mathrm{K}r\{\mathrm{B}(\mathrm{C}+\mathrm{D})-\mathrm{C}r\}}. \quad \ldots\ldots \quad (\mathrm{B})$$

From (A),

$$r\mathrm{D}=\mathrm{B}(\mathrm{C}+\mathrm{D})-\mathrm{C}r,$$

$$r(\mathrm{C}+\mathrm{D})=\mathrm{B}(\mathrm{C}+\mathrm{D});$$

$$\therefore\ r=\mathrm{B} \quad \text{or} \quad \mathrm{C}=-\mathrm{D}.$$

From (B),

$$\mathrm{L}=\frac{\mathrm{K}r\{\mathrm{B}(\mathrm{C}+\mathrm{D})-\mathrm{C}r\}}{\mathrm{B}}$$

$$=\mathrm{K}r\left(\mathrm{C}+\mathrm{D}-r\frac{\mathrm{C}}{\mathrm{B}}\right).$$

If $r=\mathrm{B}$, $\mathrm{L}=\mathrm{KBD}$.

If $\mathrm{C}=-\mathrm{D}$, $\mathrm{L}=\mathrm{K}r^2\dfrac{\mathrm{D}}{\mathrm{B}}$.

As $r=\mathrm{B}$ is the only solution possible, the method can only be used with a telephone under the conditions given by Maxwell, and is impracticable on account of the trouble met with in obtaining the double adjustment.

V. *Note on Magnetization.—On Sequences of Reversals.* *By* R. H. M. Bosanquet *.

THE present paper arose out of a recent discussion, at a meeting of the Physical Society, on Prof. Ayrton's paper on the magnetic resistance of a broken ring. I alluded on that occasion to the determinations of the magnetism of rings and bars which I had made. In answer to a question as to the course I had pursued as to ascending or descending values of magnetism, I stated that in all cases I had taken the values in the ascending order. But I omitted to make it clear that the observations were made by reversal, so that the direct effects of residual magnetism were necessarily eliminated. The arrangement I have always employed may be described as an ascending sequence of reversals.

There can be no doubt, however, that, in sequences of reversals, the history of previous magnetizations produces some effect. I still consider that the best way of attacking the problem is to take the first ascending sequence, in which the history is a minimum, and then as a subsequent study to deal with the effects of the history.

Immediately after the discussion above alluded to I made a few experiments on the first bar I ever completely determined, with the view of ascertaining the general character presented by series of successive reversals. The original determinations will be found at Phil. Mag. (1884) xvii. pp. 531–6, Soft-Iron Bar I.

I shall confine myself at present to the values of magnetic resistance. The reason for preferring this datum is that, when plotted with the induction as abscissa, the form of the curve of magnetic resistance is approximately independent of the arrangement of the metal, the only material differences occurring in the region of saturation. Thus values for bars and rings differ only by a constant, which may be regarded as due to the shape. Bars with pole-pieces have similar curves lying between those for bars and rings. See Phil. Mag. xxii. p. 303, and the paper above referred to.

* Communicated by the Physical Society: read April 23, 1887.

I shall call the old experiments the first ascending sequence (A). Then I made a series which I shall call the second ascending sequence (B). Then a final descending sequence (C).

The first values of B, which follow the old saturation, have the resistance somewhat higher than that found when the bar was fresh. After a few reversals, however, the effect of the history seems to disappear as the values ascend, and the value for mean inductions (minimum value of resistance) is almost exactly the same as before. The saturation-value corresponds to a higher induction than would be given by the old curve.

The succeeding curve of descending values (C) shows increased resistance, or less magnetism, in the mean inductions. As the small inductions are approached the curve of C crosses the old curve, and ends with a lower resistance, or greater magnetism than the original initial value.

The effects of ascending and descending sequences of reversals on the initial values would therefore appear to be opposite in direction. At the same time the recent experiments are few in number, and a much more extensive course of work will have to be done before conclusions on these points can be drawn with generality.

I should like to say a few words on the relation of results of this description to the molecular hypothesis, by means of which I have represented a large number of experiments. First, as to the position in which the hypothesis stands.

I am quite unable to understand how Weber's hypothesis can be applied to account for such laws as we are dealing with, *i. e.* where the initial magnetic resistance is greater than that for mean inductions. The hypothesis, which I have worked out and applied in detail to a large number of experimental cases of all sorts*, depends on two chief processes; the one of which accounts chiefly for the larger initial values of the magnetic resistance, the other for the larger saturation-values.

First, as to the saturation-values. Each magnetic particle is supposed to transmit the magnetism through a certain axis and not otherwise. Using the analogy of a hole in a bead, packed with wires, the permeability so far is measured by the portion of the hole left unoccupied, or by what I call the defect of saturation. This alone would lead to a law of permeability similar to Frölich's supposed law of the conductivity of magnets with ends.

Next, to account for the initial values. The axes of the particles being distributed uniformly in all directions, there

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* Phil. Mag. (1885) xix. pp. 73 & 333; xx. p. 318; xxii. p. 298; xxiii. p. 350.

will be an action arising from the tension of the lines of force tending to draw all the axes towards the mean direction of the magnetism; and thus the length of the path, and consequently the resistance, will be diminished as the magnetism increases. This has all been worked out in detail and shown to be capable of representing the facts minutely, in the papers referred to. All I want to show here is that we can refer our results as to increase or diminution of resistance by sequences of reversals to this theory. For the permeability would be diminished by partially filling up the saturation-capacity, and increased by partially fixing the molecules in positions of diminished resistance. All magnetization tends to retain the latter change in the same direction; *i. e.* to diminish the mean inclination of the axes, and consequently the resistance. But successive reversals of diminishing currents are capable of removing the greater part of the magnetism that sticks in the particles (subpermanent magnetism). This accounts generally for the observation that immediately after saturation the resistance is increased, but when a descending sequence of reversals is interposed the resistance is diminished. For the large residual charge after saturation diminishes the permeability to an extent exceeding the effect of inclination of axes. But when the residual charge is dispersed by the reversals of a descending sequence, the effect of the diminished inclination of the axes remains, and increases the initial permeability.

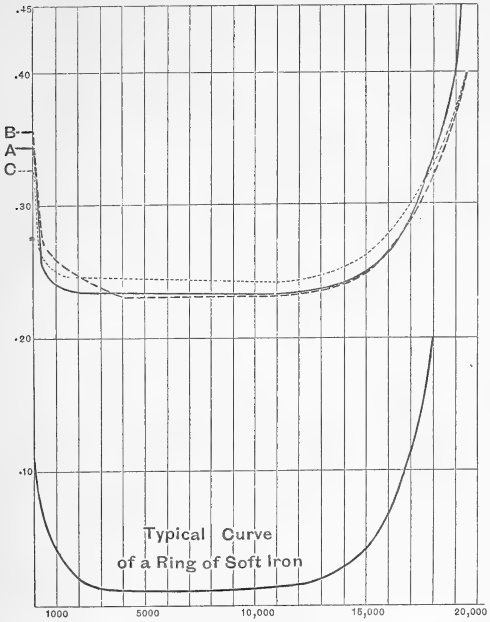
The figure and Tables which follow exhibit the results of the experiments in question.

Soft-Iron Bar ·938 centim. radius,
28·55 centim. length.

A. Original ascending sequence.		B. Second ascending sequence.		C. Descending sequence.	
𝔅.	ρ.	𝔅.	ρ.	𝔅.	ρ.
	centim.		centim.		centim.
25	·343	14	·356	15	·328
168	·315	106	·342	237	·289
414	·272	258	·314	978	·254
1,423	·237	276	·305	2,529	·250
2,467	·238	384	·286	5,577	·246
10,210	·233	1,244	·257		
17,214	·299	4,384	·231		
17,858	·331	19,396	·385		
19,289	·445				
19,650	·552				

Sequences of Reversals of Magnetism of Soft-iron Bar.

ρ = magnetic resistance, in centimetres.



Bar { A ——— original ascending sequence.
B - - - - - second ascending sequence.
C . . . . . . descending sequence.

The length of the ring is supposed to be the same as that of the bar; *i. e.* 28·55 centim.

VI. *On the Self-induction of Wires.*—Part VII.

By Oliver Heaviside*.

[Errata.—Part V. Equation (25*a*), *for* C_0 *read* C_0^2, and for (m^2 read (m^2+n^2). Last equation on page 23, put p after the } in the numerator. Part VI. Equation (46*c*), *for* $2c_1$ *read* $2\alpha c_1$. Fifth line from end of page 200, *for* L_2 *read* L'_2. Second line on page 201, for x read z.]

SOME *Notes on Part* VI.—(1) After my statement on pp. 177, 178, vol. xxiii., of the general condition of conjugacy of a pair of conductors, and the interpretation of the set of equations into which it breaks up, I stated that in cases where, by the presence of inverse powers of p, there could not be any

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* Communicated by the Author.

steady current in either of the to-be conjugate conductors due to impressed force in the other, a true resistance-balance was still wanted to ensure conjugacy, when the currents vary. I am unable to maintain this hasty generalization. In the example I gave, equations (59*c*) to (61*c*), in which each side of the quadrilateral consists of a condenser and a coil in sequence, so that there can be no steady current in the bridge-wire, it is true that the obvious simple way of getting conjugacy is to have a true resistance-balance. The conditions may then be written

$$R_1R_4=R_2R_3, \quad S_1S_4=S_2S_3, \quad L_1L_4=L_2L_3\,; \quad . \quad . \quad (1d)$$

and either

$$\left.\begin{array}{lll} & x_1=x_2, \quad \text{and} & y_1=y_2, \\ & x_3=x_4, & y_3=y_4\,; \\ \text{or else} & & \\ & x_1=x_3, \quad \text{and} & y_1=y_3, \\ & x_2=x_4, & y_2=y_4\,; \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad (2d)$$

where R stands for the resistance and L for the inductance of a coil, S for the capacity of the corresponding condenser, x for the coil time-constant L/R, and y for the condenser time-constant RS; that is, we require either vertical or else horizontal equality of time-constants, electrostatic and electromagnetic, subject to certain exceptional peculiarities similar to those mentioned in connexion with the self-induction balance. It is also the case that on first testing the power of evanescence of the other factor on the right of equation (61*c*), it seemed to always require negative values to be given to some of the necessarily positive quantities concerned. But a closer examination shows that this is not necessary. As an example, choose

$$\left.\begin{array}{llll} R_1=1, & R_2=2, & R_3=3, & R_4=10, \\ L_1=\frac{10}{7}, & L_2=5, & L_3=\frac{10}{21}, & L_4=\frac{5}{3}, \\ S_1=7, & S_2=5, & S_3=\frac{21}{29}, & S_4=\frac{15}{29}. \end{array}\right\} \quad . \quad (3d)$$

It will be found that these values satisfy the whole of equations (61*c*), and yet the resistance-balance is not established. No doubt simpler illustrations can be found. We must therefore remove the requirement of a resistance-balance when there can be no steady current, although the condition of a resistance-balance, when fulfilled, leads to the simple way of satisfying all the conditions.

(2) *Similar Systems.*—If $V=Z_1C$ be the characteristic equation of one system and $V=Z_2C$ that of a second, V being the potential difference and C the current at the terminals,

they are similar when

$$Z_1/Z_2=n, \text{ any numeric.} \quad \ldots\ldots \quad (4d)$$

Here Z is the symbol of the generalized resistance of a system between its terminals, when it is, save for its terminal connexions, independent of all other systems; a condition which is necessary to allow of the form $V=ZC$ being the full expression of the relation between V and C, Z being a function of constants and of p, p^2, p^3, &c., and p being d/dt. To ensure the possession of the property (4d), we require first of all that one system should have the same arrangement as the other, as a coil for a coil, a condenser for a condenser, or equivalence (as, for instance, by two condensers in sequence being equivalent to one); and, next, that every resistance and inductance in the first system be n times the corresponding resistance and inductance in the second system, and every capacity (electrostatic) in the second system be n times the corresponding one in the first.

Then, if the two systems be joined in parallel, and exposed to the same external impressed force at the terminals, the potentials and electromotive forces will be equal in corresponding parts, whilst the current in any part of the second system will be n times that in the corresponding part of the first. Also the electric energy, the magnetic energy, the dissipativity, and the energy-current in any part of the second system are n times those in the corresponding part of the first.

The induction-balance got by joining together corresponding points through a telephone is, of course, far more general than the Christie balance, limited to four branches, each subject to $V=ZC$; at the same time, however, it is less general than the conditions which result when the full differential equation is worked out*.

By the above, any number of similar systems may be joined in parallel, having then equal electromotive forces, and their currents in the ratio of the conductances. They will behave as a single similar system, the conductance of any part of which is the sum of the conductances of the corresponding parts in the real systems; and similarly for the capacities and for the reciprocals of the inductances. If, on the other hand, they be put in sequence, the resultant Z is the sum of the separate Z's, the current in all is the same, and the electromotive forces are proportional to the resistances.

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* This general property is, it will be seen, of great value in enabling us to avoid useless and lengthy mathematical investigations. In the 'Electrician' for May 27, 1887, I have shown how to apply it to the at first sight impossible feat of balancing iron against copper.

When the systems are not independent the above simplicity is lost; and I have not formulated the necessary conditions of similarity in an extended sense except in some simple cases, of which a very simple one will occur later in connexion with another matter.

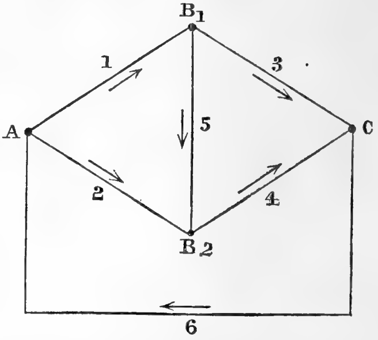
(3) *The Christie Balance of Resistance, Self and Mutual Induction.*—The three general conditions of this are given in equations (72*c*) to (74*c*). If, now, we introduce the following abbreviations,

$$\left.\begin{aligned} m_1 &= \quad \mathrm{L}_1+\mathrm{L}_2 \;\; +\mathrm{L}_5 \;\; +2(\mathrm{M}_{15}-\mathrm{M}_{12}-\mathrm{M}_{25}), \\ m_3 &= \quad \mathrm{L}_3+\mathrm{L}_4 \;\; +\mathrm{L}_5 \;\; +2(\mathrm{M}_{45}-\mathrm{M}_{34}-\mathrm{M}_{35}), \\ m_6 &= \quad \mathrm{L}_2+\mathrm{L}_4 \;\; +\mathrm{L}_6 \;\; +2(\mathrm{M}_{62}+\mathrm{M}_{64}+\mathrm{M}_{34}), \\ m_{13} &= -\mathrm{L}_5+\mathrm{M}_{13}-\mathrm{M}_{14}-\mathrm{M}_{15}-\mathrm{M}_{23}+\mathrm{M}_{24}+\mathrm{M}_{25}+\mathrm{M}_{35}-\mathrm{M}_{45}, \\ m_{36} &= -\mathrm{L}_2+\mathrm{M}_{12}+\mathrm{M}_{14}+\mathrm{M}_{16}-\mathrm{M}_{24}-\mathrm{M}_{26}+\mathrm{M}_{25}+\mathrm{M}_{45}+\mathrm{M}_{56}, \\ m_{61} &= -\mathrm{L}_4+\mathrm{M}_{32}+\mathrm{M}_{34}+\mathrm{M}_{36}-\mathrm{M}_{24}-\mathrm{M}_{46}-\mathrm{M}_{25}-\mathrm{M}_{45}-\mathrm{M}_{56}, \end{aligned}\right\} \quad (5d)$$

the conditions mentioned reduce simply to

$$\left.\begin{gathered} \mathrm{R}_1\mathrm{R}_4=\mathrm{R}_2\mathrm{R}_3, \\ (m_1+m_{13}+m_{16})\mathrm{R}_4-m_{36}\mathrm{R}_1=(m_{31}+m_3+m_{36})\mathrm{R}_2-m_{16}\mathrm{R}_3, \\ (m_1+m_{13})m_{16}=(m_3+m_{13})m_{36}. \end{gathered}\right\} \quad (6d)$$

The interpretation is, that as there are only three independent currents in the Christie arrangement, there can be only six independent inductances, viz. three self and three mutual; and these may be chosen to be the above *m*'s, whose meanings are as follows. Let the three circuits be $\mathrm{AB_1B_2A}$, $\mathrm{CB_2B_1C}$, and $\mathrm{AB_2CA}$ in the figure, so that the currents in them are C_1, C_3, and C_6. Then m_1, m_3, and m_6 are the self, and m_{13}, m_{36}, m_{61} the mutual inductances of the three circuits.



Now if the four sides of the quadrilateral consist merely of short pieces of wire, which are not bent into nearly closed curves, it is clear that (6*d*) are the true conditions, to which alone can definite meaning be attached; the inductance of a short wire being an indefinite quantity, depending upon the position of other wires. We may therefore start *ab initio* with only these six inductances, and immediately deduce* the

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* 'Electrician,' April 1, 1887, p. 457.

conditions (6*d*), saving a great deal of preliminary work. But, on coming to practical cases, in which the inductances do admit of being definitely localized in and between the six branches of the Christie, we have to expand the *m*'s properly, using (5*d*) or as much of them as may be wanted, and so obtain the various results in Part VI. Therefore equations (6*d*) are only useful as a short registration of results, subject to (5*d*), and in the remarkably short way in which they may be got; a method which is, of course, applicable to any network, which can only have as many independent inductances as there are independent circuits, *plus* the number of pairs of the same.

(4) *Reduction of Coils in Parallel to a Single Coil.*—On page 186, Part VI., in speaking of the inductometer, I referred to the most useful property that a pair of equal coils in parallel behave as one coil to external impressed force, whatever be the amount of mutual induction between them; a property which, excepting in the mention of mutual induction, I had pointed out in 1878*. But, although there appears to be no other case in which this property is true for any value of the mutual inductance, which is the property wanted, yet, if a special value be given to it, any two coils in parallel will be made equivalent to one.

The condition required is obviously that Z, the generalized resistance of the two coils in parallel, should reduce to the form $\mathrm{R}+\mathrm{L}p$. Equation (30*c*) gives Z; to make the reduction possible, on dividing the denominator into the numerator, the second remainder must vanish. Performing this work, we find

$$\mathrm{Z}=\frac{r_1r_2}{r_1+r_2}+\frac{l_1l_2-m^2}{l_1+l_2-2m}p\,; \quad . \quad . \quad . \quad . \quad . \qquad (7d)$$

which shows the effective resistance and inductance of the coils in parallel, r_1 and r_2 being their resistances, and l_1, l_2, m the inductances; subject to

$$\frac{r_1}{r_2}=\frac{l_1-m}{l_2-m}\,; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (8d)$$

giving a special value to *m*, which, if it be possible, will allow the coils to behave as one coil, so that, when put in one side of the Christie, the self-induction balance can be made. This equation (8*d*) is the expression of the making of coils 1 and 2 *similar*, in the extended sense, being the simple case to which I referred. Let a unit current flow in the circuit of

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* Journal S. T. E. vol. vi. p. 303, "On Electromagnets."

the two coils. Then l_1-m and l_2-m are the inductions through them, and these must be proportional to the resistances, making therefore the actual inductions through them always the same.

Similarly, if any number of coils be in parallel, exposed to the same impressed force V, with the equations

$$\left.\begin{array}{l} V=(r_1+l_1p)C_1+m_{12}pC_2+m_{13}pC_3+\ldots, \\ V=m_{21}pC_1+(r_2+l_2p)C_2+m_{23}pC_3+\ldots, \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \end{array}\right\} \quad . \quad . \quad (9d)$$

we have, by solution,

$$\left.\begin{array}{l} DC_1/V=N_{11}+N_{21}+N_{31}+\ldots, \\ DC_2/V=N_{21}+N_{22}+N_{23}+\ldots, \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \end{array}\right\} \quad . \quad . \quad (10d)$$

if D be the determinant of the coefficients of the C's in (9*d*), and N_{rs} the coefficient of m_{rs} in D. So, if $C=C_1+C_2+\ldots$ be the total current, we have

$$C=V(\Sigma N)/D\text{; therefore } Z=D/\Sigma N, \quad . \quad . \quad (11d)$$

where the summation includes all the N's. To reduce Z to the single coil form, we require the satisfaction of a set of conditions whose number is one less than the number of coils.

The simplest way to obtain these conditions is to take advantage of the fact that, if any number of coils in parallel behave as one, the currents in them must at any moment be in the ratio of their conductances. Then, since, by (9*d*),

$$\left.\begin{array}{l} V-r_1C_1=p(l_1C_1+m_{12}C_2+m_{13}C_3+\ldots), \\ V-r_2C_2=p(m_{21}C_1+l_2C_2+m_{23}C_3+\ldots), \\ V-r_3C_3=p(m_{31}C_1+m_{32}C_2+l_3C_3+\ldots), \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \end{array}\right\} \quad . \quad . \quad (12d)$$

are the equations of electromotive forces, when we introduce

$$r_1C_1=r_2C_2=r_3C_3=\ldots \quad . \quad . \quad . \quad . \quad . \quad (13d)$$

into them, we obtain the required conditions:—

$$\frac{l_1}{r_1}+\frac{m_{12}}{r_2}+\frac{m_{13}}{r_3}+\ldots=\frac{m_{31}}{r_1}+\frac{l_2}{r_2}+\frac{m_{23}}{r_3}+\ldots$$

$$=\frac{m_{31}}{r_1}+\frac{m_{32}}{r_2}+\frac{l_3}{r_3}+\ldots \quad . \quad . \quad (14d)$$

The induction through every coil at any moment is the same in amount; also the electromotive force due to its variation, and the electromotive force supporting current, and the impressed force.

(5) *Impressed Force in the Quadrilateral. General Property of a Linear Network.*—In my remarks on p. 190, vol. xxiii., relating to the behaviour of batteries when put in the quadrilateral, I, for brevity in an already long section, left out any reference to the theory. As is well known, in the usual Christie arrangement (see figure, above) the steady current in 5, due to an impressed force in any one of 1, 2, 3, 4, is the same whether 6 be open or closed, if a steady impressed force in 6 give no current in 5. But the distribution of current is not the same in the two cases; so that, when we change from one to the other, the current in 5 changes temporarily; as may be seen in making Mance's test of the resistance of a battery, or by simply measuring the resistance of the battery in the same way as if it had no E.M.F., using another battery in 6, but taking the galvanometer zero differently. We, in either case, have not to observe the absence of a deflection; or, which is similar, the absence of any change in the deflection; but the equivalence of two deflections, at different moments of time, between which the deflection changes. Hence Mance's method is not a true nul method, unless it be made one by having an induction-balance as well as one of resistance; in which case, if the battery behave as a mere coil or resistance, which is sometimes nearly true, especially if the battery be fresh, we may employ the telephone instead of the galvanometer.

The proof that the complete self-induction condition, $Z_1Z_4=Z_2Z_3$, where the Z's stand for the generalized resistances of the four sides of the quadrilateral, when satisfied, makes the current in the bridge-wire due to impressed force in, for example, side 1, the same whether branch 6 be open or closed, without any transient disturbance, is, formally, a mere reproduction of the proof in the problem relating to steady currents. Thus, suppose

$$C_5=\frac{Ae_1}{B}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (12d)$$

where e_1 is a steady impressed force in side 1, and A and B the proper functions of the resistances, in the case of the common Christie, but without the special condition $R_1R_4=R_2R_3$, which makes a resistance-balance. Then we know that, if we introduce this condition into A and B, the resistance R_6 can be altogether eliminated from the quotient A/B, making C_5 due to e_1 independent of R_6.

Now, in the extended problem, in which it is still possible to represent the equation of a branch by $V=ZC$, wherein Z is no longer a resistance, we have merely to write Z for R in the expansion of A/B to obtain the differential equation of C_5;

and consequently, on making $Z_1Z_4=Z_2Z_3$, we make A/B independent of Z_6. Hence, the current in the bridge-wire is independent of branch 6 altogether when the general condition of an induction-balance is satisfied, making branches 5 and 6 conjugate.

But, as is known to all who have had occasion to work out problems concerning the steady distribution of current in a network, there is a great deal of labour involved, which, when it is the special state involved in a resistance-balance, is wholly unnecessary. This remark applies with immensely greater force when the balance is to be a universal one, for transient as well as permanent currents; so that the proper course is either to assume the existence of the property required at the beginning, and so avoid the reductions from the complex general to the simple special state, or else to purposely arrange so that the reductions shall be of the simplest character. Thus, to show that C_5 is independent of branch 6, when there is an impressed force in (say) side 1, making no assumptions concerning the nature of branch 6, we may ask this question, Under what circumstances is C_5 independent of C_6? And, to answer it, solve for C_5 in terms of e_1 and C_6, and equate the coefficient of C_6 to zero.

Thus, writing down the equations of E.M.F. in the circuits AB_1B_2A and $B_1CB_2B_1$ in the above figure, we have

$$\left.\begin{aligned} e_1&=Z_1C_1+Z_5C_5-Z_2C_2,\\ 0&=Z_3C_3+Z_4C_4-Z_5C_5,\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (15d)$$

when there is no mutual induction between different branches, but not restricting Z to a particular form; and now putting

$$C_4=C_6-C_1+C_5,\quad C_3=C_1-C_5,\quad C_2=C_6-C_1, \quad . \quad (16d)$$

we obtain

$$\left.\begin{aligned} e_1+Z_2C_6&=(Z_1+Z_2)C_1+Z_5C_5,\\ Z_4C_6&=(Z_3+Z_4)C_1-(Z_3+Z_4+Z_5)C_5\,;\end{aligned}\right\} \quad . \quad (17d)$$

which give

$$C_5=\frac{(Z_3+Z_4)e_1+(Z_2Z_3-Z_1Z_4)C_6}{(Z_1+Z_2)(Z_3+Z_4)+Z_5(Z_1+Z_2+Z_3+Z_4)}, \quad . \quad (18d)$$

making C_5 independent of C_6 when the condition of conjugacy of branches 5 and 6 is satisfied.

If there are impressed forces in all four sides of the quadrilateral, then (18*d*) obviously becomes

$$C_5=\frac{(Z_3+Z_4)(e_1-e_2)-(Z_1+Z_2)(e_3-e_4)+(Z_2Z_3-Z_1Z_4)C_6}{(Z_1+Z_2)(Z_3+Z_4)+(Z_1+Z_2+Z_3+Z_4)Z_5}, \quad (19d)$$

which makes C_5 always zero if $e_1=e_2$, $e_3=e_4$, and $Z_1Z_4=Z_2Z_3$. As an example, let $e_2=0$, $e_4=0$; then, if there is conjugacy of 5 and 6, and also

$$e_1/(Z_1+Z_2)=e_3/(Z_3+Z_4), \quad . \quad . \quad . \quad . \qquad (20d)$$

the impressed forces are also balanced. Putting, therefore, batteries in sides 1 and 3, and letting them work an intermitter in branch 6, we obtain a simultaneous balance of their resistances and E.M.F.'s, and know the ratio of the latter. If self-induction be negligible, we may take Z as R, the resistance; if not negligible, it must be separately balanced.

But should there be mutual induction between different branches, this working-out of problems relating to transient states by merely turning R to Z partly fails. We may then proceed thus:—As before, write down the equations of E.M.F. in the circuits AB_1B_2A and CB_2B_1C, using the six independent inductances of these and of the circuit CAB_2C. Thus,

$$\left.\begin{aligned} e_1&=R_1C_1+R_5C_5-R_2C_2+p(m_1C_1+m_{13}C_3+m_{16}C_6),\\ 0&=R_3C_3-R_4C_4-R_5C_5+p(m_{31}C_1+m_3C_3+m_{36}C_6),\end{aligned}\right\} \qquad (21d)$$

if there is an impressed force in side 1. As before, eliminate C_2, C_3, and C_4 by (16d), and we obtain

$$\left.\begin{aligned} e_1+(R_2-pm_{16})C_6&=\{R_1+R_2+p(m_1+m_{13})\}C_1+(R_5-pm_{13})C_5,\\ (R_4-pm_{36})C_6&=\{R_3+R_4+p(m_3+m_{13})\}C_1-(R_3+R_4+R_5+pm_3)C_5,\end{aligned}\right\} \qquad (22d)$$

which, by solution for C_5, give its differential equation at once in terms of e_1 and C_6. To be independent of C_6, we require

$$\begin{aligned} &(R_2-pm_{16})\{R_3+R_4+p(m_3+m_{13})\}\\ &\qquad =(R_4-pm_{36})\{R_1+R_2+p(m_1+m_{13})\}, \quad . \qquad (23d)\end{aligned}$$

which, expanded, gives us the three equations (6d) again, showing that C_5 depends upon e_1 and the nature of sides 1, 2, 3, and 4, subject to (23d), and of 5, but is independent of the nature of C_6 altogether, except in the fact that the mutual induction between branch 6 and other parts of the system must be of the proper amounts to satisfy (23d) or (6d).

The extension that is naturally suggested of this property to any network whose branches may be complex, and not independent, is briefly as follows. The equations of E.M.F. of the branches will be of the form

$$\left.\begin{aligned} e_1+V_1&=Z_1C_1+Z_{12}C_2+Z_{13}C_3+\ldots,\\ e_2+V_2&=Z_{21}C_1+Z_2C_2+Z_{23}C_3+\ldots,\\ &\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot\end{aligned}\right\} \quad . \quad . \quad . \qquad (24d)$$

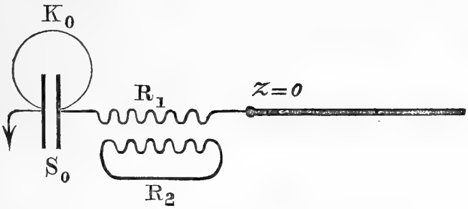
wherein the Z's are differentiation-operators.

Suppose branches m and n are to be conjugate, so that an E.M.F. in m can cause no current in n. First exclude m's equation from (24d) altogether, and, with it, Z_{mm}. Then write down the equations of E.M.F. in all the independent circuits of the remaining branches, by adding together equations (24d) in the proper order; this excludes the V's, and leaves us equations between the e's and all the independent C's, but one fewer in number than them. Put the C_m terms on the left side, then we can solve for all the currents (except C_m) in terms of C_m and the e's. That the coefficient of C_m in the C_n solution shall vanish is the condition of conjugacy, and when this happens, C_n is not merely independent of e_m but also of Z_{mm}, though not of Z_{m1}, Z_{m2}, &c.

I have dwelt somewhat upon this property, and how to prove it for transient states, because, although it is easy enough to understand how the current in one of the conjugate branches, say n, is independent of current arising from causes in the other conjugate branch, m, yet it is far less easy to understand how, when m is varied in its nature, and therefore wholly changes the distribution of current in all the branches (except one of the conjugate ones) due to impressed forces in them, it does not also change the current in the excepted branch n. Conscientious learners always need to work out the full results in a problem relating to the steady-flow of current before they can completely satisfy themselves that the property is true.

Note on Part III. *Example of treatment of terminal conditions. Induction-Coil and Condenser*—One of the side-matters left over for separate examination when giving the main investigation of Parts I. to IV. was the manner of treatment of terminal conditions when normal solutions are in question, especially with reference to the finding of the terms in the complete solution arising from an arbitrary initial state which are due to the terminal apparatus, concerning which I remarked in Part III. that the matter was best studied in the concrete application. There is also the question of finding the nature of the terminal arbitraries from the mere form of the terminal equation, without knowledge of the nature of the arrangement in detail, except what can be derived from the terminal equation.

Let, for example, in the figure, the thick line to the right be the beginning of the telegraph-line, and what is to the left of it the terminal apparatus, consisting of an induction-coil and a shunted condenser. The line is joined through the primary of the induction-coil, of resistance R_1, to the condenser of capacity S_0, whose shunt has the conductance K_0, and whose further side is connected to earth, as symbolized by the arrow-head*. Let R_2 be the resistance of the secondary coil, and L_1, L_2, M the inductances, self and mutual, of the primary and the secondary. At the distant end of the line,



where $z=l$, we may have another arrangement of apparatus, also joined through to earth, though this is not necessary. The line and the two terminal arrangements form the complete system, supposed to be independent of all other systems.

Now suppose there to be no impressed force in any part of the system, so that its state at a given moment depends entirely upon its initial state at the time of removal of the impressed forces; after which, owing to the existence of resistance, it must subside to a state of zero electric force and zero magnetic force everywhere (with some exceptional cases in which there is ultimately electric force, though not magnetic force), the manner of the subsidence to the final state depending upon the connexions of the system. The course of events at any place depends upon the initial state of every part, including the terminal apparatus, which may be arbitrary, since any values may be given to the electrical variables which serve to fully specify the amount and distribution of the electric and magnetic energies.

Suppose that V, the potential difference, and C, the current in the line, are sufficient to define its state, *i. e.* as electrical variables, when the nature of the line is given, and that u and w are the normal functions of V and C in a normal system of subsidence. Then, at time t, we have

$$V=\Sigma A u \epsilon^{pt}, \qquad C=\Sigma A w \epsilon^{pt}, \quad . \quad . \quad . \quad . \quad (1e)$$

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* It is not altogether improbable that the arrangement shown in the figure, with the receiving instrument placed in the *secondary* circuit, would be of advantage. A preliminary examination of the form of the arrival-curve when this arrangement is used for receiving at the end of a long cable, with $K_0=0$, yields a favourable result. But the examination did not wholly include the influence of the resistances on the form of the curve.

wherein the p's are known from the connexions of the whole system; each normal system having its own p, and also a constant A to fix its magnitude. The value of A is thus what depends upon the initial state, and is to be found by an integration extending over every part of the system. In one case, viz., when the initial state is what could be set up finally by any distribution of steadily acting impressed force, we do not need to perform this complex integration, since we may obtain what we want by solving the inverse problem of the setting up of the final state due to the impressed forces, as done by one method in Part III., and by another in Part IV. If also the initial state of the apparatus be neutral, so that it is the state of the line only that determines the subsequent state, we can pretty easily represent matters, viz., by giving to A the value

$$A=\frac{\int_0^l (\mathrm{SU}u-\mathrm{LW}w)dz}{\Delta}, \quad . \quad . \quad . \quad . \quad (2e)$$

wherein U and W are the initial V and C in the line, whose capacity and inductance per unit length are S and L; so that the numerator of A is the excess of the mutual electric over the mutual magnetic energy of the initial and a normal state, whilst the denominator Δ is twice the excess of the electric over the magnetic energy of the normal state itself, which quantity may be either expressed in the form of an integration extending over the whole system, or, more simply, and without any of the labour this involves, in the form of a differentiation with respect to p of the determinantal equation. For instance, when we assume $\mathrm{L}=0$, and we make the line-constants to be simply R and S, its resistance and capacity per unit length (constants), as we may approximately do in the case of a submarine cable that is worked sufficiently slowly to make the effects of inertia insensible, in which case we have

$$-\frac{d\mathrm{V}}{dz}=\mathrm{RC}, \quad -\frac{d\mathrm{C}}{dz}=\mathrm{S}\dot{\mathrm{V}}, \quad \frac{d^2\mathrm{V}}{dz^2}=\mathrm{RS}\dot{\mathrm{V}}; \quad . \quad (3e)$$

so that we may take

$$u=\sin(mz+\theta), \quad w=-\frac{m}{\mathrm{R}}\cos(mz+\theta), \quad . \quad . \quad (4e)$$

if $-m^2=\mathrm{RS}p$; then equation (2e) becomes

$$A=\frac{\mathrm{S}\int_0^l \mathrm{U}u\,dz+\mathrm{Y}_0+\mathrm{Y}_1}{\frac{1}{2}\mathrm{S}l\left\{1-\cos^2 ml\,.\,\frac{d}{d(ml)}\mathrm{F}(ml)\right\}}, \quad . \quad . \quad . \quad (5e)$$

where the undefined terms Y_0 and Y_1 in the numerator depend upon the terminal apparatus, and F in the denominator is defined by

$$\tan ml = \frac{m}{\mathrm{S}p} \cdot \frac{\mathrm{Z}_1 - \mathrm{Z}_0}{(m/\mathrm{S}p)^2 + \mathrm{Z}_1\mathrm{Z}_0} = \mathrm{F}(ml), \quad . \quad . \quad (6e)$$

which is the determinantal equation arising out of the terminal conditions

$$\mathrm{V} = \mathrm{Z}_0\mathrm{C} \text{ at } z=0, \quad \text{and } \mathrm{V} = \mathrm{Z}_1\mathrm{C} \text{ at } z=l. \quad . \quad . \quad (7e)$$

[See equations (177) to (180), Part IV.] We have now to add on to the numerator of A the terms corresponding to the initial state of the terminal apparatus, when it is not then neutral. As the process is the same at both ends of the line, we may confine ourselves to the $z=0$ apparatus, according to the figure. First we require the form of Z_0, the negative of the generalized resistance of the terminal apparatus. It consists of three parts, one due to the condenser, a second to the primary coil, and a third to the presence of the secondary; thus,

$$-\mathrm{Z}_0 = (\mathrm{K}_0 + \mathrm{S}_0 p)^{-1} + (\mathrm{R}_1 + \mathrm{L}_1 p) - \mathrm{M}^2p^2/(\mathrm{R}_2 + \mathrm{L}_2 p), \quad . \quad (8e)$$

showing the three parts in the order stated. Now, as shown in Part III., $d\mathrm{Z}_0/dp$ expresses twice the excess of the electric over the magnetic energy in a normal system (when p becomes a constant), per unit square of current. Performing the differentiation, we have

$$\frac{d\mathrm{Z}_0}{dp} = \frac{\mathrm{S}_0}{(\mathrm{K}_0 + \mathrm{S}_0 p)^2} - \mathrm{L}_1 + \frac{2\mathrm{M}^2 p}{\mathrm{R}_2 + \mathrm{L}_2 p} - \frac{\mathrm{L}_2\mathrm{M}^2p^2}{(\mathrm{R}_2 + \mathrm{L}_2 p)^2}. \quad . \quad (9e)$$

Here we may at once recognize that the first term represents twice the electric energy of the condenser per unit square of current, that the second term is the negative of twice the magnetic energy of the unit primary current, and that the fourth is, similarly, the negative of twice the magnetic energy of the secondary current per unit primary current; whilst the third, which at first sight appears anomalous, is the negative of twice the mutual magnetic energy of the unit primary and corresponding secondary current. Thus, if w_0 be the normal current-function, that is, by (4e), $w_0 = -(m/\mathrm{R}) \cos\theta$, we have

$$\frac{w_0}{\mathrm{K}_0 + \mathrm{S}_0 p}, \quad w_0, \quad \text{and } -\frac{\mathrm{M}pw_0}{\mathrm{R}_2 + \mathrm{L}_2 p}, \quad . \quad . \quad (10e)$$

as the expressions for the normal potential-difference of the condenser, for the primary current, and for the secondary current. If then V_0, C_1, and C_2 are the initial quite arbitrary values of the difference of potential of the condenser, of the primary and the secondary currents, their expansions must be

$$V_0=\Sigma A\frac{w_0}{K_0+S_0 p},\quad C_1=\Sigma A w_0,\quad C_2=\Sigma A\frac{(-Mpw_0}{R_2+L_2 p}. \quad . (11e)$$

Also, the excess of the mutual electric over the mutual magnetic energy of the initial state V_0, C_1, C_2, and the normal state represented by (10*e*) is

$$Y_0=w_0\left\{\frac{S_0V_0}{K_0+S_0 p}-(L_1C_1+MC_2)+(L_2C_2+MC_1)\frac{Mp}{R_2+L_2 p}\right\} \quad (12e)$$

and this is what must be added to the numerator in (5*e*) to obtain the complete value of A, if we also add the corresponding expression Y_1 for the apparatus at the other end, if it be not initially neutral. Using this value of A in (1*e*) and in (11*e*) with the time-factor ϵ^{pt} attached, and in the corresponding expansions for the other end, we thus by (1*e*) and (11*e*) express the state of the whole system at any time.

Since, initially, V is U, and independent of the state of the terminal apparatus, it follows that in the expansion

$$U=\Sigma A u,$$

the parts of A depending upon the apparatus contribute nothing to U, so that, by (5*e*) and (12*e*), we have the identities

$$0=\Sigma A\frac{w_0 u}{K_0+S_0 p},\quad 0=\Sigma A w_0 u,\quad 0=\Sigma A\frac{w_0 pu}{R_2+L_2 p}, \quad . \quad . \quad (13e)$$

for all the values of z from 0 to l.

It may have been observed in the above that the use of (9*e*) was quite unnecessary, owing to the forms of the normal functions in (10*e*) being independently obtainable from our *à-priori* knowledge of the terminal apparatus in detail, from which knowledge the form of Z_0 in (8*e*) was deduced; so that, without using (9*e*), we could form (11*e*) and (12*e*). I have, however, introduced (9*e*) in order to illustrate how we can find the complete solution, without knowing the detailed terminal connexions, from a given form of Z. We must either decompose dZ_0/dp into the sum of squares of admissible functions of p, multiplied by constants, say,

$$\frac{dZ_0}{dp}=a_1f_1^2+a_2f_2^2+a_3f_3^2+\ldots, \quad . \quad . \quad . \quad (14e)$$

where a_1, a_2, &c. are the constants, and f_1, f_2 the functions of p; or else into the form of the sum of squares and products, thus

$$\frac{dZ_0}{dp}=a_1f_1^2+a_2f_2^2+a_3f_3^2+b_1f_1f_2+b_2f_1f_3+\ldots. \quad . \quad (15e)$$

When this is done, we know that the terminal arbitraries are

$$F_1=\Sigma A f_1 w_0, \quad F_2=\Sigma A f_2 w_0, \quad F_3=\Sigma A f_3 w_0, \; . \; . \quad (16e)$$

and that

$$Y_0=w_0\{a_1F_1f_1+a_2F_2f_2+a_3F_3f_3+\ldots\} \; . \; . \quad (17e)$$

in the case (14*e*) of sums of squares, wherein the F's may have any values, assuming that we have satisfied ourselves that they are all independent; with the identities

$$0=\Sigma A f_1 u, \quad 0=\Sigma A f_2 u, \quad \&c. \; . \; . \; . \quad (18e)$$

Thus, in the case (9*e*), the first, second, and fourth terms are of the proper form for reduction to (14*e*), but the third is not. We are certain, therefore, that there cannot be more than three arbitraries, if there be so many. Now, if we do not recognize the connexion between the third term and those which precede and follow it (as may easily happen in some other case), we should rearrange the terms to bring it to the form (14*e*) ; for instance, thus :—

$$\frac{dZ_0}{dp}=\frac{S_0}{(K_0+S_0p)^2}-\left(L_1-\frac{M^2}{L_2}\right)-\frac{M^2R_2^2/L_2}{(R_2+L_2p)^2}, \quad (19e)$$

which is what we require. We may then take

$$\left.\begin{array}{lll} f_1=(K_0+S_0p)^{-1}, & f_2=1, & f_3=(R_2+L_2p)^{-1} \\ a_1=S_0, & a_2=-(L_1-M^2L_2), & a_3=-M^2R_2^2/L_2 \end{array}\right\} . \; . \quad (20e)$$

Further, we can certainly conclude, provided a_1 is positive, and a_2 and a_3 are negative, that the first term on the right of (19*e*) stands for electric (or potential) energy, and the remainder for magnetic (or kinetic). It is clear that we may assume any form of Z that we please of an admissible kind (*e. g.*, there must be no such thing as $p^{\frac{1}{2}}$), find the arbitraries, and fully solve the problem that our data represent, whether it be or be not capable of a real physical interpretation on electrical principles. I have pursued this subject in some detail for the sake of verifications; it is an enormous and endless subject, admitting of infinite development. Owing, however, to the abstractly mathematical nature of the investigations—to say nothing of the length to which they expand, although when carried on upon electrical principles they are much simplified, and made to have meaning—I merely propose to give later one or two examples in which circular functions of p are taken to represent Z.

Although, however, the state of the line at any moment is fully determinable for any form of the terminal Z's, when they alone are given, from the initial state of the line, provided the initial values of the terminal arbitraries be taken to be zero, and although it is similarly determinable when particular values are given to the arbitraries, whose later values also are determinable by affixing the time-factor, it does not appear that this determinateness of the later values of the terminal arbitraries is always of a complete character, when the sole data relating to them are the form of Z and their initial values. For it is possible for a terminal arrangement to have a certain portion conjugate with respect to the line; and although the state of the line will not be affected by initial energy in that portion, yet it will influence the later values of the other terminal arbitraries. This might wholly escape notice in an investigation founded upon a given form of Z with undetailed connexions, owing to the disappearance from Z of terms depending upon the conjugate portion. In such a case the reduced form of Z cannot give us the least information concerning the influence of the portion conjugate to the line. It is as if it were non-existent. If, however, Z be made more general, so as to contain terms depending upon the conjugate portion, although they be capable of immediate elimination from Z, it would seem that the indeterminateness must be removed.

Some Notes on Part IV. *Looped metallic circuits. Interferences due to Inequalities, and consequent limitations of application.*—It is scarcely necessary to remark that, in the investigation of Parts I. and II., the choice of a round wire or tube surrounded by a concentric tube for return conductor was practically necessitated in order to allow of the use of the well-known J_0 and J_1 functions and their complements, because it was not merely the total current in the wire with which we were concerned, but also with its distribution. Next, in order that it should be a question of self-induction, and not one of mutual induction also, with fearful complications, it was necessary to impose the condition that the wire, tubular dielectric, and outer tube should be a self-contained system, making the magnetic force zero at the outer boundary. It is true that no external inductive effect is observable when the double-tube circuit is of moderate length. But electrostatic induction is cumulative; and it is certain that, by sufficiently lengthening the double tube, we should ultimately obtain observable inductive interferences. Our investigation, then, only strictly applies when the double tube is surrounded on all sides, to an infinite distance, by a medium of infinite elastivity and resistivity.

[Maxwell termed $4\pi/c$, when c is the dielectric constant, the electric elasticity. I make this the elastivity: first, to have one word for two; next, to avoid confusion with mechanical elasticity; and thirdly, to harmonize with the nomenclature I have used for some time past. Thus:—

Flux.			Force.
Conduction-Current.	Resistivity.	Resistance.	Electric.
	Conductivity.	Conductance.	
Induction	Inductivity.	Inductance.	Magnetic.
Displacement . .	Elastivity.	Elastance.	Electric.
	Permittivity.	Permittance.	

The elastance of a condenser is the reciprocal of its capacity, and elastivity is the elastance per unit volume, as resistivity is the resistance per unit volume, and conductivity the conductance per unit volume. As for "permittivity" and "permittance," there are not wanting reasons for their use instead of "specific inductive capacity" (electric), and "electrostatic capacity." The word capacity alone is too general; it must be capacity for something, as electrostatic capacity. It is an essential part of my scheme to always use *single* and unmistakable words, because people will abbreviate. Again, capacity is an unadaptable word, and is altogether out of harmony with the rest of the scheme. Now the flux concerned is the electric displacement, involving elastic resistance to yielding from one point of view, and a capacity for permitting the yielding from the inverse; hence elastance and permittance, the latter being the electrostatic capacity of a condenser. There are now only two gaps left, viz. for the reciprocals of inductivity and inductance. "Resistance to lines of force" and "magnetic resistance" will obviously not do for permanent use.]

If this restriction be removed, we have self- and mutual induction concerned, and interferences; or, even if there be no external conductors, we have still the electric current of elastic displacement, and with it electric and magnetic energy outside the double tube. But, ignoring these, we have the following striking peculiarities:—Putting on one side the question of the propagation of disturbances *into* the conductors, which is so interesting a one in itself, we find that the electrical constants are three in number—the resistance, capacity, and inductance of the double-tube per unit of its length; whilst the electrical variables are two—the current in each conductor, and their difference of potential. The effective resistance per unit length is the sum of their resistances, which may be divided between the two conductors in any ratio; the capacity is that of the dielectric between them;

and the inductance is the sum of that of the dielectric, inner, and outer conductors. Another remarkable peculiarity is, that equal impressed forces, similarly directed in the two conductors at corresponding places, can do nothing; from which it follows that the effective impressed force may, like the effective resistance, be divided between the conductors in any proportion we please.

In Part IV., having in view the rapidly extending use of metallic circuits of double wires looped, excluding the earth, consequent upon the development of telephonic communication in a manner to eliminate inductive interferences, I extended the above-described method to a looped circuit consisting of a pair of parallel wires. So far as propagation into the wires is concerned, it is merely necessary that they should not be too close to one another to allow of the application of the J_0 and J_1 functions to them separately. Now suspended wires are usually of iron, and are not set too close, so that the application is justified. On the other hand, buried twin wires, though very near one another, are of copper, and also considerably smaller than the iron suspended wires; so that the diffusion effect, though not so well representable by the above-named functions, is made insignificant. Dismissing, as before, this question of inward propagation, we have, just as in the tubular case, two electrical variables and three constants, viz. the potential difference of the wires, the current in each, and the effective resistance, capacity, and inductance.

First of all, let the wires be alone in an infinite dielectric. Then we have similar results to these concerning the double-tube. The effective resistance, which is the sum of the resistances of the wires, may be divided between them in any proportions; and so may be the effective impressed force. The effective capacity is that of the condenser, consisting of the dielectric bounded by the two wires, the surface of one being the positive, and that of the other the negative coating. Or, in another form, the effective capacity is the reciprocal of the elastance from one wire to the other. In the standard medium, this elastance is, in electrostatic units, the same as the inductance, of the dielectric, in electromagnetic units. Thus,

$$L_0 = 2\mu \log \frac{r_{12}^2}{r_1 r_2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1f)$$

if r_1 and r_2 be the radii of the wires, and r_{12} their distance apart (between axes), and μ the inductivity of the dielectric.

And
$$S=c\left(2\log\frac{r_{12}^2}{r_1 r_2}\right)^{-1}. \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2f)$$

Their product, when in the same units, is v^{-2}, the reciprocal of the square of the speed of undissipated waves through the dielectric. The two variables, potential difference and current, fully define the state of the wires, except as regards the diffusion effect in them, of course, and an effect due to outward propagation into the unbounded dielectric from the seat of impressed force, which is made insignificant by the limitation of the magnetic field (in sensible intensity) due to the nearness of the wires as compared with their length. To L_0 has to be added a variable quantity, whose greatest value is $\frac{1}{2}\mu_1+\frac{1}{2}\mu_2$, if μ_1 and μ_2 are the inductivities of the wires, to obtain the complete inductance per unit length.

So far, then, there is a perfect correspondence between the double-tube and the double-wire problem. But when we proceed to make allowance for the presence of neighbouring conductors, as, for instance, the earth, although there is a formal resemblance between the results in the two cases, when the proper values are given to the constants concerned, yet the fact that in one case the outer conductor encloses the inner, whilst in the other this is not so, causes practical differences to exist. For example, there are two constants of capacity concerned in the concentric tube case, that of the dielectric between them, and that of the dielectric outside the outer tube. But in the case of looped wires there are three, which may be chosen to be the capacity of each wire with respect to earth including the other wire, and a coefficient of mutual capacity. There are, similarly, three constants of inductance, and two of resistance, and at least two of leakage, viz. from each wire to earth, with a possible third direct from wire to wire. This is when the wires are treated in a quite general matter, and arbitrarily operated upon; so that there must be four electrical variables, viz. two currents and two potential-differences. I have somewhat developed this matter in my paper "On Induction between Parallel Wires"*; and as regards the values of the constants of capacity concerned, in my paper "On the Electrostatic Capacity of Suspended Wires"†. As may be expected, the solutions tend to become very complex, except in certain simple cases. If, then, we can abolish this complexity, and treat the double wire as if it were a single one, having special electrical constants, we make a very important improvement. I have at present to point out certain

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* Journal S. T. E. and E., vol. ix. p. 427. † Ibid. vol. ix. p. 115.

peculiarities connected with the looped-wire problem in addition to those described in Part IV., and to make the necessary limitations of application of the method and the results which are required by the presence of the earth.

First of all, even though the wires be not connected to earth, if they be charged and currented in the most arbitrary manner possible, we must employ the four electrical variables and the ten or eleven electrical constants as above mentioned. On the other hand, going back to the looped wires far removed from other conductors, there are but two electrical variables and four constants (counting one for leakage). Now bring these parallel wires to a distance above the earth which is a large multiple of their distance apart. The constant S of capacity is a little increased. The method of images gives

$$S=c\left(2\log\frac{s_1s_2r_{12}^2}{r_1r_2s_{12}^2}\right)^{-1}; \quad . \quad . \quad . \quad . \quad (3f)$$

where r_1, r_2 are the radii of the wires, r_{12} their distance apart, s_1, s_2 their distances from their images, and s_{12} the distance from either to the image of the other; but, owing to s_1s_2/s_{12}^2 being nearly unity, the capacity S does not sensibly differ from the value in an infinite dielectric, or the earth has scarcely anything to do with the matter*. If, however, the wires be brought close to the earth, the increase of capacity will become considerable; this is also the case when the wires are buried. The extreme is reached when each wire is surrounded by dielectric to a certain distance, and the space between and surrounding the two dielectrics is wholly filled up with well-conducting matter. Then the capacity S becomes the reciprocal of the sum of the capacities of the two wires with respect to the enveloping conductive matter; in another form, the effective elastance is the sum of the elastances of the two dielectrics. Returning to the suspended wires, if the earth were infinitely conducting, the effective inductance would be the reciprocal of S in $(3f)$ with μ written for c, in electromagnetic units, with $\frac{1}{2}(\mu_1+\mu_2)$ added; whilst, allowing for the full extension of the magnetic field into the earth, we should

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* On the other hand, Mr. W. H. Preece, F.R.S., assures us that the capacity is *halved* (Proc. Roy. Soc. March 3, 1887, and Journal S. T. E. and E., June 27 and Febr. 10, 1887). This is simple, but inaccurate. It is, however, a mere trifle in comparison with Mr. Preece's other errors; he does not fairly appreciate the theory of the transmission of signals, even keeping to the quite special case of a long and slowly worked submarine cable, whose theory, or what he imagines it to be, he applies, in the most confident manner possible, universally! There is hardly any resemblance between the manner of transmission of currents of great frequency and slow signals.

have the formula $(1f)$, giving a slightly greater value. The effective resistance is of course the sum of the resistances, and the effective leakage resistance would be the sum of the leakage resistances of the two wires with respect to earth, if that were the only way of getting leakage between the wires, but it must be modified in its measure by leakage being mostly from wire to wire over the insulators, arms, and only a part of the poles.

But, if there be any inequalities between the wires, differential effects will result, due to the presence of the earth, in spite of its little influence on the value of the effective capacity; whereby the current in one wire is made to be not of the same strength as in the other, and the charge on one wire not to be the negative of that on the other. The propagation of signals from end to end of the looped-circuit will not then take place exactly in the same manner as in a single wire. To allow for this, we may either bring in the full comprehensive system of electrical constants and variables; or, perhaps better, exhibit the differential effects separately by taking for variables the sum of the potentials of the wires (taking earth at zero potential) and half the difference of the strength of current in them, in addition to the difference of potential of the wires and half the sum of the current strengths, which last are the sole variables when the wires are in an infinite dielectric, or, else, are quite equal. By adopting the latter course our solutions will consist of two parts, one expressing very nearly the same results as if the differential effects did not exist, the other the differential effects by themselves.

Another result of inequalities is to produce inductive interferences from parallel wires which would not exist were the wires equal. As an example, let an iron and a parallel copper wire be looped, and telephones be placed at the ends of the circuit. Even if the wires be well twisted, there is current in the telephones caused by rapid reversals in a parallel wire whose circuit is completed through the earth. Again, if two precisely equal wires be twisted, and telephones placed at the ends as before, the insertion of a resistance into either wire intermediately will upset the induction-balance and cause current in the terminal telephones when exposed to interference from a parallel wire. This interference can be removed by the insertion of an equal resistance in the companion-wire at the same place. In the working of telephone metallic circuits with intermediate stations and apparatus, we not only introduce great impedance by the insertion of the intermediate apparatus, thus greatly shortening the length of line that can be worked through, but we produce inductive interferences from parallel wires, unless the intermediate apparatus be double, one part being in circuit with one wire, the other part (quite similar) in circuit with the other. In mentioning my brother's system of bridge-working of telephones in Part V., whereby the intermediate impedance is wholly removed, I mentioned, without explanation, the cancelling of inductive interferences. The present and preceding paragraphs supply the needed explanation of that remark. The intermediate apparatus, being in bridges across from one wire to the other, does not in the least disturb the induction-balance, so that transmission of speech is not interfered with by foreign sounds.

But theory goes much further than the above in predicting interferences than practice up to the present time verifies. For instance, if two perfectly equal wires be suspended at the same height above the ground and be looped at the ends, terminal telephones will not be interfered with by variations of current in a parallel wire equidistant from both wires of the loop-circuit, having its own circuit completed through the earth. But if the loop-circuit be in a vertical plane, so that one wire is at a greater height above the ground than the other, there must be terminal disturbance produced, even when the disturbing wire is equidistant. Similarly in the many other cases of inequality that can be mentioned.

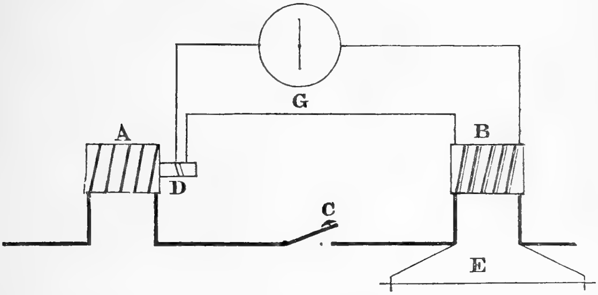
The two matters, preservation of the induction-balance, and transmission of signals in the same manner as on a single wire, are intimately connected. If we have one, we also have the other. The limitations of application of the method of Part IV. may be summed up in saying that the loop-circuit must either be far removed from all conductors, in which case equivalence of the wires is quite needless; or else they must be equal in their electrical constants. In the latter case the effective resistance R is the double of that of either wire, and the effective capacity, inductance, and leakage are to be measured as before described, whilst the variables are the potential-difference of the wires and the current in each. But the four electrical constants may vary in any (not too rapid) manner along the line. And the impressed force (in the investigations of Part IV.) may also be an arbitrary function of the distance, provided it be put, half in one wire, half in the other, oppositely directed in space. For, although equal, similarly directed impressed forces will cause no terminal disturbance (and none anywhere if other conductors be sufficiently distant), yet disturbances at intermediate parts of the line will result. It is true that the most practical case of impressed force is when it is situated at one end only of the circuit, when it is of course equally in both wires, or not in them at all; but there is such a great gain in the theoretical treatment of these problems by generalizing, that it is worth while to point out the above restriction.

Besides this case of equality of wires, which is precisely the one that obtains in practice, there are other cases in which, by proper proportioning of the electrical constants of the two looped wires, the induction-balance is preserved; and, simultaneously, we obtain transmission of signals as on a single wire. Their investigation is a matter of scientific interest, though scarcely of practical importance.

I have yet to add investigations by the method of waves (mentioned in Part IV.), by which method I have reached interesting results in a simple manner.

VII. *Note on* Prof. Carey Foster's *Method of Measuring the Mutual Induction of two Coils. By* James Swinburne[*].

PROF. FOSTER suggests at the end of his paper[†] that his method may be of use in dynamo work. Wishing to get some arrangement for testing the induction through any part of a dynamo, and for testing samples of iron for Messrs. Crompton and Co., the writer devised an arrangement which dispenses with the objectionable ballistic galvanometer. It was tried roughly last summer, with the view of making a permanent arrangement if the method worked well.



The primary current was led through an electromagnet, A, which represented the dynamo, and through one wire of a double-wound coil, B, which represented a pair of coils of known mutual induction. This coil was shunted by a wire, E,

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[*] Communicated by the Physical Society: read February 26, 1887.

[†] Phil. Mag. [5] vol. xxiii. p. 121 (February 1887).

with sliding contacts. This circuit was made or broken by the switch C. A pilot wire, D, round the model electromagnet was connected in series with the secondary coil of B and with the ordinary reflecting-galvanometer, G. The arrangement was fairly sensitive; but had one fault, which was in fact foreseen. Instead of no current, the galvanometer gets a current first one way and then the other, so that the spot jerks each way. This might be remedied by another arrangement of the wires; but a similar difficulty will arise in dynamo work. The flux of the induction through the standard coil varies as the flux of the primary current; but the flux of the induction through, say, a section of a field-magnet depends on the saturation of the iron. To avoid these difficulties, a galvanometer with a heavy ballistic astatic needle of the kind introduced by Ayrton and Perry will be used. This has been made by Mr. Dobson, lately one of Messrs. Crompton's pupils. It is of somewhat novel design. The coils, of which there are several sets, can be changed without dismounting the needle, so that the instrument can be used as a high or low resistance or differential galvanometer; and can be also used as a ballistic, or as a flywheel or integrating galvanometer. If necessary, little arms with balls like those of a minute fly-press will be added. The final apparatus was not put in hand till quite lately, as there was no suitable room available for such work; but it is now being made.

It was intended to calibrate the induction-coils from coils whose mutual induction could be calculated; or from a dynamo by measuring the induction through the armature with a known exciting current, and then running the dynamo and measuring its electromotive force and speed. This method has, by the way, been recently mentioned by M. Kapp, for calibrating a ballistic galvanometer. Prof. Foster's method seems infinitely more convenient, and may save much trouble if a good condenser is to be had. Of course an ammeter will be in the primary circuit.

It would seem that a flywheel-galvanometer might be used for such purposes as finding the ohm from a pair of coils with calculated mutual induction. Suppose, for instance, a contact-breaker makes and breaks contact in the primary circuit with a known frequency, and allows the impulses one way in the secondary coil to go round one coil of a differentially-wound flywheel-galvanometer. A constant current in the other coil is regulated to oppose these impulses; and from these the ohm might be got. Either the contacts must be made slowly enough for the current in the primary to come to its permanent value, or a correction must be made for the error. There is also a small error due to capacity. This suggestion is made with hesitation, as it seems probable that somebody must have thought of the arrangement or tried it. It is infinitely easier to think of a novelty, or an apparent novelty, than to find out whether it is really new. For instance, on looking the matter up a little for this note, the writer found that the null method of comparing the coefficients of mutual induction of coils is mentioned in a footnote to Brillouin's elaborate memoir on the comparison of coefficients of induction. Maxwell also mentions a loaded differential galvanometer for comparing frequent condenser-discharges with a known current. Roïti's method is also given in Mascart and Joubert's book.

VIII. *On the Cause of Iridescence in Clouds. By* G. JOHNSTONE STONEY, *M.A., D.Sc., F.R.S.; a Vice-President, R.D.S.**

WHEN the sky is occupied by light cirro-cumulus cloud, an optical phenomenon of the most delicate beauty sometimes presents itself, in which the borders of the clouds and their lighter portions are suffused with soft shades of colour like those of mother-of-pearl, among which lovely pinks and greens are the most conspicuous. Usually these colours are distributed in irregular patches, just as in mother-of-pearl; but occasionally they are seen to form round the denser patches of cloud a regular coloured fringe, in which the several tints are arranged in stripes following the sinuosities of the outline of the cloud.

I cannot find in any of the books an explanation of this beautiful spectacle, all the more pleasing because it generally presents itself in delightful summer weather. It is not mentioned in the part of Moigno's great *Répertoire d'Optique* which treats of meteorological optics, nor in any other work which I have consulted. It seems desirable, therefore, to make an attempt to search out what appears to be its explanation.

At the elevation in our atmosphere at which these delicate clouds are formed the temperature is too low, even in midsummer, for water to exist in the liquid state; and, accordingly, the attenuated vapour from which they were condensed passed at once into a solid form. They consist, in fact, of tiny crystals of ice, not of little drops of water. If the precipitation has been hasty, the crystals will, though all small, be of many sizes jumbled together, and in that case the beautiful

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optical phenomenon with which we are now dealing will not occur. But if the opposite conditions prevail (which they do on rare occasions), if the vapour had been evenly distributed, and if the precipitation took place slowly, then will the crystals in any one neighbourhood be little ice-crystals of nearly the same form and size, and from one neighbourhood to another they will differ chiefly in number and size, owing to the process having gone on longer or taken place somewhat faster, or through a greater depth, in some neighbourhoods than others. This will give rise to the patched appearance of the clouds which prevails when this phenomenon presents itself. It also causes the tiny crystals, of which the cloud consists, to grow larger in some places than others.

Captain Scoresby, in his 'Account of the Arctic Regions,' gives the best description of snow-crystals formed at low temperatures with which I am acquainted. From his observations it appears—(*a*) that when formed at temperatures several degrees below the freezing-point, the crystals, whether simple or compound, are nearly all of symmetrical forms; (*b*) that thin tabular crystals are extremely numerous, consisting either of simple transverse slices of the fundamental hexagon, or, more frequently, of aggregations of these attached edgewise and lying in one plane; and (*c*) that, according as atmospheric conditions vary, one form of crystal or another largely preponderates. A fuller account of these most significant observations is given in the Appendix to this paper.

Let us then consider the crystals in any one neighbourhood in the sky, where the conditions that prevail are such as to produce lamellar crystals of nearly the same thickness. The tabular plates are subsiding through the atmosphere—in fact falling towards the earth. And although their descent is very slow, owing to their minute size, the resistance of the air will act upon them as it does upon a falling feather; it will cause them, if disturbed, to oscillate before they settle into that horizontal position which flat plates finally assume when falling through quiescent air. We shall presently consider what the conditions must be, in order that the crystals may be liable to be now and then disturbed from the horizontal position. If this occasionally happens, the crystals will keep fluttering, and at any one moment some of them will be turned so as to reflect a ray from the sun to the eye of the observer from the flat surface of the crystal which is next him. Now, if the conditions are such as to produce crystals which are plates with parallel faces, and as they are also transparent, part only of the sun's ray that reaches the front face of the crystal will be reflected from it: the rest will enter the crystal, and, falling on the parallel surface behind, a portion will be there reflected, and, passing out through the front face, will also reach the eye of the observer. These two portions of the ray—that reflected from the front face and that reflected from the back—are precisely in the condition in which they can interfere with one another, so as to produce the splendid colours with which we are familiar in soap-bubbles. If the crystals are of diverse thicknesses the colours from the individual crystals will be different, and the mixture of them all will produce merely white light; but if all are nearly of the same thickness, they will transmit the same colour towards the observer, who will accordingly see this colour in the part of the cloud occupied by these crystals. The colour will, of course, not be undiluted; for other crystals will send forward white light, and this, blended with the coloured light, will produce delicate shades in cases where the corresponding colours of a soap-bubble would be vivid.

We have now only to explain how it happens that on very rare occasions the colours, instead of lying in irregular patches, form definite fringes round the borders of the cloudlets. The circumstances that give rise to this special form of the phenomenon appear to be the following:—While the cloud is in the process of growth (that is, so long as the precipitation of vapour into the crystalline state continues to take place) so long will the crystals keep augmenting. If, then, a cloudlet is in the process of formation, not only by the springing up of fresh crystals around, but also by the continued growth of the crystals within it, then will that patch of cloud consist of crystals which are largest in its central part, and gradually smaller as their situation approaches the outside. Here, then, are conditions which will produce one colour round the margin of the cloud, and that colour mixed with others, and so giving rise to other tints, further in. In this way there comes into existence that iris-like border which is now and then seen.

The occasional upsetting of the crystals, which is required to keep them fluttering, may be produced in any of three ways. The cloudlets may have been formed from the blending together of two layers of air saturated at different temperatures, and moving with different velocities or in different directions. Where these currents intermix a certain amount of disturbance will prevail, which, if sufficiently slight, would not much interfere with the regularity of the crystals, and might yet be sufficient to occasion little draughts, which would blow them about when formed. Or, if the colder layer is above, and if it is in a sufficient degree colder, there need not be any previous relative motion of the two layers; the inevitable convection-currents will suffice. Another, and probably the most frequent, cause for little breezes in the neighbourhood of the cloudlets is, that when the cloudlets are formed they immediately absorb the heat of the sun in a way that the previously clear air had not done. If they absorb enough they will rise like feeble balloons, and slight return-currents will travel downwards round their margins, throwing all crystals in that situation into disorder.

I do not include among the causes which may agitate the crystals another cause which must produce excessively slight currents of air, namely that arising from the subsidence of the cloudlets owing to their weight. The crystals will fall faster where in cloud masses than in the intervening portions where the cloud is thinner. But the subsidence itself is so slow, that any relative motions to which differences in the rate of subsidence can give rise are probably too feeble to produce an appreciable effect. Of course, in general, more than one of the above courses will concur; and it is the resultant of the effects which they would have separately produced that will be felt by the crystals.

If the precipitation had taken place so very evenly over the sky that there were no cloudlets formed, but only one uniform veil of haze, then the currents which would flutter the crystals may be so entirely absent that the little plates of crystals can fixedly assume the horizontal position which is natural to them. In this event the cloud will exhibit no iridescence, but, instead of it, a vertical circle through the sun will present itself. This on some rare occasions is a feature of the phenomenon of parhelia.

It thus appears that the occasional iridescence of cirrus clouds is satisfactorily accounted for by the concurrence of conditions, each of which is known to have a real existence in Nature. We may, in fact, recapitulate our knowledge on the subject as follows:—Captain Scoresby's observations show that the crystals of ice formed in the atmosphere do not agglutinate into snow-flakes, except at temperatures bordering on the freezing-point. At temperatures even a few degrees lower, the crystals remain distinct from one another, and at low temperatures are for the most part unmutilated and perfect geometrical figures. He has also shown that all the forms which crystals of ice can assume do not present themselves together; but that some one or two forms generally preponderate over the others, the preponderating form varying according to atmospheric conditions, which he does not seem to have fully traced out. Thin tabular crystals are frequently the preponderating form, and become more delicate and thin and diminish in size as the cold increases. Hence it is to be presumed that the state of the atmosphere in the region of the cirrus clouds will sometimes be such as to produce crystalline plates of a tolerably uniform thickness. When the atmosphere has been in this state at the formation of the cloud, we shall accordingly have either the phenomenon of iridescence, or the twin phenomenon of a vertical column of reflected light passing through the sun. This latter phenomenon will present itself but seldom, as it requires unusual quietude in the region of the cloud to allow the crystals to settle down sufficiently into the horizontal position. And, accordingly, on the rare occasions when the vertical column is seen, the cloud has been observed to present a gauze-like uniformity of appearance, which is an independent evidence of the calmness which is essential. But it much more frequently happens that the cloud is flocculent in its structure, and exposed to little breezes blowing in various directions, excited by one or more of the causes that have been indicated above. Whenever draughts of this kind intervene, the little tabular crystals are every now and then tossed about, and will then flutter; since being flat plates, subsiding through a resisting fluid, they will oscillate after each such disturbance in their progress towards the horizontal position. Whenever these events happen, we have conditions which must result in that iridescent phenomenon which is the subject of our inquiry.

[*Addition, made June* 1887.—When the fluttering of the lamellar crystals which form an iridescent cloud is gentle, the crystals will not incline much from the horizontal position; and as this is the case which most frequently occurs, it is desirable fully to consider the consequences of it. With clouds of this kind the iridescent colours will be seen only when the sun is low in the sky, and only in parts of the cloud that are at no great distance from him. In more distant clouds when the sun is low, and in all the clouds when the sun is high in the sky, the flat surfaces of the crystals do not become sufficiently inclined to reflect the sun's rays to the spectator, and accordingly all the light which reaches the eye from clouds that are so situated has been reflected by edges of crystals; or scattered in irregular ways, and is mere white light. This is the case that is oftenest seen: only a few clouds near the setting or rising sun exhibit their soft iridescence. But some few times in one's life the display may be seen in all quarters of the sky, and with the sun well up in the heavens; and the phenomenon is then one of the most enchanting presented to us by nature. This exquisite spectacle can only reveal itself on those rare occasions on which the crystals are tossed about in an unusual degree.]

APPENDIX.

Extracts from Captain Scoresby's record of his Observations on Snow Crystals formed at Low Temperatures, in his 'Account of the Arctic Regions,' vol. i., from pages 425 to 433, and plates 8, 9, 10, and 11 in vol. ii.:—

"When the temperature of the air is within a degree or two of the freezing-point, and much snow falls, it frequently consists of large irregular flakes, such as are common in Britain." "But in severe frosts, though the sky appears perfectly clear, lamellar flakes of snow, of the most regular and beautiful forms, are always seen floating in the air and sparkling in the sunbeams, and the snow which falls, in general, is of the most elegant texture and appearance." "The various modifications of crystals may be classed under five general kinds or genera:—1. Lamellar. 2. A lamellar, or spherical nucleus, with spinous ramifications in different planes. 3. Fine spiculæ, or six-sided prisms. 4. Hexagonal pyramids. 5. Spiculæ, having one or both extremities affixed to the centre of a lamellar crystal."

We are more particularly concerned with the first and the last of these genera. About the first, Captain Scoresby says:—

"1. *Lamellar Crystals.*—The varieties of this kind are almost infinite. They occur at all temperatures, and in the greatest abundance, and most of the specimens are extremely thin, transparent, and of an exquisitely delicate structure. They may be subdivided into several distinct species:—

(*a*) Stelliform; having six points radiating from a centre, with parallel collateral ramifications in the same plane. This species is the most general form met with." "It occurs in greatest profusion when the temperature approaches the freezing-point.

(*b*) Regular hexagon. This occurs in moderate, as well as in the lowest temperature; but it becomes more delicate and thin, and diminishes in size as the cold increases. Some specimens consist of simple transparent plates, others are beautifully variegated within the perimeter by white lines, forming smaller hexagons or other regular figures in immense variety."

(*c*) "Aggregations of hexagons. This beautiful species admits of immense variety. It occurs chiefly at low temperatures."

(*d*) "Combinations of hexagons, with radii or spines and projecting angles. This constitutes the most extensive species in the arrangement."

About the last, or fifth, genus, Captain Scoresby says:—

"5. *Spiculæ or prisms having one or both extremities inserted in the centre of a Lamellar Crystal.*—This is the most singular genus I have ever seen, and has been observed but twice. It resembles a pair of wheels, united by an axle-tree; the wheels consisting of hexagonal or other lamellar crystals, and the axle of a slender crystal." "Some of this extraordinary figure occurred along with the last-described genus [Hexagonal pyramids]. Of which kinds, principally, a quantity of snow, three or four inches in depth, once fell on the deck of the ship in which I sailed, in the course of a few hours. The temperature when this kind of crystal fell was in one instance 22°, and in the other 20°" [Fahrenheit].

In four engraved plates Captain Scoresby delineates ninety-six different forms, magnified from three to seven times. And he attaches a letter to most of them referring to an annexed table, in which the state of the atmosphere and weather, when each form was observed, stand recorded. In what he further adds to his record, he says :—

> "Many instances, it may be observed, occur of mutilated and irregular specimens, some wanting two or three radii, and others having radii of different shapes. But in low temperatures the greatest proportion of crystals that fall are probably perfect geometrical figures."

The foregoing are the parts of Captain Scoresby's record of his invaluable observations upon ice-crystals, which more particularly throw light on the cause why cirrus clouds are occasionally iridescent ; but the rest of the account of this accurate observer, and the admirable drawings which he made of the crystals, will also well repay careful study. Thus, if the cloud consist of crystals of Scoresby's fifth genus, or if crystals of the first and third genera are both present, and if the air is so calm that the crystals can remain in the terminal position into which they would come in falling through still air, then we shall have the phenomenon of both a horizontal and a vertical circle through the sun making a cross ; whereas, if the crystals are of the first genus only, the vertical circle will present itself without the horizontal. I have myself seen the phenomenon in this latter form. Crystals of either the first or the fifth genus, if occasionally agitated so that they will keep fluttering, would give rise to iridescence if of sufficiently uniform thickness.

Captain Scoresby describes lamellar flakes of snow floating in the air and sparkling in the sunbeams, as always present during severe frosts, when the sky is clear. The beautiful appearance they would have is a familiar one in chemical laboratories, when a glass vessel, in which precipitated tabular crystals are subsiding through the mother-liquid, is placed in the direct light of the sun. The whole liquid then seems alive with minute specks flashing with the brilliant colours of thin plates.

IX. *On a Magnetic Potentiometer.* *By* A. P. CHATTOCK*.

IN arranging some experiments on the magnetic resistances (so-called) of certain air and iron fields with a view to the more satisfactory designing of dynamos, I have been led, by the familiar analogy between magnetic and electrical circuits, to adopt the following method of measurement; which I venture to describe, partly on account of its convenience, partly because the measurement of magnetic resistances seems likely to play an important part in the practical application of electromagnetism.

The resistance between two points on a magnetic circuit may be expressed as the ratio of their potential difference to the total induction passing from one to the other (provided there is no reverse magnetomotive force between them).

The measurement of the total induction is of course a simple matter; but, so far as I am aware, no method of directly measuring differences of potential has yet been suggested.

Let A and B be two points in a magnetic field connected by any line of length l; and let H represent magnetic force resolved along l.

Then, if V be the difference of potential between A and B,

$$V=\int H\,.\,dl.$$

If, instead of points, A and B represent two equal plane surfaces of area a, and $\overline{V}$ be their average difference of potential,

$$a\overline{V}=\int V\,.\,da=\int H.\ dv,$$

v being the volume of a tube of constant cross section, a, connecting A and B by *any* path.

Now let a wire helix be wound uniformly upon such a tube, with n turns per unit length, and allow H to vary with time, t. Provided there be no magnetic substance inside the helix, an electromotive force, E, will be set up in the latter, such that

$$E=\frac{d}{dt}\iint H\,.\,da\,.\,dn=\frac{d}{dt}\,n\int H\,.\,dv=na\frac{d\overline{V}}{dt}.$$

The value of E is thus proportional to the rate of change of $\overline{V}$; and to this *alone*, if external inductive effects are guarded against by winding the wire in an even number of layers (n and a being constant). Hence, if the wire be connected with a ballistic galvanometer, and $\overline{V}$ be altered suddenly from V_1 to V_2, the needle of the galvanometer will be thrown through

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* Communicated by the Physical Society: read May 14, 1887.

the angle θ such that

$$V_1 - V_2 = K \sin \frac{\theta}{2},$$

and the combination forms what may be called a magnetic potentiometer.

In exploring a permanent field with such an apparatus, the best way is, perhaps, to fix one end of the helix in a clip, thereby keeping its potential constant, and to move the other end from point to point in the field. For this purpose the wire should be wound upon a flexible core, the average length of which, whether bent or straight, must be constant (otherwise $da \,.\, dn$ will not be equal to $n \,.\, dv$ in the last equation).

I have therefore constructed a helix by winding wire uniformly on to a piece of solid indiarubber, of constant cross section (in my case 37 centim. long and about 1 centim. diameter), leaving a small space between one turn and the next to allow the indiarubber to bend without elongating.

With this apparatus I made the following measurements of the potential difference between the ends of a permanent bar-magnet, in order to test the accuracy of the method. In the first set of readings the free end of the helix was moved at one leap from end to end of the magnet, giving a mean reading of 6·017 ; in the two other sets it was moved between the same two points in two and four leaps respectively, the resulting readings being added together in each case. The means of these were 6·047 and 6·048. The final mean was thus 6·037; and from this the most discordant reading differed by about 1 per cent. The close agreement between the second and third values was, no doubt, accidental ; but the difference between them and the first may have been due to the difficulty of moving quickly from end to end of the magnet, a distance of over 40 centim.

In order to reduce these results to absolute measure, the helix (still connected with the galvanometer) was subjected to a known magnetomotive force by passing it through a coil of n turns and bringing its ends close together outside the coil, a current C being then started or stopped in the latter. The magnetomotive force due to this was of course equal to $4\pi n C$, and this being substituted for $V_1 - V_2$ in the last equation determined the value of K. In my case these values were $n = 20$, $C = 0{\cdot}182$ C.G.S., and galvanometer throw $= 0{\cdot}34$. The difference of potential between the ends of the magnet was thus

$$\frac{4\pi \times 20 \times 0{\cdot}182}{0{\cdot}34} \times 6{\cdot}037 = 810 \text{ C.G.S.}$$

A more suitable core for the helix than indiarubber is the flexible gas-tubing made of plaited and varnished canvas. It is very uniform in cross section; and by withdrawing the metal spiral upon which it is woven and mounting it on a spindle in a screw-cutting lathe, it is easy to wind the wire uniformly upon it.

The use of the lathe is the more desirable, as measurements of potential by the helix depend very much for their accuracy upon the uniformity with which it is wound; this being especially the case if its position in the field does not happen to coincide with the direction of the lines of force.

To keep the turns in place the small spaces between them may be filled with soft cotton-thread.

University College, Bristol.

X. *On the "Dimensions" of Temperature in Length, Mass, and Time; and on an Absolute C.G.S. Unit of Temperature.* *By* CHARLES V. BURTON, *B.Sc.* (*Lond.*).*

SIR W. THOMSON'S second absolute scale gives us the means of finding the *ratio* between two temperatures, independently of any arbitrary convention as to the size of degrees. We are therefore bound to consider temperature as a physical quantity capable of exact measurement. Now every such quantity has certain dimensions in length, mass, and time; and I here propose to find the dimensions of temperature.

Let temperatures be represented on Sir W. Thomson's absolute scale, so that nothing remains arbitrary except the size of the degrees. Consider an absolutely perfect gas whose temperature on this scale $= t$. Let the mean kinetic energy of a molecule of the gas $=$ E. Then we have

$$E = kt, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where k is the same for all temperatures and for all *perfect* gases; being independent of the mass per molecule of the gas, and determined solely by the size of the degrees on our scale of temperature.

If we write (1) in the form

$$t = \frac{E}{k}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

we see that a temperature (t) is completely determined by the average-kinetic-energy-per-molecule (E) of a perfect gas

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* Communicated by the Author.

which has that temperature; and that t is proportional to E. Thus temperature is a quantity of the same kind as energy, and its dimensions are therefore

$$ML^2T^{-2}.$$

Again, we have seen that k depends only on the size of the degrees on our scale of temperature; and these degrees have hitherto been chosen of an arbitrary size.

If we were intimately acquainted with the constitution of bodies, the mass of their separate molecules, and so on, we should be able (without requiring any experiments on that hypothetical condition of matter called "perfect gas") to find the value of k corresponding to degrees of any given size on the absolute scale. Or, again, a given value of k will determine the size of the degrees on our scale. If we assume $k=1$, we get an absolute unit of temperature, derived from those of length, mass, and time, and independent of any other quantities. The relation (2) then becomes $t=E$; *i. e.* any temperature is measured by the mean-kinetic-energy-per-molecule of a perfect gas having that temperature. With the C.G.S. system of units, *the unit temperature would be that of a perfect gas whose mean kinetic energy per molecule was one erg.*

From our present knowledge of the mass of molecules, and of the properties of imperfect gases, we may form a very rough idea of the relation between the Centigrade and the C.G.S. scales of temperature. For a perfect gas we have the relation

$$PV=\tfrac{2}{3}\times \text{kinetic energy of gas},$$
$$=\tfrac{2}{3}nE;$$

where n = number of molecules in the volume V, and E, as before, is the mean-kinetic-energy-per-molecule. Now let P = one atmosphere = (roughly) 10^6 dynes per square centimetre, let V = 1 cubic centim., and let the temperature be 0° C. Then, according to Sir W. Thomson's measurements, n = about 6×10^{21};

$$\therefore\ E=\frac{3}{2}\cdot\frac{PV}{n},$$
$$=\frac{3}{2}\cdot\frac{10^6\times 1}{6\times 10^{21}},$$
$$=2\cdot5\times 10^{-16}.$$

This number is then a very rough estimation of the value of the temperature 0° C. in absolute C.G.S. measure. Thus 273 Centigrade degrees are equivalent to $2\cdot5\times 10^{-16}$ absolute units of temperature ; therefore the absolute unit of temperature is equivalent to

$$\frac{273}{2 \cdot 5} \times 10^{16} \text{ Centigrade degrees,}$$

$$= \text{about } 10^{18} \text{ Centigrade degrees.}$$

These figures are only intended to convey a very rough notion of the relation. They cannot of course be considered as even approximately correct, owing to the great uncertainty as to the mass of molecules.

Having seen that *temperature* is a quantity of the same dimensions as *energy*, and knowing that the same is also true of *heat*, it follows that *entropy*, whose dimensions are *heat* ÷ *temperature*, is a purely numerical quantity ; and the unit of entropy is therefore independent of all other physical units. In fact, *the entropy of a perfect gas increases by unity, when (without altering in temperature) it receives by conduction a quantity of heat equal to the mean energy of one of its molecules.* This is seen by putting

$$\frac{\Delta H}{\Theta} = 1 ; \quad \therefore \Delta H = \Theta = E,$$

where Θ is the absolute (C.G.S.) temperature.

XI. *Researches on Spectrum Analysis.*

By Prof. A. F. SUNDELL*.

CERTAIN natural phenomena, such as the aurora borealis, zodiacal light, and solar corona, have occasioned numerous attempts to obtain the spectra of gases in a highly rarefied condition and at a low temperature. Under these conditions spectra generally become very feeble, and therefore difficult to observe. The following experiments show that tolerably bright spectra may be obtained by an advantageous employment of means already known.

Since the brightness of the spectrum depends, in the first place, upon the thickness of the radiating layer, "end-on" tubes† are employed by preference. I have employed tubes as long as possible (up to $1\frac{1}{2}$ metre long). The end towards the spectroscope was simply melted together, and rounded as well as might be in the process. The other end was drawn out and melted on to the tubes leading to the mercury-pump.

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* Translated from a separate impression from the *Acta Societatis Scientiarum Fennicæ*, vol. xv., communicated by the Author.

† Such tubes have been employed by Prof. P. Smyth for observations on gaseous spectra *in vacuo* (*Beibl.* vii. 1883, p. 286).

I have not employed electrodes fused into the tubes themselves. As a rule, the tubes have been rendered luminous by means of tinfoil coatings near the ends of the tubes, as employed by Salet, Hasselberg, and others. By the adoption of this method the process of discharge no doubt becomes most nearly like that of Nature, and the temperature is kept as low as possible. The source of electricity was a Holtz machine without Leyden jars, the terminals of which were connected by means of wires with the coating of the tube. The coating thus corresponded to the external coatings of the jars of the machine, the strata of air against the inner wall of the tube serving as the inner coating.

I have usually employed a spectroscope constructed by Wrede in Stockholm, belonging to the Central Meteorological Institute of Helsingfors, which was used by Prof. Lemström* in his observations on the aurora in the years 1871–73. The spectroscope consists of one dispersing-prism and one reflecting-prism. The dispersion is somewhat small, not being sufficient to divide the sodium-lines. Ten divisions on the head of the micrometer-screw correspond to intervals of 0·000006 and 0·0000006 millim. in the extreme red and extreme violet respectively. The scale was constructed as follows:—A thin glass plate (a microscopic cover-glass) was covered on one side with indian-ink and five fine parallel lines ruled through the black coating. The other surface was coated with Balmain's luminous paint. This plate was then fixed in the eyepiece of the spectroscope, so that the fine lines were parallel to the lines of the spectrum and in focus at the same time and occupied about half of the field of view†. It was rendered luminous by burning a match before the eyepiece. If a bright line in the spectrum was to be measured, this was done just before the experiment; but for fainter lines the measurement was made after the phosphorescence had become feebler; and with very faint lines it is necessary that the index-lines should only be very slightly luminous, so that they may not overpower the lines of the spectrum. In such cases the adjustment was much easier, because there were five index-

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* This spectroscope is fully described in the *Öfversigt af Finska Vetenskaps-Societetens förhandlingar*, xv. pp. 21–23, 1873. Dr. Fuchs has described a reflecting direct-vision prism as new in the *Zeitschrift für Instrumentenkunde*, 1881, p. 352; this prism is the most remarkable feature of the Wrede spectroscope, and was described to the Royal Swedish Academy of Sciences so long ago as 1870.

† I have described an index of this sort in the *Astr. Nachrichten*, No. 2430 (1882). I have since learned that Prof. Vogel had employed phosphorescent marks in 1881 for the measurement of spectra of feeble luminosity (*Zeitschrift für Instrumentenkunde*, 1881, p. 20).

lines instead of only one. Generally the spectrum-lines were brought into coincidence with the central index-line by turning the micrometer-screw; only for three lines in the extreme violet the outer index-line was used, because the screw-thread ceased here. The wave-lengths were determined by means of a table which I had constructed from a curve of wave-lengths belonging to the spectroscope, from the difference between the position of a line and that of the sodium-line. By way of control, the lithium-line 5706 and the strontium-lines 4606 and 4305 were read in each series of measurements, the corrections for the spectrum-lines measured being determined by simple interpolation.

The larger spectroscope, belonging to the Physical Laboratory of the University, which I have employed for some measurements, is also constructed according to Wrede's principle. It has two prisms of heavy flint glass, and reflecting crown-glass prisms. The index was constructed by H. Biese, who had published the idea of a self-luminous index at about the same time as myself[*]. The index consists of a fine slit in a thin brass plate; the side of the slit turned towards the prisms is filled with a phosphorescent substance. The dispersion is considerable; ten divisions of the micrometer correspond to a change of 0·0000013 in the extreme red and 0·0000002 millim. in the extreme violet; the sodium-lines D_1 and D_2 are divided. The tables for the wave-lengths were calculated from a curve drawn by H. Biese.

Most of the observations were made in a dark room in the Physical Laboratory of the Polytechnic Institute of Helsingfors. I take the opportunity of expressing my thanks both to the Director of the Institute, H. Qvist, and to the Professor of Physics, Dr. Slotte, both for the use of the room so specially suited for these experiments, and also for the use of various apparatus necessary for my experiments from the collection of instruments belonging to the Laboratory. The electrical machine was stationed in a neighbouring room, where it was kept in action by means of a water-motor. For the rarefaction of the gas in the spectral tube, I employed the mercury-pump which I had constructed for the Polytechnic Institute[†]. The movable tube had neither tap nor stopper, and the globe with phosphoric anhydride was melted on. The spectral tube employed was 155 centim. long, and at one end had an internal diameter of 10·8 millim. and glass 2·1 millim. thick; at the other end the tube was 12·9 millim.

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[*] *Öfversigt af Finska Vetenskaps-Societetens förhandlingar*, xxiv. p. 30.

[†] *Acta Societatis Scientiarum Fennicæ*, vol. xv. p. 169.

wide, and the glass 1·6 millim. thick. The tube rested in a horizontal position on glass supports, depending from wooden consoles attached to the wall of the room. The connexion with the air-pump was made by means of a Kundt's glass spiral. At the same time a spectral tube was put up in the same way in the Physical Laboratory of the University, by the kind permission of Prof. Lemström.

In the tube in the Polytechnic Institute the following gases were successively examined :—air, hydrogen, oxygen, air, nitrogen, hydrogen, air, oxygen. The apparatus was unaltered all the time; the new gas was introduced, in the manner peculiar to my pump*, after the former gas had been removed as completely as possible. I give here only the results for air, regarding the results obtained with the other gases merely as preliminary, since I was not able to work with pure gases. I therefore reserve the complete examination of these gases for a future occasion, and give here the results already obtained with them only briefly.

I wish to express my thanks to Herr G. Melander for the help he gave me during the whole investigation.

The conducting-globes had a diameter of 27·8 millim.; the sparks were generally 5 millim. long. It appeared that a great length of spark might be injurious to the tube. On one occasion a tube of thin glass broke whilst a discharge passed between the tinfoil coating and the layer of air clinging to the inner wall of the tube. I have further remarked that of the capillary canals, which always exist in the walls of the tube parallel to its length, those which lie nearest the inside easily burst and discharge the air which they contain into the tube, when the discharges are powerful. It therefore often happens that the pressure in the tube suddenly increases very much, and the luminosity becomes very vivid, if, after having produced a great exhaustion, the air has been driven from the inner wall of the tube by electric discharges. In one case I was able, after such an occurrence, to detect the small holes in the capillary channel which had burst, and the little splinters of glass.

I have made the following observations with regard to the mercury-lines which are to be expected when the mercurial pump is employed. In tubes containing *pure* air, nitrogen, or oxygen, the mercury-lines appear only at high exhaustions. Only the strongest line 546 appears in tubes containing air; but in nitrogen and oxygen tubes also the lines 579 (double), 492, and 436. In hydrogen tubes, and in tubes with impure air (containing carbon dioxide), these lines appear already at

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* *Loc. cit.* p. 178.

considerable pressure, together with the hydrogen and air lines; in hydrogen tubes the lines 408, 405, and (at high exhaustion) 483* feebly, were also seen.

Under the circumstances named, the air began to be luminous at a pressure of 10–12 millim.†; the end with the positive coating became luminous somewhat sooner (at 14 millim.) than the whole tube. It was, however, possible to produce a feeble luminosity of the tube at a considerably higher pressure (about 50 millim.) by pushing a conductor along the tube in contact with the wall until a spark passed between the conductor and the one coating. An unexpected phenomenon was observed at a pressure of about 8 millim., inasmuch as the light appeared to be stratified in a peculiar manner. As I propose to examine this stratification more particularly, I describe it here only briefly. The first quarter from the positive coating was strongly luminous, the intensity diminishing slightly towards the end of this portion. The second quarter also began with a strong luminosity, which became somewhat weaker towards the middle of the tube. The third quarter was distinctly stratified; in one case there were only three or four luminous stratifications, which oscillated tolerably rapidly and appeared like luminous balls; in another experiment I observed a number of thinner layers. Only towards the beginning of the fourth quarter was there a thicker layer: all the stratifications oscillated rapidly‡. The last quarter next the negative coating was always strongly luminous without stratification§. The narrow spiral glass tube which made connexion with the pump was also generally strongly luminous, whether the coating at this end was negative or positive.

The spectroscope showed a large number of bands. With narrow bands the reading was taken for the middle; with broader bands for both edges. The following wave-lengths are the arithmetic means of various measurements within the limits of pressure (0·2 to 1·2 millim.) between which the

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* E. Wiedemann has investigated the spectroscopic behaviour of mercury-vapour mixed with other gases at high temperatures, Wied. *Ann.* v. p. 547 (1878). Compare also H. W. Vogel, *Berlin. Monatsberichte*, 1879, p. 586.

† All measurements of pressure are given in columns of mercury at about 20° C.; for details of the method of measuring pressure, I refer to the paper already cited on the air-pump (*Acta Soc. Scient. Fennicæ*, xv. p. 169).

‡ Similar balls of light (*Glimmlichtkugeln*) have been observed under certain conditions by Reitlinger and v. Urbanitzky in short and wide tubes (Wied. *Ann.* xiii. p. 673).

§ I have observed a similar stratification in a hydrogen-tube.

spectrum was the brightest. The observations made with the large spectroscope are noted. The intensity, estimated for the spectrum as seen in the small spectroscope, is given on a scale from 1 to 5. The minus sign after a number indicates that the intensity falls short of that number, and the plus sign that it exceeds it; so that 1+ means an intensity between 1 and 2 but nearer 1 than 2, whilst 2− means intensity nearer 2 than 1.

No.	Wave-length.	Intensity.	Remarks.
1.	0,0006778	1−	
2.	6680	1	Nos. 2–17 observed in the large spectroscope; Nos. 2–10 with a wider slit.
3.	6600	1+	
4.	6527	1	
5.	6445	1	
6.	6373	1	
7.	6294	1	
8.	6229	1−	
9.	6164	1−	
10.	6111	1−	
11.	6052	2−	Breadth 31×10^{-7}.
12.	5996	1+	„ 38.
13.	5934	1	„ 36.
14.	5881	1−	„ 33.
15.	5833	1−	„ 33 (double?).
16.	5784	1−	„ 30 (double?).
17.	5734	1	
18.	5665	1−−	Very weak.
19.	5621	1−	
20.	5382	……	Sharp edge of a band, beginning about 557°, very weak, and gradually becoming stronger.
21.	5304	1−−	Very weak.
22.	5228	2−	In the large spectroscope four lines: 5224, 5195, 5179, 5145.
23.	5164	1+	
24.	5067 4996	2	In the large spectroscope 5060, 5029, 4983.
25.	4910	2	
26.	4813	2	In the large spectroscope 4832, 4812.
27.	4700	3+	„ „ 4715, 4707.
28.	4659	2+	„ „ 4663, 4647.
29.	4574	3	
30.	4491	2+	In the large spectroscope 4500, 4488.
31.	4415	1	
32.	4341	3	
33.	4293 4275	5	In the large spectroscope 4278, 4269.
34.	4209	1	
35.	4152	1	In the large spectroscope 4140.
36.	4074	2+	„ „ 4057.
37.	4020	2−	„ „ 3995.
38.	3910	1−	Broad band.

The spectrum underwent no change if the connexions of the coatings with the conductors were reversed. I have observed the following changes with change of pressure. With a tolerably wide slit the spectrum could be measured at a pressure of 12 millim. Feeble continuous light began at 557. The bands 22 and 23 appeared as a continuous band of mean wave-length 5192. Further, 24, 25, 26, 27, and 28 appeared united into one band, which was also the case with 29, 30, 32, 33, 34, 36, 37. Nos. 11 and 31 appeared at a pressure of 5·5 millim.; and all the bands became visible at 2·3 millim. When the pressure sank below 0·2 millim. all the bands became decidedly weaker, the feebler bands disappearing first with increasing exhaustion. Thus, at a pressure of 0·02 millim. the bands 1 to 9, 21 to 26, 31, 34, 35, and 38 had disappeared, and the bands 11 to 17 had united to form a continuous glow. At 0·01 millim. there were left only 10, 27, 28, 33; and at 0·0023 millim. only 27, 28, and 33; whilst at 0·0013 millim. the band 28 also vanished. If I pushed the exhaustion still further I could see no air-lines, although the tube was still faintly luminous, the mercury-line 546 being quite distinct. No luminosity was produced by single discharges; at a pressure of 0 0007 millim. the tube only became luminous very seldom, and if the pressure was less than 0·0003 millim. there was generally no light to be perceived. With the same velocity of the induction machine the sparks followed each other much more rapidly than at higher pressures. We may therefore conclude that with high values the tube no longer acts as a conductor, because the rarefied air is no longer present in sufficient quantity.

With a very high exhaustion the walls of the tube fluoresced strongly, especially near the positive coating; and at each discharge this coating emitted a sharp sound, like that of a spark. Some of Crookes's kathode-rays were seen at a pressure of 0·002 millim.

In one case, however, each discharge produced light in the tube, although fully exhausted. The tube had been heated in an air-bath to a temperature of about 250° C. for several hours daily during ten days, in order to disengage the air from the sides of the tube as much as possible. The pressure was reduced on each occasion to a few millionths of a millimetre by continuous pumping. In the intervals while the tube was not heated, the pressure rose from one day to another by about 0·00015 millim. After this preparation I examined the effect of discharges, and found that a tolerably strong luminosity was produced, which was finely stratified throughout the tube. In the spectroscope only the five previously mentioned mercury-lines 579, 546, 492, 483, 436 were seen. Since pure air generally shows only the mercury-line 546 in high vacua, the air in this tube was probably a little contaminated with coal-gas, introduced when the apparatus was last fused together. After the discharge had gone on for some minutes, the pressure was measured and found to be 0·00053. Hence we see that, in spite of the persistent heating, gas still adhered to the wall of the tube and was disengaged by the electricity.

How obstinately air adheres to a glass surface is particularly shown by the tube put up in the Physical Laboratory of the University. This tube had been exhausted from time to time for four months, each time to a few millionths of a millimetre; yet, after some time, there was always a considerable pressure. Thus, on the 8th of January the pressure was 0·0013 millim., on the 14th 0·0011 millim., on the 21st 0·0005 millim., and on the 22nd of February 0·0028 millim.* This tube was provided with electrodes of a special kind. Before the tube was closed, thin aluminium-foil was introduced into the tube, corresponding to the external tinfoil coatings, from which narrow strips projected along the axis of the tube towards the middle, and served as electrodes. I was led to this arrangement, not being able to melt the electrodes into the tube; the action may be considered as nearly the same as that of the ordinary electrodes melted into the glass. This tube, in fact, ceased to be luminous at a pressure of about 0·004 millim. and a spark-length of 5 millim. When the pressure was still smaller (a few millionths of a millimetre), I could not perceive any luminosity, even with the greatest possible spark-length (15 millim.); but the luminosity commenced suddenly upon the bursting of one of the capillary canals mentioned above.

In the hydrogen-tube I obtained a very pure hydrogen spectrum; although the gas had been prepared in the usual way from zinc and sulphuric acid, both free from arsenic, the gas had a strong odour of hydrocarbons. The luminosity began at a pressure of 30 millim. (with gas containing air at 43 millim.). At a pressure of 0·35 millim. the spectrum showed the lines C (=656), F (=486), and 434 (united with the mercury-line 436 to a band?), as well as numerous faint lines (especially in the red and orange) of the second hydrogen spectrum investigated by Hasselberg. The tube was luminous at the highest vacuum attainable, at least at the positive coating; the light failed only in individual discharges. Crookes's rays were seen even at a pressure of 0·008 millim.

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* I have not observed the phenomenon mentioned by Bessel-Hagen (Wied. *Ann.* xii. p. 440, 1881), that the air liberated from the walls of the tube by electric discharges condenses again, so that after some time the pressure decreases again.

Only in one case (with hydrogen containing air) have I observed fine stratifications in a high vacuum.

If the tube contained oxygen (prepared from potassium chlorate) the luminosity began at about 30 millim.; the spectrum was brightest at about 0·2 millim. The two bands 563–556 and 529–523 were specially conspicuous, and these two could be recognized with greater exhaustion when the tube was only faintly luminous. At high vacua the light failed oftener than with the hydrogen-tube.

With nitrogen I obtained in the small spectroscope the same spectrum as with air.

Helsingfors, May 26, 1885.

XII. *On the Electrostatic Force required to produce Sparks in Air and other Gases. By* G. A. Liebig, *Ph.D., Johns Hopkins University*[*].

[Plate II.]

THE following experiments were undertaken at the suggestion of Prof. Rowland, and the results submitted from time to time to his inspection.

Historical.

Determinations of the difference of potential required to produce sparks in air appear to be numerous. But the earlier measurements, such as those made by Volta[†], Riess[‡], and Gaugain[§], in which the difference of potential was determined in arbitrary units by means of electroscopes, unit jars, &c., have little more than a historic value. It was, however, noticed by these observers that the length of spark increased more rapidly than the charge; and Van Oettingen[‖] deduced an empirical formula,

$$q = c \log (1 + \epsilon l),$$

in which c and ϵ are constants, l is the length of spark, and q the charge, to express this relation. The first experiments on the subject, giving results in absolute measure, were made by Sir William Thomson[¶]. In his investigation the sparks were made to pass between two plates, one of which was flat, the other slightly convex; and the difference of potential

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[*] Communicated by the Author.

[†] *Identita*, p. 53.

[‡] Pogg. *Ann.* xl. p. 333 (1837).

[§] *Ann. de Chim. et de Phys.* viii. p. 108 (1866).

[‖] Pogg. *Ann.* Jubelbd., p. 275 (1874).

[¶] 'Electrostatics and Magnetism.'

determined by means of one of his recently constructed Absolute Electrometers. It was found that the electrostatic force per unit length diminished at first rapidly, with increasing difference of potential, then slowly, and approached a limit the value of which was supposed by Thomson to be about 130 C.G.S. electrostatic units per centimetre.

In 1882 Baille[*] published an account of some experiments in which sparks nearly 1 centim. long were passed between two plates. His results are expressed in the formula

$$V^2 = 10500\,(l + 0{\cdot}08)\,l \text{ C.G.S. electrostatic units,}$$

provided l is not greater than 1 centim.

Similar expressions have been given by Macfarlane[†]; namely, for sparks less than 1 centim. long he deduces from his observations,

$$V = 66{\cdot}940\sqrt{l^2 + 0{\cdot}20503\,l} \text{ C.G.S. electrostatic units.}$$

The agreement between these expressions is not satisfactory; they give, for instance, for a spark 0·1 centim. long, values which differ by about 10 per cent.

In gases other than air very much less has been done; indeed no direct observations appear to have been made. Macfarlane[‡] compared the difference of potential necessary to give a 0·5 centim. spark in air with the difference required to give a spark of same length in several other gases. Calling this difference unity for air, he finds for

Carbon dioxide.	Nitrogen.	Hydrogen.	Coal-gas.
·951	·930	·634	·935

From which it appears that hydrogen offers a much smaller resistance to the disruptive discharge than any other one of the gases experimented upon.

Instruments.

The electrometer used in the present work was a very fine instrument, made from designs of Prof. Rowland by T. Edelmann, of München, on the principle of Sir Wm. Thomson's Absolute Electrometer. But it was much larger than, and differed in other and important respects from, those generally in use. One feature was the attachment of the movable disk to the end of the arm of a balance, from the other end of which depended, on knife-edges, a regular balance-pan. This whole system was inclosed in a metallic case, through a hinged

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[*] *Ann. de Chim. et de Phys.* xxv. p. 486.

[†] Trans. Roy. Soc. Edinb. xxviii. p. 636 (1877).

[‡] Phil. Mag. [5] x. p. 389 (1880).

door in the side of which weights could be placed upon the scale-pan without disturbing in any way the movable disk. The guard-ring and lower plate of the electrometer measured nearly 35 centim. in diameter, whilst the movable disk had a diameter of almost exactly 10 centim. All of these plates were ground with great accuracy to plane surfaces. The lower movable plate was insulated from the rest of the apparatus by means of its support, which was a vulcanite rod sufficiently thick and strong to keep the plate quite rigid. It ended with a metal stem on which was engraved a millimetre-scale (from 0 to 80 millim.), and fitted into a metal socket provided with a vernier divided to 10ths of a millim., and a tangent-screw and clamp. Rough adjustments were made by releasing the clamp and moving the plate by its vulcanite support. It was then clamped and set to within 1-10th millim. by the tangent-screw, and readings made by estimation to 1-20th millim. The plate with its attachments was prevented from falling, when released from its socket, by a counterpoise at the end of a lever. The plates between which the sparks were made to pass were of brass, and were ground to spherical segments. Their radius of curvature was 9·76 centim., and diameter 4·83 centim. These plates were firmly attached by insulating handles to the frame and carriage, respectively, of a dividing-engine, with which their distance apart was measured.

During the experiments on coal-gas, hydrogen, &c., the plates were enclosed in an air-tight cylindrical vessel made of glass and wood, with a capacity of about 400 cubic inches, and provided with stuffing-boxes at each end, through which the rods holding the plates passed. The gases, which were dried over calcium chloride, were admitted into the chamber in a steady stream by upward or downward displacement, according to density. The gases were always at ordinary temperatures (18° to 20° C.) and pressures. In the case of CO_2, coal-gas, and hydrogen, the pressures in the vessel were just sufficient to force the gases through in an almost constant stream. After every series of experiments the plates were polished and all traces made by the sparks obliterated. This could always be accomplished by a few minutes' rubbing on a concave surface (which had been used originally to grind the plates with) covered with paper and dry rouge. The electricity was furnished by a small Holtz machine, and the wires leading from it to the electrometer and a single Leyden jar (always kept in the circuit) were covered with glass tubes to prevent leakage as far as possible.

Adjustments and Corrections.

The balance of the electrometer was carefully adjusted to equilibrium before beginning the experiments. The guard-ring and movable disk were made parallel to the lower plane by means of a piece of plane parallel glass, which was moved from place to place; the lower plate being raised or lowered by three set-screws, until the glass just touched both surfaces in its different positions. This adjustment, which served also to give the zero-reading of the electrometer, was made several times in order to avoid constant errors.

A correction to the distance between the plates, which is rendered necessary when the guard-ring and suspended disk are not in the same plane, was avoided in this case by so adjusting the latter that in its zero position it was exactly in the plane of the guard-ring, and prevented by a set-screw under the balance-arm from moving further away from the lower plate. The extent of its downward motion under the action of the electrical forces need not be considered; inasmuch as the quantity to be measured, for a given difference of potential, is that distance between the plates at which the suspended disk can *just* be moved. As the equilibrium is unstable, a force which is sufficient to move the disk at all will move it downward as far as the supports permit.

The interval between the guard-ring and disk was 1 millim., and a correction was applied to the area of the latter, as calculated from the formula (Maxwell, 'Electricity and Magnetism,' i. p. 307, footnote)

$$A=\tfrac{1}{2}\pi\left\{R^2+R_1{}^2-(R_1{}^2+R)\frac{\alpha}{D+\alpha}\right\},$$

where R_1-R is the thickness of the annular space between the suspended disk and guard-ring, and

$$\alpha=0{\cdot}220635\,(R_1-R).$$

The weights used in the balance-pan varied from 0·2 to 5 grms., a different weight being employed for every 20 or 25 readings.

To find the difference of potential or the electrostatic force from observations on an absolute electrometer, we have only to apply the formula

$$V_1-V=D\sqrt{\frac{8\pi g w}{A}}.$$

V_1-V is the difference of potential to be measured; D is the distance between the plates of the electrometer; A is the area (corrected) of the suspended disk; *w* the weight in the balance; and g the force of gravity at the place in which the observations are made.

In order to determine whether the method of finding when the plates were in contact, by observing when the sparks just disappeared, was an accurate one, the plates were connected with a battery and galvanometer, and alternate readings taken. As a result, it was found that the former method gave, in general, values about $\frac{1}{500}$ millim. too small, though they were occasionally too large; but in any case the difference was small enough to be neglected.

As the plates between which the sparks passed were spherical, a correction to the measured difference of potential was rendered necessary in order to reduce the values to plane surfaces.

In the preliminary experiments a considerable difficulty was encountered in measuring the difference of potential for sparks of small length when the plates were close together. Unless the machine was run very slowly, the sparks were given off in such rapid succession that the suspended disk had not time to swing back to zero before a new impulse acting on it drew it downward again. Even the damping effect of that part of the metal case which is above it cannot prevent this; and a reading taken under such circumstances would unquestionably give values very much too large. However, by turning the Holtz machine very slowly indeed, and by increased experience, errors of this kind were finally avoided.

Results.

The results of the experiments are given in Tables I., II., III., and IV., and the curves corresponding to them on Plate II. In each diagram one curve gives the distance and total electromotive force, and one the relation between distance or length of spark and electromotive force per centimetre. The values are in all cases in C.G.S. electrostatic units; and about 150 observations were used to plot each of the curves. In order to show the form of the curve for the total electromotive force more plainly, it was found desirable to construct this on a scale three times as large as that used for the force per unit length. The ordinates are therefore to be measured according to one of two scales, both of which are constructed on the Plate.

TABLE I.—Air.

Length of spark, in centimetres.	Electro-static force, per centim. C.G.S.	Total force, C.G.S.	Length of spark, in centimetres.	Electro-static force, per centim. C.G.S.	Total force, C.G.S.
·0066	398·5	2·630	·2398	127·7	30·622
·0105	319·7	3·357	·2800	125·7	35·196
·0143	280·9	4·017	·3245	122·7	39·816
·0194	235·7	4·573	·3920	119·9	47·001
·0245	206·4	5·057	·4715	117·0	55·165
·0348	206·6	7·190	·5588	114·0	63·703
·0438	195·5	8·863	·6226	112·4	69·980
·0604	179·9	10·866	·7405	111·0	82·195
·0841	161·1	13·548	·8830	108·2	95·540
·0903	153·0	13·816	·9576	107·0	102·463
·1000	150·0	15·000	1·0672	103·8	110·775
·1520	137·8	20·946	1·1440	102·7	117·489
·1860	133·2	24·775			

TABLE II.—Coal-gas.

Length of spark, in centimetres.	Electro-static force, per centim. C.G.S.	Total force, in C.G.S. units.	Length of spark, in centimetres.	Electro-static force, per centim. C.G.S.	Total force, in C.G.S. units.
·0060	291·6	1·750	·3439	117·7	40·477
·0144	235·0	3·384	·3601	117·0	42·132
·0255	212·6	5·421	·4268	112·7	48·100
·0340	205·5	6·885	·4708	110·4	51·976
·0451	178·1	8·032	·5430	107·9	58·590
·0546	177·9	9·713	·5586	107·6	60·105
·0679	166·5	11·305	·6325	107·0	67·677
·0863	156·6	13·515	·7450	103·9	77·405
·0958	150·9	14·456	·8410	100·6	84·604
·1240	143·0	17·732	·8760	99·4	87·074
·1590	130·9	20·813	·9791	97·0	94·973
·1953	127·1	25·799	1·0676	94·9	101·315
·2244	128·4	28·813	1·1858	92·4	109·686
·2840	122·9	34·904			

TABLE III.—Carbon Dioxide.

Length of spark, in centimetres.	Electro-static force, per centim. C.S.G.	Total force, C.G.S.	Length of spark, in centimetres.	Electro-static force, per centim. C.G.S.	Total force, C.G.S.
·0047	344·5	1·619	·2380	141·6	33·701
·0092	305·7	2·812	·2688	138·0	37·094
·0155	283·7	4·397	·3257	129·1	42·048
·0237	268·8	6·371	·3922	123·0	48·241
·0282	252·0	7·106	·4290	120·0	51·480
·0327	233·3	7·629	·4911	116·0	56·968
·0391	224·3	9·357	·5274	112·6	59·385
·0540	210·0	11·340	·6170	109·3	67·438
·0853	182·0	15·542	·7330	107·2	78·577
·0940	179·0	16·826	·8132	104·0	84·573
·1209	174·9	21·145	·8752	102·5	89·708
·1430	164·2	23·481	·9381	102·2	95·874
·1728	157·0	27·130	1·0275	101·3	104·086

TABLE IV.—Hydrogen.

Length of spark, in centimetres.	Electro-static force, per centim. C.G.S.	Electro-static force, total C.G.S.	Length of spark, in centimetres.	Electro-static force, per centim. C.G.S.	Electro-static force, total C.G.S.
·0210	244·4	5·132	·3898	84·6	32·977
·0409	184·6	7·550	·4335	82·1	35·590
·0542	156·4	8·477	·4975	77·8	38·705
·0691	144·7	9·999	·5296	75·7	40·091
·0843	134·0	11·296	·5640	75·1	42·356
·0970	131·5	12·755	·6400	72·5	46·400
·1056	127·4	13·453	·7240	69·0	49·956
·1275	120·0	15·300	·7930	67·7	53·686
·1552	112·9	17·522	·8238	66·9	55·112
·1972	104·0	20·509	·8896	65·6	58·358
·2300	99·3	22·839	·9750	63·9	62·302
·2865	94·1	26·960	1·0109	63·0	63·687
·3215	92·5	29·739	1·0870	62·5	67·937

On examining the curves, it will be remarked that, although the values of the electrostatic force differ for different gases, the general form of the curves which give the relation between the potential and length of spark is nearly the same for all. In every case there seems to be a sudden fall in the electrostatic force per centimetre at first, after which the change becomes more and more gradual and uniform. It further appears probable that the value given by Sir William Thomson as the limit to which the force per unit length approaches is somewhat too large. Indeed, it seems impossible to determine exactly what the limit is. For when the plates are separated by a considerable interval, even when they are spherical segments, the sparks have a tendency to leave at points removed from the centre, which of course makes their length indeterminate; and if they are given a smaller radius of curvature, the correction becomes too large to be used with safety.

It is intended to continue the investigation with plates of different metals. Some later experiments, in which nickel-plated brass plates were used, seem to indicate a smaller value of the electrostatic force, the difference being more marked at small than at great distances. It was noticed that with brass plates the sparks limned out a considerable area around the central point, whereas when the plates were nickel-plated the effects were greatly diminished. This would give rise to greater surface-inequalities in one case than in the other; and although the plates were frequently polished, might perhaps offer an explanation of the fact just referred to.

However, the observations are too few in number to furnish reliable information; and at present (May) the warm weather prevents a continuance of the work by interfering with insulation.

Johns Hopkins University,
Baltimore, June 2, 1887.

XIII. *On the Law of Molecular Force.*
By William Sutherland, *M.A., B.Sc.*[*]

THE object of the present paper is to show that the law of molecular force, which I previously announced (Phil. Mag. Aug. 1886) as holding for matter in the gaseous state, holds also for matter in the liquid state, and therefore, we may be sure, for matter in any state; and that accordingly it is the one fundamental law of the action of molecule on molecule at molecular distances. This general law of molecular force is :—Any two molecules of matter attract one another with a force proportional directly to the product of their masses, and inversely to the fourth power of the distance between them.

In my previous paper I confined myself to showing that with such a law of force the potential energy of the molecules of a body would be proportional inversely to the volume occupied by them, and that this conclusion agreed with the results of Thomson and Joule's experiments on the expansion of gases through porous plugs. Now, of course, since Laplace gave his great theory of capillary action to the world, it has been an obvious result of the general conditions which he postu-

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[*] Communicated by the Author.

lates for the unknown law of molecular force in order that it may account for capillary action, that the potential energy of a number of molecules must be inversely proportional to the volume occupied by them; but my attention was first drawn to the form A/r^4 as that expressing the law of molecular force by the fact that it is the only particular case of the general form A/r^m (m having any value fractional or integral) which satisfies Laplace's conditions when they are established on a satisfactory physical basis. The fact (which I will afterwards show) that Laplace's conditions tacitly exclude the possibility of expressing the law of molecular force by a finite series of inverse powers of r fractional or integral, unless the series reduces to A/r^4, gives the law of the inverse fourth power a special claim to investigation, and also calls for an inquiry into the physical soundness of the conditions; for while to the mathematician the conception of a function of r, which increases rapidly as r diminishes, but which cannot be expressed as a finite series of inverse powers of r, is easy enough, the physicist, who naturally looks on the law of gravitation as a first approximation to the complete law of the action of matter on matter (for which I would propose the name *molic* force, including gravitation, molecular and chemic force), must find the exclusion of a law expressible by a series of inverse powers of r highly arbitrary, and suggestive rather of the domination of nature by formulæ, than of her expression, which is the object of the physicist.

Laplace's expression for the resultant attraction of a mass of liquid of density ρ with a plane surface on a column of the same liquid of section ω, terminating at the plane surface and normal to it, is

$$2\pi\rho^2\omega\int_0^\infty dz\int_0^\infty ldl\int_l^\infty (r)dr,$$

$mm'f(r)$ representing the attraction between two molecules of masses m and m'. The only express stipulation made at first about r is that its value is to be insensible at sensible distances. To remove the vagueness inherent in this general expression of this condition, Gauss (*Werke*, V., Principia generalia theoriæ figuræ fluidorum in statu equilibrii) casts it into the following definite form:—

Let M be a mass such as we have to deal with in ordinary experiments, and which is therefore negligible in comparison with the mass of the earth, then $Mf(r)$ being of the same dimensions as g is comparable with it, and the definite expression of the above condition is that $Mf(r)$ is to be negligible in comparison with g when r has a sensible value, but that it may have a very large value in comparison with g for insensible values of r. Even yet there is a vagueness in the use of the terms sensible and insensible, the only method of removing which is to stipulate that the law of force is to be such that if the actions between molecules in an experimental body farther apart than a certain distance L are taken account of, they will contribute to the quantity whose amount is to be investigated a portion negligible in comparison with the portion contributed by the actions between molecules at a distance apart less than L.

But although this is the only stipulation to begin with, the above triple integral involves important tacit assumptions. The use of infinite upper limits in an expression to be applied to finite bodies involves nothing really objectionable; they merely express loosely in a symbolical form the above condition, that actions at sensible distances are insensible; but when we come to examine the meaning of the lower limit 0 in the last integration, it is quite otherwise; two fundamentally important assumptions are involved. It must be remembered that the triple integral is intended to replace a double sum $\Sigma_1^n \Sigma_1^N m\mathrm{M}f(r) \cos\theta$, which is the true expression for the resultant attraction of the liquid mass on the column, m being the mass of any molecule of the column, M of any molecule of the liquid mass, r the distance between m and M, and θ the angle which r makes with the axis of the column, n the number of molecules in the column, and N the number in the liquid mass. Comparing the sum with the integral, we see that the assumption is made that the discontinuous molecules may be imagined to be spread out into continuous matter, and that the base of the continuous column may be considered to be in actual contact with the continuous liquid mass, no matter what was the original distance of the bottom layer of molecules of the column from the top layer of the liquid mass. We might allow the same latitude to the meaning of 0 as we have to ∞ and take it as meaning some small limit; but we are not entitled to assume that this small limit is fixed and independent of the original distance that separates a molecule and its neighbours before the imaginary continuous distribution was made. Thus, while the adoption of a fixed upper limit in the integrations, independent of the distance between a molecule and its neighbours, is only in strict accordance with the first express stipulation, the fixing of the lower limit of the last integration as independent of that distance is an additional condition of the first importance. Of course, within such range of values as experiments permit, the integral may be independent of variations in the true small lower limit; but the fact must not be lost sight of that, when we assume this to be the case, we are tacitly stipulating for another special property for the unknown function f.

Let us obtain general expressions for the potential energy which the molecules in unit mass of a single chemical substance possess by virtue of their mutual attractions, and also for the virial of these attractions. Let $mm'\phi(r)$ be the potential energy of two molecules, then $d\phi(r)=-f(r)dr$. For the potential energy of the molecules in unit mass and for the virial of their attractions, we shall have the expressions $\frac{1}{2}\Sigma\Sigma m^2\phi(r)$ and $\frac{1}{2}\cdot\frac{1}{2}\Sigma\Sigma m^2 rf(r)$, the double Σ relating to the two distinct summations that have to be performed, and the $\frac{1}{2}$ being introduced as a factor to neutralize the effect of counting twice each pair of molecules as acting on one another, in the process of summation.

To obtain values for these sums in the form of integrals we have to imagine the matter of the molecules as uniformly spread throughout the space in which they are distributed. If there are n molecules in a space v, then $\frac{v}{n}$ is the volume of a space which we will call the domain of a molecule, to distinguish it from the spatial extension of the molecule, and from what is often called its sphere of action. To find the boundaries of a domain, let us imagine at any moment the distribution of the molecules to be uniform, and that the matter of each expands out in the form of a sphere, the rate of expansion being the same for all. Let them finally press upon one another like plastic expanding bodies, until no portion of the space is unoccupied by matter; then the whole space is divided into n polyhedra, of which each is the domain of a molecule. Imagine the matter of one polyhedron gathered into its centre, then, by drawing matter uniformly from the continuous mass, and building it up round the faces of the polyhedron so as to form a spherical cavity, or, if necessary, by cutting matter away from the faces of the polyhedron and distributing it uniformly throughout the continuous mass, we can finally, with only an infinitesimal alteration of the density of the continuous mass, form a continuous mass round any molecule gathered into the centre of its domain; so that the potential energy of the molecule and of the continuous mass so formed is the same as that of the molecule and all the other original molecules. The radius a of the spherical domain thus defined is then the lower limit to be used in the desired integrals. The upper limit is the length L already defined.

With a molecule m as centre describe a spherical shell of radius r and thickness dr; let ρ be the density of the continuously distributed matter, which, of course, is the same as that of the original molecular distribution, then the potential energy of m and the shell is

$$4\pi m\rho r^2\phi(r)\,dr,$$

and the potential energy of the whole continuous mass and the molecule m is

$$\int_a^{\mathrm{L}} 4\pi m\rho r^2\phi(r)\,dr,$$

neglecting, in accordance with the definition of L, the action on m of all molecules beyond the surface of the sphere of radius L.

For the virial of the forces acting between m and all the other molecules, we have

$$\tfrac{1}{2}\int_a^{\mathrm{L}} 4\pi\rho r^3 f(r)\,dr.$$

Summing these two expressions for the n molecules and dividing each by 2, we get results which do more than represent the sums $\frac{1}{2}\Sigma\Sigma m^2\phi(r)$ and $\frac{1}{2}\,.\,\frac{1}{2}\Sigma\Sigma m^2 rf(r)$, as each includes the action of the n molecules on a layer of molecules of thickness L added over the whole surface S. If, then, v is the volume occupied by the n molecules, the number of molecules in the layer is $\frac{\mathrm{LS}}{v}\,.\,n$. If, then, we imagine a layer of thickness L removed from the surface of the actual body, and sum each of the previous integrals for the $n\left(1-\frac{\mathrm{LS}}{v}\right)$ molecules remaining, we shall obtain two sums too small to represent the desired sums, seeing that the mutual actions of the molecules in the layer removed will not be taken into account. Thus, the whole potential energy of the molecules lies between

$$2\pi mn\rho\int_a^{\mathrm{L}} r^2\phi(r)\,dr,$$

and

$$2\pi n\left(1-\frac{\mathrm{LS}}{v}\right)m\rho\int_a^{\mathrm{L}} r^2\phi(r)\,dr,$$

and may be represented by

$$2\pi n\left(1-\theta\frac{\mathrm{LS}}{v}\right)m\rho\int_a^{\mathrm{L}} r^2\phi(r)\,dr,$$

where θ is a proper fraction.

The term involving S plays an important part in the theory of capillary action; but for our present purpose we may neglect it in comparison with the other term, if we make the assumption usual in capillary theory that the smallest value which it is permissible to take for L is negligible in comparison with the dimensions of experimental bodies.

Noticing that nm is the mass of the whole body, which we assume to be unity, we get for the potential energy of the n molecules,

$$2\pi\rho\int_a^{\mathrm{L}} r^2\phi(r)dr,$$

and for the virial of their forces,

$$\pi\rho\int_a^{\mathrm{L}} r^3 f(r)dr.$$

Now if (as Laplace does in his expression for the attraction of a fluid on a column) we were to replace a and L in the two integrals by 0 and ∞, we should make the two integrals constants, and we should have the potential energy of the molecules of a body and the virial of their attractions both proportional directly to its density, or inversely to its volume, whatever the law of force might be, so long as it satisfied the assumed conditions. But the highly artificial character of these conditions becomes evident if we consider how the true lower limit a was obtained, and how it must be a function of the distance between a molecule and its nearest neighbours, and therefore of v; a natural assumption is that a is approximately proportional to $\sqrt[3]{v}$. Now in the treatment usual in capillary theory, the above integrals would be supposed to vanish at their upper limits, so that their values depend entirely on the values at the lower limit; before, then, we can treat the integrals as constants, we have to give $\phi(r)$ and $f(r)$ a certain special property.

It is easy now to see that $\phi(r)$ cannot be of the form

$$\frac{\mathrm{A}}{r^m}+\frac{\mathrm{B}}{r^n}+\dots$$

if it is to possess this property, unless when the series takes the form $\frac{\mathrm{A}}{r^3}$, which corresponds to the law of the inverse fourth power of the distance; for, assuming $\phi(r)$ to be expressible as such a series, and integrating the expression for the potential energy, we get

$$\frac{2\pi\rho\mathrm{A}}{m-3}\left(\frac{1}{a^{m-3}}-\frac{1}{\mathrm{L}^{m-3}}\right)+\frac{2\pi\rho\mathrm{B}}{n-3}\left(\frac{1}{a^{n-3}}-\frac{1}{\mathrm{L}^{n-3}}\right)+\dots$$

It is obvious that we cannot admit any exponent such as m or n to be less than 3, or we should have a part of the expression for the potential energy increasing with the upper limit L, which is opposed to the fundamental hypothesis of the theory of molecular force, assumed in the definition of L. For values of m or n greater than 3, the terms involving L become negligible in comparison with those involving a, and the potential energy is expressible as $2\pi\rho$ multiplied by a series of inverse powers of a; but a is a function of v, so that if the series is finite, the potential energy cannot vary inversely as v.

If, however, the series reduces to the single term $\frac{A}{r^3}$, then the potential energy becomes

$$2\pi\rho A \log\frac{L}{a},$$

which, remembering that $\frac{L}{a}$ is a very large number, we see to be independent of variations of a within the range of present experimental possibilities.

Thus, then, it is established that the only law of force expressible by a finite series of inverse powers of r which is consistent with the variation of molecular potential energy of a body inversely as its volume is the law of the inverse fourth power. The same statement holds as regards variation of the virial of the molecular force inversely as the volume. When

$$\phi(r)=\frac{A}{r^3},\quad f(r)=\frac{3A}{r^4},$$

and the value of the virial becomes

$$3\pi\rho A \log\frac{L}{a}.$$

To show the relation between the general expressions for the potential energy and virial, let us integrate the latter by parts, remembering that

$$f(r)dr=-d(\phi)r,$$

$$\pi\rho\int_a^L r^3 f(r)\,dr=-\pi\rho\int_a^L r^3 d\phi(r)$$

$$=\left[-\pi\rho r^3\phi(r)\right]_a^L+3\pi\rho\int_a^L r^2\phi(r)dr.$$

According to the treatment usual in the theory of capillary action, the first term of the last expression would be shown to vanish, because $\phi(L)$ is negligible; and in the lower limit, when a is replaced by 0, the term $0^3\phi(0)$ is assumed to vanish, as it is stipulated that $\phi(0)$ must be finite. On these assumptions the virial of the molecular forces is found to be $\frac{3}{2}$ of the potential energy of the molecules—the same ratio as follows from the law of the inverse fourth power without assumption.

Thus, then, if we proceed to deduce from experimental data the value of the potential energy of the molecules of a body and the value of the virial of the molecular forces, and prove the latter to be $\frac{3}{2}$ of the former, we shall have obtained weighty confirmation of the Kinetic Theory of Matter and of the idea of the action of molecules on one another as centres of force, but we shall possess no absolute test between the law of the inverse fourth power and any one of the unknown number of unknown laws satisfying the conditions that they can be expressed by some function $f(r)$, insensible for sensible values of r but sensible for insensible values of r, yet incapable of expression as a finite series of inverse powers of r; and, further, that $f(r)$ must be such that $\int_a^{\mathrm{L}} r^3 f(r)dr$ and $\int_a^{\mathrm{L}} r^2 \int_r^{\infty} f(r)\,dr$ are both unaltered by variations in a, while $a^3\int_a^{\infty} f(r)dr$ is negligible in comparison with $\int_a^{\mathrm{L}} r^2 \int_r^{\infty} f(r)dr$; but such an absolute test will hardly be required to distinguish between the nearness to the truth of the simple and natural and of the elaborate and artificial.

In order to find whether the virial of the molecular forces of a body does actually vary inversely as its volume, it is necessary to construct empirical equations for the relations between pressure, volume, and temperature found experimentally for different fluids, and then to compare them with Clausius's fundamental equation of the virial as applied to the Kinetic Theory of Gases,

$$\tfrac{3}{2}pv = \tfrac{1}{2}\Sigma m\mathrm{V}^2 + \tfrac{1}{2}\cdot\tfrac{1}{2}\Sigma\Sigma m^2 r f(r).$$

The double sum in this equation as often written is multiplied by only one factor $\frac{1}{2}$, the other being understood to be contained in the meaning of the symbol of summation.

If, then, the virial of the molecular forces of all bodies is to vary inversely as the volume occupied by them, we must always find in their characteristic equations for pv a term independent of the temperature and varying inversely as the specific volume. Van der Waals, who endeavoured to apply this idea of the construction of characteristic equations on the one type to be studied in the light of the equation of the virial, was led by certain theoretical views to consider that the term, independent of temperature, should have the form $\frac{a(v-b)}{v^2}$. But the equations constructed on this principle were found applicable within only a narrow range of temperature and pressure. Now, before any definite conclusions can be drawn from the forms of the terms in an empirical equation, that equation must be proved to hold over a very wide range of values of the variables in it. A brief comparison which I will give below of the different types of equation that have been constructed for gases, and of the different interpretations that have been given to their forms, will show the necessity for this condition of wide applicability in an equation.

To find whether the virial of the molecular forces of a body varies inversely as its volume, I resolved to seek for empirical equations to represent the splendid series of experimental results that Amagat has published for several gases (*Ann. de Chim. et de Phys.* 5 série, xxii.). In order to be able to compare molecular potential energy and virial, I constructed the characteristic equation for CO_2 first, because Thomson and Joule's experiments on the expansion of CO_2 through porous plugs, and Regnault's experiments on expansion of CO_2, give no data for finding its molecular potential energy. In view of the great importance of the deductions to be drawn from the form of the equation, it seemed to me to be absolutely necessary that it should represent not only the whole range of Amagat's experiments, which extend from pressures of about 40 atmospheres to pressures of 400, and from a temperature of 18° to 100° C., but also such independent experimental data as are available. The search proved to be a sufficiently laborious one.

The form of equation which Rankine adopted to represent the result of Regnault's experiments on the compressibility and dilatation of air and CO_2 may be taken as the point of departure of certain more recent formulæ. It was (Phil. Trans. 1854, p. 336) :—

$$pv = \mathrm{A}(274+t) - \frac{\mathrm{B}}{(274+t)v},$$

where t represents temperature Centigrade. Thomson and Joule, by the direct integration of the differential equation expressing their approximate empirical law, that the fall of temperature of a gas escaping through a plug from a region of one pressure to a region of another is proportional directly to the difference of pressure and inversely to the square of the absolute temperature of the gas, were led to the form

$$pv = A\theta - \frac{Bp}{\theta^2},$$

where θ is temperature on Thomson's absolute thermodynamic scale, given within ordinary experimental limits by the equation

$$\theta = 273{\cdot}7 + t.$$

If, in the small second term, we consider that $p \propto \frac{1}{v}$, the equation takes Rankine's form—

$$pv = A\theta - \frac{B}{\theta v}.$$

If T represents temperature, measured from the absolute zero of an air-thermometer, on which the temperature of melting ice is supposed to be 273°, then within ordinary ranges of temperature we can put $\theta = T + \Delta$, the value of Δ being ·7, as determined by Thomson and Joule ("Thermal Effects of Fluids in Motion," part iv., Phil. Trans. 1862). With θ expressed in terms of T, Thomson and Joule's equation becomes

$$pv = AT + A\Delta - \frac{B}{Tv},$$

where the occurrence of the small constant term $A\Delta$ is of great importance in thermodynamic and molecular theory.

Hirn*, by his thermodynamic ideas, was led to the conclusion that the equation of a body in any state could be obtained by generalizing the equation to a perfect gas; thus,

$$(p + R)(v - \psi) = AT.$$

R is a term resulting from the molecular attractions, and is called by Hirn the internal pressure. He determines it for different substances as a function of the volume, involving two constants and ψ, which he calls the atomic volume, and takes as representing the actual spatial extension of the molecules whose united domains form the volume v.

Athanase Dupré was the first writer, to my knowledge, who endeavoured to show the bearings of Laplace's ideas of molecular forces on the form of characteristic equations and to apply them. Let $\phi(v, t)$ be the potential energy (*travail mécanique interne*) of the molecules of a body whose volume is v at temperature t. Then it follows from Laplace's theory, with the assumptions involved in it, that $\phi(v, t)$ varies

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* "Théorie Mécanique de la Chaleur," *Ann. de Ch. et de Ph.* 4 série, xi.

inversely as v when t is constant; so that we may put

$$\phi(v, t) = \frac{f(t)}{v}.$$

In his "Troisième Mémoire sur la Théorie Mécanique de la Chaleur,"* Dupré obtains an equation which may be written

$$p = \frac{\mathrm{AT}}{v+\psi} - \frac{\partial \phi(v, t)}{\partial v} = \frac{\mathrm{AT}}{v+\psi} + \frac{f(t)}{v^2}.$$

ψ is a constant for each substance, which he calls its covolume. The amount of experimental data in existence did not admit of any adequate testing of this type of equation.

These forms all belong to a period prior to the publication of Clausius's theorem of the virial; and they were constructed and studied almost entirely for thermodynamical purposes. But since the kinetic theory has been strengthened by that theorem, much encouragement has been given to regard the characteristic equation rather from the molecular point of view. Van der Waals, whose work I know only through a *résumé* (Wiedemann's *Beiblätter*, i. p. 10), starting from the equation of the virial, and applying Laplace's theory of molecular force in evaluating the virial, arrives at an equation similar both to Dupré's and to Hirn's; except that the R in Hirn's formula is made to vary inversely as the square of the volume, or the $f(t)$ of Dupré's is reduced to a constant. His method of evaluating the virial of the internal forces is as follows:—

All round the surface of a body we can imagine a layer, of thickness L, isolated, where L is the distance at which the action of two molecules on one another becomes negligible: then if we disregard for the moment the forces which act between the remainder of the body and the layer, and the mutual actions of the molecules of the layer, we see that the virial of the remaining molecular forces vanishes, because for every force acting on a given molecule which has a component X, there is another force whose component in the same direction is $-\mathrm{X}$; hence the only parts of the whole virial

$$-\tfrac{1}{2}\Sigma(\mathrm{X}x + \mathrm{Y}y + \mathrm{Z}z)$$

which it is necessary to take account of are those arising from the actions of the inner molecules on the outside layer and the part arising from the actions amongst themselves of the molecules of the layer. But this last part is negligible in comparison with the virial of all the forces at play in the whole body; so that the virial of all the mutual attractions of the

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* *Ann. de Ch. et de Ph.* 4 série, iv. (1865).

molecules of the body reduces to that part due to action between the layer and the matter it encloses. Now the resultant attraction of the enclosed mass on a portion of the layer whose surface is unity is $K\rho^2$, where K is Laplace's constant and ρ is the density of the body: hence the internal virial will have the same value as if it arose from a pressure $K\rho^2$, which Van der Waals calls the molecular pressure. According to the usual method of estimating the virial pressure over a closed surface, we find that the virial of the internal forces is $\frac{3}{2}K\rho^2 v$, and thus the equation of the virial becomes

$$\tfrac{1}{2}\Sigma m V^2 = \tfrac{3}{2}(p + K\rho^2)v.$$

Van der Waals then argues that the external pressure p which the containing wall exerts on a mass of gas depends on the number of collisions which it experiences in unit time; and he finds that, on the elastic-sphere theory of molecules, if the spatial extension of all the molecules in volume V is b_1, then the number of collisions per unit time against the walls will be greater than if the molecules were points with the same mass and moving with the same velocity in the ratio $\frac{v}{v-4b_1}$; and the pressure will be correspondingly increased. Hence, according to Van der Waals, we must correct the previous equation to the form

$$\tfrac{1}{2}\Sigma m V^2 = \tfrac{3}{2}(p + K\rho^2)(v - b),$$

where b represents $4b_1$. He then assumes that $\frac{1}{2}\Sigma mV^2$ is always directly proportional to the temperature as measured absolutely on the air-thermometer, and reduces the above to his typical form

$$\left(p + \frac{a}{v^2}\right)(v - b) = R(1 + at).$$

Whatever may be the value of the reasoning by which this form has been derived from the equation of the virial, there is no doubt about the service which it has done in Van der Waals's hands to the cause of molecular science; but the equation as it stands has been found wanting when applied to any series of experiments with a wide range of pressure and temperature.

To meet the deficiencies in it, Clausius (Wiedemann's *Ann.* ix. 1879) constructed the form

$$p = \frac{RT}{v - \alpha} - \frac{C}{T(v + \beta)^2},$$

which is evidently derived from Rankine's form with two new constants added to widen its scope. In this formula Clausius regards $\frac{3}{2}\frac{Cv}{T(v+\beta)^2}$ as representing the virial of the internal forces, and considers that Van der Waals's formula is defective because it fails to take account of the dependence of the virial of the molecular forces on temperature. He draws a contrast between the perfect gaseous state and the liquid state: in the former the molecules are under one another's influence during only a short time compared to the time of traversing the free path, while in the latter it is only exceptionally that a molecule escapes from the influence of its neighbours. The state of an actual gas is supposed to lie between these two extremes; and Clausius contemplates in an actual gas the occurrence of collisions, after which the pair of colliding molecules do not separate but travel along together, oscillating opposite one another. The lower the temperature the larger will be the number of such pairs, and the greater will be the mean value of the attractions, seeing that the molecules which remain together attract one another more powerfully on account of their greater proximity. Accordingly, the term which in the characteristic equation represents the mutual attractions must be assumed to increase with falling temperature. This is Clausius's argument for showing that the variation of the internal virial inversely as the temperature is not merely a matter of pure convenience in his empirical formula, but is probably a fact in nature. I have reproduced it at some length, because not only has he applied his formula to CO_2, but also Sarrau (*Comptes Rendus*, xciv.) has shown that it can represent well the behaviour of H, N, O, CH_4, C_2H_4 in Amagat's experiments within certain limits. Now, while Clausius's argument can readily be admitted as showing the possibility of a certain dependence of the internal virial on temperature, it gives no clue to what we may call the amount of that dependence. It seems to me that it must really be negligible, or, at all events, if we regard the molecular attractions of gaseous CO_2 as the residuum at comparatively great molecular distances of the forces which constitute the cohesion of liquid and solid CO_2, we can hardly, from what we know of the molecular forces involved in elasticity, imagine that their effect in the virial should be so profoundly dependent on temperature as it would be if the forces themselves actually varied inversely as the temperature. In a subsequent attempt* to apply his form to ether and water, Clausius has found it necessary to replace $\frac{1}{T}$ in the virial term of the above equation

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* Wiedemann's *Ann.* xiv. 1881, and *Ann. de Ch. et de Ph.* 5 série, xxx. 1883

by a function involving three constants, so that his equation takes the complicated form

$$\frac{p}{\mathrm{RT}}=\frac{1}{v-\alpha}-\frac{\mathrm{AT}^{-n}-\mathrm{B}}{(v+\beta)^2}.$$

Sarrau (*Comptes Rendus*, ci.) has modified the form of Clausius's equation for CO_2, and introduced another constant ϵ, thus:—

$$p=\frac{\mathrm{RT}}{v-\alpha}-\frac{\mathrm{K}\epsilon^{-\mathrm{T}}}{(v+\beta)^2};$$

but even this form, he finds, fails to represent the behaviour of CO_2 when its volume is reduced to less than $\frac{1}{300}$ of the normal volume, which at the lower temperatures (40° C.) corresponds to a pressure of about 80 metres of mercury, while Amagat's experiments extend to 320 metres.

The last formula to be mentioned is that of Dr. Walter (Wiedemann's *Ann.* xvi. 1882)—

$$(p+f)ve^{-\frac{\beta}{v}}=\tfrac{2}{3}k\mathrm{T},$$

where e is the base of the natural logarithms and β and f are functions of the temperature, left undefined. In a somewhat similar manner to Van der Waals, he argues, to give a meaning to his term $e^{-\frac{\beta}{v}}$, that it represents a correction which it is necessary to make in the equation of the virial on account of the mutual impenetrability of the molecules. And this brings us to an important point. The equation of the virial does not require any correction. It expresses a perfectly general dynamical theorem as to the relation between the mean translatory kinetic energy of any system of bodies in stationary motion and the forces at play among them, and applies as rigorously to a system of extended bodies as to a system of ideal extensionless particles. When the equation of the virial is applied to a number of molecules of a gas confined in vessel of volume v, $\frac{3}{2}pv$ represents the virial of the pressure applied all over the surface, and is perfectly independent of what mechanical arrangement there may be inside to influence the number of collisions, so long as the motion is stationary. The ideas of Van der Waals and Dr. Walter affect profoundly the interpretation which is to be given to a characteristic equation, and, if proved by their application to be correct, would upset the kinetic theory.

H. A. Lorentz (Wiedemann's *Ann.* xii.), accepting Van der Waals's equation as an empirically good one, suggests (but does not press the suggestion) that at low densities the term involving b might be taken as representing the virial of the repulsive forces that are supposed to act during the collisions of molecules. This opens up the whole of the important question of the repulsive forces or of the elastic properties usually assigned to molecules; and this is the part of the kinetic theory in its actual state least satisfactory to the earnest natural philosopher. Sir William Thomson only gave expression to the aspirations which his development of the gyrostatic principle and its attendant speculations on "Elasticity a mode of Motion" have awakened in the minds of physicists for some means of escape from those highly artificial conceptions of the classical elasticians of laws of force changing from attraction to repulsion, which still survive in the idea of an elastic molecule, when, in his address to the Mathematical and Physical section at the Montreal meeting of the British Association (Nature, vol. xxx. 1884), he sketched the possibility of apparent repulsion through attraction. If, therefore, we can prove that molic force is always attractive, an important step will be gained towards homogeneity in our conceptions of force in nature, and there will be only the one general specific property of attraction to explain by the ultimate theory of molecular structure, which will account for the action of molecule on molecule. At first sight it might appear as if the very form of the equation of the virial, as applied in the kinetic theory of gases, militated against the elimination of repulsive forces; for the term $\frac{3}{2}pv$, which represents the virial of the applied pressure, must, on a theory of pure attraction, really be considered as representing the virial of the attractions of the molecules of the bounding walls on the molecules inside. Now the pressure, whatever its origin, is equivalent to a repulsive action from the walls on the fluid inside; so that we have to face the apparent contradiction of the virial of attractive forces between two sets of molecules being replaceable by the virial of an imaginary repulsion between them. But to avoid this difficulty, we have only to make the conception, which in itself will be found an important one in Physics and Chemistry, that the surface-molecules of two bodies in contact always penetrate amongst one another. Let us see the bearing of this idea on the determination of the virial of the attractions of the molecules of a body B on the molecules of a fluid A bounded by B. If a molecule of A gets to the far side of a molecule of B, the attraction on it will be in the same sense as that in which we usually consider the pressure to act; when the molecule of A is on the near side of a molecule of B, the attraction on it is in the opposite sense. The value of the virial of the action of the molecules of B on those of A depends, therefore, entirely on the average relative positions of the molecules of A to those of B in the mixed transition-layer between pure A and pure B. It may be negative or it may be positive, or it may be zero. The distinction between pressure and tension in the action of one body on another represents a difference in the interlinkings of the orbits of the surface molecules. The ordinary idea, that the pressure of a fluid is due to repulsive actions during molecular impact, is not competent to explain the tension that must act between the top portion of the mercury and the glass, when the column of mercury in a barometric tube is got to fill the tube and stand at a height above that which corresponds to the atmospheric pressure. A great array of facts supports the general idea of the mingling of molecules at the surface of contact of two bodies; for example, such an action as the rusting of iron suggests the penetration of oxygen atoms to great molecular depths; while Spring's experiments on chemical combination, produced merely by the application of powerful pressure to mixed powders of different elements, demonstrate molecular interpenetration of bodies in a striking manner. Under powerful enough pressure, a granule of sulphur and a granule of lead diffuse into one another and combine as completely as if their molecules had the freedom of the gaseous state.

Amagat (*Ann. de Chim. et de Phys.* 5 série, xxviii.) gives, as a general result of his experimental studies, that at a given volume the rate of variation of the pressure of a gas with temperature is constant; and Ramsay and Young (Nature, 13th Jan. 1887) have announced the same fact in connexion with their studies of the vapours of ethylic alcohol, ether, and methylic alcohol. Stating this result symbolically, we have

$$\frac{\partial p}{\partial t} = f(v),$$

and integrating,

$$p = f(v)t + \mathrm{F}(v),$$

a type to which Van der Waals's equation belongs, but not Rankine's or Clausius's. But this result is only a first approximation for CO_2, as a close study of Amagat's numbers for CO_2 shows; while Andrews has arranged his later experimental determinations (Phil. Trans. 1875) to bring out the variation of $\frac{\partial p}{\partial t}$ with temperature in a marked manner. Thus $\frac{\partial p}{\partial t}$ is not a function of volume only, but varies slowly with temperature, and Van der Waals's form of equation cannot represent the actual facts. No series of experiments has yet been conducted over a range sufficiently wide and accurately enough to enable us to determine $\frac{\partial^2 p}{\partial t^2}$ in a useful manner; so that, without a guiding hypothesis, the search for a suitable form to give the right *nuances* to $\frac{\partial^2 p}{\partial t^2}$ would be a very difficult matter. As I wished to construct an equation to test the law of the inverse fourth power, it was necessary for me to assume $F(v)$ in the above equation to be of the form $\frac{l}{v^2}$, where l is a constant independent of the temperature, and to take as the general type of my equation,

$$pv = Tf(v, T) - \frac{l}{v}.$$

The approximate determination of l is an easy matter; but the determination conjointly of the final accurate value of l, and of the simplest form of the function f of two variables applicable to the experimental facts, is exceedingly difficult. After a prolonged trial of many different forms, I was led to adopt that given by Dr. Walter, but with his functions β and f determined to be of the forms $\frac{b}{\sqrt{T}}$, and $l - cT$ for CO_2; thus

$$pv = \left(ae^{\frac{b}{v\sqrt{T}}} + \frac{c}{v}\right)T - \frac{l}{v};$$

where e is the base of the natural logarithms, T the temperature reckoned on the air-thermometer from absolute zero, and a, b, c, l are four constants having the following values:—

I. When the unit of pressure is that of a kilogramme weight per square metre, and the unit of volume is a cubic metre (v being the volume of a kilogramme of CO_2),

$$a = 19{\cdot}257, \quad b = {\cdot}02785, \quad c = {\cdot}038577, \quad l = 35{\cdot}327.$$

II. When the unit of pressure is that of one atmosphere, and the unit of volume is that volume which the mass of gas considered occupies at 0° C. and under a pressure of one atmosphere,

$$a = {\cdot}0036976, \quad b = {\cdot}0552, \quad c = {\cdot}000014666, \quad l = {\cdot}01343.$$

As Amagat measured volumes in a unit suitable to the circumstances of his experiments, but has not given its value, I have calculated it to be $\frac{1}{3410}$ of that volume which the mass of gas experimented on would have occupied at 0° C. and 760 millim. pressure.

In order to compare the equation with the results of Andrews's experiments as well as with those of Amagat, I have converted Andrews's pressures (which are given in air-manometer atmospheres, *i. e.* which are calculated from the indications of an air-manometer on the assumption that air obeys the Boyle-Mariotte law) into pressures in metres of mercury, by means of the data for air at 16°, published by Amagat (*Comptes Rendus*, xcix. 1884). The temperature of the air-manometer varied in Andrews's experiments: thus, in the 35°·5 series it was about 16°, in the 64° series it was 9°, and in the 100° series about 6°; but I have assumed it in all cases to be 16°. To convert Andrews's pressures, which are greater than 65 metres of mercury, the greatest in Amagat's air-determinations, I have had to use Amagat's determinations for nitrogen at 17°·7* with such a correction as the difference in the behaviour of air and nitrogen at lower pressures suggests. In order to compare Amagat's results with those of Andrews at the temperatures of 35°·5 C., 64°, and 100°, we must regard his series at 35°·1 as taken at 35°·5, and we must interpolate for the 64° series from his series for 60° and 70°. This I have done for the construction of the subjoined table, in which the top row of figures contains the volumes to which unit volume at 0° and 760 millim. is reduced, and the successive rows contain the corresponding pressures in metres of mercury as given by Andrews, by Amagat, and by the equation.

CO_2 at 100° C.

Volume	·07628	·04158	·02712	·01422	·00625	·00359	·00293	·00264
Pressure, Andrews ...	13·15	23·33	34·36	59 77	110·53	178·0		
" Amagat	......	......	34·30	59·20	109·40	175·0	236·5	303·0
" equation ...	13·18	23·42	34·44	60·00	109·63	176·7	241·6	301·5

CO_2 at 64° C.

Volume	·06671	·03560	·02 64	·01083	·00377	·00277	·00235	·00226
Pressure, Andrews ...	13·28	23·58	34·79	60·40	110·6	178·0		
" Amagat......	......	......	34·80	60·20	108·0	164·0	272·0	320·0
" equation ...	13·34	23·78	35·07	60·22	103·0	169·6	278·5	318·8

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* *Ann. de Chim. et de Phys.* 5 sér. vol. xxii.

CO_2 at 35°·5 C.

Volume	·01368	·00496	·00303	·00263	·00235	·002082	·002073	·002053
Pressure, Andrews ...	42·40	60·54	66·67	80·96				
„ Amagat	42·80	61·00	70·00	91·00	140	......	......	320
„ equation ...	43·29	53·34	67·57	106·80	176	314·5	320	338

CO_2 at 6°·9 C. ...... at 13° C.

Volume	·07921	·02589	·01377
Pressure, Andrews ...	9·08	23·35	35·42
„ equation ...	9·19	23·86	36·86

It will be seen that the results of the two experimenters for 100° agree well with one another, and that the equation represents them satisfactorily; and the same may be said of the results for 64°, except that Andrews's pressure of 178 metres for volume ·00277 is in remarkable disagreement with Amagat's number 164. There is room for an error in my correction of Andrews's air-manometer pressure; but we see that the error must be small, for the same pressure receiving the same correction when it corresponds to volume ·00359 at 100° is brought into good harmony with Amagat's number and with the equation. Thus we receive a first evidence of the discrepancies to be encountered in a more formidable manner when we study the numbers for 35°·5. Here, at comparatively large volumes, the discrepancies between equation and experiment are sufficiently striking, but hardly more startling than the difference between the two experimenters' results. The magnitude of these differences perplexed me for a long time, and I could assign or imagine no adequate cause for them until, by the discovery that the usually assigned critical temperature of CO_2 (31° C.) is too low by more than 10°, I was led to see that capillary action exercises a profound disturbing action on the relations of pressure, volume, and temperature of a fluid in the neighbourhood of the critical state. As both Andrews's and Amagat's experiments were conducted in capillary tubes, we can see in the difference of their tubes a reason for the difference in their results. I may mention that the serious discrepancies between the results of the two experimenters and of the equation are confined to the neighbourhood of the pressures for which *pv* is a minimum;

at points remote from that the agreement is good: thus, at volume ·002053 Amagat's experimental pressure is 320 metres of mercury, while the equation gives 338. The difference between the two results is seen to be negligible if we look at it in another light. Thus, for a pressure of 320 metres of mercury the equation gives a volume ·002073, which differs by only 1 per cent. from Amagat's measurement for the same pressure—a difference quite within the present limits of experimental error in the measurement of small volumes at high pressures.

Let us now proceed to the important question of the true critical temperature of CO_2. The critical temperature, volume, and pressure are determined by the characteristic equation, and the two equations

$$\frac{\partial p}{\partial v}=0 \quad\text{and}\quad \frac{\partial^2 p}{\partial v^2}=0,$$

which may be written

$$p=\frac{a\mathrm{T}}{v}e^{\frac{b}{v\sqrt{\mathrm{T}}}}+\frac{c\mathrm{T}-l}{v^2}, \quad \ldots \ldots \ldots \quad (1)$$

$$0=-\frac{a\mathrm{T}}{v^2}\left(1+\frac{b}{v\sqrt{\mathrm{T}}}\right)e^{\frac{b}{v\sqrt{\mathrm{T}}}}-2\frac{c\mathrm{T}-l}{v^3}, \quad \ldots \quad (2)$$

$$0=\frac{a\mathrm{T}}{v^3}\left(2+\frac{4b}{v\sqrt{\mathrm{T}}}+\frac{b^2}{v^2\mathrm{T}}\right)e^{\frac{b}{v\sqrt{\mathrm{T}}}}+6\frac{c\mathrm{T}-l}{v^4}. \quad \ldots \quad (3)$$

Eliminating the exponential factor from (2) and (3), we get

$$3\left(1+\frac{b}{v\sqrt{\mathrm{T}}}\right)=2+\frac{4b}{v\sqrt{\mathrm{T}}}+\frac{b^2}{v^2\mathrm{T}};$$

$$\therefore \quad \frac{b^2}{v^2\mathrm{T}}+\frac{b}{v\sqrt{\mathrm{T}}}-1=0.$$

Considering that $\sqrt{\mathrm{T}}$ must be taken with a positive sign, only the positive root of this equation for $\frac{b}{v\sqrt{\mathrm{T}}}$ is permissible, and we have at the critical state

$$\frac{b}{\sqrt{\mathrm{T}}}=\frac{\sqrt{5}-1}{2}=f \text{ say};$$

and eliminating v between this and (2) we get

$$0=a\mathrm{T}\,(1+f)e^f+2f\frac{c\mathrm{T}-l}{b}\sqrt{\mathrm{T}},$$

$$\therefore\ 0=2f\frac{c\mathrm{T}}{b}+a\sqrt{\mathrm{T}}(1+f)e^f-2f\frac{l}{b},$$

a quadrate equation for the square root of the critical temperature, of which the physically permissible root is

$$\sqrt{\mathrm{T}}=\frac{-a(1+f)e^{f}+\sqrt{[a(1+f)e^{f}]^{2}+16f^{2}\cdot\frac{cl}{b^{2}}}}{4f\frac{c}{b}}.$$

Substituting the numerical values of all the constants, we find

$$\mathrm{T}=315^{\circ}\cdot 8, \quad \text{and} \quad t=42^{\circ}\cdot 8 \text{ C.}$$

Thus the critical temperature given by the equation is 42°·8, instead of 31° as found by Andrews in his experiments with capillary tubes. The equation gives the critical volume of a kilogramme of CO_2 as ·002536 cubic metres, or ·005026 of that volume occupied by the gas at 0° C. and 760 millim., and the critical pressure as 849700 kilogrammes weight per square metre, or 82·23 atmospheres, or 62·5 metres of mercury. Andrews found the critical pressure in his tubes to be something about 75 or 77 atmospheres.

As I deemed it indispensable that the characteristic equation should give with great accuracy the numerics of the critical state before any sound conclusions could be drawn from its form as to the internal virial, these discrepancies caused me to abandon the above equation for many months, during which I tried, by variations in the values of the constants, and by variations in the whole form, to get a reasonably simple equation that would represent Amagat's experimental results, and at the same time give the critical temperature as 31° C.; but with many forms, involving a larger number of constants, I always found that if Amagat's numbers for 100° and 70° were represented accurately in their whole range, a critical temperature markedly above 31° was obtained. At length, in the hope of getting a further clue to the proper form of equation, I looked up Regnault's data for the saturation-pressures of CO_2 at different temperatures*, and found that he declares explicitly that at 42° C. he had liquid CO_2. His words are:—"Enfin par la quantité d'acide carbonique que l'on fait sortir de l'appareil à la fin des expériences on a reconnu que même à la température de 42° il devait rester encore beaucoup d'acide carbonique liquide." The pressure of saturation which he gives for 42°·5 C. is 61967 millim. of mercury; a number in good agreement with the critical pressure of 62·5 metres of mercury determined above by the equation as corresponding to a critical temperature of 42°·8 C.

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* "Forces élastiques des vapeurs," *Mémoires de l'Académie,* xxvi. p. 618.

Now Regnault's experiments were conducted on a large quantity of CO_2 quite removed from capillary influences, so that there is no difficulty in understanding the reason for the apparent conflict of his result with those of Andrews. The true critical temperature of CO_2 was near 42°·5 C.; Andrews had found the critical temperature of CO_2 when enclosed in a certain capillary tube to be 31°, a temperature which depends on the tube.

[To be continued.]

XIV. *Notices respecting New Books.*

Metretike: or, the Method of Measuring Probability and Utility. By F. Y. Edgeworth, *M.A.* (London, The Temple Company; 68 pp.)

In this pamphlet Prof. Edgeworth, as per his Table of Contents, discusses "The relation of the Calculus of Probabilities to the Philosophy of Utility is partly (i) of Similarity (pp. 1–52); partly (ii) of Identity (pp. 52–56).

"(I.) Similarity exists both (A) as to the methods of calculation and (B) as to the problems proposed.

"(A) The similarity is contemplated under five headings, constituted each by the introduction of an attribute, which tends to render the calculation inexact (pp. 1–50). Under each of these headings it is argued that there have been granted to the Calculus of Probabilities postulates analogous to those which the Calculation of Utility requires to be granted.

"(B) The canons of Belief and Conduct are of similar origin and authority (pp. 50–52).

"(II.) The Theory of Errors, inasmuch as, philosophically, the *quæsitum* is rather the '*most advantageous*' than the *most probable* combination of the given observations, involves the principle of Utility (pp. 52–56)."

Then follow two appendices, "On the Method of Least Squares" (pp. 57–66), and "A List of Cognate Writings, by the same Author" (p. 67).

Many of these papers are already familiar, or, at any rate, known, to our readers, as they have been printed in the 'Philosophical Magazine' at varying dates, from Oct. 1883 to quite a recent issue in the present year; others, whose titles are given, are on their way. The Author touches on many themes, and it goes without saying that there is much matter furnished for careful reading and weighing. Appendix I. may be commended to our mathematical readers, as containing an interesting account and discussion of the Method of Least Squares.

XV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xxiii. p. 468.]

February 9, 1887.—Prof. J. W. Judd, F.R.S., President, in the Chair.

THE following communications were read:—

1. "Evidence of Glacial Action in the Carboniferous and Hawkesbury Series, New South Wales." By T. W. Edgworth David, Esq., F.G.S.

After giving a tabular statement of the sequence of rocks connected with the coal-bearing beds in New South Wales, the Author passed in review the notices by previous observers of glacial action in the Carboniferous beds of that country, terminating with the discovery by Mr. R. D. Oldham of polished and striated boulders in fossiliferous marine beds of Carboniferous age at Branxton. The author had since found another extensive deposit of similar beds at Grass-tree near Musclebrook, 28 miles N.W. of Branxton, and described the section there exposed in a railway-cutting. A fine calcareous sandy shale, reddish to greenish brown in colour, was crowded with round and subangular fragments of rock, from pebbles no larger than marbles up to a third of a ton in weight. The surfaces of these fragments were in many cases ground and scratched. The parent rock of some of the boulders was 30 miles distant.

The evidence of ice-action in the Triassic Hawkesbury series was also described. This evidence was twofold, and consisted of the disrupted angular fragments of shale first observed by Mr. Wilkinson, and of contemporaneously contorted current-bedding, of which no account had previously been published. The contortions were represented on a diagram, and attributed to a lateral thrust such as would be produced by the grounding of floating ice.

The discovery by Mr. Wilkinson of polished and striated boulders in some gold-bearing conglomerates believed to be of Siluro-Devonian age was also noticed.

2. "The Terraces of Rotomahana, New Zealand." By Josiah Martin, Esq., F.G.S.

The Author, after deploring the recent calamity, proceeded to describe the *White Terrace.* Its origin, the Terata Geyser, was situated in a crater-like escarpment near the centre of a conical hill of steaming and partially decomposed felspathic tuff on the south-east side of the warm lake, Rotomahana. The Terrace was divided into:—

1. The Upper Terrace, with its long horizontal lines of cups, steaming and overflowing with hot water.
2. The Middle Terrace, with its massive steps and shaggy fringes, without basins or receptacles for the overflow.
3. The Lower Plateau, a series of shallow basins and wide, level platforms.

The great cauldron and the action of the Geyser were described in much detail. The following analysis of the water was given in *grains per gallon.*

Silica, free and combined with soda	50
Sodium and potassium chlorides	60
Alkalies, chiefly soda..................	30
Sodium sulphate, &c..................	10
Total	150

The amount of rock-material withdrawn in solution would amount to about 10 tons per day; observations lead to the conclusion that 10 per cent. of the silica would be deposited on the surface covered by the overflow—equivalent to about 120 tons per annum. Then followed the description of the three divisions previously defined, including such local features as the *Giant Buttress*, the *Boar's Head*, and the *Broken Basin*—the latter a circular pool 12 feet in diameter and 30 inches deep—the only warm-water basin on the White Terrace of sufficient depth to be used as a bath. The central portion of the Middle Terrace was distinguished by a series of massive, rugged, and rippled perpendicular steps, many of which exceeded 6 feet in height, and were variously ornamented. The margin of the lower plateau was somewhat undefined towards the lake.

The Author observed that the comparative study of local phenomena must precede any attempt to explain the origin of the Terraces. The phenomena of mud volcanoes exhibited at the plateau of Rotokanapanapa afford to the geologist valuable indications of the probable appearance of the Terata cauldron in the earlier stages of its activity. These phenomena he described, and arrived at the conclusion that the initial activity of Terata was of a similar nature, and that the successive periods which mark the history of the formation of the White Terrace correspond with the increasing activity of its source. Given a crater-lake of seething mud, as activity increased, the outer wall of the crater would occasionally be broken down, and mud streams would be liberated. Such periodical overflows he regarded as having built up the curious and complex terraced series. This hypothesis he applied in detail, and concluded that deposition and removal combined to produce the great variety of forms which existed. Thermal activity within the cauldron having at length removed the softened rock, the deposition of siliceous incrustation commenced.

Lastly he gave a short description of the Pink Terrace, and remarked that by the catastrophe of June 10th the waters of the lakes Rotomakirisi and Rotomahana were drawn into the new fissure at the base of Tarawera, whilst the Terraces were blown away.

3. "The Eruption of Mount Tarawera." By Capt. F. W. Hutton, F.G.S.

The paper commenced with a description of the country in which the eruption took place. From Tongariro to White Island, in the Bay of Plenty, a distance of 130 miles, there extends a belt, 20 or 30 miles wide, abounding in solfataras, geysers, hot springs, &c., and composed of volcanic rocks, chiefly rhyolite, with some augite-andesite. About the middle of this belt lie the mountain and lake of Tarawera, and two or three miles further south Lake Rotomahana, the spot where the famous Pink and White Terraces existed. Before the recent eruption there were no craters on Mount Tarawera, the form of which was a ridge, apparently due to denudation.

A description of the eruption was then given. Although earthquakes had prevailed for some time in the neighbourhood, they were not violent. Shortly after midnight on the 10th June a series of tremendous explosions took place from various parts of the Tarawera ridge, and columns of steam were thrown up with quantities of red-hot stones. The whole mountain appeared as if on fire. A column of steam was then sent up from near Okaro far to the west, and, finally, a great explosion took place in Lake Rotomahana, and steam rushed forth to a height exceeding that of the columns from Tarawera. These eruptions from the plain were not accompanied by any red-hot stones; the ejecta were of much lower temperature. The principal eruption, accompanied by violent earthquakes and loud noises of various kinds, was over by 5.30 A. M., and the mountain craters ceased to be active within twenty-four hours, but steam with some stones and mud continued to issue from the Rotomahana and Okaro craters for several days, and steam has ever since been emitted from Rotomahana.

The results of the eruption in the form of fissures on Mount Tarawera, the change of Rotomahana from a lake to a crater of larger dimensions, with precipitous walls, the formation of a new lake between this crater and Tarawera, and the formation of a number of small craters about Okaro, were then briefly noticed. The materials ejected were composed of augite-andesite, and rhyolites, both compact and vesicular. The mineral structure and distribution over the surrounding country of various forms of pumice, scoria, and ash were described, and it was shown that there was a difference in the substances ejected from the mountain craters of Tarawera and those from the craters in the plain at Rotomahana and Okaro, the former comprising pumice and scoria, which were not thrown out from the latter, and but little steam issuing from the higher craters when compared with the enormous volumes emitted from the lower vents.

The cause of the eruption was ascribed to the reheating of old lava-streams saturated with water. This reheating was apparently not due to crushing; for, had it been so, the preceding earthquakes would have been more violent; but probably to molten rock coming up from below and heating the rocks near the surface. The eruptions from Rotomahana and Okaro were purely hydrothermal.

February 23.—Prof. J. W. Judd, F.R.S., President, in the Chair.

The following communication was read:—

1. "On the Origin of Dry Chalk Valleys and of Coombe Rock." By Clement Reid, Esq., F.G.S.

Whilst engaged in examining the Pleistocene deposits of Sussex, for the Geological Survey, the Author observed that the Coombe Rock differs from anything commonly seen in the strongly glaciated districts of the Yorkshire and Lincolnshire Wolds. As in these localities, the seaward slope of the South Downs is broken by the line of a partially buried sea-cliff before passing under the low-lying drift areas. Subsequent to the formation of this sea-cliff a mass of angular flint and chalk detritus spread out from the Downs over the low lands, being seldom found far up the valleys. This is the Coombe Rock, which passes further on into a worthless mixture of angular flint and loam, and at a still greater distance into almost clean brick-earth. It is not of glacial origin, neither is it marine, nor is it a gravel formed by ordinary fluviatile action. A study of the contours of the Downs may give us, the Author thinks, a key to the mode of its formation.

The rolling outline of the Downs, and the steep-sided dry valleys point to conditions which have passed away. However much rain may fall, the upper parts of these valleys are always dry, and no running water can be found where the incline of the bottom of the valley exceeds the slope of the plane of saturation—never more than 60 feet per mile. Three explanations have been offered:—

(1) Former submergence and rise in level of the plane of saturation.

(2) Former higher level of the plane of saturation before the valleys were cut down to their present depth.

(3) Increase in the rainfall.

None of these theories is sufficient to account for the origin of Coombes and the transport of Coombe Rock. There is no evidence of submergence whilst the Coombes were being eroded; on the contrary, the descent of the Coombes to the sea-level near Rottingdean and elsewhere, is suggestive of a slight elevation. The deep trenching of the Downs by valleys, and the consequent lowering of the plane of saturation is applicable to many of the slightly inclined Coombes, but the whole structure of the country shows that the outlet for the water must have been as clear then as now. Since the dry chalk valleys play no part in the present superficial drainage, it would make but little difference in the plane of saturation if they were filled up again. If springs had formerly existed in the higher valleys, their gradual failure would have left evidence in the shape of gravel deposits and terraces. Moreover, as an objection, both to the first and second theories, it is urged that if valleys had been cut back by springs, some of them should fall to the north, where most of the springs occur, whereas the Coombes open to the south. Lastly he finds no traces of the "hypothetical pluvial period."

In suggesting an origin for the dry valleys and Coombe Rock, he considers that the fauna and flora, both at Fisherton and Bovey Tracey, point to a great degree of cold, from 20° to 30° lower than what now prevails in the South of England. The ground would thus be frozen to the depth of several hundred feet, and the drainage system of the chalk entirely modified. There would be no underground circulation. The summer rains would immediately run off any steep slope, often in violent torrents. These would tear up the layer of rubble already loosened by the frost, carrying down masses of unthawed chalk too rapidly for solvents to have much effect. No Coombe Rock is found in valleys that have a greater slope than 100 feet per mile. There is no need of excessive rainfall; it might have been a dry period corresponding to that of the Löss.

If the time had not been short, all soft rocks in the South of England would have been planed down to one gently undulating surface like the plains of Russia and Siberia. Such Tundra-conditions may have occurred more than once.

2. "Probable Amount of former Glaciation of Norway, as demonstrated by the present condition of Rocks upon and near the Western Coast." By W. F. Stanley, Esq., F.G.S.

The observations on which this paper are based were made in June last, during a voyage along the west coast of Norway. Inland conditions were also noted in the Hardanger and Sogne Fjords, and a few trips up some of the valleys enabled these inland observations to be further extended. The author limited his work to searching for outline evidence of ice-action. The aspect of the coast for hundreds of miles consecutively has a uniform character of jagged and pointed rocks nearly to the sea-level. At the mouths of the fjords the rocks are more rounded, particularly at heights less than 100 feet. Within the Arctic Circle the Swartisen glacier reaches nearly to the sea, and here the rocks are more rounded.

He exhibited sketches showing the characteristic forms of the rocks, and concluded from a study of these that ice had never prevailed along the entire western coast of Norway, neither had inland ice of any considerable thickness flowed over this coast in sufficient volume to wear off the points of the sharply fractured granite. Even the rocks below 100 feet are not more worn than is sometimes the case in tropical climates. The "shark's teeth" of the Lofotens have not been planed down, nor is there any vestige of the great ice-sheet of our text-books within the Arctic Circle. Even in the fjords there is no evidence of ice-action until we arrive at the head, where it is very evident. There can be no better demonstration of the extent of former glaciation than in the Romsdal valley, where the line of the worn base extends as high up the rock as 600 feet. He also instanced the principal glaciers of the Folge Fjord, now about 7 miles from the open water of the fjord, though formerly within 1½ mile. The angular character of the low rocky island in front of Odde shows that it cannot have advanced further.

The Author concluded that at no period within geologically recent, say Tertiary times, has ice extended much further than at present. Seeing that the morainic matter now in the valleys has been derived from the hills, there must formerly have been a greater extent of land above the snow-line, and this would cause a former extension of glaciers without resort to any extraneous theory or change of climate. The Great Ice Age has left no trace on the Norwegian littoral.

March 23.—Prof. J. W. Judd, F.R.S., President, in the Chair.

The following communications were read:—

1. "Notes on the Structures and Relations of some of the older Rocks of Brittany." By T. G. Bonney, D.Sc., LL.D., F.R.S., Professor of Geology in University College, London, and Fellow of St. John's College, Cambridge.

These notes are the results of a visit to some of the more interesting geological sections in Brittany, in the autumn of last year. The author is greatly indebted for information to the Rev. E. Hill, who took part in the summer excursion of the Société Géologique de France, and to Dr. Charles Barrois, who has for long been engaged in investigating the geology of Brittany.

(1) The Author briefly noticed the glaucophane-amphibolites and the associated schists of the Ile de Groix, which have been already admirably described by Dr. C. Barrois. He considered the evidence to be on the whole in favour of the view that the former were originally igneous rocks intrusive in the latter, but modified by subsequent pressure (the marks of which are very conspicuous in the schists), and by mineral change, which probably produced the glaucophane, the garnets being anterior to the mechanical disturbance.

(2) The next part of the paper treated of sections in the district about Quimperlé. Cases were cited of granite, modified by pressure so as to result in a gneissoid rock, and of banded gneisses also modified by subsequent pressure, but, in the Author's opinion, indubitably banded gneisses anterior to the mechanical disturbance, and exhibiting structures which, in his opinion, lend themselves more readily to a theory of some kind of original stratification of the constituents than to any other. The amphibolites in this region are undoubtedly of igneous origin (intrusive), but subsequently modified. In one part of the district are granitoid gneisses, but little modified by subsequent mechanical action, which in structure differ greatly from the granites, and much resemble the older Archæan gneisses of other regions. A "hälleflinta" to the north of Quimperlé proves to be in part a rhyolitic rock, modified by subsequent pressure; part, however, may be an indurated tuff of similar composition.

(3) In this part of the paper were noticed the crystalline rocks of Roscoff, and (more briefly) the Palæozoic strata about Morlaix, with the mineral and structural modifications due to pressure and to the action of intrusive igneous rocks. The author pointed out that, in the latter case, the results either of pressure-metamorphism or of contact-metamorphism differ much from the crystalline schists, which, both in Brittany and elsewhere, are regarded as of Archæan age; and that here in the north at Roscoff, we have a series of banded gneisses, less modified by subsequent pressure than in the south, the structures of which are very difficult to explain on any theory of a "rolling out" of a complicated association of igneous rocks, but which are such as would naturally result from some kind of stratification of the original constituents.

The result of the author's work is to strengthen the opinion which he has already expressed, that while the structures of some foliated rocks may be regarded as primarily due to pressure operating on suitable materials, the structure of others seems opposed to this explanation. At any rate the latter rocks appear to have assumed a crystalline condition with a semblance of stratification in Pre-Cambrian times; so that, whatever may be their genesis, they are rightly called Archæan gneisses and schists.

2. "The Rocks of Sark, Herm, and Jethou." By Rev. E. Hill, M.A., F.G.S.

The author described the island of Sark, about three miles long by two broad, with the smaller areas of Little Sark and Brecqhou. Little Sark is attached to Great Sark by a narrow ridge (the Coupée), which the weather is rapidly degrading, while Brecqhou is completely separated by a narrow strait. The greater part of these islands consists of dark hornblendic banded rocks, which closely resemble those of the Lizard, and show by their alternation of materials and their phenomena of current-bedding that they have originated by some kind of deposition. These were shown to lie unconformably on a gneiss, seen only at the eastern extremity of the island, in and around the Creux Harbour. Over this the beds lie in a dome, and as they slope away on the N., W., and S., they pass under a highly-crystalline rock, which has been called a metamorphic gneiss. This rock was described, and evidence given to show that it is really a granite,—an igneous rock which has overflowed the hornblendic beds.

Veins and dykes were briefly noticed; they include a dyke of mica-trap. The islands of Herm and Jethou, lying between Guernsey and Sark, were also described. Jethou contains a fine raised beach. They consist of granite which presents signs of an E. and W. dip. A probability was shown that this granite is part of the mass overlying Sark.

Finally the age of these rocks was shown to be Archæan, and attention was called to the evidence they give that some at least of the Archæan rocks did not originate out of igneous masses by crush, but were formed by some process which, if not aqueous sedimentation, at all events was some kind of successive deposition.

3. "Quartzite Boulders and Grooves in the Roger Mine at Dukinfield." By James Radcliffe, Esq., F.G.S.

Quartzite boulders have from time to time been found imbedded in the roof of the Roger Mine coal-seam. Similar boulders had previously been described from coal-seams both in Leicestershire and the forest of Dean. The composition of the Roger-Mine boulders was shown by notes furnished by Prof. Bonney to be quartzose grit and quartzite, containing some grains of felspar, epidote, and tourmaline and flakes of mica. This composition resembled that of some of the pebbles in the Bunter conglomerate of the Midland counties, and also that of some of the Loch Maree quartzites. The boulders varied in weight from 166 pounds to 4 pounds, and appeared to have been dropped into the coal, one boulder having been found standing edgeways. They were half imbedded in the seam, half enclosed in the overlying grey shale.

In the upper surface of the coal in the same mine, grooves were found running about S. 50° E., the mean direction of faults, slips, &c., being S. 26° W. The sides of these grooves were raised, as if by pressure, and each depression commenced as a small groove, then increased in depth and breadth, and finally died out.

XVI. *Intelligence and Miscellaneous Articles.*

THE ADAMS PRIZE, CAMBRIDGE UNIVERSITY.

(St. John's College Lodge, May 7, 1887.)

THE Examiners give notice that the following is the subject for the Adams Prize to be adjudged in 1889:—

On the Criterion of the Stability and Instability of the Motion of a Viscous Fluid.

It appears from experiment (see Phil. Trans. for 1883, p. 935) that the steady motion in a tube is stable or unstable, according as the velocity is less or greater than a certain amount; and it is inferred from theory, confirmed by experiment, that in two geometrically similar systems the motion is stable or unstable according as $\mu/\rho c\mathrm{U}$ is greater or less than a certain numerical quantity n; c, U being a length and a velocity which define the linear scale and the scale of velocity in the system, and ρ, μ the density and coefficient of viscosity of the fluid; but the quantity n has not hitherto been obtained even in a simple case except by experiment.

It is required either to determine generally the mathematical criterion of stability, or to find from theory the value of n in some simple case or cases. For instance, the case might be taken of steady motion in two dimensions between two fixed planes, or that of a simple shear between two planes, one at rest and one in motion.

Should the investigation not be found practicable for even a simple case of the motion of a viscous fluid, some substantial advance might be made in what has been done for a perfect fluid (see Proceedings of the Mathematical Society, vol. xi. p. 57), the title of the Essay being modified accordingly.

The Prize is open to the competition of all persons who have at any time been admitted to a Degree in this University.

Each Essay should be accompanied by a full and careful abstract pointing out the parts which the author considers to be new, and indicating the parts which are to be regarded as of more importance than the rest.

The Essays must be sent in to the Vice-Chancellor on or before the sixteenth day of December, 1888, privately. Each is to have some motto prefixed, and to be accompanied by a paper sealed up with the same motto and the words Adams Prize on the outside, and the Candidate's full name with his College and Degree written within.

The papers containing the names of those Candidates who may not succeed will be destroyed unopened.

Any Candidate is at liberty to send in his Essay either written (but not in his own hand) or printed or lithographed.

The successful Candidate receives about £170. He is required to print the Essay at his own expense, and to present a copy to the University Library, to the Library of St. John's College, and to each of the four Examiners.

C. Taylor, *Vice-Chancellor.*
G. H. Darwin, *Plumian Professor.*
G. G. Stokes.
Rayleigh.

ON ATMOSPHERIC ELECTRICITY. BY R. NAHRWOLD.

The author had shown in an earlier paper that electricity which issues from a point into an earth-connected receiver, charges, in preference, the floating dust particles in the enclosed space, which are then quickly driven to the sides. If these sides are coated with glycerine the particles remain there, and the space sooner becomes free from dust than when allowed to settle gradually.

He describes an experiment by which the action of electricity on the dust diffused in the air is demonstrated in a striking manner.

A tubulated glass bell-jar, about 30 centim. in height and 20 centim. in diameter, was carefully cleaned and then coated on the inside with glycerine. The bell-jar was inverted, and the upper opening closed by a zinc sheet, which, with the layer of glycerine, was in conducting communication with the earth. Through a hole about 2 centim. in diameter in the centre of this lid a copper wire, provided with fifteen fine sewing-needles, was fixed by an insulator. This wire was made of two wires, of about 5 millim. diameter, twisted together, and the needles are stuck in between these two at a distance of 1 centim. apart and at right angles to the wire. The copper wire was connected with one pole of a Töpler's machine, the other pole of which was to earth, so that when the machine was worked the electricity issued from the needles into the bell-jar.

Tobacco-smoke could be blown through the lower aperture, so that the bell-jar was quite opaque, and even the needles in the centre could not be seen. When now the machine was worked, the smoke disappeared after two or three turns and the bell-jar was as clear as before.

Aqueous vapour, sal-ammoniac, phosphoric anhydride, and soot could be used, but not so advantageously as tobacco-smoke. If the inside was not coated with glycerine the same results were obtained, but required a much longer time.

The other results obtained by the author are as follows:—

(1) It has been more conclusively proved than heretofore that the electricity issuing from the points does not electrify the air itself statically, but the dust in it, which consists of suspended liquid or solid particles.

(2) It has been shown that fine particles are driven away from an ignited platinum wire, which are diffused in the surrounding air; and in this way air free from dust, which is not suited for receiving electricity from points, becomes again capable of acquiring a charge.

(3) The conclusion has been arrived at that the electricity issuing from an ignited platinum wire does not give a static charge to the air itself, but that the charges observed as atmospheric electricity have their seat in the particles already repelled into the air.

(4) It is accordingly highly probable that atmospheric air, and no doubt other gases also, cannot be statically electrified.

(5) A fresh experiment is given which shows that, at ordinary temperatures, negative electricity of high potential issues more readily into the air than positive.—Wiedemann's *Annalen*, No. 7, 1887.

ON AN EXPERIMENTAL DETERMINATION OF THE WORK OF MAGNETIZATION. BY A. WASSMUTH AND G. A. SCHILLING.

If soft iron is brought from a very great distance near a magnet, and thereupon so rapidly removed that the magnetism does not diminish, more work (W) is consumed in the removal than was gained in the approach; for in the removal the attraction is stronger. The difference, $A=W-L$, is the work of magnetization. It is assumed that the magnetizing force x acts on all parts of the body used, an elongated ellipsoid of rotation, with equal force in the direction of the axis of rotation. The work of magnetization is calculated for 1 cubic millim., if the moment of the cubic millimetre is μ. W is then $=x\mu$, and $L=\int\mu dx$, as is shown by calculation and by the experiments. Hence

$$A=x\mu-\int\mu dx=\int x d\mu.$$

In the experiments the iron ellipsoid, with its axis of rotation, was in the same vertical plane as the limbs of a large electromagnet provided with large pole-pieces, so that the field was as uniform as possible; it was suspended to a balance, so that the attraction p at various vertical distances z from the tips of the magnet-ends could be determined. The surface of the curve whose abscissæ correspond to z, and the ordinates to p, defines the variation of work, which according to theory must be equal to $\int\mu dx$. The forces x and μ were measured by the currents induced in a fixed coil, on changing the polarity, in one case with, and in another case without, the freely-suspended iron core; and the currents were reduced to absolute measure by a terrestrial inductor introduced into the circuit. Experiment confirmed the theoretical anticipations. They prove that the work for the change of position of the iron is equal to the corresponding increase and decrease of $\int\mu\, dx$; the work of magnetization is thus equal to the change of $\int x\, d\mu$. Only in case, when μ is proportional to x, do $\int x\, d\mu$ and $\int\mu\, dx$ give the same value, $\frac{1}{2}x\mu$. The work is in that case proportional to x^2, on which may be based a method of determining x.—*Wiener Berichte*, xciv. (1886); *Beiblätter der Physik*, No. 4 (1887).

THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FIFTH SERIES.]

AUGUST 1887.

XVII. *On the Maintenance of Vibrations by Forces of Double Frequency, and on the Propagation of Waves through a Medium endowed with a Periodic Structure. By* Lord Rayleigh, *Sec. R. S., Professor of Natural Philosophy in the Royal Institution*[*].

THE nature of the question to be first considered may be best explained by a paragraph from a former paper[†], in which the subject was briefly treated. "There is also another kind of maintained vibration which, from one point of view, may be regarded as forced, inasmuch as the period is imposed from without, but which differs from the kind just referred to (ordinary forced vibrations) in that the imposed periodic variations do not tend directly to displace the body from its configuration of equilibrium. Probably the best-known example of this kind of action is that form of Melde's experiment in which a fine string is maintained in transverse vibration by connecting one of its extremities with the vibrating prong of a massive tuning-fork, *the direction of motion of the point of attachment being parallel to the length of the string*[‡]. The effect of the motion is to render the tension of the string periodically variable; and at first sight there is nothing to cause the string to depart from its equilibrium condition of straightness. It is known, however, that under these circumstances the equilibrium position may become unstable, and that the string may settle down into a state of permanent and

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[*] Communicated by the Author.

[†] "On Maintained Vibrations." Phil. Mag. April 1883, p. 229.

[‡] "When the direction of motion is transverse, the case falls under the head of ordinary forced vibrations."

vigorous vibration *whose period is the double of that of the point of attachment*"[*]. Other examples of acoustical interest are mentioned in the paper.

My attention was recalled to the subject by Mr. Glaisher's Address to the Astronomical Society[†], in which he gives an interesting account of the treatment of mathematically similar questions in the Lunar Theory by Mr. Hill[‡] and by Prof. Adams[§]. The analysis of Mr. Hill is in many respects incomparably more complete than that which I had attempted; but his devotion to the Lunar Theory leads the author to pass by many points of great interest which arise when his results are applied to other physical questions.

By a suitable choice of the unit of time, the equation of motion of the vibrating body may be put into the form

$$\frac{d^2w}{dt^2}+2k\frac{dw}{dt}+(\Theta_0+2\Theta_1\cos 2t)w=0; \quad . \quad . \quad . \quad (1)$$

where k is a positive quantity, which may usually be treated as small, representing the dissipative forces. $(\Theta_0+2\Theta_1\cos 2pt)$ represents the coefficient of restitution, which is here regarded as subject to a small imposed periodic variation of period π. Thus Θ_0 is positive, and Θ_1 is to be treated as relatively small.

The equation to which Mr. Hill's researches relate is in one respect less general than (1), and in another more general. It omits the dissipative term proportional to k; but, on the other hand, as the Lunar Theory demands, it includes terms proportional to $\cos 4t$, $\cos 6t$, &c. Thus

$$\frac{d^2w}{dt^2}+(\Theta_0+2\Theta_1\cos 2t+2\Theta_2\cos 4t+\ldots)w=0; \quad . \quad (2)$$

or

$$\frac{d^2w}{dt^2}+\Theta w=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where

$$\Theta=\Sigma_n\Theta_n e^{2int}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

n being any integer, and i representing $\sqrt{(-1)}$. In the present investigation $\Theta_{-n}=\Theta_n$.

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[*] "See Tyndall's 'Sound,' 3rd. ed. ch. iii. § 7, where will also be found a general explanation of the mode of action."

[†] Monthly Notices, Feb. 1887.

[‡] "On the Part of the Motion of the Lunar Perigee which is a Function of the Mean Motions of the Sun and Moon," *Acta Mathematica*, **8**:1, 1886. Mr. Hill's work was first published in 1877.

[§] "On the Motion of the Moon's Node, in the case when the orbits of the Sun and Moon are supposed to have no Eccentricities, and when their Mutual Inclination is supposed to be indefinitely small." Monthly Notices, Nov. 1877.

It will be convenient to give here a sketch of Mr. Hill's method and results. Remarking that when Θ_1, Θ_2, &c. vanish, the solution of (3) is

$$w = \mathrm{K}e^{ict} + \mathrm{K}'e^{-ict}, \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where K, K′ are arbitrary constants, and $c = \surd(\Theta_0)$, he shows that in the general case we may assume as a particular solution

$$w = \Sigma_n b_n e^{ict+2int}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

the value of c being modified by the operation of Θ_1, &c., and the original term $b_0 e^{ict}$ being accompanied by subordinate terms corresponding to the positive and negative integral values of n.

The multiplication by Θ, as given in (4), does not alter the form of (6); and the result of the substitution in the differential equation (3) may be written

$$(c+2m)^2 b_m - \Sigma_n \Theta_{m-n} b_n = 0, \quad . \quad . \quad . \quad . \quad (7)$$

which holds for all integral values of m, positive and negative. These conditions determine the ratios of all the coefficients b_n to one of them, *e. g.*, b_0, which may then be regarded as the arbitrary constant. They also determine c, the main subject of quest. Mr. Hill writes

$$[n] = (c+2n)^2 - \Theta_0; \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

so that the equations take the form

$$\left.\begin{array}{l}
. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \\
\ldots + [-2]b_{-2} - \Theta_1\ b_{-1} - \Theta_2\ b_0 - \Theta_3\ b_1 - \Theta_4\ b_2 - \ldots = 0, \\
\ldots - \Theta_1\ b_{-2} + [-1]b_{-1} - \Theta_1\ b_0 - \Theta_2\ b_1 - \Theta_3\ b_2 - \ldots = 0, \\
\ldots - \Theta_2\ b_{-2} - \Theta_1\ b_{-1} + [0]b_0 - \Theta_1\ b_1 - \Theta_2\ b_2 - \ldots = 0, \\
\ldots - \Theta_3\ b_{-2} - \Theta_2\ b_{-1} - \Theta_1\ b_0 + [-1]b_1 - \Theta_1\ b_2 - \ldots = 0, \\
\ldots - \Theta_4\ b_{-2} - \Theta_3\ b_{-1} - \Theta_2\ b_0 - \Theta_1\ b_1 + [2]b_2 - \ldots = 0, \\
. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad .
\end{array}\right\} . \quad (9)$$

The determinant formed by eliminating the b's from these equations is denoted by $\mathfrak{D}(c)$; so that the equation from which c is to be found is

$$\mathfrak{D}(c) = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

The infinite series of values of c determined by (10) cannot give independent solutions of (3),—a differential equation of the second order only. It is evident, in fact, that the system of equations by which c is determined is not altered if we replace c by $c+2\nu$, where ν is any positive or negative integer. Neither is any change incurred by the substitution of $-c$ for c. "It follows that if (10) is satisfied by a root $c=c_0$, it will also have, as roots, all the quantities contained in the expression

$$\pm c_0 + 2n,$$

where n is any positive or negative integer or zero. And these are all the roots the equation admits of; for each of the expressions denoted by $[n]$ is of two dimensions in c, and may be regarded as introducing into the equation the two roots $2n+c_0$ and $2n-c_0$. Consequently the roots are either all real or all imaginary; and it is impossible that the equation should have any equal root unless all the roots are integral."

On these grounds Mr. Hill concludes that $\mathfrak{D}(c)$ must be such that

$$\mathfrak{D}(c)=\mathrm{A}\left[\cos(\pi c)-\cos(\pi c_0)\right] \quad . \quad . \quad . \quad (11)$$

identically, where A is some constant independent of c; whence on putting $c=0$,

$$\mathfrak{D}(0)=\mathrm{A}[1-\cos(\pi c_0)], \quad . \quad . \quad . \quad . \quad (12)$$

in which, if we please, c_0 may be replaced by c. The value of A may now be determined by comparison with the particular case $\Theta_1=0$, $\Theta_2=0$, &c., for which of course $c=\surd\Theta_0$. Thus if $\mathfrak{D}'(0)$ denote the special form then assumed, *i. e.* the simple product of the diagonal constituents,

$$\mathfrak{D}'(0)=\mathrm{A}[1-\cos(\pi\surd\Theta_0)], \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

and

$$\frac{1-\cos(\pi c)}{1-\cos(\pi\surd\Theta_0)}=\frac{\sin^2(\frac{1}{2}\pi c)}{\sin^2(\frac{1}{2}\pi\surd\Theta_0)}=\frac{\mathfrak{D}(0)}{\mathfrak{D}'(0)}. \quad . \quad (14)$$

The fraction $\mathfrak{D}(0)\div\mathfrak{D}'(0)$ is denoted by $\square(0)$. It is the determinant formed from the original one by dividing each row by the constituent in the diagonal, so as to reduce all the diagonal constituents to unity, and by making c vanish. Thus

$$\frac{1-\cos(\pi c)}{1-\cos(\pi\surd\Theta_0)}=\square(0), \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

where

$$\square(0)=\left|\begin{array}{ccccc}
. & . & . & . & . \\
\cdots+\quad 1 & -\frac{\Theta_1}{4^2-\Theta_0} & -\frac{\Theta_2}{4^2-\Theta_0} & -\frac{\Theta_3}{4^2-\Theta_0} & -\frac{\Theta_4}{4^2-\Theta_0}\cdots \\
\cdots-\frac{\Theta_1}{2^2-\Theta_0} & +\quad 1 & -\frac{\Theta_1}{2^2-\Theta_0} & -\frac{\Theta_2}{2^2-\Theta_0} & -\frac{\Theta_3}{2^2-\Theta_0}\cdots \\
\cdots-\frac{\Theta_2}{0^2-\Theta_0} & -\frac{\Theta_1}{0^2-\Theta_0} & +\quad 1 & -\frac{\Theta_1}{0^2-\Theta_0} & -\frac{\Theta_2}{0^2-\Theta_0}\cdots \\
\cdots-\frac{\Theta_3}{2^2-\Theta_0} & -\frac{\Theta_2}{2^2-\Theta_0} & -\frac{\Theta_1}{2^2-\Theta_0} & +\quad 1 & -\frac{\Theta_1}{2^2-\Theta_0}\cdots \\
\cdots-\frac{\Theta_4}{4^2-\Theta_0} & -\frac{\Theta_3}{4^2-\Theta_0} & -\frac{\Theta_2}{4^2-\Theta_0} & -\frac{\Theta_1}{4^2-\Theta_0} & +\quad 1\quad\cdots \\
. & . & . & . & .
\end{array}\right| \quad (16)$$

The value of $\square(0)$ is calculated for the purposes of the Lunar Theory to a high order of approximation. It will here suffice to give the part which depends upon the squares of Θ_1, Θ_2, &c. Thus

$$\square(0)=1+\frac{\pi\cot(\frac{1}{2}\pi\sqrt{\Theta_0})}{4\sqrt{\Theta_0}}\left[\frac{\Theta_1^2}{1-\Theta_0}+\frac{\Theta_2^2}{4-\Theta_0}+\frac{\Theta_3^2}{9-\Theta_0}+\ldots\right]. \quad (17)$$

Another determinant, $\nabla(0)$, is employed by Mr. Hill, the relation of which to $\square(0)$ is expressed by

$$\nabla(0)=2\sin^2(\tfrac{1}{2}\pi\sqrt{\Theta_0})\,.\,\square(0); \quad . \quad . \quad . \quad (18)$$

so that the general solution for c may be written

$$\cos(\pi c)=1-\nabla(0). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

Mr. Hill observes that the reality of c requires that $1-\nabla(0)$ should lie between -1 and $+1$. In the Lunar Theory this condition is satisfied; but in the application to Acoustics the case of an imaginary c is the one of greater interest, for the vibrations then tend to increase indefinitely.

Cos (πc) being itself always real, let us suppose that πc is complex, so that

$$c=\alpha+i\beta,$$

where α and β are real. Thus

$$\cos\pi c=\cos\pi\alpha\cos i\pi\beta-\sin\pi\alpha\sin i\pi\beta;$$

and the reality of $\cos\pi c$ requires either (1) that $\beta=0$, or (2) that $\alpha=n$, n being an integer. In the first case c is real. In the second

$$\cos\pi c=\pm\cos i\pi\beta=1-\nabla(0), \quad . \quad . \quad . \quad (20)$$

which gives but one (real) value of β. If $1-\nabla(0)$ be positive,

$$c=\pm i\beta+2n; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

but if $1-\nabla(0)$ be negative,

$$\cos\pi c=-\cos i\pi\beta,$$

whence

$$c=\pm i\beta+2n+1. \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

The latter is the case with which we have to do when Θ_0, and therefore c, is nearly equal to unity; and the conclusion that when c is complex, the real part is independent of Θ_1, Θ_2, &c. is of importance. The complete value of w may then be written

$$w=e^{\beta t}\Sigma b_n e^{it(1+2n)}+e^{-\beta t}\Sigma b'_n e^{it(1+2n)}, \quad . \quad . \quad . \quad (23)$$

the ratios of b_n and also of b_n' being determined by (9). After the lapse of a sufficient time, the second set of terms in $e^{-\beta t}$ become insignificant.

In the application of greatest acoustical interest Θ_0 (and c) are nearly equal to unity; so that the free vibrations are performed with a frequency about the half of that introduced by Θ_1. In this case the leading equations in (9) are those which involve the small quantities [0] and [−1]; but for the sake of symmetry, it is advisable to retain also the equation containing [1]. If we now neglect Θ_2, as well as the b's whose suffix is numerically greater than unity, we find

$$\frac{b_{-1}}{\Theta_1[1]}=\frac{b_0}{[1][-1]}=\frac{b_1}{\Theta_1[-1]}, \quad . \quad . \quad . \quad (24)$$

and

$$[0][1][-1]-\Theta_1^2\{[1]+[-1]\}=0. \quad . \quad (25)$$

For the sake of distinctness it will be well to repeat here that

$$[0]=c^2-\Theta_0, \quad [-1]=(c-2)^2-\Theta_0, \quad [1]=(c+2)^2-\Theta^0.$$

Substituting these values in (25), Mr. Hill obtains

$$(c^2-\Theta_0)\{(c^2+4-\Theta_0)^2-16c^2\}-2\Theta_1^2\{c^2+4-\Theta_0\}=0,$$

and neglecting the cube of $(c^2-\Theta_0)$, as well as its product with Θ_1^2,

$$(c^2-\Theta_0)^2+2(\Theta_0-1)(c^2-\Theta_0)+\Theta_1^2=0;$$

and from this again

$$c^2=1+\sqrt{\{(\Theta_0-1)^2-\Theta_1^2\}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (26)$$

It appears, therefore, that c is real or imaginary according as $(\Theta_0-1)^2$ is greater or less than Θ_1^2. In the problem of the Moon's apse, treated by Mr. Hill,

$$\Theta_0=1{\cdot}1588439, \qquad \Theta_1=-0{\cdot}0570440;$$

and in the corresponding problem of the node, investigated by Prof. Adams,

$$\Theta_0=1{\cdot}17804{,}44973{,}149,$$
$$\Theta_1=0{\cdot}01261{,}68354{,}6.$$

In both these cases the value of c is real, though of course not to be accurately determined by (26).

Mr. Hill's results are not immediately applicable to the acoustical problem embodied in (1), in consequence of the omission of k, representing the dissipation to which all actual vibrations are subject. The inclusion of this term leads, however, merely to the substitution for $(c+2n)^2-\Theta_0$ in (8) of

$$(c+2n)^2-2ik(c+2n)-\Theta_0;$$

so that the whole operation of k is represented if we write

$(c-ik)$ in place of c, and (Θ_0-k^2) in place of Θ_0. Accordingly

$$\cos\pi(c-ik)=1-\nabla'(0), \quad . \quad . \quad . \quad . \quad . \qquad (27)$$

$\nabla'(0)$ differing from $\nabla(0)$ only by the substitution of Θ_0-k^2 for Θ_0.

If $1-\nabla'(0)$ lies between ± 1, $(c-ik)$ is real, so that

$$c=ik\pm\alpha+2n. \quad . \quad . \quad . \quad . \quad . \quad . \qquad (28)$$

In this case both solutions are affected with the factor e^{-kt}, indicating that whatever the initial circumstances may be, the motion dies away.

It may be otherwise when $1-\nabla'(0)$ lies beyond the limits ± 1. In the case of most importance, when Θ_0 is nearly equal to unity, $1-\nabla'(0)$ is algebraically less than -1. If

$$\cos i\pi\beta=-1+\nabla'(0), \quad . \quad . \quad . \quad . \quad . \quad . \qquad (29)$$

we may write

$$c=1+i(k\pm\beta)+2n. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (30)$$

Here again both motions die down unless β is numerically greater than k, in which case one motion dies down, while the other increases without limit. The critical relation may be written

$$\cos(i\pi k)=-1+\nabla'(0). \quad . \quad . \quad . \quad . \quad . \quad . \qquad (31)$$

From (30) we see that, whatever may be the value of k, the vibrations (considered apart from the rise or subsidence indicated by the exponential factors) have the same frequency as if k, as well as Θ_1, Θ_2, &c. vanished.

Before leaving the general theory it may be worth while to point out that Mr. Hill's method may be applied when the coefficients of d^2w/dt^2 and dw/dt, as well as of w, are subject to given periodic variations. We may write

$$\Phi\frac{d^2w}{dt^2}+\Psi\frac{dw}{dt}+\Theta w=0, \quad . \quad . \quad . \quad . \quad . \quad . \qquad (32)$$

where

$$\Phi=\Sigma\Phi_n e^{2int}, \quad \Psi=\Sigma\Psi_n e^{2int}, \quad \Theta=\Sigma\Theta_n e^{2int}. \qquad (33)$$

Assuming, as before,

$$w=\Sigma_n b_n e^{ict+2int}, \quad . \quad . \quad . \quad . \quad . \quad . \qquad (34)$$

we obtain, on substitution, as the coefficient of $e^{ict+2imt}$,

$$-\Sigma_n b_n(c+2n)^2\Phi_{m-n}+i\Sigma_n b_n(c+2n)\Psi_{m-n}+\Sigma b_n\Theta_{m-n},$$

which is to be equated to zero. The equation for c may still be written

$$\mathfrak{D}(c)=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (35)$$

where

$$\mathfrak{D}(c)=\begin{vmatrix} \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ \ldots[-2,0], & [-1,-1], & [0,-2], & [1,-3], & [2,-4], & \ldots \\ \ldots[-2,1], & [-1,0], & [0,-1], & [1,-2], & [2,-3], & \ldots \\ \ldots[-2,2], & [-1,1], & [0,0], & [1,-1], & [2,-2], & \ldots \\ \ldots[-2,3], & [-1,2], & [0,1], & [1,0], & [2,-1], & \ldots \\ \ldots[-2,4], & [-1,3], & [0,2], & [1,1], & [2,0], & \ldots \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \end{vmatrix} \quad . \; . \quad (36)$$

and

$$[n,r]=(c+2n)^2\Phi_r-i(c+2n)\Psi_r-\Theta_r. \quad . \quad . \quad (37)$$

By similar reasoning to that employed by Mr. Hill we may show that

$$\begin{aligned}\mathfrak{D}(c)=&\,\mathrm{A}\,(\cos\pi c-\cos\pi c_0)\\ &+\mathrm{B}\,(\sin\pi c-\sin\pi c_0)\ldots,\end{aligned}$$

where A and B are constants independent of c; and, further, that

$$\mathfrak{D}(0)=\mathrm{A}(1-\cos\pi c)-\mathrm{B}\sin\pi c. \quad . \quad . \quad . \quad (38)$$

If all the quantities Φ_r, Ψ_r, Θ_r vanish except Φ_0, Ψ_0, Θ_0, $\mathfrak{D}(0)$ reduces to the diagonal row simply, say $\mathfrak{D}'(0)$. Let c_1, c_2 be the roots of

$$\Phi_0\frac{d^2w}{dt^2}+\Psi_0\frac{dw}{dt}+\Theta_0 w=0, \quad . \quad . \quad . \quad . \quad (39)$$

then

$$\begin{aligned}\mathfrak{D}'(0)&=\mathrm{A}\,(1-\cos\pi c_1)-\mathrm{B}\sin\pi c_1,\\ &=\mathrm{A}\,(1-\cos\pi c_2)-\mathrm{B}\sin\pi c_2\,;\end{aligned}$$

so that the equation for c may be written

$$\begin{vmatrix}\mathfrak{D}\,(0), & 1-\cos\pi c, & \sin\pi c, \\ \mathfrak{D}'(0), & 1-\cos\pi c_1, & \sin\pi c_1, \\ \mathfrak{D}'(0), & 1-\cos\pi c_2, & \sin\pi c_2,\end{vmatrix}=0. \quad . \quad . \quad (40)$$

In this equation $\mathfrak{D}(0)\div\mathfrak{D}'(0)$ is the determinant derived from $\mathfrak{D}(0)$ by dividing each row so as to make the diagonal constituent unity.

If $\ldots\Psi_{-1}$, Ψ_0, $\Psi_1\ldots$ vanish (even though $\ldots\Phi_{-1}$, Φ_0, $\Phi_1\ldots$ remain finite), $\mathfrak{D}(c)$ is an even function of c, and the coefficient B vanishes in (38). In this case we have simply

$$\frac{1-\cos\pi c}{1-\cos\pi\sqrt{\Theta_0}}=\frac{\mathfrak{D}(0)}{\mathfrak{D}'(0)},$$

exactly as when Φ_1, Φ_{-1}, Φ_2, $\Phi_{-2}\ldots$ vanish.

Reverting to (24), we have as the approximate particular solution, when there is no dissipation,

$$w=\frac{e^{(c-2)it}}{(c-2)^2-\Theta_0}+\frac{e^{cit}}{\Theta_1}+\frac{e^{(c+2)it}}{(c+2)^2-\Theta_0}. \quad . \quad . \quad (41)$$

If c be real, the solution may be completed by the addition of a second, found from (41) by changing the sign of c. Each of these solutions is affected with an arbitrary constant multiplier. The realized general solution may be written

$$w=\frac{\mathrm{R}\cos(c-2)t+\mathrm{S}\sin(c-2)t}{(c-2)^2-\Theta_3}$$
$$+\frac{\mathrm{R}\cos ct+\mathrm{S}\sin ct}{\Theta_1}+\frac{\mathrm{R}\cos(c+2)t+\mathrm{S}\sin(c+2)t}{(c+2)^2-\Theta_0}, \quad . \quad . (42)$$

from which the last term may usually be omitted, in consequence of the relative magnitude of its denominator. In this solution c is determined by (26).

When c^2 is imaginary, we take

$$4s^2=\Theta_1{}^2-(\Theta_0-1)^2; \quad . \quad . \quad . \quad . \quad . \quad (43)$$

so that

$$c^2=1+2is, \quad c=1+is, \quad c-2=-1+is.$$

The particular solution may be written

$$w=e^{-st}\{\Theta_1 e^{-it}+(1-\Theta_0-2is)e^{it}\}; \quad . \quad . \quad . \quad (44)$$

or, in virtue of (43),

$$w=e^{-st}\{(1-\Theta_0+\Theta_1)\cos t+2s\sin t\}; \quad . \quad . \quad (45)$$

or, again,

$$w=e^{-st}\{\sqrt{(\Theta_1+1-\Theta_0)}\,.\cos t+\sqrt{(\Theta_1-1+\Theta_0)}\,.\sin t\}. \quad . \quad (46)$$

The general solution is

$$\left.\begin{aligned} w=\mathrm{R}e^{-st}&\{(1-\Theta_0+\Theta_1)\cos t+2s\sin t\} \\ +\mathrm{S}e^{st}\;&\{(1-\Theta_0+\Theta_1)\cos t-2s\sin t\} \end{aligned}\right\}, \quad . \quad . \quad (47)$$

R, S being arbitrary multipliers.

One or two particular cases may be noticed. If $\Theta_0=1$, $2s=\Theta_1$, and

$$\left.\begin{aligned} w=\mathrm{R}'e^{-st}&\{\cos t+\sin t\} \\ +\mathrm{S}'e^{st}\;&\{\cos t-\sin t\} \end{aligned}\right\}. \quad . \quad . \quad . \quad . \quad . \quad (48)$$

Again, suppose that

$$\Theta_1{}^2=(\Theta_0-1)^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (49)$$

so that s vanishes, giving the transition between the real and imaginary values of c. Of the two terms in (46), one or other preponderates indefinitely in the two alternatives. Thus, if $\Theta_1=1-\Theta_0$, the solution reduces to $\cos t$; but if $\Theta_1=-1+\Theta_0$, it reduces to $\sin t$. The apparent loss of generality by the merging of the two solutions may be repaired in the usual way by supposing s infinitely small.

When there are dissipative forces, we are to replace c by $(c-ik)$, and Θ by (Θ_0-k^2); but when k is small the latter substitution may be neglected. Thus, from (26),

$$c=1+ik+\tfrac{1}{2}\sqrt{\{(\Theta_0-1)^2-\Theta_1^2\}}. \quad . \quad . \quad . \quad (50)$$

Interest here attaches principally to the case where the radical is imaginary; otherwise the motion necessarily dies down. If, as before,

$$4s^2=\Theta_1^2-(\Theta_0-1)^2, \quad . \quad . \quad . \quad . \quad . \quad (51)$$

$$c=1+ik+is, \quad c-2=-1+ik+is, \quad . \quad . \quad (52)$$

and

$$w=\frac{e^{(c-2)it}}{(c-ik-2)^2-\Theta_0}+\frac{e^{c\,it}}{\Theta_1},$$

or

$$w=e^{-(k+s)t}\{\Theta_1e^{-it}+(1-\Theta_0-2is)e^{it}\},$$

or

$$w=e^{-(k+s)t}\{(1-\Theta_0+\Theta_1)\cos t+2s\sin t\}. \quad . \quad . \quad (53)$$

This solution corresponds to a motion which dies away.

The second solution (found by changing the sign of s) is

$$w=e^{(s-k)t}\{(1-\Theta_0+\Theta_1)\cos t-2s\sin t\}. \quad . \quad . \quad (54)$$

The motion dies away or increases without limit according as s is less or greater than k.

The only case in which the motion is periodic is when $s=k$, or

$$4k^2=\Theta_1^2-(\Theta_0-1)^2; \quad . \quad . \quad . \quad . \quad . \quad (55)$$

and then

$$w=(1-\Theta_0-\Theta_1)\cos t-2k\sin t. \quad . \quad . \quad . \quad (56)$$

These results, under a different notation, were given in my former paper*.

If $\Theta_0=1$, we have by (51), $2s=\Theta$; and from (53), (54),

$$w=\mathrm{R}e^{-(k+s)t}\{\cos t+\sin t\}+\mathrm{S}e^{-(k-s)t}\{\cos t-\sin t\}.. \quad . \quad (57)$$

---

* In consequence of an error of sign, the result for a second approximation there stated is incorrect.

In the former paper some examples were given drawn from ordinary mechanics and acoustics. To these may be added the case of a stretched wire, whose tension is rendered periodically variable by the passage through it of an intermittent electric current. It is probable that an illustration might be arranged in which the vibrations are themselves electrical. Θ_0 would then represent the stiffness of a condenser, Ψ_0 resistance, and Φ_0 self-induction. The most practicable way of introducing the periodic term would be by rendering the self-induction variable with the time (Φ_1). This could be effected by the rotation of a coil forming part of the circuit.

The discrimination of the real and imaginary values of c is of so much importance, that it is desirable to pursue the approximation beyond the point attained in (26). From (11) we find

$$\frac{\mathfrak{D}(1)}{\mathfrak{D}'(1)}=\frac{1+\cos(\pi c)}{1+\cos(\pi\sqrt{\Theta_0})}; \quad . \quad . \quad . \quad (58)$$

from which, or directly, we see that if $c=1$, corresponding to the transition case between real and imaginary values,

$$\mathfrak{D}(1)=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (59)$$

If, as we shall now suppose, Θ_2, Θ_3 ... vanish, (59) may be written in the form

$$\begin{vmatrix} \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ \ldots 1, & a_2, & 1, & 0, & 0, & 0 \ldots \\ \ldots 0, & 1, & a_1, & 1, & 0, & 0 \ldots \\ \ldots 0, & 0, & 1, & a_1, & 1, & 0 \ldots \\ \ldots 0, & 0, & 0, & 1, & a_2, & 1 \ldots \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \end{vmatrix}=0, \quad . \quad (60)$$

where

$$a_1=\frac{\Theta_0-1}{\Theta_1}, \quad a_2=\frac{\Theta_0-9}{\Theta_1}, \quad a_3=\frac{\Theta_0-25}{\Theta_1}, \quad . \quad (61)$$

The first approximation, equivalent to (26), is found by considering merely the central determinant of the second order involving only a_1; thus,

$$a_1^2-1=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (62)$$

The second approximation is

$$a_2^2\left\{\left(a_1-\frac{1}{a_2}\right)^2-1\right\}=0. \quad . \quad . \quad . \quad . \quad . \quad (63)$$

The third is

$$a_3^2\left\{a_2-\frac{1}{a_3}\right\}^2\left\{\left(a_1-\frac{1}{a_2-\frac{1}{a_3}}\right)^2-1\right\}=0, \quad . \; . \; . \quad (64)$$

and so on. The equation (60) is thus equivalent to

$$a_1-\frac{1}{a_2-}\;\frac{1}{a_3-}\;\frac{1}{a_4-}\ldots=\pm 1\,; \quad . \; . \quad (65)$$

and the successive approximations are

$$N_1=\pm D_1, \quad N_2=\pm D_2, \quad . \; . \; . \; . \; . \quad (66)$$

where

$$\frac{N_1}{D_1},\; \frac{N_2}{D_2},\ldots$$

are the corresponding convergents to the infinite continued fraction*.

In terms of Θ_0, Θ_1, the second approximation to the equation discriminating the real and imaginary values of c is

$$(\Theta_0-1)(\Theta_0-9)-\Theta_1^2=\pm\Theta_1(\Theta_0-9). \quad . \; . \; . \quad (67)$$

One of the most interesting applications of the foregoing analysis is to the case of a laminated medium in which the mechanical properties are periodic functions of one of the coordinates. I was led to the consideration of this problem in connexion with the theory of the colours of thin plates. It is known that old superficially decomposed glass presents reflected tints much brighter, and transmitted tints much purer, than any of which a single transparent film is capable. The laminated structure was proved by Brewster; and it is easy to see how the effect may be produced by the occurrence of nearly similar laminæ at nearly equal intervals. Perhaps the simplest case of the kind that can be suggested is that of a stretched string, periodically loaded, and propagating transverse vibrations. We may imagine similar small loads to be disposed at equal intervals. If, then, the wave-length of a train of progressive waves be approximately equal to the *double* interval between the loads, the partial reflexions from the various loads will all concur in phase, and the result must be a powerful aggregate reflexion, even though the effect of an individual load may be insignificant.

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* The relations of determinants of this kind to continued fractions has been studied by Muir (Edinb. Proc. vol. viii.).

The general equation of vibration for a stretched string of periodic density is

$$\left(\rho_0+\rho_1\cos\frac{2\pi x}{l}+\rho_1{}'\sin\frac{2\pi x}{l}+\rho_2\cos\frac{4\pi x}{l}\right.$$
$$\left.+\rho_2{}'\sin\frac{4\pi x}{l}+\ldots\right)\frac{d^2w}{dt^2}=\mathrm{T}\frac{d^2w}{dx^2}, \quad . \quad . \quad . \quad (68)$$

l being the distance in which the density is periodic. We shall suppose that $\rho_1{}', \rho_2{}', \ldots$ vanish, so that the sines disappear, a supposition which involves no loss of generality when we restrict ourselves to a simple harmonic variation of density. If we now assume that $w \propto e^{ipt}$, or $\propto \cos pt$, we obtain

$$\frac{d^2w}{d\xi^2}+(\Theta_0+2\Theta_1\cos 2\xi+2\Theta_2\cos 4\xi+\ldots)w=0, \quad . \quad . \quad (69)$$

where $\xi=\pi x/l$, and

$$\Theta_0=\frac{p^2l^2\rho_0}{\pi^2\mathrm{T}}, \quad 2\Theta_1=\frac{p^2l^2\rho_1}{\pi^2\mathrm{T}}, \text{ \&c.}; \quad . \quad . \quad . \quad (70)$$

and this is of the form of Mr. Hill's equation (2).

When c is real, we may employ the approximate solutions (41), (44). The latter (with ξ written for t) gives, when multiplied by $\cos pt$ or $\sin pt$, the stationary vibrations of the system. From (41) we get

$$w=\frac{\cos[pt+(c-2)\xi]}{(c-2)^2-\Theta_0}+\frac{\cos[pt+c\xi]}{\Theta_1}, \quad . \quad . \quad (71)$$

in which, if $c=1$ nearly, the two terms represent waves progressing with nearly equal velocities in the two directions. Neither term gains permanently in relative importance as x is increased or diminished indefinitely.

It is otherwise when the relation of Θ_0 to Θ_1 is such that c is imaginary. By (44) the solution for w, assumed to be proportional to e^{ipt}, now takes the form

$$\left.\begin{aligned} w=\mathrm{R}e^{-s\xi}\{\Theta_1e^{i(pt-\xi)}+(1-\Theta_0-2is)e^{i(pt+\xi)}\},\\ +\mathrm{S}e^{s\xi}\ \{\Theta_1e^{i(pt-\xi)}+(1-\Theta_0+2is)e^{i(pt+\xi)}\}.\end{aligned}\right\} \quad (72)$$

Whatever may be the relative values of R and S, the first solution preponderates when x is large and negative, and the second preponderates when x is large and positive. In either extreme case the motion is composed of two progressive waves moving in opposite directions, *whose amplitudes are equal in virtue of* (43).

The meaning of this is that a wave travelling in either direction is ultimately totally reflected. For example, we may so choose the values of R and S that at the origin of x there is a wave (of given strength) in the positive direction only, and we may imagine that it here passes into a uniform medium, and so is propagated on indefinitely without change. But, in order to maintain this state of things, we have to suppose on the negative side the coexistence of positive and negative waves, which at sufficient distances from the origin are of nearly equal and ever-increasing amplitudes. In order therefore that a small wave may emerge at $x=0$, we have to cause intense waves to be incident upon a face of the medium corresponding to a large negative x, of which nearly the whole are reflected.

It is important to observe that the ultimate totality of reflexion does not require a special adjustment between the frequency of the waves and the linear period of the lamination. The condition that c should be imaginary is merely that Θ_1 should numerically exceed $(1-\Theta_0)$. If λ be the wave-length of the vibration corresponding to e^{ipt} and to density ρ_0,

$$\frac{p^2\rho_0}{\pi^2\mathrm{T}}=\frac{4}{\lambda^2}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (73)$$

and thus the limits between real and imaginary values of c are given by

$$\frac{\lambda^2}{4l^2}-1=\pm\frac{\rho_1}{2\rho_0}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (74)$$

If ρ_1 exceeds these limits a train of waves is ultimately totally reflected, in spite of the finite difference between $\frac{1}{2}\lambda$ and l*.

---

* A detailed experimental examination of various cases in which a laminated structure leads to a powerful but highly selected reflexion would be of value. The most frequent examples are met with in the organic world. It has occurred to me that Becquerel's reproduction of the spectrum in natural colours upon silver plates may perhaps be explicable in this manner. The various parts of the film of subchloride of silver with which the metal is coated may be conceived to be subjected, during exposure, to *stationary* luminous waves of nearly definite wave-length, the effect of which might be to impress upon the substance a periodic structure recurring at intervals equal to *half* the wave-length of the light; just as a sensitive flame exposed to stationary sonorous waves is influenced at the loops but not at the nodes (Phil. Mag. March 1879, p. 153). In this way the operation of any kind of light would be to produce just such a modification of the film as would cause it to reflect copiously that particular kind of light. I abstain at present from developing this suggestion, in the hope of soon finding an opportunity of making myself experimentally acquainted with the subject.

In conclusion, it may be worth while to point out the application to such a problem as the stationary vibrations of a string of variable density fixed at two points. A distribution of density,

$$\rho_0+\rho_1\cos\frac{2\pi x}{l}+\rho_2\cos\frac{4\pi x}{l}+\ldots\quad.\quad.\quad.\quad.\quad(75)$$

is symmetrical with respect to the points $x=0$ and $x=\frac{1}{2}l$, and between those limits is arbitrary. It is therefore possible for a string of this density to vibrate with the points in question undisturbed, and the law of displacement will be

$$w=\cos pt\left\{\mathrm{A}_1\sin\frac{2\pi x}{l}+\mathrm{A}_2\sin\frac{4\pi x}{l}+\mathrm{A}_3\sin\frac{6\pi x}{l}+\ldots\right\}.\ (76)$$

When, therefore, the problem is attacked by the method of Mr. Hill, the value of c obtained by the solution of (69) must be equal to 2. By (15) this requires

$$\square(0)=0.\quad.\quad.\quad.\quad.\quad.\quad.\quad.\quad(77)$$

This equation gives a relation between the quantities Θ_0, Θ_1, $\Theta_2,\ldots$; and this again, by (70), determines p, or the frequency $(p/2\pi)$ of vibration.

Since $\Theta_0=4$ nearly, the most important term in (17) is that involving $\Theta_2{}^2$. The first approximation to (77) gives

$$\Theta_0=4+\Theta_2\ ;$$

whence, by (70),

$$\left(\frac{2\pi}{p}\right)^2=\frac{l^2(\rho_0-\frac{1}{2}\rho_2)}{\mathrm{T}}.\quad.\quad.\quad.\quad.\quad.\quad.\quad(78)$$

To this order of approximation the solution may be obtained with far greater readiness by the method given in my work on Sound*; but it is probable that, if the solution were required in a case where the variation of density is very considerable, advantage might be taken of Mr. Hill's determinant $\square(0)$. There are doubtless other physical problems to which a similar remark would be applicable.

Terling Place, Witham,
June 19, 1887.

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* 'Theory of Sound,' vol. i. § 140. In comparing the results, it must be borne in mind that the length of the string in (78) is denoted by $\frac{1}{2}l$.

XVIII. *On the Production of Sudden Changes in the Torsion of a Wire by Change of Temperature.* *By* R. H. M. Bosanquet, *St. John's College, Oxford*[*].

[Plates VI. & VII.]

THE wire which forms the subject of the following observations is the finest platinum wire which can be procured; its diameter is about ·001 inch, and the length of the piece used was about 3 feet 6 inches. It originally formed the suspension of a galvanometer having a pair of nearly astatic needles about 7 inches long. The steel needles could be replaced by brass ones for the purpose of examining the properties of the suspension.

During the summer no disturbances worth attention were noticed; but in the winter, when the use of the stove occasioned rapid changes of temperature, disturbances became evident. The brass needles were employed throughout the present investigation.

The tables and diagrams which follow, except the last, all refer to one and the same state of the wire, the suspended weight not having been lifted during their progress. The lifting of the weight entirely changes the condition of the wire so far as these phenomena are concerned. The last table and diagram exhibit a subsequent state in which the phenomena are less distinct and in reversed direction.

The angular position of the needles was read on a scale of degrees attached to the circular glass shade surrounding them. This position, which we may call the elongation, is the abscissa of the diagrams. The temperature was measured by a very sensitive thermometer with a long spiral bulb, hung up alongside of the wire. The temperature in degrees Fahrenheit forms the ordinate of the diagrams.

The principal effects are as follows:—The elongation varies between the limits of about 21° and 85°. Rising temperature drives it up to about 85°, where it remains while the temperature continues to rise. Falling temperature drives it down to about 21°, where it remains while the temperature continues to fall. The change of elongation is usually complete or nearly so by the time that the reversed change of temperature amounts to from 2° to 5°.

If the change of temperature is reversed as soon as the elongation reaches either of its limits, the line of the diagram returns along its original course: see diagram, April 27th.

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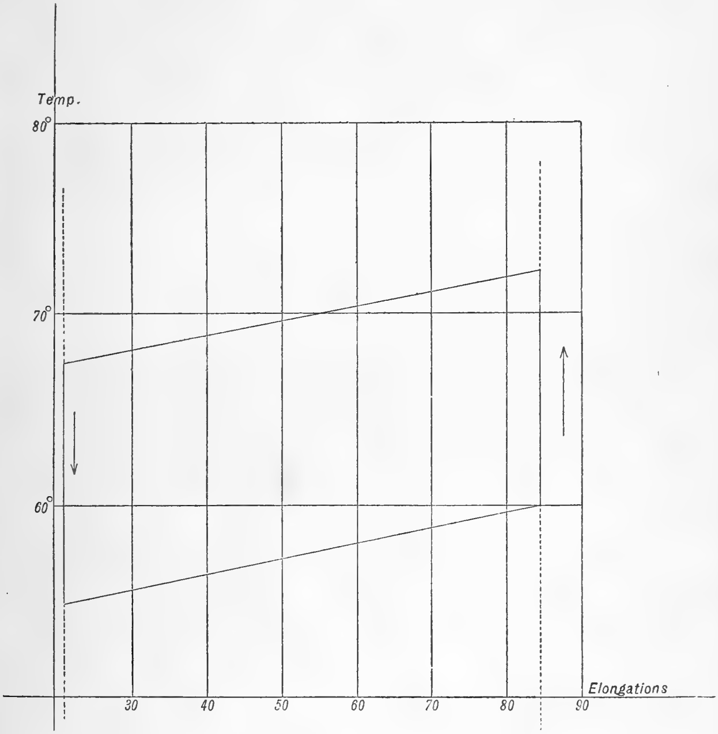
[*] Communicated by the Physical Society: read May 14, 1887.

(The zigzags in the middle of this diagram arise from the rapid movement setting the needles in vibration.)

If the change of temperature is pushed in each direction after the limiting elongation is attained, we obtain a cycle of events which can be most simply represented by a parallelogram or series of parallelograms, such as the following:—



Here the vertical lines represent the limits of elongation, the one corresponding to rising temperature, the other to falling temperature. On reversal of the direction of change of temperature such lines as the sloping lines are described. The inclination of these is nearly constant, and they may lie at any height between the lines of limiting elongation.

The above figure is a typical representation of the character of the principal effect; but the lines are never really straight, and there are subsidiary effects, which, however, in the present case are small compared with that above described.

The tables and figures at the end of this paper explain themselves.

There was some difficulty in obtaining that part of the curve which corresponds to the lower horizontal line of the typical parallelogram, as the corresponding change of temperature usually took place in the small hours of the morning. The diagram May 4th–5th shows a case in which this part of the curve was obtained. This exhibits a subsidiary effect, which consists of a tendency to increase the elongation * in the neighbourhood of the lower limit when the temperature is constant. Another very clear case of this effect will be found on the diagram for April 28th. My impression is that conditions of wire are common in which this effect is considerably developed. In such cases changes of temperature give rise to excursions, after which there is a more or less rapid return to a mean position.

The observations and diagrams hitherto referred to deal with changes in the angular position of a body hanging from the wire at rest. Observations were also made of the periods of vibration, for the purpose of ascertaining what changes took place in the torsional rigidity of the wire. The results of these observations are as follows:—

Periods of vibration.

Mean period.		Mean elongation.	Number of observed periods.
m.	s.		
4	45	89·2	4
4	53	88·5	4
4	41	85·4	13
5	0	84·6	4
5	39	83·7	4
6	3	77·0	4
5	52	72·1	12
6	33	57·9	4
4	23	54·6	2
5	33	46·4	4
5	16	37·0	2
3	56	35·0	8
3	4	32·3	2
2	29	28·6	2
2	40	27·5	4

It appears from these numbers that the period of vibration is least when the elongation is in the neighbourhood of the lower limit, increases rapidly as the limit is departed from up to a maximum value, and diminishes again slightly as the upper limit is approached and attained; that is to say, the torsion force has

a maximum at the lower limit of elongation;
a minimum between the limits;
and an increase up to the upper limit.

The observations for the period are necessarily complicated

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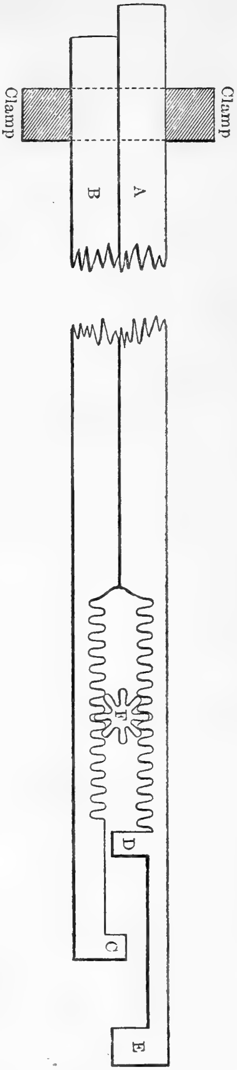
* *I. e.* to return from the lower limit towards the mean position.

by the changes in the position of rest, and they are not of great accuracy or consistency; but there can be no doubt that the course of the values is as above described.



It only remains to suggest what may be possible in the way of explanation of these phenomena. It appears to me that the only way of throwing any light on them is to imagine a mechanism capable of producing angular changes similar to those in question under the influence of changes of temperature.

A B are materials differently expansible by heat, capable of shifting, but with difficulty, through the clamp which binds them together.

Suppose C to be free between D and E; then any change of temperature will cause C to shift its position between D and E, and the pinion F will be turned round by the racks between which it lies. As soon as C comes up either against D or E the rotation of F is stopped, and the bars are compelled to shift in the clamp, if there is further change of temperature.

This arrangement exactly reproduces the typical changes observed in the present experiments. It is intended to serve the purpose of a formula.

The suggestion which we obtain from the above mechanism is that of loose contact between the parts involved, in this case probably between the molecules of the metal. As these peculiarities appear to originate in the extension of the wire by the suspended weight, it is possible that a partial separation of the molecules involving loose contact may arise out of this extension.

In any such case the molecules would bed against each other at the opposite parts of the loose contact, under the conditions which would give rise to either of the limiting elongations in the above experiments. The whole framework would be much stiffer when the loose contacts were thus bedded together than when they were free. In this way it would be possible to account for the increased values of the torsion force at the limiting elongations, and its lesser values in the intermediate region.

It would be easy to imagine several types of framework with loose contacts which would be capable of realizing the phenomena; but at present evidence in support of any particular arrangement would be wanting.

Observations of Temperature and Elongation.

Time.	April 25th.		April 26th.		April 27th.		April 28th.		April 29th.		April 30th.	
	Temp.	Elongation.	Temp.	Elongation.	Temp.	Elongation.	Temp.	Elongation.	Temp.	Elongation.	Temp.	Elongation.
9 A.M.	46°·8	81·5	45°·7	77·0	46°·3	75·5	48°·8	75·0	50°·0	65·7	47°·2	81·5
9.15	47·8	84·0	46·0	77·0	47·5	79·2	49·5	77·0	50·4	71·3	49·0	82·0
9.30	48·0	78·0	46·7	78·5	48·1	80·2	50·5	80·0	51·0	75·0	49·8	82·0
9.45	48·2	79·0	47·2	78·5	49·2	81·0	51·7	80·5	51·5	77·5	50·3	81·5
10.0	48·7	78·4	49·0	79·0	50·9	81·0	55·0	80·5	51·7	78·3	54·0	82·5
10.15	49·0	79·0	48·7	79·2	54·0	81·0	60·6	80·0	52·1	79·5	58·0	83·3
10.30	49·8	78·3	50·3	79·8	57·3	81·8	63·8	83·5	53·8	80·5	62·0	84·0
10.45	50·5	78·0	49·5	79·2	60·2	81·2	67·5	84·0	58·4	79·5	66·2	81·5
11.0	51·0	78·3	49·7	78·8	62·5	81·3	70·6	81·6	63·5	78·3	71·5	82·0
11.15	51·4	78·3	49·5	79·2	63·3	82·2	72·4	81·2	68·3	80·2	74·1	80·8
11.30	51·6	78·3	49·6	79·2	65·2	82·5	73·3	81·2	69·6	80·5	76·2	81·0
11.45	51·6	78·8	49·7	79·4	66·5	83·0	73·5	81·5	70·0	81·5	76·2	81·2
12.0	51·8	78·8	50·2	79·5	68·9	83·0	72·8	81·5	69·8	81·5	77·0	82·7
12.15	52·4	79·0	50·3	79·7	69·4	82·3	72·4	82·5	69·6	81·5	76·3	82·5
12.30	53·8	78·5	...	...	69·0	82·3	72·0	82·0	69·5	82·5	75·0	82·3
12.45	54·0	78·7	50·2	79·3	69·2	82·5	71·6	82·0	69·0	82·0	65·0	78·5
1 P.M.	54·0	78·7	50·5	79·8	68·6	82·0	71·3	82·5	68·8	82·0	78·0	64·6
2.0	54·2	79·2	49·7	78·0	76·5	85·0	70·7	83·0	66·8	82·0		
2.15	54·2	79·2	49·7	78·3	77·0	83·5	70·4	83·0	66·2	82·2		
2.30	54·4	79·0	49·6	78·0	77·7	83·5	70·0	82·5	66·0	83·0		
2.45	54·4	79·0	49·8	78·5	78·7	84·5	69·2	82·5	63·2	76·0		
3.0	54·5	78·8	50·6	80·2	80·3	84·5	68·6	84·0	61·4	50·5		
3.15	54·5	78·8	51·4	82·0	82·4	84·5	68·4	83·0	60·7	37·0		
3.30	54·7	78·8	53·4	82·0	84·3	84·0	68·1	79·8	59·9	29·6		
3.45	55·0	78·5	57·6	82·3	84·4	84·0	67·6	83·5	59·6	29·5	59·3	32·0
4.0	55·0	77·7	60·3	83·0	84·6	84·0	67·0	77·5	59·0	27·5		
4.15	54·5	77·0	61·2	83·0	72·4	77·0	66·2	68·5	56·1	22·5		
4.30	54·5	76·7	62·0	84·0	70·0	65·0	65·5	61·0	55·5	22·5		
4.45	54·3	77·5	63·1	84·0	68·0	33·0	64·8	47·7	55·2	22·5		
5.0	54·3	77·5	64·2	83·3	66·6	26·3	64·3	40·0	54·6	22·0		
5.15	...	...	...	...	...	...	63·7	34·5				
5.30	55·3	85·0	64·0	83·2	64·0	22·5	63·3	30·4	53·6	23·0		
5.45	55·5	88·0	62·6	83·2	66·0	27·0	61·4	24·3	53·6	22·0		
6.0	55·2	82·0	62·6	82·3	68·5	36·7	60·1	22·0	54·8	22·5		
6.15	55·0	76·0	61·2	82·3	68·3	36·8	59·0	21·0	52·8	20·8	56·5	25·5
6.30	55·0	71·3	60·7	82·2	69·5	60·0	59·6	21·8	51·8	21·7		
6.45	54·4	60·5	60·1	82·4	70·7	70·0	59·1	21·3	51·3	21·5		
7.0	53·7	50·3	59·5	82·0	74·5	85·0	56·0	21·3	50·7	22·0		
7.15	53·0	40·3	59·0	80·0	76·1	85·0	54·8	21·0	50·0	20·8		
7.30	52·5	33·0	58·7	79·2	76·5	84·3	54·6	21·0	49·2	22·5		
7.45	52·2	29·5	58·1	78·5	76·0	83·5	54·6	21·5	49·0	21·0		
8.0	52·0	27·0	57·8	75·0	75·7	82·0	54·6	21·5	48·7	21·0		

Time.	May 2nd.		May 3rd.		May 4th.		May 5th.		May 7th.		May 9th.	
	Temp.	Elongation.	Temp.	Elongation.	Temp.	Elongation.	Temp.	Elongation.	Temp.	Elongation.	Temp.	Elongation.
9 A.M.	46°·2	75·5	51°·7	83·0	56°·0	36·5	58°·0	83·0	50°·0	122·5	57°·4	102·5
9.15	46·5	77·5	...	...	...	...	55·3	69·0	50·2	122·0	58·7	98·0
9.30	46·7	78·5	...	...	54·1	23·5	55·0	59·0	50·5	119·8	59·2	96·8
9.45	47·0	78·5	...	...	53·8	23·2	54·9	55·5	51·8	115·0	60·5	95·0
10.0	47·6	80·0	...	...	53·5	23·0	...	...	52·3	106·0	61·5	92·0
10.15	48·2	80·5	...	...	53·3	23·5	...	...	53·5	101·0	64·3	84·5
10.30	49·3	81·0	...	...	53·0	23·5	...	...	56·8	90·0	70·0	83·0
10.45	50·4	80·5	65·3	87·5	52·8	23·5	...	...	...	...	72·7	81·5
11.0	51·5	81·0	68·0	87·0	52·8	24·0	...	...	...	...	76·2	81·8
11.15	...	...	69·0	85·0	52·6	24·5	...	...	69·8	78·5	78·8	80·8
11.30	57·5	85·0	70·2	86·0	52·7	26·0	...	...	71·5	80·7	80·2	80·0
11.45	...	...	70·4	85·0	52·7	29·0	...	...	73·5	79·0	82·0	80·0
12.0	62·5	82·0	...	...	52·7	30·5	...	...	65·1	82·5	82·0	80·3
12.15	...	...	...	...	53·0	30·0	...	...	63·3	87·5	81·7	80·5
12.30	67·0	83·5	...	...	52·7	30·0	...	...	61·5	94·5	81·5	81·0
12.45	67·2	82·0	...	...	52·6	29·0	...	...	60·7	96·5	81·2	81·5
1 P.M.	68·0	82·0	...	...	52·8	33·5	...	...	61·2	98·5	81·0	82·5
2.0	69·2	83·7	69·7	86·0	52·6	38·5	...	...	...	...	79·3	92·0
2.15	60·8	79·0	...	...	52·6	39·5	...	...	...	...	69·2	117·5
2.30	60·2	76·5	...	...	52·7	40·5	...	...	...	...	68·2	124·5
2.45	57·8	70·0	...	...	52·2	39·5	...	...	...	...	67·4	123·0
3.0	...	...	...	...	52·2	41·0	...	...	...	...	67·3	123·0
3.15	57·5	59·0	...	...	...	...	...	...	...	...	67·0	123·0
3.30	58·4	60·5	...	...	...	...	...	...	...	...	66·0	122·8
3.45	...	...	...	...	51·8	34·5	...	...	...	...	65·1	122·8
4.0	60·3	86·0	...	...	...	...	...	...	...	...	65·0	123·0
4.15	...	...	...	...	...	...	...	...	...	...	65·0	123·0
4.30	58·5	69·0	...	...	51·0	34·5	...	...	...	...	65·5	123·5
4.45	...	...	...	...	...	...	...	...	...	...	64·5	123·0
5.0	57·5	45·0	62·5	40·0	50·8	36·0	...	...	...	...	65·0	123·0
5.15												
5.30	56·0	33·0	61·5	30·0	50·7	40·0	...	...	...	...	64·3	123·0
5.45	...	...	...	...	...	...	...	...	...	...	64·3	123·0
6.0	53·6	26·0	...	...	50·5	41·5	...	...	...	...	65·0	123·0
6.15	...	...	...	...	...	...	...	...	...	...	63·5	123·0
6.30	52·5	22·7	...	...	50·5	42·5	...	...	...	...	63·5	123·0
6.45	...	...	...	...	...	...	...	...	59·7	121·5	63·0	123·0
7.0	51·8	22·0	59·0	23·0	50·8	43·5	...	...	...	...	62·8	123·0

After May 5th the wire was readjusted, and the subsequent readings refer to an altered condition of the wire.

Supplementary Observations.

Date.	Time.	Temp.	Elongation.
April 25	9.30 P.M.	49°·0	24·5
" 25	10.0	48·5	23·0
" 26	10.45	50·8	22·5
" 27	9.20	63·7	23·0
" 28	10·20	54·5	25·0
" 29	9.20	50·0	24·5
" 29	11.0	48·6	25·5
" 30	11.20	45·8	23·5
May 4	10.0	48·6	40·0
" 4	11.0	56·8	86·0
" 5	11.0	51·8	118·0
" 6	7.45	51·0	124·0
" 6	9.20	52·5	124·5
" 8	9.15	59·0	124·0
" 10	8.50 A.M.	54·7	143·5

XIX. *On the Value of "γ" for a Perfect Gas.*

By CHARLES V. BURTON, *B.Sc. (Lond.).*[*]

THE ratio, γ, between the two elasticities or the two specific heats has been measured, in the case of gases, by various experimental methods. Some of these methods are based on the assumption that the gas in question may be considered approximately perfect.

The object of the following investigation is to show that γ has the same value ($\frac{5}{3}$) for all perfect gases, and can be calculated for this hypothetical class of bodies from purely theoretical considerations.

Let us take a certain portion of perfect gas whose volume is v, pressure p, and molecular kinetic energy W. The isothermal elasticity of the gas under these conditions,

$$E_t = p.$$

Now, in order to find the isentropic elasticity, let the gas be compressed adiabatically until its volume is diminished by dv, the volume after compression being $v_1 = v - dv$; and the pressure being at the same time increased to $p_1 = p + dp$.

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[*] Communicated by the Author.

Let the work done in compressing the gas be dW; and W_1 the kinetic energy after compression. Then, since the gas is a perfect one,

$$\begin{aligned} W_1 &= W + dW \\ &= W + p\,dv \text{ in the limit.} \end{aligned}$$

Now, by a well-known relation, we have

$$\left.\begin{aligned} pv &= \tfrac{2}{3}W, \\ p_1v_1 &= \tfrac{2}{3}W_1 = \tfrac{2}{3}(W + p\,dv); \end{aligned}\right\}$$

$$\begin{aligned} \therefore\ p_1 &= \frac{\tfrac{2}{3}(W + p\,dv)}{v_1} \\ &= \frac{pv + \tfrac{2}{3}p\,dv}{v - dv} \\ &= p\left(1 + \frac{2}{3}\cdot\frac{dv}{v}\right)\left(1 + \frac{dv}{v}\right) \\ &= p\left(1 + \frac{5}{3}\cdot\frac{dv}{v}\right); \end{aligned}$$

$$\therefore\ dp = p_1 - p = \frac{5}{3}p\cdot\frac{dv}{v}.$$

Therefore the isentropic elasticity

$$\begin{aligned} E_\phi = v\cdot\frac{dp}{dv} &= \frac{5}{3}p \\ &= \frac{5}{3}E_t. \end{aligned}$$

So that $\gamma = \dfrac{5}{3}$ for all perfect gases.

XX. *On the Law of Molecular Force.*
By William Sutherland, *M.A., B.Sc.*[*]

[Concluded from p. 134.]

It is not difficult to illustrate how profoundly the relations of pressure and density of saturated vapour may be affected by capillary action at temperatures near the critical. Sir William Thomson has shown (Proc. Roy. Soc. Edinb., Feb. 1870) that, if inside a closed vessel containing a liquid and its vapour a capillary tube dips into the liquid, the pressure of saturation p of the vapour in contact with the free curved surface of the liquid in the capillary tube is connected with ϖ, the pressure of saturation of the vapour in contact with the plane surface, by the equation

$$p=\varpi-\frac{\mathrm{E}\sigma}{\rho-\sigma}\left(\frac{1}{r}+\frac{1}{r'}\right),$$

where E is the surface energy of the liquid per unit area, σ the average density of the vapour between the levels of the free surfaces of the liquid inside and outside the capillary tube, ρ the density of the liquid, r and r' are the principal radii of curvature of the curved surface of the liquid in the tube, reckoned as positive when the surface is concave to the vapour, that is when the liquid rises in the tube.

At temperatures near the critical, when σ becomes nearly equal to ρ, the factor $\frac{\sigma}{\rho-\sigma}$ becomes very large. It has been assumed by some writers that $\mathrm{E}\left(\frac{1}{r}+\frac{1}{r'}\right)$ vanishes at the critical temperature, and therefore becomes very small at temperatures near the critical, on the supposition that, as the critical temperature is the limiting temperature at which capillary elevation or depression can occur in a tube, the plane surface is naturally the limiting form which the free surface of the liquid in a capillary tube attains at the critical temperature. But that the plane surface is not a limiting case is shown by such an ordinary example as the convex meniscus of mercury in a glass tube. However, we have the definite experiments of Wolf (*Ann. de Chim. et de Phys.* 3 sér. xlix., 1857) to show that the plane surface is not the limiting form of a capillary meniscus at the critical temperature; for he found that sulphuric ether, sulphide of carbon, naphtha, and alcohol at temperatures near their

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[*] Communicated by the Author.

critical were depressed in capillary tubes of 2 millim. diameter, and presented markedly convex surfaces. Thus, for these substances, $E\left(\frac{1}{r}+\frac{1}{r'}\right)$ acquires a measurable negative value, and the whole expression $-\frac{E\sigma}{\rho-\sigma}\left(\frac{1}{r}+\frac{1}{r'}\right)$ may acquire a large positive value at temperatures near the critical. We thus see how the pressure of saturation of CO_2 for temperatures below but near the critical temperature, as obtained by experiments in capillary tubes, might differ greatly from those obtained by experiments in large vessels. This, then, illustrates the origin of the apparent conflict between the experiments of Regnault and of Andrews.

Some experiments of Kayser's (Wiedemann's *Ann.* xv.) bring out in a clear manner the difference between the laws of compressibility and dilatation of a fluid as studied in capillary tubes and in large vessels. When SO_2 is compressed at 0° C. in a vessel containing a quantity of powdered glass, it shows no pressure of saturation; its isothermal is a continuous curve representing quite a different relation between pressure and volume from that which holds when SO_2 is compressed free from capillary restraint.

The full explanation of these facts will be forthcoming only when the capillary theory of Laplace and Gauss receives completion in regard to the matter which, as it left their hands, it has proved inadequate to explain, namely, the influence of temperature in capillary phenomena. The equation of Gauss for the potential energy of a mass of liquid enclosed in a solid vessel contains three terms, representing the potential energy of the liquid due to gravity, that which corresponds to the mutual attractions of its molecules, and that which corresponds to the attractions between the molecules of the liquid and those of the solid; four terms should be added to make the equation complete as regards the whole mass of a fluid partly liquid and partly vapour,—namely, a term to represent the potential energy of the vapour due to gravity, that which corresponds to the mutual attractions of the molecules of vapour, that which corresponds to the actions between liquid and vapour, and finally that which corresponds to the actions between solid and vapour.

Having discovered that the discarded equation is thus in excellent agreement with Regnault's work on CO_2 in bulk, and that the differences between its results at 35°·5 in the neighbourhood of the critical point and those of Andrews and Amagat are traceable entirely to the experimental circumstances, I resumed my study of it and proceeded to test its power to give the pressures of saturated vapour of CO_2 as determined by Thilorier (*Ann. de Chim. et de Phys.* 2 sér. lx. 1835), Mitchell (Journ. Franklin Inst. xxvi. 1838), Faraday (Phil. Trans. 1845), Regnault (*Mém. de l'Acad. des Sciences,* xxvi.), and Andrews (Proc. Roy. Soc. xxiii. 1874–75).

To Maxwell (Journ. Chem. Soc. 1875) we owe the method of obtaining the pressure of saturation of a fluid at any temperature from its characteristic equation. Accepting James Thomson's suggestion that the isothermal for a fluid below its critical temperature, when traced by means of the characteristic equation for the fluid, ought to be a continuous curve lying partly above and partly below the isopiestic of saturation, he showed from thermodynamical considerations that the area enclosed by the part above the isopiestic should be equal to that enclosed by the part below. Expressed in symbols, this is

$$P(v_3-v_1)=\int_{v_1}^{v_3} p\,dv,$$

where P is the pressure of saturation at temperature T, v_3 is the volume of the saturated vapour at T°, and v_1 the volume of the liquid at T° and pressure P; so that v_3 and v_1 are the greatest and least of the three real roots for v of the characteristic equation, when the temperature has the value T and the pressure the value P. Applying this condition to the equation for CO_2, we obtain the following equation for the pressure of saturation at T° as a function of T:—

$$P(v_3-v_1)=\int_{v_1}^{v_3}\left(\frac{aT}{v}\,e^{\frac{b}{v\sqrt{T}}}+\frac{cT-l}{v^2}\right)dv,$$

where v_3 and v_1 are the greatest and least real roots of the equation

$$Pv=aTe^{\frac{b}{v\sqrt{T}}}+\frac{cT-l}{v}.$$

The only part of the integral which is not integratable is that involving the exponential factor, which, however, can be expressed as the difference of two exponential integrals which are now regarded as primary, since the tabulation of values for them by Soldner, Bretschneider, and more elaborately by J. W. L. Glaisher (Phil. Trans. 1870). However, as the values of v_3 and v_1 are indeterminable functions of P and T, we are unable to use the tables in the present application of the integral, and must content ourselves with determining P graphically—that is, by tracing the isothermal for certain definite numerical values of T, and then by trial drawing the isopiestic for P so that it cuts off equal areas.

In the subjoined table I give the pressures of saturation as found by the different experimenters and by Maxwell's principle with the characteristic equation. Two series of determinations by Regnault are given. Regnault is the only observer who has reduced his air-manometer pressure to true metres of mercury; but I have not thought it worth while to apply any correction to the results of the other observers, in view of the other larger sources of error that cannot be allowed for. I have simply multiplied their pressures in atmospheres by ·76. The first row of numbers gives the temperatures of observation and the second the corresponding saturation pressures.

Saturation Pressures of CO_2.

	Thilorier.	Mitchell.	Faraday.	Regnault, I.	Regnault, II.	Andrews (capill. tubes).	Maxwell's principle and equation.
Temp. C...	30·00	30·00	......	30·90	29·30	28·3	30
Pressure...	55·48	54·72	......	53·35	54·41	53·5	49
Temp. C...	0	0	0	0·25	0·18	0	0·21
Pressure...	27·36	27·36	29·26	27·17	26·82	26·63	26·2
Temp. C...	......	......	−26·10	−25·8	−25·5	......	−25·50
Pressure...	......	......	13·53	12·9	12·9	......	13·35

It will be seen that the equation gives results as good as are to be looked for in view of the differences in the different experimental determinations. The only number that is quite out of harmony with all the experimental determinations is 49, the pressure of saturation yielded by the equation for 30°; but when we see that at 30°·9 Regnault gets 53·35 in his first series, and that he finds a pressure greater by 1 metre, namely 54·41, in his second series for a temperature 1°·6 lower, which is equivalent to a difference of 2 metres out of 50 in measurements made at the same temperature, we cannot regard the difference of 49 from the experimental numbers as serious, especially when we find Faraday's estimate at 0° greater by about 2 metres out of 30 than the mean of Regnault's determinations at nearly the same temperature.

If we desire to carry the application of our equation to much lower temperatures, we are met by the difficulty that solid CO_2 melts according to Faraday at a temperature of $-57°$ under a pressure of 5·33 atmospheres; and according to Regnault, at a temperature of $-78°{\cdot}2$ solid CO_2 evaporates freely into the air under a pressure of 760 millim. Now at these temperatures and pressures a kilogramme of CO_2 can exist entirely in the solid, entirely in the liquid, and entirely in the gaseous state; so that its true and complete characteristic equation has three real roots for v corresponding to these three states, and it must also have two real roots corresponding to physically impossible homogeneous passage from the gaseous to the liquid and from the liquid to the solid states. Thus at these temperatures and pressures our equation ought to give five real roots for v, which it does not do, nor does any equation yet proposed. But it is enough for the present if we bridge the discontinuity between the gaseous and liquid states; much experimental work remains to be done before we can do so for the solid and liquid states by means of a single continuous equation. However, to get an idea as to how far the equation, when applied by means of Maxwell's principle, assumed still to hold, would succeed in giving the pressure of saturation at these temperatures, I made the necessary calculations, and found for the pressure of saturation at $-57°$ 5 metres of mercury, instead of 5·33 atmospheres as Faraday determined it, and at $-78°$ 1·5 metres or 2 atmospheres, instead of 1 atmosphere as Regnault found it or 1·14 atmosphere as Faraday.

When we pass on to test the equation by its power to give the volume of liquid CO_2 at different temperatures and pressures, we are met by remarkable discord amongst the experimental results. The subjoined table contains the volume in litres of a kilogramme of liquid CO_2 under pressures of 100, 200, and 300 atmospheres, as determined in capillary tubes by Cailletet and Hautefeuille (*Comptes Rendus*, xcii.) at the temperatures 0° and $-23°$ C., as determined in capillary tubes by Amagat for 18° C. (*Ann. de Chim. et de Phys.* 5 sér. xxii.), and as furnished by the equation for the same temperatures.

Volumes of 1 kilogramme of Liquid CO_2, in litres.

	18°.		0°.		−23°.	
	Amagat.	Equation.	Cailletet and Hautefeuille.	Equation.	Cailletet and Hautefeuille.	Equation.
100 atmosph.	1·169	1·266	1·016	1·153	0·916	1·064
200 „	1·065	1·142	0·962	1·071	0·888	1·010
300 „	1·025	1·064	0·931	1·016	0·869	0·971

It will be observed that the volumes given by the equation are all larger than those given by experiment. But at the lower pressures the disagreement is such as we are not unprepared to meet if we admit that capillary action alters the critical temperature from 42°·8 to 31°, and also produces the discrepancies between Amagat's and Andrews's determinations for 35°·5; for from the numbers tabulated before for 35°·5 we see that the deformation of the isothermals due to capillary action is greatest for pressures about 100 atmospheres, and we cannot expect the deformations to disappear in a sudden manner as we pass from 35°·5 to 18°. At the higher pressures the numbers given by the equation for 18° approach towards equality with those of Amagat; but the differences between the equation numbers and Cailletet and Hautefeuille's for the same temperature still amount to about 10 per cent. even at the high pressures. But the determinations of these experimenters and those of Amagat are not reconcilable with one another, as can be seen by forming the products pv and converting to Amagat's units, and plotting the results on Amagat's diagrams representing all his own experimental results (*Ann. de Chim. et de Phys.* 5 sér. xxii.); and in a less striking manner we can see the discrepancies by noting that the expansion between 0° and 18° as reckoned from Amagat's and Cailletet and Hautefeuille's results is much greater than that reckoned from −23° to 0°, although the latter interval is nearly 1·3 times the former. The following table shows this conflict in the experimental results :—

Changes of Volume of Liquid CO_2.

	From 0° to 18°.	From −23° to 0°.
100 atmospheres ...	·153	·100
200 „ ...	·103	·074
300 „ ...	·094	·062

The following table contains a comparison of the volumes in litres of a kilogramme of liquid CO_2 in contact with its saturated vapour at different temperatures, as determined by Thilorier (*Ann. de Chim. et de Phys.* 2 sér. lx., 1835), Mitchell (Journ. Franklin Inst. xxvi., 1838), and Cailletet and Mathias (*Comptes Rendus*, cii. May 1886), and by the equation.

Volumes, in litres, of a kilogramme of Liquid CO_2 in contact with its Saturated Vapour.

Temp.	Thilorier.	Equation.	Temp.	Mitchell.	Equation.	Temp.	Cailletet and Mathias.	Equation.
−20°	1·111	1·124				−23	0·965	1·124
0	1·205	1·242	0°	1·08	1·242	0	1·087	1·242
30	1·670	1·600	23	1·36	1·450	13	1·220	1·346
						22	1·380	1·443

While the equation is found to give results in excellent accord with Thilorier's determinations, it is in serious disaccord with those of Mitchell and of Cailletet and Mathias. Unfortunately Thilorier does not describe how he determined his densities; so that we cannot imagine the cause of such a large difference as that which exists between his determinations 1·205 at 0° and 1·087, the value found by Cailletet and Mathias, or 1·08 by Mitchell at the same temperature. Mitchell obtained his results by weighing the liquid in minute bulbs. Cailletet and Mathias first determined the density of saturated vapour of CO_2 at different temperatures in capillary tubes; then they introduced liquid CO_2 in unequal quantities into two vertical tubes containing mercury and communicating with one another by a horizontal tube containing mercury; the two tubes were brought to the temperature desired, and the difference of level of the mercury in them was observed, and also the height of each of the columns of liquid CO_2; the density of the liquid was then obtainable in terms of these measurements, and the determined density of the saturated vapour. The vapour-densities were determined from $-23°$ C. to 30°, and the liquid-densities from $-34°$ to 22°; and both sets of densities were then represented graphically in one diagram with temperatures for abscissæ and densities for ordinates. The two branches of the curve ought to pass into one another at the critical temperature; Cailletet and Mathias, completing the curve for the small portion that was not determined by experiment, according to the obvious tendencies of the determined portions, found the critical temperature to lie between 31° and 32°. Thus, again, we meet a determination of the critical temperature in discord with Regnault's work. Probably most of the discord can be traced to the determination of the vapour-densities in capillary tubes, in which it has been shown that the density of saturation near the critical temperature may be very different from the density of saturation of the vapour in contact with a plane surface of the liquid and at the same pressure. The form of Cailletet and Mathias's curve would have to be considerably altered if 42°·8 is the true critical temperature of CO_2; but how far a change in the density of the liquid would have to contribute to this alteration of form we cannot say.

Thus, then, with the experimental evidence at present available, we cannot construct any equation which will bridge over the discontinuity between the liquid and gaseous states satisfactorily; because, if it represents the results of some experiments, it will fail to represent others of equal weight. I can only draw attention to the fact that Thilorier, whose determinations of pressure agree excellently with Regnault's, found densities differing by as much as 15 per cent. from those of Cailletet and Mathias. There is certainly occasion for an experimental inquiry (and much promise of interesting results to those who have the facilities for making one) into the effect of extent of surface in contact with solid and curvature of surface on the compressibility and dilatability of fluids. There ought to be a physical effect corresponding to each term in the equation of Gauss for the energy of a fluid enclosed in a solid.

Before passing on to consider the equation in its thermodynamic aspect, it may be well to give an idea of its correspondence with experiment at low pressures and high temperatures. Amagat has given (*Comptes Rendus*, xciii., 1881) the following values of the ratio $\frac{pv}{p'v'}$ at different temperatures where p' is about 2·85 metres of mercury and $v=2v'$, and the values of the same ratio, as calculated from the equation, are furnished for comparison:—

Values of $\frac{pv}{p'v'}$.

	50°.	100°.	200°.	300°.
Amagat	1·0145	1·0087	1·0040	1·0020
Equation	1·0144	1·0086	1·0028	1·0001

The agreement is satisfactory at the high temperatures, if it is remembered that the values of the ratio given by experiment cannot be considered to be free from an error of at least 1 in 1000. But it must also be borne in mind how limited the range of temperature is which I was restricted to using in determining the form of Dr. Walter's function β, on account of the unknown amount of distortion which the isothermal for 35°·5 experiences through capillary action both in Amagat's and Andrews's experiments, a distortion which is most serious at the most important part of the curve. I have shown that, as regards CO_2, the experimental evidence is in such a state that no form can with finality be proved to be the one fit and proper form. The equation which I have given possesses the merit of representing the compressibility of CO_2 at 100° and at 70° up to a pressure of 400 atmospheres, of having revealed the important effect of capillary action on the circumstance of the critical state, and of giving the saturation pressures of CO_2 almost exactly as they have been experimentally determined.

The discussion, to which I proceed, of our equation in the light of the thermal effects studied by Thomson and Joule when gases are allowed to escape from under pressure through porous plugs, and of Regnault's experiments on the expansion of gases, is now of the utmost importance, as in these experiments we have to do with changes in the molecular potential energy.

According to the law of the inverse fourth power, without further hypothesis, we saw that the potential energy of a number of molecules is $\frac{2}{3}$ of the virial of their mutual attractions. Now, in the characteristic equation, $\frac{3}{2} \cdot \frac{l}{v}$ is the virial of the molecular attraction, and $\frac{3}{2} l \left(\frac{1}{v_1} - \frac{1}{v_2}\right)$ is the change of the virial on the expansion of the gas from volume v_1 to volume v_2. Hence the change of molecular potential energy due to the same expansion is $l\left(\frac{1}{v_1}-\frac{1}{v_2}\right)$. But, apart from molecular theory entirely, we have Thomson's thermodynamic equation for the cooling-effect δ which a gas experiences in expanding through a porous plug from volume v_1 to volume v_2,—

$$\mathrm{K}_p\delta=\int_{v_2}^{v_1}\left(\theta\frac{\partial p}{\partial\theta}-p\right)dv+p_2v_2-p_1v_1\,;$$

where it is to be noted that θ means temperature taken as the reciprocal of Carnot's function and measured on Thomson's absolute thermodynamic scale, K_p is the mean specific heat of the gas in dynamic units between its temperatures on the high- and low-pressure sides of the plug, p_2 and p_1 are the values of the pressure on the low- and high-pressure sides respectively, and v_2 and v_1 are the volumes occupied by a kilogramme of the gas at the pressures p_2 and p_1 and at the temperature which prevails on the high-pressure side of the plug.

From the characteristic equation we have

$$v\frac{\partial p}{\partial \mathrm{T}}=ae^{\frac{b}{v\sqrt{\mathrm{T}}}}\left(1-\tfrac{1}{2}\frac{b}{v\sqrt{\mathrm{T}}}\right)+\frac{c}{v}.$$

Within the experimental range of temperature we may consider

$$\frac{\partial p}{\partial \mathrm{T}}=\frac{\partial p}{\partial\theta},$$

and within the range of volumes we can replace $e^{\frac{b}{v\sqrt{\mathrm{T}}}}$ by $1+\frac{b}{v\sqrt{\mathrm{T}}}$; whence

$$v\frac{\partial p}{\partial\theta}=a+\tfrac{1}{2}\frac{ab}{v\sqrt{\mathrm{T}}}+\frac{c}{v}\text{ approximately;}$$

also

$$pv=a\mathrm{T}+\frac{ab}{v\sqrt{\mathrm{T}}}+\frac{c\mathrm{T}-l}{v}\text{ approximately;}$$

$$\therefore\int_{v_1}^{v_2}\left(\theta\frac{\partial p}{\partial\theta}-p\right)dv=\int_{v_1}^{v_2}\left\{\frac{a}{v}(\theta-\mathrm{T})-\frac{1}{v^2}\left(\frac{ab(2\mathrm{T}-\theta)}{2\sqrt{\mathrm{T}}}-(\theta-\mathrm{T})c-l\right)\right\}dv$$

$$=a\Delta\log\frac{v_2}{v_1}+\left(\frac{1}{v_1}-\frac{1}{v_2}\right)\left(l+\Delta c-\frac{ab(\mathrm{T}-\Delta)}{2\sqrt{\mathrm{T}}}\right),$$

where, as before, $\Delta=\theta-\mathrm{T}$.

Now, in the formation of the original equation for δ it is supposed that heat is imparted to the gas on the low-pressure side of the plug, until at the constant pressure p_2 prevailing there it acquires the temperature which it had on the high-pressure side. The escaping gas does work p_2v_2 on the atmosphere, but at the same time it has work p_1v_1 done on it by the gas behind, so that $p_2v_2-p_1v_1$ is the total external work done by the gas; but the gas also gains potential energy by expansion, so that the total loss of kinetic energy is equal to the sum of these two quantities. But $K_p\delta$ is the quantity of energy which has to be given to the gas after its loss of kinetic energy to bring its temperature back to what it was on the high-pressure side. Now, if the temperature of a body were a function of only the kinetic energy of its molecules, we could say that $K_p\delta$ is the change of the kinetic energy of a kilogramme of gas on account of the expansion; but the characteristic equation shows that temperature is not a function of only the kinetic energy, for it asserts that the kinetic energy of translation of the molecules in a kilogramme is $\frac{3}{2}\left(ae^{\frac{b}{v\sqrt{T}}}+\frac{c}{v}\right)T$, whence we see that we cannot consider $K_p\delta$ as giving accurately the total change of kinetic energy due to the performance of external work and the gain of potential energy. But if from $K_p\delta$ we subtract $p_2v_2-p_1v_1$, we shall get a remainder depending for the most part on the change of potential energy due to expansion. If, then, we bear in mind the values already given for the constants of the characteristic equation, and the value ·7 for Δ, we see from the last equation above that the greater part of the said remainder arises from the term $l\left(\frac{1}{v_1}-\frac{1}{v_2}\right)$, which, according to the molecular theory, expresses the change of molecular potential energy. The term $a\Delta\log\frac{v_2}{v_1}$ is small in comparison with $l\left(\frac{1}{v_1}-\frac{1}{v_2}\right)$; but its meaning is very important, as it is bound up with the difficult question of the dynamical explanation of the second law of thermodynamics. The term $\Delta c\left(\frac{1}{v_1}-\frac{1}{v_2}\right)$ may be taken with $a\Delta\log\frac{v_2}{v_1}$, but it is really negligible; while the last term, $\frac{ab\mathrm{T}}{2\sqrt{\mathrm{T}}}\left(\frac{1}{v_1}-\frac{1}{v_2}\right)$, neglecting Δ in the numerator, arises from the fact that the relation between the kinetic energy of the molecules of a gas and its temperature involves the volume occupied, and therefore varies with expansion. In fact, $-\frac{ab\mathrm{T}}{2\sqrt{\mathrm{T}}}\left(\frac{1}{v_1}-\frac{1}{v_2}\right)$ represents a heating-effect due to this cause that almost exactly neutralizes the cooling-effect, $a\Delta\log\frac{v_2}{v_1}$, within the range of the actual experiments; so that the part of the total cooling-effect not due to external work reduces, on numerical substitution, almost exactly to the value of the term $l\left(\frac{1}{v_1}-\frac{1}{v_2}\right)$. Thus, then, we see that the general discussion of the characteristic equation in the light of Thomson's equation for the cooling-effect shows that the potential energy of the molecules of a kilogramme of CO_2 may be taken as represented by $\frac{l}{v}$, and is therefore $\frac{2}{3}$ of the virial of their mutual attractions. Whence, in view of the general considerations adduced at the commencement of this paper, we may consider the law of the inverse fourth power to be established.

We will now proceed to calculate the total cooling-effects δ, and compare them with those obtained experimentally for CO_2 by Thomson and Joule ("Fluids in Motion," part ii., Phil. Trans. 1854; part iv., 1862; also 'Mathematical and Physical Papers of Sir William Thomson,' vol. i.; and Joule's Scientific Papers, vol. ii. pp. 247, 342) and by Regnault ("Sur la détente des Gaz," *Mém. de l'Acad. des Sciences*, xxxvii.).

Making the same approximations as previously, we get

$$p_2v_2-p_1v_1=\left(\frac{1}{v_1}-\frac{1}{v_2}\right)\left\{l-\left(\frac{ab}{\sqrt{T}}+c\right)T\right\};$$

$$\therefore\quad K_p\delta=\left(\frac{1}{v_1}-\frac{1}{v_2}\right)\left\{2l-\left(\frac{3ab}{2\sqrt{T}}+c\right)T\right\}+a\Delta\log\frac{v_2}{v_1}.$$

We see that, except for the small term $a\Delta\log\frac{v_2}{v_1}$, δ is proportional to $\frac{1}{v_1}-\frac{1}{v_2}$, or within the limits of pressure employed by Thomson and Joule to p_1-p_2. This was the first result discovered by Thomson and Joule; and, in accordance with it, they reduced their determinations to the one standard of the cooling-effect experienced by the gas in escaping through a porous plug into the atmosphere under a pressure in excess of the atmospheric by that of 100 English inches, or 2·54 metres of mercury.

In the following Table the numbers taken from Thomson and Joule's "Fluids in Motion," part ii., are marked II.; the others are from part iv. The values of K_p at different temperatures have been obtained by interpolating from

Regnault's data by the equation

$$K_p = 426(\cdot187 + \cdot00027t).$$

Cooling-effects experienced by CO_2 when escaping at different temperatures through a porous plug into the atmosphere under an excess of pressure of 2·54 metres of mercury.

Temperature C.	7°·38.	7°·96.	19°·1.	35°·6.	54°.	91°·5.	93°·5.	97°·5.
Observed by Thomson and Joule	4·367	4·215	3·876 II.	3·407	2·951	2·348 II.	2·164	2·135
Calculated from equation	3·940	3·920	3·670	3·330	3·010	2·470	2·453	2·395

When the great difficulty of these experiments with porous plugs is considered (they baffled Regnault himself, "Sur la détente des Gaz," p. 707), the agreement between the calculated and observed cooling-effects will be seen to be satisfactory. Regnault, while unable to get satisfactory results by the porous-plug method, obtained some valuable data by allowing gases to expand through a fine hole in a thin wall into a calorimeter, and also by allowing them to expand through silver capillary tubes of different lengths. As the excess of pressure which he employed was on the whole greater than the average excess in Thomson and Joule's experiments, and was such that we can no longer consider $\frac{1}{v_1} - \frac{1}{v_2}$ as proportional to $p_1 - p_2$, I have applied Thomson's equation for the cooling-effect to the actual circumstances of each of Regnault's experiments.

Cooling-effects experienced by CO_2 in escaping at different temperatures into the atmosphere under different pressure-excesses.

Temperature at the high pressure.	Excess of pressure, in millim. of mercury.	Observed by Regnault.	Calculated from equation.
−25° C.	7541	18°·73	15°·30
3	3354	5·43	5·50
3	7764	12·73	12·93
15	38591	81·96	73·80
100	4682	4·72	4·34
100	7921	9·88	7·30

If, for the moment, we set apart the results for 15° and −25°, on account of the large sources of experimental error incident to the employment of the very high excess of pressure (38591 millim., or 50 atmospheres) in the one set and the low temperature in the other, we find the agreement to be excellent, except in the case of the high excess (7921) at 100°, where Regnault found a cooling of 9°·88, and the equation gives 7°·30; but here the experiment is evidently at fault, because, while the pressure-excess is not much more than $\frac{3}{2}$ of 4682, the cooling-effect is more than double 4°·72—a result in complete disaccord with all Thomson and Joule's and Regnault's general results.

With regard to Regnault's number for 15°, obtained from capillary-tube experiments, we can see at once how the experiments are certain to yield too great a cooling-effect; for it is certain that the gas at the high-pressure end of the capillary tube, which is assumed to be all at the temperature 15° of the bath surrounding it, cannot be at that temperature quite close to the capillary tube, where the gas is doing work at the great rate due to the excess of pressure of 50 atmospheres; thus it is certain that the gas is cooled before it enters the capillary tube. Under these circumstances we may consider the agreement between the experimental cooling-effect 81°·96 and the theoretical one of 73°·8 to be all that could be expected. The same sort of remarks apply to the experimental determination at −25° (the number given above is the mean of five determinations made by Regnault).

Thus, then, the experiments of Regnault, which the great experimenter lamented as having cost him all too dear in thought, effort, and time for the results achieved, no less than those which cost the great English experimenters so much time valuable to science, are of fundamental value to molecular physics, if it is allowed that the agreement between them and the results deduced from the characteristic equation for CO_2 is such as to justify the statement that the potential energy of the molecules of a kilogramme of CO_2 occupying a volume v is $\frac{l}{v}$, and is equal to $\frac{2}{3}$ of the virial of the molecular attraction.

As Thomson and Joule and Regnault conducted expansion experiments on air, further light may be obtained by a brief discussion of them.

In constructing a characteristic equation for air I used Amagat's data for 16° (*Comptes Rendus*, xcix.), and his general result that, at 100° and up to 8-atmospheres' pressure, the departures of air from the Boyle-Mariotte law were too small to be measurable. These, with Regnault's value for the coefficient of expansion of air under a pressure of one atmosphere, and for the density of air, led to the following equation :—

$$pv = \frac{avT}{v-c} - \frac{l}{v};$$

with the following values for the constants when the unit of pressure is that of a kilogramme weight per square metre and the unit of volume is a cubic metre, v being the volume occupied by a kilogramme of air :—

$$a = 29{\cdot}3, \quad c = {\cdot}001056, \quad l = 12{\cdot}372.$$

This equation was intended for use only within the range of pressures of Thomson and Joule's experiments ; but it happens to give the critical temperature and pressure of air with considerable accuracy. When these are determined as for CO_2, we get the critical temperature as $-154°$ C., the critical pressure as 40 atmospheres, and the critical volume of a kilogramme of air as ·003167 cubic metres. Wroblewski's determinations (Wiedemann's *Ann.* xxvi. 1885) make the critical temperature about $-140°$ C. and the critical pressure about 40 atmospheres. We have seen in the case of CO_2 how a difference of $\pm 10°$ is possible between the true critical temperature of a gas and its critical temperature in a capillary tube ; hence the above equation for air, though constructed on limited data, is probably as accurate an equation as is possible with our present data. The values of the ratio $\frac{p_1v_1}{p_0v_0}$ given by it for 4° C. do not agree exactly with those given by Regnault's equation for 4°,

$$\frac{p_1v_1}{p_0v_0} = 1 - {\cdot}0011054\left(\frac{v_0}{v_1} - 1\right) + {\cdot}00001938\left(\frac{v_0}{v_1} - 1\right)^2.$$

Thus, for example, when $p_1 = 15$ atmospheres, Regnault's equation gives

$$\frac{p_1v_1}{p_0v_0} = 1{\cdot}0117;$$

while the equation above gives 1·0085. The difference is due to the fact that Regnault's determinations at 4° and Amagat's at 16° are not strictly reconcilable with one another. Amagat's were made in capillary tubes ; but as he has pushed his determinations to higher pressures than Regnault, so as to pass the point for which pv is a minimum, and is thus likely to have obtained with more accuracy the general sense of the changes in pv, I adopted his results, seeing that the discrepancy is more likely to be due to a slight misestimation on Regnault's part of the small differences of his ratios from 1 than to capillary action in Amagat's tube, which, at the temperature and pressure in question, must be slight; but the point is one not unworthy of being inquired into, whether the slight discord between Regnault's and Amagat's experiments is due to capillary action.

To determine from the equation the cooling-effects experienced by air in passing through porous plugs, we can write it in the form

$$pv = a\mathrm{T}\left(1+\frac{c}{v}\right)-\frac{l}{v};$$

$$\therefore\ v\frac{\partial p}{\partial \mathrm{T}} = a\left(1+\frac{c}{v}\right);$$

$$\therefore\ \theta\frac{\partial p}{\partial \theta}-p = a\frac{\Delta}{v}\left(1+\frac{c}{v}\right)+\frac{l}{v^2};$$

$$\therefore\ \mathrm{K}_p\delta = \int_{v_1}^{v_2}\left\{a\frac{\Delta}{v}\left(1+\frac{c}{v}\right)+\frac{l}{v^2}\right\}dv+p_2v_2-p_1v_1$$

$$= a\Delta\log\frac{v_2}{v_1}+ac\Delta\left(\frac{1}{v_1}-\frac{1}{v_2}\right)+l\left(\frac{1}{v_1}-\frac{1}{v_2}\right)+p_2v_2-p_1v_1.$$

Here, as before, $\mathrm{K}_p\delta$ is the quantity of energy which has to be imparted to the gas on the low-pressure side to bring, not the kinetic energy, but the temperature of the gas to its original value on the high-pressure side. If we subtract $p_2v_2-p_1v_1$ from this, and neglect the term involving c, on account of its smallness, we have $a\Delta\log\frac{v_2}{v_1}+l\left(\frac{1}{v_1}-\frac{1}{v_2}\right)$ as the amount of energy imparted to compensate not only for conversion of the amount of kinetic energy $l\left(\frac{1}{v_1}-\frac{1}{v_2}\right)$ into potential energy, but also to compensate for the cooling $a\Delta\log\frac{v_2}{v_1}$ which would occur even in a gas for which $l=0$, or in the ideal perfect gas.

Many writers have ignored this result of the difference Δ which Thomson and Joule demonstrated to exist between the temperature of melting ice, as measured on the absolute thermodynamic and the air-thermometers, and they have asserted that, for a perfect gas whose equation is $pv=a\mathrm{T}$ the cooling-effect obtainable must be zero; whereas, while the equation for air at temperatures near 100° C. may be written $pv=a\mathrm{T}$, Thomson and Joule were able to demonstrate a quite measurable cooling-effect at that temperature. Before the cooling-effect for a body can be zero, its equation must be

$pv = a\theta$. The import of the term $a\Delta \log \frac{v_2}{v_1}$ is therefore thermodynamical, and cannot at present be explained on purely molecular grounds. Its relative importance in the case of air is much greater than in that of CO_2; for at 0° C. its value is $\frac{3}{10}$ of that of $l\left(\frac{1}{v_1} - \frac{1}{v_2}\right)$, and at 100° it is $\frac{3}{8}$ of it. Taking account of the fact, then, that $a\Delta \log \frac{v_2}{v_1}$ has nothing to do directly with the forces acting between the molecules, we find, in the purely thermodynamic estimate of the energy imparted to the expanding gas to keep its temperature constant, the term $l\left(\frac{1}{v_1} - \frac{1}{v_2}\right)$. But this is the term which our theory of molecular force would lead us to expect as representing the change of kinetic energy due to increase of potential energy. Hence we may consider it as proved that the molecular potential energy of a kilogramme of air occupying a volume v is $\frac{l}{v}$.

For the total cooling effect we get, on substituting for $p_2v_2 - p_1v_1$ its value,

$$K_p\delta = a\Delta \log \frac{v_2}{v_1} + \left(\frac{1}{v_1} - \frac{1}{v_2}\right)\{2l - ac(T - \Delta)\}.$$

As v_1 and v_2 are directly proportional to T, if p_1 and p_2 have always the same values at different temperatures, the term $acT\left(\frac{1}{v_1} - \frac{1}{v_2}\right)$ is constant, and when evaluated numerically is almost equal to $a\Delta \log \frac{v_2}{v_1}$, within the range of Thomson and Joule's experiments; so that $K_p\delta$ reduces nearly, but not quite, to $2l\left(\frac{1}{v_1} - \frac{1}{v_2}\right)$; whence we find the cooling-effect proportional to $p_1 - p_2$, which is the experimental result.

Cooling-effects for Air escaping at different temperatures through porous plugs into the atmosphere under an excess of pressure of 100 English inches, or 2·54 metres of mercury.

Temperature C.	7°·1.	17°.	39°·5.	92°·8.
Observed by Thomson and Joule	·88	·86 II.	·75	·51
Calculated from equation	·96	·93	·86	·72

The calculated numbers are all larger than the observed; but we must take account of the smallness of the quantities and the magnitude of the possible experimental errors: thus, the experimental cooling for 17°, namely ·86°, is the mean of the results of seven series of experiments, the extremes being ·806° and ·958°. Moreover, Regnault's determinations seem to show that Thomson and Joule's results for air are perhaps somewhat too small. Thus, as the mean of six series of experiments at about 16° C., in which air expanded twelve times in succession through the fine hole between the successive chambers of a calorimeter before it passed from the high-pressure to the low-pressure states, Regnault found the cooling-effect due to a pressure-excess of 1 metre of mercury to be ·3548°, which corresponds to a cooling-effect of ·9012° for a pressure excess of 100 English inches. Again, Regnault, by expanding air through a silver capillary tube 2·5 metres long, found the cooling-effect at about 16° to be ·377° for a pressure-excess of 1 metre of mercury—that is, ·9576° for an excess of 100 English inches. These are the only two definite determinations made by Regnault for air; and we find that their mean, ·93°, is identical with the number given by the equation for 17°.

Van de Waals (Wiedemann *Beiblätter*, i.), with an equation founded on Regnault's determinations, obtains a cooling-effect at 17° almost identical with Thomson and Joule's result; but in his equation for the cooling-effect, the term $a\Delta \log \frac{v_2}{v_1}$, arising from the difference between the absolute thermodynamic and absolute air-thermometric scales, is omitted; if included, it would increase his theoretical effect by about ·3 of its own amount.

As far, then, as the experiments on the cooling of air by expansion are available as evidence, they go to show that the potential energy of the molecules of a kilogramme of air is $\frac{l}{v}$, and is equal to $\frac{2}{3}$ of the virial of the molecular attractions.

Thomson and Joule and Regnault also conducted experiments on the cooling of hydrogen by expansion; but its amount was too small for satisfactory measurement. Regnault obtained a slight cooling-effect; so also did Thomson and Joule in their researches (part ii.); but in their researches (part iv.) they found a slight heating-effect. The only conclusion possible is that the real effect for hydrogen is smaller than the possible experimental errors.

From Amagat's experiments on hydrogen I obtained the following equation :—

$$pv=\left(ae^{\frac{b}{v}}-\frac{c}{v}\right)\mathrm{T}-\frac{l}{v};$$

where

$$a=422{\cdot}14,\quad b={\cdot}012831,\quad c={\cdot}83497,\quad l=376{\cdot}5\,;$$

the unit of pressure being that of a kilogramme weight per square metre, and the unit of volume a cubic metre, v being the volume of a kilogramme of hydrogen. Amagat's unit of volume is $\frac{1}{3402}$ of that occupied by the gas at 0° and 760 millim. The critical temperature given by this equation is $\mathrm{T}=30^{\circ}{\cdot}56$ or $t=-242^{\circ}{\cdot}4$; the critical pressure is 22 atmospheres, and the critical volume of a kilogramme of hydrogen is ·02076 cubic metre.

Olszewski (*Comptes Rendus,* 101. p. 238) states that he has submitted hydrogen to pressures varying from 20 to 180 atmospheres at −220° C. without getting it liquefied. The temperature −220° was measured by means of a hydrogen-thermometer, on the assumption, I presume, that hydrogen under pressures near that of 1 atmosphere obeys Charles's or Gay-Lussac's law right down to that low temperature. The above equation fully justifies Olszewski's assumption, and his reliance on the hydrogen-thermometer ; for whether it is used as a constant-volume or constant-pressure thermometer, its indications for volume or pressure near the normal, as calculated by Charles's or Gay-Lussac's law, are not appreciably different from those calculated from the above equation. This is an important fact in Physics—that there should exist a substance capable of giving regular thermometric measurements almost down to absolute zero.

We may regard the above-determined critical temperature of hydrogen as remarkably near the truth, considering how far beyond Amagat's experimental range we are extrapolating. Wroblewski (*Comptes Rendus,* 100. p. 979), by suddenly expanding hydrogen cooled down to the melting-point of solid nitrogen, from a pressure of 180 atmospheres to that of 1 atmosphere, got signs of liquefaction such as Olszewski had also obtained in a similar manner, while his thermopile in the fluid indicated −208° or −211°; but he cannot say how nearly the thermopile itself actually came to the true temperature of ebullition of hydrogen under a pressure of 1 atmosphere.

The cooling effect for hydrogen at 0° C., expanding through plugs under a pressure-excess of 100 inches of mercury, is found from the above equation to be ·2°. Regnault found a cooling of ·04°, and Thomson and Joule (part ii.) found a cooling of ·07° ; but in their latest determination the effect found for hydrogen was an irregular heating-effect. This shows the amount of experimental error possible in these very difficult experiments, and also shows the degree of accuracy which is to be looked for in the experiments on air and CO_2.

The only other experiments that I know to be on record on cooling by expansion are those which Hirn made with steam (*Théorie Mécanique de la Chaleur*); but before they can be studied in the same manner as those on CO_2 and air have been in this paper, an equation on the pure "Virial" model will have to be constructed for steam.

The fields of application for the molecular law of the inverse fourth power are wide and inviting. In the theory of capillary action it ought to lead to the accurate determination of the size of the molecular domain in different substances under specified conditions, or, in other words, of the number of molecules in unit mass of all bodies. In the theory of elasticity its possibilities are vast; for, applied in connexion with the great accumulation of facts bearing on crystalline structure, it ought ultimately to render possible a general theory of the structure of molecules. But the most inviting direction for immediate research is towards the law of variation of l with variation in molecular constitution. It remains to be seen whether the experimental data at present available are sufficient to give a clue to it; but there is no doubt that the progress of molecular physics and chemical dynamics requires experimental data for the structure of the characteristic equations of a large number of bodies, for example of the members of many homologous series of organic compounds; for, before we can hope to understand the atomic forces at play in chemical action, we must possess a complete knowledge of molecular actions.

With $\frac{G}{r^2}+\frac{M}{r^4}$ to express the law of molic force through the whole range of distances from molecular up to astronomical, one is tempted to speculate whether the law of the terms representing atomic or chemic force may not be expressed by one or more higher powers of $\frac{1}{r^2}$, representing a force insensible at molecular distances as the molecular term of molic force is insensible at astronomical distances, but sensible at atomic distances, with the associated idea that atomic distances are exceedingly small compared to molecular. This conception would (speaking in a purely relative manner) reduce the molecules almost to mathematical points, and would almost remove the difficulty as to the collisions of molecules. In this manner we can endeavour to realize all the actions of matter on matter as pure attractions.

XXI. *Stability of Fluid Motion (continued from the May and June numbers).—Rectilineal Motion of Viscous Fluid between two Parallel Planes*[*]. *By* Sir W. THOMSON, *LL.D., F.R.S.*

27. SINCE the communication of the first of this series of articles to the Royal Society of Edinburgh in April, and its publication in the Philosophical Magazine in May and June, the stability or instability of the steady motion of a viscous fluid has been proposed as subject for the Adams Prize of the University of Cambridge for 1888[†]. The present communication (§§ 27–40) solves the simpler of the two cases specially referred to by the Examiners in their announcement, and prepares the way for the investigation of the less simple by a preliminary laying down, in §§ 27–29, and equations (7) to (12) below, of the fundamental equations of motion of a viscous fluid kept moving by gravity between two infinite plane boundaries inclined to the horizon at any angle I, and given with any motion deviating infinitely little from the determinate steady motion which would be the unique and essentially stable solution if the viscosity were sufficiently large. It seems probable, almost certain indeed, that analysis similar to that of §§ 38 and 39 will demonstrate that the steady motion is stable for any viscosity, however small; and that the practical unsteadiness pointed out by Stokes forty-four years ago, and so admirably investigated experimentally five or six years ago by Osborne Reynolds, is to be explained by limits of stability becoming narrower and narrower the smaller is the viscosity.

Let OX be chosen in one of the bounding planes, parallel to the direction of the rectilineal motion; and OY perpendicular to the two planes. Let the x-, y-, z-, component velocities, and the pressure, at (x, y, z, t), be denoted by $U+u, v, w$, and p respectively; U denoting a function of (y, t). Then, calling the density of the fluid unity, and the viscosity μ, we have, as the equations of motion[‡],

$$\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (1);$$

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[*] Communicated by the Author, having been read before the Royal Society of Edinburgh, July 18, 1887.

[†] See Phil. Mag. July 1887, p. 142.

[‡] Stokes's Collected Papers, vol. i. p. 93.

$$\left.\begin{aligned}
\frac{d}{dt}(\mathrm{U}+u)+(\mathrm{U}+u)\frac{du}{dx}+v\frac{d}{dy}(\mathrm{U}+u)+w\frac{dw}{dz}&=\mu\nabla^2(\mathrm{U}+u)-\frac{dp}{dx}+g\sin\mathrm{I},\\
\frac{dv}{dt}+(\mathrm{U}+u)\frac{dv}{dx}+v\frac{dv}{dy}+w\frac{dv}{dz}&=\mu\nabla^2 v-\frac{dp}{dy}-g\cos\mathrm{I},\\
\frac{dw}{dt}+(\mathrm{U}+u)\frac{dw}{dx}+v\frac{dw}{dy}+w\frac{dw}{dz}&=\mu\nabla^2 w-\frac{dp}{dz},
\end{aligned}\right\}\ (2);$$

where ∇^2 denotes the "Laplacian" $\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+\frac{d^2}{dz^2}$.

28. If we have $u=0$, $v=0$, $w=0$; $p=\mathrm{C}-g\cos\mathrm{I}\,y$; the four equations are satisfied identically; except the first of (2), which becomes

$$\frac{d\mathrm{U}}{dt}=\mu\frac{d^2\mathrm{U}}{dy^2}+g\sin\mathrm{I}\quad\ldots\ldots\quad(3).$$

This is reduced to

$$\frac{dv}{dt}=\mu\frac{d^2v}{dy^2}\quad\ldots\ldots\ldots\quad(4),$$

if we put

$$\mathrm{U}=v+\tfrac{1}{2}g\sin\mathrm{I}/\mu\,.\,(b^2-y^2)\quad\ldots\quad(5).$$

For terminal conditions (the bounding planes supposed to be $y=0$ and $y=b$), we may have

$$\left.\begin{aligned}v&=\mathrm{F}(t)\ \text{ when } y=0\\ v&=\mathfrak{F}(t)\quad\ ,,\quad y=b\end{aligned}\right\}\quad\ldots\ldots\quad(6),$$

where F and $\mathfrak{F}$ denote arbitrary functions. These equations (4) and (6) show (what was found forty-two years ago by Stokes) that the diffusion of velocity in parallel layers, *provided it is exactly in parallel layers*, through a viscous fluid, follows Fourier's law of the "linear" diffusion of heat through a homogeneous solid. Now, towards answering the highly important and interesting question which Stokes raised,—Is this laminar motion unstable in some cases?—go back to (1) and (2), and in them suppose u, v, w to be each infinitely small: (1) is unchanged; (2), with U eliminated by (5), become

$$\frac{du}{dt}+[v+\tfrac{1}{2}c(b^2-y^2)]\frac{du}{dx}+v\left(\frac{dv}{dy}-cy\right)=\mu\nabla^2u-\frac{dp}{dx}\quad..\quad(7),$$

$$\frac{dv}{dt}+[v+\tfrac{1}{2}c(b^2-y^2)]\frac{dv}{dx}=\mu\nabla^2v-\frac{dp}{dy}\quad..\quad(8),$$

$$\frac{dw}{dt}+[v+\tfrac{1}{2}c(b^2-y^2)]\frac{dw}{dx}=\mu\nabla^2w-\frac{dp}{dz}\quad..\quad(9);$$

where

$$c=g\sin\mathrm{I}/\mu.\quad\ldots\ldots\ldots\quad(10)$$

and, for brevity, p now denotes, instead of as before the pressure, the pressure $+g \cos \mathrm{I}\, y$.

We still suppose v to be a function of y and t determined by (4) and (6). Thus (1) and (7), (8), (9) are four equations which, with proper initial and boundary conditions, determine the four unknown quantities u, v, w, p; in terms of x, y, z, t.

29. It is convenient to eliminate u and w; by taking $\frac{d}{dx}, \frac{d}{dy}, \frac{d}{dz}$ of (7), (8), (9), and adding. Thus we find, in virtue of (1),

$$2\left(\frac{dv}{dy}-cy\right)\frac{dv}{dx}=-\nabla^2 p \quad . \quad . \quad . \quad . \quad (11).$$

This and (8) are two equations for the determination of v and p. Eliminating p between them, we find

$$\frac{d\nabla^2 v}{dt}-\left(\frac{d^2 v}{dy^2}-c\right)\frac{dv}{dx}+\left[v-\tfrac{1}{2}c(b^2-y^2)\right]\frac{d\nabla^2 v}{dx}=\mu\nabla^4 v \quad . \quad . \quad (12),$$

a single equation which, with proper initial and boundary conditions, determines the one unknown, v. When v is thus found, (8), (7), (9) determine p, u, and w.

30. An interesting and practically important case is presented by supposing one or both of the bounding planes to be kept oscillating in its own plane; that is, F and $\mathfrak{F}$ of (6) to be periodic functions of t. For example, take

$$\mathrm{F}=a\cos\omega t,\ \mathfrak{F}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

The corresponding periodic solution of (4) is

$$v=a\frac{\epsilon^{(b-y)\sqrt{\frac{\omega}{2\mu}}}-\epsilon^{-(b-y)\sqrt{\frac{\omega}{2\mu}}}}{\epsilon^{b\sqrt{\frac{\omega}{2\mu}}}-\epsilon^{-b\sqrt{\frac{\omega}{2\mu}}}}\cos\left(\omega t-y\sqrt{\frac{\omega}{2\mu}}\right) \quad . \quad . \quad (14).$$

In connexion with this case there is no particular interest in supposing a current to be maintained by gravity; and we shall therefore take $c=0$, which reduces (7), (8), (9), (11), (12), to

$$\frac{du}{dt}+v\frac{du}{dx}+\frac{dv}{dy}v=\mu\nabla^2 u-\frac{dp}{dx} \quad . \quad . \quad . \quad (15),$$

$$\frac{dv}{dt}+v\frac{dv}{dx}=\mu\nabla^2 v-\frac{dp}{dy} \quad . \quad . \quad . \quad (16),$$

$$\frac{dw}{dt}+v\frac{dw}{dx}=\mu\nabla^2 w-\frac{dp}{dz} \quad . \quad . \quad . \quad (17),$$

$$2\frac{dv}{dy}\frac{dv}{dx}=-\nabla^2 p \quad . \quad . \quad . \quad . \quad . \quad . \quad (18),$$

$$\frac{d\nabla^2 v}{dt}-\frac{d^2 v}{dy^2}\frac{dv}{dx}+v\frac{d\nabla^2 v}{dx}=\mu\nabla^4 v \quad . \quad . \quad . \quad . \quad . \quad . \quad (19);$$

in all of which v is the function of (y, t) expressed by (14).

These equations (15) ... (19) are of course satisfied by $u=0$, $v=0$, $w=0$, $p=0$. The question of stability is, Does every possible solution of them come to this in time? It seems to me probable that it does; but I cannot, at present at all events, enter on the investigation. The case of $b=\infty$ is specially important and interesting.

31. The present communication is confined to the much simpler case in which the two bounding planes are kept moving relatively with constant velocity; including as sub-case, the two planes held at rest, and the fluid caused by gravity to move between them. But we shall first take the much simpler sub-case, in which there is relative motion of the two planes, and no gravity. This is the very simplest of all cases of the general question of the Stability or Instability of the Motion of a Viscous Fluid. It is the second of the two cases prescribed by the Examiners for the Adams Prize of 1888. I have ascertained, and I now give (§§ 32 ... 39 below) the proof, that in this sub-case the steady motion is wholly stable, however small or however great be the viscosity; and this without limitation to two-dimensional motion of the admissible disturbances.

32. In our present sub-case, let βb be the relative velocity of the two planes; so that in (6) we may take $\mathrm{F}=0$, $\mathfrak{F}=\beta b$; and the corresponding steady solution of (4) is

$$v = \beta y \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20).$$

Thus equation (19) becomes reduced to

$$\left.\begin{aligned} \frac{d\sigma}{dt} + \beta y \frac{d\sigma}{dx} &= \mu\nabla^2\sigma, \\ \text{where} \qquad \sigma &= \nabla^2 v \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (21);$$

and (18), (15), (16), (17) become

$$2\beta\frac{dv}{dx} = -\nabla^2 p \quad . \quad . \quad . \quad . \quad . \quad (22),$$

$$\frac{du}{dt} + \beta y\frac{du}{dx} + \beta v = \mu\nabla^2 u - \frac{dp}{dx} \quad . \quad . \quad . \quad . \quad (23),$$

$$\frac{dv}{dt} + \beta y\frac{dv}{dx} \qquad = \mu\nabla^2 v - \frac{dp}{dy} \quad . \quad . \quad . \quad . \quad (24),$$

$$\frac{dw}{dt} + \beta y\frac{dw}{dx} \qquad = \mu\nabla^2 w - \frac{dp}{dz} \quad . \quad . \quad . \quad . \quad (25).$$

It may be remarked that equations (22) ... (25) imply (1), and that any four of the five determines the four quantities u, v, w, p. It will still be convenient occasionally to use (1).

We proceed to find the complete solution of the problem before us, consisting of expressions for u, v, w, p satisfying (22)...(25) for all values of x, y, z, t; and the following initial and boundary conditions :—

$$\left.\begin{array}{r}\text{when } t=0:\ u, v, w \text{ to be arbitrary functions}\\ \text{of } x, y, z, \text{ subject only to (1)}\end{array}\right\} \quad . \quad (26);$$

$$\left.\begin{array}{l}u=0,\ v=0,\ w=0, \text{ for } y=0 \text{ and all values of } x, z, t\\ u=0,\ v=0,\ w=0, \text{ for } y=b \qquad \text{''} \qquad \text{''}\end{array}\right\} (27).$$

33. First let us find a particular solution $\mathbf{u}, \mathbf{v}, \mathbf{w}, \mathbf{p}$, which shall satisfy the initial conditions (26), irrespectively of the boundary conditions (27), except as follows :—

$$\left.\begin{array}{l}\mathbf{v}=0, \text{ when } t=0 \text{ and } y=0\\ \mathbf{v}=0, \text{ when } t=0 \text{ and } y=b\end{array}\right\} \quad . \quad . \quad . \quad (28).$$

Next, find another particular solution, $\mathfrak{u}, \mathfrak{v}, \mathfrak{w}, \mathfrak{p}$, satisfying the following initial and boundary equations :—

$$\mathfrak{u}=0,\ \mathfrak{v}=0,\ \mathfrak{w}=0, \text{ when } t=0 \quad . \quad . \quad . \quad (29);$$

$$\left.\begin{array}{r}\mathfrak{u}+\mathbf{u}=0,\ \mathfrak{v}+\mathbf{v}=0,\ \mathfrak{w}+\mathbf{w}=0, \text{ when } y=0\\ \text{and when } y=b\end{array}\right\} \quad . \quad . \quad (30).$$

The required complete solution will then be

$$u=\mathfrak{u}+\mathbf{u}, \quad v=\mathfrak{v}+\mathbf{v}, \quad w=\mathfrak{w}+\mathbf{w} \quad . \quad . \quad . \quad (31).$$

34. To find $\mathbf{u}, \mathbf{v}, \mathbf{w}$, remark that, if μ were zero, the complete integral of (21) would be

$$\sigma = \text{arb. func. } (x-\beta yt) ;$$

and take therefore as a trial for a type-solution with μ not zero,

$$\sigma = \mathrm{T}\epsilon^{\iota[mx+(n-m\beta t)y+qz]} \quad . \quad . \quad . \quad . \quad (32);$$

where T is a function of t, and ι denotes $\sqrt{-1}$. Substituting accordingly in (21), we find

$$\frac{d\mathrm{T}}{dt} = -\mu[m^2+(n-m\beta t)^2+q^2]\mathrm{T} . \quad . \quad . \quad (33);$$

whence, by integration,

$$\mathrm{T}=\mathrm{C}\epsilon^{-\mu t\left[m^2+n^2+q^2-nm\beta t+\frac{m^2}{3}\beta^2t^2\right]} \quad . \quad . \quad . \quad (34).$$

By the second of (21), and (32), we find

$$v=-\mathrm{T}\frac{\epsilon^{\iota[mx+(n-m\beta t)y+qz]}}{m^2+(n-m\beta t)^2+q^2} \quad . \quad . \quad . \quad . \quad (35);$$

whence, by (22),

$$p=-2\beta m\iota\mathrm{T}\frac{\epsilon^{\iota[mx+(n-m\beta t)y+qz]}}{[m^2+(n-m\beta t)^2+q^2]^2}\quad . \quad . \quad . \quad (36).$$

Using this in (25), and putting

$$w=\mathrm{W}\epsilon^{\iota[mx+(n-m\beta t)y+qz]}\quad . \quad . \quad . \quad . \quad . \quad . \quad (37),$$

we find

$$\frac{d\mathrm{W}}{dt}=-\mu[m^2+(n-m\beta t)^2+q^2]\mathrm{W}-\frac{2\beta mq\mathrm{T}}{[m^2+(n-m\beta t)^2+q^2]}\quad . \quad (38),$$

which, integrated, gives W.

Having thus found v and w, we find u by (1), as follows:—

$$u=-\frac{(n-m\beta t)v+qw}{m}\quad . \quad . \quad . \quad . \quad . \quad (39).$$

35. Realizing, by adding type-solutions for $\pm\iota$ and $\pm n$, with proper values of C, we arrive at a complete real type-solution with, for v, the following—in which K denotes an arbitrary constant:

$$v=\tfrac{1}{2}\mathrm{K}\left\{\frac{\epsilon^{-\mu t[m^2+n^2+q^2-nm\beta t+\frac{1}{3}m^2\beta^2t^2]}}{m^2+(n-m\beta t)^2+q^2}\,\frac{\cos}{\sin}\,[mx+(n-m\beta t)y+qz]\right.$$
$$\left.-\frac{\epsilon^{-\mu t[m^2+n^2+q^2+nm\beta t+\frac{1}{3}m^2\beta^2t^2]}}{m^2+(n+m\beta t)^2+q^2}\,\frac{\cos}{\sin}\,[mx-(n+m\beta t)y+qz]\right\}\quad (40).$$

This gives, when $t=0$,

$$v=\frac{\mp\mathrm{K}}{m^2+n^2+q^2}\sin ny\,\frac{\sin}{\cos}\,(mx+qz)\quad . \quad . \quad . \quad (41),$$

which fulfils (28) if we make

$$n=i\pi y/b\quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (42);$$

and allows us, by proper summation for all values of i from 1 to ∞, and summation or integration with reference to m and q, with properly determined values of K, after the manner of Fourier, to give any arbitrarily assigned value to $v_{t=0}$ for every value of x, y, z,

$$\left.\begin{array}{llll}\text{from} & x=-\infty & \text{to} & x=+\infty,\\ \text{,,} & y=0 & \text{,,} & y=b,\\ \text{,,} & z=-\infty & \text{,,} & z=+\infty.\end{array}\right\}\quad . \quad . \quad . \quad (43).$$

The same summation and integration applied to (40) gives $\mathbf{v}$ for all values of t, x, y, z; and then by (38), (37), (39) we find corresponding determinate values of w and u.

36. To give now an arbitrary initial value, $\mathbf{w}_0$, to the

z-component of velocity, for every value of x, y, z, add to the solution (u, v, w), which we have now found, a particular solution (u', v', w') fulfilling the following conditions:—

$$\left.\begin{array}{l} v'=0 \text{ for all values of } t, x, y, z; \\ w'=\mathrm{w}_0-w_0 \text{ for } t=0, \text{ and all values of } x, y, z \end{array}\right\} \quad . \quad (44),$$

and to be found from (25) and (1), by remarking that $v'=0$ makes, by (22), $p'=0$, and therefore (23) and (25) become

$$\frac{du'}{dt}+\beta y\frac{du'}{dx}=\mu\nabla^2 u' \quad . \quad . \quad . \quad . \quad . \quad (45),$$

$$\frac{dw'}{dt}+\beta y\frac{dw'}{dx}=\mu\nabla^2 w' \quad . \quad . \quad . \quad . \quad . \quad (46).$$

Solving (46); just as we solved (21), by (32), (33), (34); and then realizing and summing to satisfy the arbitrary initial condition, as we did for v in (40), (41), (42), we achieve the determination of w'; and by (1) we determine the corresponding u', *ipso facto* satisfying (45). Lastly, putting together our two solutions, we find

$$\mathrm{u}=u+u', \quad \mathrm{v}=\mathrm{v}, \quad \mathrm{w}=w+w' \quad . \quad . \quad . \quad (47)$$

as a solution of (26) without (27), in answer to the first requisition of § 33. It remains to find $\mathfrak{u}, \mathfrak{v}, \mathfrak{w}$, in answer to the second requisition of § 33.

37. This we shall do by first finding a real (simple harmonic) periodic solution of (21), (22), (23), (25), fulfilling the condition

$$\left.\begin{array}{l} \left.\begin{array}{l} u=\mathrm{A}\cos\omega t+\mathrm{B}\sin\omega t \\ v=\mathrm{C}\cos\omega t+\mathrm{D}\sin\omega t \\ w=\mathrm{E}\cos\omega t+\mathrm{F}\sin\omega t \end{array}\right\} \text{ when } y=0 \\ \left.\begin{array}{l} u=\mathfrak{A}\cos\omega t+\mathfrak{B}\sin\omega t \\ v=\mathfrak{C}\cos\omega t+\mathfrak{D}\sin\omega t \\ w=\mathfrak{E}\cos\omega t+\mathfrak{F}\sin\omega t \end{array}\right\} \text{ when } y=b \end{array}\right\} \quad . \quad . \quad (48),$$

where A, B, C, D, E, F, $\mathfrak{A}$, $\mathfrak{B}$, $\mathfrak{C}$, $\mathfrak{D}$, $\mathfrak{E}$, $\mathfrak{F}$ are twelve arbitrary functions of (x, z). Then, by taking $\int_0^\infty d\omega f(\omega)$ of each of these after the manner of Fourier, we solve the problem of determining the motion produced throughout the fluid, by giving to every point of each of its approximately plane boundaries an infinitesimal displacement of which each of the three components is an arbitrary function of x, z, t. Lastly, by taking these functions each $=0$ from $t=-\infty$ to $t=0$, and each equal to minus the value of **u**, **v**, **w** for every point of each boundary, we find the $\mathfrak{u}$, $\mathfrak{v}$, $\mathfrak{w}$ of § 33. The solution of our problem of § 32 is then completed by equations (31). To do all this is a mere routine after an imaginary type solution is provided as follows.

38. To satisfy (21) assume

$$v=\epsilon^{\iota(\omega t+mx+qz)}\mathcal{V}$$

$$=\epsilon^{\iota(\omega t+mx+qz)}\{\mathrm{H}\epsilon^{y\sqrt{(m^2+q^2)}}+\mathrm{K}\epsilon^{-y\sqrt{(m^2+q^2)}}+\mathrm{L}f(y)+\mathrm{MF}(y)\}\cdot \quad (49),$$

where H, K, L, M are arbitrary constants and f, F any two particular solutions of

$$\iota(\omega+m\beta y)\sigma=\mu\left[\frac{d^2\sigma}{dy^2}-(m^2+q^2)\sigma\right] \quad . \quad . \quad (50).$$

This equation, if we put

$$m\beta/\mu=\gamma, \text{ and } m^2+q^2+\iota\omega/\mu=\lambda \quad . \quad . \quad . \quad (51),$$

becomes

$$\frac{d^2\sigma}{dy^2}=(\lambda+\iota\gamma y)\sigma \quad . \quad . \quad . \quad . \quad . \quad . \quad (52);$$

which, integrated in ascending powers of $(\lambda+\iota\gamma y)$, gives two particular solutions, which we may conveniently take for our f and F, as follows :—

$$\left.\begin{aligned} f(y)&=1-\frac{\gamma^{-2}(\lambda+\iota\gamma y)^3}{3\,.\,2}+\frac{\gamma^{-4}(\lambda+\iota\gamma y)^6}{6\,.\,5\,.\,3\,.\,2}-\frac{\gamma^{-6}(\lambda+\iota\gamma y)^9}{9\,.\,8\,.\,6\,.\,5\,.\,3\,.\,2}+\&\text{c.}\\ \mathrm{F}(y)&=\lambda+\iota\gamma y-\frac{\gamma^{-2}(\lambda+\iota\gamma y)^4}{4\,.\,3}+\frac{\gamma^{-4}(\lambda+\iota\gamma y)^7}{7\,.\,6\,.\,4\,.\,3}-\frac{\gamma^{-6}(\lambda+\iota\gamma y)^{10}}{10\,.\,9\,.\,7\,.\,6\,.\,4\,.\,3}+\&\text{c.}\end{aligned}\right\}(53).$$

39. *These series are essentially convergent for all values of y.* Hence in (49) we have a solution continuous from $y=0$ to $y=b$; and by its four arbitrary constants we can give any prescribed values to $\mathcal{V}$, and $\frac{d\mathcal{V}}{dy}$, for $y=0$ and $y=b$. This done, find p determinately by (24); and then integrate (25) for w in *an essentially convergent series* of ascending powers of $\lambda+\iota\gamma y$, which is easily worked out, but need not be written down at present, except in abstract as follows :—

$$w=\mathcal{W}\epsilon^{\iota(\omega t+mx+qz)} \quad . \quad . \quad . \quad . \quad . \quad (54):$$

where

$$\left.\begin{aligned}\mathcal{W}=\mathrm{H}\mathfrak{F}_1(\lambda+\iota\gamma y)+\mathrm{K}\mathfrak{F}_2(\lambda+\iota\gamma y)+\mathrm{L}\mathfrak{F}_3(\lambda+\iota\gamma y)\\ +\mathrm{M}\mathfrak{F}_4(\lambda+\iota\gamma y)+\mathrm{P}\epsilon^{y\sqrt{(m^2+q^2)}}+\mathrm{Q}\epsilon^{-y\sqrt{(m^2+q^2)}}\end{aligned}\right\}(55).$$

Here P and Q are the two fresh constants, due to the integration for w. By these we can give to $\mathcal{W}$ any prescribed values for $y=0$ and $y=b$. Lastly, by (1), with (49), we have

$$\left.\begin{aligned} u &= \mathcal{U}\epsilon^{\iota(\omega t+mx+qz)} \\ \text{where}\quad \mathcal{U} &= -\left(\frac{1}{m\iota}\frac{d\mathcal{V}}{dy}+\frac{q}{m}\mathcal{W}\right) \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (56).$$

Our six arbitrary constants, H, K, L, M, P, Q, clearly allow us to give any prescribed values to each of $\mathcal{U}$, $\mathcal{V}$, $\mathcal{W}$, for $y=0$ and for $y=b$. Thus the completion of the realized problem with real data of arbitrary functions, as described in § 37, becomes a mere affair of routine.

40. Now remark that the ($\mathbf{u}$, $\mathbf{v}$, $\mathbf{w}$) solution of § 34 comes essentially to nothing, asymptotically as time advances, as we see by (33), (34), and (38). Hence the ($\mathfrak{u}$, $\mathfrak{v}$, $\mathfrak{w}$) of § 37, which rise gradually from zero at $t=0$, comes asymptotically to zero again as t increases to ∞. We conclude that the steady motion is stable.

[To be continued.]

XXII. *On Evaporation and Dissociation.*—Part VI. (*continued*). *On the Continuous Change from the Gaseous to the Liquid State at all Temperatures.* *By* WILLIAM RAMSAY, *Ph.D.*, *and* SYDNEY YOUNG, *D.Sc.*[*]

[Plates III.–V.]

THE following pages give a further proof of the correctness of the relation $p=bt-a$, where $v=$constant, applicable both to gases and liquids. The data for methyl alcohol apply solely to the gaseous state, for the very high pressures which its vapour exerts precluded measurements at temperatures above its critical point. With ethyl alcohol the determinations of the compressibility of the liquid are more complete than with ether; the experimental observations in the neighbourhood of the critical volume are, however, not very numerous, for the highest temperature for which an isothermal was constructed is 246°, the critical temperature being 243°·1. The values of a and b at volumes near the critical are consequently somewhat uncertain. The data for the gaseous condition are, however, pretty full. We have also a considerable number of data for acetic acid (Trans. Chem. Soc. 1886, p. 790). Here the temperature at which the highest isothermal was measured was the highest conveniently attainable by our method, viz. 280°. But as the critical temperature

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[*] Communicated by the Physical Society: read April 23, 1887.

lies much higher, the pressures were in no case very great. The behaviour of acetic acid, however, contrasted with that of the alcohols, ether, and carbon dioxide is very striking. The equation $p=bt-a$ does not apply; in other words lines of equal volume are not straight, but are curves of double flexure. We shall consider the meaning of this peculiarity after adducing data.

1. *Methyl Alcohol.*—The data are at present in the hands of the Royal Society. The values of b were, as before, determined by reading points on the isothermal curves at equal volumes; constructing isochors graphically, and having thus obtained approximate values of b, these were smoothed by plotting them as abscissæ, the ordinates being the reciprocals of the volumes. The values of b given in the Table which follows were those read from this curve. The values of a were calculated from the equation $a=bt-p$, the mean value obtained from all the readings at each volume being taken as correct.

TABLE I.

Vol.	b.	log b.	a.
c. c. per gram.			
7	626·5	2·79692	263430
8	569·0	2·75511	235370
9	509·0	2·70672	206290
10	452·5	2·65562	179090
11	405·3	2·60778	156660
12	365·0	2·56229	137810
14	299·0	2·47567	107460
16	248·5	2·39533	84730
18	211·0	2·32428	68364
20	184·8	2·26670	57474
25	139·8	2·14551	39724
30	112·5	2·05115	29834
40	80·0	1·90309	18913
50	62·0	1·79239	13401
70	43·5	1·63849	8631
100	26·95	1·43056	4092
135	18·95	1·27761	2398
170	14·50	1·06137	1624
200	12·00	1·07918	1187
240	9·65	0·98453	794
280	8·00	0·90309	455
340	6·30	0·79934	307
400	5·15	0·71181	159
450	4·57	0·65992	130

The following Table gives the pressures read from the isothermals from which these values of a and b were obtained; and we have added, for the sake of comparison, the pressures recalculated by help of these values.

TABLE II.

Vol. c. c. per gram	Temperatures.															
	240°.		239°·5.		239°.		238°.		236°.		234°.		232°.		230°.	
	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.
	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.
7	58220	57970	57700	57650	57280	57340	56660	56710	55250	55460	54200	54210				
8	56660	56530	56260	56240	55930	55960	55330	55390	54050	54250	53190	53110	52060	51570		
9	54990	54830	54580	54570	54240	54320	53790	53810	52640	52790	51910	51780	50800	50760	49630	49740
10	53200	53040	52860	52810	52600	52590	52040	52140	51180	51240	50390	50330	49430	49420	48370	48520
11	51390	51260	51160	51060	50870	50860	50320	50450	49640	49640	48780	48830	48000	48020	47040	47210
12	49450	49430	49380	49250	49150	49070	48610	48700	48070	47980	47190	47250	46530	46510	45710	45790
14	45980	45930			45670	45630	45260	45330	44800	44730	44150	44130	43620	43530	42930	42940
16	42800	42750			42570	42500	42190	42260	41780	41760	41250	41260	40840	40760	40200	40270
18	39920	39876			39660	39666	39400	39456	39000	39036	38600	38616	38250	38186	37740	37766
20	37360	37328			37110	37143	36900	36958	36600	36589	36205	36220	35940	35849	35400	35481
25	32000	31994			31810	31854	31670	31714	31440	31435	31120	31156	30900	30876	30600	30596
30	27845	27878			27805	27766	27630	27653	27420	27429	27200	27204	27000	26978	26795	26753
40	22065	22127			22000	22047	21930	21967	21820	21807	21650	21647	21510	21487	21375	21327
50	18380	18405													17820	17785
70	13590	13685													13210	13250
100	9690	9733													9460	9464
135	7300	7323													7180	7134
170	5800	5815													5679	5670
200	4960	4969													4856	4849
240	4153	4156													4064	4060
280	3558	3559													3494	3478
340	2921	2925													2873	2862
400	2491	2483													2442	2431
450	2211	2214													2178	2169

Table II. (*continued*).

Vol. c. c. per gram.	Temperatures.													
	225°.		220°.		200°.		180°.		160°.		120°.		100°.	
	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.
	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.
10	46260	46260												
11	45280	45180												
12	43980	43960	42000	42140										
14	41400	41440	39810	39950										
16	38990	39020	37740	37780										
18	36740	36716	35660	35656										
20	34600	34556	33660	33633										
25	29970	29897	29210	29198										
30	26200	26191	25700	25629	23790	23379	20600	21129						
40	20960	20927	20620	20527	19060	18927	17140	17327						
50					15980	15925	14600	14685						
70					11980	11945	11140	11075	10240	10205				
100					8640	8655	8200	8117	7605	7577				
135					6520	6565			5830	5808				
170					5221	5235			4696	4655	4056	4075		
200					4467	4489			4040	4009	3522	3529		
240					3748	3770			3404	3375	2999	2999		
280					3229	3239			2939	2919	2605	2599	2405	2439
340					2670	2673			2435	2421	2172	2169	2022	2043
400					2274	2277			2079	2071	1863	1865	1740	1762
450					2024	2030			1860	1849	1665	1666	1565	1575

It is evident that the agreement here is very satisfactory, and that any differences are well within the limits of experimental error, and of error in reading from curves.

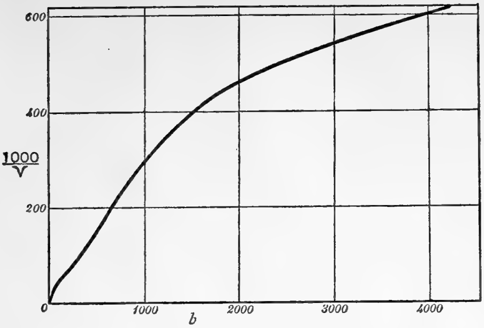
2. *Ethyl Alcohol.*—The data for ethyl alcohol are published in the 'Philosophical Transactions,' 1886, part i. p. 123. In constructing isothermals for the liquid, which are published in plate iii. of that memoir, isobars were constructed from the isothermal curves drawn through the experimental points; these isobars were made use of in finding isochoric lines at which the relations of temperature and pressure could be compared. This method applies to volumes not greater than 3 cub. centim. per gram. Between volumes 3 and 12 cub. centim. per gram the isochoric lines were constructed from the diagram from which plate vi. was copied (the smaller divisions are omitted on the printed plate). For volumes above 12 cub. centim. per gram the actual observations were made use of which are detailed on pp. 144–151; the slight alteration in volume caused by the expansion of the measuring-tube was here neglected; at the utmost the error introduced is 0·15 per cent. of the total volume.

The only satisfactory manner of comparing the found and calculated pressures for liquid alcohol is to reproduce the diagram showing the compressibility of the liquid, denoting the calculated pressures by crosses, the actual experimental observations being represented by circles. Although there is apparently only one volume at which a comparison is possible on each of the lower isotherms, yet it must be remembered that the values of a and b are deduced from the isobaric curves, to construct which numerous points on each isotherm were made use of.

Table III. on page 202 gives the values of a and b for definite volumes. These were read from three overlapping curves, on which values of b were abscissæ, the reciprocals of the volumes being ordinates; the actual values of b in the region of the critical volume were, for reasons previously given, interpolated. The form of this curve is remarkable. It is equivalent to the first term in Clausius's and Van der Waals's formulæ, $\frac{RT}{(v-\beta)}$; but it is evident that its form cannot be represented so simply. Its general form is represented in the accompanying woodcut.

The same general form is to be noticed in the case of ether. The curve obtained in plotting a against volume is of similar form.

The comparison of actual and calculated results at volumes below 3 cub. centim. per gram is difficult. As it is most



important to show the truth of the equation $p=bt-a$ for liquids, we have decided, after due consideration, to reproduce the isothermals exhibiting compressibility, inserting crosses (×) to denote the pressures calculated by means of the values of b and a given in the preceding Table. It will be noticed that these crosses are often more in concordance with the actual observations than are the lines drawn before we had discovered this relation. These are shown on Plate III.

Between volumes 3 and 12 cub. centim. per gram we give a table of comparison (Table IV.).

For higher volumes a comparison is made with the results of actual experiment. The volume at the mean temperature is given in each case, although at the extreme temperatures the divergence in some cases amounts to 0·15 per cent. (Table V.).

The values of a and b given are the best available from the determinations; but as these are few at volumes near the critical volume, the range of temperature being too small to permit of ascertaining the values with accuracy, no great dependence can be placed on them. This is proved by the fact that at 220°, if the areas included between the vapour-pressure line and the serpentine curve are made equal, the vapour-pressure shows an error of about 3 per cent. We do not regard this as in any way opposed to our conclusions; for while with ether about the critical volume, the range of temperature from which the values of b and a were calculated amounted to 55 degrees, and while at volumes slightly greater measurements up to 85 degrees were made use of, with alcohol the total range of temperature was only 3 degrees at corresponding volumes. They cannot, therefore, have at all the same claim to accuracy.

Acetic Acid.—The data are given in the Trans. Chem. Soc. 1886, p. 790. As in the original paper we have omitted to state the volumes of 1 gram, we give a Table (VI.) containing these values.

TABLE III.

Volume.	*b*.	log *b*.	*a*.
c. c. per gram.			
1·4	6295	3·79900	2376800
1·45	5725	3·75778	2270060
1·5	5250	3·72016	2164680
1·55	4800	3·68124	2041900
1·6	4410	3·64444	1925370
1·65	4075	3·61013	1817760
1·7	3750	3·57403	1702370
1·75	3490	3·54283	1608180
1·8	3240	3·51054	1511410
1·85	3000	3·47712	1413870
1·9	2795	3·44638	1327920
1·95	2600	3·41497	1242030
2·0	2435	3·38650	1168030
2·1	2150	3·33244	1037000
2·2	1940	3·28780	938360
2·3	1765	3·24674	854080
2·4	1640	3·21484	793230
2·6	1435	3·15685	691280
2·74	1330	3·12385	638080
3·0	1170	3·06819	556040
3·5	977	2·98989	456490
4·0	842	2·92531	386810
4·5	741	2·86982	334710
5	662	2·82086	294080
6	556	2·74507	240150
7	484	2·68484	204010
8	428·5	2·63195	176650
9	381	2·58092	153610
10	341·2	2·53301	134650
12	276·5	2·44169	104590
14·28	221	2·34439	79460
18·25	155	2·19033	50530
22·13	113·7	2·05576	33080
26·08	88·4	1·94645	23120
30·0	72·1	1·85793	17030
37·8	52·4	1·71933	10470
53·45	34·2	1·53403	5400
76 6	22·6	1·35411	2902
108·2	15·3	1·18469	1565
131·7	12·7	1·10382	1282
170·5	9·6	0·98227	886
484	3·3	0·51851	204

TABLE IV.

Vol.	Temperatures.																			
	246°.		244°·35.		243°·55.		243·1°.		242°.		238°.		236°.		234°.		232°.		230°.	
	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read,	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.
c. c. p. gr.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.
3·5	50180	50580	49100	48950	48400	48060	47640	47730												
4·0	50000	50190	48960	48790	48270	48120	47610	47740												
4·5	49840	49870	48730	48640	48180	48050	47520	47720												
5·0	49480	49500	48350	48400	48110	47880	47300	47580	46970	46850										
6·0	48420	48410	47460	47490	46670	46800	46210	46190	45210	45080										
7·0	47060	47180	46320	46380	45760	45780	45270	45250	44350	44280	43400	43310								
8·0	45500	45740	44920	45030	44440	44500	43030	44030	43220	43170	42450	42310	41600	41460	40640	40600				
9·0	43860	44130	43350	43500	42930	43020	42550	42600	41880	41840	41210	41080	40470	40320	39690	39560	38860	38790	38050	38030
10·0	42130	42430	41680	41870	41350	41440	40990	41070	40420	40390	39800	39900	39150	39020	38480	38340	37810	37660	37110	36980
12·0	38700	38910	38320	38460	38050	38110	37780	37810	37270	37250	36780	36700	36250	36150	35710	35600	35130	35040	34550	34490

TABLE V.

Temperature.	Volumes.											
	14·28 c. c. per gram.		18·25 c. c. per gram.		22·13 c. c. per gram.		26·08 c. c. per gram.		30·0 c. c. per gram.		37·8 c. c. per gram.	
	Pressures.		Pressures.		Pressures.		Pressures.		Pressures.		Pressures.	
	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.
	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.
246°	34928	35240	29929	29915	25936	25931	......	......	20430	20390	16817	16726
244·35	34657	34870	29891	29658	25664	25651	......	......	20269	20270	16704	16639
243·1	34546	34600	29438	29465	25570	25600	......	......	20158	20180	16611	16573
242·0	34093	34360	29372	29295	25554	25476	......	......	20216	20101	16566	16516
241·0	34150	34130	29277	29139	25482	25361	......	......	20013	20029	16535	16463
240·0	33900	33910	29037	28985	25303	25248	......	......	19959	19957	16500	16410
239·0	33890	33690	28785	28830	25262	25134	......	......	20000	19885	16408	16359
238	33767	33470	28746	28675	24988	25021	......	......	19910	19813	16322	16306
237	33338	33250	28612	28520	24980	24907	......	......	19802	19748	16336	16254
236	33106	33030	28371	28365	24872	24794	......	......	19694	19669	16249	16202
234	32721	32590	27740	28055	24504	24566	......	......	19458	19524	16135	16097
232	32091	32140	27448	27745	24100	24338	......	......	19108	19380	15759	15992
230	31636	31700	27144	27435	23827	24111	21190	21345	19082	18975	15733	15887
225	30601	30600	26546	26660	23365	23543	20809	20903	18729	18875	15436	15625
220	29632	29490	......	......	22964	22975	......	......	18355	18515	15214	15363
210	......	......	24723	24335	21992	21837	19769	19577	17899	17794	14912	14839
200	......	......	......	......	20814	20700	......	......	17140	17073	14444	14315
190	......	......	......	......	......	......	17846	17809	16364	16352	13805	13791
180	......	......	......	......	......	......	......	......	......	......	13183	13267

Table V. (*continued*).

Tempe-rature.	Volumes.											
	53·45 c. c. per gram.		76·6 c. c. per gram.		108·2 c. c. per gram.		131·7 c. c. per gram.		170·5 c. c. per gram.		484 c. c. per gram.	
	Pressures.		Pressures.		Pressures.		Pressures.		Pressures.		Pressures.	
	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.
°	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.
246	12381	12350	......	......	......	......	5276	5309	4091	4096	......	......
244·35	12313	12293	......	......	......	......	5259	5288	4090	4080	......	......
243·1	12260	12251	8793	8762	6332	6331	......	......	4053	4069	......	......
242·0	12238	12213	......	......	......	......	5216	5258	4062	4058	......	......
241·0	12207	12179	......	......	......	......	5212	5246	4055	4048	......	......
240·0	12188	12145	......	......	......	......	5198	5233	4046	4039	......	......
239·0	12161	12111	......	......	......	......	5185	5220	4029	4029	......	......
238	12087	12076	8673	8647	6274	6253	......	......	4030	4020	......	......
237	12094	12042	8676	8624	......	......	......	......	4022	4010	......	......
236	12073	12008	8669	8602	6249	6223	......	......	4029	4000	......	......
234	11969	11940	8628	8556	6210	6192	......	......	3999	3981	......	......
232	......	......	......	......	6089	6161	......	......	3935	3962	......	......
230	11635	11803	......	......	......	......	......	......	3898	3943	......	......
225	11488	11632	8251	8353	5968	6055	......	......	3863	3895	......	......
220	11366	11461	8168	8240	5916	5978	......	......	3823	3847	......	......
210	11193	11119	......	......	......	......	4873	4852	......	......	......	......
200	10863	10777	......	......	......	......	4758	4725	......	......	......	......
190	10553	10435	......	......	......	......	4647	4598	......	......	......	......
180	......	......	......	......	5400	5366	......	......	......	......	......	......
170	9704	9751	7131	7110	5261	5213	......	......	......	......	......	......
160	9254	9409	6915	6884	5105	5060	4272	4217	......	......	......	......
150	......	......	6632	6658	4933	4907	4128	4090	......	......	......	......
140	......	......	......	......	4749	4754	3978	3963	......	......	......	......
130	......	......	......	......	......	......	3838	3836	......	......	......	......
120	......	......	......	......	......	......	......	......	2942	2887	......	......
110	......	......	......	......	......	......	......	......	......	......	1062	1060
100	......	......	......	......	......	......	......	......	......	......	1026	1027
90	......	......	......	......	......	......	......	......	......	......	993	994

Table VI.

Temp. 50°.	
Pressure.	Vol. of 1 gram.
millim.	c. c.
13·4	15100
15·4	13300
16·95	11979
18·2	10640
21·0	9325
24·15	8000
26·5	7101
30·0	6214
34·9	5329
51·35	3567

Temp. 78°·4.	
18·8	14550
19·3	14760
20·95	13320
21·1	13250
21·85	12430
22·85	11980
23·35	12000
23·45	11550
24·9	10660
27·0	9774
27·15	9760
27·3	9858
29·3	8868
31·8	7962
32·7	7833
35·85	7092
40·15	6217
42·05	6075
46·4	5326
52·9	4631
54·35	4441
63·2	3865
66·75	3553
71·7	3281
87·2	2670
91·2	2496
101·25	2220
106·6	2132
121·5	1826
124·65	1782
125·3	1767
141·3	1519
151·7	1421
157·75	1375
164·2	1306
172·25	1243
180·8	1175
191·75	1066

Temp. 92°.	
Pressure.	Vol. of 1 gram.
millim.	c. c.
20·45	14650
24·55	11990
30·58	9325
39·35	7105
51·1	5329
66·45	4000
75·65	3515
96·8	2649
104·2	2440
135·8	1825
138·6	1792
170·5	1403
181·05	1327
234·9	982
268·4	843

Temp. 105°·1.	
21·85	15150
31·9	10210
53·6	5782
74·65	4005
83·45	3590
91·4	3110
106·3	2721
126·3	2220
150·4	1823
155·4	1778
186·2	1431
199·75	1334
232·6	1123
286·5	884·6
298·6	841·5
337·4	729·3
422·4	561·0
460·35	504·9

Temp. 117°·95.	
75·75	4488
85·35	3871
163·5	1868
210·45	1403
256·4	1121
328·6	841·5
386·1	701·3
467·1	561·0
601·8	420·8
632·5	392·7
676·8	364·7

Temp. 118°·2.	
Pressure.	Vol. of 1 gram.
millim.	c. c.
24·1	15100
29·1	12430
36·65	9769
44·0	7984
55·3	6217
69·25	4885
84·05	4010
88·6	3782
121·5	2664
172·6	1781
224·2	1335
319·0	876·6
391·9	689·2
433·6	611·0

Temp. 132°·9.	
101·7	3591
132·7	2665
174·5	1969
214·9	1546
285·2	1122
360·4	851·3
470·7	617·1
700·2	391·3
747·6	364·7

Temp. 147°·6.	
88·5	4516
133·9	2805
181·6	2045
225·1	1627
263·5	1352
341·8	1010
427·9	785·4
507·0	645·2
602·5	533·0
729·7	420·8

Temp. 162°·5.	
95·9	4460
134·3	3142
183·8	2244
274·0	1459
373·1	1038
450·5	841·5

Table VI. (*continued*).

Temp. 162°·5 (*cont.*).	
Pressure.	Vol. of 1 gram.
millim.	c. c.
526·7	701·3
640·3	561·0
729·5	476·9

Temp. 184°·1.	
98·05	4654
140·7	3223
188·2	2384
242·9	1823
336·8	1290
425·7	1010
520·4	813·5
666·4	617·2
791·6	504·9

Temp. 162°·5.	
1413	217·1
1523	197·3
1616	184·1
1777	164·0
1907	150·5
2049	137·3
2128	130·6
2212	124·0
2291	117·4
2390	110·7
2456	104·1

Temp. 183°·7.	
Pressure.	Vol. of 1 gram.
millim.	c. c.
1630	216·8
1764	197·0
1876	183·8
2066	163·8
2299	143·7
2599	123·8
2993	103·9
3511	84·17
3733	77·64

Temp. 200°.	
1780	216·8
1984	190·5
2341	157·1
2852	123·9
3679	90·75
4452	71·14
5178	58·15

Temp. 240°.	
2139	217·5
2389	191·1
2851	157·6
3512	124·2
4578	91·02
5612	71·36
6604	58·33
8043	45·27
9006	38·72
10295	32·16
11930	25·60

Temp. 279°·85.	
Pressure.	Vol. of 1 gram.
millim.	c. c.
20669	15·82
21043	15·34
21444	14·86
21762	14·38
22147	13·90
22565	13·42
22993	12·93
23383	12·44
23907	11·95
24075	11·70
24260	11·46

Temp. 280°·4.	
2400	217·5
2475	211·1
2556	204·5
2639	197·9
2731	191·3
2824	184·6
3284	157·7
4085	124·4
5383	91·12
6661	71·44
8742	51·87
11048	38·77
12742	32·20
15105	25·63
18496	19·04
20693	15·74
21848	14·42
23433	12·44

These numbers were mapped on curve-paper, and pressures were read from the curves, corresponding to even volumes.

TABLE VII.

Vol.	50°.	78°·4.	92°.	105°·1.	118°·1.	132°·9.	147°·6.	162°·5.	184°·1.
c. c.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	m m .	mm.
15000	13·85	18·7	20·0	22·0	24·3				
8000	24·00	32·25	35·15	39·65	43·9				
4000	45·45	60·4	66·55	75·1	83 65	93·0	97·7	106·0	114·7
2000	......	111·0	124·9	140·3	155·0	171·5	185·5	204·5	221·5
1000	......	205·0	231·2	257·5	284·5	315·2	345·0	385·0	430·0
500	......	......	......	469·7	517·5	563·0	635·0	700·5	802·5

Table VII. (*continued*).

Vol.	162°·5.	183°·7.	200°.	240°.	280°.
c. c.	mm.	mm.	mm.	mm.	mm.
220 ...	1405	1610	1755	2115	2380
150 ...	1910	2225	2435	2985	3440
100 ...	2545	3080	3400	4235	4962
80 ...	......	3670	4055	5105	6030
60 ...	......	......	5057	6465	7750

Before discussing these results we give similar tables calculated and read from numbers given in the Messrs. Natansons's memoir on nitric peroxide (Wiedemann's *Annalen*, 1886, p. 606). As their results are stated as vapour-densities, we have converted their numbers into volumes of 1 gram.

TABLE VIII.

Pressure.	Vol. of 1 gram.
Temp. −12°·6.	
millim.	c. c,
115·4	1648
Temp. 0°.	
37·96	6235
86·57	2539
172·48	1208
250·66	808·1
Temp. 21°.	
491·60	479·8
516·96	453·0
556·50	419·4
619·17	360·6
Temp. 49°·7.	
26·8	15585
93·75	4144
182·7	2008
261·4	1354
497·75	651
Temp. 73°·7.	
millim.	c. c.
49·65	9214
64·75	7017
67·72	6754
107·47	4184
164·59	2701
302·04	1420
504·14	816·8
633·27	635·9
Temp. 99°·8.	
11·73	41542
23·22	20993
34·80	13929
57·35	8468
79·57	6067
89·67	5408
108·65	4454
116·58	4156
142·29	3402
202·24	2375
371·27	1277
520·98	900
Temp. 99°·8 (*cont.*).	
658·31	705·5
675·38	687·7
732·51	629·7
Temp. 117°·91.	
58·24	8972·3
Temp. 129°·9.	
35·99	15076
66·94	8100
78·73	6892
104·77	5182
152·46	3568
169·71	3194
247·86	2186
297·95	1823
550·29	980·1
Temp. 151°·4.	
117·98	4869
475·41	1210
666·22	861·1

These numbers, after mapping, gave the following pressures for even volumes :—

TABLE IX.

Volumes.	−12°·6.	0°.	49°·7.	73°·7.	99°·8.	129°·9.	151°·4.
c. c.	mm.	mm.	mm.	mm.	mm.	mm.	mm.
16000	......	......	26·3	......	30·0	34·5	
8000	......	30·7	50·8	56·5	61·2	68·8	
4000	......	58·5	96·2	112·5	121·2	136·0	146·3
2000	97·0	110·0	183·7	220·0	241·0	271·3	294·0
1000	......	204·2	353·0	422·5	473·5	540·0	573·5

On comparing the results for acetic acid and for nitric peroxide with those for methyl and ethyl alcohols, ether, and carbonic anhydride, the most noticeable feature is that the isochoric lines are not straight. If it is true that the limiting formulæ for acetic acid and for nitric peroxide are, respectively, $C_2H_4O_2$ and $C_4H_8O_4$, and NO_2 and N_2O_4*, then, on the supposition that the lower formulæ remain correct, and that the density remains constant, it is possible to calculate pressures and temperatures corresponding to given isochoric lines; these lines would be those of a perfect gas, straight, and radiating from absolute zero of temperature and pressure; and the isochoric line corresponding to a volume v of the simpler molecule $C_2H_4O_2$, or NO_2, will be the same as the isochoric line for the formula $C_4H_8O_4$ or N_2O_4, and the volume $2v$. Now as the density of the more complex molecule diminishes with rise of temperature and pressure, it is to be expected that the isochoric line should lie between these lines, and that it should coincide at high temperatures with the isochoric line v, and at low temperatures with the line $2v$.

This will be readily understood on reference to the accompanying plates. Plate III. represents isochoric lines for acetic acid, and Plate IV. for nitric peroxide. Take the volume 2000 cub. centims. per gram. The line AB represents the relations of temperature to pressure on the assumption that the formula of acetic acid is $C_2H_4O_2$, and its corresponding density 30. The line EF represents similar relations on the assumption that the formula is $C_4H_8O_4$, and the density 60. This line, it will be seen, also corresponds to a volume of 4000 cub. centims. of gas of density 30. The line CD represents the observed relations of temperature to pressure for acetic acid when the volume is 2000 cub. centims. per gram. It is a curve of double flexure, which, if produced towards B, apparently might touch and become continuous with the line AB; and if produced towards E might coincide with the line

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* We must here direct attention to an arithmetical mistake in our former paper, "On the Nature of Liquids" (Phil. Mag. Feb. 1887, p. 137). The density of nitric peroxide at −12°·6 is stated to be 52·54, it should be 42·54; and the deductions from that erroneous statement must therefore be withdrawn.

EF. For these large volumes of gas, it is almost certain that the density would become normal at a sufficiently high temperature; but at low temperatures and pressures it cannot be proved, owing to condensation, whether the molecular formula would be $C_4H_8O_4$; that is, whether the line CD would coincide with the line EF.

On Plate III. are also represented similar relations for ether, at volumes of 4000 and 1000 cub. centims. per gram; the points representing the observed relations of temperature to pressure fall on the isochoric lines; that is to say, the divergence from Boyle's and Gay-Lussac's laws is too small to be detected by experiment. At smaller volumes, however (those at 300 and 250 cub. centims. per gram are shown), the line passing through the observed points falls below the theoretical line, but is not quite parallel to it. It follows therefore, that if the isochoric lines are perfectly straight, they would cut the normal isochoric lines at an extremely high temperature. The physical meaning of this behaviour is that, if the temperature of a gas, at constant volume, be raised sufficiently high, the density must equal and then fall below the normal. It is evident that this must be the case. For the pressure of a gas depends on the number of molecules present in unit volume, on the average velocity of each molecule, and on the number of impacts on unit area of the surface of the containing vessel, in unit time. With constant volume, since the mean distance between the molecules remains constant, the cohesion of the molecules is assumed to be constant. But the rise of pressure produced by rise of temperature of a theoretical gas is based on the assumption that each impact takes place at the centre of each molecule; that is, that the actual volume of the molecules themselves is *nil*. But as this is not the case, as impacts must take place at some distance from the centres of the molecules, they must necessarily be more frequent. The effect of cohesion is to reduce the pressure of the gas, by reducing the average velocity of the molecules, and this, for any given volume, by a constant amount. Hence, below a certain temperature, the pressure will be less than that of a normal gas, and if the temperature be reduced sufficiently, will become negative. With rise of temperature, the average velocity of each molecule will increase at the same rate as in the case of a theoretical gas; but the number of impacts, and, consequently, the pressure, will increase at a greater rate than if the gas were perfect; hence a temperature will ultimately be reached when the pressure is as much decreased by cohesion, as it is raised by the more frequent encounters of the molecules; and at that temperature the density of the gas will be normal. At still higher temperatures the pressure, and therefore the value of the expression $\frac{pt}{v}$, will be greater, and the vapour-density less, than that of a theoretical gas.

The formula which we find to represent the relations of temperature and pressure at constant volume, $p=vt-a$, represents these facts. The reciprocal of the vapour-density is the product of pressure and volume, divided by the product of the absolute temperature into half the molecular weight, or

$$\frac{1}{\text{vap. dens.}}=\frac{pv}{t}\times\frac{2}{\text{mol. wt.}}$$

Now while at small volumes the rate of diminution of volume of a substance increases more and more slowly with rise of pressure, there is no limit conceivable to the pressure which may be applied. Hence the value of the expression $\frac{pv}{t}$ must ultimately be greater than unity. The relation of $\frac{pv}{t}$ to the pressure for ether has been calculated by means of our equation for a few isothermals, and is shown in the annexed figure. From the direction of the lines, it would appear that if produced to still higher pressures, the product pv would reach and exceed unity. This state has indeed been reached by Natterer in compressing the so-called permanent gases at temperatures far above the critical points.

It is possible, by means of our equation, to follow these lines into the unrealizable state, at low temperatures, where pressure becomes negative. An example is given at 150°. It is obvious that those isothermals which include negative pressures will intersect each other at the zero of pressure and $\frac{pv}{t}$, and will form loops in the negative region. This is shown on Pl. IV.

It has recently been suggested by Wroblewski (*Wien. Monatsb. der Chemie*, 1886, p. 383), in a paper from the conclusions of which we differ in every point, that the minimum values of pv mapped against pressure form a curve continuous with the vapour-pressure curve. This is distinctly not the case. The curve is cut by the vapour-pressure curve at the critical point. It is, however, approximately continuous with the curve shown on plate ix. in our previous paper on this subject, representing the pressures corresponding to the inferior apices of the serpentine isothermals. The minimum product of pressure and volume probably does not occur at volumes corresponding to these apices, but at slightly lower volumes. We have proved this to be the case at high temperatures, and it is probably also the case at lower temperatures.

In conclusion, we should state that we have purposely omitted reference to the formulæ of Clausius, Van der Waals, and Sarrau. It appears to us that the relations of a and b in our formula to the volume is by no means so simple as that expressed by their formulæ.

XXIII. *Secular Cooling of the Earth in relation to Mountain-Building.* *By* T. Mellard Reade, *C.E., F.G.S.*[*]

In Chapter XI. of my 'Origin of Mountain-Ranges' I attempted to show that the effects of secular cooling on the earth's crust would not be that assumed by what is now called the "Contraction theory" of Mountain-formation.

But, first, it will be well to state what this theory is. Roughly speaking, then, the earth in its present state is considered to be divided into two parts; a cooled solid outer crust which does not contract, and a heated nucleus which diminishes in volume as it parts with its heat. From this it results that the hard crust, in fitting itself from time to time to the shrinking nucleus, throws up the ridges of the earth's surface called Mountain-ranges. This, in its crude form, is the theory which is usually presented to the reader.

A very little consideration will serve to show that the effects of the cooling of a body like the earth will be very much more complex.

If we assume the crust to be a hard shell, say 30 miles thick, with a temperature of 50° at the outer surface, and 3050° at the inner surface, we shall find that the circumferential contraction of the inner surface of the shell in cooling will be much more relatively than the radial contraction of the earth from the centre to this zone; consequently the hard crust will at this zone be not in a state of compression, but one of tension or stretching.

By means of a diagram (plate 18) I have shown that an outer portion or shell of this hard crust, of very limited depth, will be in a state of compression; but by far the larger volume of the crust will constitute a "shell of contraction," where all the rocks constituting it will tend to stretch along circumferential lines. This stretching would end in the fracture of the shell were it not for the weight of the superimposed mass, which ensures continuity by what I have called "compressive extension."

It is unnecessary for me to go into further details as the reader can find the full particulars in the chapter referred to[†]; excepting that I would point out that the external cooling portion of the earth is shown to be divisible into a thin outer

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[*] Communicated by the Author.

[†] 'Origin of Mountain-Ranges,' pp. 121–128.

shell—of compression greatest at the external surface, gradually diminishing downwards until it is *nil* at the under surface, and an inner shell of much greater volume, in which the beds are all in a state of tension by contraction, the contraction being greatest at the zone of greatest cooling, and *nil* on the inner and outer surfaces of the shell. Hence it follows that at the zone of contact of these imaginary shells the rock will be neither subject to tension nor compression.

Below these shells exists an uncooled nucleus, geologically speaking, of hitherto nearly constant volume.

In a recent paper* Mr. C. Davison arrives at practically the same results, although he makes no reference to my work, which was published in 1886. If Mr. Davison reached his results without knowing of mine it strengthens my case considerably, and I am glad to have confirmation from so able a mathematician.

But while we are practically agreed as to the strains set up in the crust of a cooling earth, we entirely differ as to their geological consequences. Without committing myself to Mr. Davison's numerical results, I will for present purposes accept his calculation that the shell of compression is now five miles deep.

I confess I cannot follow Mr. Davison in considering that "owing to the pressure of the continental masses, crust-stretching by lateral tension takes place principally beneath the ocean-basins." Assuming that the isogeotherms follow approximately the surface-contours of the land, whether beneath or above the level of the oceans, the greater pressure will be over the ocean-basins, as the weight of ocean-water will be additional to that of an equal depth of crust. It is, however, quite as probable that the cooled crust is thicker under the oceans, and such is assumed by many physicists. It also appears to me to be a physical misconception to suppose that a portion of the earth's crust would stretch like a cooling bar of iron held firmly in a vice at either end, or that the pressure of the continental masses would take the place of the vices by holding down the contracting body. I have tried to form a mental picture of the relation Mr. Davison wishes to show existing between coast-lines, earthquakes, sedimentation, and mountain-building, but must confess my inability to do so with the aid only of the abstract.

The statement that "the rate of mountain-making diminishes with the increase of time" is opposed to all geological fact. The greatest mountains are the youngest; and Mur-

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* "On the Distribution of Strain in the Earth's Crust resulting from Secular Cooling, with special reference to the Growth of Continents and the Formation of Mountain-chains." Received by the Royal Society, April 7, 1887, and just published in abstract.

chison even held the view that at an early period of the earth's history there were no great mountains *. We have ample grounds for believing neither view to be correct.

Prof. Bonney, in a Note appended to Mr. Davison's paper, attempts to further elucidate the geological results that would follow from this view of the effects of the earth's contraction. I am sorry that I am unable to agree with his suggestions. When I wrote the chapter referred to I felt, if the views there expounded were admitted (which they practically are by Messrs. Davison and Bonney), that they were fatal to the Contraction-theory of Mountain-building; I think so still.

In what way, I may ask, could gneissic axial cores, having the fan-structure which characterizes most great mountain-ranges, have been forced up and the overlying beds thrown back upon themselves, if the tangential compression, gradually diminishing downwards, did not extend five miles vertically into the earth's crust? for during the early periods of mountain-making the depth of crust under compression must have been, according to Mr. Davison, much less.

For myself I go much further, and say that such axial cores and other phenomena of mountain-ranges which I have elsewhere dealt with at length, cannot be satisfactorily accounted for except on the hypothesis, within certain limits, of the compression increasing, not diminishing, with the depth.

To illustrate these effects I made a series of experiments, which are partially detailed in pp. 331–333 and plate 42.

Although we differ radically in some important particulars, I welcome Messrs. Davison and Bonney's theoretical investigations, as truth cannot but gain by the friction of various ideas.

XXIV. *On an Addition to* Bunsen's *Ice-Calorimeter. By* C. V. Boys, *A.R.S.M., Demonstrator of Physics at the Science Schools, S. Kensington*†.

IT is probable that no single instrument has excited so much admiration on the part of physicists as Bunsen's calorimeter; its beautiful simplicity, its marvellous sensibility, and its accuracy in skilled hands place it in the first rank among instruments employed in investigations for which it is available. In our uncertain climate, however, it is not often found in actual use; for unless a large supply of perfectly pure snow, free from all saline contamination, can be obtained, it cannot be employed to its fullest advantage, and because possibly, according to Bunsen's directions, it should be used in a room not much above the freezing-point.

* 'Siluria,' fifth edition, p. 498.

† Communicated by the Author.

If the instrument is filled properly with pure water boiled to drive out the air, and is then set up in common ice scraped into an artificial snow with a chisel, the minute amount of salt included in cavities is sufficient to lower the temperature of the outer ice below the melting-point of the ice in the instrument; and thus, inappreciable in other ways as this difference is, it causes a steady freezing of the water in the calorimeter, which is quite sufficient to make accurate work impossible.

In consequence of the attention that has been given to this instrument, not only by the inventor but by other physicists of experience, I naturally hesitate to make any suggestion for its improvement. I dare not call the slight modification about to be described an improvement, but an addition is a term which I may safely employ.

The reason, as already stated, why this instrument fails to work satisfactorily when used with ordinary ice is that there is a slight difference of temperature between the inside and the outside. In consequence of the continuous contact of the outer ice or snow with the whole surface of the glass, the conductivity for heat is very great—that is, the amount that will enter or leave the instrument for 1° difference is very great. The amount of heat passing through the walls is the product of the difference of temperature by the conductivity. This product is usually made very small by making only one factor very small. It can more easily be made small by making both factors small; if one is very small, so much the better.

This process of reasoning is so obvious that it can hardly fail to have been used by some; but I have not heard that it has, nor am I aware of its ever having been put to the test of experiment. I have therefore lately compared the behaviour of an instrument put up in the usual way, but in common ice, with the same instrument arranged to have a small conductivity and in the same ice.

The plan that I have adopted to reduce the conductivity, while still retaining the power to increase it to its usual extent at will, is to provide a protecting cover of glass in which the instrument can lie, the two tubes passing through and being supported by a thin indiarubber cork fixed into the upper end of the glass cover.

There is a third hole in the cork, through which is passed a glass tube with a stopcock. If there is any hurry to cool the instrument, ice-cold water is poured in upon the ice so as to reach above the lower end of the protecting tube, and the stopcock opened and the air drawn out if necessary until the water reaches the cork. The water may be changed once or twice by blowing through the stopcock and drawing out the air again; thus the instrument is quickly brought to the freezing-point. The water may then be drained away, when the instrument will be left in an ice-cold chamber, but not in contact with anything but air, except at its upper end.

For this arrangement the two tubes above the joint should be two or three times the usual length, but an ordinary instrument can easily be adapted by prolonging these tubes at their upper end. It is essential that both tubes be carried through the ice much further than usual, so that no heat may be conducted down to the working part, as any stray heat would be more mischievous than usual, in consequence of the insulation of the instrument. The upper end cut off from a gas jar makes a convenient glass cover. Its projecting rim should be placed above, where it serves to support the whole instrument, if it is allowed to rest on a tripod-ring standing in the ice-box. The index-tube that I have used has a mean capacity of ·0001285 cub. centim. per millimetre, very nearly as fine as that used by Bunsen. The instrument was carefully filled with the distilled water from the general supply, which occasionally is found to be by no means pure. From the steady melting observed in the experiments, it would appear to contain some impurity. The ice used in the box was ordinary Norway ice. When the instrument was put up in the usual way in this ice, the movement of the index showed generally a melting in the instrument, which was reduced enormously when it was surrounded by its cover and placed in the same ice. With the cover, in some cases, there was a movement of not more than 1 millim. in about three hours; but I did not, in these cases, obtain a perfectly trustworthy comparison. In one case, when the movement was greater, I measured the rate with the cover, then without, and then with the cover again. It showed a melting at the rate of 4 millim. an hour with the cover, of 27 an hour without the cover, of 30 an hour without the cover the next day, and of 4 an hour with it on again. Thus, by means of the cover the instrument was brought from a state in which the unfavourable conditions made it almost unserviceable to one in which it could be well used. In another case I purposely put a trace of salt with the ice in the box, and found the rate of freezing to be from 8 to 10 times as great without the cover as with. In all these cases the instrument had been quickly taken from the ice, had its cover removed, and been at once replaced in the cavity left in the ice by the cover. The ice was then pressed down with a stick; but it is probable that there was by no means so continuous a current as there would have been had the freshly-cut ice been put in after the instrument. Hence it is probable that the ratio of about 7 to 1 observed hardly represents the full value of the insulation given by the cover. The temperature of the room during the experiments varied between 22° and 23° C. There can be little doubt that if favourable conditions, that is, pure snow outside, pure water inside, &c. were obtained, and if the instrument were more carefully insulated, the column might be kept far more steady than is possible in the usual way.

In a few words, by the use of the cover we allow the instrument to remain at its own freezing-point, instead of trying to compel it to assume some temperature which may be, which is sure to be, slightly different.

XXV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 141.]

April 6, 1887.—Prof. J. W. Judd, F.R.S., President, in the Chair.

THE following communications were read:—

1. "On the Rocks of the Malvern Hills." Part II. By Frank Rutley, Esq., F.G.S.

The details of the microscopic examination of the rocks constituted the principal part of the present paper. The Author finds that the truly eruptive rocks are more plentiful in the range than he was at first led to suppose. In all 33 rock-specimens were described, and in some cases Mr. Timmins's analyses were quoted. The author commenced with the rocks of the North Hill and concluded with those of the Raggedstone Hill. Rocks between a little south of the summit of the Worcestershire Beacon on to Wind's Point, those of Midsummer Hill, and those of Keys Hill were not collected. The following are the general results:—

	Eruptive.	Foliated.	Stratified.
North Hill...............	Hornblende-gabbro. Diorite. Quartz-syenite.	Gneissic Syenite. Gneissic Diorite.	Altered Tuff?
North Hill (above West Malvern).	Mica-diorite.	Biotite-gneiss.	
North Hill (The Dingle).	Mica-diorite.		
Worcestershire Beacon	Granulite? Granite. Diorite. Epidotite?		
Herefordshire Beacon	Eucrite. Basalt. Devitrified Obsidian.	Hornblendic Gneiss.	Diabase-tuff?
Swinyard's Hill.........	Pegmatite. Hornblende-pegmatite. Diorite.	Biotite-gneiss. Biotite-muscovite. Gneiss.	
Hollybush Pass.........	Diabase.		
Raggedstone Hill	....	Mica-schist. Micaceous Quartzite-schist.	Altered Sandstone. Quartzite.

In the first instance a separation must be effected of rocks showing foliation or lamination, or of which the origin has been sedimentary, from those which show no such structure, and which must be regarded as eruptive: there is, in fact, a banded and an unbanded series. The gneisses are altered volcanic tuffs or sedimentary rocks of eruptive material derived from the disintegration of rocks of dioritic or syenitic character.

The rocks of the North Hill, as may be gathered from the tabular classification, are truly eruptive in many cases; whilst the foliated varieties are made up of the *débris* of rocks rich in hornblende, which may have had an eruptive origin. The rocks of the Herefordshire Beacon are chiefly gneissic: the eucrite-basalt occurs at a buttress of the hill. The pegmatite of Swinyard's Hill has apparently been faulted into its present position. South of Midsummer Hill fine-grained gneissic rocks, quartzite-schists, &c., are met with.

There is no reason to suppose that the alteration of any ordinary sedimentary rocks could have resulted in such a vast amount of hornblende as is found in these gneisses. The gneissic rocks of the Malvern Hills may be composed of the detritus of eruptive rocks.

The rocks of the Malvern Hills show in their structure but little resemblance to the foliation induced by shearing, the crystals seldom exhibiting any marked lenticular form, while there is but little likeness to the pseudo-fluxion structure described by Lehmann, &c.

The Author concluded that the rocks of the Malvern Hills represent part of an old district consisting of plutonic and, possibly, of volcanic rocks associated with tuffs, sedimentary rocks composed mainly or wholly of eruptive materials, and grits and sandstones; that the structural planes in these rocks (sometimes certainly, at others possibly) indicate planes of stratification, and that the foliation, in many cases if not in all, denotes lamination due to deposition either in water or on land surfaces, probably more or less accentuated or altered by the movements which produced the upheavals, subsidences, and flexures prevalent in the range.

2. "On the alleged Conversion of Crystalline Schists into Igneous Rocks in County Galway." By C. Callaway, Esq., D.Sc., F.G S.

This paper was an inquiry into the theory, held by many Irish geologists, that granite and other igneous rocks are the last term in a progressive series in the metamorphism of aqueous sediments. The evidence collected by the Author was regarded as entirely hostile to this view. In Knockseefin, the typical section, he found diorite intrusive in gneiss and granite intrusive in the diorite, but no passage between any two. The igneous veins sometimes displayed a foliated structure. At Shaunarea the phenomena were similar; but the granite in contact with the gneiss was much crushed and decomposed. In the region south of Glendalough the intrusion of granite in diorite and schist gave rise to the peculiar mixtures which had been described as "metamorphosed conglomerate." The granite was intruded along the joints of the diorite, sometimes separating the joint-blocks from each other, and completely enclosing them. It was noticed that when schists were penetrated by granite isolated folia often retained their parallelism, and this was accounted for partly by the slowness of the intrusion, partly by regional pressure. Even when mere flakes of the schist were enclosed in granite there was no passage between the two. The granite, both in masses and veins, was often foliated, but the blocks of included diorite were not; and this seemed to suggest that the foliation of the granite was acquired before complete congelation of the larger masses. There was also a foliation concentric to included blocks of diorite.

At the town of Galway the "metamorphic sedimentary" rocks were a coarse-grained hornblendic gneiss of Hebridean aspect, and in some parts of it was a structure similar to that of the "metamorphosed conglomerate;" but the included blocks of diorite had acquired a definite orientation, apparently due to pressure. An igneous origin for some of the coarser gneisses was thus suggested. It was concluded that there was no proof of the conversion of schists into igneous rocks, the evidence collected tending to show, on the other hand, that igneous rocks were sometimes converted into schists.

3. "A Preliminary Inquiry into the Genesis of the Crystalline Schists of the Malvern Hills." By C. Callaway, Esq., D.Sc., F.G.S.

The Author's researches amongst the crystalline rocks of Connaught had suggested certain lines of investigation which had subsequently been followed out in the Malvern district. He had satisfied himself that many of the Malvern schists had been formed out of igneous rocks; but at present he limited himself to certain varieties.

The materials from which these schists were produced were diorite (several varieties), granite, and felsite.

The metamorphism had been brought about by lateral pressure. Evidence of this was seen in the intense contortion of granite-veins and in the effects of crushing as observed under the microscope.

The products of the metamorphism were divided into two groups:—

A. Simple schists, or those formed from one kind of rock. The varieties described were the following:—*Hornblende-gneiss*, or diorite which had been crushed and modified. *Mica-gneiss*, formed from granite. In the first stage of the crushing, the quartz and felspar lay in lenticular fragments, separated from each other by cracks, the fragments and cracks being roughly parallel. As the metamorphism proceeded, the cracks became less evident, and the respective minerals were flattened out into comparatively uniform folia. Mica gradually came in, at first in the form of a partial coating to felspar crystals, and, at a further stage, in regular folia. *Mica-schist*, formed from felsite. The felsite gradually acquired a parallel structure. *Pari passu* with this mechanical alteration, a mineral change was observed. Mica at first appears in very small quantity, either filling cracks or accentuating the parallelism. In a more advanced stage, the mica lies in imperfect folia, and sometimes forms a partial coating to grains of quartz. At last there is little left but quartz and mica, the latter in folia, and enveloping individual quartz granules.

B. Injection schists, formed by the intrusion of veins, which had acquired parallelism by pressure. Veins of diorite in diorite produced *duplex diorite-gneiss*, and veins of granite in diorite originated *granite diorite-gneiss*.

It was further noted that

(1) Generally the particular varieties of schist occurred in the vicinity of the igneous masses to which they were most nearly related in mineral composition.

(2) The mineral banding of the rocks in the field was more like vein-structure than stratification.

The Author accepted the received view of the age of the schists. The parallel structure was clearly antecedent to the Cambrian epoch, and the occurrence of similar rocks as fragments in the Uriconian conglomerate of Shropshire seemed to indicate that the Malvernian schists were older Archæan.

April 27.—Prof. J. W. Judd, F.R.S., President, in the Chair.

The following communications were read:—

1. "On the London Clay and Bagshot Beds of Aldershot." By H. G. Lyons, Esq., R.E., F.G.S.

The Author first described the section from Thorn Hill on the N. to Redan Hill on the S., plotted from the 6-in. Ordnance Survey on a scale of 6 in. to 1 mile horizontal, and 12 in. to 1 mile vertical. This section comprises beds from the Woolwich and Reading series to the Upper Bagshot inclusive. It showed a dip of 2¼° to the N., which is regular or nearly so throughout. A few feet of Upper-Bagshots occur on Thorn Hill (365 feet); at the base of these the Pebble-Bed crops out, forming also much of the surface of the South Camp. The Middle Bagshots on the south slope of the hill are estimated from the South Camp boring at 53 feet, with a marked clay-bed at the base; and below these a few feet of the Lower Bagshots are exposed in the intervening valley. The greater part of Redan Hill (364 feet) is made up of Lower Bagshots; but towards the top a few feet of the basal clays of the Middle Bagshots have been exposed by a recent trench. Although the elevation is practically the same as that of Thorn Hill, the rest of the Bagshot series is cut out owing to the northerly dip. These results differ from those of previous observers, *e. g.* the Geological Survey carry the Lower Bagshots to the top of the Redan Hill, as do Messrs. Monckton and Herries; whilst of the anticlinal, alleged by Mr. Irving to exist in this traverse, there appears to be no trace. The Author also observed that the arguments for overlap of the upper beds and for the erosion of the London Clay are not borne out by the facts.

The second section described runs from Gravel-Pit Hill on the N. to Ash Green on the S. It was drawn to the same scale, and showed the beds from the Chalk to the Middle Bagshots inclusive. Dip northerly 2¼° to 2° 50′ at south end. A spur of the Fox Hills (Gravel-Pit Hill) is seen to consist of Upper Bagshots of the normal type down to the lower shoulder of the spur, which is capped by the Pebble-beds marking the junction of the Upper and Middle Bagshots. The Ash-station well shows the basement-beds of the Lower Bagshots, of a character very similar to those in the Brookwood and South-Camp deep boring. The position of the outcrop of the London Clay also is in favour of a regular and persistent northerly dip, corresponding in degree with that given at East Wyke farm by Messrs. Monckton and Herries. The thickness of the London Clay was calculated at 330 feet, which is about the same as at South Camp, leaving no margin for its erosion before the deposition of the Bagshots.

The third section was drawn, also on the same scale, through Aldershot town, showing the beds from the Woolwich and Reading series to the Middle Bagshots inclusive. The dip is 2¼° to the N., and regular, as in the other two cases. The following thicknesses are given:—

		ft.
Middle Bagshots	about	55
Lower Bagshots	„	115
London Clay	„	335

It was inferred from various calculations, as also from direct observation, that the thickness of the London Clay shows no diminution throughout the section, being nearly the same also at Ash and at Aldershot Place.

In "Cæsar's Camp" the Pebble-bed occurs at altitudes ranging from 500 to 550 feet.

The Author concluded that wherever we can fix the top or base of the London Clay we get a northerly dip of 2¼° to 3°, showing a fairly constant thickness of from 330 to 340 feet. The same thing occurs from Odiham on the west to Ash on the east, whilst at Brookwood the London Clay is thicker. He also assumed the existence of a passage from the London Clay up into the Bagshot beds in the deep wells or borings at Wellington College, at Brookwood, and at South Camp. Hence at these points there can have been no great erosion or unconformity. The overlying Bagshots lie conformably on the London Clay and on each other.

2. "Supplementary Note on the Walton Common Section." By W. H. Hudleston, Esq., M.A., F.R.S., Sec.G.S.

The principal object of this paper was to point out the occurrence of certain beds of clay or loam in what are usually known as the "Lower Bagshot Sands" of West Surrey. It was shown that the sandy series, no. 3, of the previous paper is overlain by a second clay series, no. 4, whose mode of occurrence and lithology were described. This is again succeeded by a third sandy series, no. 5, which, it is believed, is maintained throughout the remainder of the cutting as far as the River Wey, with occasional clay patches deposited in small basin-shaped hollows of the sand.

The nature and geological position of the brick-earth of Hatch on Woburn Hill was next described. This forms a portion of the "clays most extensively developed between Addlestone and Chertsey," referred by Prof. Prestwich to his Middle Bagshots, and mapped as such by the Geological Survey. The clay is seen to occur as a lenticular mass, 21 ft. thick at its maximum, in a hollow of loose yellow sand; the current-bedding of the upper loamy layers is very marked towards the north end, with a strong false dip to the south, *i. e.* towards the centre of the basin.

Accepting as the true datum line for the base of the Middle Bagshots in this district, "the foliated clays, more or less sandy, having a thickness of 14 ft.," which are shown by Prof. Prestwich to be typically developed in the railway-cutting on Goldsworth Hill, it was contended that the Hatch brick-earth cannot be correlated with these. The true basal beds of the Middle Bagshots in this district differ somewhat in their physical characters; but it was on stratigraphical grounds mainly that the Author endeavoured to show that the Hatch brick-earth should, despite its argillaceous nature, be assigned to the Lower Bagshots. A diagrammatic section from St. George's Hill (245 ft.), through Woburn Hill (92 ft.), to St. Ann's Hill (230 ft.), was given, and the possible existence of a trough or synclinal towards the centre of the section discussed. It was shown from the position of the Hatch brick-earth that if the Lower Bagshots retain anything like the mean thickness of, say 120 ft., which prevails in this district, the London Clay surface must here be 60 ft. *below* O. D., on the supposition that these beds represent the basal clays of the Middle Bagshots; whereas, at Chertsey, in the valley of the Thames itself, the London Clay surface coincides with O. D.

In conclusion, it was held (1) that the more we study the Bagshot beds of this area the less likely are we to see a passage between the curiously diversified Lower Bagshots and the much more uniform and homogeneous London Clay; (2) that, until we realize the considerable though sporadic development of clays in the Lower Bagshots, we shall be in danger of referring beds to the Middle Bagshots which do not belong to them, and thereby give encouragement to a speculative stratigraphy which can only mislead.

XXVI. *Intelligence and Miscellaneous Articles.*

A NEW METHOD OF REDUCING OBSERVATIONS RELATING TO SEVERAL QUANTITIES.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

MAY I call the attention of your readers to a method of reducing observations relating to several quantities, which I have described in the current number of *Hermathena* (Trinity College, Dublin). This method is a generalization of that *Method of Situation* which Laplace has applied to observations relating to a single quantity, in the second supplement to his *Théorie Analytique.*

The method may be thus described in the case of two variables, x and y. Find an approximate solution by some rough process (such as simply adding together several of the equations so as to form two independent simultaneous equations). Take the point thus determined as a new origin, and substitute in the n (transformed) equations for one of the variables x a series of values $\pm\delta$, $\pm 2\delta$, &c. Corresponding to each of these substitutions we have n equations for y. For each of these systems determine the *Median* according to Laplace's *Method of Situation*. This series of Medians forms one locus for the sought point. A second locus is found by transposing x and y in the directions just given. The intersection of these loci is the required point.

The method may be extended to any number of variables. For example, in the case of four variables, x, y, z, w, we should construct one table containing the Median values of w corresponding to each triplet of values assigned to x, y, z; and three similar tables for the Median values of x, y, z, respectively. The system of values for x, y, z, w, which is identical in all the tables, forms the required solution.

The advantages claimed for the new method are that, while in the typical case of the laws of facility being all Probability Curves, the generalized *Method of Situation* is only slightly less accurate, and is considerably less laborious, than the *Method of Least Squares*; in the abnormal case of Discordant Observations the proposed method is not only more convenient, but better.

It is much to be wished that some practical astronomer would give this method a trial by employing it in some laborious and important calculation.

F. Y. EDGEWORTH,
King's College, London.

A NEW PHOTOGRAPHIC SPECTROSCOPE. BY C. C. HUTCHINS.

The constant demands of spectrum analysis for ever increasing accuracy can be satisfied only by instruments of the highest dispersion and most perfect defining-powers; and when applied to photography the dispersion must be produced directly, and not by enlarging lenses at the camera. The large apparatus of Rowland does this most beautifully, as the writer, who has used it for the past year, can testify; but the fact that a large room must be set aside for its accommodation, and moreover that the large concave gratings are very difficult to obtain, will forbid its use to most workers.

I have therefore devised the following simple and, it would seem upon short trial, effective arrangement for producing the desired results:—

A rather long slit is placed at the focus of a crown-glass (or, better, quartz) lens of 40-feet focus. The ray from the slit, after passing through the lens, falls upon a large flat grating, mounted to turn about an axis passing through the middle line of the ruled surface. The spectrum is projected by the same lens upon a horizontal arc of 40-feet radius, and is observed a little to one side or above the slit. The spectrum will not be normal throughout its length unless the radius of projection be kept constant. I think this had better be provided for by employing two lenses of crown-glass, the one nearer the grating fixed, the other movable, than by the use of a corrected lens, to avoid the absorption of flint-glass in the achromatic combination.

More or less absorption when glass is used is unavoidable; and this, with the variation of the focal plane of the lens for light of different wave-lengths, constitutes the most serious defects of the apparatus—defects which are avoided in the Rowland instrument. However, compensating advantages are not wanting. The Rowland apparatus integrates all impressions received at the slit. The ordinary comparison-prism cannot be used with it, and the lower orders of spectra are too narrow to admit of convenient division at the camera; in fact, the spectra begin at nothing at the slit, and spread in a widening band as we move toward the higher orders.

The new instrument possesses the advantages of the analyzing-spectroscope. The spectrum can be made wide or narrow, or divided at the slit. A preliminary trial has given the following results:—A flat grating of 14,000 lines to the inch and a ruled surface $2 \times 1\frac{1}{2}$ inches was employed, with a lens of 37-feet focus. The latter was placed close to the grating, and its spectrum observed near the slit. In the second spectrum b_1 and b_2 were fully an inch apart as projected on a screen, and D_1 and D_2 had a separation of 13 to 14 millim.

The excellence of the definition is shown by the fact that, with an ordinary reading-glass of 6-inch focus, E was seen double, and fourteen lines were counted between D_1 and D_2. With this form of apparatus the amount of dispersion can be varied at pleasure by simply altering the relative distances of slit and camera from the grating, but in such a way that slit and camera shall occupy conjugate foci of the lens.—Silliman's *American Journal*, July 1887.

ON THE ELECTRIFICATION OF GASES BY IGNITED BODIES.

BY J. ELSTER AND H. GEITEL.

The results of this investigation may be summed up as follows:—

(1) The phenomenon that insulated conductors near an ignited body are electrically charged holds also for gases, which have been freed from dust by filtration through glycerine wadding.

(2) This holds also even when the rarefaction is raised to the extreme limits of a Crookes's tube.

(3) The electrification is positive for red heat; and all temperatures beyond in all gases as yet examined, with the exception of hydrogen, which at a high temperature has the opposite behaviour.

(4) For air and carbonic acid the maximum of electrification is at a yellow heat.

(5) The layer of gas which surrounds an incandescent body shows a different deportment as respects positive and negative electricity. That electricity whose sign is opposite that developed in the gas by ignition is most rapidly discharged.—Wiedemann's *Annalen*, No. 5, 1887.

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

[FIFTH SERIES.]

SEPTEMBER 1887.

XXVII. *On some Methods of Determining and Comparing Coefficients of Self-induction and Mutual Induction.* By Prof. C. Niven, *F.R.S.*[*]

§ 1. PROF. CAREY FOSTER'S paper, published in the Philosophical Magazine (February 1887), has lately recalled attention to the methods of comparing or determining coefficients of induction by means of the galvanometer. He has shown how to express the coefficient of induction of two coils in terms of the capacity of a condenser. The problem there presented is one of several which arise in the comparison of the three kinds of transient currents which are met with (1) in the charge of condensers, (2) in the mutual induction of two circuits, (3) in the self-induction of a circuit.

Of the corresponding problems which occur in the comparison of electrical constants three may be looked upon as still somewhat imperfectly solved. These are the comparison of two coefficients of self-induction, the comparison of a coefficient of self-induction of a coil with that of mutual induction between any other two coils, and the determination of a coefficient of self-induction in terms of the capacity of a condenser. Prof. Chrystal has indeed given, in his paper on the Differential Telephone, various solutions of the last; but his methods involve the use of the telephone, while Maxwell's method in this case requires that a single adjustment should secure both no steady current through the galvanometer and no transient current. The same remark applies to his method of comparing the coefficients of self-induction of two coils; and his

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[*] Communicated by the Author.

solution of the second problem only compares the coefficient of self-induction of a coil with that of the mutual induction between this coil and another.

It seemed desirable so to extend these methods that in the first two cases the adjustments for no permanent and no transient current should be independent of each other, and to obtain the means of comparing the self-induction of any coil with that of the mutual induction of any two other coils.

The differential galvanometer appears at first sight to possess special advantages for these problems; but the methods are easily adapted to the Wheatstone-bridge, which practically converts the ordinary into a differential galvanometer; and the adjustments with it are more easily effected than with the other, which usually requires for these methods that the coils of the galvanometer should be suitably shunted.

I shall therefore begin by giving methods adapted to the ordinary galvanometer, reserving to the end of each article an account of those which may be employed with the differential galvanometer.

§ 2. In these inquiries it is useful to know not only the condition that there should be no current through the instrument, but also the expression for the total flow when this condition is not satisfied, in order to be able to select the best arrangement of the bridge so that the galvanometer shall be most sensitive, and in the case of the differential galvanometer to ascertain what shunts should be used with it. And for this purpose Maxwell's method is particularly suitable.

Let a network of conductors be considered to form a system of meshes the currents round which are $\dot{x}$, $\dot{y}$, . . . , and let electromotive forces E_{xy} .. act along the sides of these: let also

$$\left.\begin{aligned}\mathrm{T}&=\tfrac{1}{2}(\mathrm{L}\dot{x}^2+2\mathrm{M}\dot{x}\dot{y}+\mathrm{N}\dot{y}^2+\ldots),\\ \mathrm{F}&=\tfrac{1}{2}(a_{11}\dot{x}^2+2a_{12}\dot{x}\dot{y}+a_{22}\dot{y}^2+\ldots),\\ \mathrm{E}&=\mathrm{E}_{xy}(\dot{x}-\dot{y})+\ldots,\end{aligned}\right\} \quad . \quad . \quad (1)$$

T being work dissipated in heat in the conductors.

The general equations of the system are typified by

$$\frac{d}{dt}\left(\frac{d\mathrm{T}}{d\dot{x}}\right)+\frac{d\mathrm{F}}{d\dot{x}}=\frac{d\mathrm{E}}{d\dot{x}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

If we have also condensers, the poles of which are attached to the network, each of these may be considered as forming an additional mesh; and if u be the charge of one of these at any time, the current round the corresponding mesh is $\dot{u}$, and must be included in the expressions for T, F, E. The energy employed in charging the condensers will usually be of the form

$$\mathrm{H}=\tfrac{1}{2}\left(\frac{u^2}{\mathrm{C}}+\ldots\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

C being the capacity of the condenser whose charge is u. The general equations of the system are of the form

$$\frac{d}{dt}\left(\frac{d\mathrm{T}}{d\dot{x}}\right)+\frac{d\mathrm{F}}{d\dot{x}}+\frac{d\mathrm{H}}{dx}=\frac{d\mathrm{E}}{d\dot{x}}. \quad . \quad . \quad . \quad . \quad (4)$$

To find the impulsive currents at the instant of making or breaking circuit we may integrate (4) once for all. We thus obtain

$$\left.\frac{d\mathrm{T}}{d\dot{x}}\right|_0^{t'}+\int\frac{d\mathrm{F}}{d\dot{x}}\,dt=0.$$

The first term of this equation involves the initial and final currents of the system, which may in most cases be written down at once: the second term

$$=a_{11}x+a_{12}y+\ldots,$$

where $x, y \ldots$ are the total flows of the currents round the several meshes. In any particular case, therefore, we may easily write down this equation if we know the expressions for T, F.

The equation of the type

$$\frac{d}{dt}\left(\frac{d\mathrm{T}}{d\dot{u}}\right)+\frac{d\mathrm{F}}{d\dot{u}}+\frac{d\mathrm{H}}{du}=0$$

must be treated differently. Suppose, for example, that we are considering the effect of making the battery-current. When the currents are steady and the condensers have received their full charge,

$$\frac{d}{dt}\cdot\frac{d\mathrm{T}}{d\dot{u}}=0,$$

and the equation becomes

$$\frac{d\mathrm{F}}{d\dot{u}}+\frac{d\mathrm{H}}{du}=0;$$

the symbols $\dot{x}, \dot{y}, u \ldots$ denoting the final values of these magnitudes, and $\dot{u}$ being put $=0$.

§ 3. *To Balance the Current of Self-induction by a Condenser**.

Method I.—Suppose the resistances in AB, BC, CD, DA, AP be a, b, d, c, R; that in the battery B; L the coefficient

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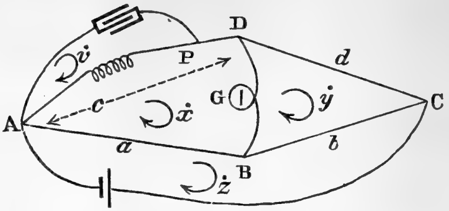
* Since this paper was placed in the printers' hands I have seen Mr. Rimington's paper in the July number of the Philosophical Magazine, from which it appears that he has anticipated me in some of the results of this article.

of self-induction of the coil or electromagnet in AD; C the



capacity of the condenser; the currents in the meshes as marked.

$T=\frac{1}{2}L(\dot{x}-\dot{u})^2$,

$F=\frac{1}{2}\{a\overline{\dot{x}-\dot{z}}^2+R\overline{\dot{x}-\dot{u}}^2+\overline{c-R}\dot{x}^2+G\overline{\dot{x}-\dot{y}}^2+d\dot{y}^2+b\overline{\dot{y}-\dot{z}}^2+B\dot{z}^2\}$,

$H=\frac{1}{2}u^2/C$.

The equations for the transient currents are

$$a\,.\,\overline{x-z}+R\,.\,\overline{x-u}+\overline{c-R}x+G\overline{x-y}=-L\dot{x}_0,$$

$$dy+b\,.\,\overline{y-z}\qquad-G\overline{x-y}=0,$$

$$-a(x-z)-b\,.\,\overline{y-z}+Bz\qquad=0,$$

$$-R\dot{x}_0+u/C=0,$$

$\dot{x}_0$ being the final current in AD.

Putting $x-y=\gamma$,

$$\overline{a+c+G}\,.\,\gamma+\overline{a+c}\,y-az=(CR^2-L)\dot{x}_0,$$

$$-G\,.\,\gamma+\overline{b+d}\,y-bz=0,$$

$$-a\gamma\quad-\overline{a+b}\,y+\overline{a+b+B}\,.\,z=0.$$

The determination of γ is simplified by recollecting that $\frac{c}{a}=\frac{d}{b}$, and thus

$$\gamma=\frac{(CR^2-L)\dot{x}_0\,.\,b}{a+c+G\,.\,b+aG},$$

and

$$\dot{x}_0=\dot{z}_0(a+b)/(a+b+c+d)$$

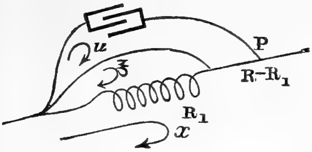
$$=\frac{a}{a+c}\cdot\frac{E}{B+\frac{c(a+b)}{a+c}}=\frac{aE}{B\,.\,\overline{a+c}+c\,.\,\overline{a+b}}$$

$$\gamma=\frac{E(CR^2-L)ab}{(B\,.\,\overline{a+c}+c\,.\,\overline{a+b})(\overline{a+c}\,.\,b+\overline{a+b}\,.\,G)}.$$

The condition $\gamma=0$ implies that $L=CR^2$. If this should involve the use of inconveniently high resistances for R, we may diminish the effect of L by shunting R_1, the coil itself, by a resistance S. The expressions for T, F will be changed into

$$T=\tfrac{1}{2}L\,.\,\overline{\dot{x}-\dot{\xi}}^2,$$

$$F=\tfrac{1}{2}\{R_1\,.\,\overline{\dot{x}-\dot{\xi}}^2+S\,.\,\overline{\dot{\xi}-\dot{u}}^2+R'\cdot\overline{\dot{x}-\dot{u}}^2+\ldots\},$$



where R_1 is the resistance of the coil, R the whole resistance between A and P, $R'=R-R_1$.

The expression for γ will be the same as before, except the first factor, which now becomes

$$C\left(R'+\frac{R_1S}{R_1+S}\right)^2-L\left(\frac{S}{R_1+S}\right)^2=CR^2-L\left(\frac{S}{R_1+S}\right)^2.$$

This remarkable proposition, that the effect of a shunt on the self-induction of a coil is to diminish it in the ratio

$$S^2:(R_1+S)^2,$$

is of great use in comparing two coils with each other.

If we write $\gamma=E(CR^2-L)/\Delta$,

$$\Delta=(B\,.\,\overline{a+c}+c\,.\,\overline{a+b})(\overline{a+c}\,.\,b+\overline{a+b}\,.\,G)/ab.$$

The value of Δ indicates the most suitable magnitudes to choose for the other resistances in the bridge.

(1) If $a=b$,

$$\Delta=(Bc+\overline{B+2c}\,.\,a)\left(1+\frac{c+2G}{a}\right).$$

This expression is a minimum when

$$a^2=\frac{Bc(c+2G)}{B+2c},$$

$$\Delta=(\sqrt{Bc}+\sqrt{\overline{B+2c}\,.\,\overline{c+2G}})^2.$$

For example, if $c=600$, $B=20$, $G=300$,

$$a^2=\frac{20\times 600\times 1200}{1220};$$

and a may be chosen about 100 ohms.

(2) If c and b be chosen, Δ will be a minimum when

$$b^2.\overline{a+c+G}=aG.(B.\overline{a+c}+ac).$$

As an illustration of these methods, the field-magnets of an Elwell-Parker dynamo were measured by a condenser having a capacity of $\frac{1}{3}$ microfarad. The resistance of the field-magnets was found to be 23·5 ohms, and when shunted the combined resistance of the magnets and shunt was 7·6. a, b, d were taken $=100, 100, 1031$; and by the aid of a rheostat and coils the resistance c was made up to 1031, when no permanent current was observed.

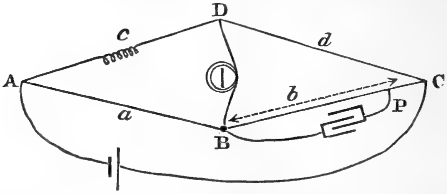
When $R=1011$, the kick due to the self-induction appeared to be balanced by the condenser; but the observation was not quite satisfactory, as the current of self-induction seemed to last longer than the other.

Thus,

$$L.\left(\frac{7{\cdot}6}{23{\cdot}5}\right)^2=(1011)^2.\frac{1}{3}\times 10^{-6}.$$

$$L=3{\cdot}256 \text{ earth-quadrants.}$$

Method II.—The condenser may be placed in BC, one pole being movable along BC. Calling $BP=R$, and using the same symbols as before, we find



$$\gamma=E.\frac{CR^2c-Lb}{b\Delta},$$

Δ having the same meaning as above.

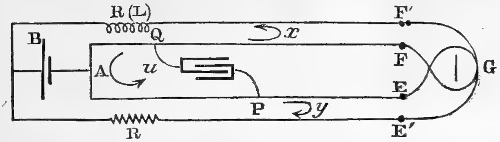
When $\gamma=0$,

$$CR^2c-Lb=0,$$

This method really constitutes an extension of Maxwell's; but it does not appear to possess any advantage over the one given above.

Method III.—Using the differential galvanometer.

The coil whose resistance is R is put in circuit with one coil of the galvanometer (G), and an additional resistance in $AF = c$,



the other coil of the galvanometer being put into circuit with similar resistances. The poles of the condenser are moved along AF and AE, or one only till the galvanometer gives no kick.

The steady current in each branch is

$$\frac{2E}{2B+R+G+c};$$

and we have in this case

$$T = \tfrac{1}{2}L\dot{x}^2,$$

$$F = \tfrac{1}{2}\{a\,.\,\overline{\dot{x}-\dot{u}}^2 + \overline{R+G+c-a}\,.\,\dot{x}^2 + b\,.\,\overline{\dot{y}+\dot{u}}^2 + \overline{R+G+c-b}\,.\,y^2 + B.\overline{\dot{x}+\dot{y}}^2\},$$

$$H = \tfrac{1}{2}\frac{u^2}{C}.$$

The equations for the transient current at make are

$$a\,.\,\overline{x-u} + \overline{R+G+c-a}\,.\,x + B\,.\,\overline{x+y} = -L\dot{x}_0,$$

$$b\,.\,\overline{y+u} + \overline{R+G+c-b}\,.\,y + B\,.\,\overline{x+y} = 0,$$

$$-(a-b)\dot{x}_0 + \frac{u}{C} = 0;$$

whence

$$x-y = \frac{1}{R+G+c}\big((a+b)u - L\dot{x}_0\big)$$

$$= \frac{2E\{\overline{a^2-b^2}\,.\,C-L\}}{(R+G+c)(2B+R+G+c)}.$$

The condition for no deflection is

$$\overline{a^2-b^2}\,.\,C-L=0.$$

It may happen, however, that if the galvanometer have a high resistance the deflections produced are always small, even when the condenser is not introduced: in this case we may improve matters by shunting each coil of the galvanometer. In this case G will stand for the combined resistance of the galvanometer and shunt; and if S be the shunt, G_0 the resistance of each coil,

$$\frac{1}{G}=\frac{1}{S}+\frac{1}{G_0};$$

the total flow through both coils

$$=\frac{S}{S+G_0}(x-y)=\frac{G}{G_0}(x-y);$$

and this will be a maximum when

$$(R+G+c)(2B+R+F+c)/G \text{ is a minimum,}$$

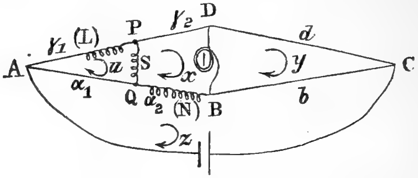
that is, when

$$G^2=(R+c)(2B+R+c).$$

§ 4. *To Compare two Coefficients of Self-induction.*

The following method, which is a modification of Maxwell's, was suggested by the one which follows and by that given in § 5.

Method I.—Wheatstone's-bridge arrangement. The coil L, whose resistance is γ_1, is first balanced by resistances α_1, b, d for steady currents; α_1 is then placed in series with the second coil (N), whose resistance suppose $=\alpha_2$, and is balanced by putting γ_2 in series with γ_1; α_1, γ_2, b, d being supposed to have no appreciable self-induction. The conditions for this are



$$\alpha_1/\gamma_1=\alpha_2/\gamma_2=b/d.$$

When these conditions are accurately satisfied no steady current should pass through the galvanometer, whether P and Q are connected by a wire or not. If the second adjustment should throw the first slightly out, the right position of Q, P being kept fixed, may be easily got by making the first part of α_2 and the last part of α_1 parts of the same wire, along which we may have sliding contact. Kohlrausch's form of the bridge is convenient for this purpose. It is also desirable in most cases to interpolate a resistance into γ_1 besides that of the coil itself.

A resistance S is then placed in PQ till no "kick" is given on making contact. Our equations in this case are

$$\mathrm{T}=\tfrac{1}{2}(\mathrm{L}\dot{u}^2+\mathrm{N}\,.\,\overline{\dot{x}-\dot{z}}^{\,2}),$$

$$\mathrm{F}=\tfrac{1}{2}\{\gamma_1\dot{u}^2+\mathrm{S}\,.\,\overline{\dot{x}-\dot{u}}^{\,2}+\gamma_2\dot{x}^2+\mathrm{G}\,.\,\overline{\dot{x}-\dot{y}}^{\,2}+d\dot{y}^2+b\,.\,\overline{\dot{y}-\dot{z}}^{\,2}$$
$$+\alpha_1\,.\,\overline{\dot{z}-\dot{u}}^{\,2}+\alpha_2\,.\,\overline{\dot{x}-\dot{z}}^{\,2}+\mathrm{B}\dot{z}^2\}.$$

From these,

$$\begin{array}{lll}\gamma_2 x+\mathrm{S}\,.\,\overline{x-u}+\alpha_2\,.\,\overline{x-z}+\mathrm{G}\,.\,\overline{x-y}=\mathrm{N}\,.\,\overline{\dot{z}_0-\dot{x}_0}, & & \\ dy+b\,.\,\overline{y-z} & -\mathrm{G}\,.\,\overline{x-y}=0, & \\ \gamma_1 u-\alpha_1\,.\,\overline{z-u}-s\,.\,\overline{x-u} & & =-\mathrm{L}\dot{u}_0=-\mathrm{L}\dot{x}_0;\end{array}$$

whence

$$u(\alpha_1+\gamma_1+\mathrm{S})=\mathrm{S}x+\alpha_1 z-\mathrm{L}\dot{x}_0.$$

Substituting in the two former equations, and putting $x-y=-\theta$,

$$-\mathrm{G}\theta+\left(\alpha_2+\gamma_2+\mathrm{S}\,.\,\frac{\alpha_1+\gamma_1}{\alpha_1+\gamma_1+\mathrm{S}}\right)x-\left(\alpha_2+\frac{\alpha_1\mathrm{S}}{\alpha_1+\gamma_1+\mathrm{S}}\right)z$$
$$=\mathrm{N}(\dot{z}_0-\dot{x}_0)-\mathrm{L}\dot{x}_0\,\frac{\mathrm{S}}{\alpha_1+\gamma_1+\mathrm{S}},$$

$$(b+d+\mathrm{G})\theta+(b+d)x\qquad -bz\qquad =0.$$

Now if we put

$$\alpha_2+\frac{\alpha_1\mathrm{S}}{\alpha_1+\gamma_1+\mathrm{S}}=f,\quad \alpha_1+\alpha_2=a,\quad \gamma_1+\gamma_2=c.$$

Since

$$\gamma_1=\alpha_1\,.\,\frac{d}{b},\quad \gamma_2=\alpha_2\,.\,\frac{d}{b},$$

$$\{(b+d+\mathrm{G})f+b\mathrm{G}\}\theta=\frac{\dot{z}_0 b}{b+d}\cdot\left(\mathrm{N}d-\mathrm{L}b\,.\,\frac{\mathrm{S}}{\alpha_1+\gamma_1+\mathrm{S}}\right).$$

$$\theta=\mathrm{E}b\left(\mathrm{N}d-\mathrm{L}b\,\frac{\mathrm{S}}{\alpha_1+\gamma_1+\mathrm{S}}\right)\Big/(b\mathrm{G}+\overline{b+d+\mathrm{G}}\,.f)(\mathrm{B}\,.\,\overline{b+d}+b\,.\,\overline{c+d}).$$

The condition of no flow, and therefore no deflection, on making or breaking contact is

$$\mathrm{N}d-\mathrm{L}b\,.\,\frac{\mathrm{S}}{\alpha_1+\gamma_1+\mathrm{S}}=0.$$

If $b=d$, and therefore $\alpha_1=\gamma_1$,

$$\mathrm{N}=\mathrm{L}\,.\,\frac{\mathrm{S}}{2\alpha_1+\mathrm{S}};$$

and the transient currents from each coil will be a maximum when

$$b^2=f\mathrm{G}(2\mathrm{B}+c)/(\mathrm{G}+2f).$$

As an example, the coils of the dynamo formerly used were compared with the field-magnets of an old dynamo of the Ladd pattern. The former of these we shall call A, the latter B; their coefficients of self-induction L, N.

b and d were each taken = 100 ohms.

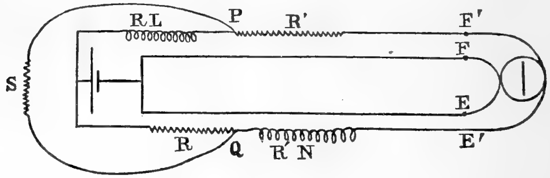
100 ohms with A in series were balanced by a resistance, the last portion of which is a wire with sliding-contact. B was then put in series with these resistances and slide, and balanced by a rheostat. The new adjustments generally put the other out a little, but by moving the sliding-key the proper position could be found when there was no current, whether the key be up or down. By making S=25 the two transient currents appeared equal, though the comparison could not be made very exactly on account of the difference of duration of the currents.

Thus,
$$L/N=\frac{247+25}{25}=10{\cdot}88.$$

I have not been able hitherto to try these methods with a ballistic galvanometer, nor do I see at present any means of making the two time-constants equal.

The result obtained above agrees with an estimate of N made by using a condenser, as in § 3.

Method II.—By the differential galvanometer.



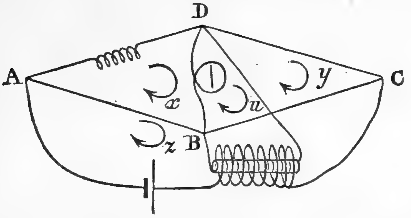
The coils whose coefficients L, N are to be compared are first balanced by rheostats in the two coils of the galvanometer, and points P, Q are then found such that the needle remains unaltered whether PQ be open or closed; then a resistance S is placed between PQ till the needle gives no deflection on opening circuit. When this is the case,
$$LS-N(S+2R)=0.$$
The expression for the flow is
$$\gamma=\frac{(LS-N\,.\,\overline{S+2R})E}{(2B+R+R'+G)(SR+R'+2RR'+G\,.\,\overline{S+2R})}.$$
When it is found desirable to use shunts for the two circuits of the galvanometer, G will represent the combined resistance of each circuit and its shunt, and the total flow in the two

circuits will be $$\frac{G}{G_0}\cdot\gamma.$$

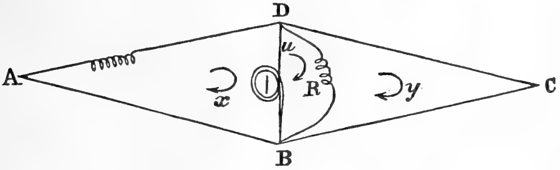
This may be made a maximum in the usual way.

§ 5. *To Compare the Coefficient of Mutual Induction of Two Coils with the Coefficient of Self-induction of a Third Coil.*

One of the coils of the pair is placed in the battery-circuit, and the other is connected to BD as a shunt to the galvanometer. The resistance of this part will be called R, that of the other will be included in the battery. The self-induction of the latter coil produces no effect on the galvanometer; this is likewise the case with R, because there is no permanent current through it. The coefficient of mutual induction will, as usual, be denoted by M; the other coil is placed in AD, as in the figure.



In dealing with this case the galvanometer, coil R, BC, CD must be looked upon as lying in one plane.



Thus,

$$T=\tfrac{1}{2}(L\dot{x}^2+2M\dot{z}\,.\,\overline{\dot{u}-\dot{y}}),$$

$$F=\tfrac{1}{2}\{c\dot{x}^2+a\,.\,\overline{\dot{x}-\dot{z}}^2+d\dot{y}^2+b\,.\,\overline{\dot{y}-\dot{z}}^2+G\,.\,\overline{\dot{x}-\dot{u}}^2+R\,.\,\overline{\dot{u}-\dot{y}}^2+B\dot{z}^2\}.$$

We observe also that

$$\dot{u}_0=0, \quad \dot{x}=\dot{y}_0.$$

Thus,

$$a\,.\,\overline{x-z}+cx+G\,.\,\overline{x-u}=-L\dot{x}_0,$$

$$b\,.\,\overline{y-z}+dy+R\,.\,\overline{y-u}=\quad M\dot{z}_0,$$

$$-G(x-u)-R(y-u)\quad=-M\dot{z}_0.$$

If $x-u=\gamma$,

$$-G\left(1+\frac{b+d}{R}\right)\gamma+\overline{b+d}\,.\,u-bz=-\frac{M}{R}\dot{z}_0(b+d)$$

$$(a+c+G)\gamma+\overline{a+c}\,.\,u-az=-L\dot{x}_0.$$

From which we obtain, eliminating u, z and putting

$$\dot{x}_0=\dot{z}_0 b/(b+d),$$

$$\dot{z}_0=E(b+d)/(B\,.\,\overline{b+d}+d\,.\,\overline{a+b}),$$

$$\gamma=\frac{E\left(\frac{aM\,.\,\overline{b+d}^2}{R}-Lb^2\right)}{\left(\overline{a+c+G}.b+aG.1+\frac{b+d}{R}\right)(B\,.\,\overline{b+d}+d\,.\,\overline{a+b})}.$$

B, it must be remembered, includes the resistance of one of the coils as well as that of the battery.

R includes the resistance of the other coil of the pair, with the additional resistance necessary to neutralize the kick of the galvanometer. For example, the coefficient of induction of a certain pair of coils used for experimental purposes was compared with the coefficient of self-induction of the field-magnets of an old dynamo of the Ladd pattern. The resistance of one of the coils (that put as a shunt to the galvanometer) was 10·5 ohms, $c=1{\cdot}79=d$, $a=b=1000$, and an additional resistance of 167 ohms was required for a balance;

$$L/M=\frac{(1001{\cdot}79)^2}{1000\times 177{\cdot}5}=5{\cdot}65.$$

On the Use of Shunts in finding Coefficients of Mutual Induction.

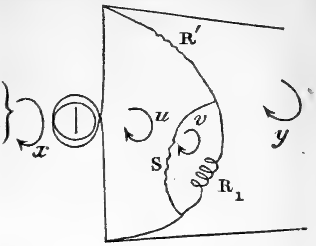
Let $R=R_1+R'$; where R_1 is the resistance of the coil proper, R' the added resistance, and let R_2 be the resistance of the second coil of the pair; then it is easy to see that if we shunt part of the current passing through R_2 into a shunt S, the coefficient of mutual induction will be diminished in the ratio

$$S : R_2+S.$$

A precisely similar rule may be proved to hold good as the result of shunting part of the current through R_1 into a shunt S.

$$T = M\dot{z}_0(\dot{v}-\dot{y}),$$

$$F = \tfrac{1}{2}\left\{G\,.\,\overline{\dot{x}-\dot{u}}^2 + S\,.\,\overline{\dot{u}-\dot{v}}^2 + R_1\overline{\dot{v}-\dot{y}}^2 + R'\,.\,\overline{\dot{u}-\dot{y}}^2 + \ldots\right\}$$



The equations are of the form

$$ax + G\,.\,\overline{x-u} + \ldots = -L\dot{x}_0,$$
$$-G\,.\,\overline{x-u} + S\,.\,\overline{u-v} + R'\,.\,\overline{u-y} + \ldots = 0,$$
$$-S\,.\,\overline{u-v} + R_1\,.\,\overline{v-y} + \ldots \quad = -M\dot{z}_0,$$
$$-R_1\,.\,\overline{v-y} - R'\,.\,\overline{u-y} + \ldots \quad = \quad M\dot{z}_0.$$

The third of these equations gives

$$(R_1 + S)v = R_1 y + Su - M\dot{z}_0,$$

which reduces the second and fourth respectively to

$$-G\cdot\overline{x-u} + \left(R' + \frac{SR_1}{R_1+S}\right)\overline{u-y} + \ldots = -M\frac{S}{R_1+S}\cdot\dot{z}_0,$$

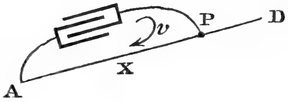
$$-\left(R' + \frac{R_1 S}{R_1+S}\right)\overline{u-y} + \ldots = \quad M\frac{S}{R_1+S}\cdot\dot{z}_0.$$

These are exactly what we should have got by supposing the resistance of R_1 affected in the usual way, and M diminished in the ratio $S : R_1 + S$.

§ 6. *To express a Coefficient of Mutual Induction in Terms of the Capacity of a Condenser.*

It is evident that a condenser and resistance placed in AD instead of an electromagnet will balance the current of mutual induction. The conditions for this may be easily inferred from what precedes; but they may be independently deduced by the general method.

With the first figure of § 5, except that a condenser whose poles are at A, P occupies the place of the coil L, we have



$$T = M\dot{z}\,.\,\overline{\dot{u}-\dot{y}},$$
$$F = \tfrac{1}{2}[X\,.\,\overline{\dot{x}-\dot{v}}^2 + \overline{c-X}\,.\,\dot{x}^2 + \ldots],$$
$$H = \tfrac{1}{2}v^2/C.$$

The first equation is

$$X(x-v) + (c-X)x + \ldots = 0\,;$$

and we have an additional equation,

$$-X\dot{x}_0=v/C,$$

whence

$$vX=cX^2\dot{x}_0.$$

Thus the subsequent investigation is the same as that of the last section if we replace L by CX^2, and the condition of zero-deflection is

$$aMb+d^2=CX^2Rb^2.$$

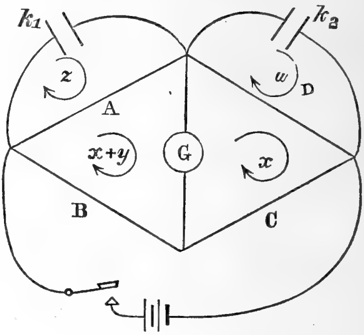
XXVIII. *Note on Comparing Capacities.*

By E. C. Rimington*.

Let two condensers, of capacities K_1 and K_2, be connected to the arms A and D of a Wheatstone-bridge, as shown in woodcut; and let

$$AC = BD,$$

so that there is a balance for permanent currents.



Let $x+y$, x, z, and w be the cyclic currents at some moment after the battery-circuit is broken, and when the condensers are discharging; and let q_1 be the charge on K_1 and q_2 that on K_2 at this moment. Then we have

$$(A+B+G)(x+y)-Gx-Az=0\,\dagger,$$

$$(C+D+G)x-G(x+y)-Dw=0;$$

also

$$z=\frac{dq_1}{dt} \quad \text{and} \quad w=\frac{dq_2}{dt}.$$

Therefore

$$(A+B)x+(A+B+G)y=A\frac{dq_1}{dt},$$

$$(C+D)x-Gy=D\frac{dq_2}{dt}.$$

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* Communicated by the Physical Society: read June 25, 1887.

† The self-induction of the galvanometer need not be taken into account, as the current in it commences and ends at zero. The arms A, B, C, and D are supposed to possess no self-induction.

Hence

$$y = \frac{\begin{vmatrix} A\dfrac{dq_1}{dt}, & A+B \\ D\dfrac{dq_2}{dt}, & C+D \end{vmatrix}}{\begin{vmatrix} A+B+G, & A+B \\ -G, & C+D \end{vmatrix}},$$

or

$$\Delta y = A(C+D)\frac{dq_1}{dt} - (A+B)D\frac{dq_2}{dt}, \quad . \quad . \quad (1)$$

where

$$\Delta = G(A+B) + (A+B+G)(C+D).$$

Integrating,

$$\Delta\int_0^\infty y\,dt = A(C+D)\int_{K_1Ax_0}^0 dq_1 - (A+B)D\int_{K_2Dx_0}^0 dq_2,$$

where x_0 is the permanent current through A and D when the battery is on.

But $\int_0^\infty y\,dt$ is the quantity which passes through the galvanometer, $=q$.

Therefore

$$\Delta q = x_0\{K_2D^2(A+B) - K_1A^2(C+D)\}.$$

If x_1 is the permanent current in B and C,

$$x_0 = x_1\frac{C}{D};$$

and since $AC=BD$, we have

$$q = x_1 BD\frac{K_2C-K_1B}{G(B+C)+B(C+D)}, \quad . \quad . \quad . \quad . \quad (2)$$

which may be written

$$q = x_1\frac{K_2C-K_1B}{\dfrac{G(B+C)}{BD}+\dfrac{C}{D}+1}.$$

If now A and D be made infinite, which will obviate the necessity of first adjusting for a permanent balance,

$$q = x_1(K_2C-K_1B).$$

Also

$$x_1 = \frac{E}{\rho+B+C},$$

where ρ is the internal resistance and E the E.M.F. of the testing battery.

$$q=\frac{E}{\rho+B+C}(K_2C-K_1B).$$

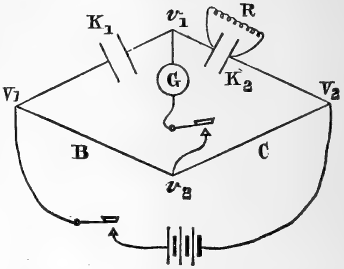
If $q=0$,

$$\frac{K_1}{K_2}=\frac{C}{B}.$$

If a key be also put in the galvanometer-circuit, and the battery-key be first depressed and then, after a certain interval of time, the galvanometer-key, we have Gott's method of comparing capacities. This allows the condenser longer time to charge; and if the galvanometer when its key is depressed shows no throw, it indicates that the potentials v_1 and v_2 are equal, and that therefore

$$\frac{K_1}{K_2}=\frac{C}{B},$$

as before.



If, however, either of the condensers has an appreciable leakage, the result will be false by this method.

Suppose the condenser K_2 has an insulation-resistance R, that of K_1 being infinite. On depressing the battery-key, as the condensers are in series, they take initial charges each equal to Q.

$$Q=(V_1-v_1)K_1=(v_1-V_2)K_2=(V_1-V_2)\frac{K_1K_2}{K_1+K_2}.$$

Let an interval of time t elapse before the galvanometer-key is depressed, and at the end of it let Q_1 and Q_2 be the charges on K_1 and K_2 respectively, and v the value of v_1. Then

$$Q_2=Qe^{-\frac{t}{K_2R}},\text{ where } e=2\cdot718,$$

$$Q_2=(V_1-V_2)\frac{K_1K_2}{K_1+K_2}e^{-\frac{t}{K_2R}}$$

and

$$Q_1=(V_1-v)K_1=\{(V_1-V_2)-(v-V_2)K_1$$

$$=\left\{(V_1-V_2)-\frac{Q_2}{K_2}\right\}K_1,$$

$$Q_2=(V_1-V_2)\left(1-\frac{K_1}{K_1+K_2}e^{-\frac{t}{K_2R}}\right)K_1.$$

Now

$$(v-\mathrm{V}_2)=\frac{\mathrm{Q}_2}{\mathrm{K}_2}=(\mathrm{V}_1-\mathrm{V}_2)\frac{\mathrm{K}_1}{\mathrm{K}_1+\mathrm{K}_2}e^{-\frac{t}{\mathrm{K}_2\mathrm{R}}}$$

and

$$(\mathrm{V}_1-v)=\frac{\mathrm{Q}_1}{\mathrm{K}_1}=(\mathrm{V}_1-\mathrm{V}_2)\left(1-\frac{\mathrm{K}_1}{\mathrm{K}_1+\mathrm{K}_2}e^{-\frac{t}{\mathrm{K}_2\mathrm{R}}}\right).$$

Hence

$$\frac{v-\mathrm{V}_2}{\mathrm{V}_1-v}=\frac{\mathrm{K}_1 e^{-\frac{t}{\mathrm{K}_2\mathrm{R}}}}{\mathrm{K}_1(1-e^{-\frac{t}{\mathrm{K}_2\mathrm{R}}})+\mathrm{K}_2}.$$

Also

$$\frac{v_2-\mathrm{V}_2}{\mathrm{V}_1-v_2}=\frac{\mathrm{C}}{\mathrm{B}}.$$

If, therefore, $v=v_2$, or there appears to be a balance,

$$\frac{\mathrm{C}}{\mathrm{B}}=\frac{\mathrm{K}_1 e^{-\frac{t}{\mathrm{K}_2\mathrm{R}}}}{\mathrm{K}_1(1-e^{-\frac{t}{\mathrm{K}_2\mathrm{R}}})+\mathrm{K}_2}$$

or

$$\mathrm{K}_1=\frac{\mathrm{K}_2\mathrm{C}}{(\mathrm{B}+\mathrm{C})e^{-\frac{t}{\mathrm{K}_2\mathrm{R}}}-\mathrm{C}}.$$

So that, unless t be made very small compared to $\mathrm{K}_2\mathrm{R}$, we cannot compare K_1 and K_2 unless we know R and t. In the case of no leakage the conditions will be the same as if there were no key in the galvanometer circuit.

To find the best conditions for the test:—Taking equation (2), and substituting for x_1 its value

$$\frac{\mathrm{E}}{\rho+\frac{(\mathrm{A}+\mathrm{D})(\mathrm{B}+\mathrm{C})}{\mathrm{A}+\mathrm{D}+\mathrm{B}+\mathrm{C}}}\times\frac{\mathrm{D}}{\mathrm{C}+\mathrm{D}},$$

which equals $\dfrac{\mathrm{ED}}{\rho(\mathrm{C}+\mathrm{D})+\mathrm{D}(\mathrm{B}+\mathrm{C})}$, since $\mathrm{AC}=\mathrm{BD}$, we obtain

$$q=\frac{\mathrm{EBD}^2(\mathrm{K}_2\mathrm{C}-\mathrm{K}_1\mathrm{B})}{\{\rho(\mathrm{C}+\mathrm{D})+\mathrm{D}(\mathrm{B}+\mathrm{C})\}\{\mathrm{G}(\mathrm{B}+\mathrm{C})+\mathrm{B}(\mathrm{C}+\mathrm{D})\}}.$$

If $q=0$, $\mathrm{K}_2\mathrm{C}=\mathrm{K}_1\mathrm{B}$.

Suppose a fractional error p to be made in the value of C

Then

$$q=\frac{\mathrm{EBD}^2p\mathrm{K}_2\mathrm{C}}{\{\rho(\mathrm{C}+\mathrm{D})+\mathrm{D}(\mathrm{B}+\mathrm{C})\}\{\mathrm{G}(\mathrm{B}+\mathrm{C})+\mathrm{B}(\mathrm{C}+\mathrm{D})\}}$$

$$=\frac{\mathrm{E}p\mathrm{K}_2}{\left(\frac{\rho}{\mathrm{D}}+\frac{\rho}{\mathrm{C}}+\frac{\mathrm{B}+\mathrm{C}}{\mathrm{C}}\right)\left(\frac{\mathrm{G}}{\mathrm{D}}+\frac{\mathrm{GC}}{\mathrm{BD}}+\frac{\mathrm{C}+\mathrm{D}}{\mathrm{D}}\right)}.$$

Now the throw is proportional to $q\sqrt{\mathrm{G}}$ for a galvanometer whose coil-volume is constant. Therefore the throw is proportional to

$$\frac{\mathrm{E}p\mathrm{K}_2}{\left(\frac{\rho}{\mathrm{D}}+\frac{\rho}{\mathrm{C}}+\frac{\mathrm{B}+\mathrm{C}}{\mathrm{C}}\right)\left(\frac{\sqrt{\mathrm{G}}}{\mathrm{D}}+\frac{\mathrm{C}\sqrt{\mathrm{G}}}{\mathrm{BD}}+\frac{\mathrm{C}+\mathrm{D}}{\mathrm{D}\sqrt{\mathrm{G}}}\right)},$$

and this is to be a maximum; which it obviously is when

$$\sqrt{\mathrm{G}}\,.\,\frac{\mathrm{B}+\mathrm{C}}{\mathrm{BD}}=\frac{\mathrm{C}+\mathrm{D}}{\mathrm{D}}\frac{1}{\sqrt{\mathrm{G}}},\quad\text{or}\quad \mathrm{G}=\frac{\mathrm{B}(\mathrm{C}+\mathrm{D})}{\mathrm{B}+\mathrm{C}}.$$

If A and D are infinite, the higher the resistance of the galvanometer the better. Also, in this case,

$$q=\frac{\mathrm{E}p\mathrm{K}_2}{\frac{\rho}{\mathrm{C}}+\frac{\mathrm{B}}{\mathrm{C}}+1}=\frac{\mathrm{E}p\mathrm{K}_2}{\frac{\rho}{\mathrm{C}}+\frac{\mathrm{K}_2}{\mathrm{K}_1}+1}.$$

Here q is largest when C is largest.

To find the conditions under which a telephone may replace the galvanometer:—

In equation (1) we have

$$\Delta y=\mathrm{A}(\mathrm{C}+\mathrm{D})\frac{dq_1}{dt}-(\mathrm{A}+\mathrm{B})\mathrm{D}\frac{dq_2}{dt}.$$

Now, in order to employ a telephone, y must always be zero. Therefore

$$\frac{\mathrm{A}}{\mathrm{A}+\mathrm{B}}\frac{dq_1}{dt}=\frac{\mathrm{D}}{\mathrm{C}+\mathrm{D}}\frac{dq_2}{dt}.$$

But

$$\frac{\mathrm{A}}{\mathrm{A}+\mathrm{B}}=\frac{\mathrm{D}}{\mathrm{C}+\mathrm{D}},\ \text{since AC}=\text{BD}.$$

Hence

$$\frac{dq_1}{dt}=\frac{dq_2}{dt}.$$

Now, since there is no current in the galvanometer, its resistance may be anything: let it be zero. Then K_1 discharges through $\frac{\mathrm{AB}}{\mathrm{A}+\mathrm{B}}$, and K_2 through $\frac{\mathrm{CD}}{\mathrm{A}+\mathrm{D}}$.

Consequently

$$q_1 = x_0 K_1 A e^{-\frac{t}{K_1 \frac{AB}{A+B}}},$$

and

$$q_2 = x_0 K_2 D e^{-\frac{t}{K_2 \frac{CD}{C+D}}}.$$

Hence

$$\frac{dq_1}{dt} = -x_0 K_1 A \cdot \frac{A+B}{K_1 AB} e^{-\frac{t}{K_1 \frac{AB}{A+B}}}$$

$$= -x_0 \frac{A+B}{B} e^{-\frac{t}{K_1 \frac{AB}{A+B}}},$$

and

$$\frac{dq_2}{dt} = -x_0 \frac{C+D}{C} e^{-\frac{t}{K_2 \frac{CD}{C+D}}}.$$

Therefore

$$\frac{A+B}{B} e^{-\frac{t}{K_1 \frac{AB}{A+B}}} = \frac{C+D}{C} e^{-\frac{t}{K_2 \frac{CD}{C+D}}},$$

and

$$\frac{A+B}{B} = \frac{C+D}{D}.$$

Therefore

$$K_1 \frac{AB}{A+B} = K_2 \frac{CD}{C+D};$$

and, since $AC = BD$,

$$K_1 B = K_2 C,$$

$$\frac{K_1}{K_2} = \frac{C}{B}.$$

If there is self-induction in the four conductors, let L_1 be the coefficient of self-induction of A, L_2 of D, L_3 of B, and L_4 of C. Then

$$(A+B+G)y + (A+B)x + L_1 \frac{d(x+y)}{dt} + L_3 \frac{d(x+y)}{dt} - A \frac{dq_1}{dt} = 0.$$

$$-Gy + (C+D)x + L_2 \frac{dx}{dt} + L_4 \frac{dx}{dt} - D \frac{dq_2}{dt} = 0.$$

Solving for y, we have

$$\Delta y = (C+D) \left\{ A \frac{dq_1}{dt} - L_1 \frac{d(x+y)}{dt} - L_3 \frac{d(x+y)}{dt} \right\}$$

$$- (A+B) \left\{ D \frac{dq_2}{dt} - L_2 \frac{dx}{dt} - L_4 \frac{dx}{dt} \right\}.$$

Integrating,

$$\Delta q = (\mathrm{C}+\mathrm{D})\left\{\mathrm{A}\int_{\mathrm{K}_1\mathrm{A}x_0}^{0} dq_1 - \mathrm{L}_1\int_{x_0}^{0} d(x+y) - \mathrm{L}_3\int_{-\frac{\mathrm{A}}{\mathrm{B}}x_0}^{0} d(x+y)\right\}$$

$$-(\mathrm{A}+\mathrm{B})\left\{\mathrm{D}\int_{\mathrm{K}_2\mathrm{D}x_0}^{0} dq_2 - \mathrm{L}_2\int_{x_0}^{0} dx - \mathrm{L}_4\int_{-\frac{\mathrm{A}}{\mathrm{B}}x_0}^{0} dx\right\}$$

$$= x_0\,\frac{\mathrm{C}+\mathrm{D}}{\mathrm{C}^2}(\mathrm{BCD}^2\mathrm{K}_2 - \mathrm{B}^2\mathrm{D}^2\mathrm{K}_1 + \mathrm{L}_1\mathrm{C}^2 - \mathrm{L}_3\mathrm{CD} - \mathrm{L}_2\mathrm{BC} + \mathrm{L}_4\mathrm{BD}),$$

since AC = BD.

Also

$$x_0 = x_1\,\frac{\mathrm{C}}{\mathrm{D}},$$

where x_1 is the permanent current in the branches B and C.

$$\therefore\ \Delta q = x_1 \,.\, \frac{\mathrm{C}+\mathrm{D}}{\mathrm{CD}}(\mathrm{BCD}^2\mathrm{K}_2 - \mathrm{B}^2\mathrm{D}^2\mathrm{K}_1 + \mathrm{L}_1\mathrm{C}^2 - \mathrm{L}_3\mathrm{CD} - \mathrm{L}_2\mathrm{BC} + \mathrm{L}_4\mathrm{BD}).$$

If $q = 0$,

$$\mathrm{BD}^2(\mathrm{K}_2\mathrm{C} - \mathrm{K}_1\mathrm{B}) + \mathrm{C}(\mathrm{L}_1\mathrm{C} - \mathrm{L}_3\mathrm{D}) + \mathrm{B}(\mathrm{L}_4\mathrm{D} - \mathrm{L}_2\mathrm{C}) = 0.$$

Hence

$$\mathrm{K}_2\mathrm{C} = \mathrm{K}_1\mathrm{B}, \quad \mathrm{L}_1\mathrm{C} = \mathrm{L}_3\mathrm{D}, \quad \mathrm{L}_4\mathrm{D} = \mathrm{L}_2\mathrm{C};$$

or

$$\frac{\mathrm{K}_1}{\mathrm{K}_2} = \frac{\mathrm{C}}{\mathrm{B}} \quad \text{and} \quad \frac{\mathrm{L}_1}{\mathrm{L}_3} = \frac{\mathrm{L}_2}{\mathrm{L}_4} = \frac{\mathrm{D}}{\mathrm{C}}.$$

Again, since

$$\Delta = \frac{\mathrm{C}+\mathrm{D}}{\mathrm{C}}\{\mathrm{B}(\mathrm{C}+\mathrm{D}) + \mathrm{G}(\mathrm{B}+\mathrm{C})\},$$

$$q = x_1\,\frac{\mathrm{BCDK}_2 - \mathrm{B}^2\mathrm{DK}_1 + \mathrm{L}_1\frac{\mathrm{C}^2}{\mathrm{D}} - \mathrm{L}_2\frac{\mathrm{BC}}{\mathrm{D}} - \mathrm{L}_3\mathrm{C} + \mathrm{L}_4\mathrm{B}}{\mathrm{B}(\mathrm{C}+\mathrm{D}) + \mathrm{G}(\mathrm{B}+\mathrm{C})}$$

$$= x_1\,\frac{\mathrm{K}_2\mathrm{BC} - \mathrm{K}_1\mathrm{B}^2 + \mathrm{L}_1\frac{\mathrm{C}^2}{\mathrm{D}^2} - \mathrm{L}_2\frac{\mathrm{BC}}{\mathrm{D}^2} - \mathrm{L}_3\frac{\mathrm{C}}{\mathrm{D}} + \mathrm{L}_4\frac{\mathrm{B}}{\mathrm{D}}}{\frac{\mathrm{BC}}{\mathrm{D}} + \mathrm{B} + \frac{\mathrm{G}(\mathrm{B}+\mathrm{C})}{\mathrm{D}}}.$$

If A and D are now made infinite,

$$q = x_1(\mathrm{K}_2\mathrm{C} - \mathrm{K}_1\mathrm{B});$$

the same result that we obtain if there is no self-induction in the branches B and C.

XXIX. *Note on Beams fixed at the Ends.*

By W. E. AYRTON, *F.R.S., and* JOHN PERRY, *F.R.S.**

I. A HORIZONTAL beam with vertical loads, fixed at the ends, being given, to find the bending-moment everywhere: this is a problem the solution of which has not hitherto been put in an elementary form. A knowledge of the bending-moment everywhere leads, of course, to a complete knowledge of the strength and stiffness of the beam. The problem is quite soluble by a method which is obvious to any one who has worked out the theorem of three moments; but even for advanced students the work is tedious; and practically the answers known for the two cases—(1) when a uniform beam is loaded merely in the middle, (2) when a uniform beam is loaded uniformly—are regarded as roughly applicable in all cases which occur in practice.

If M is the bending-moment at a section, I the moment of inertia of the section about its neutral line, and E Young's modulus of elasticity for the material, then $\frac{M}{EI}$ is the curvature of the beam. If OO' is a short length of the beam, $\frac{M}{EI}OO'$ is the angle which the originally parallel sections at O and O′ now make with one another. Hence, if we divide the beam into a great number of parts, and if M is taken in the middle of each division, the moment of inertia there being I, the sum of all such terms as $\frac{M}{EI}.OO'$ (if OO' is one of the elementary lengths) gives the angle between the two end sections of the beam.

This principle, that $\Sigma\frac{M}{EI}.OO'$ gives the angle between the two end sections of the beam, is, of course, well known. It is the basis of the theory of flat spiral springs. As applicable to beams, it may be put in the words of Professor Cotterill ('Applied Mechanics,' § 169, p. 334):—" . . . The angle (i) between two tangents to the deflection-curve of a beam is proportional to the area of the curve of bending moments [had he been speaking of non-uniform beams, Mr. Cotterill would have said the area of the curve whose ordinate is $\frac{M}{EI}$] intercepted between two ordinates at the points considered." It is also the basis of Prof. Fuller's beautifully simple method of dealing

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* Communicated by the Physical Society: read June 11, 1887.

with arch-ribs fixed at the ends; and, indeed, it was when putting Prof. Fuller's method before our students that we discovered the following simple application of the principle to beams. In spite of the apparent rashness of such a statement about such a well-ventilated subject as the elasticity of beams, we believe our method to be quite new.

II. In the case of a horizontal beam fixed at the ends, the angle between the end sections remains equal to nothing; and hence, in this case,

$$\Sigma \frac{M}{I} = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Now, if the beam were merely supported at the ends, from a given system of loading it is easy to find what the bending-moment everywhere would be, either by numerical calculation or a link-polygon method. Let us suppose the diagram of this bending-moment m to be known. Then the bending-moment M of a beam fixed at the ends with the same loading, is $m-c$, where c is a constant; so that it is only necessary from the condition (1) to determine this constant c.

(1) is

$$\Sigma \frac{m-c}{I} = 0, \quad \text{or} \quad \Sigma \frac{m}{I} = \Sigma \frac{c}{I},$$

$$\text{or} \quad c = \frac{\Sigma \frac{m}{I}}{\Sigma \frac{1}{I}}. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The rule then is, knowing m and I at every point:—Divide the beam into any number, n, of equal parts; find $\frac{m}{I}$ at the middle of each part, and add all the values together: this is the numerator in (2). Fin d $\frac{1}{I}$ at the middle of each part, and add the values together: this is the denominator in (2); so that c is known. Subtract c from every value of m, or diminish all the ordinates of the m diagram by the amount c, and we have the M diagram; that is, the real diagram of bending-moment of the beam fixed at the ends, with any distribution of loading and any variation in cross section.

III. If the cross section is the same everywhere, it is obvious that (2) becomes

$$c = \frac{1}{n} \Sigma m, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

if the beam was divided into n parts. In fact, c is the average value of m for the whole span. The application of (3) to beams of uniform section, fixed at the ends with one load in the middle or with a uniformly distributed load, is very simple.

IV. The condition (1) is applicable when I at every cross section of the beam is previously settled in any arbitrary manner. Let us give to I such a value that the beam shall be of uniform strength everywhere, that is that

$$\frac{\mathrm{M}}{\mathrm{I}}y = f_c \text{ or } f_t, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where y is the greatest distance of any point in the section from the neutral line on the compression- or tension-side, and f_c and f_t are the constant maximum stresses in compression or tension to which the material is subjected in every section.

f_c may generally be taken as equal to f_t; and if d the depth of the beam, y may generally be taken as equa to $\frac{1}{2}d$. Hence (4) becomes

$$\frac{\mathrm{M}}{\mathrm{I}}d = \pm 2f, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

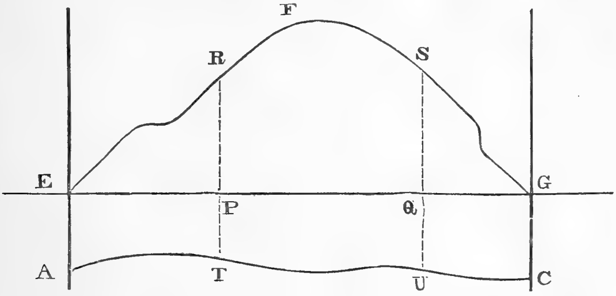
the + sign being taken when M is positive, and the − sign when M is negative.

Hence (1) becomes

$$\Sigma \frac{1}{\pm d} = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

the negative sign being taken from the ends of the beam to the points of inflexion, and the positive sign being taken between the two points of inflexion.

Now to satisfy (6) we have merely to solve the following



problem :—In the figure E A C G E is a diagram whose ordinates represent the value of $\frac{1}{d}$, or the reciprocal of the depth of the beam, which may be arbitrarily fixed; E F G E is a diagram of the values of m, easily drawn when the loading is known. We are required to find two points, P and Q, such that

$$\text{area ET} + \text{area QC} = \text{area PU}$$

and

$$\text{PR} = \text{QS}.$$

When found, these points P and Q are the *points of inflexion*, and PR or QS is what we called c. That is, $m - \text{PR}$ is the real bending-moment M at every place; and, knowing the bending-moment and depth d, it is easy to find I, as the condition of uniform strength is given in (5), or

$$\text{I} = \pm\frac{\text{M}d}{2f},$$

I being of course always positive.

We have found it very easy to solve this problem by trial. First find the area EC. Choose two points P′, Q′, whose ordinates P′R′ and Q′S′ are equal, and such that the area P′U′ seems to the eye nearly half the area EC. Measure the area P′U′, subtract from $\frac{1}{2}$EC, divide by about 4Q′U′ or 4P′T′ or by the mean value of these two. This will give an approximation to the error in the positions of P′ and Q′. With an easy exercise of one's judgment, it will probably not be necessary to make a second approximation.

V. If the depth of the beam is constant, the problem of finding the diagram of bending-moment for a beam of uniform strength is of course solved by finding two points, P and Q, whose distance asunder is half the length EG of the beam, and at which the ordinates PR and QS are equal. Subtract PR or QS from every ordinate of the m diagram E F G, and we have the M diagram.

It will be seen that, with any system of loading and any variation of section in a beam, it is quite easy to get the diagram of bending-moment; and also that, with any system of loading and any variation of depth of beam, it is quite easy to get diagrams of bending-moment and moment of inertia for beams of uniform strength. Many other applications of the method are obvious.

XXX. *On Binomial Biordinals.*

By Sir JAMES COCKLE, *M.A., F.R.S., F.R.A.S., &c.*[*]

65. THIS is the conclusion of a set of papers with various titles, but the articles of which are numbered consecutively. The latest paper of the set appeared in the number for May 1882 (vol. xiii. p. 357).

66. The biordinal of art. 62 is, by means of

$$v=x^a(1+x^2)^{\frac{e-a}{2}}u=x^a\mathrm{X}^{1-\frac{\mathrm{U}}{4}}u,$$

transformed into

$$v''+\frac{1}{4}\left\{\frac{1-\mathrm{I}^2}{x^2}+\frac{\mathrm{V}_2}{\mathrm{X}}-\frac{\mathrm{V}x^2}{\mathrm{X}^2}\right\}v=0,$$

where

$$\mathrm{V}=\mathrm{U}^2-4\mathrm{U},\quad \mathrm{V}_2=\mathrm{V}+\mathrm{I}^2-\mathrm{J}^2.$$

67. Hence, by the double substitution

$$x^2=t^3 \quad\text{and}\quad t^{-\frac{1}{4}}v=y,$$

we obtain

$$y^{\backprime\backprime}+3sy=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (19)$$

where the grave accents denote differentiations with respect to t, and (if $\mathrm{T}=1+t^3$),

$$s=\frac{3}{4^2}\left\{\left(\frac{4}{9}-\mathrm{I}^2\right)\frac{1}{t^2}+\frac{\mathrm{V}_2t}{\mathrm{T}}-\frac{\mathrm{V}t^4}{\mathrm{T}^2}\right\}.$$

68. Operating on (19) with $\frac{d}{dt}+\lambda$, we get (compare art. 16)

$$y^{\backprime\backprime\backprime}+3\lambda y^{\backprime\backprime}+3sy^{\backprime}+3(s^{\backprime}+3\lambda s)y=0. \quad . \quad . \quad . \quad . \qquad (20)$$

69. Take a new variable ζ connected with y by

$$e^{\int\lambda dt}y=e^{\int p dt}\zeta,$$

and giving rise to the terordinal

$$\zeta^{\backprime\backprime\backprime}+3p\zeta^{\backprime\backprime}+3q\zeta^{\backprime}+r\zeta=0. \quad . \quad . \quad . \quad . \quad . \qquad (21)$$

70. Then, by equating the criticoids of (20) and (21), we get

$$q=s+p^2-\lambda^2+p^{\backprime}-\lambda^{\backprime},$$

$$r=3(s^{\backprime}+2\lambda s+pq)+2(\lambda^3-p^3)+p^{\backprime\backprime}-\lambda^{\backprime\backprime}.$$

71. When (21) is binomial, then p, q, and r may be taken

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[*] Communicated by the Author.

of the respective forms

$$p=\frac{a_2}{t}+2(\omega+3)\frac{t^2}{\mathrm{T}},\quad q=\frac{m_1}{t^2}+\frac{m_2 t}{\mathrm{T}},\quad r=\frac{m_3}{t^3}+\frac{m_4}{\mathrm{T}},$$

wherein a_2, ω, and the m's are constants.

72. In general we should have

$$q=m_1t^{-2}+m_2t\mathrm{T}^{-1}+n_2t^4\mathrm{T}^{-2},$$

$$r=m_3t^{-3}+m_4\mathrm{T}^{-1}+n_3t^3\mathrm{T}^{-2}+n_4t^6\mathrm{T}^{-3};$$

but when (21) is binomial the n's vanish.

73. Now if ω and A be determined from

$$(\omega+2)(\omega+3)=\frac{\mathrm{V}}{16},\quad \mathrm{A}=\frac{1}{2}\left\{\omega+1+\frac{\mathrm{I}^2-\mathrm{J}^2}{16(\omega+2)}\right\},$$

all the n's will vanish if

$$\lambda=\frac{\mathrm{A}}{t}-\frac{\omega t^2}{\mathrm{T}},$$

wherein a_2 is arbitrary.

74. Let $\theta=-2(\omega+3)$; then p is obtained from λ by the substitution of a_2 for A, and of θ for ω.

75. Putting $-\frac{1}{3}b=\mathrm{V}$, and recurring to arts. 18 and 32, it will be seen that it is in fact the system (18) of art. 64 which is now under discussion.

76. Introducing a quantity K, defined by

$$\mathrm{K}=\frac{3^2}{4^2}\left(\frac{4}{9}-\mathrm{I}^2\right),$$

we get

$$m_1=a_2(a_2-1)-\mathrm{A}(\mathrm{A}-1)+\frac{1}{3}\mathrm{K},$$

$$m_2=\frac{3}{4^2}\mathrm{V}_2+2\omega(\mathrm{A}+1)+4(\omega+3)(a_2+1),$$

$$m_3=a_2{}^3-3a_2{}^2+2a_2+\mathrm{K}(a_2-2)$$
$$+\mathrm{A}(2\mathrm{A}^2-3a_2\mathrm{A}+3a_2+2\mathrm{K}-2),$$

$$m_4=\frac{3^2}{4^2}\mathrm{V}_2+\frac{3^2}{8}\mathrm{V}_2\mathrm{A}-2\mathrm{K}\omega+3a_2m_2-3\theta m_1$$
$$+6a_2{}^2\theta-6\mathrm{A}^2\omega+6(\omega+2).$$

77. Multiplying the binomial terordinal into $\mathrm{T}t^3$, substituting for T and putting $\mathrm{D}=t\,\frac{d}{dt}$, we get

$$(1+t^3)\mathrm{D}(\mathrm{D}-1)(\mathrm{D}-2)\zeta+3\{a_2+(a_2-\theta)t^3\}\mathrm{D}(\mathrm{D}-1)\zeta$$
$$+3\{m_1+(m_1+m_2)t^3\}\mathrm{D}\zeta+\{m_3+(m_3+m_4)t^3\}\zeta=0.$$

78. If we suppose this equation to be put under the form

$$f(\mathrm{D})\zeta+t^3\mathrm{F}(\mathrm{D})\zeta=0,$$

we shall have

$$f(\mathrm{D})=\mathrm{D}^3+3(a_2-1)\mathrm{D}^2+\{3(m_1-a_2)+2\}\mathrm{D}+m_3$$

$$=(\mathrm{D}+a_2-1)^3-\left\{3(\mathrm{A}-\tfrac{1}{2})^2+\frac{3^2}{4^2}\mathrm{I}^2\right\}(\mathrm{D}+a_2-1)$$

$$+(2\mathrm{A}-1)\left\{(\mathrm{A}-\tfrac{1}{2})^2-\frac{3^2}{4^2}\mathrm{I}^2\right\}.$$

79. Put $\mathrm{D}+a_2-1=\Delta$ and $\mathrm{A}-\frac{1}{2}=\mathrm{B}$, then

$$f(\mathrm{D})=\Delta^3-\left(3\mathrm{B}^2+\frac{3^2}{4^2}\mathrm{I}^2\right)\Delta+2\mathrm{B}\left(\mathrm{B}^2-\frac{3^2}{4^2}\mathrm{I}^2\right).$$

80. In other words,

$$f(\mathrm{D})=(\Delta+2\mathrm{B})(\Delta-\mathrm{B}+\tfrac{3}{4}\mathrm{I})(\Delta-\mathrm{B}-\tfrac{3}{4}\mathrm{I}).$$

81. The calculation of $\mathrm{F}(\mathrm{D})$ was still more troublesome than that of $f(\mathrm{D})$, and my original result was in defect by a unit. But the masterly hand which corrected the defect pointed out a shorter process of calculation, viz. that to be given in arts. 88, 89.

82. If we put

$$\mathrm{D}+a_2+2\omega+5=\nabla,\quad \mathrm{A}-\omega-\tfrac{1}{2}=\mathrm{G},$$

we shall at last find

$$\mathrm{F}(\mathrm{D})=(\nabla+2\mathrm{G})(\nabla-\mathrm{G}+\tfrac{3}{4}\mathrm{J})(\nabla-\mathrm{G}-\tfrac{3}{4}\mathrm{J}).$$

83. But a_2 is arbitrary; and if $a_2=1$, then ∇ becomes $\mathrm{D}+2\omega+6$, and $\nabla+2\mathrm{G}$ becomes $\nabla+2\mathrm{A}+5$, or $\nabla+2\mathrm{B}+6$, while $\nabla-\mathrm{G}$ becomes $\mathrm{D}-\mathrm{B}+3\omega+6$. Hence, writing the terordinal in the form

$$f(\mathrm{D})\zeta+\mathrm{F}(\mathrm{D}-3)t^3\zeta=0,$$

we have

$$\mathrm{F}(\mathrm{D}-3)=(\mathrm{D}+2\mathrm{B}+3)(\mathrm{D}-\mathrm{B}+3\omega+3+\tfrac{3}{4}\mathrm{J})(\mathrm{D}-\mathrm{B}+3\omega+3-\tfrac{3}{4}\mathrm{J}).$$

84. Moreover, turning to arts. 80, 82, 83, we see that if

$$f(\mathrm{D})=\phi(\mathrm{D},\mathrm{B},\mathrm{I}),$$

then

$$\mathrm{F}(\mathrm{D}-3)=\phi(\mathrm{D}+2\omega+3,\mathrm{B}-\omega,\mathrm{J}),$$

and

$$\phi(\mathrm{D},\mathrm{B},\mathrm{I})\zeta+\phi(\mathrm{D}+2\omega+3,\mathrm{B}-\omega,\mathrm{J})t^3\zeta=0.$$

85. This result may be further simplified by taking a new variable Z, given by $\mathrm{Z}=t^{-\mathrm{B}}\zeta$; for we thus obtain

$$\phi(\mathrm{D}+\mathrm{B},\mathrm{B},\mathrm{I})\mathrm{Z}+\phi(\mathrm{D}+\mathrm{B}+2\omega+3,\mathrm{B}-\omega,\mathrm{J})t^3\mathrm{Z}=0,$$

which (see arts. 80, 82–4) is equivalent to

$$(\mathrm{D}+3\mathrm{B})(\mathrm{D}+\tfrac{3}{4}\mathrm{I})(\mathrm{D}-\tfrac{3}{4}\mathrm{I})\mathrm{Z}$$

$$+(\mathrm{D}+3\mathrm{B}+3)(\mathrm{D}+3\omega+3+\tfrac{3}{4}\mathrm{J})(\mathrm{D}+3\omega+3-\tfrac{3}{4}\mathrm{J})t^3\mathrm{Z}=0.$$

86. Since

$$\omega^2+5\omega+6=\tfrac{1}{16}\mathrm{V}=\tfrac{1}{16}\mathrm{U}^2-\tfrac{1}{4}\mathrm{U},$$

therefore

$$\omega=\tfrac{1}{4}\mathrm{U}-3, \quad \text{or else } \omega=-\tfrac{1}{4}\mathrm{U}-2\,;$$

Also, by art. 79,

$$\mathrm{B}=\mathrm{A}-\tfrac{1}{2}=\tfrac{1}{2}\left\{\omega+\frac{\mathrm{I}^2-\mathrm{J}^2}{16(\omega+2)}\right\}.$$

87. Again, by art. 69,

$y=e^{\int(p-\lambda)dt}\zeta=$ (see arts. 71 and 73) $t^{a_2-\mathrm{A}}\mathrm{T}^{(\omega+2)}\zeta=$ (see art. 85) $t^{a_2-\mathrm{A}+\mathrm{B}}\mathrm{T}^{(\omega+2)}\mathrm{Z}=$ (see arts. 79, 86), $t^{a_2-\frac{1}{2}}\mathrm{T}^{(\omega+2)}\mathrm{Z}=t^{\frac{1}{2}}\mathrm{T}^{(\omega+2)}\mathrm{Z}$, when, as in art. 83, $a_2=1$.

88. The shorter process (see art. 81) is this. By art. 78,

$$\begin{aligned} f(\mathrm{D})&=\mathrm{D}^3+3(a_2-1)\mathrm{D}^2+\{3(m_1-a_2)+2\}\mathrm{D}+m_3\\ &=(\mathrm{D}+a_2-1)^3-\alpha(\mathrm{D}+a_2-1)+\beta \end{aligned}$$

suppose. Also (see art. 77),

$$\begin{aligned} \mathrm{F}(\mathrm{D})&=\mathrm{D}^3+3(a_2-\theta-1)\mathrm{D}^2+\{3(m_1+m_2-a_2+\theta)+2\}\mathrm{D}+m_3+m_4\\ &=(\mathrm{D}+a_2-1-\theta)^3-\gamma(\mathrm{D}+a_2-1-\theta)+\mathrm{H} \end{aligned}$$

suppose. Then

$$\mathrm{F}(\mathrm{D})-f(\mathrm{D})=-3\theta\mathrm{D}^2+3(m_2+\theta)\mathrm{D}+m_4$$

$$=-3\theta(\mathrm{D}+a_2-1)^2+(3\theta^2-\gamma+\alpha)(\mathrm{D}+a_2-1)+\mathrm{H}-\beta+\gamma\theta-\theta^3.$$

89. Hence, accents now meaning derived functions,

$$3m_1=f'(0)+3a_2-2=3(a_2-1)^2+3a_2-\alpha-2,$$

$$3m_2=\mathrm{F}'(0)-f'(0)-3\theta=-6a_2\theta+3\theta+3\theta^2-\gamma+\alpha\ \ldots, \quad (22)$$

$$\begin{aligned} m_4=\mathrm{F}(0)-f(0)&=-3\theta(a_2-1)^2+a_2(3\theta^2-\gamma+\alpha)\\ &\quad-\theta^3-3\theta^2+\gamma-\alpha+\mathrm{H}-\beta+\gamma\theta, \end{aligned}$$

$$m_4+3\theta m_1-3a_2m_2=6a_2{}^2\theta-\theta(\theta+1)(\theta+2)+(\gamma-\alpha)(\theta+1)+\mathrm{H}-\beta. \quad (23).$$

90. By means of (22) and (23) γ and H can be found. The results, after all reductions, are

$$\gamma=3\mathrm{G}^2+\tfrac{9}{16}\mathrm{J}^2 \quad \text{and} \quad \mathrm{H}=2\mathrm{G}(\mathrm{G}^2-\tfrac{9}{16}\mathrm{J}^2),$$

and the F(D) of art. 82 follows.

91. When I wrote art. 64 I was not aware of the turn which the discussion would take. The errata indicated at the beginning of vol. ix. of this Series should not be overlooked.

12 St. Stephen's Road, Bayswater, W.,
June 3, 1887.

XXXI. *The Effect of Change of Temperature in Twisting or Untwisting Wires which have suffered Permanent Torsion.* *By* Herbert Tomlinson, *B.A.*[*]

IN a paper recently communicated to the Physical Society[†], Mr. R. H. M. Bosanquet has drawn attention to the remarkable behaviour of a very fine hard-drawn platinum wire, which twisted or untwisted very appreciably when subjected to small variations of temperature. As some experiments made by myself nearly eight years ago, but of which the results have not been as yet published, seem to bear on the interesting phenomenon in question, I now venture to bring them forward. I had previously been investigating the effects of permanent extension and compression on the longitudinal elasticity of wires in the following manner:—A wire of from 60 to 90 centimetres in length and 1 millimetre in diameter was suspended vertically, with its upper extremity clamped to a rigid support, and its lower one attached to a little brass block provided with a hook at the bottom, to which weights could be attached. Permanent torsion was imparted to the wire, which was afterwards subjected to various amounts of longitudinal stress. The permanent twisting of the wire in the direction of a right-handed screw causes the portion A B C D (fig. 1) to be permanently extended along the diagonal A C and compressed along the diagonal B D (fig. 2).

Fig. 1.

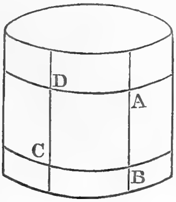
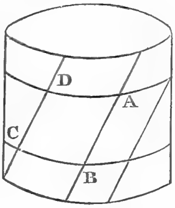


Fig. 2.



If now the wire be loaded, the stress will produce temporary twist or untwist according as the elasticity is greater or less in the direction B D than in the direction A C. A full account of these experiments will be found in the 'Philosophical Transactions'[‡], so that it will suffice to state here that all the wires examined showed temporary twist or untwist to be the result of loading. The amount of twist or untwist

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[*] Communicated by the Physical Society: read June 25, 1887.

[†] Phil. Mag. vol. xxiv. No. 147, August 1887, p. 160.

[‡] Part I. 1883: "The Influence of Stress and Strain on the Physical Properties of Matter."

produced by loading was comparatively small, and was measured by the usual mirror lamp-and-scale arrangement, the mirror being attached to the brass block at the end of the wire.

It then occurred to me that a similar arrangement might be used for testing the effect of permanent extension and compression on the thermal expansibility of a metal. A wire was suspended in the axis of an air-chamber, consisting of two concentric brass cylinders enclosing an annular space between them 6 millim. thick. The length of the air-chamber was 120 centim. and its outer diameter 10 centim. When the required amount of permanent torsion had been imparted to the wire, steam from a boiler was admitted into the annular space through a tube soldered into it near its lower extremity, and passed out through a similar tube near the top, where it was condensed in a vessel of cold water. After a period ranging from ½ hour to 2 hours, when the *permanent* untwisting which invariably accompanied the heating had ceased to become sensibly greater, the position of the light on the scale was noted and the action of the steam stopped. The annular space was now filled with cold water, and in about ten minutes afterwards the position of the light on the scale was again noted. The difference between the two readings was a measure of the amount of temporary twist or untwist produced by the change of temperature, the last being, on the average, 85° C. In several cases the heating and cooling were repeated with little or no variation in the amount of temporary twist or untwist. The length of each wire was nearly 120 centimetres and the diameter 1 millimetre.

The results of the experiments will be found in the following Table, in which are given the amount of temporary twist or untwist in scale-divisions, the amount of permanent untwist, and the load hanging on the wire during the experiment. The wires were all well annealed before being permanently twisted.

We may gather from the Table the following particulars:—

(1) Rise of temperature from 15° C. to 100° C. produces in permanently twisted iron, aluminium, and silver temporary untwist, showing for these metals less thermal expansibility in the direction of permanent extension than in the direction of permanent compression. With copper and platinum, on the contrary, rise of temperature produces temporary twist.

(2) The amount of temporary twist or untwist is in all cases small compared with the amount of permanent untwist produced by rise of temperature. It is also absolutely very small*.

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* 100 scale-divisions only represent torsion through an angle of 2° 20′.

(3) A few turns of permanent torsion suffice to produce the maximum alteration of thermal expansibility which can be effected by this means.

Metal ...	Copper.		Iron.		Aluminium		Silver.		Platinum.	
Load on the wire, in kilos.	1		1		½		1		1	
Number of complete turns of permanent torsion.	Permanent untwist produced by heating. P.	Temporary effect of heating. + signifies temporary twist, T.	P.	T.	P.	T.	P.	T.	P.	T.
10	......	.. ...	24	3·5−	800	74−	298	2·5−	...	4+
20	......	31+	...	......	...	71−				
30	......	......	...	6·0−						
40	......	......	...	......	...	......	...	3·0−		
50	555	36+	...	......	...	74−	...	......	63	10+
100	690	41+	...	5·0−	...	......	...	3·0−	138	
150	......	......	...	......	...	78−	...	......	273	12·5+
200	861	......	58	......	...	......	...	......	...	13·0+

Small variations of the load hanging on the wire produced no sensible effect on the temporary twist or untwist following on rise of temperature. The case, however, was different when large loads were employed. With platinum, for example, by increasing the load from 1 to 7 kilos. the temporary twist produced by rise of temperature was increased threefold. With copper also there was a similar effect produced by largely increasing the load. On the contrary, it would seem that with these metals, in which untwist is produced by rise of temperature, increase of load diminishes the untwist; and if the load be sufficiently large, the wire begins to be twisted temporarily by rise of temperature. This was found to be the case with annealed iron. The wire was twisted permanently with gradually increasing amounts of load on it, and after each twisting was slightly heated by running a burner up and down it. As before, with moderate loads there was temporary untwist on heating; when the load reached 10 kilos. there was neither temporary twist nor untwist; and finally, when the load was 16 kilos., there was very appreciable temporary twist.

Annealed piano-steel behaved like annealed iron; but with unannealed piano-steel rise of temperature caused a very appreciable temporary *twist*, instead of untwist, even with small loads on.

A remarkable feature about all these experiments was the great difference with different metals in the facility with which the effect of permanent torsion in one direction could be reversed by permanent torsion in the opposite direction. With copper, for example, one complete revolution in the opposite direction was sufficient to reverse the effect of 200 complete turns of permanent torsion, whilst with iron the difficulty of reversing the effect of previous torsion was considerable.

I am inclined to believe that the phenomenon observed by Mr. Bosanquet is to be, at any rate partly, attributed to unequal expansion in different directions. I have always found in wires which have been hard-drawn a certain amount, and sometimes a considerable amount of permanent torsion: this, we have seen, will cause temporary twist or untwist to be produced by rise of temperature. The amount of twist or untwist in any case, however, observed by myself was very much less than that observed by Mr. Bosanquet; and it would be of interest to ascertain how far the comparatively very large variation of torsion with small rise of temperature which occurred with the platinum wire used by him, is to be attributed to the comparatively great longitudinal stress on the wire[*].

XXXII. *Remarkable Effect on raising Iron when under Temporary Stress or Permanent Strain to a Bright-red Heat.* *By* Herbert Tomlinson, *B.A.*[†]

It has been shown in the preceding paper that an annealed iron wire which has been permanently twisted is temporarily untwisted when the temperature is raised to 100° C., provided there is not too great a load on the end of the wire; but that if the load on the end of the wire is sufficiently great, a temporary twist follows on the rise of temperature. Fresh experiments were therefore entered on with the view of ascertaining whether, with a small load on the end of the wire, the temporary untwist produced by rise of temperature would be changed to twist when the temperature exceeded a certain limit. A few preliminary trials were made by merely heating a portion of the permanently twisted wire with a burner; and it was found that when the wire reached a bright red heat a most remarkable and sudden change occurred, the wire sharply

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[*] See above, the effect of increasing the load on the wire.

[†] Communicated by the Physical Society: read June 25, 1887.

twisting in the same direction as that in which it had previously suffered permanent torsion. When the burner was removed and the wire was cooled to a temperature which seemed slightly lower than that at which the phenomenon occurred in the first instance, it as suddenly untwisted; and when it had again attained the temperature of the room, there remained a decided *permanent twist in addition to that which had been originally imparted**. This additional permanent twist is rendered more remarkable by the fact that rise of temperature beyond a dull red, but not extending to a bright red, is attended with a considerable *permanent untwist.* The phenomenon is evidently closely associated with one discovered by Mr. Gore so far back as 1869, and described in the 'Proceedings of the Royal Society' for that year. Mr. Gore's experiments were conducted in the following manner:—A thin iron wire, fixed at one end to a binding-screw, is attached at the other to an index which multiplies any motion of the wire; the wire is stretched horizontally by a feeble spring, and is heated by an electric current or by a row of gas-jets. According to Mr. Gore, no anomalous action is observed on *heating* the wire to bright incandescence; but when cooling begins, the index moves back until a moderate red heat is attained, when suddenly the pointer gives a jerk, indicating a momentary *elongation* of the wire during the progress of its contraction. In 1873 Professor Barrett extended Mr. Gore's researches; and in a paper full of interest†, entitled "Certain remarkable Molecular Changes occurring in Iron Wire at a low red Heat," showed that, under suitable conditions, the sudden change could be observed *not only on cooling, but on heating.* It is apparent, from the description given above, that the phenomenon observed by Mr. Gore resembles the one observed by myself, in that there is a sudden change in the iron when it has been raised to a bright red and afterwards cooled to a certain temperature. But in Mr. Gore's experiment the wire was under temporary *stress*, whilst in mine it was permanently *strained*; and whereas in the one case, on cooling below a certain temperature, there was a sudden *yielding* to the *stress*, in the other there was a sudden *diminution* of permanent *strain.* A careful consideration of the results of my own experiments, and of those of Mr. Gore and Professor Barrett, satisfied me that in this, as in many other instances, temporary stress and permanent strain act in oppo-

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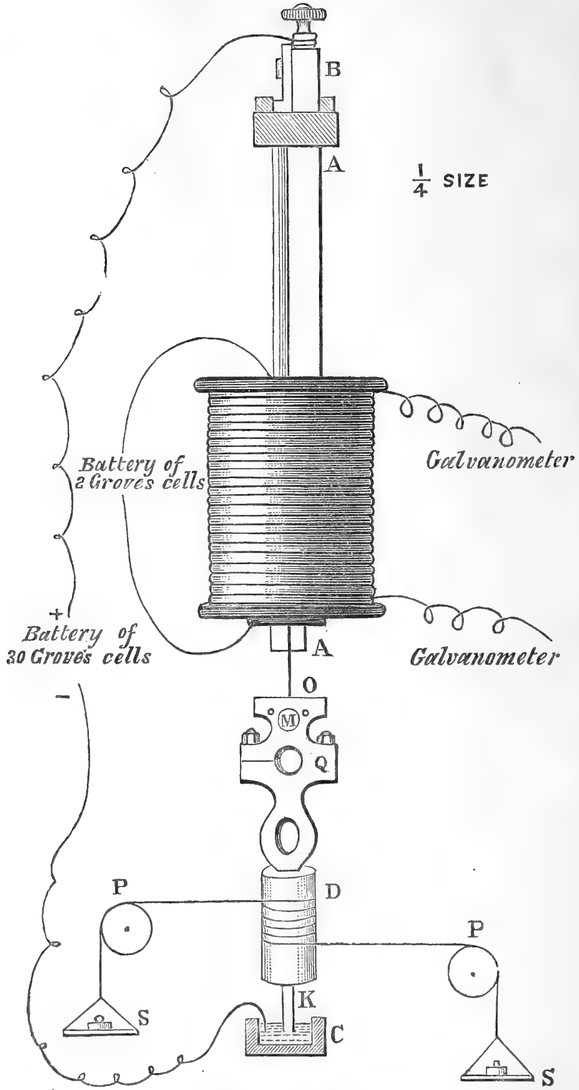
* This is not so if the wire be heated rather slowly; but, on the contrary, in this case there is a very decided *permanent untwist* (see experiment V.).

† Phil. Mag. ser. 4, vol. xlvi. p. 472.

site directions as regards their effects on the physical properties of matter. I was moreover led to test, not only the effects of torsional stress and strain, but also those of other mechanical stresses and strains, for the most part with arrangements which will now be described.

Fig. 1.



The wire was suspended vertically in the axis of a glass tube, A (fig. 1), being clamped at its upper extremity into a brass block, B, resting on a wooden support fitting on to the top of the tube, and provided with a terminal for making connexion with one pole of a battery of thirty Grove's cells, arranged ten in series and three in parallel arc. The current from the battery passed through a set of resistance-coils arranged so that the resistance could be altered by small amounts at a time ; the current was also conducted through an amperemeter and through the wire, passing in or out of the latter through the intermediation of a mercury-cup, C. The lower extremity of the wire was clamped into a second brass block, O, to which was secured a mirror, M, reflecting the light of a lamp on to a scale placed at a distance of one metre. This block was provided with a circular aperture, Q, into which, if necessary, a bar could be introduced, and at its lower extremity terminated in a brass cylinder, D, having a piece of rather stout brass wire, K, projecting from its centre, vertically downwards, and dipping into the mercury-cup C. When it was required to subject the wire to torsional stress, two fine silk threads were wrapped in opposite directions round D, and passed, as in the figure, over two fixed pulleys, P, to two small cardboard scale-pans, S, on which weights could be placed. When the wire was not required to be torsionally stressed the fine silk threads and the pulleys were dispensed with.

Experiment I.—A piece of very soft and carefully annealed iron wire[*], 30 centim. long and 1 millim. in diameter, was subjected to torsional stress in the manner described above. The pans S each weighed 10 grms., and in each of them was placed a load of 20 grms., so that the torsional couple amounted to 30 × 1·6 in gramme-centimetre-units[†]. A current of gradually increased amount was passed through the wire, and when the temperature approached a bright red the wire began to twist rapidly and permanently under the influence of the stress[‡]. The current was shortly afterwards stopped ; and when a temperature between bright red and dull red had been reached the wire began suddenly to twist further, the amount of the sudden twist being about 90 degrees. When the wire had cooled down to the temperature of the room there was left a considerable permanent twist. In this experiment there was no perceptible temporary untwist on heating, but merely a slight check in the rate at which the wire was permanently twisting as soon as the critical tempe-

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[*] This wire was specially prepared for me by Messrs. Johnson and Nephew, and is capable of suffering a permanent elongation of 25 per cent. before breaking.

[†] The diameter of D was 1·6 centimetre.

[‡] The *first* appearance, however, of sensible permanent twist occurred rather suddenly at *a dull red heat.*

rature was reached. Other experiments were made with smaller and smaller torsional stresses until only the cardboard scale-pans were left to produce torsion: even here, however, it was impossible to get any sign of temporary untwist on heating. This mode of producing torsion was accordingly abandoned, and the stress was applied in the manner described in Experiment II.

Experiment II.—A cork was fitted into the central aperture of the block O, and through the centre of the cork was passed a knitting-needle about 25 centim. in length and 1½ millim. in diameter, which was magnetized rather feebly by rubbing it with a small bar-magnet. The knitting-needle, when first placed in position, was nearly in the magnetic meridian, but afterwards the block B was turned until the needle eventually was nearly at right angles to the plane of the magnetic meridian, so that the wire might be under a feeble torsional stress due to the action of the earth's horizontal magnetic force on the needle. Immediately the wire reached a bright red temperature there was a very perceptible sudden temporary untwist, and on cooling a sudden temporary twist at nearly the same temperature[*]. This experiment was repeated with another piece of the same iron wire; but now the needle, instead of being twisted through 90 degrees from the north and south position, in which it originally lay when there was no torsion on the wire, was only twisted through about 10 degrees. Even the extremely feeble torsional stress now acting on the wire was not only sufficient to produce the phenomenon, but also a very decided *permanent* twist. The permanent twisting began directly the wire, on being heated, reached *a dull red heat*, and continued with increasing rapidity until the temporary untwist occurred at a bright red heat.

Experiment III.—The temperature at which, *on cooling*, the sudden change took place was evidently very much higher than that at which it occurred with the specimens of iron used by Mr. Gore and Professor Barrett; and this I felt inclined to attribute to the comparative softness of the iron used by myself. I accordingly tried several other specimens of iron and steel, both in the annealed and in the unannealed condition. With some of these the phenomenon did not occur on *cooling* until a dull red, or even at a still lower temperature; but it was found essential to its production that the iron *should have been previously raised to the temperature of bright incan-*

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[*] These effects can easily be shown by merely heating a small portion the wire with a Bunsen-burner.

descence[*]. On *heating*, the untwisting always showed itself at the temperature of bright red.

In consequence of the sudden change on cooling not occurring till a dull red heat had been reached in his experiments, Professor Barrett connected the phenomenon in question with another of equal interest. It is well known that, at a temperature of dull red, iron begins very rapidly to lose its magnetic properties ; and, according to Professor Barrett, the two phenomena occur simultaneously. It is no doubt the case that, on *cooling*, the sudden jerk occurs simultaneously with the sudden regaining of magnetic properties in *some* specimens[†] of iron or steel ; but it by no means follows that we have not two very distinct critical temperatures—one at or about a dull red, at which iron loses or regains its magnetic properties according as the wire is being heated or cooled; and another at a much higher temperature, namely near a bright red, at which sudden changes, certainly not less profound, occur. This is shown in the next experiment, which at the same time illustrates the fact already mentioned, that the effect of permanent strain is opposite to that of temporary stress.

Experiment IV.—The glass tube (fig. 1) was placed inside a magnetizing solenoid[‡], consisting of a single layer of cotton-covered copper wire $\frac{1}{20}$ inch diameter, and connected through a key with a battery of two Grove's cells. Surrounding the solenoid and concentric with it is a secondary coil, consisting of 840 turns of cotton-covered copper wire partly $\frac{1}{20}$ inch diameter and partly $\frac{1}{10}$ inch diameter. This coil has a resistance of about 1 ohm, and is connected through a key with a very delicate Thomson's reflecting-galvanometer of about 7 ohms' resistance. A piece of the same soft iron wire already mentioned was subjected to thirty complete revolutions of permanent torsion, and was finally released from all torsional stress. When the wire, on heating, reached the temperature of dull red, a momentary deflection of the needle of the Thomson's galvanometer took place, indicating a sudden loss of magnetic permeability; and as soon as a bright red heat had been attained it, as in the preliminary observations, *twisted* sharply and suddenly. The battery which was employed to heat the wire had its circuit now broken, and the

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[*] This fact, which was also noticed by Mr. Gore and Prof. Barrett, is significant.

[†] The reason of this will be found in the remarks on the recalescence of iron.

[‡] Only the ends of this solenoid can be seen in the figure, the rest being enveloped by the secondary coil.

wire was allowed to cool: first ensued a sudden momentary *untwist* of the wire, whilst *a few seconds afterwards* a kick of the galvanometer-needle in the opposite direction to that which had occurred on heating, indicated that the iron had suddenly regained its magnetic properties. This experiment was repeated several times; and though the sudden jerk on heating and cooling became less and less in intensity, and probably would eventually have vanished, it did not do so after six heatings and coolings*.

Experiment V.—The last experiment had shown the great persistency of the permanent torsional strain, even after the wire had been several times heated to a white heat. A fresh series of trials was made with iron which had suffered more or less permanent torsion, for the purpose of examining this persistency more closely. In these trials it was attempted to reanneal the wire after the permanent torsion had been imparted, by passing the flame of a Bunsen-burner very slowly up and down it so as to heat all parts in turn to a white heat, the wire being in some cases entirely free from any load on it, and in others having merely the slight load due to the block and its appendages. In no case could the sudden jerk at a bright red heat be got rid of entirely; though the same wire, when tested previously to imparting permanent torsion, had shown no trace whatever of the phenomenon. It is evident that iron cannot be satisfactorily annealed by the process mentioned above†.

These trials also brought out distinctly the fact that at a dull red heat the iron begins to *permanently* untwist rapidly, so that a considerable amount of the permanent twist originally imparted can be got rid of by maintaining the temperature for some time between bright red and dull red. Directly, however, the higher critical temperature is reached there is a sudden twist, which in the case of very soft iron is partly temporary and partly permanent, and in the case of steel wire which is unannealed, or hard iron wire, is principally permanent‡.

Experiment VI.—A piece of the well-annealed iron wire, about 16 inches long, was clamped at one end and sustained in a horizontal position, save in so far as it was bent by its own

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* I had the pleasure of repeating the experiment before the Physical Society.

† I write this because some observers seem to think that iron can be satisfactorily annealed in this way. In this, I believe, they are mistaken; the iron should be maintained at a high temperature for a considerable period, and afterwards be allowed to cool very slowly.

‡ With some unannealed specimens I found it difficult to detect any trace whatever of *untwisting* temporarily when the iron *cooled* below the critical temperature.

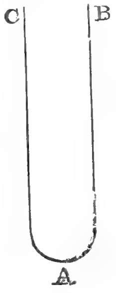
weight. The wire was heated by a burner about three inches from the clamp. When the temperature reached a bright red, the wire began to bend permanently very rapidly by its own weight; and when the burner was removed and the wire cooled, a *sudden further bending took place* at a temperature a little below bright red; and when cooled to the temperature of the room, the wire remained permanently bent. Here the attempt to unbend, which the wire no doubt made at the critical temperature when it was *heated*, was masked by the permanent bending. Another piece was therefore taken and heated at a point further away from the clamp, so that the bending-stress should not be so great. With care it was found possible to detect that the wire suddenly *straightened* itself when the critical temperature was reached on *heating.*

Similar trials were made with other specimens of iron and steel not so well annealed; and with these, when cooling, the sudden bending took place at a temperature lower than that at which it occurred with the very soft iron. With one specimen of wire, $1\frac{1}{2}$ millim. thick, it occurred at a temperature *apparently* below that of visible red*; but, as Professor Barrett justly remarks, the internal temperature of the thicker wires is no doubt masked by the cooling of the surface, whereas in thin wires the cooling throughout is extremely rapid. I found also that a piece of this specimen of iron *appeared* to lose its magnetic properties at a temperature below visible red, whereas with the thinner wires the *apparent* temperature at which this took place was somewhere about a dull red.

The very small amount of bending-*stress* which is required to bring out the phenomenon makes it a little difficult to detect with certainty the *opposite* effect of bending-*strain*; but it may be managed after a few trials in the following manner:—

Experiment VII.—A piece of the very soft iron wire was bent, as in fig. 2, with the portions A C, A B in a vertical position; the end C was secured to a clamp, and the bend A heated by a burner. If A B has been so arranged that its centre of mass is very nearly vertically above the part heated, there will be little bending-stress. In this experiment, on heating the bend A to a bright red, the end B jerked suddenly towards C, and when the burner was removed almost immediately jerked back again. If A B is arranged so that there is a little more bending-stress, it is curious to notice the struggle which sometimes ensues at a bright red heat as to

Fig. 2.



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* Prof. Barrett also remarks that the phenomenon occurs on cooling at an *apparently* lower temperature with thick wires than thin ones.

which way the end B shall be jerked[*]. This experiment was repeated with other specimens of iron and steel and with similar results, except that on cooling, as with the torsional stress and strain, the phenomenon occurred at a lower temperature than with the very soft iron wire.

The question now arises, Is there any sudden *molecular* change at a bright red heat even where there is *no stress or strain affecting the wire*? Professor Barrett brings forward evidence in support of the view that there is, which I am afraid is not conclusive. He says:—"If, however, this molecular change be entirely due to alteration in cohesion, then the removal of the spring ought to cause the anomalous behaviour to disappear. But it does not. Without the spring, an iron wire can be seen by the naked eye to undergo a momentary contraction during heating, and a momentary and more palpable elongation during cooling[†]. Fixing one end of the wire, and bending the other extremity at right angles so that it may dip into a trough of mercury, and thus preserve contact with the battery, both actions can be seen." I have little doubt myself that the effects observed here were really due to the stress produced by the *weight of the wire itself*. So far as my own experiments go, I have not been able to detect any sudden change in the wire when sufficient care is taken to prevent the weight of the wire itself from producing an effect[‡]. Nevertheless I am inclined to believe, with Professor Barrett, that a sudden *molecular* change does occur at the critical temperature even when the wire is quite free from any mechanical stress or strain, though as yet experimental evidence is wanting to furnish sufficient evidence of such a change.

The Recalescence of Iron.

This curious phenomenon was, I believe, first discovered by Professor Barrett, and mention is made of it in his paper, already alluded to, in the following words:—"On September 12th I was examining the condition of the wire in a darkened room, when a new and unexpected change revealed itself. During the cooling of the wire it was found that just as it reached a very dull red heat, a sudden accession of temperature occurred, so that it glowed once more with a bright red

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[*] That is, as to whether the effect of stress or that of strain shall predominate.

[†] The effect on cooling could always be got in my own experiments also with greater distinctness than the effect on heating.—H. T.

[‡] It is difficult to realize, without actually putting the matter to the test of experiment, what a very small amount of stress or strain suffices to bring out the phenomenon.

heat. Illuminating the index and scale of the apparatus, which was watched by an assistant, it was at once found that the *reheating of the wire occurred simultaneously with the momentary elongation.*" Professor Barrett also ascertained that, in order to bring out the phenomenon, it was necessary *previously to heat the wire to a white heat*; and, further, that *wherever the momentary expansion of the wire is feeble or absent, there likewise this recalescence is also feeble or absent.*

In my own mind I have little doubt that this beautiful phenomenon may be accounted for in the following manner:—When the iron has been heated above a bright red and is then cooling, it reaches the critical temperature at which there is a *tendency* for the sudden change to take place in the permanently strained or temporarily stressed wire: the change does not, however, *actually* take place at this temperature in consequence of the so-called coercitive force of the iron. When the iron is very soft and well annealed, the temperature at which the jerk takes place is comparatively near the temperature at which the jerk occurred on heating. But when the iron is hard-drawn or only imperfectly annealed, there may be a considerable difference in the two temperatures. Suppose, then, that the iron has cooled to the temperature at which its magnetic properties are suddenly restored. At this temperature a commotion more or less profound takes place among the molecules; and this is sufficient to give them a start towards those positions which they have all along, after the higher critical temperature was passed, been trying to assume. When once started the molecular motion continues, the energy of position is rapidly converted into the energy of motion, and this again into the energy of heat, so that the wire once more glows. It by no means follows that the phenomenon of recalescence occurs with *every* specimen of iron at the critical temperature at which iron loses its magnetic properties; indeed I have noticed sometimes *more than one* evident sudden accession of heat* during the *same* cooling. Moreover, in the case of the very soft iron wire, with which the sudden jerk occurred at a much higher temperature on cooling than with the harder specimens, there was no perceptible trace of the phenomenon at a dull red heat. Probably there was a sudden check of the rate of cooling when the jerk occurred, though reglowing at this point was not noticed. I did not, however, pay much attention to the matter.

The view just advanced respecting the phenomenon of recalescence is, I think, justified by the following considerations:—

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* Sometimes there is merely a sudden check in the rate of cooling without any sensible recalescence.

(1) The phenomenon is entirely absent in very well-annealed iron wire which has not, after annealing, suffered strain, nor is at the time under stress.

(2) It is not sensible unless the temperature of the wire has been *previously* raised to a bright red*.

(3) It becomes more and more sensible as the temperature at which the sudden jerk occurs in the wire becomes lower and lower, and is most pronounced in those wires in which the jerk does not take place until the wire suddenly loses its magnetic properties.

(4) It is not sensible unless there is a sudden jerk; the jerk and recalescence occur at the same instant.

Gore's phenomenon and the phenomenon of recalescence do not occur in pure, or nearly pure, specimens of nickel or cobalt; though, as is well known, nickel loses its magnetic properties at a much lower temperature than iron, namely from about 350° C. to 400° C. A most careful examination of three different specimens of nickel wire, procured from Messrs. Johnson and Matthey, was made†. Unfortunately pure nickel wire cannot be drawn; but two out of the three specimens showed no trace whatever of the phenomena. The third specimen showed very obvious signs of both phenomena; but when tested was found, like iron, to lose its magnetic properties at a dull red heat instead of at the lower temperature at which pure nickel loses its magnetic properties, and therefore probably contained rather a large quantity of iron. The other two specimens lost their magnetic properties at a temperature of about 400° C. Only one specimen of cobalt was examined. Cobalt has not been drawn into wire as yet, though both Messrs. Johnson and Matthey, and Mr. W. Wiggin, jun., have kindly attempted to do so for me. Mr. Wiggin was, however, so good as to have rolled for me a specimen of cobalt, which is very nearly pure, in a strip about $\frac{1}{2}$ millim. thick, 12 millim. broad, and 60 centim. long. This strip was tested with a Bunsen-burner and with a large blowpipe, and showed no trace at any temperature of either phenomenon when subjected to bending-stresses of various amounts. Unlike nickel and iron, cobalt does not lose its magnetic properties at any temperature at which it has been tested.

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* This is not the case as regards the phenomenon of iron suddenly losing its magnetic properties at a dull red heat.

† Professor Barrett had previously shown the phenomenon to be absent in nickel.

The Working of Iron at High Temperatures.

There can be little doubt that such experiments as these are important from a technical point of view. It is known, I am not aware whether generally or not, that it is dangerous to work iron within certain ranges of temperature. This one can easily understand from what has gone before; for it is manifest that if the temperature of a mass of iron is not the same throughout, and if the *mean* temperature of the mass be near the higher of the two critical temperatures, or if it is cooling from a bright red heat near either of the two critical temperatures, the effect of a blow or of any stress will be very different on different parts of the mass, and will be fatal to that uniformity of structure and strength which it is so desirable to procure.

Summary.

(1) There are two distinctly marked critical temperatures for iron; the lower somewhere about dull red, and the higher somewhere about bright red*.

(2) At the former of these critical temperatures the iron begins suddenly to lose its magnetic properties if it is being heated, and to gain them if it is being cooled.

(3) At or *near*† the latter critical temperature the iron, on being heated, if under the slightest torsional, longitudinal, or flexural stress or strain, begins to exhibit a remarkably sudden change. If the metal be under stress, the sudden change resembles that which would result from a sudden increase of elasticity. If the metal be permanently strained, there is a sudden increase in the amount of the strain.

(4) When the iron is cooling, a sudden change, but opposite in direction to that which occurs on heating, takes place. The temperature at which the sudden change takes place is, for very well annealed iron, nearly the same as that at which the change takes place on heating. If, however, the wire be not annealed, the change may be delayed by the so-called coercitive force of the metal until the lower critical temperature is reached. When the change is so delayed, the phenomenon of recalescence occurs.

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* I hope at some future time to be able to fix these critical temperatures more exactly.

† It is not unlikely that the so-called coercitive force may cause the change to take place at a slightly higher temperature than the critical temperature.

XXXIII. *The Choice of Means. By* F. Y. Edgeworth, *M.A., Lecturer on Logic at King's College, London*[*].

WHAT is the best Mean?, is a question which I have elsewhere[†] attempted to answer generally. A supplement to that answer, with special reference to the case of *Discordant Observations*, is intended here.

One large class of Discordant Observations belongs to the category of errors whose law of facility is a compound of different Probability-Curves[‡]. According to what is known or surmised about the components, different methods may be appropriate. It is here submitted that very generally, and in the absence of special knowledge about the genesis of the observations, the compound source of error may be treated as belonging to the category of curves other than Probability-Curves[§]. This category also comprises the species of Discordant[||] Observations other than that above defined.

What, then, are the methods proper to this category? They are two—the *Method of Least Squares* and the *Method of Situation*[¶]. The Method of Least Squares is a good method[**], for the same reason that it may be a good plan for an Insurance Company to deal with a tailor or farmer according to the general statistics for adult healthy males of the same age and country; if it is either impossible to obtain, or not worth while to utilise, the special vital statistics for the different occupations. What corresponds in our case to the abstracted attribute of occupation, is the generally unknown form of the facility-function and the ignored mutual distances between the given observations. What corresponds to the attributes retained by the Insurance Company is the Arithmetic Mean

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[*] Communicated by the Author.

[†] "Observations and Statistics" (Cambridge Philosophical Transactions, 1885), corrected by the Appendix to *Metretike* (London Temple Publishing Company, 1887).

[‡] In the notation of the paper referred to, $a\,c\,\bar{d}\,e\,\bar{f}\,\bar{g}$. See examples in the paper on Discordant Observations in this Journal, April 1887.

[§] More exactly in the notation proposed by the writer, $a\,c\,\bar{d}\,\bar{e}\,g\,h$; that is, ($a$) relating to an objective (not fictitious) Mean, (c) not extending to infinity, ($\bar{d}$) treated by way of Inverse Probability, ($\bar{e}$) curves other than "Probability," (gh) unique and symmetrical. For instance, the law to which Prof. Newcomb reduces his transit-observations (cited below) constitutes such a curve, provided that its infinite branches are cut off.

[||] *E. g.* caused by "Mistakes" which do not obey the typical Law of Error.

[¶] Laplace, *Théorie Analytique*, Supplement 2, sect. 2. *Mécanique Céleste*, book iii. See 'Observations and Statistics.'

[**] For a fuller statement of this reasoning see the writer's *Metretike*.

of each cluster, together with its given *weight* (or inverse mean-square-of-error). We know by the *Law of Error* that if ξ were the real point, then the probability that the Arithmetic Mean of any cluster of errors ranging under any (one and the same) facility-curve would occur at the point x (between x and $x+\Delta x$) is $\Delta x \times \frac{1}{\sqrt{\pi} c} e^{\frac{-(\xi-x)^2}{c^2}}$, where $c^2=$ twice the mean-square-of-error of the facility-curve under which each member of the cluster ranges. Hence, *looking only at the Arithmetic Mean and weight* of the given cluster, x_1, x_2, &c., we may say that the *à posteriori* probability of its having resulted from the real point ξ is proportioned to $\frac{1}{\sqrt{\pi} c} e^{\frac{-(\xi-x)^2}{c^2}}$; where x is the Arithmetic Mean, and c the weight of the cluster. Thus the Probability-Curve, with its peculiar facilities for calculation, again makes its appearance. We can apply the rules primarily appropriate to observations obeying the typical *Law of Error*, to find both the *most probable* and the *most advantageous* Mean. And thus we find that the Arithmetic Mean is the best solution for a single cluster (and the Weighted Arithmetic Mean for several clusters). The *Method of Least Squares* is seen to be our best course when we have thrown overboard a certain portion of our data—a sort of sacrifice which has often to be made by those who sail upon the stormy seas of Probability.

But this explanation must be received with some qualifications. The analogy of insurance shows that when we do not utilise all our information, it may be a delicate question what part of it it is best to utilise. Take the case put by Dr. Venn*, of an insurance company which is dealing with a consumptive Englishman in Madeira. The ideally best plan would be to use the statistics for consumptive Englishmen resident in Madeira. But failing that, it may be a nice question whether it is better to refer the customer to the category of consumptive persons, or of Englishmen, resident in Madeira. Similarly in our case it might be better, instead of directing our exclusive attention to the Arithmetic Mean and the correlated characteristic of the inverse mean-square-of-error, to prescind in the same sense some other Mean, and in particular the Median, with its correlate the Greatest Ordinate†. The Median of any cluster fluctuates according to a Probability-

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* 'Logic of Chance,' ch. viii.

† Laplace, *Théorie Analytique*, Supplement 2, sect. 2. See an illustration in my paper on Problems in Probabilities, Phil. Mag. Oct. 1886.

Curve whose modulus is $\frac{\sqrt{s}}{\sqrt{2}\,\mathrm{P}}$, P being the Greatest Ordinate of the common facility-curve. The criterion whether the Median or Arithmetic Mean is the better reduction*, is presumably the character of the correlated Probability-Curve†. The reduction to which corresponds the smaller Modulus is presumably the better; since thus we obtain a smaller "probable" error, and, what is often more important, a smaller *improbable*, or, as Mr. Merriman proposes to call it, "huge" error.

Which of the reductions will have the smaller Modulus will depend upon the character of our facility-curve. For Probability-Curves, and presumably functions in their neighbourhood, it is shown by Laplace‡ that the Arithmetic Mean has the advantage. But for curves whose head reaches high, while their extremities stretch out far, the Median has the advantage§.

Now the grouping of *Discordant Observations* is apt to assume this form. Accordingly the Median is proposed as the Mean proper to this class of observations. If we have been deceived by the appearance of Discordance‖ (as in Gen. Colby's case, cited by Airy), and the facility-curve was really a normal Probability-Curve, yet we shall have lost little by taking the Median instead of the Arithmetic Mean. For the error¶ of the former is of the same order as (only 1·3 greater than) the error of the latter. And, if the observations are really discordant, the derangement due to the larger deviations will not be serious, as it is for the Arithmetic Mean**.

In illustration of the former proposition, let us take forty observations made by Bessel (on Saturn's ring), which Prof. Chauvenet has cited†† as presumably of the normal type. The Median is found by arranging the observations in the order of magnitude and taking a mean between the twentieth and twenty-first‡‡. They are both +·01, the Arithmetic Mean

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* Dr. Venn's criterion is much the same.

† Laplace, *loc. cit.* *Cp.* Camb. Phil. Trans. 1885, pp. 168–9.

‡ *Loc. cit.*

§ Camb. Phil. Trans. p. 168.

‖ See the paper on Discordant Observations in the April number of this Journal.

¶ Laplace, *loc. cit.*

** This advantage of the Median over the Arithmetic Mean has been noticed by Fechner, Mr. Galton, and others.

†† 'Astronomy,' vol. i. p. 495.

‡‡ There is a little indeterminateness here (the number of observations being even) of not much practical importance when the number is large.

being taken as zero. Now the probable error incident to the Arithmetic Mean is found by Prof. Chauvenet to be ·022. There is then no reason for much preferring 0 to 0·01.

As a second example of the safety of the method, let us take the observations cited by Sir G. Airy at the end of (the later editions of) his 'Theory of Errors.' The original observations, which have been submitted to me by the kindness of Mr. Turner, of the Royal Observatory, Greenwich, are in number 636. Arranging them in the order of magnitude and counting from the lowest, we shall find that the 318th observation is $-0·03$ (where zero is the Arithmetic Mean). The 319th observation has the same value. And accordingly that is the value of the Median. Now, according to Sir G. Airy's results, the probable error of the Arithmetic Mean is about $\frac{·57}{\sqrt{642}}$; that is, about ·022. The value $-·03$, therefore, is not a bad one upon the supposition that the given observations are perfectly normal, that they range under a single probability-curve.

Upon the supposition, which there is reason for entertaining, that the given observations are somewhat "discordant," the value $-·03$ is just as eligible as zero. It may be remarked that, by Laplace's formula, the probable-error for the Median is about ·03. So that upon neither supposition is there much to choose between the two results.

As an example of the general case, where the method is not only safe but useful, let us take the 684 observations (of transits of Mercury)* discussed by Prof. Newcomb in the 'American Journal of Mathematics, 1885. The Arithmetic Mean being taken as zero, I find for the Median $-0·45$. The error of this result may be found by the formula above given, if we put for the greatest ordinate 30; that being the number of observations per unit of abscissa in the neighbourhood of the centre. I find for the Modulus 0·6, and for the Mean error (in the sense of square root of half-modulus) about ·55. On the other hand, Prof. Newcomb's method (if I have rightly worked the laborious arithmetic which it involves) gives for the correction of the Arithmetic Mean $-·6$, subject to a Mean Error of about ·5. In view of so slight a difference between the results and so great a liability to error in the calculation, is there much reason for preferring the laborious to the rough and ready method? In reducing observations it is possible to go further and not fare any better.

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* They are given in full in the 'Astronomical Papers of the American Ephemeris,' vol. i.

XXXIV. *Stability of Motion (continued from the May, June, and August Numbers).—Broad River flowing down an Inclined Plane Bed.* *By* Sir WILLIAM THOMSON, *F.R.S.**

41. CONSIDER now the second of the two cases referred to in § 27—that is to say, the case of water on an inclined plane bottom, under a fixed parallel plane cover (ice, for example), both planes infinite in all directions and gravity everywhere uniform. We shall include, as a sub-case, the icy cover moving with the water in contact with it, which is particularly interesting, because, as it annuls tangential force at the upper surface, it is, for the steady motion, the same case as that of a broad open river flowing uniformly over a perfectly smooth inclined plane bed. It is not the same, except when the motion is steadily laminar, the difference being that the surface is kept rigorously plane, but not free from tangential force, by a rigid cover, while the open surface is kept almost but not quite rigorously plane by gravity, and rigorously free from tangential force. But, provided the bottom is smooth, the smallness of the dimples and little round hollows which we see on the surface, produced by turbulence (when the motion is turbulent), seems to prove that the motion must be very nearly the same as it would be if the upper surface were kept rigorously plane, and free from tangential force.

42. The sub-case described in § 31 having been disposed of in §§ 32–40, we now take the including case, described in the first half-sentence of § 31; for which we have, as steady solution, according to (5),

$$\mathrm{U}=\beta y-\tfrac{1}{2}cy^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (57),$$

if we reckon y from the bottom upwards. Thus (7), (8), (9), (11), (12) become

$$\frac{du}{dt}+(\beta y-\tfrac{1}{2}cy^2)\frac{du}{dx}+(\beta-cy)v=\mu\nabla^2 u-\frac{dp}{dx} \quad . \qquad (58),$$

$$\frac{dv}{dt}+(\beta y-\tfrac{1}{2}cy^2)\frac{dv}{dx} \qquad =\mu\nabla^2 v-\frac{dp}{dy} \quad . \qquad (59),$$

$$\frac{dw}{dt}+(\beta y-\tfrac{1}{2}cy^2)\frac{dw}{dx} \qquad =\mu\nabla^2 w-\frac{dp}{dz} \quad . \qquad (60),$$

$$2(\beta-cy)\frac{dv}{dx}= \qquad -\nabla^2 p \quad . \qquad (61),$$

$$\frac{d\nabla^2 v}{dt}+c\frac{dv}{dx}+(\beta y-\tfrac{1}{2}cy^2)\frac{d\nabla^2 v}{dx}=\mu\nabla^4 v \quad . \quad . \quad . \qquad (62).$$

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* Communicated by the Author.

43. We have not now any such simple partial solution as that of §§ 34, 35, 36 for the sub-case there dealt with; and we proceed at once to the virtually inclusive* investigation specified in § 37, and, as in § 38, assume

$$v=\epsilon^{\iota(\omega t+mx+qz)}\,\mathcal{V} \quad . \quad . \quad . \quad . \quad . \quad . \quad (63).$$

This gives

$$\frac{d}{dt}=\iota\omega,\quad \frac{d}{dx}=\iota m,\quad \text{and}\quad \nabla^2=\frac{d^2}{dy^2}-m^2-q^2 \quad . \quad (64):$$

and (62) becomes therefore

$$\mu\frac{d^4\mathcal{V}}{dy^4}-\{2\mu(m^2+q^2)+\iota[\omega+m(\beta y-\tfrac{1}{2}cy^2)]\}\frac{d^2\mathcal{V}}{dy^2}$$
$$+\{\mu(m^2+q^2)^2+\iota[\omega+m(\beta y-\tfrac{1}{2}cy^2)](m^2+q^2)-\iota cm\}\mathcal{V}=0 \quad . \quad (65),$$

or, for brevity,

$$\mu\frac{d^4\mathcal{V}}{dy^4}+(e+fy+gy^2)\frac{d^2\mathcal{V}}{dy^2}+(h+ky+ly^2)\mathcal{V}=0 \quad . \quad (66).$$

To integrate this, assume

$$\mathcal{V}=c_0+c_1y+c_2y^2+c_3y^3+c_4y^4+\&\text{c.} \quad . \quad . \quad . \quad (67);$$

and, by equating to zero the coefficient of y^i in (66), we find

$$(i+4)(i+3)(i+2)(i+1)\mu c_{i+4}+(i+2)(i+1)ec_{i+2}$$
$$+(i+1)ifc_{i+1}+[i(i-1)q+h]c_i+kc_{i-1}+lc_{i-2}=0 \quad . \quad (68).$$

Making now successively $i=0$, $i=1$, $i=2$, . . ., and remembering that c with any negative suffix is zero, we find

$$\left.\begin{array}{l}
4.3.2.1.\mu c_4+2.1.ec_2+hc_0=0,\\
5.4.3.2.\mu c_5+3.2.ec_3+2.1.fc_2+hc_1+kc_0=0,\\
6.5.4.3.\mu c_6+4.3.ec_4+3.2.fc_3+(2.1.g+h)c_2+kc_1+lc_0=0,\\
7.6.5.4.\mu c_7+5.4.ec_5+4.3.fc_4+(3.2.g+h)c_3+kc_2+lc_1=0,\\
\qquad\&\text{c.}\qquad\qquad\qquad\&\text{c.}\qquad\qquad\qquad\&\text{c.}
\end{array}\right\} (69)$$

These equations, taken in order, give successively c_4, c_5, c_6, . . ., each explicitly as a linear function of c_0, c_1, c_2, c_3; and by

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* The Fourier-Sturm-Liouville analysis (Fourier, *Théorie de la Chaleur*; Sturm and Liouville, Liouville's Journal for the year 1836, and Lord Rayleigh's 'Theory of Sound,' § 142, vol. ii. shows how to express an arbitrary function of x, y, z by summation of the type solutions of §§ 37, 39 above and § 43 (63), (67), (70) here, and so to complete, whether for our present case or former sub-case, the fulfilment of the conditions (26), (27), without using the method of §§ 34, 35, 36.

using in (67) the expressions so obtained, we find

$$\mathfrak{V}=c_0\mathfrak{F}_0(y)+c_1\mathfrak{F}_1(y)+c_2\mathfrak{F}_2(y)+c_3\mathfrak{F}_3(y) \quad . \quad . \quad (70),$$

where c_0, c_1, c_2, c_3 are four arbitrary constants, and $\mathfrak{F}_0$, $\mathfrak{F}_1$, $\mathfrak{F}_2$, $\mathfrak{F}_3$ four functions, each wholly determinate, expressed in a series of ascending powers of y which by (68) we see to be *convergent for all values of* y, unless μ be zero. The essential convergency of these series proves (as in § 39 for the case of no gravity) that *the steady motion* ($u=0$, $v=0$, $w=0$) *is stable, however small be* μ, *provided it is not zero.*

44. The less is μ, the less the convergence. When μ is very small there is divergence for many terms, but ultimate convergence.

45. In the case of $\mu=0$, the differential equation (66), or (67), becomes reduced from the 4th to the 2nd order, and may be written as follows:—

$$\frac{d^2\mathfrak{V}}{dy^2}=\left\{m^2+q^2-\frac{cm}{\omega+m(\beta y-\frac{1}{2}cy^2)}\right\}\mathfrak{V} \quad . \quad . \quad (71).$$

This, for the case of two-dimensional motion ($q=0$), agrees with Lord Rayleigh's result, expressed in the last equation of his paper on "The Stability or Instability of certain Fluid Motions" (Proc. Lond. Math. Soc. Feb. 12, 1880). The integral, but now with only two arbitrary constants (c_0, c_1), is still given in ascending powers of y by (67) and (68), which, with $\mu=0$, and the thus-simplified values of e, f, g put in place of these letters, becomes

$$-\iota[(i+2)(i+1)\omega c_{i+2}+(i+1)im\beta c_{i+1}]$$
$$+\left[\frac{\iota}{2}i(i-1)mc+h\right]c_i+kc_{i-1}+lc_{i-2}=0 \quad . \quad (72).$$

For very great values of i this gives

$$\omega c_{i+2}+m\beta c_{i+1}-\tfrac{1}{2}mcc_i=0 \quad . \quad . \quad . \quad . \quad (73),$$

which shows that ultimately, except in the case of one particular value of the ratio c_1/c_0,

$$c_{i+1}/c_i=\zeta^{-1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (74),$$

where ζ denotes the smaller root of the equation

$$\omega+m\beta y-\tfrac{1}{2}mcy^2=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (75).$$

Hence there is certainly not convergence for values of y exceeding the smaller root of (75), and thus the proof of stability is lost.

46. But the differential equation, simplified in (71) for the case of no viscosity, may no doubt be treated more appro-

priately in respect to the question of stability or instability, by writing it as follows [ζ', ζ denoting the two roots of (75)],

$$\frac{d^2\mathcal{V}}{dy^2}=\left\{m^2+q^2+\frac{2}{\zeta'-\zeta}\left(\frac{1}{\zeta-y}-\frac{1}{\zeta'-y}\right)\right\}\mathcal{V} \quad . \quad (76),$$

and integrating with special consideration of the infinities at $y=\zeta$ and $y=\zeta'$. One way of doing this, which I merely suggest at present, and do not follow out for want of time, is to assume

$$\mathcal{V}=\mathrm{C}\{\zeta-y+c_2(\zeta-y)^2+c_3(\zeta-y)^3+\&c.\},$$
$$+\mathrm{C}'\{\zeta'-y+c_2'(\zeta'-y)^2+c_3'(\zeta'-y)^3+\&c.\} \quad . \quad . \quad (77),$$

where C and C′ are two arbitrary constants, and $c_2, c_3, \ldots,$ $c_2', c_3', \ldots$ coefficients to be determined so as to satisfy the differential equation. This is very easily done; and when done shows that each series converges for all values of y less, in absolute magnitude, than $\zeta'-\zeta$, and diverges for values of y exceeding $\zeta'-\zeta$. The working out of this in detail would be very interesting, and would constitute the full mathematical treatment of the problem of finding sinuous stream-lines (curves of sines) throughout the space between two "cat's-eye" borders (corresponding to $y=\zeta$ and $y=\zeta'$) which I proposed in a short communication to Section A of the British Association at Swansea, in 1880*, "On a Disturbing Infinity in Lord Rayleigh's solution for Waves in a plane Vortex stratum." It is to be remarked that this disturbing infinity vitiates the seeming proof of stability contained in Lord Rayleigh's equations (56), (57), (58).

47. Realizing (63), and interpreting the result in connexion with (57), we see that

(*a*) The solution which we have found consists of a wave-disturbance travelling in any (x, z) direction, of which the propagational velocity in the x-direction is $-\omega/m$.

(*b*) The roots (ζ, ζ') of (75) are values of y at places where the velocity of the undisturbed laminar flow is equal to the x-velocity of the wave-disturbance.

Hence, supposing the bounding-planes to be plastic, and force to be applied to either or both of them so as to produce an infinitesimal undulatory corrugation, according to the formula $\cos(\omega t+mx+qz)$, this surface-action will cause throughout the interior a corresponding infinitesimal wave-motion if *ω/m is not equal to the value of* U *for any plane of*

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[*] Of which an abstract is published in 'Nature' for November 11, 1880, and in the British Association volume Report for the year. In this abstract cancel the statement "is stable," with reference to a certain steady motion described in it.

the fluid between its boundaries. But the infinity corresponding to $y=\zeta$ or $y=\zeta'$ will vitiate this solution *if* ω/m *is equal to the value of* U *for some one plane of the fluid or for two planes of the fluid*; and the true solution will involve the "cat's-eye pattern" of stream-lines, and the enclosed elliptic whirls*, at this plane or these planes.

48. Now let the fluid be given moving with the steady laminar flow between two parallel boundary planes, expressed by (57), which would be a condition of kinetic equilibrium (proved stable in § 43) under the influence of gravity and viscosity; and let both gravity and viscosity be suddenly annulled. The fluid is still in kinetic equilibrium; but is the equilibrium stable? To answer this question, let one or both bounding-surfaces be infinitesimally dimpled in any place and made plane again. The Fourier synthesis of this surface-operation is

$$\int_0^\infty\int_0^\infty\int_0^\infty d\omega\, dm\, dq\, f(\omega)\,\mathrm{F}(m)\,\mathfrak{F}(q)\cos\omega t\cos mx\cos qz \quad (78),$$

or

$$\tfrac{1}{2}\int_0^\infty\int_0^\infty\int_0^\infty d\omega\, dm\, dq\, f(\omega)\,\mathrm{F}(m)\,\mathfrak{F}(q)\{\cos(\omega t-mx) - \cos(\omega t+mx)\}\cos qz\;. \quad (79),$$

which implies harmonic surface-undulations travelling in opposite x-directions, with all values from 0 to ∞ of (ω/m), the $\pm x$ of wave-velocity. Hence (§ 47) the interior disturbance essentially involves elliptic whirls. Thus we see that the given steady laminar motion is *thoroughly* unstable, being ready to break up into eddies in every place, on the occasion of the slightest shock or bump on either plastic plane boundary. The slightest degree of viscosity, as we have seen, makes the laminar motion stable; but the smaller the viscosity with a given value of $g\sin\mathrm{I}$, or the greater the value of $g\sin\mathrm{I}$ with the same viscosity, the narrower are the limits of this stability. Thus we have been led by purely mathematical investigation to a state of motion agreeing perfectly with the following remarkable descriptions of observed results by Osborne Reynolds (Phil. Trans. March 15, 1883, pp. 955, 956):—

"The fact that the steady motion breaks down suddenly, shows that the fluid is in a state of instability for disturbances of the magnitude which cause it to break down. But the fact that in some conditions it will break down for a large disturbance, while it is stable for a smaller disturbance, shows

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* See my former paper on the "Disturbing Infinity" already referred to.

that there is a certain residual stability, so long as the disturbances do not exceed a given amount."...

"And it was a matter of surprise to me to see the sudden force with which the eddies sprang into existence, showing a highly unstable condition to have existed at the time the steady motion broke down."

"This at once suggested the idea that the condition might be one of instability for disturbance of a certain magnitude, and stable for smaller disturbances."

49. The motion investigated experimentally by Reynolds, and referred to in the preceding statements, was that of water in a long straight uniform tube of circular section. It is to be hoped that candidates for the Adams Prize of 1888 may investigate this case mathematically, and give a complete solution for infinitesimal deviations from rectilineal motion. It is probable that for it, and generally for a uniform straight tube of any cross section, including the extreme, and extremely simplified, case of rectilinear motion of a viscous fluid between two parallel *fixed* planes, which I have worked out above, the same general conclusion as that stated at the end of § 26 and in §§ 43–48 will be found true.

50. In the case of no gravity ($g \sin I = 0$), and the viscous fluid kept in "shearing" or "laminar" motion by relative motion of the two parallel planes, there is, when viscosity is annulled, no disturbing instability in the steady uniform shearing motion, with its uniform molecular rotation throughout, which viscosity would produce; and therefore our reason for suspecting any limitation of the excursions within which there is stability, and for expecting possible *permanence* of any kind of turbulent or tumultuous motion between two *perfectly* smooth planes (or between two polished planes with any practical velocities) does not exist in this case. But a great variety of general observation (and particularly Rankine and Froude's doctrine of the "skin-resistance" of ships, and Froude's experimental determination of the resistance experienced by a very smooth, thin, vertical board, 19 inches broad and 50 feet long, moved at different uniform speeds* through water in a broad deep tank 278 feet

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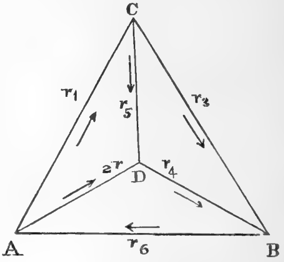
* 'Report to the Lords Commissioners of the Admiralty on Experiments for the Determination of the Frictional Resistance of Water on a Surface under various conditions, performed at Chelston Cross (Torquay), under the Authority of their Lordships.' By W. Froude. (London: Taylor and Francis. 1874.)

Froude found that, at a constant velocity of 600 feet per minute, the resistance of the water against one of his smoothest surfaces, at positions two feet abaft of the cutwater and 50 feet abaft of the cutwater, respectively, was ·295 of a pound per square foot, and ·244 of a pound per square long) makes it certain that if water be given at rest between two infinite planes both at rest, and if one of the planes be suddenly, *or not too gradually*, set in motion, and kept moving uniformly, the motion of the water will be at first turbulent, and the ultimate condition of uniform shearing will be approached by gradual reduction and ultimate annulment of the turbulence. I hope to make a communication on this subject to Section A of the British Association in Manchester, and to have it published in the October number of the Philosophical Magazine. Corresponding questions must be examined with reference to the corresponding tubular problem, of an infinitely long, straight, solid bar kept moving in water within an infinitely long fixed tube. It is to be hoped that the 1888 Adams Prize will bring out important investigations on this subject.

[To be continued.]

XXXV. *Note on an Elementary Proof of certain Theorems regarding the Steady Flow of Electricity in a Network of Conductors.* By ANDREW GRAY, *M.A., F.R.S.E., Professor of Physics in the University College of North Wales*[*].

THE following elementary proof of the principal theorems of a network of conductors may be of interest. It will be necessary to consider first the well known and, for our purpose, typical case of a network of five conductors, shown in the figure. We assume the so-called laws of Kirchhoff, namely the principle of continuity applied to the steady flow of electricity in a linear system; and the theorem (at once deducible from Ohm's law) that in any closed circuit of conductors forming part of a linear system, the sum of the products obtained by multiplying the current in each part taken in order round the circuit by its resistance, is equal to the sum of the electromotive forces in the circuit.



Let the wire joining A B contain

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foot. Remark that this astonishingly great force of a quarter of a pound per square foot (!!) is the resistance due to uniform laminar flow of water between two parallel planes $\frac{1}{30}$ of a centimetre ($\frac{1}{900}$ of a foot!) asunder, when one of the planes is moving relatively to the other at 10 feet (300 centimetres) per second, if the water be at the temperature 0° Cent., for which the viscosity, calculated from Poiseuille's observations on the flow of water in capillary tubes, is $1{\cdot}34\times10^{-5}$ of a gramme weight per square centimetre.

[*] Communicated by the Author.

a battery of electromotive force E, the only electromotive force in the system, and let the resistances and directions of the currents in the various parts of the system be as indicated in the diagram. Let r_6 be the resistance of the battery and the wires connecting it with A and B, and let γ_1, γ_2, &c. be the strengths of the currents flowing in the resistances r_1, r_1, &c., respectively, in the directions indicated by the arrows. By the principle of continuity we get

$$\left.\begin{aligned}\gamma_3&=\gamma_1-\gamma_5,\\ \gamma_4&=\gamma_2+\gamma_5,\\ \gamma_6&=\gamma_1+\gamma_2.\end{aligned}\right\}\quad\ldots\ldots\quad(1)$$

Applying the second principle to the circuits BACB, ACDA, CBDC, we obtain the three equations

$$\left.\begin{array}{lll}\gamma_1(r_1+r_3+r_6)+\gamma_2 r_6-\gamma_5 r_3 & & =\mathrm{E},\\ \gamma_1 r_1 & -\gamma_2 r_2+\gamma_5 r_5 & =0,\\ \gamma_1 r_3 & -\gamma_2 r_4-\gamma_5(r_3+r_4+r_5) & =0,\end{array}\right\}\quad\ldots\quad(2)$$

Eliminating γ_1 and γ_2, we find

$$\gamma_5=\frac{\mathrm{E}(r_2r_3-r_1r_4)}{\mathrm{D}};\quad\ldots\ldots\quad(3)$$

where

$$\begin{aligned}\mathrm{D}=r_5r_6(r_1+r_2+r_3+r_4)+r_5(r_1+r_3)(r_2+r_4)\\ +r_6(r_1+r_2)(r_3+r_4)+r_1r_3(r_2+r_4)+r_2r_4(r_1+r_3).\quad\ldots\quad(4)\end{aligned}$$

By substituting for γ_2 in the second and third of equations (2) its value $\gamma_6-\gamma_1$, we get

$$\left.\begin{aligned}\gamma_1(r_1+r_2)+\gamma_5 r_5\qquad\qquad &-\gamma_6 r_2=0,\\ \gamma_1(r_3+r_4)-\gamma_5(r_3+r_4+r_5)&-\gamma_6 r_4=0.\end{aligned}\right\}\quad\ldots\quad(5)$$

From these we obtain, by eliminating γ_1,

$$\gamma_5=\frac{\gamma_6(r_2r_3-r_1r_4)}{r_5(r_1+r_2+r_3+r_4)+(r_1+r_2)(r_3+r_4)}.\quad\ldots\quad(6)$$

By means of equations (3) and (6) we can very easily solve the problem of finding the equivalent resistance of the system of five resistances, r_1, r_2, &c., between A and B. For let R be this equivalent resistance; since γ_6 is the current flowing through the battery, we have $\gamma_6=\mathrm{E}/(r_6+\mathrm{R})$. Substituting this value of γ_6 in (6), equating the values of γ_5 given by (3) and (6), and solving for R, we get

$$\mathrm{R}=\frac{r_5(r_1+r_3)(r_2+r_4)+r_1r_3(r_2+r_4)+r_2r_4(r_1+r_3)}{r_5(r_1+r_2+r_3+r_4)+(r_1+r_2)(r_3+r_4)}.\quad\ldots\quad(7)$$

The following theorem, which can be verified by experiment, will be of use in what immediately follows.

Any two points in a linear system which are at different potentials may be joined by a wire without altering in any way the state of the system, provided the wire contains an electromotive force equal and opposite to the difference of potential between the two points. For the wire before being joined will, in consequence of the electromotive force, have the same difference of potentials between its extremities as between the two points; and if the end of the wire which is at the lower potential be joined to the point of lower potential, it will have the potential of that point and no change will take place in the system. The other end will then be at the potential of the other point, and may be supposed coincident with that point, without change in the state of the system. The new system thus obtained plainly satisfies the principle of continuity and the other theorem assumed above, and is therefore possible; and it can be proved that it is the only possible arrangement under the condition that the state of the original system shall remain unaltered.

As a particular case, any two points in a linear circuit which are at the same potential may be connected either directly or by a wire of any resistance, without altering the state of the system.

Further, it follows that *if an electromotive force in one conductor of a linear system produces no current in another conductor of the system, either of the conductors may be removed without altering the current in the other.* For let one conductor be removed: the potentials at the points of the system at which it was attached will in general then be altered. And since any two points in a linear system between which there is a difference of potentials may, without altering the state of the system in any way, be joined by a wire which contains an electromotive force equal and opposite to the difference of potentials, we may suppose the conductor replaced with an electromotive force in it equal to the difference of potential now existing between the two points, and its presence or removal will not affect the current in any part of the system. But the same result may be attained, of course, without removing the conductor, by simply placing within it the required electromotive force; and this by hypothesis does not affect the current in the other conductor. Hence the removal of one conductor does not affect the current in the other.

If A, B, C, D be four points of meeting in a network of linear conductors, in one wire of which joining AB there is an electromotive force, while CD is connected by one or more separate wires, the network can be reduced to a system of six conductors arranged as in the figure, and such that the wires AB, CD, the currents in them, and the potentials at their extremities remain unchanged. For currents will enter any one mesh of the network at certain points and leave it at certain other points. One of the former points must be the point of maximum potential in the mesh, one of the latter the point of minimum potential. The circuit of the mesh, therefore, consists of two parts joining these two points, and to any point in one of the parts will correspond a point of the same potential in the other part. We may therefore suppose every point in one in coincidence with points of the same potential in the other; that is, the mesh replaced by a single wire joining the two points, and such that the currents entering or leaving it by wires joining it to the rest of the system and the potentials at the points of junction, are not altered.

Since the only electromotive force is in the wire AB, the current must enter the network at one of its extremities, A, say, and leave at the other extremity; A and B are therefore the points of maximum and minimum potential of the network. Hence we can replace the meshes of the system one by one by single wires, keeping CD unaltered until we have reduced the network to two meshes, one on each side of CD, connected, if necessary, by single wires to A and B respectively. Each mesh and connecting-wire can be replaced by two wires joining A and B respectively with C and D, and thus the whole system is reduced to an equivalent system of the form shown in the figure. We can now deduce from this simple system relations for the currents and potentials in the conductors AB, CD, which will hold for these conductors in the more complex system.

Let the electromotive force hitherto supposed acting in AB be transferred to CD, while the resistances r_5, r_6 are maintained unaltered. The value of γ_6 will be obtained from (6) by retaining the numerator unaltered and interchanging r_5 and r_6, r_1+r_2 and r_1+r_3, r_3+r_4 and r_2+r_4 in D. But these interchanges will not effect any alteration in the value of D; and hence the new value of γ_6 is equal to the former value of γ_5. Hence the theorem:—*An electromotive force which, placed in any conductor* C_l *of a linear system, causes a current to flow in any other* C_p *would, if placed in* C_p, *cause an equal current to flow in* C_l.

If the arrangement is such that when the electromotive force is in C_l the current in C_m is zero, the current in C_l will be zero when the electromotive force is in C_m; and no electromotive force in one will produce a current in the other. The two conductors are in this case said to be *conjugate*.

We can easily obtain another important theorem. The five conductors AC, AD, BC, BD, CD in the figure may be regarded as the reduced equivalent of a network of conductors, at one point of which, A, a current of amount γ_6

enters, and at another point of which, B, the same current leaves. Multiplying the expression for γ_5 by r_5, we get for the difference of potentials between C and D the value

$$\gamma_5 r_5 = \frac{\gamma_6 r_5(r_2 r_3 - r_1 r_4)}{r_5(r_1+r_2+r_3+r_4)+(r_1+r_2)(r_3+r_4)}. \quad . \quad . \qquad (8)$$

But the resistance of the system of five conductors, between the points C, D, is the reciprocal of the sum $1/(r_1+r_2)+1/(r_3+r_4)+1/r_5$ of the conductivities of the three arcs which join C, D, that is,

$$\frac{r_5(r_1+r_2)(r_3+r_4)}{r_5(r_1+r_2+r_3+r_4)+(r_1+r_2)(r_3+r_4)},$$

and if a current of amount γ_6 enter at C and leave at D, the difference of potentials between C and D will be equal to this expression multiplied by γ_6. The product multiplied by $r_1/(r_1+r_2)$ is the difference of potentials between C and A, and multiplied by $r_3/(r_3+r_4)$ is the difference of potentials between C and B. Hence, the difference of potentials between A and B is the difference of these products, or

$$\frac{\gamma_6 r_5(r_1 r_4 - r_2 r_3)}{r_5(r_1+r_2+r_3+r_4)+(r_1+r_2)(r_3+r_4)},$$

the same value as that given in (8) for the difference of potentials between C and D. Hence the theorem:

If to a current entering at one point A *of a linear system, and leaving at another point* B, *there correspond a certain difference of potentials between two other points* C *and* D, *then to an equal current entering the system at* C *and leaving* at D *there will correspond the same difference of potentials between* A *and* B*.

XXXVI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 222.]

May 11, 1887.—Prof. J. W. Judd, F.R.S., President, in the Chair.

THE following communications were read:—

1. "Further Observations on *Hyperodapedon Gordoni.*" By Prof. T. Huxley, LL.D., F.R.S., F.G.S.

2. "On the Rocks of the Essex Drift." By Rev. A. W. Rowe, M.A., F.G.S.

The rocks of the drift in Essex are of such great variety that it is

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* The theorems just proved have been obtained in different ways by Kirchhoff (*Pogg. Ann.* Bd. lxxii. 1847, and *Ges. Abhand.* p. 22), and Maxwell (El. and Mag. vol. i., second edition, p. 371) from a consideration of the general theory of a linear system.

difficult both to get a really representative collection and to classify them when they have been collected. About two hundred specimens have been taken, and sections have been made of one hundred and fifty of these. There is a remarkable absence of granite of any kind, and only two specimens of syenite have been found. Quartz-porphyrites and quartz-tourmaline rocks are fairly abundant, felsites are rarely met with, but felspar porphyrites are very abundant; trachytes also are found, but there is some reason for suspecting that these do not really belong to the drift, but have been imported in very early times. The most abundant of the igneous rocks are the dolerites; but all the coarser dolerites and those of a true ophitic character are wanting. Many of the specimens are of subophitic texture, and bear a general likeness to the subophitic dolerites of Central England, though without having any special points of resemblance; some of the specimens, however, are strikingly like the rocks of the Whin-Sill, and that too in certain special points. The dolerites of trachytic texture, or basalts, do not at all resemble those of the North of England, but some of them are almost identical with certain Scandinavian basalts. One or two specimens deserve special mention, and among them a hypersthene-bearing dolerite that is more nearly ophitic than any of the others. Two specimens of granulite containing hypersthene are interesting as belonging to a well-characterized type. The crystalline schists are not abundant; among them is a hornblende schist containing abundance of tourmaline. The sandstones, some of which are of very large size, belong chiefly to the Carboniferous series, and, as a rule are unfossiliferous. Two blocks, however, of fossiliferous sandstone of a somewhat peculiar character have been found, and have been identified with the sandstone of the Lower Neocomian series in Lincolnshire. Of the limestones there are a great number of blocks of a hard grey crystalline limestone of the Carboniferous series containing some very perfect specimens of Foraminifera; and two specimens from the Rhætic beds, which are of peculiar interest if, as it is said, the Rhætic beds do not now come to the surface anywhere in the North of England. The greater number, however, of the limestones belong to the Jurassic series; there are also many lumps of very hard chalk which have been identified with the hard chalk of Cambridgeshire. The microscopic sections of the Chalky Boulder-clay show that amid grains of quartz, sand &c. there are a great number of minute Foraminifera still wonderfully well preserved. The way in which the Chalky Boulder-clay and the gravels lie was well shown in a railway-cutting near Dunmow some short time ago, and happily a small photograph of the section was taken at the time, for that part of the cutting has now been covered in. This investigation cannot be said, so far, to have been productive of any great results; but it is possible that this attempt at classifying and describing the rocks of the drift may be of some assistance to those who are considering the general question of the glacial drift.

3. "On Tertiary Cyclostomatous Bryozoa from New Zealand." By Arthur W. Waters, Esq., F.G.S.

May 25.—Prof. J. W. Judd, F.R.S., President, in the Chair.

The following communications were read:—

1. "On the Remains of Fishes from the Keuper of Warwick and Nottingham." By E. T. Newton, Esq., F.G.S.; with Notes on their Mode of Occurrence by the Rev. P. B. Brodie, M.A., F.G.S., and E. Wilson, Esq., F.G.S.

2. "Considerations on the Date, Duration, and Conditions of the Glacial Period with reference to the Antiquity of Man." By Prof. Joseph Prestwich, M.A., F.R.S., F.G.S.

After showing how the discoveries in the valley of the Somme and elsewhere, 28 years ago, led geologists who had previously been disposed to restrict the age of man, to exaggerate the period during which the human race had existed, the author proceeded to discuss the views of Dr. Croll on the date of the Glacial epoch. Dr. Croll, who had at first referred this to an earlier phase of orbital eccentricity, commencing 980,000 years ago, subsequently regarded it as coinciding with a minor period of eccentricity that commenced 240,000 and terminated 80,000 years since. This last estimate was chiefly supported by the amount of denudation that had subsequently taken place.

The efficacy of the increased eccentricity of the earth's orbit in producing the cold of the Glacial epoch was shown to be very doubtful; for as similar changes in the eccentricity had occurred 165 times in the last 100 millions of years, there must have been many glacial epochs in geological times, several of them much more severe that of the Pleistocene period. But of such glacial epochs there was no valid evidence. Another inference from Dr. Croll's theories, that each glacial epoch consisted of a succession of alternating cold and warm or interglacial phases was also questioned, such alternations as had been indicated having probably been due to changes in the distribution of land and water, not to cosmical causes. The time requisite for such interglacial periods as were supported by geological evidence was more probably hundreds than thousands of years.

Recent observations in Greenland by Professor Helland, Mr. V. Steenstrup, and Dr. Rink, had shown that the movement of ice in large quantities was much more rapid, and consequently the denudation produced much greater than was formerly supposed. The average rate of progress in several of the large iceberg-producing glaciers in Greenland had been found to be 36 feet daily. Applying these data and the probable accumulation of ice due to the rainfall and condensation to the determination of the time necessary for the formation of the ice-sheet, the author was disposed to limit the duration of the Glacial epoch to from 15,000 to 20,000 years, including in this estimate the time during which the cold was increasing, or preglacial time, and that during which the cold was diminishing, or postglacial time.

Details were then given to show that the estimate of one foot on an average being removed from the surface by denudation in 6000 years, on which estimate was founded the hypothesis of 80,000 years having elapsed since the Glacial epoch, was insufficient, as a somewhat heavier rainfall and the disintegrating effects of frost would produce far more rapid denudation. It was incredible that man should have remained physically unchanged throughout so long a period. At the same time the evidence brought forward by Mr. Tiddeman, Dr. Hicks, and Mr. Skertchly of the occurrence of human relics in preglacial times, had led the author to change his views as to the age of the high-level gravels in the Somme, Seine, Thames, and Avon valleys, and he was now disposed to assign these beds to the early part of the Glacial epoch, when the ice-sheet was advancing. This advance drove the men who then inhabited western Europe to localities such as those mentioned which were not covered with ice. Man must, however, have occupied the country but a short time before the land was overwhelmed by the ice-sheet. The close of the Glacial epoch, *i. e.* the final melting of the ice-sheet, might have taken place from 8000 to 10,000 years since. Neolithic man made his appearance in Europe 3000 to 4000 years B.C., but may have existed for a long time previously in the east, as in Egypt and Asia Minor civilized communities and large States flourished at an earlier date than 4000 B.C.

3. "Notes on some Carboniferous Species of *Murchisonia* in our Public Museums." By Miss Jane Donald.

June 8.—Prof. J. W. Judd, F.R.S., President, in the Chair.

The following communications were read:—

1. "A Revision of the Echinoidea from the Australian Tertiaries." By Prof. P. Martin Duncan, M.B., F.G.S.

2. "On the Lower Part of the Upper Cretaceous Series in West Suffolk and Norfolk." By A. J. Jukes-Brown, Esq., B.A., F.G.S., and W. Hill, Esq., F.G.S.

The district described in this paper is that of West Suffolk and Norfolk, and is one which has never been thoroughly examined; for no one has yet attempted to trace the beds and zonal divisions which are found at Cambridge through the tract of country which lies between Newmarket and Hunstanton. Until this was done the Hunstanton section could not be correlated definitely with that of the neighbourhood of Cambridge. It was the Authors' endeavour to accomplish this, and the following is an outline of the results obtained by them.

The paper was divided into six parts:—(1) Stratigraphical, (2) Palæontological, (3) Microscopical, (4) Chemical Analyses, (5) Faults and Alteration of Strike, (6) Summary and Inferences. In the four first parts separate lines of argument were followed, and each led to the same set of conclusions.

The chief interest of the paper probably centres in the Gault, and its relations to the Chalk Marl and the Red Chalk. Quite recently the very existence of Gault in Norfolk has been disputed, but the Authors think the facts they adduce and the fossils they have found will decide that point. The Gault at Stoke Ferry is about 60 feet thick, and in the outlier at Muzzle Farm *Ammonites interruptus* occurs plentifully in the form of clay-casts with the inner whorls phosphatized. At Roydon a boring was made which showed the Gault to be about 20 feet thick, the lower part being a dark blue clay, above which were two bands of limestone enclosing a layer of red marl, and the upper 10 feet were soft grey marl; the limestones contained *Amm. rostratus*, *Amm. lautus*, *Inoceramus sulcatus*, and *Inoc. concentricus* (?), while the marls above contained *Belemnites minimus* in abundance. At Dersingham another boring was made which proved the grey marl (2 feet) to overlie hard yellow marl, passing down into red marl, which rests on Carstone. The grey marl thins out northward, and as the red marl occupies the position of the Red Chalk, the Authors believe them to be on the same horizon, an inference confirmed by the presence of Gault *Ammonites* in the Red Chalk.

Another point of importance is the increasingly calcareous nature of the Gault as it is followed northward through Norfolk. This was regarded as evidence of passing away from the land supplying inorganic matter, and approaching what was then a deeper part of the sea; this inference is borne out by the microscopical evidence.

As regards the Chalk Marl, it also becomes more calcareous: at Stoke it is still over 70 feet thick, and its base is a glauconitic marl which can be traced to Shouldham and Marham, but beyond this the base is a hard chalk or limestone, which is conspicuous near Grimston and Roydon, and passes, as the Authors believe, into the so-called "sponge bed" at Hunstanton.

The Totternhoe Stone is traced through Norfolk, but is thin at Hunstanton (2 feet); its existence, however, enables the limits of the Chalk Marl to be defined, with the result that some 13 feet of the hard chalk at Hunstanton must be referred to that subdivision.

The Grey Chalk also thins northward, and from 90 feet near Cambridge is reduced to about 30 at Hunstanton. The Belemnite-marls are traceable in Norfolk, but either thin out or are replaced by hard white chalk near Heacham.

The Melbourne Rock is continuous, and maintains similar characters throughout.

The total diminution in the thickness of Lower Chalk is from 170 feet at Newmarket to 55 feet at Hunstanton, viz. 115 feet. An endeavour was made to estimate the amount and extent of Gault removed by erosion from Arlesey and Stoke Ferry.

5. "On some Occurrences of Piedmontite-schist in Japan." By B. Kotô, Esq.

The occurrence of mangan-epidote or piedmontite in connexion with the glaucophane-bearing rocks, in the crystalline schists of Japan, had already been indicated by the Author. But the *mura-*

saki" or violet-rock contains it as an essential component. This is well developed near Tokusima, and its geological range has been traced further. The piedmontite occurs in this rock along with fine quartz-grains under a schistose arrangement, the accessories being muscovite, greenish-yellow garnet, rutile, some felspars, iron-glance, &c.

The crystals of piedmontite are elongated, cracked, and much striated, and occur with the orthopinacoid parallel to the planes of schistosity. The crystal faces are, as a rule, well developed, thus differing from common epidote, regarded as a rock-forming mineral. Twinning is rare; cleavages upon the base and orthopinacoid are sometimes observed. The clino-pinacoidal sections of the mineral show the most intense colours: the polarization-colours are magnificent. The following is the analysis:—

SiO_2	36·16
Al_2O_3	22·52
Fe_2O_3	9·33
Mn_2O_3	6·43
CaO	22·05
MgO K_2O, Na_2O	0·84
H_2O	3·20
	100·53

The chemist expresses a doubt as to whether the iron exists in the state of sesquioxide or monoxide. The Author then alludes to the difference of opinion as to the state of oxidation of iron in the Swedish and Alpine piedmontites, and suggests that the Japanese mineral supplies a missing link between the two. The Japanese mineral was originally mistaken for tourmaline, and the rock called Tourmaline-schist by E. Naumann. Although comparatively rare both in Piedmont and Sweden, in certain parts of Japan it is so abundant as to constitute a rock-forming mineral, whilst as an accessory it occurs also in the glaucophane-schist.

The Author further describes a peculiar epidote, containing iron, from the glaucophane-schist, and also a peculiar garnet, occurring in rhombic dodecahedra about the size of a pea, which includes many other minerals, but no glaucophane. The garnet is of a deep yellow colour, and is anisotropic, a circumstance probably due to strain from the interposition of other minerals.

June 23.—Prof. J. W. Judd, F.R.S., President, in the Chair.

The following communications were read:—

1. "On Nepheline Rocks in Brazil, with special Reference to the Association of Phonolite and Foyaite." By Orville A. Derby, Esq., F.G.S.

The Author refers to the phonolites and associated basalts of Fernando Noronha, a deep-sea island off the north-eastern shoulder of the continent of South America. Nepheline rocks of a somewhat different character are abundantly developed on the mainland, and under conditions favourable for throwing light on the relations existing between the granitic type, foyaite, and the other members of the group. There are some mountains near Rio de Janeiro composed of these rocks, as is also the peak of Itatiaia, 3000 metres high, the loftiest mountain of eastern South America. A cursory examination of some of these localities having shown an apparent relation between foyaite, phonolite, trachyte and certain types of basalt, Mr. Derby determined to visit the Caldas region, where a railway under construction gave unusual facilities for examining this series. A fine development of foyaite, phonolite and tuff was found, associated with several types that have not yet been met with in the other localities. The existence of a leucite basalt was recognized.

The bulk of the paper was devoted to a detailed description of these railway-sections, and the following deductions are drawn:—

1. The substantial identity, as regards mode of occurrence and geological age, of the Caldas phonolites and foyaites.
2. The connexion of the latter through the phonolites with a typical volcanic series containing both deep-seated and aerial types of deposits.
3. The equal, if not greater antiquity of the leucite rocks as compared with the nepheline rocks, whether felsitic, as phonolite, or granitic as foyaite.
4. The probable palæozoic age of the whole eruptive series.

2. "Notes on the Metamorphic Rocks of South Devon." By Miss Catherine A. Raisin, B.Sc.

This communication consisted mainly of detailed observations, supplementary to those published by Prof. Bonney in the Society's Journal for 1884, on the slaty and metamorphic rocks of South Devon in the neighbourhood of Salcombe estuary. In the first part of the paper details were given of the sections exposed around the estuary, at Hope Cave to the westward, and in several localities to the eastward as far as Hall Sands, all confirmative of Professor Bonney's views, and showing that the slaty beds to the northward do not pass into the mica and chlorite schists to the south, but are separated from the latter by a line of faults.

Descriptions were then given of microscopic slides from various parts of the metamorphic rocks. Some of these showed the action of secondary forces. The effects of lateral pressure in producing cleavage-planes and a kind of jointing were also commented upon.

An attempt was also made to determine the succession of chlorite-, mica- and micaceo-chloritic schists around Salcombe estuary. The beds appeared to succeed each other in the following order, commencing from the northward:—

1. (*a*) Interbanded series south of Halwell Wood &c.
   (*b*) A thick band of chlorite-schist at Scoble and at Snapes Point &c.
2. (*a*) Mica-schist north of the side estuary.
   (*b*) Interbanded series south of the side estuary.
3. Mica-schist of Portlemouth Ferry.
4. Chlorite-schist of Bickerton.

3. "On the Ancient Beach and Boulders near Braunton and Croyde in North Devon." By Prof. T. M'Kenny Hughes, M.A., F.G.S.

The Author observes that amongst the raised beaches of S.W. England we generally find included the sand cliffs of Saunton Down and Middleborough on the coast west of Barnstaple. These deposits possess a further interest owing to the occurrence at their base of large boulders. In 1866 Mr. Spence Bate, in opposition to the prevailing view, concluded that the so-called raised beach is the undestroyed remnant of an extensive district of wind-borne sand similar to that which now exists on Braunton Burrows. The points to which attention was invited are as follows:—

(1) Is this deposit on the southern slope of Saunton Down a raised beach?

(2) Were the above-mentioned boulders carried to their present position by ice?

The paper was fully illustrated by diagrams, showing the relations of the recent deposit, and by figures showing the mode of occurrence of three of the most remarkable boulders. The conclusions were:—(1) That the ancient beach of Saunton Down and Croyde is not a *raised beach* in the ordinary acceptation of the term. The top is subaerial talus, the middle part is blown sand, the base only marine, and the marine part is not above the reach of the waves of the sea at its present level. (2) The boulders of granite and felsite which occur at the base of the ancient beach were transported to their present position by the waves of the sea. Such as are of local origin could have reached the sea by the ordinary processes of denudation; such as are possibly of northern origin could have been carried down the Irish Channel on bergs, and been thrown up by the sea to their present position at any period subsequent to their transportation southwards by ice; but their presence does not imply any *local* glaciation.

4. "Notes on the Formation of Coal-seams, as suggested by evidence collected chiefly in the Leicestershire and South Derbyshire Coal-field." By W. S. Gresley, Esq., F.G.S.

The Author's principal object in this paper was to bring forward evidence in opposition to the view now generally accepted that coal-seams were formed from vegetation growing on the spot.

He showed that during a very extensive experience he had only once or twice detected stems passing into a bed of coal and connected with the Stigmaria-roots in the underclay. If, as was generally stated, the Stigmariæ were the roots of the trees that formed the coal, such instances ought to be common. Not only, however, were they very rare, but the abundance of the Stigmariæ was extremely variable, and these roots, instead of becoming more thickly matted together in the uppermost part of the underclay, as they should be if they were roots of the coal-forests, were generally distributed, as a rule, throughout the clay in a manner that showed them to have been in all probability independent organisms. Stigmarian roots, when found connected with a stem, were more often on the top of the coal-seam than at the bottom.

Other reasons assigned for rejecting the hypothesis that coal-seams were formed of plants that grew upon the spot were the occasional absence of underclays, the sharp division between the coal-seams themselves, and the beds above and below them; the distinct lamination of every seam and its division into layers of different mineral character that are persistent over large areas; the presence of foreign bodies in the underclay, and especially of pebbles and boulders transported from a distance; the presence of similar foreign bodies, and occasionally of remains of aquatic mollusca, fish, &c. in the coal itself; and the circumstance that many coal-seams are impregnated with salt, and are associated with beds containing marine fossils.

5. "Note on some Dinosaurian Remains in the Collection of A. Leeds, Esq. Part I. *Ornithopsis Leedsii.* Part II. *Omosaurus*, sp." By J. W. Hulke, Esq., F.R.S., F.G.S.

6. "Notes on some Polyzoa from the Lias." By Edwin A. Walford, Esq., F.G.S.

7. "On the Superficial Geology of the Southern Portion of the Wealden Area." By J. Vincent Elsden, Esq., B.Sc.

The Author, after referring to Sir R. Murchison's paper, published more than thirty years ago, on "The Distribution of the Flint Drift of the S.E. of England," proceeded to give in detail his observations on the angular flint-deposits of the Arun, Adur, Ouse, and Cuckmere basins. He also noticed a sandy or loamy deposit containing angular fragments of ironstone, and generally a few small angular flints that occurred on the surface of the Lower Greensand and, to a small extent, on the Weald Clay. A block of granite, weighing between 5 and 6 lbs., was found on the chalk escarpment at Kilhurst Hill.

The angular flint-drift occurred mainly on the higher parts of the area, and was wanting in the river-valleys, where, however, river-gravels derived from the denudation of the older deposits were abundantly developed. This distribution of the angular drift was shown to be incompatible with the theory of its origin advocated by Sir R. Murchison and some other geologists, who attributed it to a violent and transitory current. The Author showed that not only in the Wealden area, but throughout many of the neighbouring districts, the angular drift consisted of the undenuded remnants of a deposit formed before the present river-valleys were cut, and that many of the river-gravels, though newer than the angular drift, were deposited when the valleys had been less excavated than they now are. This was Mr. Topley's view with respect to the northern portion of the Wealden area. Mr. Searles V. Wood's marine theory of the origin of these gravels was discussed and shown to be refuted by their mode of occurrence. It was, moreover, contended that the drift, although composed of local materials, was probably of subaqueous origin, and not merely subaerial. The discovery of a granite boulder might, if confirmed by other discoveries, lead to a modification of the views generally held as to the physical character of the area during the glacial period.

8. "Report on Palæo-botanical Investigations of the Tertiary Flora of Australia." By Dr. Constantin Baron von Ettingshausen, For.Corr.G.S.

9. "On some new Features in *Pelanechinus corallinus*," By T. T. Groom, Esq.

10. "On Boulders found in Seams of Coal." By John Spencer, Esq., F.G.S.

The discovery of a boulder weighing 6 lbs., and composed of granite, in the Gannister or Mountain-Mine seam of the Rossendale district, at Old Meadows Pit, near Bacup, Lancashire, had led the Author to call attention to the occasional occurrence of similar boulders in various parts of Lancashire and Yorkshire. Such boulders were always isolated, and sometimes imbedded in the seam, sometimes in its upper surface. They were always waterworn and rounded, and were composed, so far as had been observed, of granite, gneiss or quartzite foreign to the district.

After considering the various suggestions that had been made as to the means by which such boulders had found their way into the coal, the Author gave the preference to the action of floating ice, both because the presence of fragments from a distance would thus be more readily explained, and because ice-scratched rocks have been found *in situ* in the Millstone Grit within three miles of the place whence the boulder mentioned was obtained.

XXXVII. *Intelligence and Miscellaneous Articles.*

ON THE OCCURRENCE OF ELECTROMOTIVE FORCES IN METAL PLATES WHICH ARE TRAVERSED BY A CURRENT OF HEAT WHILE PLACED IN THE MAGNETIC FIELD. BY A. VON ETTINGSHAUSEN AND W. NERNST.

IN the course of our observations of Hall's phenomenon, we were led by certain irregularities to make the following experiments.

A rectangular plate of bismuth about 5 centim. in length, 4 centim. broad, and 2 centim. thick, provided in the longer sides with two electrodes opposite each other, is brought into the field of an electromagnet in such a way that the lines of force cut the plane of the plate at right angles. This is supported by being clamped at the shorter sides in strips of copper, but protected from direct contact with the latter by plates of mica.

When one or the other copper strip is heated, a current of heat traverses the plate lengthwise. When the magnetic field of the electromagnet is established, a permanent galvanic current is then observed in a galvanometer connected with the electrodes of the plate, which lie nearly on an isothermal line. The direction of this current changes with the direction of magnetization, and with the direction of the current of heat in the plate; if the bismuth is heated on both sides, the action of the magnet disappears.

The electromotive force which produces the current is proportional to the strength of the magnetic field, to the distance of the electrodes, probably also to the fall of heat along the plate; it seems independent of the thickness of the plate.

It was obvious to suppose that the cause of the electromotive force might be thermoelectrical: the temperature of the two electrodes (the copper wires soldered to the bismuth plates) might have been altered under the influence of magnetism. Direct experiments with thermoelements, which were placed carefully insulated between two plates traversed by a current of heat, showed no change of temperature in consequence of magnetic action; nor was there any when, instead of electrodes, thermoelements (argentan-copper) were soldered to the plates; the electromotive force was also seen to be independent of the nature of the electrodes. Hence there is no deflection of the heat current in the bismuth plate in consequence of magnetic forces.

If the electrodes lie in the direction of the current of heat, if they are therefore anisothermal, and if the thermoelectric current which takes place between them even without a magnetic field is compensated, then, when the field is produced, an electromotive force is produced in one or the other direction, but mostly varying in strength.

In eight bismuth plates from different sources, the direction of the "transverse" currents, that is the "thermomagnetic currents" at right angles to the current of heat has been ascertained to be the same; the current flowed in such a direction through the plate that from the starting point of the current in the plate, to the starting point of the current produced, we get a motion *opposed* to the direction of the current producing the field. Only in one plate, in preparing which the metal was rapidly cooled, was there seen a different behaviour; after melting and slow cooling this bismuth also came within the above rule.

As respects magnitude, we observed that using a magnetic field of the absolute strength 5000 (C.G.S.) in an almost rectangular plate 5 centim. in the side, and 1·9 millim in thickness, which on one side was heated by means of a hot copper strip, while the other side was cooled by ice-water, an electromotive force of $\frac{1}{800}$ of a volt was observed.

The direction of the "longitudinal" electromotive force, which, as we observed, did not change with the field, was such that the galvanic current flowed in the plate from the heated to the colder electrode; yet here also individual differences seem to prevail. The longitudinal effect was feebler with the magnetic forces applied than the transverse one, yet it increased more rapidly than the latter with increasing strength of the magnetic field (probably in proportion to the square).

For a further investigation we examined more minutely a bismuth plate which was provided with eight equidistant electrodes arranged on the periphery of a circle; all the electrodes were within the homogeneous magnetic field.

If we denote their positions by N., S., E., W., N.E., S.W., S.E., N.W., the heat current flowed from west to east, and the thermomagnetic effect was observed between two opposite electrodes for two different intensities of the magnetic field (2480 and 4320). By careful regulation of the flow of heat the galvanometer showed pretty regular deflections.

When the thermal stream-lines in the plate were exactly in the W.E. direction, by connecting the N. and S. with the galvanometer the purely transversal, with the E.W. the longitudinal, and with the N.E., S.W., and S.E., N.W., the effect resulting from these components was obtained. As, in reality, the above condition is not accurately fulfilled the result of a transverse and longitudinal effect is always obtained, which is seen in the unequal intensities of the currents observed when the direction of the magnetic field is reversed; each individual effect may, as is obvious, be separately calculated. We find thus for the transverse (τ) and longitudinal (λ) effects the following values:—

Strength of Field 2480.

	N.S.	E.W.	N.E., S.W.	N.W., S.E.
τ	142	3	90	96
λ	3	13	13	11

Strength of Field 4320.

	N.S.	E.W.	N.E., S.W.	N.W., S.E.
τ	245	5	153	163
λ	7·5	42	38	38

While the ratio of the strengths of field is 1·74, the ratio of the transverse effects is respectively 1·73, 1·70, 1·70, 1·70; that of the longitudinal on the contrary 2·50, 3·23, 2·92, 3·45—in the mean very near the ratio of the square (3·03) of the strength of field.

We endeavoured to obtain actions of this kind in plates of other metals. Hitherto this has only succeeded distinctly with antimony, nickel (two specimens) cobalt, iron (two specimens) and steel. No, or at any rate uncertain effects are observed with copper, zinc, aluminium and palladium. The direction of the transverse current is the same with antimony, nickel, and cobalt as with bismuth; with iron and steel, on the contrary, it is reversed, but in all the action is considerably weaker. As to the longitudinal effect which must in every case exist, we are wanting in certain materials of observation.

We compared equal large plates of antimony, nickel, and cobalt with a bismuth plate by simultaneously heating them on one side, and cooling on the other while separated from each other by a plate of mica, and placed in the magnetic field. The ratio of the thermomagnetic effects referred to Bi=100, were for Sb=5·5, for Ni 4·8, and for Co=0·5.

In how far the observed currents can be brought in connexion with Hall's phenomenon must for the present be left undecided. We may be allowed to mention that Sb, Co, Fe, and steel have a positive rotatory power, Bi, and Ni, on the contrary, a negative one, which is in agreement with our own measurements of the Hall's phenomenon in the plates in question, although our numbers differ greatly from those of Hall.

If the bismuth plate previously mentioned was traversed by a galvanic current, then in order to produce in the same magnetic field a Hall's electromotive power of the same strength as the thermomagnetic which we observed, it must have an intensity of 15 amperes, assuming the same intensity in all parts of the plate.

This phenomenon appears in any case to be ultimately related to the molecular structure of the metals.—Wiedemann's *Annalen*, No. 10, 1886.

ON THE EXPERIMENTS MADE AT THE INSTANCE OF THE ELECTROTECHNICAL UNION ON THUNDER-STORMS AND PROTECTION AGAINST LIGHTNING. BY L. WEBER.

In order to investigate the influence of the efficiency of lightning-conductors, the author arranged two lightning-conductors, about 6 metres high, which carried the various points, about 5 metres from each other, and introduced a galvanometer into the earth-contact. Experiments made with this arrrangement in various places in the Riesengebirge, as well as in Breslau and the neighbourhood, showed the applicability of the method. Observations were also made on the increase of potential with the height. A kite, the conducting-cord of which was connected with a galvanometer, was allowed to rise to various heights near a detached house in the vicinity of Breslau, and the following results were obtained:—

Height, in metres	45	71	107	140	115	78	41	139
Strength of current	29	61	451	1078	627	257	40	1332

The unit of current is 10^{-9} amperes. At the beginning of the observations the sky was covered with light cirrostratus clouds; towards the end it cleared up. The observations agree with those of Exner, found by electroscopic means (*Beiblätter*, vol. xi. p. 292). Some experiments were also made during the passage of clouds, which sometimes showed their negative electricity. In thunderstorms the galvanometer shows a momentary agitation, which corresponds to a return shock in the lightning-conductor. The needle then begins to be deflected in the opposite direction with a gradually increasing strength, until after a fresh flash a throw in the opposite direction is observed. The intensity of the deflection increases with the proximity of the lightning-flashes.—*Beiblätter der Physik*, vol. xi. p. 376; *Electrotechnische Zeitschrift*, vol. vii. p. 445.

ON THE MELTING-POINT OF ICE AT PRESSURES UNDER ONE ATMOSPHERE. BY B. J. GOOSENS.

The author finds that when the pressure is reduced from 760 to 5 millim. the melting-point of ice is raised 0·0066°. The determinations were made as follows:—

A glass tube was closed at the bottom by a cork, through which thermoelements were inserted, so that one set of solderings was inside and the other outside the tube. After the tube had been filled with water, and placed in a vessel which also contained distilled water, the upper end of the tube was connected with the air-pump. The water in both vessels was then frozen, and as the ice again began to melt, the whole apparatus was brought into a large vessel containing lumps of ice. The ice in the interior kept for several days. If some in the inner was melted all could be frozen by lowering the pressure. A galvanometer was connected with the thermoelements, and readings were made, firstly, when there was the same pressure in both vessels; and, secondly, after the pressure in the inner vessel was lowered to 5 millim.; and after waiting until the temperatures were constant.—*Arch. Neerland*, xx. p. 449. *Beiblätter der Physik*, No. 7, 1877.

ON THE THOMSON EFFECT. BY A. BATELLI.

The experiments were made with cadmium, which is relatively homogeneous and shows the phenomenon in particular strength. Two perfectly equal rods of pure cadmium 36 centim. in length, and 5 millim. in thickness, were covered in the middle and at each end for a length of 5 millim. with copal varnish, which resists temperatures over 200°, and does not transmit mercury, and were wound with a very thin silk ribbon. The two bars passed through two perfectly similar boxes of sheet-iron filled with equal quantities of mercury, in which were immersed the middles of the rods. Where each end projected from the box thermoelements of thin iron and argentan wires were fastened to the rods. The varnished ends of the rods were in melting ice, and the unvarnished in a steam bath, and were connected there by a thick copper wire. The cold ends were connected with the poles of a battery of 2 Bunsen's elements. In the mercury in the two iron boxes (which was kept in motion by a stirrer) dipped the two varnished junctions of a thermopile of two iron-argentan elements connected with a sensitive Thomson's galvanometer. By dipping a hot iron bar in the mercury box and observing the deflections the masses of mercury could be so equalised that their water values were equal; the water value of the boxes could also be determined. If i is the intensity of the current, the quantities of heat in the two boxes are $a\,i^2 \pm b\,i$, where $2\,bi$ is twice the Thomson effect for the strength of the current i. By inverting the bars their irregularities were also eliminated.

The deflection of the galvanometer was observed from 30 to 30 seconds, every 20 minutes before, during, and after passing the current, and the intensity of the latter was observed every minute.

It was thus found that when the temperatures of the ends projecting from the iron boxes were 63°·5 and 42°·5, and the strengths of the currents were between 0·3 and 0·5 units (C.G.S.) the Thomson effect was proportional to the latter. The heat developed in a second by unit current, at a place with the mean temperature 53°, whose ends differ by a temperature of 1° C., is about E=0·000011215 thermal units. At higher temperatures a petroleum vapour bath was used instead of a steam bath, in which the ends of the rods near the iron boxes were at 124·1 and 92·7. In this case the action for the mean temperature 108°·4 was E=0·000013403.

On the assumption of Tait that the Thomson effect is proportional to the absolute temperature (273+53) and (273+108·4) the latter number should be 0·000013121.

On heating in boiling petroleum the places near the iron boxes were at the temperatures 263°·5 and 220°, and for the mean temperature 243°·25 the effect was 0·00001786, while it should be 0·00001897 on Tait's hypothesis. The observations agree therefore with this hypothesis.—*Atti della R. Acc. di Torino*, 22, p. 48, 1886. *Beiblätter der Physik*, vol. xi. p. 463.

ON HYGROMETRIC SUBSTANCES. BY HENRI DUFOUR.

The author investigated the behaviour of various hygrometric substances. He denotes by α the ratio between the aqueous vapour absorbed and the weight of dry substance (moisture =100); and by β the coefficient of hygrometric expansion, that is, the total expansion which a bar of unit length undergoes when it has taken up the maximum aqueous vapour.

Horn 10 millim. in thickness	$\alpha=0{\cdot}10$	$\beta=0{\cdot}061$
Gelatine	$\alpha=0{\cdot}34$	$\beta=0{\cdot}108$
Goldbeaters-skin.................	$\alpha=0{\cdot}43$	$\beta=0{\cdot}060$

The last body is what he most recommends.—*Arch. d. Gen.* [3] xvi. pp. 197–199, 1886. *Beiblätter der Physik*, No. 7, 1887.

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

[FIFTH SERIES.]

OCTOBER 1887.

XXXVIII. *On Hot Gases as Conductors of Electricity.*

By JOHN BUCHANAN, *B.Sc.**

[Plate VIII.]

THE measurements described below were made in the spring of 1886 in the Physical Laboratory of University College, London. They were designed with the object of getting an approximate idea of the magnitude of the quantities that enter into the phenomena of the discharge of electricity by a flame.

The method used was essentially that ordinarily employed in connexion with the measurement of extremely high resistances, viz. by means of a condenser of known capacity and a quadrant-electrometer†.

I am not aware that this method has been used before for measurements connected with hot gases.

Two flat pieces of platinum-foil (*c*, figure), 2·5 centim. square, were placed with their planes nearly parallel and vertical. They were joined by platinum wires to the respective binding-screws A and B of a condenser of 1 microfarad capacity. The condenser was charged from a battery of Leclanché cells. A Thomson's quadrant-electrometer, D, served to indicate the changes in the condenser-potentials in the usual way, by the motion of a spot of light over a scale. A gas-flame, reduced to the smallest size compatible with its existence, was placed between the two pieces of platinum-foil. The form of the gas-burner was such that the

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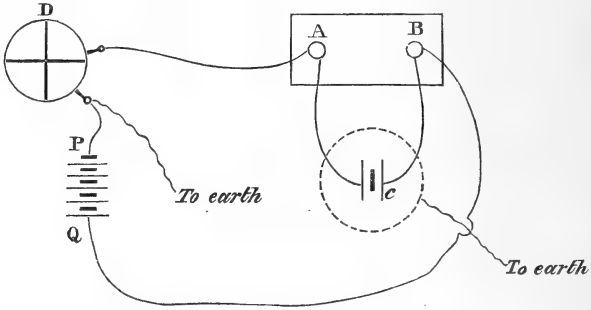
* Communicated by the Author.

† Clerk Maxwell's 'Electricity and Magnetism,' 2nd ed., vol. i. § 355.

gas issued from a very narrow horizontal slit, giving a flame as nearly "linear" as possible. To protect the flame from draughts of air, and also to ensure that no electrical disturbance could arise from outside influences, the flame and platinum pieces were surrounded by a metal cylinder, open at both ends, of about 10 centim. diameter and 36 centim. high, which was permanently connected to earth by a wire. The position of the cylinder is indicated by the dotted circle round *c* in the figure. The stem of the gas-burner, and the wires from the pieces of foil passed through holes in the side of the cylinder without touching it.

In the first regular experiments that were made the respective pairs of quadrants of the electrometer were joined to the binding-screws of the condenser, one pair of quadrants being as usual connected to earth. The condenser was then charged; 4 Leclanchés sending the spot of light near to the end of the scale. The flame being in action, the battery was disconnected and the scale-readings taken at equal intervals of time until the spot of light was down to near the zero. From these readings the rate of "leakage" could be found. Obviously the range of potentials was very limited.

For the later experiments, specimens of whose results are given below, an arrangement was suggested to me by Prof. G. C. Foster, F.R.S., which is applicable to much wider ranges of potential.



The figure gives a diagram of the connexions. The binding-screw B of the condenser was kept in connexion with one electrode, Q, of the battery; the other electrode, P, with the earth and with one pair of quadrants; whilst the other binding-screw, A, of the condenser was connected to the insulated pair of quadrants during the whole of each set of observations.

Before beginning a set of readings, the two pairs of quadrants of the electrometer were put into connexion with one another and with P; this charged the condenser. Since the quadrants were connected, the spot of light stood initially at zero. The two pairs of quadrants were then disconnected from each other, and discharge by the flame showed itself on the electrometer-scale by a motion of the spot of light out over the scale from the zero.

Each set of readings had of course to be concluded when the light moved off the scale—that is, when the difference of potential between the pairs of quadrants had increased to about that of 4 Leclanchés.

The mean distance between the pieces of platinum-foil was 0·75 centim.; the height of the flame was approximately 0·4 centim. Readings were made at intervals of 20 seconds, time being given by the laboratory chronometer.

There are given in detail in the tables (*a*), (*b*), (*c*), (*d*) below the results of observations, using batteries of 8 and 6 Leclanchés respectively. The curves (α), (β), (γ), (δ) (Plate VIII.) are obtained by plotting-out the numbers in the first two columns of these tables, taking the observed scale-reading as ordinate and time as abscissa; the agreement with one another of the observations is well shown by the smoothness of the curves. In the tables the columns headed "calculated deflection" are obtained from the equations which were found, by trial, to suit the curves best. The scale of readings is such that 134 divisions = 2 volts, nearly.

To find the constant for the apparatus, leakage was allowed to go on for an hour without a flame. The potential fell from 715 scale-divisions to 637 in that time.

(*a*)

+ quadrant insulated. 8 Leclanchés = 715 divisions.

Time.	Deflection.		Time.	Deflection.	
	Observed.	Calculated.		Observed.	Calculated.
m s			m s		
0 0	697	697	4 0	497·5	496
20	678		20	484·5	
40	659		40	470	
1 0	640	640	5 0	457	455
20	622		20	444	
40	604·5		40	431	
2 0	588	589	6 0	419	418
20	571		20	407·5	
40	555		40	395·5	
3 0	540	540	7 0	384	384
20	525·5		20	373	
40	510·5		40	362·5	
			8 0	352·5	352·5

Equation to curve α is

$$y = 697 \times 10^{-\cdot 037t},$$

t being in minutes.

(*b*)

— quadrant insulated. 8 Leclanchés = 715 divisions.

Time.	Deflection.		Time.	Deflection.	
	Observed.	Calculated.		Observed.	Calculated.
m s			m s		
0 0	693	693	3 0	506	506
20	669		20	489	
40	647		40	472	
1 0	625	624	4 0	457	456
20	604		20	441	
40	583		40	427	
2 0	563	562	5 0	412	411
20	543		20	397	
40	523		40	384	
			6 0	370	370
			20	357	

Equation to curve β is

$$y = 693 \times 10^{-\cdot 0454t},$$

t being in minutes.

(*c*)

— quadrant insulated. 6 Leclanchés = 540 divisions.

Time.	Deflection.		Time.	Deflection.	
	Observed.	Calculated.		Observed.	Calculated.
m s			m s		
0 0	522	522	5 0	299	299
20	504·5		20	287	
40	487		40	275·5	
1 0	470·5	470	6 0	265	265
20	455		20	254·5	
40	437·5		40	244	
2 0	421	421	7 0	234	234
20	406		20	225	
40	391·5		40	215·5	
3 0	378	377	8 0	207	207
20	364		20	198·5	
40	349·5		40	191	
4 0	336	336	9 0	183	182
20	324				
40	311				

Equation to curve γ is

$$y = 522 \times 10^{-\cdot 0454t - \cdot 00061t^2},$$

t being in minutes.

(*d*)

+ quadrant insulated. 6 Leclanchés = 540 divisions.

Time.	Deflection.		Time.	Deflection.	
	Observed.	Calculated.		Observed.	Calculated.
m s			m s		
0 0	525	525	6 0	303	303
20	509		20	293·5	
40	494·5		40	285	
1 0	480		7 0	277	
20	466		20	268·5	
40	452		40	260·5	
2 0	438·5	435	8 0	253	254
20	426		20	246	
40	413		40	239	
3 0	400·5	397	9 0	232·5	233
20	388		20	226	
40	377		40	219·5	
4 0	365	362	10 0	214	215
20	354		20	208·5	
40	343·5		40	202·5	
5 0	332·5	331	11 0	197	
20	322		20	192	
40	312		40	187	
			12 0	182·5	182·5

Equation to curve δ is

$$y = 525 \times 10^{-\cdot 04136t + \cdot 00025t^2},$$

t being in minutes.

It will be observed, both from the tables and from the curves, that the rate of leakage is more rapid when the insulated quadrant is charged negatively than when charged positively.

Taking the approximate rate of leakage from the results in tables (*a*) and (*d*), and subtracting the rate without the flame, we have:—

Mean logarithmic rate per minute $= \frac{1}{2}(\cdot 037 + \cdot 0414) - \cdot 0014$,
Mean logarithmic rate per second $= \cdot 00063$,
and 1 microfarad $= 10^{-15}$ (C.G.S.) units ;
$\log_\epsilon 10 = 2\cdot 303$;

$$\therefore \text{“Resistance”} = \frac{10^{15}}{\cdot 00063 \times 2\cdot 303} \text{ (C.G.S.) units.}$$

$$\text{,,} \quad = 689 \times 10^{15} \text{ (C.G.S.) units.}$$

$$\text{,,} \quad = 689 \times 10^{6} \text{ ohms.}$$

In the same way, using the results from tables (*b*) and (*c*), we get

$$\text{“ Resistance ”} = \frac{10^{15}}{\cdot 00073 \times 2\cdot 303} \text{ (C.G.S.) units.}$$

$$\text{,,} \qquad = 595 \times 10^{15} \text{ (C.G.S.) units.}$$

$$\text{,,} \qquad = 595 \times 10^{6} \text{ ohms.}$$

Some measurements in which brass knobs were used instead of the platinum-foils yielded results similar to those just given. The flame was, however, of a rounded conical form, about 5 millim. high. The knobs were each 1·7 centim. in diameter, the distance between their nearest points being 8 millim. A Daniell cell gave 65 divisions of a deflection on the electrometer-scale.

The results of the observations were plotted-out as described above, and the equations to the curves found by trial. Taking, as before, y to represent the reading on the electrometer-scale at the time t, in minutes, I find :—

With the + pole of the battery to earth, in two different measurements,

$$y = 315 \times 10^{-\cdot 0394t - \cdot 00148t^2},$$

$$y = 340 \times 10^{-\cdot 039t - \cdot 00092t^2}.$$

The negative pole put to earth gives

$$y = 313\cdot 5 \times 10^{-\cdot 0328t - \cdot 000395t^2}.$$

These quantities are of the same dimensions as those given above, and need not be discussed further here.

Gordon's College, Aberdeen.
August 1887.

XXXIX. *Oxygen in the Sun.* *By* Professor JOHN TROWBRIDGE *and* C. C. HUTCHINS*.

SINCE the time it was announced that hydrogen existed in great abundance in the sun's atmosphere and was a controlling element in its economy, there have been no more interesting questions in solar physics than those touching the presence of other gases in the sun's body and atmosphere; and when we consider the important part that oxygen plays in terrestrial affairs, the great variety of combinations into which it enters, and its high constituent percentage in the composition of the earth itself, a peculiar interest, second to that of no other element perhaps, attaches to its probable presence in the sun.

The investigation of the spectrum of oxygen as a research by itself, and as connected with its presence in the sun, has

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* From an advance proof from the Proceedings of the American Academy of Arts and Sciences, vol. xxiii. Communicated by Professor J. Trowbridge, of Harvard University, Cambridge, Mass., U.S.

occupied many eminent physicists; but the fact that the latest and most complete investigations have left the minds of scientific men still in doubt, has led the writers to take up the question again with more perfect and powerful apparatus and increased facilities, in order, if possible, to add something to the knowledge of the subject.

The question of the existence of oxygen in the sun was first seriously investigated, we believe, by Dr. Henry Draper, who published in the American Journal of Science for 1877 and 1879, and in foreign journals, papers accompanied by reproductions of his photographs. Dr. Draper was firmly persuaded of the existence of oxygen in the sun's atmosphere, and based this belief upon the apparent coincidence of the lines of oxygen taken in air with certain bright spaces in the sun's spectrum which appeared upon his photographs.

Prof. John Christopher Draper published a paper in the 'American Journal of Science' for 1878, in which he stated his conviction that oxygen exists in the sun; but his line of argument was just the reverse of that of Dr. H. Draper. While the latter apparently proved the existence of oxygen in the sun by the coincidence of its bright lines with bright spaces in the solar spectrum, the former was led to believe that the bright oxygen lines coincided with dark lines in the sun.

Both observers abandoned the old method of eye observation, and took advantage of the improvements in photography to record the oxygen lines upon a sensitive plate. Dr. H. Draper was led to abandon Geissler's tubes filled with oxygen, and to employ the electric spark in common air, on account of the greater brilliancy of the lines, while Prof. J. C. Draper still adhered to tubes filled with rarefied oxygen. The oxygen lines had been mapped by previous observers, notably by Thalén, and Schuster had shown that there were four spectra of oxygen which could be produced under varying conditions of temperature and pressure.

The photographs of Dr. Henry Draper's oxygen spectrum, together with the juxtaposed solar spectrum, were submitted to the French Academy of Sciences in Paris, June 23, 1879, by M. Cornu. From the remarks of M. Faye we make the following extract:—

"Dr. H. Draper has, however, succeeded in discovering oxygen, not in the chromosphere, but in the photosphere, where it discloses itself by bright lines. It is obvious that this gas is dissociated at a depth, and is immediately taken up by multiple combinations in the region and at the temperature of the brilliant surface. I see in these facts the hope of a confirmation, and above all of an extension, of the views I have put forth on the constitution of the sun; but whatever may be the fate that the progress of spectrum analysis reserves to them, I express here my admiration for the discovery of Mr. Draper, and I hope that his results, so well confirmed by the photographic proofs that our learned member, M. Cornu, has shown the Academy, will meet with no delay in being universally accepted by competent judges."

The opinion thus expressed by so eminent an authority as M. Faye testifies to the strength of the evidence brought forward by Dr. Draper. With the exception of Prof. John C. Draper, physicists, in so far as they have expressed their views, have generally accepted the hypothesis of Dr. Draper. No one, to our knowledge, has critically examined the hypothesis of bright lines in the solar spectrum.

The reader of Dr. H. Draper's account of his experiments will remember the difficulties he encountered in obtaining an air-spectrum of sufficient brightness to record itself upon the photographic plate. The time that has elapsed since his work does not seem to have made those difficulties less, and, in spite of all our ingenuity has been able to devise, we have been practically confined to taking the spark in free air or oxygen at atmospheric pressure, notwithstanding the broad and hazy character of the lines under these conditions.

Not to record a long list of failures extending over several months, we will briefly describe the arrangements in their final form.

An alternating current dynamo driven at 2000 revolutions per minute was connected to a commutator of four segments upon a fixed spindle, around which revolved two pairs of brushes. The result of this combination was that the current was very frequently and sharply interrupted. This interrupted current was used to excite three large quantity-coils connected in series. From two to twelve jars were employed as a condenser to the secondary current. The spark was taken between two stout rods of aluminium placed immediately in front of the slit, and the spark passed between them with a deafening rattle, and gave about the light of two candles. We tried Dr. Draper's device of a soapstone compressor for the spark, but in our hands the walls of the soapstone near the spark melted down, and formed a conducting surface over which the current passed.

The photographic apparatus is the large instrument of Professor Rowland,—a concave grating with ruled surface 6 × 2 inches, mounted upon an iron girder 23 feet long, moving upon two tracks at right angles, as has been previously described by him and others. Sunlight is introduced by a heliostat with mirror silvered on first surface, and an image of the sun formed on the slit by means of a quartz lens of five feet focus. The method of working with the apparatus so arranged has been as follows.

The points of aluminium being permanently fixed in front of the slit, sunlight is introduced, the camera brought to focus once for all, and set to any required wave-length upon a convenient scale. The photographic plate is then placed in the camera, and a shutter immediately in front is set to expose the upper half of the plate. Exposure for the sun is then made; the sunlight is then shut out, and the shutter moved to cover that part of the plate already exposed, and the lower half exposed. The spark is then started and worked from 15 to 30 minutes. In addition to the spectrum of lines there is a considerable continuous spectrum, which after a time causes fogging of the plates; so there does not seem to be any gain in an exposure of more than half an hour. The feebleness of the air-lines can be judged of when we state that, with the same plate, breadth of slit, &c., we get a metallic spectrum in the arc in ten seconds, strongly photographed. There was sufficient iron present in the electrodes as impurity to give the strongest iron-lines feebly, and these have been of use in determining that no displacement had happened, although, from the nature of the arrangements, such disturbance could hardly occur.

On the negative produced as above indicated the two spectra lie exactly edge to edge, like a vernier and scale, and are in the best possible position for the accurate determination of the position of the air-lines. The original plan contemplated a determination of the wave-lengths of all the air-lines throughout the entire spectrum; but persistently bad weather and other causes have compelled the postponement of the completion of this work, though we are now able to give it complete from wave-length 3740 to wave-length 5030.

The photographic map of the solar spectrum of Professor Rowland has made easy what would otherwise have been an undertaking of extreme labour and difficulty. The best of engraved maps of the violet region of the spectrum to beyond F are comparatively worthless. Even on the elaborate map of Vogel, the result of years of labour, it is difficult to recognize with certainty other than the more prominent lines, and you never feel quite sure of your positions; but we turn to the map of Rowland with the certainty of finding every line in its true order and magnitude, so that what was formerly most difficult has now become very simple, and the position of any well-defined air or metallic line can be read directly, by comparison of the photograph with the map, to the tenth of a wave-length.

We here give a table of wave-lengths as determined from our photograph of the sun and air spectra :—

3749·80	Strong, agrees.
3755·35	" "
3830·60	Faint and broad.
3839·275	Dim and broad.
3842·30	Very faint.
3843·00	"
3850·70	Faint.
3857·40	"
3863·80	"
3864·90	"
3882·45	Strong.
3893·50	Faint.
3894·95	"
3896·40	"
3896·90	"
3900·975	Sharp.
3902·20	Very faint.
3906·00	Sharp.
3912·30	Fairly strong.
3919·25	Strong, agrees.
3935·10	Very faint.
3936·90	Faint.
3938·80	"
3939·80	"
3940·70	"
3941·40	"
3942·48	Sharp.
3946·20	"
3948·10	Very faint.
3949·00	Sharp, may agree.
3951·45	"
3954·85	Strong.
3956·175	Strong, agrees.
3958·10	Faint.
3958·90	
3959·975	Sharp.
3963·70	"
3968·70	
3973·60	Strong.
3981·40	
3982·97	Faint.
3992·87	Sharp, agrees.
3995·10	Very strong.
3998·81	Very faint, may agree.
4008·39	
4011·34	Faint.
4035·34	Band.
4041·39	Band.
4066·84	Faint.
4070·24	Strong, may agree.
4072·34	"
4076·19	"
4078·83	Faint, agrees.
4085·24	
4085·84	
4088·64	Faint.
4093·09	"
4097·49	"
4105·04	Strong.
4105·21	"
4109·76	Very strong.
4011·01	Very faint.
4112·16	"
4119·36	Fairly strong.
4120·46	Faint.
4121·52	"
4121·56	"
4123·82	Agrees.
4132·82	Faint.
4133·79	"
4145·87	
4147·42	
4151·92	
4153·57	May agree.
4155·42	Faint.
4156·79	
4164·72	Faint.
4166·72	"
4169·47	Agrees.
4172·12	
4175·72	Band.
4177·92	Very faint.
4179·92	Faint band.
4185·32	Very strong.
4190·00	"
4193·77	Very faint.
4198·72	
4199·22	May agree.
4202·12	"
4205·72	Very faint.
4206·92	Band.
4209·12	Very faint.
4214·92	"
4223·17	Faint on band,
4224·92	" "
4225·92	" "
4228·52	Band.
4236·67	"
4241·92	
4249·02	
4253·42	Very faint.
4266·32	Faint.
4271·22	"
4274·82	Very faint.
4277·90	Faint.
4279·90	Fairly strong.
4282·40	Faint.
4291·90	"
4303·80	Very faint.
4305·67	"
4309·87	Faint and sharp.
4312·72	" "
4315·52	" "
4317·20	Strong.
4319·50	"
4322·80	Faint, may agree.
4323·90	Very faint.
4325·90	Agrees.
4327·60	Very faint.
4328·42	"
4330·37	"
4331·20	"
4332·40	Sharp.
4336·77	
4345·52	Strong.
4347·47	Faint.
4347·94	Strong.
4349·30	"
4351·40	"
4353·70	"
4356·62	Faint.
4362·90	
4365·40	Faint.
4366·92	Strong.
4369·60	Faint.
4371·40	"
4379·70	"
4381·50	"
4385·30	Very faint.
4385·40	"
4386·50	Nebulous.
4396·30	Faint.
4401·22	
4415·00	Strong, agrees.
4417·17	" "
4421·00	Faint.
4426·00	"
4430·04	"
4431·90	Very broad dim band.
4434·27	Sharp.
4439·47	Broad dim band.
4443·00	" "
4447·09	Very strong.
4452·40	Sharp.
4456·00	Faint and sharp.
4459·90	Faint.
4465·40	Sharp.
4466·00	"
4468·02	Very faint.
4469·50	"
4472·90	"
4477·87	Broad and faint.
4481·87	Sharp.
4487·94	
4489·90	Faint.
4496·97	Sharp.
4498·95	Faint.
4503·05	Fairly strong.
4507·72	"
4511·85	Sharp.
4520·50	Strong, may agree.
4544·50	Fairly strong.
4565·97	Sharp.
4572·02	Sharp, agrees.
4577·50	Sharp.
4578·55	"
4582·32	"

4583·15	Very strong.
4587·45	Sharp.
4588·05	,,
4588·92	Very faint.
4589·40	,,
4590·00	,,
4590·95	Strong, may agree.
4592·00	
4592·95	Strong.
4596·20	,,
4601·37	Very strong.
4607·20	,,
4609·45	Sharp, may agree.
4612·75	Faint.
4614·05	Strong, agrees.
4621·42	Strong.
4630·73	Very strong.
4634·00	Sharp.
4638·90	Strong.
4640·75	Rather faint.
4641·90	Fairly strong.
4643·45	Strong.
4645·40	Faint.
4649·25	Strong.
4651·02	Fairly strong.
4654·10	Faint.
4654·85	,,
4655·90	Faint band.
4658·05	Very faint.
4659·60	,,
4665·70	Faint.
4667·55	,,
4671·65	,,
4672·30	,,
4673·30	Very faint.
4674·95	Faint, may agree.
4676·40	Faint, may agree.
4681·10	Very faint.
4682·40	,,
4687·15	,,
4688·80	,,
4691·40	,,
4694·15	Strong.
4695·15	Faint.
4696·70	Very faint.
4699·40	Broad and faint.
4700·40	Faint.
4701·65	,,
4703·02	Agrees.
4705·42	Fairly strong.
4710·20	,,
4712·87	Very faint.
4719·92	,,
4731·27	,,
4733·95	,,
4740·20	,,
4744·20	,,
4753·82	Sharp.
4760·07	,,
4763·82	,,
4771·82	,,
4775·07	,,
4782·62	Very strong.
4788·27	Very faint.
4791·32	Sharp, agrees.
4798·97	Very faint.
4800·82	,,
4802·37	Very strong.
4808·94	Very faint.
4810·02	Faint.
4811·92	,,
4813·52	,,
4816·60	Very strong.
4820·90	Faint.
4822·12	Faint.
4825·12	Faint, may agree.
4842·00	Faint but sharp.
4863·92	,, ,,
4877·70	Faint.
4878·80	Very faint.
4879·90	,,
4891·27	,,
4894·90	,,
4898·70	,,
4906·77	,,
4907·67	,,
4913·69	Sharp.
4915·12	Sharp, but faint.
4916·86	Sharp.
4936·86	Band.
4940·85	Sharp.
4945·01	,,
4945·81	,,
4950·21	,,
4951·41	Nebulous band.
4953·85	Sharp, agrees.
4955·16	
4960·16	
4969·85	
4972·85	
4979·90	
4983·06	Sharp, may agree.
4993·95	Faint.
4997·60	,,
4999·31	Agrees.
5001·55	Faint.
5011·06	Sharp, agrees.
5012·50	Faint.
5018·55	May agree.
5022·95	Faint, may agree.
5033·85	Very faint.

In regard to the accuracy that may be expected of the above positions, we feel sure that few of them are wrong by more than a tenth of a wave-length, and these are of the class "Very faint," or "Broad and nebulous." The better defined lines we believe to be correct to within less than the above amount. The method of comparison we have used admits of much greater accuracy than this, but the ill-defined character of the air-lines puts a limit to their accurate placing. Compared with Thalén's positions, they should be credited with ten times the accuracy at least. Some of Thalén's bands are resolved into two or more in our instrument.

Prof. John C. Draper projected his spectra upon a scale of wave-lengths by means of a stereopticon—a method which does not inspire confidence in his results, when we consider the distortion produced by projecting-lenses.

The scientific world seems largely to have accepted the wave-lengths of Ångström and Thalén as final. One eminent authority speaks of them as the "ne plus ultra" of spectroscopic accuracy; and any attempt to revise or correct them may be looked upon as presumptuous. However, we believe the time has arrived when the whole of Thalén's work on metallic spectra must be re-examined. It is safe to say that he has tabulated not more than one line in many metals where several exist, and his positions are occasionally wrong by as much as two wave-lengths.

As yet no approach to the accuracy with which the solar spectrum has been delineated has been attempted in metallic spectra—a remarkable fact, when we consider that the chief interest that attaches to the study of the solar spectrum is in its connexion with spectra of terrestrial elements.

The test of the existence of oxygen in the sun is the coincidence of the bright lines of the spectrum of oxygen with bright lines or with dark lines of the solar spectrum. If the bright lines of any metallic vapour formed in the electric arc or the electric spark coincide with the dark lines of the solar spectrum which is photographed directly above the spectrum of the metal on the same sensitive plate, the evidence is usually considered conclusive in regard to the existence of the metal in the sun. In the case of iron, where hundreds of lines of the metal coincide with dark lines in the solar spectrum, not only in exact position but in general grouping and character, the evidence cannot be doubted by any one who has carefully examined it. When a majority of the lines of any metal coincide with dark lines in the solar spectrum under high dispersion, not only in position but in grouping, while a few of the metal lines have no representatives in the solar spectrum, there is a probability that the corresponding lines wanting in the sun have been obliterated by superposed lines or bands of other metals. In our paper "On the Existence of Carbon in the Sun," we shall call attention to a case of such obliteration. It is probable, also, that the non-appearance of certain lines in the sun may be due to certain conditions of temperature. We shall discuss this point more fully in the paper on Carbon, above referred to.

The same remarks apply to the coincidence of the lines of any element with the supposed bright spaces in the sun. The value of the test of coincidence increases with the number of coincidences. If an element has only two or three lines, and these two or three agree in position with dark lines in the solar spectrum, the evidence of the existence of the element in the sun is not conclusive. It is supported, however, if there is any striking peculiarity in the lines of the element which is reproduced in the corresponding lines in the solar spectrum. Thus the nebulous character of the lines of magnesium is perfectly reproduced in the corresponding lines in the solar spectrum. The test of coincidence, therefore, requires primarily a normal spectrum, and the highest possible dispersion. The earlier observers were limited to instruments of small dispersion, and the entire number of lines observed in the solar spectrum was small compared with that given by the best modern apparatus. The chances for an apparent coincidence were therefore much greater, and evidence of a very misleading character could be obtained.

In Dr. H. Draper's published photograph, the coincidence of the greater part of the oxygen lines with bright bands in the solar spectrum is quite striking; and it is not a matter for surprise that he was led to conclude the connexion between the two spectra to be a physical one, and to announce the existence of oxygen in the sun as proved. Instances are not infrequent where instrumental imperfection or lack of power has led to results unsupported by later and more powerful research. Witness the spots of Venus of the older observers. Now when we apply to the spectra of the sun and oxygen a dispersion and definition that show the minute detail of each, the "bright bands" at once vanish, or no longer appear as such, and all the apparent connexion between them and the oxygen lines disappears also. The bright bands of Dr. H. Draper's spectrum are found to be occupied by numerous dark lines, of various degrees of intensity; but the hypothesis of Prof. J. C. Draper, that these are the true representatives of the oxygen lines, is rendered untenable by the lack of any systematic connexion between the two. It happens quite frequently that an oxygen line falls centrally upon the space between two dark lines of the solar spectrum, but not more frequently than we might expect as a matter of chance, when we consider the vast number of lines and spaces; and the fact that the spaces are no brighter than the surrounding background of the solar spectrum would not seem to permit of their interpretation as bright lines.

The subject of bright lines in the solar spectrum is one upon which men will probably differ, and we have sought information upon it. Of course there is no *à priori* reason why such bright lines should not exist, as they do in many stars; but we have photographed the sun's spectrum every day that the sun has shone for nearly five months, without finding a line that could with certainty be pronounced brighter than its neighbours; and it must be admitted that the photograph is the best of photometers in such a case.

In regard to the other three spectra of oxygen of Schuster we have nothing to say; but so far as concerns the spark spectrum in air and the solar spectrum from wave-lengths 3749·8 to 5033·85 we can safely affirm that there is no physical connexion between them.

XL. *On the Existence of Carbon in the Sun.*

By Professor JOHN TROWBRIDGE *and* C. C. HUTCHINS*.

FROM the presence of absorption-bands in the solar spectrum at high altitudes, Captain Abney has been led to believe in the existence of certain hydrocarbons between the earth and the sun; and Siemens's theory of the conservation of solar energy depends upon the supposed existence of carbon vapour in interplanetary space. It is not our purpose to discuss Abney's observations, or the truth of Siemens's hypothesis. We wish to call attention to the remarkable character of the carbon spectrum, formed by the Voltaic arc in air between carbon terminals; and to draw attention to the evidence presented by the juxtaposed solar spectrum of the existence of carbon in the sun.

In our early experiments the carbon terminals between which the Voltaic arc was formed were heated several hours, while a stream of chlorine gas was passed over them. This operation was not entirely successful in removing metallic impurities. Subsequently we discovered that the spectra of these impurities could be readily distinguished from the marked fluted carbon spectrum, and we therefore employed the ordinary compressed carbon sticks employed in electric lighting.

For our work the nicest adjustment of slit was necessary, in order that no displacement of spectrum lines could possibly occur when the carbon spectrum was photographed in juxtaposition with the solar spectrum. This was accomplished by the use of a slit, the jaws of which opened equally.

One of Rowland's concave gratings, of 21 feet 6 inches in curvature and 14,000 lines to the inch, was employed. In order to avoid any possible displacement of the photographic camera during the operation of photographing the carbon spectrum immediately below the solar spectrum, a drop-shutter was arranged directly in front of the sensitive plate, the movement of which was independent of any movement

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* From an advance proof from the Proceedings of the American Academy of Arts and Sciences, vol. xxiii. Communicated by Professor J. Trowbridge, of Harvard University, Cambridge, Mass., U.S.

of the camera. Preliminary experiments showed us the importance in this work of employing a spectroscope of great dispersion and of fine definition, giving also a normal spectrum. The use of a prism-spectroscope would undoubtedly have masked the phenomenon we have observed. For our purposes, therefore, Rowland's apparatus was peculiarly advantageous.

Our experiments led us to conclude that there is positive evidence in the solar spectrum of the existence of carbon in the sun. Before giving an account of our experiments in detail, a few observations may not be considered out of place.

One who studies the solar spectrum by itself, and who has had no experience in the formation and observation of metallic spectra, is apt to regard the dark lines in the solar spectrum as fixed in character and condition. A line which is seen by one observer, and not by another, is generally regarded as a terrestrial line formed by absorption in the earth's atmosphere. Certain lines are well known to be due to the terrestrial absorption, as can be easily proved by their appearance when the sun is observed at sunset, when the rays of light have to penetrate a greater thickness of the earth's atmosphere than at midday. The shifting layers of vapour in the sun's atmosphere also may, in certain cases, obliterate or strengthen certain lines of a metal. To understand this it is only necessary to extend the reasoning of the conservation of energy to the subject. It is a common lecture experiment to reverse the metallic lines by passing the rays of light produced by the vapour of the element through a layer of vapour colder than that of the source of the rays. The energy of the rays is thus absorbed in heating the colder layer. When the temperature of the vapour is increased, and becomes equal to that of the source, no reversal takes place. Thus, on the sun's surface the conditions for a reversal may be wanting at certain times, and faint lines may become bright. Their brightness may not be sufficient to affect the general illumination of the solar spectrum of which they form a part. Conditions may arise, moreover, in which the temperature of the reversing vapour may be called critical—at such a temperature that the faint reversal is sufficient to extinguish the bright line of a metal without producing a well-defined dark line. At certain epochs, also, the temperature of the vapour of any element in the sun may be higher than at other times; and certain lines may thus appear which are wanting when the temperature falls. One is forced to these conclusions in observing the conditions under which the varying character of metallic spectra are produced. For instance, we have caused the rays from iron vapour to traverse a long and dense layer of iron vapour, and have observed that the strength of the lines and the number of reversals have been largely increased. In another experiment, the lower carbon of the electric lamp we employed occupied the centre of an electro-magnet. This was accomplished by passing the carbon through a hollow iron cone, and surrounding the latter by layers of wire, through which the electric current employed in generating the light passed. In this case the electric arc was spread out at right angles to the pole of the magnet, into a fan-like, intensely hot flame, which roared loudly, and which rarefied, so to speak, the iron vapour between the carbon terminals. The strength of the lines and the number of reversals were diminished under this new condition.

Another phenomenon may happen. When an excess of the vapour of one metal floats over or is mixed with that of another, the lines of one metal are superimposed upon those of another in the solar spectrum, and the stronger spectrum of one element may easily obliterate the weaker spectrum of another. Thus we have succeeded in completely obliterating the fluted spectrum of carbon in the green and blue, by photographing upon it the spectrum of iron, of nickel, and of cerium. A species of composite photograph was thus obtained. It is possible that in the future Galton's ingenious method of composite photography may be applied to the solar spectrum ; and by a judicious selection of photographs of the elements, a composite photograph may be obtained which will closely resemble portions of the solar spectrum, and will enable us to judge of the composition of the reversing layers of the sun.

To the varying conditions which we have thus outlined are due, we believe, the disappearance in the sun's spectrum of the marked fluted spectrum of carbon in the green and blue portions.

A careful examination of the fluted spectrum of carbon, however, with the juxtaposed solar spectrum, discloses a remarkable fact : while traces of obliteration of the evidence of carbon vapour are seen, yet the general character of the lines in the solar spectrum immediately juxtaposed with the fluted spectrum of carbon near H lead us to believe that there is unmistakable evidence of the existence of carbon vapour in the sun. When the arrangement of the fine lines of the spectrum of carbon is plotted as a curve, and that of the dark lines in the solar spectrum immediately above the carbon spectrum is also plotted, the two curves have a remarkable similarity in character, running with a slight convexity toward one axis.

In the first fluting at wave-length 3883·7 within the limit of ten wave-lengths, over 28 of the spaces between the fine bright lines of the flutings coincide with dark lines immediately in juxtaposition in the solar spectrum. When we consider that the progressive arrangement of these lines is exactly the same both in the spectrum of carbon and that of the sun, we cannot believe that this coincidence is the result of chance. On examining the spectrum of carbon in the region near H still further, a remarkable number of coincidences of the spaces between the bright lines of the carbon spectrum with dark lines in the solar spectrum will be observed. We are led, therefore, to conclude that the fluted spectrum of carbon is an example of the reversal of the lines of a vapour in its own vapour. Fluted spectra occur at comparatively low temperatures. When carbon is ignited, we have at first a continuous spectrum. When the temperature increases and the carbon is volatilized, fluted spectra occur, which consist of interruptions of the continuous spectrum by fine line reversals occurring in harmonic order. The same phenomenon can be observed in the spectrum of iron lines: through the centre of an iron line, when a sufficient amount of iron vapour surrounds the Voltaic arc in which iron is volatilized, reversal lines are always seen. Now if the iron lines were arranged in regular order, the reversals would also be in like regular order, and would coincide with similar reversals in the solar spectrum. Assuming the conditions at the sun's surface to be the same as those we have in the Voltaic arc when carbon is volatilized, the character of the carbon spectrum should exactly agree with the character of the solar spectrum juxtaposed. This is found to be true to a remarkable degree in comparing portions of the solar spectrum with portions of the fluted spectrum of carbon beginning at wave-length 3883·7.

Our hypothesis leads us to conclude, that, at the point of the sun's atmosphere where carbon is volatilized, so as to produce the peculiar arrangement of reversals observed, the temperature of the sun approximates to that of the Voltaic arc.

XLI. *On Expansion and Contraction with Rise and Fall of Temperature in Wires under Elongating Stress.* *By* J. T. Bottomley, *M.A., F.R.S.E., F.C.S.*[*]

[Plate IX.]

AT the Glasgow Meeting of the British Association in 1876 a committee was appointed for the purpose of initiating secular experiments on the elasticity of wires; the place of the experiments being the tower of the University of Glasgow. From time to time reports have been presented to the British Association on the subject of these wires; but a few words respecting them will help to introduce and to explain the bearings of the present short communication.

In order to test the permanence of wires under long continued pulling stress, three pairs of wires—gold, platinum, and palladium, were hung up, ten years ago, in a great iron tube about 15 metres long, erected for the purpose. These pairs are loaded, at the lower extremity, one with a heavy load about half the breaking weight, and the other with a light load less than one tenth of the breaking-weight of the wire; and from time to time comparisons as to length are made between the two wires of each pair, a special kathetometer and proper marks on the wires being used for this purpose.

The observations on the suspended wires are made at different times of the year; and it is not possible to choose times for the observations such that the temperature shall be always the same; nor is it even possible to be sure that the temperature from top to bottom of the great tube is approximately uniform, though probably the two wires of each pair are almost accurately at equal temperatures, compared from point to point along their lengths.

It is consequently desirable, for the purpose of being able to interpret the meaning of these observations, to ascertain whether there is any perceptible difference in the expansibility of the same wire under small and great elongating stresses. The experiments described in this paper were undertaken with this object.

A tube of tin-plate (Plate IX. fig. 1), about 5 centimetres in internal diameter and 6 metres long, has been erected vertically, being supported with the help of brackets (as shown in the figure) in a convenient position, close to a staircase by means of which access can easily be had to the top of the tube. Within this tube two wires, cut from the same hank,

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[*] Communicated by the Author, having been read before Section A of the British Association at its recent Meeting in Manchester.

are hung side by side from the same top support; the support being a brass plate with small holes drilled in it through which the wires are drawn and into which they are soldered. The brass plate just mentioned closes the top of the tube loosely; and it is closed at the bottom also, except for small apertures through which the wires pass perfectly freely. A plentiful supply of steam from the University heating-apparatus enables me to raise the temperature of the whole tube in two or three minutes to the condensing-point of steam; and a tube (shown in the figure) carries away the condensed water.

The wires which I have used up to the present time have been fine copper wires, about No. 34, ·008 inch in diameter (similar in size to the wires used in the secular experiments in the tower); and, as in secular wires, one of them carries about half its breaking-weight, the other something less than one tenth. The weights are rectangles of thin lead; and each has little feet of brass wire which rest very lightly against opposite sides of a vertical glass plate (marked *p p* in fig. 1). This glass plate prevents the lead rectangles from turning round or getting into torsional oscillation.

Before describing the measuring-apparatus, it will be convenient for me to say a few words as to the requirements, and as to a difficulty which is met with in carrying out the experiments. The wires having been suspended as described above and their weights attached to the lower extremities, a scale divided into half-millimetres was attached to one of them (the lighter), and an index or pointer moving over the scale was attached to the other. To aid in reading, a lens or Quincke microscope-kathetometer (shown in the figure) could be used, though at the commencement this was unnecessary. Thermometers were also inserted into holes in the tube at different heights.

As soon as the steam was allowed to enter the tube both wires were seen to extend, and the heavily weighted wire extended much more than the lightly weighted wire. The steam was turned off after about ten minutes, and the tube and wires allowed to cool. In an hour or more, when it was quite certain that everything had returned to the initial temperature, the marks were examined again, and it was found that the wires had *not* returned to their original length; but had suffered a considerable amount of permanent elongation, the heavily weighted wire having suffered more elongation than the other. This was to be expected; but I was not prepared to find that, when the heating was repeated again and again, the *permanent* elongation steadily increased, though each time by a perceptibly diminishing amount.

As soon as this was found I determined to add temporarily to each of the stretching weights a small extra weight, and to commence a hardening process which should ultimately give me the wires in a condition more stable and more suitable for the proposed experiments. Accordingly two extra weights were made, each about one sixth of the permanent stretching-weight; so that the lightly loaded wire carried during this process $\frac{1}{10}+\frac{1}{60}$ of the breaking-weight, and the heavily loaded wire $\frac{1}{2}+\frac{1}{12}$ of the breaking-weight. These extra weights were removed after the hardening process was complete. I then arranged to have the steam turned on so as to heat up the tube and the wires two or three times a day, an interval of at least one or two hours being given between each heating to allow the tube to cool again perfectly. At the end of each operation the wires were compared. The behaviour of the wires during the hardening process surprised me greatly. I was quite prepared, after the preliminary experiments, to find that a good many heatings and coolings might be required before a permanent condition was reached; but it required over two months and more than one hundred heatings and coolings before anything like permanence was reached in the first pair of wires I tried. At the end of this time, to my great disappointment, the heavily weighted wire broke down one night, having been left perfectly safe in the afternoon*.

A fresh pair of wires was therefore suspended about the end of February last, the greatest care being taken to avoid any initial disturbance of the wires, and to see that they were free from visible kinks or bends of any kind; and on March 2 the hardening process was commenced on these wires. As I judged from the first attempt that the desired condition had been very nearly reached before the breakdown took place, I considered it best not to reduce the working weight, but rather to accomplish what was required by taking extreme care to avoid any needless disturbance†. In the middle of April, after about 120 heatings and coolings, the wires seemed to have assumed a nearly permanent condition, and the extra weights were removed. The measuring-apparatus, to be described immediately, had been already attached; and during ten days, from April 21 till the end of

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* I think it is probable that a copper wire hung up with perhaps two thirds or three quarters of its ordinary breaking-weight attached to the lower end and alternately heated and cooled would break down; and I intend to make some trials of this kind on copper wire and wires of other metals.

† I must here express my indebtedness to Mr. Thomas A. B. Carver, the student in charge of these experiments, now Thomson Experimental Scholar in the University of Glasgow. Without such perseverance, patience, and careful manipulation as he brought to bear, these experiments could not have been carried out to a successful issue.

the month, satisfactory measurements were made as to the relative expansion of the heavily and lightly loaded wires. At the end of the month of April I ceased to have the necessary supply of steam, and the experiments therefore came, for the time, to an end. I have therefore erected a wooden protection round the lower end of the tube, and the wires are left hanging exactly as they were, and will remain so till the middle of October, when I propose to continue the experiments.

It now remains for me to describe the arrangement for the final comparison between the expansions of the lightly loaded and heavily loaded wires. This consists of three parts: a multiplying lever, a fixed scale, and a movable scale (see figs. 1, 2, & 3). The multiplying-lever is one of Sir William Thomson's aluminium levers, used in his milliampere balances. The lever and supports were in fact taken from a disused trial instrument, and the knife-edges and supports were inverted and adapted to the purpose in hand. One of the supports (a) of the lever is attached to the lightly loaded wire, and the other (b) to the heavily loaded wire (Pl. IX. figs. 1 & 2), and the free end of the lever moves over the movable circular scale ($m\,n$, fig. 1). Supposing the two wires to expand and contract equally, the lever would simply be carried down and up parallel to itself. But if both wires expand and contract together, but one more than the other, the lever will be carried down and up bodily, and will be tilted as well.

The method which I have adopted up to the present of comparing the two wires has been to take the lightly loaded wire as the standard; and, using the fixed scale $s\,t$ and a Quincke microscope-kathetometer, to lower or raise the zero of the movable scale $m\,n$ by means of a screw, $h\,h'$ (shown enlarged, fig. 3), by as much as the knife-edge of the support a has been lowered or raised by the expansion or contraction of the lightly loaded wire which bears it. The reading of the pointer on the movable scale then gives the difference in expansion or contraction between the two wires.

I am not yet prepared to give a definite numerical statement: and as I have so far only had a single satisfactory set of experiments, I feel that even such results as I have obtained require confirmation. There seems, however, to be no doubt that there is a measurable difference between the expansion by heat of the heavily loaded and lightly loaded wires, the expansion of the heavily loaded wire being the greater.

The corresponding result for gold, platinum, and palladium wires must be carefully determined by special experiments, and must be taken into account in the deductions to be made from the observations on the secular wires referred to in the opening paragraph of this paper. I hope, therefore, that though these experiments are far from complete, even this preliminary account of them may not be without interest to the Mathematical and Physical Section of the British Association.

XLII. *Integral Weights in Chemistry.*
By T. Sterry Hunt, *LL.D., F.R.S.*[*]

1. In approaching the discussion of integral weights in Chemistry, it is important to consider in the first place what we have elsewhere noticed as the distinction between dynamical and chemical phenomena. The passing alterations in the volume of any species resulting from variations of temperature are dynamical; but beyond certain limits these produce more or less permanent changes of state, which are chemical changes. Such are the condensation of gases or vapours to liquids and solids, the vaporization of these, the fusion of solids, and also the transformations alike of gaseous, liquid, and solid species, whether elemental or complex, which are comprehended under the general head of chemical metamorphosis. These changes, as is well known, are influenced by pressure, which favours or retards them, as the case may be. The phenomenon of elasticity in gases and vapours is apparently a manifestation of chemical change or metamorphosis, giving rise to new and unstaple species.

2. All changes of state in matter may, as we have elsewhere shown, be included under the two heads of (1) metamorphosis, which includes homogeneous integration and disintegration, and (2) metagenesis, which includes heterogeneous integration and disintegration. All such changes are subordinated to simple relations of measure, of number, and of weight, as is apparent in definite and multiple proportions, and in progressive series. These relations are best exemplified in the case of gases and vapours, where the weight at 0° temperature and 760 millim. pressure of a given volume of the lightest known species, namely hydrogen gas, is made the unit. The weight of a like volume of any other gas or vapour reduced to the same standard temperature and pressure, and compared with this unit, is sometimes designated its equivalent weight. Inasmuch, however, as in chemical union or integration of gases and vapours the most frequent cases are in the proportion of one to one or one to two volumes, the condensation in

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[*] Communicated by the Author, having been read before the Meeting of the British Association for the Advancement of Science at Manchester, September 1, 1887.

each being in a simple relation of volumes, the weight of this unit-volume of any species has also been called its proportional weight or combining weight.

These relative weights of equivalent volumes become, in the language of the atomic hypothesis, atomic or molecular weights. Since, however, a gas or vapour, whether it be of a so-called elemental species, or one known to be formed by homogeneous or by heterogeneous integration, is a unit or integer the specific gravity of which, at standard temperature and pressure, varies directly as its equivalent or combining weight; and, further, since this weight is nothing else than the specific gravity of the gas or vapour, hydrogen being unity ($H_2=2$), it may, dispensing with all hypothesis, be designated the weight of the integer, or, in other words, as the integral weight of the gaseous or vaporous species.

3. From the time of Gay-Lussac, the fact that all chemical reactions, so far as regards gaseous or vaporous species, are subordinated to a simple relation of volumes, and that the specific gravity of such species is but a function of the equivalent or integral weight, has been well understood; the volume at standard temperature and pressure being invariable. The attempts made to establish a similar relation between the specific gravity and the integral weights of liquid and solid species have, however, hitherto been unsuccessful for the reason that the law of volumes was lost sight of, and the volume for such species, instead of being regarded, as in the case of gaseous bodies, as a constant unit, was assumed to be an arbitrary and a variable quantity; the so-called atomic or molecular volume being conceived as conditioned, not only by crystalline form, but by various other circumstances. That the law of volumes, as proclaimed by Gay-Lussac, applies not only to gaseous species, but to liquids and solids also, was, however, maintained by the present writer in 1853, "as leading the way to a correct understanding of the equivalent volumes of the latter;" although the constantly affirmed tradition of the subordination of the so-called molecular volume of solid species to the variations of crystalline form prevented him, for many years afterward, from arriving at the simple solution of the problem embodied in the assertion made at the same time, that "the doctrine of chemical equivalents is that of the equivalency of volumes;" or, in other words, that the law of volumes is universal, and that for all species—solid, liquid, or gaseous—the volume, under proper conditions of pressure, is the same.

4. Hydrogen, as the lightest known species, is the unit of integral weight in Chemistry; and a litre of this gas at 0° and

760 millim. being assumed as a unit of volume for all species, the weight of a litre of any other gas or vapour at the standard temperature and pressure is its integral weight. In like manner the integral weight of a liquid species is the weight of the same volume at its boiling-point (as indicated in 1853) under a pressure of 760 millim.; while for any solid species it is the weight of the same volume at the highest temperature which that species can sustain without undergoing a change of state. In other words, the weight of liquids should be compared with that of the gaseous unit of hydrogen at the temperature at which they are generated by the metamorphosis, through condensation, of the corresponding gaseous species. In like manner the weight of solids should theoretically be determined at the temperature at which they are generated by the metamorphosis of gaseous or liquid species. The weights thus obtained for equal volumes of the various liquid and solid species, as well as for the gaseous species, are evidently the specific gravities of these species; that of hydrogen at the standard temperature and pressure being unity, ($H_2=2$). They are at the same time the integral weights of the species compared.

5. The advantages of this natural unit of specific gravity over the arbitrary ones hitherto adopted are evident. That which, for motives of convenience, has hitherto been employed for gases and vapours, namely the weight of dry atmospheric air at standard temperature and pressure, is the weight of a mixture of gases and not of a chemical integer; while water, adopted as the unit for liquids and for solids, is, on the contrary, a liquid integer, condensed by refrigeration from 100° (the temperature of its production at 760 millim. pressure) either to 15° or to 4°, its maximum density. The specific weight of hydrogen at standard temperature and pressure is the truly scientific unit of specific gravity for all bodies, whether gaseous, liquid, or solid; the integral weight of this unit entering as a factor into the specific gravities thus calculated, and causing these to represent the equivalent or integral weights of the species compared.

6. It is obvious that the integral weight of water must in like manner enter into the specific gravities of liquids and solids for which this species is assumed as unity; and hence the importance of determining as near as possible its integral weight. The ratio between the weights of equal volumes of water-vapour at 100° and 760 millim., and of water at 0°, as deduced from direct determinations of the density of steam compared with air, and of air compared with water, has been found to be 1 : 1698. If now we attempt to fix this ratio by

calculating the specific gravity of steam from that of hydrogen gas at 100°, as determined by Regnault, we find (assuming $O=16$) for the weight of a litre of steam at 100° and 760 millim., 0·590148 grm.; and dividing by this number the weight of a litre of water at 4° (=1000 grms.) get 1694·49. If, however, we take $O=15·9633$, as deduced by Stas, we get instead, 0·58894 for the weight of a litre of steam; and with this as the divisor find a ratio 1 : 1697·97. Correcting this for the density of water at 0°, we have 1 : 1697·74; an approximation to the experimental figure which shows the revised integral weight of oxygen (15·9633) to be very near the truth. But to fix the integral weight of water, we must consider its density at 100°, the temperature of its formation under a pressure of 760 millim., which, being 0·95878, gives for the number of volumes of steam at the above temperature and pressure, which are condensed into one volume of water at the same temperature, 1628·04. The formula of water is thus 1628 (H_2O), of which the coefficient multiplied by 17·9633 gives, for the integral weight of water, 29244. In close confirmation of this figure, if we take the ordinarily received weight for the litre of hydrogen at 0° and 760 millim., namely 0·896 grm., we find for the litre of water-vapour ($H_2O=17·9633$) at 100° and 760 millim., 0·589088 grm. Comparing this with the weight of a litre of water at 100°, we have

$$0·589088 : 958·78 :: 2 : x=29236·4.$$

This value of x, giving the relation of weight between equal volumes of H_2O and of liquid water, both at 100°, is very near that calculated above for 1628 (H_2O) = 29244, which we may regard as the integral weight of water.

7. From this value thus deduced for water by comparison with hydrogen, we proceed to calculate for liquid and solid species their integral weights. But the integral weight of water, the received unit of specific gravity for such species, is that of a body a litre of which, though weighing 1000 grm. at 4°, weighs but 958·78 grm. at 100°; so that, in calculating specific gravities by comparing the weight of bodies with that of an equal volume of water, it is with water at the lesser density that the comparison should be made. Since most liquid and solid species expand, like water, by heat, it is theoretically desirable to take the volume of these at the highest temperature which they can sustain without change of state. This point in many cases is hard to fix. The ready transformation, by expansion, of arragonite into calcite by heat; the condensation, on the contrary, which goes on when silver iodide is heated from 0° to 116°; that of Rose's fusible alloy

between 59° and its melting-point; and the remarkable condensation effected, even by a gentle heat, when the grey brittle species of tin passes into the ordinary white malleable form (not less than the fact that quartz and many native silicates at temperatures below their melting-points acquire that increased solubility which they possess, with augmented value, after fusion),—all tend to show that chemical changes may take place by heat in other solid species where hitherto unsuspected. They show moreover the uncertainty which attends any attempt to fix by calculation the augmentation of volume in a solid species by heat beyond the limits within which experiment has determined the rate of expansion.

8. It is clear that if a species could, without chemical change, be heated to a point at which its augmentation of volume from 4° would be just equal to that of water from 4° to its boiling-point at 760 millim., its specific gravity as compared with this liquid at 100° would be the same as that found for the same species at 4° compared with water at the same temperature. The cubical expansion of water for each degree between 4° and 100° is approximately ·00043, while that for iron from 0° to 300° is given as ·000044, and that for quartz from 0° to 100° at ·000040. The coefficient of expansion for these bodies being thus about one tenth that of water, it would be necessary, in order to attain an augmentation comparable to that of water at 100°, to raise them to 1000° and upward, or to temperatures much above those required to produce chemical changes in quartz and in most native silicates. The coefficient of cubical expansion of these last for each degree between 0° and 100° is moreover much less than that for iron and for quartz, varying in general from ·000020 to ·000028. The latter rate of expansion, if constant to 500° (even below which many native species are chemically changed), would reduce the specific gravity of the species at that temperature only 0·014; and up to 1000° only 0·028. The differences between these numbers and 0·043, which represents the cubical expansion of water from 4° to 100°, thus mark the extent of the errors involved in the determination of the specific gravities of such species with water at 4°. The imperfections and impurities of most natural and artificial crystalline species introduce errors not less considerable in the determinations of their specific gravity. The best figures obtained for such species involve in most cases possible deviations, in the one or the other direction, as great as those due to the different rates of expansion of water and these species; so that we may take the approxi-

mative specific gravities got with water at 4° (or better at 15°) as an available basis for fixing the integral weights of most solid species.

9. We are thus led to the doctrine of high integral weights and of polymerism for liquid and solid species, suggested by Favre and Silbermann in 1847 from their thermo-chemical studies, and by Graham in 1849 from his researches in diffusion. In 1853 this view of polymerism was further discussed by the present writer, who then proposed for the carbon spars, regarded as polycarbonates, provisional integral weights, varying with their densities from about 1500 to 2500. The subject has much more recently engaged the attention of Guthrie, of Spencer Pickering, and especially of Louis Henry in his inquiry into the Polymerization of Metallic Oxyds*. Studies of the ammonio-cobalt bases, the formulas of which require integral weights of not less than from 500 to 2500, have thrown further light upon the subject ; and especially the late researches of Wolcott Gibbs on the salts of what he has designated "The Complex Inorganic Acids," among which he has made known polytungstates the simplest formulas of which lead to integral weights of not less than 5000, and in one case of 20058. The high numbers thus deduced for the last two classes of bodies represent, however, the simplest possible integral weights, like that for water-vapour ; while the species themselves, as known to us, are derived from these theoretical species by what is called polymerization, or, more correctly, by homogeneous integration, being, like water, and like ice, examples of metamorphosis by condensation.

10. As we calculate the integral weights of all gaseous species by a comparison of their densities with that of hydrogen gas, so we may calculate those of liquid and solid species, either by comparing their densities directly with that of hydrogen or else with that of water at 100° ; remembering that this has an integral weight of 29244, and that these densities are subject to corrections for expansion, as already set forth. The integral weights of these species being thus known, and also those of the simpler species from which they are formed by homogeneous integration, it is obvious that by dividing the first by the second we get the coefficient of condensation. Thus, calcium carbonate, $CCaO_3 = 99{\cdot}89$; and as

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* This paper, which in an English translation is published in the Philosophical Magazine for August 1885, first appeared in 1879 as "Etudes de Chimie Moléculaire, 1re partie, les Oxydes Métalliques," in the *Annales de la Société Scientifique de Bruxelles.*

calcite with a specific gravity of 2·7536 corresponds to an integral weight of 79912, we find the coefficient =800 ; since 99·89 : 79912 : : 1 : 800. Inverting this proportion we get 79912 : 99·89 : : 1 : ·00125, which is the reciprocal of the coefficient, since 800 × ·00125 = 1·00000. Calcite with the above density is 800 ($CCaO_3$), while water is 1628 (H_2O), for 17·9633 : 29244 : : 1 : 1628, the reciprocal of its coefficient being ·00061425.

11. The weight of water-vapour is fixed by experiment; but for calcium carbonate, which is not known in a gaseous form, the theoretical integral weight (generally designated by p) is deduced from the simplest admissible formula therefor, and is a number which, as we have seen, corresponds to the specific gravity of such a hypothetical carbonate, hydrogen gas being unity. Representing by d the specific gravity of the condensed species, calcite, on the same hydrogen basis (water = 29244), we have for calcite, sp. gr. 2·7356, by the above given proportions, its coefficient of condensation, and also the reciprocal thereof, in terms of hydrogen.

In inquiries into the so-called molecular or atomic volume of liquid and solid species from the time of Leroyer and Dumas, while using the same symbols, and making p, as above, to represent the specific gravity on the hydrogen basis, d has been taken on the basis of water as unity (1 = 29244), so that having employed the ordinary formula for molecular volume, $p \div d = v$, we multiply the value of v thus obtained by the coefficient of condensation and get, not 1, but 29244. Otherwise, dividing the value of v by 29244, we obtain the reciprocal of the coefficient, as before, upon the hydrogen basis. Chemical integration being effected, not by juxtaposition of molecules, but by identification of volume, the so-called molecular volume of a given liquid or solid species is thus the reciprocal of its coefficient of condensation.

For a further discussion of this question of integral weights in some of its relations, the reader is referred to the author's lately published volume, entitled 'A New Basis for Chemistry;' and also to a still more recent essay "On Chemical Integration," read before the National Academy of Sciences at Washington, in April, and published in the 'American Journal of Science' for August 1887.

XLIII. *On the Existence of certain Elements, together with the Discovery of Platinum, in the Sun.* By C. C. Hutchins *and* E. L. Holden*.

LATE in the fall of 1886 it was decided by the writers, who were then at work in the Physical Laboratory of Harvard University, to attempt a revision of some of the previous work in regard to the chemical constitution of the sun, as well as to discover, if possible, new facts bearing on the same subject. For the purpose of this investigation a magnificent diffraction-grating, made by Professor Rowland, of Baltimore, was kindly placed at our disposal by Professor John Trowbridge, under whose supervision and direction the subsequent work has been done.

After some delay caused by the mounting of the grating and its attachments, work was begun early in January 1887, but, owing to bad weather and other hindrances, was not regularly and systematically prosecuted till somewhat later.

The grating used is of speculum metal with a ruled surface measuring 6 inches by 2, having 14,438 lines to the inch. It is concave, its radius of curvature being 21½ feet, and is mounted according to Professor Rowland's method. Suffice it to say, that the method is such that, by simply rolling the camera along an iron track, it passes not only from one part of the spectrum to another, but also to the spectra of different orders, at the will of the operator. As the distances on this track are proportional to the relative wave-lengths of the lines that fall successively on a given point in the camera, it is easy, by means of a suitable scale of equal parts placed beside the track, to set the centre of the photographic plate instantly within a single wave-length of any given line in the spectrum.

And here let us parenthetically state that all our wave-lengths are those given by Professor Rowland's photographic map of the solar spectrum, the position of every line referred to being carefully identified upon the map, and its absolute wave-length thus determined. Although some of the negatives contain many lines too faint to show on the map, yet we feel confident that our numbers correspond in all cases to those of the map within one tenth of a wave-length.

The light is brought into the room by means of a *porte lumière*, and then sent through the slit after total reflexion

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* From an advance proof from the Proceedings of the American Academy of Arts and Sciences, vol. xxiii. Communicated by Professor J. Trowbridge, of Harvard University, Cambridge, Mass., U.S.

by a right-angled prism. Before striking the prism it passes through a cylindrical lens, which condenses it to a band of light about 2 inches long and $\frac{1}{8}$ inch wide. The jaws of the slit move equally in opposite directions, so that, however widely they may be opened, no lateral displacement of lines can result from this cause.

Directly in front of the slit is placed a large tin lantern containing an electric lamp; the image of the arc can be brought exactly upon the slit by means of an adjustable lens in the front of the lantern. In the lower carbon of the lamp is made a cup-shaped cavity, which is filled with the substance a spectrum of which is desired. It is not at all necessary that this be in the form of a metal, for any ordinary compound is at once reduced by the intense heat and the presence of carbon vapour to the metallic state.

The plan of working has been as follows. The apparatus being arranged as described, the sunlight is admitted and the desired portion of solar spectrum photographed upon the upper half of the plate; then the sunlight is excluded by a shutter, and the image of the electric arc containing the proper metal is allowed to fall upon the slit, and its spectrum photographed on the lower half of the plate. (Most of the plates used were those made by the M. A. Seed Co., and were cut to the size of 8 inches by 2. The most sensitive plates were obtained, and even then we found the required time of exposure for some parts of the spectrum inconveniently long.)

In order to effect the exposure of either half of the plate at will, we placed directly in front of the camera an opaque screen, in which was a rectangular opening one half the size of the plate. By turning a handle, this screen is raised or lowered without the slightest disturbance of camera or plate. The metallic spectrum, being thus photographed immediately below the solar spectrum, can be compared with it at leisure.

These spectra are then examined with the aid of a glass magnifying about ten diameters, and any coincidences between solar and metallic lines carefully noted according to their wave-lengths. In order to eliminate any personal error, they are examined by both observers separately, and their results afterwards compared.

To eliminate errors arising from suspected impurities of materials, as also from the impurities known to exist in the carbons employed, we took what we called "comparison photographs." For these, we placed in the carbon cup a portion of the substances known or suspected to be present as impurities in our metal, and then photographed the spectrum thus given on the upper half of the plate: a piece of the metal under experiment was then placed in the lamp, and the spectrum photographed on the lower part of the plate. Any lines due to impurities would then extend entirely across the plate, while those of the pure metal would extend only half-way. In addition to this precaution we consulted all accessible tables and plates as to the position of known lines of metallic spectra, and also compared together all our photographs of the same region. If all of these tests left any doubt as to the origin of a given line, it was at once subjected to special investigation until all doubt was removed.

The dispersion given by the apparatus in the order of spectrum in which we worked is such that a single wave-length occupies on the negative a space of 1·12 mm. This makes the distance between the lines D_1 and D_2 6·7 mm., while the length of spectrum from A to H is about 4·1 m. With so great dispersion it would hardly be possible to mistake the position of a line by any very considerable amount, or to confound neighbouring lines belonging to different metals.

For reasons readily apparent, it was found so difficult to photograph under high dispersive power those parts of the spectrum not lying between wave-length 3600 and wave-length 5000, that our photographic work was done chiefly within those limits. It was, however, supplemented in many cases by eye observations in other portions of the spectrum.

We are convinced that there is much in the whole matter of coincidences of metallic and solar lines that needs re-examination; that something more than the mere coincidence of two or three lines out of many is necessary to establish even the probability of the presence of a metal in the sun. With the best instruments the violet portion of the solar spectrum is found to be so thickly set with fine lines that, if a metallic line were projected upon it at random, in many places the chances for a coincidence would be even, and coincidences could not fail to occur in the cases of such metals as cerium and vanadium, which give hundreds of lines in the arc.

Moreover, a high dispersion shows that very few lines of metals are simple and short, but, on the contrary, winged and nebulous, and complicated by a great variety of reversal phenomena. A "line" is sometimes half an inch wide on the photographic plate, or it may be split into ten by reversals.

At first we believed that these reversals were due to defects in the ruling of the grating, but we are convinced that they are true phenomena from the following experiments:—(1) The wings continue when various portions of the grating are covered. (2) They are the same in three successive orders of spectra. (3) They are very different in different metals, and in some are not seen at all. (4) We arranged a flat grating, with collimator and projecting lens, each of 5-feet focus, and found that with this apparatus the same phenomena appeared.

On pages 87 and 88 of 'The Sun' Professor Young gives a list of elements in the sun according to the best authorities, which is followed by a list of doubtful elements. Some of these we have examined with the following results:—

Cadmium.—The coincidence of the two lines given by Lockyer at wave-lengths 4677 and 4799 is perfect. These are the only cadmium-lines near, and sun-lines in the vicinity are not numerous.

Lead.—The evidence for lead, due to Lockyer, is based upon three lines at 4019·7, 4058·2, and 4061·8. We have photographed these lines with the sun many times. They are broad and nebulous, and often several times reversed. Lines in solar spectrum numerous and faint. 4019·7 and 4058·2 certainly do not coincide. 4061·8 is very difficult to pronounce upon; it may coincide.

Cerium, Molybdenum, Uranium, and *Vanadium.*—These four metals may be classed together. Lockyer finds four coincidences each for molybdenum and vanadium, three for uranium, and two for cerium. The arc-spectrum of each is characterized by great complexity and vast numbers of lines. So numerous are the lines, in fact, that often on the photographs the total space occupied by them is greater than the space not so occupied. A plate 10 inches long may contain a thousand or so. Evidently coincidences between these and solar lines cannot fail to occur as matters of chance, and therefore prove nothing. One can easily count a hundred or so such coincidences without the slightest conviction that the connexion is other than fortuitous. Of course all this is nothing against the probability of these metals being in the sun; but at the same time those peculiarities of grouping, strength of lines, and other characteristics which occur in the case of iron and other spectra, and which alone can serve as evidence in such cases, are conspicuously absent.

Among the metals whose existence in the solar atmosphere has seemed probable, we have examined the following:—

Bismuth.—The line of the above metal at 4722·9, the only line of bismuth in the arc in that whole region, coincides perfectly with the more refrangible of a very faint pair of solar lines.

Tin.—The solitary tin-line at 4525, thought by Lockyer to coincide, falls directly between two fine lines in the solar spectrum.

Silver.—Lockyer mentions a certain possibility of silver in the solar atmosphere from the apparent agreement of two of its nebulous lines with solar lines. One of these we have never been able to find in the course of many photographs of the region in which it is given by him.

We find seven lines of silver between 4000 and 4900. Of these seven, three are what Thalén calls nebulous; so broad and hazy that their true positions cannot be determined with much accuracy. These lie at about 4055·5, 4063·6, and 4212. A fourth line at 4023 is of the same general character, but has a sharp reversal which agrees with a solar line. The remaining three lines are represented in the sun, and are given by Thalén in the spark-spectrum of the metal.

4476·2. Very strong line; nebulous on lower edge. Sun-line strong. (Thalén, 4475.)

4668. Strong, solitary line. (Thalén, 4666·5.)

4874·3. Fairly strong. (Thalén, 4874.)

Thus, between the limits given above, every line of silver, as far as can be determined, coincides with a solar line.

Potassium.—We could find but two lines of potassium, the same that were examined by Lockyer, 4044·5 and 4048·35. Each line is reversed four times, which increases the difficulty of locating them exactly. 4048·35 seems to agree with a solar line. The solar line near 4044·5 is very faint, and it is next to impossible to decide the question of an agreement.

Lithium.—The blue line of lithium presents a curious case. The very broad and nebulous line has a rather sharp reversal near the centre, and somewhat toward the lower edge a broader and less clearly defined reversal. Both these reversals agree with solar lines at 4602·5 and 4603·2. It is possible that one of the reversals may be due to the presence of some other substance, say calcium; but if that were true, it would seem that both reversals would be nearly, if not quite, obliterated. Further experiment may clear the matter up. 4603·2 is given to iron by Thalén.

Platinum.—As far as we can learn, no evidence has hitherto been offered to show the occurrence of this metal in the solar atmosphere. We were somewhat surprised therefore upon meeting with coincidences. Between 4250 and 4950 we find 64 lines of platinum, sixteen of which agree with solar lines. The latter are at the following places:—

4291·10
4392·00 (Thalén 4389·4)
4430·40
4435·20
4440·70
4445·75 (Thalén 4442·0)
4448·05
4455·00
4481·85
4552·80 (Thalén 4551·8)
4560·30
4580·80
4852·90 (Thalén 4851·5)
4857·70
4899·00
4932·40

We have taken all possible care to make this statement accurate, and to admit no lines about which there seemed to be any question. There are seven other lines not included in the list, the probability of agreement of which is at least as good as that upon which potassium is admitted.

In all these experiments everything has been done to bring out and show upon the photograph as much as possible. The lamp, constructed for the purpose and fed by a powerful dynamo, gave an arc from a half to three fourths of an inch long, and burned with a long flame and so intense a heat that it could be worked for but a few minutes at a time. Anyone who has carried out a series of experiments like this is alone competent to appreciate the great labour and the endless difficulties and perplexities that attend them.

Our thanks are especially due to Dr. Wolcott Gibbs for his hearty encouragement, and for the use of valuable apparatus and chemicals.

XLIV. *The Empirical Proof of the Law of Error. By* F. Y. EDGEWORTH, *M.A., Lecturer at King's College, London*[*].

THE Law of Error is here used as a short title for the proposition that Observations (including statistical returns) tend to group themselves in the manner represented by the Probability-curve $y=\frac{1}{\sqrt{\pi c}}e^{-\frac{x^2}{c^2}}$. The empirical proof of this proposition is obtained by comparing actual groups with the theoretical arrangement. This examination of fact is most efficient when there is present deductive reasoning; both suggesting what interrogations should be put to experience, and corroborating the answers.

The deductive proof of the Law of Error consists of a mathematical theory which is the foundation of the *Method of Least Squares*. According to this theory, observations tend to be grouped according to the Law of Error when the following conditions are fulfilled. Each observation must be one

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[*] Communicated by the Author.

and the same definite and constant function of a number of variable elements. Each variable assumes for different observations different values according to some law of facility. And the number of the variables on which an observation depends must be large.

This is the general statement of the conditions; but the nearer definition requires care. Quetelet's simple illustrations have countenanced the supposition that the elementary variables must all obey one and the same law of facility, and that of the simplest kind. But it is shown by Poisson's generalization of Laplace's theory that the facility-curves may be different and of almost any species. If they are unsymmetrical, the figure in which the observations group themselves will still be a Probability-curve—with respect to the central portion at least; for the extremities will be apt to differ from the typical form and from each other. It is not even necessary to assume the facility-curve for each variable to be a constant function. We might suppose these forms to shift and slide between the times at which different observations are taken, provided there remain constant the centre of gravity of all the facility-curves and their average mean-square of error.

Nor, again, need the observation be the simple sum of the variables or elements. It may be any linear function of them. Now, as by Taylor's theorem any function may be regarded as linear for small values of its variables, it might seem that, whatever the relation of the observation to its constituent elements, the law of facility for the observations must be in the neighbourhood of its centre a probability-curve. It will be found, however, that this proposition holds in general for such small distances about the centre that any other symmetrical curve would equally well represent the grouping. The range over which the law prevails is apt to be of a smaller order than when the elementary variables are combined by simple addition*.

As we can seldom be certain beforehand how far the necessary conditions are fulfilled, the theoretical analysis does not enable us to dispense with the empirical verification. However, the hints afforded by theory assist us in examining experience. We shall not expect Social Statistics, *e. g.* Bank Reserves, to comply with the Law of Error, when they depend on one or two great causes such as a crisis or a war†. When

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* *Cf.* Glaisher, 'Memoirs of the Astronomical Society,' vol. xl. p. 105.

† The number of elements necessary for some approximation to the Law of Error is less than might be expected. The present writer has shown, by forming sums of *five* digits taken at random from mathematical

there is secular increase of any quantity, *e. g.*, at some periods, of Bank-notes-in-the-hands-of-the-public, then, the facility-curves not being constant, we shall not look for the Law of Error. Nor, when we have reason to suspect that the facility-curves of the variables are not symmetrical, that there is a drift of tendency in one direction, shall we expect the resulting arrangement to be symmetrical; though it is quite possible that the central portion of the generated curve, the body as distinguished from the extremities, may be of the typical form. In no case can the extremities, say the regions at a distance of 1·5 × modulus from the centre, be expected to fulfil the law accurately*.

The empirical investigation thus directed may conveniently be separated into two steps. We should first ascertain whether, and how far the curve (or class of curves) under consideration is symmetrical; next, whether, as to that part of it which is symmetrical, it is to be regarded as the Probability-curve and not another.

I. Whether an observed group is symmetrical or not is in the main to be determined by inspection and common sense. However, the Theory of Probabilities may supply some hints and warnings useful in cases which are not self-evident. One caution is that the position of an extreme observation is not *per se* a safe test that the limb whose extremity is furthest extended is really longest. Though of course if, examining repeated specimens of a certain category, we find that one limb seems ever longer than the other, this persistent disparity cannot be due to accident. It is thus that the present writer has proved the fact that prices are apt to diverge from their average more in excess than in defect†. Even in that case, however, it was well to take account of the penultimate and antepenultimate, as well as the extreme, observations.

It is a very good plan, as Dr. Venn has recommended ‡, to compare the mean-error above and that below the apparent, the arithmetic, mean. The significance of this result may be tested by the formula which Laplace has given for the error of the sum

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tables, that even so small a number of elements will afford a respectable approximation to the perfect probability-curve. ("Mathematical Theory of Banking," published in abstract in the Report of the British Association, 1886.)

* See illustrations of the last two propositions in my paper "On the Law of Error" &c., Phil. Mag. April 1886.

† Report of the British Association, 1887: Memorandum on the best method of measuring the change in the value of money.

‡ In an important letter to 'Nature,' Sept. 1, 1887.

of errors, *abstraction faite du signe**. Only we should take account that the mean from which we measure our errors above and below is itself liable to error. When we take the Greatest Ordinate as the mean, this sort of correction is not possible.

It appears that the mean error above and that below the Arithmetic Mean can only be different when the Arithmetic Mean and the Median are different. Accordingly an equally good test of symmetry may be afforded by comparing the Arithmetic Mean and the Median and estimating the probability of the observed difference occurring if the curve were really symmetrical. Take, for example, the heights of 25,878 American recruits recorded in the Report of the International Statistical Congress, vol. ii. p. 748. The Arithmetic mean is there given as 68·2. For the Median I find 68·15. For the Modulus-squared, which tests the difference to be expected between these Means, we have (by adding the squares of the Modulus applicable to each Mean separately) $\frac{2Se^2}{n^2} + \frac{n}{2P^2}$†; where the e's are errors measured from the Arithmetic Mean, P is the greatest ordinate (the number of observations per unit of abscissa at the densest part of the given group). Here $2Se^2$, as ascertained by actual counting (as well as by induction from other anthropometrical statistics‡) is 13 nearly. $n=25{,}878$. And P is 4054. Whence for the sought Modulus we have $\sqrt{\cdot 0005 + \cdot 0008} = \cdot 04$ nearly. And the observed difference (between the Median and Arithmetic Mean) is ·05: that is, very slightly greater than the corresponding Modulus; so that no great significance attaches to the observed indication of asymmetry.

As a second example, take the anthropometrical statistics cited by Mr. Merriman in his 'Method of Least Squares' (1885), art. 136. There the number of observations is 18,780. The Arithmetic Mean, as calculated by Mr. Merriman, is 67·24. The Median, as estimated by me, is 67·28. $\frac{2Se^2}{n}$, as deduced from Mr. Merriman's results, is 12·25. And P is 3000 nearly. Hence, for the Modulus appertaining to the difference between the Median and Arithmetic Mean, we have ·04; the same as the observed difference. Accordingly the indication of asymmetry is insignificant.

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* *Theor. Analytique*, Book ii. art. 19; adapted by Todhunter, 'History .... of Probability,' art. 1006. See below, p. 342.

† See the formula quoted from Laplace in my paper "On Problems in Probabilities" in the Philosophical Magazine, October 1886.

‡ See "Methods of Statistics," Journal of the Statistical Society, Jubilee volume (1885), p. 195.

It may be observed that this test of symmetry does not postulate that the curve under observation is a Probability-curve. The method is particularly convenient when, as in the case of measurements of human stature, we know beforehand by a copious induction the value of the mean-square-of-error.

II. We have next to test whether any other facility-curve fits symmetrical groups of observations better than the Probability-curve. We need not for this purpose try on an indefinite number of curves. We are limited by the condition that the substitute for the Probability-curve must be an equally simple form. It must not have more than one parameter; for, of course, by multiplying parameters we can make any species of curve fit any data. Nor must the variable be involved in a very complicated manner; for we require a serviceable as well as a faithful representative of the data.

These considerations appear to limit the field of competition to a few rival curves, some algebraic and some exponential. The chief algebraic competitors are the Right Line and the Parabola. The exponential curves are related to the Probability-curve, in that, where the latter involves the variable abscissa in the second power, the former involve it in the first and third power respectively. We might call them the First and Third Exponentials. Affecting each of the functions so designated with a proper factor (rendering the integral between extreme limits equal to unity), we have as the ordinates of the four rival curves the following:—

$$\frac{1}{c_1}\left(1-\frac{x}{c_1}\right); \quad \frac{3}{4c_2}\left(1-\frac{x^2}{c_2{}^2}\right); \quad \frac{1}{2c_3}e^{-\frac{x}{c_3}}; \quad \frac{1}{{}^*1{\cdot}7859\ldots c_4}e^{-\frac{x^3}{e_4{}^3}}.$$

Each of these curves possesses one parameter, which is to be determined from the data. For this purpose we should adopt some simple and uniform method which may give no advantage to the Probability-curve. A method which well fulfils these conditions is to equate the observed mean error $\left(\frac{Se}{n}\right)$ with that function of the parameter which ought to be equal to that datum, if the curve corresponded to the actual grouping. We have thus, putting ϵ for the observed mean-error, the following equations for the parameters:—

$$c_1=3\epsilon; \quad c_2=\tfrac{8}{3}\epsilon; \quad c_3=\epsilon; \quad c_4={}^\dagger 1{\cdot}9784\ldots\epsilon.$$

To these may be added the equation for the parameter (the

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* $=\frac{2}{3}\Gamma(\frac{1}{3})$.

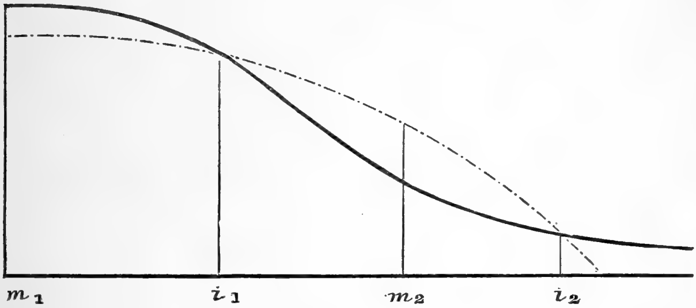
† $=\Gamma(\frac{1}{3})\div\Gamma(\frac{2}{3})$.

Modulus) of the Probability-curve :—

$$c = {}^*1{\cdot}772 \ldots \epsilon.$$

In testing the fit of these curves we may pursue two methods. We may compare computed with experienced values, either with respect to small parts or some large aggregate. For instance, and in particular, we may examine either *ordinates* or *areas* ; either small strips, or a large extent, of area. The former test yields the greatest quantity of evidence if we take the trouble of extracting it ; but for the same labour the comparison of areas yields a better result than that of ordinates.

Confining ourselves here to the more summary tests, we may observe that it is not indifferent what point we select in order to observe whether the ordinate given by experience at that point, or the area within it, is better represented by the Probability-, or some rival, curve. Consider in the accompanying figure the relation of the Probability-curve (the continuous line) to the parabola (the discontinuous line), each being supposed to have the same mean error, in the sense above explained. If we compare real with computed ordinates in the neighbourhood of either the point i_1 or i_2, it is clear that there will not be much to choose between the parabola and the Probability-curve. It would be better to select as the theatres of our comparison the neighbourhood of m_1 or m_2, where the difference between the computed ordinates of the rival curves is a maximum. Conversely, i_1 and i_2 would be good points to select for the comparison of areas.



It will be desirable to bring to the empirical investigation a knowledge of these critical points, as they may be called, for all the curves with which we are concerned. They may be expressed conveniently in terms of the computed Modulus of the Probability-curve, that is $\sqrt{\pi}\epsilon$. In the accompanying

[*] $= \sqrt{\pi}.$

table the first column represents degrees of the abscissa, measuring each a tenth part of the Modulus. The remaining columns show, for each of the rival curves, at which of those degrees its critical points occur. Thus, in the second column *i* opposite to the entry ·1–·2 indicates that there is an intersection between the Right Line and the Probability-curve at some point whose abscissa lies between a tenth and two tenths of the Modulus. In the same column *m* indicates that, somewhere between the points ·3 and ·4, the difference between the ordinate of the Right Line and the Probability-curve becomes a maximum.

Modulus.	Right Line.	Parabola.	First Exponential.	Third Exponential.
0–·1 ……				
·1–·2 ……	*i*			
·2–·3 ……				
·3–·4 ……	*m*		*i*	
·4–·5 ……		*i*		*i*
·5–·6 ……				
·6–·7 ……	*i*		*m*	*m*
·7–·8 ……				
·8–·9 ……				
·9–1 ………		*m*		
1–1·1 ……				
1·1–1·2 ……	*m*			
1·2–1·3 ……		*i*		

Having this clue, let us now examine the data of experience. Let us take, as a first example, 470 observations (of the right ascension of Sirius and Altair) made by Bradley, and discussed by Bessel in his *Fundamenta Astronomiæ* (p. 20)*. We shall first compare the fit of the rival curves in respect of some selected ordinates, the points of comparison being determined upon the principle above mentioned.

We start with the datum (adopted from Bessel) that the

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* They are quoted by Chauvenet in his 'Manual of Astronomy,' and also by Merriman in his 'Method of Least Squares,' 1885.

Mean Error ($\frac{Se}{n}$, our ϵ) is ·3119. Hence it follows that the Modulus of the Probability-curve is ·55282. We are thus able to graduate the abscissa in aliquot parts of the Modulus, so as to select the proper points of comparison with each rival curve. Then, using the datum $\epsilon = {}$·3119 to determine the parameters of the rival curves, we compute the ordinates (or small strips of area) at each of the selected regions.

In the accompanying table the first column contains parts of a second; the second column the corresponding fractions of the Modulus (*e.g.* ·1 second = ·181* Modulus). The third column gives for each interval the number of observations actually experienced. The fourth column gives the numbers as computed by Bessel for the Probability-curve. The remaining columns contain numbers computed by me for the rival curves at regions selected by comparing the second column of this table with the first column of the preceding table. In computing the numbers I have used the same approximative formula as Bessel, *mutatis mutandis*, viz.

Seconds.	Modulus.	Experience.	Probability-curve.	Right Line.	Parabola.	First Exponential.	Third Exponential.
0–·1...	·181	94	95			130	
·1–·2...	·362	88	89	73			
·2–·3...	·543	78	78				
·3–·4...	·724	58	64			50	71
·4–·5...	·904	51	50				
·5–·6...	1·085	36	35		51		
·6–·7...	1·266	26	24	29			

$$N(\Delta' - \Delta)[\phi(\Delta) + \phi(\Delta')],$$

where N is the total number of observations, here 470; Δ' and Δ are the superior and inferior limits of an interval (*e.g.* ·1 and ·2); and ϕ is the form of a rival curve (*e.g.*, for the Right Line $\frac{1}{c_1}\left[1 - \frac{x}{c_1}\right]$, where $c_1 = 3\epsilon$, and $\epsilon = {}$·3119).

This empirical proof of the Probability-curve is striking, and would be more striking if we extracted all the evidence which the completed table would yield. If we do not care to take that trouble, we are likely to obtain a better return for the same labour by comparing areas rather than ordinates. This comparison is effected in the accompanying table.

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* More exactly, ·18089.

Seconds.	Modulus.	Experience: Strips.	Experience: Areas.	Right Line.	Parabola.	First Exponential.	Probability-curve.	Ecart of Probability-curve.	Modulus of Ecart.	Ratio of Ecart to Modulus.	Odds in favour of Probability curve.
0–·1......	·181	94									
·1–·2......	·362	88	182			247	184	2	15	·13	7
·2–·3.. ...	·543	78	260	253	244		262	2	15	·13	7
·3–·4......	·724	58	318	316			326	8	14	·57	·7
·4–·5......	·905	51									
·5–·6......	1·085	36									
·6–·7......	1·266	26	431		453		435	4	8	·5	1
				2	22	65	Ecart of Rival Curve.				
				14	6	15	Modulus of Ecart.				
				·14	3	4	Ratio of Ecart to Modulus.				
				·18	50000	10^8	Odds against Rival Curve.				

In this table the first three columns are the same as in the last table; the fourth column contains the sum of the experienced observations from zero up to the point indicated by the corresponding entry in the second column. For instance, 182 in the fourth column means that between zero and the point ·362 Modulus of Probability-curve, that is, ·2 of a second, there actually occurred 182 observations. The next three columns give the computed values of the number of observations up to the critical point of each rival curve. The Third Exponential, on account of the difficulty which its integration presents, has been omitted. The eighth column contains the computed number of observations up to certain points for the Probability-curve. The ninth column contains the écart or difference between the computed and experienced numbers for the Probability-curve. The tenth column gives the Modulus which measures the probability of a certain écart occurring. It is deduced from the formula $\sqrt{2p(1-p)N}$, where p is the proportion of the observations which, according to theory, should occur within the point (*e.g.* for the point ·724, $p=·69$); N is 470. The last column gives the odds (to one) *in favour* of the Probability-curve; the odds that, if the Probability-curve held good, the écart would be at least as large as it is. The last four *rows* give the corresponding results for the Rival curves, their écart, Modulus of écart, Ratio of écart to Modulus, and thence deduced odds (to one) *against* the Rival Curves.

It is noticeable that the Right Line comes out unscathed from this ordeal. The results would doubtless have been more striking had the number of observations been greater. This is the case in the next example (table, p. 341), which consists of the height-measurements of 683068 Italian recruits tabulated by Signor Perozzo*. The Median and Arithmetic mean being coincident at the point or compartment 1·62 metres, I have thought it legitimate to suppose the curve folded about the central ordinate; so that, measuring from that centre as zero, we shall now have between 0 and ·005 (metre) the number of observations which Signor Perozzo puts at 1·62—that is, between 1·615 and 1·625. Between ·005 and ·015 of the new arrangement we shall have the sum of the observations registered for 1·61 and 1·63; and so on.

By actual counting I find for the Mean Error ·053. Whence the Modulus$=\sqrt{\pi}\times$Mean Error$=·094$. By means of this datum the second column is graduated and the critical points are selected. After the explanation of the preceding table, further comment is unnecessary. It is noticeable that

* *Annales de Demographie*, 1878; *Annali di Statistica*, 1878, vol. ii.

the odds against the Probability-curve being exact are enormous*. But, if we must adopt some simple representative of the experienced data, the case is as if we had to choose between different explanations of a fact which has occurred; each of which hypotheses is *à priori* improbable, but one of which must be true. Upon this view, the odds in favour of the Probability-curve are stupendous.

It is not to be thought that the method of examining wholes rather than small parts is confined to the comparison of areas. We might regard the sum of the number of observations as a particular case of the sums of powers of errors. The sums of the apparent errors all taken positively, the sums of their squares and other powers, all supply empirical data proper to this method. We might put the facility-curve which is on trial in the form $\alpha\phi(\beta x)$; determine the constants α and β by means of any two of the above-named data; then for the curve so determined, calculate some other sum of powers; and then compare that computed value with the corresponding datum of experience.

For instance, let us apply the First Exponential to the group of 25878 height-measurements mentioned above. Let us take for our basis of computation the observed sum-of-squares-of-errors, 166072, and the observed zero-power of errors, viz. the number of observations. The resulting curve is of the form

$$y = 25878\frac{1}{2c}e^{-\frac{x}{c}},$$

where c is to be determined from the equation

$$166072 = \text{S}e^2 = 25878\int_0^\infty \frac{x^2}{c}e^{-\frac{x}{c}}$$

$$= 25878\,c^2\int_0^\infty x^2e^{-x}dx = 25878 \times 2c^2.$$

Whence

$$c^2 = 3{\cdot}21, \quad c = 1{\cdot}8.$$

For the curve so determined we have now to compute the sum, or Mean, of errors. The Mean Error $= c = 1{\cdot}8$. This result is to be compared with experience, which gives for the Mean Error 2—a discrepancy whose significance may thus be tested. By a formula of Laplace already referred to, the Modulus-squared for testing the error of the mean (first

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* A complex curve formed by the superposition of several Probability-curves having different centres (corresponding to different provinces) would doubtless be a nearer approximation. See on this point my letter to 'Nature,' Sept. 22, 1887.

Centimetres.	Modulus.	Experience: Strips.	Experience: Areas.	Right Line.	Parabola.	First Exponential.	Probability-curve.	Ecart of Probability-curve.	Modulus of Ecart.	Ratio of Ecart to Modulus.	Odds against Probability-curve.
·005	·053	41119									
·015	·159	78480	119599				121524	1925	447	4·3	10^{9}
·025	·265	78127									
·035	·371	73649	271375			277033	273357	1982	570	3·5	1,000,000
·045	·477	66176	337551		314080		337697	146	580	·25	·4
·055	·583	62565									
·065	·689	56369	456485	425046			457750	1265	550	2·2	998
				31439	23471	5658	Ecart of Rival Curve.				
				565	580	570	Modulus of Ecart.				
				55	40	10	Ratio of Ecart to Modulus.				
				10^{1400}	10^{700}	10^{44}	Odds against Rival Curve.				

power of) error, *abstraction faite du signe*, is

$$\frac{\text{twice (Mean-square-of-error} - \text{(Mean-Error)}^2)}{\text{Number of Observations}};$$

that is, here,

$$2(6{\cdot}4-4)\div 25878 = {\cdot}00019.$$

Whence the Modulus is about ·014, while the observed écart is ·4—some thirty times larger. This gives odds of nonillions to one against the First Exponential. The corresponding odds against the Probability-curve are some hundreds to one.

It should be observed that the sums of powers may be taken for integral portions of the curve's extent, rather than for the whole. This plan seems theoretically more correct, since the fulfilment of the law of error is to be looked for rather in the body of the curve than at the extremities. There arise, however, practical difficulties about the computation in the case of some curves; in the absence of tables for the values of $\int x^2\phi(x)\,dx$. The Probability-curve itself affords an instance.

Different modes of verification will be appropriate to different cases. But it is not the purpose of this paper to provide a complete Manual of empirical evidence; but rather to show in what sort of way the examination of experience may be assisted by the Mathematical Theory of Errors and Method of Statistics.

XLV. *On the Propagation of Laminar Motion through a turbulently moving Inviscid Liquid. By* Sir WILLIAM THOMSON, *LL.D., F.R.S.*[*]

1. IN endeavouring to investigate turbulent motion of water between two fixed planes, for a promised communication to Section A of the British Association at its coming Meeting in Manchester, I have found something seemingly towards a solution (many times tried for within the last twenty years) of the problem to construct, by giving vortex motion to an incompressible inviscid fluid, a medium which shall transmit waves of laminar motion as the luminiferous æther transmits waves of light.

2. Let the fluid be unbounded on all sides, and let u, v, w be the velocity-components, and p the pressure at (x, y, z, t). We have

$$\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (1),$$

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[*] Communicated by the Author, having been read before Section A of the British Association at its recent Meeting in Manchester.

$$\frac{du}{dt}=-\left(u\frac{du}{dx}+v\frac{du}{dy}+w\frac{du}{dz}+\frac{dp}{dx}\right) \quad . \quad . \quad . \quad (2),$$

$$\frac{dv}{dt}=-\left(u\frac{dv}{dx}+v\frac{dv}{dy}+w\frac{dv}{dz}+\frac{dp}{dy}\right) \quad . \quad . \quad . \quad (3),$$

$$\frac{dw}{dt}=-\left(u\frac{dw}{dx}+v\frac{dw}{dy}+w\frac{dw}{dz}+\frac{dp}{dz}\right) \quad . \quad . \quad . \quad (4).$$

From (2), (3), (4) we find, taking (1) into account,

$$-\nabla^2 p=\frac{du^2}{dx^2}+\frac{dv^2}{dy^2}+\frac{dw^2}{dz^2}+2\left(\frac{dv}{dz}\frac{dw}{dy}+\frac{dw}{dx}\frac{du}{dz}+\frac{du}{dy}\frac{dv}{dz}\right) \quad . \quad (5).$$

3. The velocity-components u, v, w may have any values whatever through all space, subject only to (1). Hence, on Fourier's principles, we have, as a perfectly comprehensive expression for the motion at any instant,

$$u=\Sigma\Sigma\Sigma\Sigma\Sigma\Sigma\,\alpha^{(e,f,g)}_{(m,n,q)}\sin(mx+e)\cos(ny+f)\cos(qz+g) \quad . \quad (6),$$

$$v=\Sigma\Sigma\Sigma\Sigma\Sigma\Sigma\,\beta^{(e,f,g)}_{(m,n,q)}\cos(mx+e)\sin(ny+f)\cos(qz+g) \quad . \quad (7),$$

$$w=\Sigma\Sigma\Sigma\Sigma\Sigma\Sigma\,\gamma^{(e,f,g)}_{(m,n,q)}\cos(mx+e)\cos(ny+f)\sin(qz+g) \quad . \quad (8);$$

where $\alpha^{(e,f,g)}_{(m,n,q)}$, $\beta^{(e,f,g)}_{(m,n,q)}$, $\gamma^{(e,f,g)}_{(m,n,q)}$ are any three velocities satisfying the equation

$$0=m\alpha^{(e,f,g)}_{(m,n,q)}+n\beta^{(e,f,g)}_{(m,n,q)}+q\gamma^{(e,f,g)}_{(m,n,q)} \quad . \quad . \quad . \quad (9);$$

and $\Sigma\Sigma\Sigma\Sigma\Sigma\Sigma$ summation (or integration) for different values of m, n, q, e, f, g. The summations for e, f, g may, without loss of generality, be each confined to two values: $e=0$, and $e=\frac{1}{2}\pi$; $f=0$, and $f=\frac{1}{2}\pi$; $g=0$, and $g=\frac{1}{2}\pi$. We shall admit large values, and infinite values of m^{-1}, n^{-1}, q^{-1}, under certain conditions [§ 4 (10), (11), (12), and § 15 below], but otherwise we shall suppose the greatest value of each of them to be of some moderate, or exceedingly small, linear magnitude. This is an essential of the averagings to which we now proceed.

4. Let xav, xzav, xyzav denote space-averages, linear, surface, and solid, through infinitely great spaces, defined and illustrated by examples, each worked out from (6), (7), (8), as follows, L denoting an infinitely great length, or a very great multiple of whichever of m^{-1}, n^{-1}, q^{-1} may be concerned :—

$$\text{xav } u = \frac{1}{2\text{L}}\int_{-\text{L}}^{\text{L}} dx\, u = \Sigma\Sigma\Sigma\Sigma\alpha_{(0,\, n,\, q)}^{(\frac{1}{2}\pi,\, f,\, g)} \cos(ny+f)\cos(qz+g) \quad . \ (10),$$

$$\text{xzav } u = \left(\frac{1}{2\text{L}}\right)^2 \int_{-\text{L}}^{\text{L}}\int_{-\text{L}}^{\text{L}} dz\, dx\, u = \overset{f}{\Sigma}\underset{n}{\Sigma}\alpha_{(0,\, n,\, 0)}^{(\frac{1}{2}\pi,\, f,\, 0)} \cos(ny+f) \quad . \ (11),$$

$$\text{xyzav } u = \left(\frac{1}{2\text{L}}\right)^3 \int_{-\text{L}}^{\text{L}}\int_{-\text{L}}^{\text{L}}\int_{-\text{L}}^{\text{L}} dz\, dy\, dx\, u = \alpha_{(0,\, 0,\, 0)}^{(\frac{1}{2}\pi,\, 0,\, 0)} \quad . \quad . \quad (12),$$

$$\text{xav } u^2 = \tfrac{1}{2}\Sigma\Sigma\Sigma\Sigma\Sigma\Sigma\left[\alpha_{(m,\, n,\, q)}^{(e,\, f,\, g)}\right]\cos^2(ny+f)\cos^2(qz+g) \quad . \ (13);$$

this with the exceptions that

in the case of $m=0$, $e=0$, we take 0 in place of $\frac{1}{2}$,

and in the case of $m=0$, $e=\frac{1}{2}\pi$ " 1 " " .

$$\text{xzav } u^2 = \tfrac{1}{4}\overset{e}{\Sigma}\overset{f}{\Sigma}\overset{g}{\Sigma}\underset{m}{\Sigma}\underset{n}{\Sigma}\underset{q}{\Sigma}\left[\alpha_{(m,\, n,\, q)}^{(e,\, f,\, g)}\right]^2\cos^2(ny+f) \quad . \quad . \quad (14),$$

$$\text{xzav } uv = \tfrac{1}{4}\ \overset{f}{\Sigma}\overset{g}{\Sigma}\underset{m}{\Sigma}\underset{n}{\Sigma}\underset{q}{\Sigma}\Big[\alpha_{(m,\, n,\, q)}^{(\frac{1}{2}\pi,\, f,\, g)}\,\beta_{(m,\, n,\, q)}^{(0,\, f,\, g)}$$

$$-\alpha_{(m,\, n,\, q)}^{(0,\, f,\, g)}\,\beta_{(m,\, n,\, q)}^{(\frac{1}{2}\pi,\, f,\, g)}\Big]\cos(ny+f)\sin(ny+f) \quad . \ (15);$$

with the exceptions for (14) that

$$\left.\begin{array}{l}\text{in the case of } \quad m=0 \text{ and } e=0 \\ \text{and in the case of } q=0 \text{ and } g=\frac{1}{2}\pi\end{array}\right\} \text{ we take 0 instead of } \tfrac{1}{4};$$

$$\left.\begin{array}{l}\text{in the case of } \quad m=0 \text{ and } e=\frac{1}{2}\pi \\ \text{and in the case of } q=0 \text{ and } g=0\end{array}\right\} \quad \text{"} \quad \tfrac{1}{2} \quad \text{"} \quad \text{"} \quad \tfrac{1}{4};$$

in the case of $m=0, e=\frac{1}{2}\pi, n=0, f=\frac{1}{2}\pi$ " 1 " " $\frac{1}{4}$;

and analogous exceptions for (15).

$$\text{xyzav } u^2 = \tfrac{1}{8}\overset{e}{\Sigma}\overset{f}{\Sigma}\overset{g}{\Sigma}\underset{m}{\Sigma}\underset{n}{\Sigma}\underset{q}{\Sigma}\left[\alpha_{(m,\, n,\, q)}^{(e,\, f,\, g)}\right]^2 \quad . \quad . \quad . \quad (16),$$

with exceptions for zeros of m and q, analogous to those of (14).

5. As a last example of averagings for the present, take xyzav of (5). Thus we find

$$\left.\begin{array}{l}-\text{xyzav } \nabla^2 p = \tfrac{1}{8}\overset{e}{\Sigma}\overset{f}{\Sigma}\overset{g}{\Sigma}\underset{m}{\Sigma}\underset{n}{\Sigma}\underset{q}{\Sigma}\left\{m\alpha_{(m,\, n,\, q)}^{(e,\, f,\, g)} + n\beta_{(m,\, n,\, q)}^{(e,\, f,\, g)} + q\gamma_{(m,\, n,\, q)}^{(e,\, f,\, g)}\right\}^2 \\ \qquad = 0 \text{ by (9).}\end{array}\right\} (17).$$

The interpretation is obvious.

6. Remark, as a general property of this kind of averaging,

$$\text{xav}\,\frac{dQ}{dx}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18),$$

if Q be any quantity which is finite for infinitely great values of x.

7. Suppose now the motion to be homogeneously distributed through all space. This implies that the centres of inertia of all great volumes of the fluid have equal parallel motions, if any motions at all. Conveniently, therefore, we take our reference lines OX, OY, OZ, as fixed relatively to the centres of inertia of three (and therefore of all) centres of inertia of large volumes; in other words, we assume no translatory motion of the fluid as a whole. This makes zero of every large average of u and of v and of w; and, in passing, we may remark, with reference to our notation of § 3, that it makes, as we see by (10), (11), (12),

$$0=\alpha_{(0,\,n,\,q)}=\alpha_{(m,\,0,\,q)}=\alpha_{(m,\,n,\,0)}=\beta_{(0,\,n,\,q)}=\&\text{c. }\&\text{c.}=\gamma_{(m,\,n,\,0)} \quad (19).$$

Without for the present, however, encumbering ourselves with the Fourier-expression and notation of § 3, we may write, as the general expression for nullity of translational movement in large volumes,

$$0=\text{ave}\,u=\text{ave}\,v=\text{ave}\,w \quad . \quad . \quad . \quad . \quad . \quad (20);$$

where ave denotes the average through any great length of straight or curved line, or area of plane or curved surface, or through any great volume of space.

8. In terms of this generalized notation of averages, homogeneousness implies

$$\text{ave}\,u^2=U^2, \qquad \text{ave}\,v^2=V^2, \qquad \text{ave}\,w^2=W^2 \quad . \quad (21),$$

$$\text{ave}\,vw=A^2, \qquad \text{ave}\,wu=B^2, \qquad \text{ave}\,uv=C^2 \quad . \quad (22);$$

where U, V, W, A, B, C are six velocities independent of the positions of the spaces in which the averages are taken. These equations are, however, infinitely short of implying, though implied by, homogeneousness.

9. Suppose now the distribution of motion to be isotropic. This implies, but is infinitely more than is implied by, the following equations in terms of the notation of § 8, with further notation, R, to denote what we shall call THE AVERAGE VELOCITY of the turbulent motion:—

$$U^2=V^2=W^2=\tfrac{1}{3}R^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (23),$$

$$0=A=B=C \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (24).$$

10. Large questions now present themselves as to trans-

formations which the distribution of turbulent motion will experience in an infinite liquid left to itself with any distribution given to it initially. If the initial distribution be homogeneous through all large volumes of space, except a certain large finite space, S, through which there is initially either no motion, or turbulent motion homogeneous or not, but not homogeneous with the motion through the surrounding space, will the fluid which at any time is within S acquire more and more nearly as time advances the same homogeneous distribution of motion as that of the surrounding space, till ultimately the motion is homogeneous throughout?

11. If the answer were yes, could it be that this equalization would come to pass through smaller and smaller spaces as time advances? In other words, would any given distribution, homogeneous on a large enough scale, become more and more *fine-grained* as time advances? Probably yes for some initial distributions; probably no for others. Probably *yes* for vortex motion given continuously through all of one large portion of the fluid, while all the rest is irrotational.

12. Probably *no* for the initial motion given in the shape of equal and similar Helmholtz rings, of proportions suitable for individual stability, and each of overall diameter considerably smaller than the average distance from nearest neighbour. Prodably also *no*, though the rings be of very different volumes and vorticities. But probably *yes* if the diameters of the rings, or of many of them, be not small in comparison with distances from neighbours, or if the individual rings, each an endless slender filament, be entangled or nearly entangled among one another.

13. Again a question: If the initial distribution be *homogeneous and æolotropic*, will it become more and more isotropic as time advances, and *ultimately quite isotropic*? Probably yes, for any random initial distribution, whether of continuous rotationally-moving fluid or of separate finite vortex rings. Possibly *no* for some symmetrical initial distribution of vortex rings, conceivably stable.

14 If the initial distribution be homogeneous and isotropic (and therefore utterly *random* in respect to direction), will it remain so? Certainly *yes.* I proceed to investigate a mathematical formula, deducible from the answer, which will be of use to us later (§ 18). By (22) and (24) we have

$$\text{xzav}\ uv = 0,\ \text{for all values of}\ t \quad . \quad . \quad . \quad (25).$$

But by (2) and (3) we find

$$\frac{d}{dt}(\text{xzav}\ uv) = -\text{xza}\ \left\{ u\frac{d(uv)}{dx} + v\frac{d(uv)}{dy} + w\frac{d(uv)}{dz} + v\frac{dp}{dx} + u\frac{dp}{dy} \right\} (26).$$

Hence

$$0 = \text{xzav}\left\{u\frac{d(uv)}{dx} + v\frac{d(uv)}{dy} + w\frac{d(uv)}{dz} + v\frac{dp}{dx} + u\frac{dp}{dy}\right\} . \quad . \quad (27).$$

This equation in fact holds for every random case of motion satisfying (30) below, because positive and negative values of u, v, w are all equally probable, and therefore the value of the second member of (27) is doubled by adding to itself what it becomes when for u, v, w we substitute $-u$, $-v$, $-w$, which, it may be remarked, and verified by looking at (5), does not change the value of p.

15. We shall now suppose the initial motion to consist of a laminar motion $[f(y), 0, 0]$ superimposed on a homogeneous and isotropic distribution $(\mathfrak{u}_0, v_0, w_0)$; so that we have

$$\text{when } t=0, \quad u=f(y)+\mathfrak{u}_0, \quad v=v_0, \quad w=w_0 \quad . \quad (28);$$

and we shall endeavour to find such a function, $f(y, t)$, that at any time t the velocity-components shall be

$$f(y, t)+\mathfrak{u}, \; v, \; w \quad . \quad . \quad . \quad . \quad . \quad . \quad (29),$$

where $\mathfrak{u}$, v, w are quantities of each of which every large enough average is zero, so that particularly, for example,

$$0 = \text{xzav}\,\mathfrak{u} = \text{xzav}\,v = \text{xzav}\,w \quad . \quad . \quad . \quad (30).$$

16. Substituting (29) for u, v, w in (2) we find

$$\frac{df(y,t)}{dt} + \frac{d\mathfrak{u}}{dt} = -\left\{f(y,t)\frac{d\mathfrak{u}}{dx} + v\frac{df(y,t)}{dy}\right\} - \left(\mathfrak{u}\frac{d\mathfrak{u}}{dx} + v\frac{d\mathfrak{u}}{dy} + w\frac{d\mathfrak{u}}{dz} + \frac{dp}{dx}\right) \quad (31)$$

Take now xzav of both members. The second term of the first member and the second term of the second member disappear, each in virtue of (30). The first and last terms of the second member disappear, each in virtue of (18) alone, and also each in virtue of (30). There remains

$$\frac{df(y,t)}{dt} = -\,\text{xzav}\left(\mathfrak{u}\frac{d\mathfrak{u}}{dx} + v\frac{d\mathfrak{u}}{dy} + w\frac{d\mathfrak{u}}{dz}\right) \quad . \quad . \quad (32).$$

To simplify, add to the second member [by (1)]

$$0 = -\,\text{xzav}\left(\mathfrak{u}\frac{d\mathfrak{u}}{dx} + \mathfrak{u}\frac{dv}{dy} + \mathfrak{u}\frac{dw}{dz}\right) . \quad . \quad . \quad . \quad (33);$$

and, the first and third pair of terms of the thus-modified second member vanishing by (18), find

$$\frac{df(y,t)}{dt} = -\,\text{xzav}\frac{d(\mathfrak{u}v)}{dy} \quad . \quad . \quad . \quad . \quad . \quad . \quad (34).$$

It is to be remarked that this result involves, besides (1), no other condition respecting $(\mathfrak{u}, v, w)$ than (30); no isotropy, no homogeneousness in respect to y; and only homogeneousness of *régime* with respect to y and z, with no mean translational motion.

The x-translational mean component of the motion is wholly represented by $f(y, t)$, and, so far as our establishment of (34) is concerned, may be of any magnitude, great or small relatively to velocity-components of the turbulent motion. It is a fundamental formula in the theory of the turbulent motion of water between two planes; and I had found it in endeavouring to treat mathematically my brother Prof. James Thomson's theory of the "Flow of Water in Uniform *Régime* in Rivers and other Open Channels"*. In endeavouring to advance a step towards the law of distribution of the laminar motion at different depths, I was surprised to discover the seeming possibility of a law of propagation as of distortional waves in an elastic solid, which constitutes the conclusion of my present communication, on the supposition of § 15 that the distribution $\mathfrak{u}_0$, v_0, w_0 is isotropic, and that $df(y, t)/dy$, divided by the greatest value of $f(y, t)$, is infinitely small in comparison with the smallest values of m, n, q, in the Fourier-formulæ (6), (7), (8) for the turbulent motion.

17. By (34) we see that, if the turbulent motion remained, through time, isotropic as at the beginning, $f(y, t)$ would remain constantly at its initial value $f(y)$. To find whether the turbulent motion does remain isotropic, and, if it does not, to find what we want to know of its deviation from isotropy, let us find $\text{xzav}\frac{d(\mathfrak{u}v)}{dt}$, by (2) and (3), as follows:—First, by multiplying (31) by v, and (3) by $\mathfrak{u}$, and adding, we find

$$v\frac{df(y,t)}{dt}+\frac{d(\mathfrak{u}v)}{dt}=-\left\{f(y,t)\frac{d(\mathfrak{u}v)}{dx}+v^2\frac{df(y,t)}{dy}\right\}$$

$$-\left\{\mathfrak{u}\frac{d(\mathfrak{u}v)}{dx}+v\frac{d(\mathfrak{u}v)}{dy}+w\frac{d(\mathfrak{u}v)}{dz}+v\frac{dp}{dx}+\mathfrak{u}\frac{dp}{dy}\right\} \quad . \quad (35).$$

Taking xzav of this, and remarking that the first term of the first member disappears by (30), and the first term of the second member by (18), we find, with V^2, as in §§ 8, 9, to denote the average y-component-velocity of the turbulent motion,

$$\frac{d}{dt}\{\text{xzav}\,(\mathfrak{u}v)\}=-V^2\frac{df(y,t)}{dy}-Q \quad . \quad . \quad (36),$$

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* Proceedings of the Royal Society, Aug. 15, 1878.

where

$$Q = \text{xzav}\left\{ \mathfrak{u}\frac{d(\mathfrak{u}v)}{dx} + v\frac{d(\mathfrak{u}v)}{dy} + w\frac{d(\mathfrak{u}v)}{dz} + v\frac{dp}{dx} + \mathfrak{u}\frac{dp}{dy} \right\} \quad . \ (37).$$

18. Let

$$p = \mathfrak{p} + \varpi \quad . \ . \ . \ . \ . \ . \ . \ . \ (38),$$

where $\mathfrak{p}$ denotes what p would be if f were zero. We find, by (5),

$$-\nabla^2\varpi = 2\frac{df(y,t)}{dy}\frac{dv}{dx} \quad . \ . \ . \ . \ . \ . \ (39),$$

and, by (27) and (37),

$$Q = \text{xzav}\left(v\frac{d\varpi}{dx} + \mathfrak{u}\frac{d\varpi}{dy} \right) \quad . \ . \ . \ . \ . \ . \ (40).$$

So far we have not used either the supposition of initial isotropy for the turbulent motion, or of the infinitesimalness of df/dy. We now must introduce and use both suppositions.

19. To facilitate the integration of (39), we now use our supposition that $\frac{d}{dt}f(y,t)$, divided by the greatest value of $f(y,t)$, is infinitely small in comparison with m, n, q, which, as is easily proved, gives

$$\varpi = 2\frac{df(y,t)}{dy}\frac{1}{-\nabla^2}\frac{dv}{dx} \quad . \ . \ . \ . \ . \ . \ (41)'$$

by which (40) becomes

$$Q = -2\frac{df(y,t)}{dy}\,\text{xzav}\left(v\frac{d}{dx} + \mathfrak{u}\frac{d}{dy} \right)\nabla^{-2}\frac{dv}{dx} \quad . \ (42).$$

Now, by (x, z) isotropy, we have

$$2\,\text{xzav}\left(v_0\frac{d}{dx} + \mathfrak{u}_0\frac{d}{dy} \right)\nabla^{-2}\frac{dv_0}{dx}$$

$$= \text{xzav}\left\{ v_0\left(\frac{d^2}{dx^2} + \frac{d^2}{dz^2}\right) + \left(\mathfrak{u}_0\frac{d}{dx} + w_0\frac{d}{dz}\right)\frac{d}{dy} \right\}\nabla^{-2}v_0 \ . \quad (43).$$

Performing integrations by parts for the last two terms of the second member, and using (1), we find

$$\text{xzav}\left(\mathfrak{u}_0\frac{d}{dx} + w_0\frac{d}{dz} \right)\frac{d}{dy}\nabla^{-2}v_0 = -\text{xzav}\left(\frac{d\mathfrak{u}_0}{dx} + \frac{dw_0}{dz} \right)\frac{d}{dy}\nabla^{-2}v_0$$

$$= \text{xzav}\,\frac{dv_0}{dy}\frac{d}{dy}\nabla^{-2}v_0\,;$$

and so we find, by (43) and (42),

$$Q_0 = -\frac{df(y,t)}{dy}\,\text{xzav}\left\{ v_0\left(\frac{d^2}{dx^2} + \frac{d^2}{dz^2}\right) + \frac{dv_0}{dy}\frac{d}{dy} \right\}\nabla^{-2}v_0 \quad (44).$$

20. Using now the Fourier expansion (7) for v_0, we find

$$-\nabla^{-2}v_0=\overset{e\;f\;g}{\underset{m\;n\;q}{\Sigma\Sigma\Sigma\Sigma\Sigma\Sigma}}\,\beta^{(e,\,f,\,g)}_{(m,\,n,\,q)}\frac{\cos\,(mx+e)\,\sin\,(ny+f)\,\cos\,(qz+g)}{m^2+n^2+q^2}\quad . \quad (45).$$

Hence we find (with suffixes &c. dropped),

$$\text{xzav}\,\frac{dv_0}{dy}\,\frac{d}{dy}\,\nabla^{-2}v_0=-\tfrac{1}{8}\,\Sigma\Sigma\Sigma\Sigma\Sigma\Sigma\,\frac{n^2\beta^2}{m^2+n^2+q^2}\quad (46)^*,$$

and

$$\text{xzav}\,v_0\left(\frac{d^2}{dx^2}+\frac{d^2}{dz^2}\right)\nabla^{-2}v_0=\tfrac{1}{8}\,\Sigma\Sigma\Sigma\Sigma\Sigma\Sigma\,\frac{(m^2+q^2)\beta^2}{m^2+n^2+q^2}\quad . \quad (47).$$

Now, in virtue of the average uniformity of the constituent terms implied in isotropy and homogeneousness (§§ 7, 8, 9), the second member of (46) is equal to $-\tfrac{1}{8}\Sigma\Sigma\Sigma\Sigma\Sigma\Sigma\,\frac{\beta^2}{3}$, and therefore (§ 9) equal to $-\tfrac{1}{9}R^2$; and similarly we see that the second member of (47) is equal to $+\tfrac{2}{9}R^2$. Hence, finally, by (44),

$$Q_0=-\tfrac{1}{9}R^2\frac{df(y,t)}{dy}\quad . \quad . \quad . \quad . \quad . \quad . \quad (48);$$

and (36) for $t=0$, with $\tfrac{1}{3}R^2$ for V^2 on account of isotropy, becomes

$$\left\{\frac{d}{dt}\text{xzav}\,(uv)\right\}_{t=0}=-\tfrac{2}{9}\,R^2\left\{\frac{df(y,t)}{dy}\right\}_{t=0}\quad . \quad . \quad (49).$$

The deviation from isotropy, which this equation shows, is very small, because of the smallness of df/dy; and (27) does not need isotropy, but holds in virtue of (30). Hence (49) is not confined to the initial values (values for $t=0$) of the two members, because we neglect an infinitesimal deviation from $\tfrac{2}{9}R^2$ in the first factor of the second member, considering the smallness of the second factor. Hence, for all values of t, unless so far as the "random" character referred to at the end of § 13 may be lost by a rearrangement of vortices vitiating (27),

$$\frac{d}{dt}\,\text{xzav}\,(uv)=-\tfrac{2}{9}R^2\frac{df(y,t)}{dy}\quad . \quad . \quad . \quad . \quad (50).$$

21. Eliminating the first member from this equation, by (34), we find

$$\frac{d^2f}{dt^2}=\tfrac{2}{9}\,R^2\,\frac{d^2f}{dy^2}\quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (51).$$

Thus we have the very remarkable result that laminar disturbance is propagated according to the well-known mode of waves of distortion in a homogeneous elastic solid; and that the velocity of propagation is $\frac{\sqrt{2}}{3}R$, or about ·47 of the

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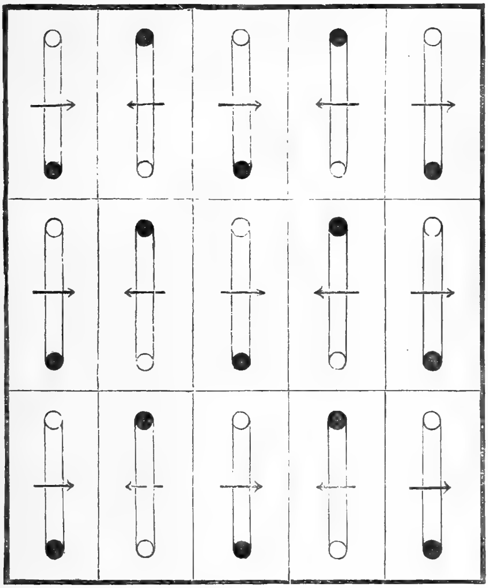
* Here and henceforth an averaging through y-spaces so small as to cover no sensible differences of $f(y,t)$, but infinitely large in proportion to n^{-1}, is implied.

average velocity of the turbulent motion of the fluid. This might seem to go far towards giving probability to the vortex theory of the luminiferous ether, were it not for the doubtful proviso at the end of § 20.

22. If the undisturbed condition of the medium be a stable symmetrical distribution of vortex-rings the suggested vitiation by "rearrangement" cannot occur. For example, let it be such as is represented in fig. 1, where the small white and black circles represent cross sections of the rings: the white where the rotation is opposite to, and the black where it is in the same direction as, the rotation of the hands of a watch placed on the diagram facing towards the spectator. Imagine first each vortex-ring to be in a portion of the fluid contained within a rigid rectangular box, of which four sides are indicated by the fine lines crossing one another at right angles throughout the diagram; and the other pair are parallel to the paper, at any distance asunder we like to imagine. Supposing the volume of rotationally moving portion of the fluid constituting the ring to be given, there is clearly one determinate shape, and diametral magnitude, in which it must be given in order that the motion may be steady. Let it be so given, and fill space with such rectangular boxes of vortices arranged facing one another oppositely in the manner shown

Fig. 1.

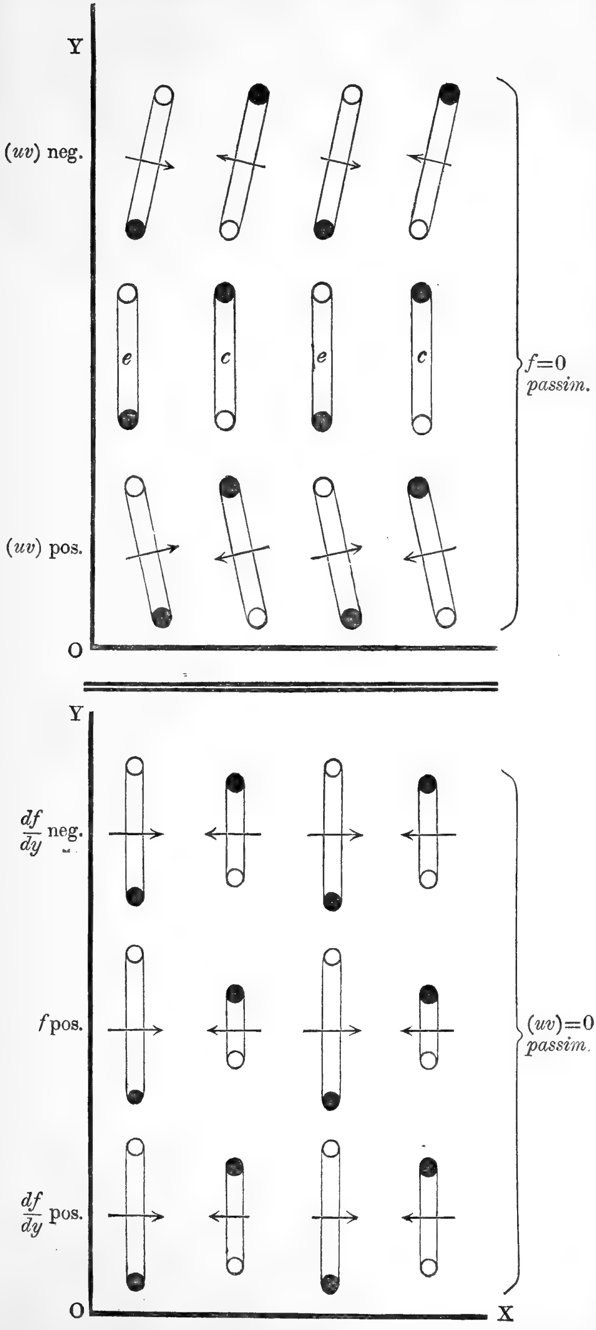


in the diagram. Annul now the rigidity of the sides of the boxes. The motion continues unchangedly steady. But is it stable, now that the rigid partitions are done away with? No proof has yet been given that it is. If it is, laminar waves, such as waves of light, could be propagated through it; and the velocity of propagation would be $R\sqrt{2/3}$ if the sides of the ideal boxes parallel to the undisturbed planes of the rings are square (which makes ave $u^2 =$ ave w^2), and if the distance between the square sides of each box bears the proper ratio to the side of the square to make ave $v^2 =$ ave $u^2 =$ ave w^2.

23. Consider now, for example, plane waves, or laminar vibrations, in planes perpendicular to the undisturbed planes of the rings. The change of configuration of the vortices in the course of a quarter period of a harmonic standing vibration, $f(y, t) = \sin\omega t \cos\kappa g$ (which is more easily illustrated diagrammatically than a wave or succession of waves), is illustrated in fig. 2, for a portion of the fluid on each side of $y=0$. The upper part of the diagram represents the state of affairs when $t=0$; the lower when $t=\pi/(2\omega)$. But it must not be overlooked, that all this §§ 22, 23 depends on the unproved assumption that the symmetrical arrangement is *stable*.

24. It is exceedingly doubtful, so far as I can judge after much anxious consideration from time to time during these last twenty years, whether the configuration represented in fig. 1, or any other symmetrical arrangement, is stable when the rigidity of the ideal partitions enclosing each ring separately is annulled throughout space. It is possible that the rigidity of two, three, or more of the partitions may be annulled without vitiating the stability of the steady symmetric motion; but that if it be annulled through the whole of space, for all the partitions, the symmetric motion is unstable, and the rings shuffle themselves into perpetually varying relative positions, with *average homogeneousness*, like the ultimate molecules of a homogeneous liquid. I cannot see how, under these conditions, the "vitiating rearrangement" referred to at the end of § 20 can be expected not to take place within the period of a wave or vibration. To suppose the overall diameter of each ring to be very small in proportion to its average distances from neighbours, so that the crowd would be analogous rather to the molecules of a gas than to those of a liquid, would not help us to escape the vitiating rearrangement which would be analogous to that investigated by Maxwell in his admirable kinetic theory of the viscosity of gases. I am thus driven to admit, in conclusion, that the most favourable verdict I can ask for the propagation of laminar waves through a turbulently moving inviscid liquid is the Scottish verdict of *not proven*.

Fig. 2.



Here (uv) means an average of the kind described in the footnote on (46).

XLVI. *On the remarkable Relationships between the Spectrum of Water-vapour and the Line-spectra of Hydrogen and Oxygen, and on the Chemical Structure of the two latter, and their Dissociation in the Sun's Atmosphere.* *By* Prof. A. GRÜNWALD[*].

BY means of a mathematical investigation into the changes which the properties, and in particular the spectra, of two gases undergo upon their chemical combination to form a new substance, I have succeeded in discovering the law, as simple as it is important, of a new mathematico-chemical disturbance-theory; and by its aid in finding very remarkable relationships between the spectra of hydrogen and oxygen on the one hand, and of water-vapour on the other, as well as discovering the chemically compound nature of hydrogen and oxygen by mathematico-spectral analysis, and in demonstrating the dissociation of hydrogen in the sun's atmosphere.

I.

The above-mentioned fundamental theorem is as follows:—

"Let a be a primary chemical element, which is chemically combined with other elements in a gaseous substance A, and occupies the volume $[a]$ in the unit-volume of A. Let the substance A combine chemically with another gaseous substance, B, to form a third, C. In this combination let the element a pass into a different chemical condition, a', giving up (or in exceptional cases taking up) a certain quantity of heat in order to permit the new compound to form, and in consequence chemically contracting (or exceptionally expanding). Let the volume which it occupies in the body C, after the new condition of chemical equilibrium has been established, be $[a']$, when the quotient $[a] : [a']$ is generally a very simple rational number in accordance with a known fundamental law of chemistry. If this is the case, the wave-lengths λ of all the rays which belong to the element a in the line-spectrum of the free substance A, and are therefore radiated by it, are related to the wave-lengths λ' of the corresponding rays which the same element emits in the new chemical condition a', in which it exists in the more complex substance A within the newly formed compound C, as the corresponding volumes $[a]$ and $[a']$."

This theorem only holds good when the substances in question are gases which are at a considerable distance from the critical point of condensation, and are not under too great

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[*] Communicated by the Author from the *Astr. Nachr.* Bd. 117.

pressure. The modifications which it requires when these conditions are not fulfilled, as well as the simple and natural assumptions upon which the proof of the theorem rests, can only be given in a fuller communication.

From the above proposition it follows in particular that for $[a']=[a]$, *i. e.* if the volume of the element a in the new compound C is the same as in the original substance A, the wave-lengths λ' of the rays emitted by a' must be equal to the wave-lengths λ previously produced by a. The only difference will consist simply in a varying change in the amplitudes and consequently in the intensities of the individual rays; from which it results that the brightness of a whole series of rays is so much diminished that they cease to be visible, whereas others become brighter. Thus the spectra of the compounds formed by the union of hydrogen with chlorine, bromine, and iodine (viz. HCl, HBr, HI) consist only of the spectra of hydrogen on the one hand, and of chlorine, bromine, or iodine on the other, but with characteristic changes of intensity of the various rays of their components.

The limitation to the gaseous condition is necessary because, as in chemistry generally so here, easily observable relationships exist only for gases. If the pressure increases, or if the gaseous substances in consequence of falling temperature (or with simultaneous increase of pressure) approach the temperature of liquefaction, the number of impacts between two different molecules will be increased under otherwise similar conditions; and the number of vibrations of the equivalent smallest particles of the different similarly constituted molecules of the same substance are altered in varying fashion by varying impacts. Whilst in the former case equivalent corresponding smallest particles of different similarly constituted molecules of the same substance emit extraordinarily feeble rays of equal wave-length, which in the spectrum are superposed, and by addition of their extremely small intensities produce a single ray of perceptible brightness, in the latter case equivalent particles of different like-constituted molecules emit not only rays of normal period, but also rays of periods more or less different from the normal, which arise from the impacts of different kinds. Together with the normal line, we have close to it rays of the latter kind—that is, the original sharp line becomes widened. If the pressure increases and the gases approach their point of liquefaction, such a line becomes still further widened; and it may happen that the resulting band-like line may extend as far as lines due to heterogeneous particles of the different molecules of the same substance. The lines of the spectrum run together, and there results a more or less continuous spectrum, in which only some light-maxima show the position of the former chief lines of the normal spectrum.

II.

The most important of the highly remarkable relationships between the line spectra of H, O, and H_2O vapour are :—

(1) "All the waves of the second or so-called compound-line spectrum of hydrogen, which have been so admirably determined by Dr. B. Hasselberg, of Pulkowa, and which are due to a more complicated structure of H molecules, may (after exclusion of the lines belonging to the elementary line-spectrum) be converted into corresponding wave-lengths of the water-spectrum by multiplying by $\frac{1}{2}$."

This proposition, which was discovered at first empirically (for a large number of rays) by comparison of the second hydrogen-spectrum with the water-spectrum as far as known at the time, and then theoretically by means of theorem I., recognized as holding good generally, is, according to I., a simple consequence of the fact that the modified hydrogen-molecule H' in H_2O-vapour occupies exactly half the volume it occupies in the free condition. This observation put me in possession of a large number of previously unknown wave-lengths of the water-spectrum, of those namely which hydrogen produces under the influence of the oxygen in water-vapour, and enabled me to send (on May 9, 1887) to Prof. G. D. Liveing, at Cambridge, a long list of hitherto unknown wave-lengths of the water-spectrum for the purpose of experimentally testing the correctness of my mathematico-chemical disturbance-theory ; a prediction which, as I am informed by letters of the 19th and 21st of June from Prof. Liveing, has been verified in the most satisfactory manner, as far as the observations have yet been made. The correspondence is shown for the rays from $\lambda=2800$ to $2607{\cdot}8$ and from $\lambda=2603$ to 2449, in the following Table.

It has not yet been possible to verify the rest of the lines predicted by me on the 9th of May, from $\lambda=2449$ to $\lambda=2207$, on account of their extreme feebleness. Below 2200 Messrs. Liveing and Dewar have not yet been able to detect any lines of the water-spectrum. Nevertheless, according to my theory, there must exist a still more refrangible section of the water-spectrum of altogether extraordinary feebleness, the wave-lengths of which correspond to the wave-lengths of the hydrogen-spectrum given by Dr. B. Hasselberg in his *Zusatz zu seinen Untersuchungen über das* II. *Wasserstoffspektrum*, 1884.

Letter of June 19.

Predicted.	Observed.	Predicted.	Observed.
2800·5	2799·8	2682·5	2683·5
2798·5	2797·6		2683·0
2792·5	2793·8	2677·5	2677·3
2768·5	2768·3	2672	2671·1
2759	2759·7	2667·5	2668·2
2753	2753·1		2666·0
2749	2748·2	2660	2659·6
2742	2742·7	2652	2651·3
2731·5	2732·1	2646·5	2645·6
	2730·5	2642·5	2642·2
2718·5	2718·2		2640·4
	2717·2	2637·5	2638·5
2713·5	2713·6		2636·8
2710	2709·5	2633	2633·3
2705·5	2706·2	2629·5	2631·1
	2705·2	2623·5	2624·1
2699·5	2701·5		2623·3
	2699·7	2615·5	2614·9
2693·5	2693·7	2612	2611·0
	2693·2	2607·5	2608·3
2687	2687·6		
	2687·2		

Letter of June 26.

Predicted.	Observed.	Predicted.	Observed.
2603	2603·2	2516	2517·6
2599	2598·6		2515·1
2595·5	2596·4	2508·5	2509·0
2591	2591·3		2508·0
2585	2584·4	2491	2491·1
2578·5	2578·3	2487·5	2487·2
2572·5	2573·4	2484	2484·9
2564·5	2565·4		2483·7
2556	2556·4	2479	2479·3
2542	2542·7	2473·5	2474·5
2535	2536·6	2467·5	2467·1
	2534·1	2464·5	2464·5
2532·5	2531·4	2460·5	2460·0
2528	2529·2	2454·5	2454·7
2525	2524·2	2449	2449·3
2520·5	2521·7		
	2519·8		

The wave-lengths of the relatively somewhat brighter lines of this section of the water-spectrum are 2248·7, 2244·8, 2230·1, 2223·5, 2205·8, 2173·5, 2169·4, 2111·0, 2110·8, 2105·9, 2102·2, 2099·6, 2097·5, 2090·8, 2089·7, 2088·2, 2087·2, 2085·3, 2078·0, 2038·7, 2034·6, 2033·2, 2031·0.

The experimental verification of this prediction can only succeed with an altogether special arrangement of apparatus. Possibly the use of several sparks or flames, one behind the other, striking through moist gas (or a broad stream of water-vapour)*, and the observation of the rays emitted in the direction of the line joining them, might succeed, since the rays emitted by the more distant sparks (or flames) would strengthen the vibrations which were produced by those nearer the observing-telescope. It might also be advisable, in accordance with a suggestion of my colleague, Prof. Zenger, to bathe the photographic plates in a solution of chlorophyll as concentrated as possible, and then to dry them.

(2) "The wave-lengths of the elementary line-spectrum of hydrogen may be divided into two groups, (*a*) and (*b*); so that the wave-lengths of the one group (*a*) are transformed into corresponding wave-lengths of the water-spectrum by multiplication by the constant 0·6336 (very nearly $\frac{19}{30}$), and those of the other group (*b*) by multiplication of the constant $\frac{4}{5}$."

From this it follows, by means of theorem I., that hydrogen consists of two primary elements, a and b; of which the one, a, produces the group (a) under the influence of b; the second, b, produces the group (b) under the influence of a. If $[a]$ and $[b]$ are relatively the volumes which the substances a and b occupy in a unit-volume of hydrogen, then

$$[a]+[b]=1,$$

and, further,

$$\tfrac{19}{30}[a]+\tfrac{4}{5}[b]=\tfrac{2}{3},$$

whence

$$[a]:[b]=4:1, \quad [a]=\tfrac{4}{5}, \quad [b]=\tfrac{1}{5}.$$

Hence hydrogen is a compound of one volume of the primary substance b, with four volumes of the other primary substance a: $H=ba_4$. It is thus a compound analogous to ammonium, NH_4, of which the volume will increase in the ratio 2 : 3 upon dissociation at a sufficiently high temperature.

The substance a is the lightest of all gaseous substances—much lighter than hydrogen; and the substance b is, if we assume a to be a monovalent element, also a gaseous element and pentavalent similar to nitrogen. The line-spectra of the

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* Or better, of several such hydrogen flames in oxygen gas.

primary substances *a* and *b* may now easily be calculated by means of (I.) from the corresponding groups (*a*) and (*b*) by multiplication with the factor $\frac{3}{2}$, in which operation, unfortunately, the errors of the wave-lengths increase in the same proportion. The star-spectra described by Mr. Huggins in the Philosophical Transactions (1880) vol. clxxi. part ii. pp. 669–690, those namely of α Aquilæ and Arcturus, of which the most refrangible rays are hydrogen-rays, and by comparison with the water-spectrum in accordance with (2) and with the solar spectrum may be confirmed as hydrogen-lines and corrected, enable us to fully complete the hydrogen-spectrum as far as known in the ultra-violet portion, and to construct a very complete spectrum of the primary element *b*. I give here the spectra which I have calculated for the elements (*a*) and (*b*) of hydrogen, comparing them with the corresponding Fraunhofer- and chromosphere-lines.

The wave-lengths of the Fraunhofer-lines were taken at once from a good copy of Angström's Atlas, and those of the chromospheric lines from Young's second list. I propose to go over the calculations and comparisons as soon as I have at my disposal Thollon's *Nouveau Dessin du Spectre Solaire*, Piazzi Smyth's 'The Visual Solar Spectrum in 1884,' and especially A. Rowland's 'Photographic Map of the Solar Spectrum.'

Spectrum of element *a*.	Solar spectrum of Ångström.
$\lambda = 9842 \cdot 4$	
7290·2*	7290
6510·2	6511
6150·6	6150·5
5653·5 &c.	5653·3

(With possible errors amounting to a unit of Ångström's scale. The first two wave-lengths are much more exact than the following ones, because they are derived from the wave-lengths of H_α and H_β, as measured with the greatest care by Mendenhall.)

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* Coincides nearly with a telluric line 7290·3 (Fievez).

Spectrum of element *b*.	Solar spectrum, according to Angström.
5954	5953·9
5951·8	5951·6
5943	5943·5
5940	5940
5924·4	5923·5
5916	5915·4
5904	5904·6
5899·3	5899
5897	5897
5892·1	5892·1
5886	5885·5 5886·5
5874·5	Chrom. 5874·9 Helium-line.
5855·4	5855·2
5833	5832·5
Several lines from 5808 to 5805	5808·2 5807·2 5805·7 5804·4 5803·5
Several lines from 5793 to 5784	5793 5792·4 5790·3 5786·8 5784·5 5783·0
5775	5774
5752·5	5752·2 5751·8
5694·1	5694
5618·2	5618·0

Spectrum of element *b*.	Solar spectrum, according to Angström.
5593·3	5593
5577·3	5577·5
5560·1	5559·6
5535·6	5536·2
5524·3	5524·8
5515·9	5515·5
5511	5511·2
5494·1	5493·8
5485·5	5485·6
5482·3	5482·5
5479·5	5479·9
5455·1	5454·8
	Chrom. 5454·7
5417·2	Chrom. 5417·9
5412·6	Chrom. 5412·4
5403·7	Chrom. 5403·1
	5403·0 to 5403·3
5389·5	5389·6
5376·4	5376·5
5364·0	5364·0 *Fe.* 5364·2
5340·0	Chrom. 5340·2 *Fe.?* Mn.?
5326·5	5327·0 *Fe.*
5274·1	5274·7
5260·1	5259·6
5199·9	Chrom. 5199·7
5187·0	Chrom. 5187·3 Probably titanium.
	&c.

Although the fundamental law I. was originally obtained only for compounds of primary elements, it is important to observe that it holds very nearly for compounds of compound radicals, so far as these really behave as "atoms." A proof of this, which is at the same time interesting and an important confirmation of my theorem, is the fact that the wave-lengths of all the hydrogen and oxygen rays, if multiplied by $\frac{2}{3}$ (the mean condensation-factor of H and O upon their combination to H_2O), are converted into the wave-lengths of corresponding H_2O rays, at least so far as the H_2O spectrum is known, and can be compared with them.

(3) In the search for the reduction-factor of the primary constituent *a* of hydrogen to the H_2O spectrum several remarkable relationships were obtained. It appeared that the ray 4158, into which the chief ray H_α 6562 of H is converted by multiplication with 0·6336, is an hitherto unknown (as such) ray of the water-spectrum, and that by means of the factor $\frac{20}{27}$

it yields another strong ray ($\lambda=3080$) of the H_2O-spectrum. The ray 4158 occurs, namely, amongst a group (3986, 4005, to 4645) of rays which H. W. Vogel has observed in the spectrum of rarefied hydrogen (whether obtained electrolytically or from potassium formiate), the derivation of which from hydrogen was, however, very doubtful. In view of the fact that in these methods of preparation of hydrogen it is difficult to avoid the presence of traces of water-vapour, it was a probable assumption that at least some of the doubtful rays, and amongst them in particular the ray 4158, might arise from the traces of water-vapour contained in this hydrogen, and might be rays in themselves very weak in presence of hydrogen, but which, being reinforced by harmonic vibrations of the latter, become visible. If this surmise were correct, it might be expected that the wave-lengths of those of Vogel's rays which belong to the same group of H_2O rays as 4158 might also be reduced to the H_2O-spectrum by multiplication with the factor $\frac{20}{27}$. The supposition was verified in a remarkable manner, in particular for the rays 3986, 4005, 4047, 4065, 4067, 4078, 4152, 4158, 4168, 4193, 4201, &c., so that these belong to the H_2O-spectrum; and by division by 0·6336 are converted into very feeble rays of hydrogen, hitherto unknown, with the exception of H_α, which on account of their extreme feebleness have escaped observation. Their approximate wave-lengths are placed together with the nearest lines of the solar spectrum, as given by Ångström.

H-rays.	Solar spectrum (Angström).
6291	6291·4
6321	6321·5
6387·3	?
6415·7	6415·6 (Chromosphere, Young).
6418·9	6418·7
6436·2	(6438 ? Ca)
6553·0	?
H_α 6562	6562
6578·3	6576·9 ? (Chromosphere, Young).
6617·7	?
6630·3	?

With a possible error of 1·6 of Ångström's unit.

A careful comparison of the spectra of hydrogen intentionally mixed with various very small quantities of H_2O-vapour is much to be desired, and ought to produce the experimental confirmation of the origin of the rays 3986, 4005, 4007, ... 4201, &c. The ray 4158 lies very near to a very weak ray, 4158·7, of the second so-called compound spectrum of hydrogen observed by Dr. B. Hasselberg, it indeed the latter be not identical with it, having its origin in the presence of a trace of aqueous vapour. The same ray was moreover observed in air and in oxygen, in the mode adopted by Plücker.

(4) α. "The wave-lengths of the elementary line-spectrum of oxygen may be divided into two groups, (H') and (O'), of which the first consists of certain rays of the second compound H-spectrum, which are converted into the corresponding rays of the water-spectrum by multiplication with $\frac{1}{2}$."

β. "The groups (O') may be divided into two groups, (b') and (O''); so that the one (b') by multiplication by $\frac{23}{32}$ passes into a group of the corresponding wave-lengths of the H_2O-spectrum, but by multiplication by $\frac{41}{46}$ into a group of homologous wave-lengths of the primary substance b chemically combined in H, whilst the other group (O'') may be converted into a corresponding group of the H_2O-spectrum by multiplying it by $\frac{5}{8}$."

γ. Finally, the last group (O'') may again be resolved into two groups (b'') and (c) of wave-lengths. The first of these (b'') multiplied by $\frac{21}{32}$ gives a group of the H_2O-spectrum, and multiplied by $\frac{59}{70}$ is reduced to a corresponding group of the H-spectrum which belongs to the primary substance b in H.

The second group (c) by multiplication by $\frac{3}{5}$ is transformed into a homologous group of H_2O-rays."

From these most important and remarkable relationships and characteristic data, which are not derived from single rays, but by the comparison of whole series of numerous rays of oxygen, H_2O-vapour, and hydrogen, we obtain by means of the fundamental theorem I. easily, and at once, the chemical structure of oxygen in the interval of temperature in which it radiates the elementary line-spectrum. If in fact H', O', b', O'', b'', c be the substances which, in their combined condition within the oxygen, emit the above similarly-denoted groups of rays, $(H)'$, (O'), (b'), (O''), (b''), and (c), and $[H']$, $[O']$, $[b']$, $[O'']$, $[b'']$, and $[c]$ be the volumes which they occupy in a unit-volume of oxygen, we have the equations

$$[H'] = \tfrac{1}{2}, \quad [O'] = \tfrac{1}{2},$$

$$[b'] + [O''] = [O'] = \tfrac{1}{2};$$

and in accordance with theorem I., in view of the facts stated under (4)β,

$$\tfrac{23}{32}[b'] + \tfrac{5}{8}[\mathrm{O}''] = \tfrac{2}{3}[\mathrm{O}'] = \tfrac{1}{3},$$

whence

$$*[b'] : [\mathrm{O}''] = 4 : 5,$$

and

$$[b'] = \tfrac{4}{9}[\mathrm{O}'] = \tfrac{2}{9}, \quad [\mathrm{O}''] = \tfrac{5}{9}[\mathrm{O}'] = \tfrac{5}{18}.$$

But, further,

$$\tfrac{5}{8}[\mathrm{O}''] = \tfrac{5}{8} \times \tfrac{5}{18} = \tfrac{25}{144}$$

is the space which O″ occupies within the oxygen in H_2O-vapour; then

$$[b''] + [c] = [\mathrm{O}''] = \tfrac{5}{18};$$

and according to theorem I., taken with the facts of 4(γ), we have

$$\tfrac{21}{31}[b''] + \tfrac{3}{5}[c] = \tfrac{5}{8}[\mathrm{O}''] = \tfrac{25}{144};$$

whence, again,

$$**[b''] : [c] = 4 : 5,$$

and

$$[b''] = \tfrac{4}{9}[\mathrm{O}''] = \tfrac{4}{9} \times \tfrac{5}{18} = \tfrac{10}{81},$$

$$[c] = \tfrac{5}{9}[\mathrm{O}''] = \tfrac{25}{162}.$$

The chemical structure of oxygen in the above-named interval of temperature is therefore expressed by the chemical volume-formula

$$\mathrm{O} = \mathrm{H}'.\mathrm{O}', \quad \mathrm{O}' = b_4 . \mathrm{O}''_5, \quad \mathrm{O}'' = b_4 c_5.$$

$$\mathrm{O} = \mathrm{H}'.[b_4 \mathrm{O}''_5] = \mathrm{H}'.[b_4 . (b_4 c_5)_5].$$

That is to say, "Oxygen in its simplest molecular condition is a compound of the modified hydrogen H′, which radiates the second so-called compound line-spectrum of hydrogen, with a substance O′ in equal volumes without condensation. The latter O′ is a compound of four volumes of the pentavalent (nitrogen-like) element *b* of hydrogen in a special condition of chemical condensation with five volumes of a substance O″, which itself again consists of four volumes of the primary element *b* (in one of the former various chemical conditions) with five volumes of a new hitherto unknown primary substance."

As I have here deduced the chemical structure of H and O by a mathematico-spectroscopic method, so I should no doubt be able to determine the chemical structure of the so-called elements nitrogen and carbon by means of my chemical theory of disturbance, if I were furnished by skilful spectroscopists with complete and trustworthy lists of the wave-lengths of ammonia, NH_3, and of methane, CH_4, at the low temperature of electric discharge (to avoid dissociation) or the absorption-spectra of these gases. These spectra must extend in the ultra-violet portion as far as wave-lengths 1728×10^{-7} millim. and 1383×10^{-7} millim., and, if possible, still further.

Hitherto, so far as nitrogen is concerned, I have only succeeded in showing that it is in its simplest chemical condition (like oxygen) a compound of the modified hydrogen H' with a substance N' in equal volumes without condensation; and that the latter substance N' must have an entire large group of atoms in common with oxygen, inasmuch as a group of more than forty N-rays (*i. e.* their wave-lengths) may be transformed by multiplication by the factor $\frac{47}{46}$ into a corresponding group of O-rays (*i. e.* their wave-lengths).

The volume-formula of nitrogen has most probably the same form as that of oxygen, only written with smaller indices because the atomic weight of nitrogen (14) is less by 2 than the atomic weight of oxygen.

In carbon also I was able, by simple comparison of the spectrum with those of H_2O-vapour, of H, and of O, to establish the existence of the primary substance b in at least two different chemical conditions, as well as the occurrence of the primary substance c, which is also contained in oxygen.

(5) **α.** In the experiments which I made to determine the factor of reduction (0·6336, very nearly $\frac{19}{30}$) for the substance a in H to the H_2O-spectrum, I found that this factor is the mean or chief reduction-factor of the four atoms which in H are combined with the substance b; but that further special reduction-factors to the H_2O-spectrum belong to the separate atoms of these four, of which I succeeded in determining two, viz. $\frac{3}{4}$ and $\frac{56}{75}$, and in verifying them, at least so far as they could be compared with the known H_2O-spectrum.

If then λ be the wave-length of an H-ray of the elementary line-spectrum which is due to the substance a in H, then $0{\cdot}6336\,\lambda$ (empirically, theoretically more nearly $\frac{19}{30}\lambda$), $\frac{3}{4}\lambda$, and $\frac{56}{75}\lambda$ are with considerable accuracy three different wave-lengths of the H_2O-spectrum, if the latter is known with sufficient exactness.

β. If, on the other hand, λ be the wave-length of an H-ray which is produced by the substance b combined in H with the element a, then not only must $\frac{4}{5}\lambda$ be a wave-length of the H_2O-spectrum according to (2), but also according to (4) *β* (or (4) *γ*, as the case may be) $\frac{46}{41}\lambda$ and $\frac{70}{59}\lambda$ must be two wave-lengths of the O-spectrum; further, $\frac{23}{32} \times \frac{46}{41}\lambda$ and $\frac{21}{32} \times \frac{70}{59}\lambda$ must be two wave-lengths of the H_2O-spectrum, and recognizable as such, provided that the elementary O-spectrum is known sufficiently exactly, which is unfortunately not the case with very faint rays.

We are thus in possession of mathematico-spectroscopic criteria for the hydrogen-rays, which possess an unmistakable resemblance to the chemical reactions for the detection and recognition of substances.

III.

The Dissociation of Hydrogen in the Sun's Atmosphere.

A careful comparison of the lines of the above-described spectrum of the primary element *b* with the corresponding Fraunhofer and chromospheric lines, as given above, shows that the element *b* must occur in the free state in the sun's atmosphere, and must be identical with helium, of which hitherto only one ray, D_3 5874·9, is known. Hence, as well as by the close agreement of the lines of the *a*-spectrum, also given above, with corresponding lines of the solar spectrum, it is proved that hydrogen occurs in the sun's atmosphere in a dissociated condition.

But if this is the case the constituent *a* must not only be present wherever the dissociation takes place, together with the helium *b*, but as the lightest of all gases it must exist above those regions of the photosphere which can only be reached by the much heavier helium and the other elements exceptionally in powerful eruptions.

But if this occurs, and if in these very high and relatively cool regions the helium *b* and the primary substance *a* meet, then, under the extremely small pressure existing there, all the conditions are present for the formation of the modified ammonium-like hydrogen, which radiates the second compound hydrogen-spectrum. Such regions, which correspond to the extreme portion of the corona, will therefore specially exhibit the lines of the second hydrogen-spectrum together with those of ordinary hydrogen.

The hitherto unknown corona-substance appears to be a gas with the properties indicated above, which emits the so-far solitary known ray at 1474 of Kirchhoff's scale, or $\lambda = 5315{\cdot}9$ (about) of Ångström's scale, and which I may perhaps be allowed to call "Coronium." It is easy, then, to assume that the primary constituent *a* of hydrogen is identical with this coronium, which, in any case, must be a different substance from helium, since the corona-line 5315·9 often remains unaltered, whilst at the same time the helium-line D_3 is either broadened, or distorted and displaced, *i. e.* exhibits the changes in form observed by Lockyer. If this assumption is correct, $\lambda = 5315{\cdot}9$ (about) must be a wave-length of the above *a*-spectrum, which does not occur amongst the numbers found by calculation only because the wave-lengths corresponding to the element *a* in hydrogen are not completely known. In this case $\lambda = 5315{\cdot}9 \times \frac{2}{3}$ must be a wave-length of the elementary H-spectrum. It is true that the wave-length (about) $\frac{2}{3} \times 5315{\cdot}9 = 3544{\cdot}0$ (about) does not occur in the list of wave-lengths at present known as belonging to hydrogen; but if we take the extremely rich section of the H_2O-spectrum corresponding to the second hydrogen-spectrum as given by Dr. Hasselberg (*see* II. (1)) for the comparison, it satisfies in a very complete manner the criteria given (II. (5) α) for hydrogen-rays which belong to the substance *a* within the hydrogen, since $\frac{19}{30} \times 3544{\cdot}0 = 2244{\cdot}5$, $\frac{3}{4} \times 3544{\cdot}0 = 2658{\cdot}0$, and $\frac{56}{75} \times 3544{\cdot}0 = 2646{\cdot}1$ differ only by a few tenths of an Ångström unit (10^{-7} millim.) from the corresponding wave-lengths of the H_2O-spectrum: 2244·8, 2658·6, and 2645·4. The ray at about $\lambda = 3544{\cdot}0$ is, therefore, an hitherto unknown ray of hydrogen, the existence of which will probably be shown experimentally; $\lambda = 5315{\cdot}9$ is a wave-length of the primary element *a* (previously overlooked in this calculation for want of sufficient data), which is, therefore, probably identical with coronium. I must not omit to remark that the line 5317·28 of the second hydrogen-spectrum described by Dr. B. Hasselberg in 1883 lies very near to the corona-line 5315·9; and from what has been said above might possibly be observed, together with other lines of the latter spectrum, in the comparatively cooler regions of the corona far removed from the photosphere. If these two lines are really different, it must be possible, under favourable circumstances, to observe both lines at the same time in the corona—5315·9 in the hot portion nearer the photosphere, and the other in the less hot regions, further from the photosphere, for which observation a favourable opportunity offers in the total solar eclipse of the 18th August.

The like holds good of the known chromosphere-line $\lambda = 4471{\cdot}2$ of Rayet (Lorenzoni's *f*), which differs by only 0·3 from the nearest line of that part of the second hydrogen-spectrum which Dr. B. Hasselberg has described in 1884 (*Zusatz zu meinen Untersuchungen*), viz. the line $\lambda = 4470{\cdot}9$; so also with the chromosphere-line 4712·5, which, like the former line 4471·2, is rather a band than a line, and like this is ascribed to Ce, but differs from the nearest line of the second H-spectrum only by 0·6.

The line 4471·2, which I was disposed to take for a helium-line, does not belong to helium, because it does not satisfy all the mathematico-spectroscopic criteria of helium-lines. This is also confirmed by the observations made by Prof. Young on August 3 and 5, 1872, in which, at the same time that D_3, 5015·0, 4571·4, &c., were very strongly disturbed, the lines 4471·2, 7055, and the corona-line 5315·9 remained unaltered. I wish particularly to observe, with reference to the forthcoming solar eclipse, that my calculations and comparisons show that the lines Fievez's 4612·5, 4564·5, Young's 5570, 5450, Tacchini's 4943, 5031, and (what is very important) two lines very near to the magnesium-lines b_1 and b_2, belong to helium; the line 7055 (?) very probably to coronium ; lastly, the lines observed May 17, 1882, by Capt. Abney and Dr. Schuster in Egypt (4501, 4473, 4401, 4224, 4212, 4195, 4179, 4085, and 4067), as well as at least one of the four red corona-lines (6489, 6492, 6494, 6498), seen simultaneously by Tacchini, viz. 6492 (if not all four), belong to the second compound spectrum of hydrogen.

The lines seen in the corona by the last-named spectroscopists and astronomers ought, therefore, in accordance with what has been said above, to originate in the comparatively cooler portions of the corona far removed from the photosphere, in which the elements of hydrogen separated in the hotter regions partly unite to form the common hydrogen, partly to form the ammonium-like hydrogen which produces the second hydrogen-spectrum.

P.S. Whilst passing through the press, a new and remarkable confirmation of the wave-lengths of the water-spectrum predicted by me on May 9, 1887, has reached me from Prof. G. D. Liveing, viz. $\lambda=2331·1$ to $\lambda=2437·2$.

Prague, July 17, 1887.

XLVII. *Notices respecting New Books.*

Coal-Tar and Ammonia. Being the Second and enlarged Edition of 'A Treatise on the Distillation of Coal-Tar and Ammoniacal Liquor.' By George Lunge, *Ph.D., Professor of Technical Chemistry in the Federal Polytechnic School, Zurich.* London: Gurney and Jackson, 1 Paternoster Row (Successors to Mr. Van Voorst). 1887.

WE had occasion to speak favourably of the first edition of this work when it appeared in 1882. At that time it consisted of 383 pages and 88 diagrams; it now forms a handsome well-printed volume of no less than 739 pages of letterpress and 191 diagrams.

Dr. Lunge, in his Introduction, notices the enormous reduction in the prices of coal-tar and its various products which has taken place since 1883. At that time coal-tar fetched at the gas-works 55*s.* and pitch 33*s.* to 35*s.* per ton, benzol, of 90-per-cent., was worth 3*s.*; whereas in 1885 tar had fallen to 12*s.*, and in 1886 to 7*s.* and 90-per-cent. benzol to 1*s.* 8*d.* per gallon. According to the, 'Journal of Gas-lighting,' tar, on August 27, 1887, was 10*s.* to 15*s.* per ton, and benzol, of 90-per-cent. 2*s.* 10*d.* per gallon; and this in spite of the fact that the great gas-companies are burning large quantities of their tar! It is not difficult to see the cause of this depreciation. It arises from the fact that the tar produced in Europe is more than is required by the makers of aniline colours and other tar products. The demand is not equal to the supply. The Introduction also contains much interesting matter, in the form of Historical Notes and Statistics, which we commend to the notice of all who are interested in the coal-tar industry.

As regards the various processes for obtaining coal-tar, the working of coke-ovens, the preparation of benzene from the various tars, including that obtained by subjecting petroleum to high temperatures, the work contains the fullest information, including diagrams of the ovens and other appliances, and lists of the patents bearing on the subject.

Chapter III., on "The Properties of Coal-tar and its Constituents," is not only very interesting, but contains more information in proportion to its length (60 pages) than in any other work we have seen.

The question of burning tar as fuel, now so much agitating gas-companies, is treated fully, and excellent illustrations are given of the apparatus best adapted for the purpose.

Chapter V. is on "The First Distillation of Coal-tar," and is treated with a fullness and clearness which leaves nothing to be desired.

Chapter VI. is on "Pitch," and in it we have instructions for the testing of the various qualities. We do not remember to have seen these methods in print previously.

Chapter VII. is on "Anthracene Oil." All the best methods of extracting the anthracene and testing it for its purity are given. The illustrations of filter-presses, washing-apparatus, &c. are excellent. The only part of this section which leaves anything to be desired is the estimation of the quantity of anthracene in tar. Unfortunately no thoroughly reliable mode of doing this has, so far as we know, yet been made public.

Chapter VIII. is on "Creosote Oil." This section is very complete. The various processes which have been devised for decomposing the oil by heat, so as to obtain more valuable products, are described and illustrated. The attempts to utilize creosote oil for lighting are described, and diagrams of the apparatus devised by Hartmann and Lucke, and Lyle and Hannay are given. The section on the "Employment of Creosote Oil for Pickling Timber" runs to 24 pages; and the specifications of Abel and Tidy, and also the views of Mr. S. B. Boulton (who is, perhaps, the highest authority on the subject living) are given. We should have liked, in this place, to see Dr. Lunge's views as to the part played by the organic alkaloids in creosote oil in the preservation of pickled timber.

Chapter IX. is on "Carbolic Acid and Naphthalene." The methods of preparing carbolic acid of various degrees of purity are given in considerable detail, and also the most reliable modes of testing the various commercial products. The best plans for subliming and distilling naphthalene are also fully described. This chapter extends to no less than 56 pages, every one of which is interesting and useful.

Chapter X. is on "Light Oil and Crude Naphtha." This portion does not admit of any specially original treatment, as the methods now in use are not very different from what they were some years ago.

Chapter XI., on "Rectifying by Steam; Final Products," is of great value, and will be greatly appreciated by practical tar-distillers. The stills best adapted for the purpose are represented by engravings, and explained in a clear and simple manner. Savalle's and Siemens's apparatus are described very fully, and the directions for their use are sufficient to enable any chemist to work them. The various kinds of benzols, toluols, and naphthas of commerce, with their boiling-points, and the apparatus used for determining the latter, are all given, as also the processes for determining the various constituents of coal-naphtha. The chapter concludes with a synoptical table of the distillation of coal-tar.

Chapter XII. commences the second part of the work, which is on Ammonia, by an exhaustive account of the various sources from which that substance is obtained. Then follows Chapter XIII., which gives the composition and analysis of ammoniacal liquor and the properties of its constituents. Chapter XIV. is on "The Working-up of Ammoniacal Liquor. The work concludes with an Addenda giving a large amount of varied information, much of which will doubtless be incorporated with the earlier articles in subsequent editions.

This work of Dr. Lunge is by far the most extensive and the best which has appeared in our language on the products of coal-tar, and will be indispensable to every one interested in the important subject of which it treats.

XLVIII. *Intelligence and Miscellaneous Articles.*

ON A NEW POLAR ACTION OF MAGNETISM ON HEAT IN A PLATE TRAVERSED BY A GALVANIC CURRENT. BY A. VON ETTINGSHAUSEN.

SOME time ago I brought before the Imperial Academy a phenomenon observed by myself and Nernst (Phil. Mag. Sept. 1887,

p. 291); which is that electromotive forces are produced in a plate of bismuth, antimony, cobalt, nickel, or iron, traversed by a current of heat and brought into a powerful magnetic field in such a manner that the lines of force cut the plate at right angles. We called the phenomenon "*thermomagnetic effect*," and in bismuth we observed both a *transverse* current, one, that is to say, at right angles to the current of heat, and a *longitudinal* one, that is, acting in the direction of the current; the former, which is by far the stronger, changes its direction when the direction of the current to which the magnetic field is due is reversed.

It was obvious to attempt a reversal of the phenomenon, that is, to demonstrate an inequality of temperature in a plate traversed by a voltaic current, which was produced by the action of magnetic forces. I used a bismuth plate 3·1 centim. in length, 2·4 centim. in breadth, and about 0·04 centim. in thickness; stout copper wires were soldered to the whole extent of the short sides of the copper plate; they served to transmit the current J through the plate; in the middle of one long side the junction of an argentan-copper thermoelement was soldered to the plate, while the other junction dipped in a vessel of water kept at the temperature of the room. The thermoelement was connected with a reflecting galvanometer. The current of two Bunsen's elements was passed through the plate, the intensity of which could be varied by a rheostat with a stout wire, and could be measured by a reflecting tangent-galvanometer. Owing to heating of the plate, a deflection of the needle is observed, which soon becomes stationary; the loss of heat by the plate is diminished by enclosing it in wadding.

The plate was placed between the elongated pole-pieces of an electromagnet. If the latter is excited, a change in the setting of the needle is observed, and after some time ($\frac{1}{2}$-1 minute) it acquires a pretty constant position; on opening the magnetizing current, the needle reverts to its former position. The difference of the adjustments is obtained with sufficient accuracy to read off each time half a minute after the opening or closing of the magnetizing current. Experiment showed that the positions of the needle were on *opposite* sides of the stationary positions if the direction of the currents was alternated; and further that the deflections changed with the direction of the current J which traversed the plate.

The direction of the deflections showed that the temperature of the junction fastened to the plate was always *raised*, when we reach the junction from the place where the current J enters the plate by a motion in the same direction as the currents replacing the magnetic field. If the phenomenon could be regarded as a displacement of the stream-lines of the plate, by which the stream was condensed in one part and dilated in another, the displacement would take place in agreement with the ponderomotive action according to Ampère's rule. The deflections of the needle on each side are rather considerable (as much as 60 divisions), although when the direction of the current J is unchanged, the absolute value is not equally great for the two directions of the current which excited the electromagnet.

It will be seen from this statement that the heating or cooling does not occur in such a way that we can regard the experiment as a *reversal* of the transverse thermomagnetic phenomenon; from analogy with the Peltier effect the change of temperature must be exactly opposite that which observation gives. The thermomagnetic action of the bismuth plate was seen to be very powerful, and was quite normal; it manifested itself in such a way, that from the place where the thermal current entered the plate to the place where the thermomagnetic current enters, the motion is opposite to the direction of the field-magnets.

In order to keep the results free from objection I placed the two solderings of the thermoelement at two opposite places in the middle of the long sides of the bismuth plate, but both plates were carefully insulated from the plate by interposing thin sheets of mica.

After closing the current J only a slight alteration of the position of the needle could be noticed, as both solderings had almost the same temperature. The excitation of the magnetic field produced the action in the way given above, and with such regularity that measurements could be made. In the homogeneous field, $M=6400$ (C. G. S.); the deflections of the galvanometer needle i, corresponding to the differences of temperature of the solderings, where the strength of the current in the plate was $I=2\cdot49$ amperes, was

$$\text{(A)}\quad \left.\begin{array}{r} i=9\cdot0+ \\ 9\cdot2- \end{array}\right\}\text{mean } 9\cdot1. \qquad\qquad \text{(B)}\quad \left.\begin{array}{r} i=10\cdot5- \\ 10\cdot0+ \end{array}\right\}\text{mean } -10\cdot2.$$

(A) and (B) mean the two directions of the current in the plate; in (A) J flows from left to right through it. The two numbers under each other are the deflections of the needle in parts of the scale with opposite directions of the current which excites the electromagnet, measured each time from the position of rest of the needle when the magnetic field is not excited. In the first case (the upper numbers), for an observer looking at the plate, the field currents are in the direction of the hands of a watch (south pole behind the plate); the signs $\pm$ signify that the deflections of the galvanometer needle are towards the great or small numbers of the scale. A deflection towards the large numbers represents an increase of temperature of the soldering of the thermoelement at the upper edge of the plate over the temperature of the other. If no current flowed through the bottom bismuth plate ($J=0$), then, when the direction of the field current was reversed, there was no action on the thermoelement. For $J=5\cdot72$ the mean of the deflections at A was 17·0, at B 19·3 divisions, hence the difference of temperature at the top and bottom edges (for the same M) is proportional to the intensity, J, of the current in the plate.

The observations were still more successful with a double plate of bismuth. Two equal plates, each 0·043 centim. thick, were placed with their surfaces parallel and near each other, so that the current J was divided between them. The distance of the plates was about 1 millim., and the solderings of the thermoelement were interposed in the space at the upper and lower edges of the plate, and carefully insulated by thin laminæ of mica.

With a strength of the magnetic field M=6400, and for J equal to 1·76 amperes, for the undivided current the mean of the deflections of the needle was $i=17·3$ divisions; for J=3·28 amperes $i=32·8$, so that the intensity is nearly proportional to the strength of the current passing through the double plate

For M=4290, J=3·05 amp., $i=20·3$ divisions,
6250, 3·05 29·9

and the action is thus also proportional to the intensity of the magnetic field.

If, instead of the *galvanic* current, a thermal current was passed through the plate, the thermoelements interposed between the plates did not show the smallest difference of temperature when the magnetic field was excited in either direction.

In order to ascertain whether the new phenomenon is seen in different bismuth plates in different intensity, and especially if in bismuth, which produces Hall's phenomenon in greatest strength, the galvanomagnetic difference of temperature is greater, I investigated a second double plate II.; each single plate was 7 centim. in length, 1·9 centim. in breadth, and about 0·08 centim. in thickness: the double plate I. previously mentioned was made of the same breadth as II.

I made successively experiments with each double plate and two different intensities of fixed M=4400 and 6310: for the same total intensity of the current J which traverses the plate the differences of temperature produced at the edges of the plates in I. and II. were about in the ratio 1·92: 1.

The rotatory powers of the bismuth plates were very different for the same M; the bismuth of the double plate which showed the greater galvanometric difference of temperature had the smaller rotatory power. On the other hand, the ratio of the sections of I. and II. is nearly 1: 1·9, and hence for the same strength of current, the *density of the current* has a preponderating influence on the strength of the effects in bismuth.

I observe in conclusion that the difference of temperature which was produced by the magnetic field M=6310 at the edges of the double plate I. for the total strength of current J=4·55 amperes amounted to about 1°·3 C.: the electromotive force of my thermoelement for a difference of 1° C. in the solderings was very nearly 11 microvolts.—*Berichte der Kaiserlichen Akademie in Wien*, January 13, 1887.

ON THE ACTION OF MAGNETISM ON ELECTRICAL DISCHARGES IN DILUTE GASES. BY PROF. BOLTZMANN.

A flat Geissler's tube in which there was a pressure of 2–5 millim. was placed in a homogeneous magnetic field; its section at right angles to the lines of force was nearly a rhomb with diagonals of 6 and 4 centim.; its thickness was about 2 centim. The electrodes were fixed at the corners of the rhomb; in tube I. platinum wires were fused in, so that the wires were in the magnetic field; in the other, II., there were fused glass tubes, in which the wires were sealed some distance outside the magnetic field. The induction current of a small Ruhmkorff's coil, with a striking distance of 1 centim., passed through the acute angles of the rhomb (primary electrodes), while the other two electrodes (the transverse electrodes) were connected with a fine-wire galvanometer. The luminous phenomenon in a Geissler's tube, as is well known, is deflected by magnetism in the same way as a wire traversed by a primary current would be, in accordance with Ampère's rule; but with a symmetrical position of the tube, and symmetrical position of the transverse electrodes towards the line of junction of the primary current, no conclusion could be drawn, whether, and in what direction a current could be produced by magnetism in the circuit which joined the transverse electrodes.

Experiment showed now that a current was always produced there, and the place at which the positive current emerged from the tube was always at that transverse electrode from which the band of light was repelled. If therefore this phenomenon is to be compared with that of Hall (Phil. Mag. (5) vol. ix. p. 225), air would behave like bismuth or gold.

If the tube was filled with H or CO_2 of almost the same pressure, these gases showed neither qualitatively nor quantitatively a demonstrable difference in comparison with air. In tube I. the current between the transverse electrodes was in the mean about the sixtieth, and in the maximum about the thirtieth of the primary current with a field of about 1800 (C.G.S.); yet this number can only give an approximation to the order of magnitude, since the circuit to the galvanometer was quite inadequately insulated for currents of such potential; and since from the electromotive force, which according to Edlund appears at the electrodes, the intensity of the current could not be taken as proportional to the electromotive force of the primary or the transverse current. In tube II. the transverse current was much smaller, probably because in addition to the rhomb it had also to traverse the narrow glass tubes.

In these experiments the inductorium, and the battery which worked it (2–3 chromic acid elements), were insulated on sealing-wax rods, so that without the action of magnetism electricity could neither enter nor emerge in large quantities through the transverse electrodes. This arrangement is quite in correspondence with that which Hall chose for observing the phenomenon which he discovered. I also made experiments in which one of the electrodes at the acute angle of the rhomb was not used; the primary current entered the tube through the other, and divided into two parts, one of which passed through one, and another through the other, transverse electrode, and which traversed a differential galvanometer in opposite directions. Here were seen all the phenomena which Righi observed in metals with the same arrangement. Magnetism produced also another kind of transverse current. The current which traversed the transverse electrodes before the action of magnetism, was increased at that electrode towards which the band of light was driven, and weakened at the other; a transverse current was thus produced in the galvanometer, which had the same direction at that transverse electrode towards which the strip of light was urged, and thus had the same direction as the primary current. It hence changed direction on reversing the field, but not on changing the primary current.

This latter transverse current was not weaker in tube II. than in tube I., or at all events not materially so. It was seen even with the ordinary (Hall's) arrangement with four electrodes, as soon as one of the primary electrodes was put to earth or was imperfectly insulated, for then only one portion of the chief current passed through the second (imperfectly insulated) primary electrode; another part flowed through the two transverse electrodes and the galvanometer-circuit, which was also imperfectly insulated from the earth. By the latter portions of the current all the phenomena which occur in Righi's arrangement would be produced.

As the well known fact that the action of a magnet hinders the passage of the current through Geissler's tubes, seemed analogous to the increase of resistance of bismuth in the magnetic field, I made an experiment in this direction. The primary current passed simultaneously through tube II. and another Geissler's tube; without the action of magnetism it divided almost uniformly between the two; but by the action of the magnetic field the division of the current was altered, as it would be on the law of branch currents, if the resistance in tube II. had been increased tenfold. —Wiedemann's *Annalen*, vol. xxxi. p. 789.

SOME EXPERIMENTS ON THE TRANSMISSION OF ELECTRICAL CURRENTS IN AIR. BY J. BORGMANN.

This paper is to be considered as a preliminary communication, in which the author describes some experiments which he has undertaken in order to ascertain the manner in which the electrical current is transmitted through air.

If one conductor of an electrical machine is put to earth, and the other is connected with an insulated point or flame, and if one end of a galvanometer-wire is put to earth while the other is connected with the second flame, an electrical current is produced as soon as the machine is worked. Lighting the flame did not affect the needle of the galvanometer; electrostatic induction was completely avoided, as the needle was completely surrounded by a copper damper, and the latter was put to earth.

It was found that when the machine was rotated with a constant velocity the galvanometer showed a constant current, the direction of which changed with the direction of the poles of the machine; it was observed that these opposite currents were never equal to each other. Wiedemann's galvanometer (Edelmann's construction) was used (one division corresponds to 5·99 . 10^{-8} ampere); or a specially constructed pure metallic galvanometer of W. Lermantow, with a bell-magnet (one division corresponding to 2·21 . 10^{-8} ampere without astatic magnet, and 2·05 . 10^{-9} ampere with astatic magnet).

When the flames were at a distance of R metres, the following deflections Δ_1 and Δ_2 were observed, corresponding to the case in which one or the other conductor of the machine was connected with the flame:—

R	1·09	1·68	2·54	4·19	
Δ_1	23	11	2	12	Lermantow's galvanometer.
Δ_2	30	7	2		

and with $R = 0{\cdot}305$ Δ_1 was found equal to about 300 divisions.

The same phenomenon is also observed by taking two pairs of insulated flames, so that between the machine and the galvanometer there are air-spaces; if a galvanometer is brought between the middle flames a constant current is also observed here.

The deflections of the two galvanometers are seen to be always proportional, which may serve as a proof that the circuit in question is a closed one. A metal or a non-metallic screen insulated from the earth and placed between the flames gives directly a decrease of the strength of 1·45 times in the case of glass, and 1·25 for metal; with an uninsulated screen the deflections of the needle disappear altogether.

Instead of the machine a voltaic battery may be used; the phenomenon is of course much feebler.

The author connects further one pole of an inductorium to earth and the other with the flame, while two telephones connected in series are used instead of the galvanometer. With an air-space between the flames of 6 metres (or as much as 11 metres when the telephone is connected with a large metal shield), the telephones sound quite distinctly; hence intermittent currents are transmitted directly through the air.

If, now, the air does actually conduct the electrical current, the latter should act directly on the needles of the galvanometer. In order to show this the author placed two insulated flames somewhat higher than the magnetic meridian, and connected these flames by wires at right angles to the meridian with the earth and the electrical machine respectively. The coils of the galvanometer should be either closed or altogether removed, otherwise the current divides in the coils; the metallic galvanometer with a bell-magnet gave a deflection of 5–7 divisions as soon as the machine was set to work. The distance of the flames was 920 millim. The direction of the current is changed when the current flows below the galvanometer, as required by Ampère's laws.—*Journal of the Russian Physical and Chemical Society* (7) xviii. 1886. From an abstract in *Beiblätter der Physik*, vol. xi. p. 369.

METHOD OF OBSERVING THE ACTION OF MAGNETS ON LIQUIDS.

BY S. T. MOREHEAD.

Some weeks ago one of my students, Mr. J. C. Child, and myself were working with a diamagnetic instrument, simply repeating well-known experiments. Plücker's method of observing the diamagnetism of liquids having failed in our hands to give satisfactory results, we hit upon a method which was new to us and which was very satisfactory. Into a glass tube of about four or five millimetres internal diameter a small quantity of liquid was introduced forming a short cylinder. This tube was placed horizontally at right angles to the line joining the poles of the magnet with the liquid nearly between the poles. When the current was turned on, the liquid was very evidently repelled. Water was repelled through a distance of about half a centimetre; wood-spirit through a greater distance. By moving the tube in the direction of its length the wood-spirit could be pushed any distance through the tube. The amount of motion is of course a function of the resistances due to adhesion and friction as well as of the repulsive force. The attraction of liquids is easily shown by the same method.

A single modification of the above plan of proceeding is to incline the tube slightly so as to make the liquid flow toward the poles. If the acquired velocity be not too great the magnet acts as a break to stop the motion. It is well to bend the tube up a little at each end to prevent the liquids from flowing out. This method is well adapted for projection so as to be seen by large audiences.—Silliman's *American Journal*, September 1887.

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

[FIFTH SERIES.]

NOVEMBER 1887.

XLIX. *On the Action of the Solvent in Electrolytic Conduction.*

By T. C. FITZPATRICK, *B.A.*[*]

IN a paper read at the Birmingham Meeting of the British Association last year I gave some results of the measurements of the conductivity of saline solutions; the solutions containing equivalent quantities of the same salt, the solvents being different. Since then I have been making further experiments on the nature of the action of the solvent in the conductivity of electrolytes, the results of which I wish to present in this paper.

As solvents, water, ethyl-alcohol, and methyl-alcohol have been employed. The water used in these measurements was obtained by the distillation of the ordinary town supply, which contains hardly a trace of ammonia or any organic matter. The water was boiled for a long time, before distillation in a glass still; and the product was found to have a fairly constant conductivity of ·0000062, though the conductivity of the water employed in each series of measurements was determined separately in each case.

The ethyl-alcohol was almost absolute, having a specific gravity of ·795; it was carefully distilled, and its conductivity measured before each series of experiments. The methyl-alcohol was treated in a similar way.

All these solvents have a certain amount of action on glass; and this action varies with the character of the glass. Quite

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[*] Communicated by the Electrolysis Committee of the British Association, having been read at the joint sitting of Sections A and B at the recent British Association Meeting in Manchester.

recently, of two bottles full of alcohol, which had been distilled at the same time and then left to stand for a week, the conductivity of the one at the end of this time was ten times as great as that of the other. In all my measurements the same glass cell has been employed; and it is now found that the resistance of the water or alcohol when left in the cell does not fall nearly so quickly as it did in the case of the first measurements, and it can be distinctly seen that the glass has been acted on.

The conductivities of these three solvents differ considerably. The average resistance-value for 250 cubic centim. of

(1) Water introduced into the cell,

$$R=15{,}000 \text{ to } 16{,}000 \text{ legal ohms.}$$

(2) Ethyl-alcohol,

$$R=50{,}000 \text{ to } 60{,}000 \text{ l. ohms.}$$

(3) Methyl-alcohol,

$$R=4000 \text{ to } 4500 \text{ l. ohms.}$$

It will be seen that the ethyl-alcohol is much the worst conductor, whilst the water is intermediate between the two alcohols.

In the last[*] circular of the Electrolysis Committee Prof. Ramsay makes mention of the work of Adolf Bartoli, that benzene is a non-conductor, and methyl-alcohol conducts well, and adds as a note, "is it pure?"

I was surprised at finding the resistance-value for this alcohol so low, and considered that it could not be pure; but after carefully distilling it from lime I obtained the same, or almost the same, value; and after again repeating the process the value was not found materially different.

It did not seem at first worth while working with a solvent of such a low resistance, but I have lately employed it; and the results are, I think, interesting, the only objection being the rapidity of its evaporation.

Salts employed.

It is a point worth noticing, that all the salts that are soluble in ethyl-alcohol are deliquescent salts with the single exception of mercuric chloride. This latter salt I experimented with, as it is also the only salt more soluble in alcohol than in water; and I thought it might be possible, by comparing the conductivities of solutions of this salt in equivalent quantities in water and alcohol, to see if the difference of conductivities of equivalent solutions depended at all on the coefficient of solubility of the salt in the particular solvent.

It was then found, as had been previously observed by

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[*] Sixth circular, May 1887, p. 8.

M. Bouty, that the behaviour of this salt is peculiar; in fact that its solutions conduct very little better than the solvent alone, whether the solvent be water or alcohol; and so far as this salt is concerned, solubility does not appear to affect the question of conductivity.

The other salts experimented with were calcium chloride and nitrate, lithium chloride and nitrate, magnesium chloride and nitrate, ferric chloride and mercuric chloride.

The calcium nitrate and lithium and magnesium salts were obtained from Dr. Schuchart, of Görlitz, and the ferric chloride was prepared anhydrous at the Cambridge Chemical Laboratory.

It is of course impossible to keep these salts perfectly anhydrous; but in some cases two sets of measurements were made with the same salt, and identical values obtained; which would prove that the error due to the salts not being perfectly anhydrous is very small indeed.

I have employed the methyl-alcohol as a solvent only for the lithium and calcium salts. In all cases the solutions have been obtained by dilution of the first solution; which was prepared by dissolving a known weight of the salt in 500 cubic centim. of the solvent; 250 cubic centim. of this solution being introduced into the cell for resistance-measurements.

These measurements have been made by the method described in my former paper*, which has been found to work excellently; and the cell I have used was the same as was employed for the previous determinations. The values for calcium chloride are given again for the sake of comparison with those of the calcium nitrate.

Salt, Calcium Chloride, $CaCl_2$.

$\frac{1}{10}$ Equivalent of this salt (5·532 grm.).

Solvent, Water.

Amount of salt in 250 cub. cent. of solvent.	Conductivity†.	Temp.	Amount of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.
$\frac{1}{20}$ E.	·015566	18°·8	$\frac{1}{320}$ E.	·001299	18°·9
$\frac{1}{40}$ E.	·008728	18·8	$\frac{1}{640}$ E.	·000681	18·9
$\frac{1}{80}$ E.	·004626	18·6	$\frac{1}{1280}$ E.	·000356	19·3
$\frac{1}{160}$ E.	·002438	18·4			

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* B.A. Report, 1886, p. 328.

† The conductivity of water employed is subtracted from values given in former paper.

Salt, $CaCl_2$.

Solvent, Alcohol.

Amount of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.	Amount of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.
$\frac{1}{20}$ E.	·0007246	18°·7	$\frac{1}{320}$ E.	·0001151	18°·6
$\frac{1}{40}$ E.	·0004793	18·7	$\frac{1}{640}$ E.	·0000702	18·6
$\frac{1}{80}$ E.	·0003028	18·8	$\frac{1}{1280}$ E.	·0000403	18·6
$\frac{1}{160}$ E.	·0001912	18·6	$\frac{1}{2560}$ E.	·0000232	18·5

Salt, $CaCl_2$.

Solvent, Methyl-alcohol, $CH_3 . OH$.

Amount of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.	Amount of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.
$\frac{1}{20}$ E.	·0057318	21°	$\frac{1}{320}$ E.	·0006400	21°·0
$\frac{1}{40}$ E.	·0033755	21	$\frac{1}{640}$ E.	·0003514	21·1
$\frac{1}{80}$ E.	·0019600	21	$\frac{1}{1208}$ E.	·0001930	21·4
$\frac{1}{160}$ E.	·0011370	21			

Salt, Calcium Nitrate, $Ca(NO_3)_2$.

$\frac{1}{10}$ Equivalent (8·184 grm.) dissolved in 500 cub. cent.

Solvent, Water.

Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.	Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.
$\frac{1}{20}$ E.	·014098	18°·2	$\frac{1}{320}$ E.	·001200	18°·4
$\frac{1}{40}$ E.	·007734	18·0	$\frac{1}{640}$ E.	·000620	18·5
$\frac{1}{80}$ E.	·004234	18·4	$\frac{1}{1280}$ E.	·000323	18·2
$\frac{1}{160}$ E.	·002247	18·3	$\frac{1}{2560}$ E.		

Salt, $Ca(NO_3)_2$.
Solvent, Ethyl-alcohol.

Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.	Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.
$\frac{1}{20}$ E.	·00055267	20°·7	$\frac{1}{320}$ E.	·000094677	20°·8
$\frac{1}{40}$ E.	·00036268	20·7	$\frac{1}{640}$ E.	·000058633	20·8
$\frac{1}{80}$ E.	·00023447	20·7	$\frac{1}{1280}$ E.	·000035396	20·8
$\frac{1}{160}$ E.	·00015011	20·8	$\frac{1}{2560}$ E.	·000021010	20·8

Salt, $Ca(NO_3)_2$.
Solvent, Methyl-alcohol.

Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.	Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.
$\frac{1}{20}$ E.	·0032610	20°·0	$\frac{1}{320}$ E.	·0004626	20°·4
$\frac{1}{40}$ E.	·0020660	20·1	$\frac{1}{640}$ E.	·0002810	20·5
$\frac{1}{80}$ E.	·0012890	20·2	$\frac{1}{1280}$ E.	·0001740	20·6
$\frac{1}{160}$ E.	·0007836	20·3			

Salt, Lithium Chloride, LiCl.

$\frac{1}{8}$ Equivalent (5·297 grm.) was dissolved in 500 cub. cent.

Solvent, Water.

Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.	Quantity of salt in 250 cub. cent of solvent.	Conductivity.	Temp.
$\frac{1}{16}$ E.	·0188090	19°·1	$\frac{1}{256}$ E.	·001440	19°·1
$\frac{1}{32}$ E.	·0101030	19·0	$\frac{1}{512}$ E.	·000748	19·4
$\frac{1}{64}$ E.	·0053376	19·2	$\frac{1}{1024}$ E.	·000377	19·4
$\frac{1}{128}$ E.	·0027870	19·2	$\frac{1}{2048}$ E.	·000192	19·4

Salt, LiCl.
$\frac{1}{4}$ Equivalent was dissolved in 500 cub. cent.
Solvent, Ethyl-alcohol.

Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.	Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.
$\frac{1}{8}$ E.	·003580	19°·3	$\frac{1}{256}$ E.	·000338	19°·4
$\frac{1}{16}$ E.	·002476	19·3	$\frac{1}{512}$ E.	·000192	19·5
$\frac{1}{32}$ E.	·001585	19·4	$\frac{1}{1024}$ E.	·000105	19·5
$\frac{1}{64}$ E.	·000973	19·2	$\frac{1}{2048}$ E.	·000057	19·5
$\frac{1}{128}$ E.	·000580	19·3			

Salt, Lithium Chloride.
$\frac{1}{8}$ Equivalent of LiCl was dissolved in 500 cub. cent.
Solvent, Methyl-alcohol.

Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.	Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.
$\frac{1}{16}$ E.	·0085396	20°·9	$\frac{1}{256}$ E.	·0009396	21°·0
$\frac{1}{32}$ E.	·0051570	20·9	$\frac{1}{512}$ E.	·0005216	21·1
$\frac{1}{64}$ E.	·0029840	21·0	$\frac{1}{1024}$ E.	·0002944	21·1
$\frac{1}{128}$ E.	·0016790	21·0			

Salt, Lithium Nitrate, $LiNO_3$.
$\frac{1}{2}$ Equivalent (34·45 grm.) was dissolved in 500 cub. cent.
Solvent, Water.

Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.	Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.
$\frac{1}{4}$ E.	·056346	18°·0	$\frac{1}{128}$ E.	·002598	18°·5
$\frac{1}{8}$ E.	·032240	18·1	$\frac{1}{256}$ E.	·001338	18·2
$\frac{1}{16}$ E.	·017780	18·4	$\frac{1}{512}$ E.	·000689	18·3
$\frac{1}{32}$ E.	·009425	18·2	$\frac{1}{1024}$ E.	·000351	18·6
$\frac{1}{64}$ E.	·004960	18·3	$\frac{1}{2048}$ E.	·000178	18·7

Salt, Li(NO$_3$).

$\frac{1}{4}$ Equivalent (17·228 grm.) was dissolved in 500 cub. cent.

Solvent, Ethyl-alcohol.

Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.	Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp
$\frac{1}{8}$ E.	·004417	19°·0	$\frac{1}{256}$ E.	·000369	19°·4
$\frac{1}{16}$ E.	·002905	18·9	$\frac{1}{512}$ E.	·000208	19·4
$\frac{1}{32}$ E.	·001825	19·0	$\frac{1}{1024}$ E.	·000115	19·5
$\frac{1}{64}$ E.	·001096	19·2	$\frac{1}{2048}$ E.	·000061	19·6
$\frac{1}{128}$ E.	·000644	19·2			

Salt, Lithium Nitrate.

$\frac{1}{8}$ Equivalent was dissolved in 500 cub. cent.

Solvent, Methyl-alcohol.

Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.
$\frac{1}{16}$ E.	·009695	20°·1
$\frac{1}{32}$ E.	·005818	20·3
$\frac{1}{64}$ E.	·003314	20·1
$\frac{1}{128}$ E.	·001816	19·2

Salt, Ferric Chloride, Fe$_2$Cl$_6$.

$\frac{1}{10}$ Equivalent (5·4 grm.) was dissolved in 500 cub. cent.

Solvent, Water.

Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.	Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.
$\frac{1}{20}$ E.	·015474	17°·5	$\frac{1}{320}$ E.	·001775	17°·5
$\frac{1}{40}$ E.	·009480	17·5	$\frac{1}{640}$ E.	·000976	17·5
$\frac{1}{80}$ E.	·005510	17·5	$\frac{1}{1280}$ E.	·000527	17·5
$\frac{1}{160}$ E.	·003160	17·5			

Salt, Ferric Chloride, Fe_2Cl_6.
$\frac{1}{10}$ Equivalent was dissolved in 500 cub. cent.
Solvent, Ethyl-alcohol.

Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.	Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.
$\frac{1}{20}$ E.	·0008004	17°·5	$\frac{1}{320}$ E.	·0000752	17°·5
$\frac{1}{40}$ E.	·0004340	17·5	$\frac{1}{640}$ E.	·0000451	17·5
$\frac{1}{80}$ E.	·0002370	17·5	$\frac{1}{1280}$ E.	·0000288	17·5
$\frac{1}{160}$ E.	·0001300	17·5			

Salt, Mercuric Chloride, $HgCl_2$.
$\frac{1}{10}$ Equivalent (13·527 grm.) was dissolved in 500 cub. cent.
Solvent, Water.

Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.	Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.
$\frac{1}{20}$ E.	·00006548	15°	$\frac{1}{320}$ E.	·00001553	15°
$\frac{1}{40}$ E.	·00004516	15	$\frac{1}{640}$ E.	·00001049	15
$\frac{1}{80}$ E.	·00003185	15	$\frac{1}{1280}$ E.	·00000740	15
$\frac{1}{160}$ E.	·00002256	15			

Salt, Mercuric Chloride.
$\frac{1}{10}$ Equivalent was dissolved in 500 cub. cent.
Solvent, Ethyl-alcohol.

Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.	Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.
$\frac{1}{20}$ E.	·000000803	15°	$\frac{1}{320}$ E.	·000000220	15°
$\frac{1}{40}$ E.	·000000609	15	$\frac{1}{640}$ E.	·000000165	15
$\frac{1}{80}$ E.	·000000446	15	$\frac{1}{1280}$ E.		
$\frac{1}{160}$ E.	·000000321	15			

Salt, Magnesium Chloride, $MgCl_2$.

$\frac{1}{10}$ Equivalent (4·737 grm.) was dissolved in 500 cub. cent.

Solvent, Water.

Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.	Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.
$\frac{1}{20}$ E.	·008166	18°·4	$\frac{1}{320}$ E.	·000612	18°·4
$\frac{1}{40}$ E.	·004310	18·4	$\frac{1}{640}$ E.	·000311	18·2
$\frac{1}{80}$ E.	·002230	18·4	$\frac{1}{1280}$ E.	·000158	18·2
$\frac{1}{160}$ E.	·001180	18·4	$\frac{1}{2560}$ E.	·000080	18·4

Salt, Magnesium Chloride.

$\frac{1}{10}$ Equivalent (4·737 grm.) was dissolved in 500 cub. cent.

Solvent, Ethyl-alcohol.

Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.	Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.
$\frac{1}{20}$ E.	·000587	17°·6	$\frac{1}{320}$ E.	·000780	17°·8
$\frac{1}{40}$ E.	·000356	17·4	$\frac{1}{640}$ E.	·000456	17·8
$\frac{1}{80}$ E.	·000217	17·8	$\frac{1}{1280}$ E.	·000267	18·0
$\frac{1}{160}$ E.	·000130	17·8			

Salt, Magnesium Nitrate, $Mg(NO_3)_2$.

$\frac{1}{10}$ Equivalent (7·389 grm.) was dissolved in 500 cub. cent.

Solvent, Water.

Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.	Quantity of salt in 250 cub. cent. of solvent.	Conductivity.	Temp.
$\frac{1}{20}$ E.	·01297	18°·8	$\frac{1}{320}$ E.	·001010	18°·8
$\frac{1}{40}$ E.	·00695	18·8	$\frac{1}{640}$ E.	·000526	18·8
$\frac{1}{80}$ E.	·00371	18·8	$\frac{1}{1280}$ E.	·000270	18·8
$\frac{1}{160}$ E.	·00197	18·9	$\frac{1}{2560}$ E.	·000137	18·8

Water Solutions.—The conductivity-values for equivalent solutions of the different salts are all of the same order, with the exception of the mercuric-chloride solutions, the peculiar behaviour of which has already been alluded to.

The chlorides are better conductors than the corresponding nitrates, though not to any great extent. The behaviour of magnesium chloride is anomalous, the conductivity-values for its solutions being almost exactly one half those of the corresponding calcium-chloride solutions. The same is not the case for the nitrate, though its conductivity is less than that of the calcium nitrate; a fresh solution of the chloride gave the same values as the former; the solution was carefully tested for calcium or ammonium chloride, but not a trace was found.

It would appear, therefore, that a solution of magnesium chloride containing 1 equivalent has the same conductivity as one of calcium chloride containing half an equivalent.

For the ferric-chloride solutions the conductivity is too high for the more dilute solution, the conductivity not being proportional to the quantity of salt in solution; this is probably due to the decomposition of the ferric into ferrous chloride.

Alcohol Solutions.—For none of the alcoholic solutions is the conductivity proportional to the amount of salt in solution; the conductivity not diminishing directly with the increase of the dilution.

Further, the conductivities vary considerably for the different salts.

The lithium salts have values comparable with one another, and the values for the calcium and magnesium salts are of the same order; but the values for the lithium salts are between 10 and 20 times as great as the conductivities for the corresponding solutions of the other salts.

The same is true of the methyl-alcohol solutions, though the lithium salts conduct only about twice as well as the calcium salts in methyl-alcohol solutions.

Next, comparing the conductivities of equivalent solutions for the same salt and the different solvents, it is clearly seen how largely the values for the conductivities of saline solutions depend on the character of the solvent.

Prof. Arrhenius considers that in dilute aqueous saline solutions, the action of the water is to cause the dissociation of the salt; but how this action is exerted he does not suggest.

Other solvents do not exert such dissociating action, he continues, and hence do not conduct. This is not quite exact, as, though the alcoholic solutions do not conduct as well as the aqueous solutions, they have a very considerable conductivity.

In all cases the water solutions conduct best, though the conductivity of the water itself is much smaller than that of the methyl-alcohol.

It should be mentioned that in the table of results the conductivity of the solvent has in all cases been subtracted, and the tabulated results are the difference between the observed values of the conductivity of the solvent and of the saline solution ; that this is legitimate is clearly shown by the results obtained by Prof. Kohlrausch for the conductivities of solutions of the same strength in water of varying conductivity. It has been before mentioned that the character of the salt, as well as of the solvent, influences the conductivity of the solution ; and I conclude that there is an interaction between the two, on the result of which interaction the conductivity chiefly depends ; this would be the formation of molecular groups in the solution.

That such molecular groups do exist in solution in certain cases experiment clearly proves.

Prof. Kohlrausch, in determining the conductivity of solutions of sulphur trioxide in water, found that, for certain strengths of solutions, the conductivity was a minimum ; namely, those that corresponded to the proportions $SO_3 . H_2O$, and $SO_3 . 2H_2O$; on further dilution the conductivity again increased. In these particular cases, the solution was composed of these molecular groups.

Again, the work of Arrhenius on isohydric solutions shows that, if solutions of two compounds of different strength be mixed, the conductivity of the resulting solution is the mean of those of the two. A molecule of the first body thus reacts with a different number of molecules of the solvent from that with which a molecule of the second reacts, and the two molecular groups can exist in solution without affecting one another.

Certain salts form compounds called cryohydrates, which separate out from solution at very low temperatures ; that such compounds exist in the solution previously to separating out in the solid form must be admitted. Quite recently, in experimenting with solutions of sodium chloride, with a view to the determination of temperature-coefficients at low temperatures, I found that of two solutions, the first of which was saturated, whilst the other had a specific gravity of 1·052, when cooled down in a freezing-mixture to −19° C., the saturated solution behaved regularly, the conductivity falling with the fall of temperature. For the more dilute solution this was not the case ; at −7° the solution began to solidify, and at −19° the tube was filled with a soft solid, which had a considerable conductivity. The conductivity of these solutions were measured also at 0°, and the conductivity for the more dilute solution was much smaller than the temperature-coefficient at higher temperature would lead one to expect. I conclude that the definite compound, the cryohydrate, existed at this temperature in the solution, though it did not separate out till −7°.

To further experiment on the existence of these molecular groups, I have determined the conductivity of two solvents; of the solvents mixed; of solutions of equivalent quantities of the same salt in the two solvents; and in the mixed solvents. The results are given below.

250 cubic centim. of the solution were in each case introduced into the cell.

Conductivity of alcohol	·0000082.
,, water	·000091.
Conductivity of 250 cubic centim. of mixed solvents	·000029.
Calculated value	·0000248.
Conductivity of $CaCl_2$ alcoholic solution	·00506.
,, ,, water ,,	·0775.
Conductivity of $CaCl_2$ solution in mixed alcohol and water	·0228.
Calculated value	·0206.

There is a considerable contraction of volume on mixing the alcohol and water; for the mixed solutions of calcium chloride the volume was made up to 500 cubic centim. by the addition of a mixture of the solvents.

The conductivity-values obtained differ by about 10 per cent. from the calculated values, both for the mixed solvents and for the mixed salt solutions. I conclude, therefore, that the alcohol reacts with the salt associated with the water, and *vice versâ*; it might be possible to obtain two solutions of different strength, such that the mixture had a conductivity the mean of the two, similar to Arrhenius' isohydric solutions.

All these experimental facts point in the same direction, to the action of the solvent being a chemical one; the salt being decomposed, and molecular groups formed in the solution.

Quite recently a paper has been published in the *Annales de Chimie* by Foussereau*, on the decomposition of metallic chlorides in solution; in which to measure the change in the character of the solution, due to such decomposition, he deter-

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* *Annales de Chimie*, July 1887.

mines at different times and temperatures its electrical conductivity. He makes a special study of ferric chloride, and shows that there is a considerable fall in its resistance-value; he does not chemically estimate the amount of the change, nor give the products of the change. Such a chemical decomposition I had suggested previously as the cause of the conductivity-values for ferric-chloride solution not being inversely proportional to the dilution. Foussereau considers this change under the influence of temperature, and my results would show that the change increases with the dilution of the solution. Such, too, may be the reason for the peculiar behaviour of the alcohol solutions to which I have already drawn attention.

This decomposition of the chlorides, and probably of other salts in solution, greatly complicates the measurement of conductivities of salt-solutions. It affects also the question of the change of conductivity with temperature.

There are many salts for which such decomposition is entirely negligible. I have determined the temperature-coefficients of a number of different solutions; the result of these experiments I hope to present on a future occasion. The resistances were measured up to temperatures of from 80° to 100°; and after cooling down, the resistance-value for the temperature of the air was found not to have altered; in these cases there could have been no definite chemical change in the solution; the conductivity-values were found to be proportional to the temperature.

This decomposition of the salt by the solvent perhaps throws light on the alteration in the value of the temperature-coefficient with dilution.

Dr. Armstrong considers this change to be due to the fact that in concentrated solutions complex molecules exist, which are decomposed in dilute solutions; for certain salt-solutions the temperature-coefficients decrease with dilution. Bouty*, in the *Journal de Physique*, gives the case of lead nitrate; and I have found the same for mercuric-chloride solutions.

If, under the action of the solvent, there are at high temperatures definite chemical changes, it seems impossible to hold the view of Arrhenius, that the action of the solvent is to cause the simple dissociation of the salt; it is difficult to believe that in a moderately dilute solution 90 per cent. of hydrochloric acid should be dissociated, the atoms of H and Cl existing free, and that only 1 per cent. of acetic acid should be so dissociated.

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* *Journal de Physique,* Jan. 1887.

The action of the solvents would seem to be twofold.

(*a*) Firstly, in certain cases to cause a decomposition of the salt, the amount of such decomposition depending on,

1. The temperature.
2. The solvent.
3. The state of dilution.

(*b*) Secondly, there is the formation of molecular groups in the solution; there is definite experimental proof that hydrates exist in concentrated solution; and it follows that such molecular groups, or more complex ones, exist in dilute solution.

The amount of water, or whatever the solvent may be, in dilute solution will be far in excess of that required to form these molecular groups, and consequently there will be a continual dissociation and recombination going on, within the solution, of these molecular groups; beyond a certain dilution further dilution will not affect the amount of such dissociation; and hence the observed fact that in dilute solution the conductivity is proportional to the amount of salt in solution. When these molecular groups are dissociated or decomposed by mutual collisions, it may be that the salt molecules being brought together, there is an atomic interchange; and that at this moment the electromotive force asserts its directive influence. But that there exist in the solution the "free" atoms is untenable; if free chlorine atoms are present in the solution, the solution must have some of the properties of a chlorine solution.

It would be more satisfactory, I think, to consider the whole of the decomposition as the result of the action of the electromotive force, exerted at the moment of the decomposition of the molecular groups; but all the experiments on the application of Ohm's law to electrolytes would show that this is not the case.

But the influence of molecular groups in the conductivity of saline solutions must be admitted, and the solvent must not be regarded on the one hand as a suspending medium for the salt molecules, or on the other as a dissociating agent.

NOTE.—Bouty, in the January number for this year of the *Journal de Physique*, states that the results obtained by the method of alternate currents for dilute solution are quite illusory, and he refers to the results that he and Foussereau obtained when testing the method; they employed a Wheatstone bridge, and placed in the fourth arm a resistance-coil of considerable resistance, and used as indicator a telephone; when a coil of which the true resistance was 10,000 ohms was employed, they obtained the minimum of sound in the telephone with a resistance of 9,300. I have, therefore, again tested my method for such effects: a coil of nearly a thousand ohms was inserted in the fourth arm of my bridge, and the values obtained with the ratio of the other arms, 10 : 10, 100 : 100, 1000 : 1000, were in the three cases the same; nor did they differ on varying the speed of alternation, the value being also the same as when the commutator was not working. What results may be obtained with a telephone as indicator I do not know, but with the galvanometer method the results obtained for dilute solutions need not be considered as entirely illusory.

Cavendish Laboratory, Cambridge.

L. *A Reply to Objections raised by* Mr. Charles Davison, *M.A., to the Argument on the Insufficiency of the Theory of the Contraction of a Solid Earth to account for the Inequalities or Elevations of the Surface. By* Rev. O. Fisher, *M.A., F.G.S.*[*]

A PAPER by Mr. Davison has appeared in the 'Philosophical Transactions,' communicated by Professor Bonney, treating of the distribution of strain in the Earth's crust resulting from secular cooling[†]. It consists of two parts. Part I. is of much interest, and shows that, upon the hypothesis of the earth having cooled as a solid sphere, there has always been a certain level, descending with the time, above which the strata are suffering compression, and below which they are being extended ("stretched"). An important note upon the mathematical treatment of this question has been appended by Professor Darwin, who appears to have acted as referee. The existence of such an unstrained level had been already perceived by Mr. Mellard Reade[‡].

Part II. of Mr. Davison's paper is devoted to a criticism of my argument on the insufficiency of the contraction theory to account for the existing inequalities of the Earth's surface, and it is in defence of what I have published in this Magazine upon this subject, that I offer the following remarks.

The whole question will be made more clear by considering first of all what would happen in two purely hypothetical cases.

1. If a sphere were to be cooled suddenly throughout by

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[*] Communicated by the Author.

[†] Phil. Trans. Roy. Soc. vol. 178 (1887), pp. 231–249.

[‡] 'The Origin of Mountain-Ranges,' chap. xi. (1886.) See Phil. Mag. August 1887.

the same amount, the linear contraction being in that case the same in all directions, it would become simply smaller, without any tendency to either cracking or crumpling.

2. If, however, the outer strata were to cool more than the inner ones, it is clear that they would become too small to fit the uncooled nucleus; and this effect would reach down to the level at which cooling, and therefore contraction, became insensible. Supposing, then, that nothing further happened to the rocks than a simple contraction according to Marriotte's law, the strata must needs crack; and we may imagine that the crust would be divided up by fissures, widely gaping towards the surface, into prisms similar in form to basaltic columns, and reaching down to the uncooled matter. The vertical thickness of a crust so cooled would be diminished by the sum of the linear contractions of the thicknesses of each infinitesimally thin shell in accordance with Marriotte's law, and the circumference of each shell, not counting the width of the cracks, would be shortened in proportion to the entire fall of temperature which had been experienced by that shell. The result in this second case, depending solely upon Marriotte's law, would be independent of the time.

3. Turning next to a third case, more nearly approaching what might be supposed to occur to a solid earth during the fall from the uniform high temperature of solidification to that which is its present distribution, the rate of cooling would not be the same at different depths. At any epoch, since the surface assumed the constant temperature of the atmosphere, the cooling at the surface is *nil.* At a certain depth, where the cooling is insensible, it is again *nil.* At some intermediate depth, depending on the time, the rate of cooling is greatest; and where it is greatest, there the rate of contraction will be greater than anywhere above or below that depth. In the case we are considering it is not probable that open cracks could anywhere be formed, unless just near the surface, because the weight of the superincumbent matter would press out the contracting shells laterally, so as to close them up. Under these circumstances we could not in general arrive at the change of dimensions by applying the coefficient of linear contraction to the horizontal and vertical dimensions separately of each shell; but wherever the shell is extended (or "stretched") we can only apply the coefficient of voluminal contraction to the shell as a whole.

Let us now fix our attention upon the condition of a particular shell of rock at the present epoch. We find it continuous and without open cracks, its temperature is falling, and, owing to the contraction of the sphere of matter interior to it, the shell is about to sink into a position where, being nearer to the centre, it will find less room to occupy. The question then is—Will the horizontal linear contraction of this shell exceed or fall short of that loss of room? If it exceeds, the shell will tend to be extended. If, on the other hand, the contraction is less than the loss of room due to the sinking through shortening of the radius, the shell will be compressed. Mr. Davison has shown that near the surface the shells are being compressed, and deeper down extended, and that there is a certain level of no strain, where there is neither extension nor compression; and that this level sinks deeper as time goes on. We see, then, that no compression has ever taken place below the level of no strain, and that, between the surface and it, all the shells have successively passed from a state of extension into one of compression. In calculating the amount of compression in this third case, it will be necessary to have regard to the position of the level of no strain at every successive moment from the commencement of the cooling, because it defines at that moment the limit, below which compression does not reach. Any calculation, in which we did not integrate for the time, would therefore give an incorrect result in the case we are now considering. I hope to send shortly a calculation of the mean height of the elevations which might be formed on this hypothesis.

4. But we may make a fourth hypothesis, impossible no doubt, but still one which, being "too highly favourable" in its impossibility, will give an amount of compression, and therefore of surface-elevations caused by it, greater than would be formed on the third and truer hypothesis. It is that which I made in my paper in this Magazine*, and of which Mr. Davison says that the argument based upon it "loses its force when we consider the natural process of a continuous and gradual cooling." But I submit that any argument derived from the smallness of the elevations cannot lose its force because the hypothesis, on which they have been estimated, errs on the side of making them not small enough. For observing that, upon the more correct hypothesis, the compression, or folding, is the excess of the loss of room due to the sinking of a shell beyond the horizontal contraction due to the cooling of that shell, if we neglect altogether the horizontal contraction of the shells, *every* shell will be compressed, and if, to keep the mass unchanged, we likewise

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* Phil. Mag. vol. xxiii. (1887) pp. 145–149. (In the figure to this paper θ should be q, and in the explanation of it OQ should be Oq.)

throw the whole contraction into the vertical dimension, *every* shell will descend too far. It is obvious therefore that, on both accounts, we get too much compression or folding. These are the suppositions that I made to simplify the calculation. The amount of sinking of the shells, and consequent compression owing to loss of room, is then simply caused by the total contraction in the vertical of the matter interior to each shell; and does not depend upon when the contraction occurred, or how long it took; and is therefore independent of the time; and is so far not according to nature. But nevertheless, seeing that it gives a superior limit to the compression and consequent elevations, if we find the result too small to suit the observed facts, it furnishes an *à fortiori* argument against the so-called "contraction theory."

The tidal theories of Professors Pierce and Darwin, appealed to in section 19 of Mr. Davison's paper to supplement the contraction-theory, involve considerations of so much complexity that I make no reference to them here.

LI. *Note on the Relation between the Size of a Planet and the Rate of Mountain-building on its Surface.* *By* CHARLES DAVISON, *M.A., Mathematical Master at King Edward's High School, Birmingham.*[*]

1. IN a recent paper[†] I have investigated the distribution of strain in the earth's crust resulting from secular cooling, supposing the earth to have been initially at a high temperature and practically solid throughout. Other conditions being the same, it is not difficult to show that, the smaller a planet, the more rapid is the rate of mountain-building on its surface, at any rate in the early periods of its history.

Supposing the planet to consist of an uncooled spherical nucleus surrounded by a series of very thin concentric spherical shells, of internal radii r_0, r_1, ... respectively (beginning from the nucleus), then the change of radius of the inner surface of the $(n+1)$th shell in a given time is proportional to

$$\frac{e}{r_n^2}(r_0^3 \,.\, \delta\theta_0 + r_1^3 \,.\, \delta\theta_1 + \,.\,.\,.\,.\, + r_n^3 \,.\, \delta\theta_n),$$

where $\delta\theta$ is the difference in the rates of cooling of two consecutive shells, and e is the coefficient of expansion[‡]. If this

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[*] Communicated by the Author.

[†] "On the Distribution of Strain in the Earth's Crust resulting from Secular Cooling, &c.," Phil. Trans. vol. 178 (1887), A. pp. 231–242.

[‡] Ibid. p. 233.

expression be positive, the corresponding shell is stretched; if it be negative, the shell is folded. The following reasoning shows that there exists a surface of zero-strain within the planet*, below which its crust is stretched, and above which it is crumpled.

2. At the surface of greatest rate of cooling, $\delta\theta$ is zero. Let r_x be the radius of this surface, and r_n the radius of the planet. Then, if cooling has not yet sensibly begun at the centre, we know that

$$\delta\theta_0+\delta\theta_1+\ \ldots\ +\delta\theta_{x-1}=\delta\theta_{x+1}+\ \ldots\ +\delta\theta_{n-1}+\delta\theta_n,$$

numerically, the terms on the left side being positive, and on the right negative.

On either side of the surface of greatest rate of cooling, let there be the same number of shells, and their thicknesses such that, numerically, $\delta\theta_0=\delta\theta_n$, $\delta\theta_1=\delta\theta_{n-1}$, and so on.

Then, in the series

$$r_0^3\,.\,\delta\theta_0+r_1^3\,.\,\delta\theta_1+\ \ldots\ +r_n^3\,.\,\delta\theta_n, \quad . \quad . \quad (1)$$

we have

$$\delta\theta_n=-\delta\theta_0, \quad \text{and} \quad r_n>r_0,$$

$$\therefore\quad r_n^3\,.\,\delta\theta_n+r_0^3\,.\,\delta\theta_0 \text{ is negative.}$$

This is the case with every pair of terms equidistant from the beginning and end of the series.

Therefore, the sum of the series is negative.

But the terms from $r_0^3\,.\,\delta\theta_0$ to $r_{x-1}^3\,.\,\delta\theta_{x-1}$ are all positive. Starting, then, from the first term, it follows that up to and including a certain term $r_y^3\,.\,\delta\theta_y$, y being greater than x and less than n, the sum is zero.

Hence, there exists a surface of zero-strain within the sphere.

If cooling have begun at the centre, this simply cuts off some of the terms from the beginning of the series, and its effect is to still further deepen the surface of zero-strain.

3. If the radius of the sphere be infinitely great, the ratios of r_n to r_0, of r_{n-1} to r_1, &c., are unity, and the sum of the series (1) is zero; *i. e.* the surface of zero-strain coincides with the surface of the sphere. In other words, on a globe of very large radius, provided its surface be initially smooth and spherical, no mountain-ranges can be formed by contraction from secular cooling, during a very long time from the commencement of its history. And, in any case, the course of geological change on such a body will probably be very different from what we know it to have been upon the earth.

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* The same proof holds of course for any sphere, however small, cooling from a uniform temperature.

4. Sir W. Thomson's well-known solution in his "Secular Cooling of the Earth" applies without sensible error to the earth, and even to smaller bodies, for many millions of years from the time of consolidation. This being the case, the rate of cooling at a given depth and time is for a considerable period independent of the radius of the planet.

Let z and z' be any pair of depths below the surface of a sphere for which the values of $\delta\theta$ are numerically equal, z' being the greater. Then, r being the radius of the sphere,

$$\frac{r-z}{r-z'}=1+\frac{z'-z}{r-z'},$$

which increases as r decreases, since z' is greater than z and less than r.

Hence, the smaller the radius of the sphere the greater are the ratios of r_n to r_0, of r_{n-1} to r_1, and so on; and therefore the deeper is the surface of zero-strain at any time below the surface of the sphere.

5. The amount of folding of any thin shell of radius r_n and thickness a is $8\pi a r_n d$*, where

$$d=\frac{\lambda e}{r_n{}^2}(r^3_{y+1}\,.\,\delta\theta_{y+1}+\;\ldots\;+r_n{}^3\,.\,\delta\theta_n),$$

λ being a constant, and r_y the radius of the surface of zero-strain.

Now, in any given time, with the assumed law of cooling, the values of $\delta\theta_n$, etc., are the same whatever be the radius of the sphere. If, then, the radius of the sphere be large compared with the depth of the surface of zero-strain (which is the case in the early periods of a planet's history), the above expression shows that d varies very nearly as r_n, the radius of the shell.

Suppose, for a moment, that at any time since consolidation the depth of the surface of zero-strain is independent of the radius of the planet, and let the crust between its surface and the surface of zero-strain be always divided into the same number of shells, so that at a given time the thickness (a) of each shell is the same.

In this case, then, $\Sigma 8\pi ard$ varies as r^2 very nearly, and therefore, if the depth of the surface of zero-strain were the same in all planets after the same period of cooling, the total amount of rock-folding in a given time in a planet would vary as the area of its surface.

But, as shown above, the depth of the surface of zero-strain

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* Ibid. p. 236.

is greater the less the radius of the sphere. Hence, at a given time since consolidation, the rate of rock-folding on the smaller of two planets is to that on the larger in a greater ratio than the surface of the former to the surface of the latter.

6. Nothing can be inferred from this as to the relative heights of individual mountain-ranges on different planets. If, however, at the same periods of their history, the mountains on a small planet be of the same or less average height than on a large one, then the rate of continental evolution on the former must, area for area, be greater than on the latter ; or, *cæteris paribus*, the continents of a small planet encroach upon its ocean-areas more rapidly than do those of a larger one.

Perhaps this may *in part* account for the possibly advanced state of development of the planet Mars, as indicated by what seem to be its comparatively extensive continents and narrow ocean-beds.

LII. *Twin Prisms for Polarimeters.*

By Professor SILVANUS P. THOMPSON, *D.Sc.*[*]

TO explain the points of novelty in the new twin-prisms now exhibited by the author, a brief *résumé* of some of the recent advances in polarimetry is needed. In the earliest apparatus, dating from the time of Biot, the polarizer was usually a bundle of glass plates, the analyzer a simple double-image prism or Nicol prism provided with a divided circle to measure its rotation. With the subsequent substitution of compensators and of spectroscopic apparatus in the analyzing portion of the apparatus, this paper has no concern, inasmuch as the prisms to be described are intended to serve as polarizers only, not as analyzers.

When the polarizer was a mere bundle of plates, or a Nicol prism, producing simple plane-polarized light of approximately homogeneous complaneity all over the visual field of the apparatus, exact measurements of the angle of rotation were not easy, simply because the eye failed, through a certain range of angle, to determine the precise position of the analyzer giving maximum extinction of light. For more exact polarimetry, Soleil[†], in 1845, introduced the biquartz

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[*] Communicated by the author, having been read before Section A of the British Association Meeting at Manchester.

[†] *Comptes Rendus*, xx. p. 1805, 1845 ; xxi. p. 426, 1845 ; xxiv. p. 973, 1847 ; and xxvi. p. 163, 1848.

plate, and the eye, instead of trying to determine the absolute degree of blackness, had merely to make a comparison between the tints of the two simultaneously visible halves of the visual field. With the same object Pohl*, in 1856, suggested the use of a thin piece of mica covering part of the visual field. This suggestion was improved upon in 1874 by Laurent†, who proposed to cover half the polarized field with a half-wave plate of quartz; this construction being known as the *saccharimètre à pénombres*, or *half-shadow* polarizer. In the Laurent apparatus, the two halves of the visible field are consequently polarized in planes inclined to one another at an angle dependent on the position given to the half-wave plate.

In 1860, Jellett ‡ introduced the ingenious triple image prism, in which, at either side of an ordinary image, are seen two extraordinary images, having their respective planes of polarization inclined at a small angle to one another. Hence, when one of these two images is at maximum extinction, the other will not be quite extinguished; and as a very small angular displacement will greatly affect the relative amounts of light in these two images, the adjustment to equality by the eye gives the exact position of the mean plane of polarization of the two images within a very narrow range. In Jellett's apparatus this prism was used as an analyzer only.

Based upon this idea, Cornu§, in 1873, suggested a prism *à pénombres*, constructed by Duboscq, consisting of a Nicol prism, which, having been cut in two longitudinally, in a plane at right angles to the balsam-film, and a small wedge ground away, was reunited with balsam, so giving in the two halves of the visible field light polarized in planes at a small angle to one another. In Cornu's apparatus, this divided Nicol prism was to be used as analyzer, with an ordinary simple polarizer. Schmidt and Haensch‖, in 1878, simplified this construction by cutting out the wedge from one half of the Nicol prism only, and again reuniting the parts. Lippich¶, in 1882, suggested for the same purpose covering half the polarized field with a smaller polarizing prism rotated

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* *Wiener Berichte*, xxii. p. 492, 1856.

† *Journal de Physique* (1) iii. p. 183, 1874. See also Dufet in *Journal de Physique* (2) i. p. 552, 1882.

‡ Rep. Brit. Assoc. 1860, ii. p. 13; Proc. Roy. Irish Acad. viii. p. 279, 1863; Trans. Roy. Irish Acad. xxv. p. 371, 1875.

§ *Bulletin Soc. Chim.* [2] xiv. p. 140.

‖ See Landolt, *Bericht über die Polarisationsapparate*, Berlin, 1880.

¶ *Zeitschrift für Instrumentenkunde*, ii. p. 176, 1882; *Wiener Berichte*, xci. p. 1059, 1885.

through a small angle, thereby attaining a similar result, but admitting of adjustment in the angle. He proposed to use square-ended prisms constructed like those described by the author* in 1881, having the balsam-film in a principal plane of section of the crystal, and having the end-faces also principal planes of section. A similar arrangement has been used by von Helmholtz. Righi† has proposed a combination of two Nicol prisms laid parallel to one another, with two pairs of plates of parallel-worked glass set V-wise to divide, and again reunite side by side the beams traversing the two Nicols. Poynting ‡ has suggested the use of a shallow cell covering the polarized field and containing a sugar solution, but into which a plate of glass is introduced, so as to diminish, over half the field, the thickness of the interposed solution, and so bring about a small angular difference of displacement of the plane of polarization between the two halves of the field. Another suggestion due to Poynting is to cover the two halves of the field with a quartz plate, of any convenient thickness, but reduced uniformly by a small amount, say 0·1 millim. over one half. Lastly, Pickering § has suggested the use of a double-image prism, arranged like the prism of a dichroiscope, so as to give two contiguous but oppositely-polarized images of a rectangular aperture placed beyond it. These will appear equally illuminated only when the analyzer is at 45°, or when the illumination of each is half the maximum illumination of either.

In the two forms of twin-prisms that are now exhibited, the angle between the planes of polarization in the two halves of the field of vision is fixed. In the one it is 90°, in the other about 2° 30′.

In 1886 the author described some forms of simple polarizing prisms cut for him by Mr. Ahrens on the plan adopted by that clever constructor for the later forms of his triple prisms, the principle of which consists in finding a pair of characteristic planes within the crystal of iceland spar such that they are at right angles with the terminal faces (which are principal planes of section), and so oriented that they pass through the line of intersection made by the terminal faces with the natural faces of cleavage. Prisms made on this plan, which is a mere modification of the plan suggested

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* S. P. Thompson, Rep. Brit. Assoc. 1881, ii. p. 563, and Phil. Mag. Nov. 1881, p. 349.

† *Repertorium der Physik*, xxii. p. 321, 1886.

‡ See Glazebrook's Physical Optics, p. 409.

§ Proc. Amer. Acad. May, 1885. See also *Zeitschrift für Instrumentenkunde*, vi. p. 281, 1886.

by the author in 1881, have several advantages over ordinary Nicol prisms. Their polarized field is more homogeneous, their transverse section is rectangular, their terminal faces are at right angles to the axis of vision; and by the method of construction adopted it is easy to obtain from one piece of spar two identical prisms cut from a single rectangular block. These points are all of advantage for the present purpose, namely, that of perfecting the apparatus for use in precise polarimetry.

Prisms cut on Ahrens's plan also possess another peculiarity; the terminal faces are, as mentioned above, principal planes of section, but the crystallographic axis lies in these faces at almost exactly 45° with the rectangular edges. Hence it comes about that, if one of these simple prisms be reversed end for end, while used as either polarizer or analyzer, the dark field is changed to bright field, or *vice versâ*, the same as if the prism had been rotated about its axis of vision through 90°. This is, of course, not the case with any of the ordinary Nicol, Foucault, or Hartnack prisms. Suppose, then, that two identical prisms, cut as described, are taken, of narrow rectangular cross-section, having the sides of the section in the proportion of 2 : 1. Let them be placed side by side so that, as the light falls through them, the visible juxtaposed fields of vision form the two halves of a square. Let one prism be then reversed end for end. The juxtaposed field of vision would still be a square divided into two halves; but these two halves will be oppositely polarized. Such a twin-prism can be used for polarimetry exactly as the Pickering prism or as these half-shadow combinations, in which there is a fixed angle of 90° between the planes of polarization of the two halves of the visual field. The first twin-prism now exhibited by the author is of this nature.

In the second form of twin-prism, the same mode of construction is followed so far as the cutting of two identical prisms of narrow rectangular section is concerned; but, after the prisms have been cut, a small wedge of about 2° 30′ is ground away from the side of one of them, and they are then again juxtaposed, so that there is this amount of angular displacement between the two halves of their joint visual field. In both cases the two prisms are ground together and cemented by balsam, so that the observer sees merely a fine line of junction down the middle of the visual field.

The author considers such constructions superior to that of Righi, because there is much less loss of light by reflexion at oblique surfaces; he also prefers his method to the methods of Laurent and of Poynting, because there is no difference in the angular displacement for light of different wave-lengths, as is inevitable where quartz, mica, or sugar is used to produce a rotation in one half of the field. It is preferable to Jellett's, because, in the Jellett prism, not only do the two images of varying intensity, which are to be compared, not lie in juxtaposition to one another, but there is the ordinary image lying between them interfering with the comparison. Lastly, the twin-prisms now described are more easy to construct than the divided Nicol prisms of Cornu and of Schmidt and Haensch.

September, 1887.

LIII. *On the Theory of Unipolar Induction. By* Prof. E. Edlund, *Professor of Physics at the Royal Academy of Sciences of Sweden*[*].

§ 1.

INCREASE or decrease in the intensity of the pole of a magnet produces, in a conductor which is approaching it, an electromotive force tending to create an electric current. The increase or decrease in the distance between the pole and the conductor gives rise to a like force, seeing that the action of the pole increases or diminishes in this case as in the former. The electric currents proceeding from this cause are said in both cases to be due to magneto-electric induction ; but it is also possible to produce electric currents by means of a magnetic pole without increasing or decreasing the intensity of the pole, and without increasing or decreasing the distance between the pole and the conductor. It is said, then, that the production of the electric current is due to unipolar induction ; an expression which is in no sense appropriate, seeing that the conditions necessary for the production of the first-mentioned currents are not applicable to the production of the second, and that moreover the additional term unipolar is the result of a mode of explaining the origin of the currents mentioned which is actually without meaning. These currents are not produced by induction, in the proper and original meaning of the term, but they appear rather, as will be shown further on, to have a magneto-electric origin—that is to say, to be due to the direct action of the magnet on an electric current. It is chiefly in Germany that the theory of the so-called Unipolar Induction has been developed, and attempts made to defend the theory against objections which have been recently raised against it.

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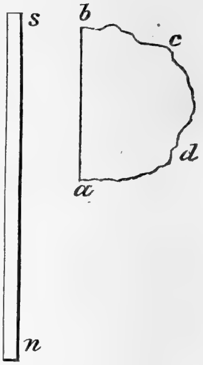
[*] Translated from a memoir presented to the Academy of Sciences of Sweden on the 9th of March, 1887. Communicated by the Author.

A correct notion of the true cause and nature of unipolar induction is of especial importance, because of the results which may be deduced from it in the explanation of several natural phenomena. I shall therefore attempt to show in the following pages that the theory hitherto accepted for the phenomena of unipolar induction is opposed to the principle of the conservation of energy, and that therefore it must be admitted to be erroneous. I shall show moreover, in a manner more complete than has previously been attempted*, that the theory of the same phenomena which I advanced a few years ago may be derived from the same principle, and that it ought therefore to lead to certain results.

§ 2.

To fix ideas, let us start from a particular example which may be considered as a typical example of unipolar induction generally. Let *s n* be a cylindrical magnet having the south pole at *s* and the north pole at *n*, and *a b* the section, in the plane of the paper, of a jacket concentrically surrounding the magnet, very easily movable, like the magnet itself, and capable of being put into rotation about the axis of the latter. Let *b c d a* represent a metallic wire pressing with its ends against the upper and lower edges of the jacket, and into which a galvanometer may be introduced. Experiments made at various times by different physicists have led, as is well known, to the following results :—

Fig. 1.



1. If the jacket only be put into rotation in a direction opposite to that of the hands of a watch as seen from above, we obtain an induced current in the direction *a b c d a*. If the rotation take place in the opposite direction, the current will also go in the opposite direction. The intensity of the current is proportional to the intensity of the magnet and to the velocity of the jacket.

2. If the magnet and the jacket are in rotation with the same angular velocity, or, in other words, if they may be considered as relatively fixed, we obtain a current of the same force and in the same direction as in the preceding case. The magnet may be in rotation in any direction and with a greater or less velocity at the same time as the jacket; the induced

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* *Bulletin* (*Öfversigt*) *des travaux de l'Acad. r. des Sciences de Suède* pour 1877; Wied. *Ann.* t. ii. p. 347; *Mémoires* (*Handlingar*) *de l'Acad. r. des Sciences,* t. xvi. 1878; Phil. Mag. [5] vi. p. 289; *Ann. de Chim. et de Phys.* tome xvi. p. 47 (1879).

current will always be the same as if the jacket only were in rotation.

3. If the magnet is in rotation whilst the jacket is at rest, no current is produced in the circuit.

4. If, without undergoing any change in their relative position, the jacket and the wire are in rotation round the magnet no current results. If we remove the jacket and put one end of the wire in contact with one of the poles, and the other end in contact with the middle of the magnet, a current is produced when the wire only is in rotation. On the other hand, the current disappears if the magnet and the electrode are in rotation in the same direction and with the same angular velocity. (Experiments of Profs. F. Exner and Czermak.)

It has been attempted to give the following explanations of the results of these experiments in accordance with the theory in favour :—

1. The current obtained in case No. 1 is produced by the electromotive forces having their seat in the jacket in rotation.

2. The current produced in No. 2, when the magnet and the jacket are in rotation with the same angular velocity, cannot, according to the old theory, be due to the electromotive forces having their seat in the jacket ; for it is admitted as an axiom that a magnet is incapable of producing induction in a conductor with which it is fixedly united*. The theory proves, in fact, that the electromotive force has its origin in the wire at rest, $b\,c\,d\,a$; and that it is, in intensity and direction, equal to the force which would be produced if the wire were in rotation in the opposite direction with the same angular velocity, whilst the jacket and the magnet remained at rest.

If the angular velocity of the magnet is different from that of the jacket and is denoted by v_1, that of the jacket being denoted by v_2, and if both take place in the same direction, their relative velocity would be $v_2 - v_1$. Now the electromotive force produced in the jacket must be proportional to this difference. But the rotation of the magnet will give rise to an electromotive force in the wire at rest, $b\,c\,d\,a$, proportional to $+v_1$. If we add these two expressions together, the sum is v_2 ; that is to say, the same electromotive force as if the jacket alone had been in rotation.

3. The old theory explains the results given under No. 3, by the supposition that the rotation of the magnet gives rise in the jacket to an electromotive force equal to that which is

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* Wiedemann, *Die Lehre von der Electricität,* t. iv. 1. §§ 67 &c. (Braunschweig, 1885) ; Wüllner, *Lehrbuch der Exp. Physik,* t. iv. p. 896 (Leipzig, 1875) ; and several other works.

produced in the wire, but acting in the opposite direction, so that the intensity of the current becomes zero.

4. The result indicated under No. 4 is explained in a similar manner.

The fact that, in the case given under No. 2, the induction produced in the wire at rest when the magnet is in rotation round its axis ought to be equal to that produced when the magnet is at rest, but the wire enters into rotation in the opposite direction but with the same angular velocity about the magnet, may, according to this theory, be explained as follows :—The lines of force, imagined by Faraday, are in rotation with the same angular velocity as the magnet ; and the electromotive force produced is proportional to the number of lines of force which the conducting-wire traverses in a given time. But since evidently the wire encounters an equal number of lines of force, whether the magnet is in rotation and the wire at rest, or whether it is the wire which rotates and the magnet which is at rest, the induction, according to this hypothesis, must be equal in the two cases.

§ 3.

The only method by which we can give a certain explanation of the origin of the currents in question is given us by the mechanical theory of heat, a theory of which I have made use for this purpose some time since. Let us introduce into the conducting-wire *b c d a* (fig. 1) a battery producing a current, which passes through the closed circuit in the direction *a b c d a*. In consequence of the action of the magnet upon the current which traverses the jacket, it commences to move in the direction, as seen from above, of the hands of a watch. We may observe that here the magnet itself does not enter into rotation. It may without appreciable resistance be turned as much on the one side as on the other by an external force. If now we reduce the jacket to rest by means of an interposed obstacle, which we will call in this case the electromotive force, denoting it by E, the resistance in the circuit itself being denoted by m, and the intensity of the current by I ; and, lastly, if we call A the calorific equivalent of the unit of work, the sum of all the heat developed by the resistance in the circuit will be equal to AI^2m, and the heat consumed* in the battery for the production of the current will be equal to AEI. But, since these two quantities must be equal, since the current has not produced any external work, we shall have

$$I^2M-EI=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

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* *Mémoires (Handlingar) de l'Acad. r. des Sciences de Suède,* t. xiv. Pogg. *Ann.* t. clix. (1876); Phil. Mag. [5] vol. iii.

If we let the jacket enter into rotation it will produce external mechanical work, and the development of heat will consequently be less than before. This is brought about by the production, in the jacket in rotation, of an electromotive force sending a current in the opposite direction to that of the battery. If we denote this current by i, the production of heat in this case will be

$$A\{(I-i)^2m-E(I-i)\}=A\{I^2m-EI-2Iim+i^2m+Ei\}.$$

In other terms [if we pay attention to equation (1)], there is produced in the closed circuit a loss of heat equal to

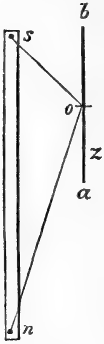
$$Aim(I-i). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

The value of the external work done will then be equal to this loss.

In conformity with the law of Biot-Savart, a magnetic pole acts upon an element of the current with a force equal to the intensity of the pole divided by the square of the distance to the element, and multiplied also by the intensity of the current and the length of the element, and by the sine of the angle α formed by the element and the line of junction between the pole and the element: the direction of this force is normal to the plane passing through the pole and the element. If the direction of motion of the element makes an angle ϕ with the normal in question, we obtain of course the component of the force along the line of motion by multiplying the expression further by $\cos\phi$. If we denote the velocity of the element by h, the intensity of the pole by M, and the intensity of the current by $(I-i)$, and, lastly, the distance between the pole and the element by ρ, we have, as the expression of the mechanical work due to the action of the pole upon an element of the current dz,

$$\frac{M(I-i)}{\rho^2}\sin\alpha\cos\phi\,.\,hdz. \quad . \quad . \quad . \quad . \quad . \qquad (3)$$

Fig. 2.



In fig. 2, let sn represent a magnet, and ab a jacket through which a current $(I-i)$ passes from a to b. Let the distance between the two poles be $2l$, and the length of ab half this distance, viz. l; and let the radius of the jacket be r. Let us consider at first an element dz of the jacket situated at the point 0 at the distance z from a. The distance ρ from the south pole to dz is then equal to $\sqrt{r^2+(l-z)^2}$, and $\sin\alpha=\frac{r}{\sqrt{r^2+(l-z)^2}}$. The normal to the plane passing through the south pole and the element coinciding with the direction of motion of the jacket, $\cos\phi$ will be unity. If the jacket moves with the velocity h, the work done in unit time by the action of the pole upon the current dz will be equal to

$$\frac{\mathrm{M}r(\mathrm{I}-i)h\,dz}{\{r^2+(l-z)^2\}^{\frac{3}{2}}}.$$

In a similar manner, we obtain for the north pole

$$\frac{-\mathrm{M}r(\mathrm{I}-i)h\,dz}{\{r^2+(l+z)^2\}^{\frac{3}{2}}}.$$

By integration of these expressions between $z=0$ and $z=l$, we have for the whole jacket

$$\frac{2\mathrm{M}l}{r}(\mathrm{I}-i)h\left\{\frac{1}{(l^2+r^2)^{\frac{1}{2}}}-\frac{1}{(4l^2+r^2)^{\frac{1}{2}}}\right\}. \quad . \quad . \quad (4)$$

Multiplying this expression by A, we obtain the quantity of heat corresponding to the amount of work in question.

Consequently we have from the formulæ (2) and (4),

$$im(\mathrm{I}-i)=2\mathrm{M}(\mathrm{I}-i)h\,.\frac{l}{r}\left\{\frac{1}{(l^2+r^2)^{\frac{1}{2}}}-\frac{1}{(4l^2+r^2)^{\frac{1}{2}}}\right\}.$$

If we remove the battery from the circuit so that $\mathrm{I}=0$, the preceding equation nevertheless continues applicable if the jacket is maintained in rotation by an external mechanical force so that h does not become equal to zero. In this form the equation shows that the square of the intensity of the induced current multiplied by the resistance of the circuit is equal to the mechanical work consumed in the rotation of the jacket. If this work is zero, the intensity of the current will also be zero.

If we suppose that the jacket consists only of an element of the circuit Δs, we shall obtain, by equating the expressions (2) and (3), after dividing the first by A and putting I equal to zero, the following law for the induction in an element of the circuit in rotation round a magnetic pole:—

$$i^2m=\frac{\mathrm{M}}{\rho^2}i\sin\alpha\cos\phi h\Delta s$$

or

$$im=\frac{\mathrm{M}}{\rho^2}\sin\alpha\cos\phi h\Delta s. \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Consequently, when an element of the circuit is in rotation round a magnetic pole, the induced current which results is proportional to the magnetic intensity at the place where the element is, multiplied by the sine of the angle which the element forms with the line drawn from the element to the pole, by the cosine of the angle between the direction of rotation of the element and the normal to the plane passing through the element and the pole, and, lastly, by the velocity and the length of the element. It should be observed in this respect that h does not denote the angular velocity of the element of the circuit, but the length of path described by the element in unit time. Without consumption of mechanical work no induced current, with its resulting development of heat, could be produced.

When the element of the circuit moves in a uniform and homogeneous magnetic field, as, for example, that due to the horizontal component of the earth's magnetism, Helmholtz and Sir W. Thomson* have obtained the same expression for the induced current as that contained in the preceding equation (5). It is thus evident that these formulæ ought to be equal in the two cases; since the action of a magnetic pole upon an element of the circuit which moves at a uniform distance from the pole ought to be continually of the same magnitude, and consequently would be under the same conditions as if the element moved in a homogeneous magnetic field.

§ 4.

The law given above in formula (5) for the inductive action of a magnetic pole upon an element of a circuit in motion does not indicate in any manner how the current is produced, nor the relation of this phenomenon to other electrical phenomena. I now propose a new formula showing this relationship, which is also of a more convenient form, and I shall show mathematically that this new formula may be deduced from formula (5).

I imagine to myself that the induction is produced in the following manner:—If the electric current consists really in the transport of a fluid, as soon as the element of the circuit begins to move the fluid would be carried with it in the direction of motion, and would thus produce a current upon which a magnetic pole could act, according to the known law of Biot-Savart. If β be the angle formed by the direction of motion with the line drawn from the element of the circuit to the pole, and ψ the angle made by the element with the normal to the plane passing through the direction of motion and the line joining the pole and the element; if then h be the velocity of the element of the circuit, and Δs its length, we shall have, in accordance with the law mentioned above,

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* Blavier, *Des grandeurs électriques*, p. 358 (Paris, 1881).

the expression

$$im = e = \frac{\mathrm{M}}{\rho^2} \sin\beta \cos\psi h\Delta s \quad . \quad . \quad . \quad . \quad (6)$$

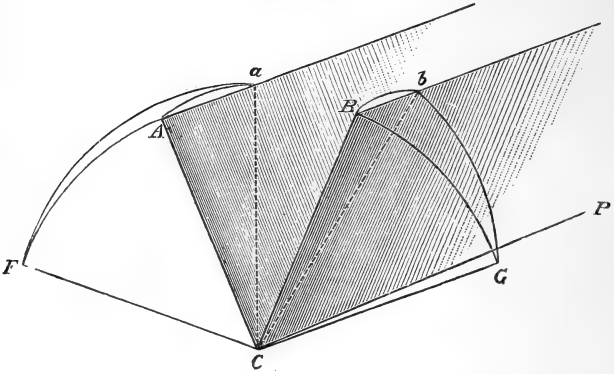
for the force with which the magnetic pole tends to conduct the electric fluid along the element of the circuit, *i. e.* for the electromotive force of induction.

In this expression e denotes the electromotive force, and i, m, M, and ρ have the same meaning as before. If the phenomena of unipolar induction can be explained by the aid of this formula, that would show that there is reason to see in them electrodynamic phenomena having no connexion with the phenomena of induction strictly so-called.

I shall show presently that formula (6) is identical with the formula (5) deduced from the mechanical theory of heat.

In fig. 3 let a C represent the element of the circuit in which the induction takes place, b C the direction of motion of this element, and C P the line of junction between the magnetic pole and the element of the circuit. The lines A C and B C are drawn at right angles to the line C P; the first in the plane passing through a C and C P, and the second in the plane passing through b C and C P. Lastly, F C is the normal to the plane b C P, and G C the normal to the plane a C P. It follows from this that the lines G C, B C, A C, and F C all lie in one plane, since each of them is perpendicular to C P. But since the angles F C B and G C A are both right angles, it follows that if we take away the angle A C B the angles F C A and B C G are equal.

Fig. 3.



According to formula (5), deduced from the mechanical theory of heat, the element of the circuit $a\mathrm{C} = \Delta s$ ought to be multiplied by sin (aCP), or, since the angle ACP is a right angle, by cos (ACa). It will be necessary, finally, to multiply this product by the cosine of the angle made by the direction of movement with the normal to the plane aCP, *i. e.* by cos (bCG). But the plane GCB being perpendicular to the plane bCB, the dihedral angle B is a right angle. We obtain consequently from the spherical triangle,

$$\mathrm{BG}b : \cos(b\mathrm{CG}) = \cos(\mathrm{GCB}) : \cos(b\mathrm{CB}).$$

The law of induction (5), deduced from the mechanical theory of heat, obtains thus the following form :—

$$im = \frac{\mathrm{M}}{\rho^2}\cos(a\mathrm{CA})\cos(\mathrm{GCB})\cos(b\mathrm{CB})\,.\,h\Delta s. \quad . \quad (5\mathrm{b})$$

We will now transform formula (6) in a similar manner.

As before, let aC denote the element of the circuit Δs, and let bC indicate the direction of movement or the direction of the current of translation produced by the motion of the element of the circuit. This current must be multiplied by sin (bCP), or, what comes to the same thing, by cos (bCB). The magnetic pole tends to conduct it into the normal to the plane passing through CP and bC, *i. e.* in the direction FC. To obtain the component in the direction aC of the element of the circuit it will be necessary to multiply, finally, by cos (aCF). The expression for the induction thus becomes

$$\frac{\mathrm{M}}{\rho^2}\cos(a\mathrm{CF})\cos(b\mathrm{CB})h\Delta s. \quad . \quad . \quad . \quad . \quad (\mathrm{a})$$

In the spherical triangle aAF, the dihedral angle at A is a right angle ; we obtain therefore

$$\cos(a\mathrm{CF}) = \cos(a\mathrm{CA})\cos(\mathrm{FCA}).$$

Introducing into the formula (a) these values of cos (aCF), and recollecting moreover that cos (FCA) = cos (GCB), we have the desired formula of induction, viz.

$$im = \frac{\mathrm{M}}{\rho^2}\cos(a\mathrm{CA})\cos(\mathrm{GCB})\cos(b\mathrm{CB})h\Delta s, \quad . \quad (5\mathrm{b})$$

a formula identical with the formula (5b).

The formula (6) established by me for the calculation of the unipolar induction in an element of the circuit moving in a magnetic field ought therefore to give correct results.

It is evident that the magnitude of the induction depends on the relative movement between the magnetic pole and the element of the circuit. But the relative movement is not altered if we give to the pole and to the element equal and parallel velocities in the same direction. Having regard to this condition, we may imagine, in the calculation of the magnitude of the induction, that the magnetic pole is at rest, and that the element of the circuit actually moves with a velocity equal in magnitude and direction to the resultant of the original velocity, and of a velocity equal to that of the magnet but in the opposite direction.

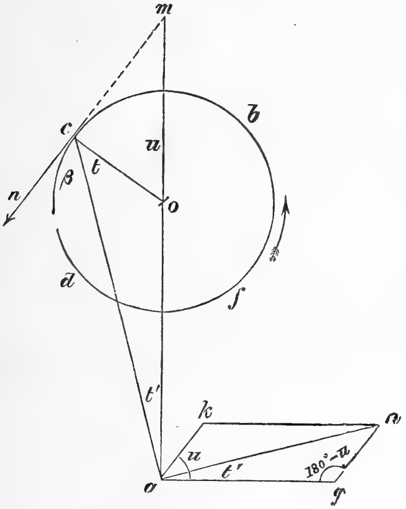
§ 5.

1. I now propose to make use of what has been said in the preceding pages for the explanation of the experiments mentioned above in § 2.

When the jacket only is in rotation, whilst the magnet and the wire are at rest, it is evident that the electromotive force ought to be produced in the jacket. The old theory and that which I am expounding agree in this respect. But this agreement ceases when we pass to the second case, that where the magnet and the jacket are put into rotation about the axis of the former. We will suppose at first the magnet and the jacket in rotation in the same direction and with the same angular velocity. In this case the two may be considered as united; and it is not possible, according to the old theory, to produce an electromotive force in the jacket; but the induction ought (always in accordance with the same theory) to take place in the metallic wire at rest. But it is of course impossible to admit that induction may take place in the wire from the fact that the jacket is in motion; and in the case where an induction is caused by it, that this induction ought to be due to the rotation of the magnet. But, as was remarked above, the magnet may be put into rotation either in the one direction or the other without consuming other work than that necessary to overcome the torsion of the thread by which the magnet is suspended, or the friction against the angles which support it. But such a consumption of work has nothing to do with the production of electricity. It is possible nevertheless that the induced current offers some slight obstacle to the rotation, although this obstacle may be too small to be perceptible. The old theory, that the production of the induced current takes place without the consumption of mechanical force, cannot, consequently, be correct. According to my theory, on the contrary, the rotation of the magnet in this case is without appreciable importance in the formation of the current; for the reason that, in virtue of well-known experiments, the resulting induced current does not offer any sensible obstacle to the rotation. By far the most important part of the induction is produced in the jacket altogether as if the magnet were at rest. The rotation of the magnet is thus almost without importance for the induction. The two theories diverge in this case in an essential manner; and this divergence is of great importance for the use of the theory to explain several other natural phenomena. The fact that the rotation of the magnet is without real significance in the case in question may be proved more completely in the following manner.

2. Supposing that the circle $bcdf$ (fig. 4) represents the horizontal section of a vertical magnet, in which plane one of

Fig. 4.



the poles lies (*e. g.* the south pole), and that vertically beneath a there is an element of the vertical circuit Δs; and let us suppose that the element of the circuit and the magnet are both in rotation with the angular velocity v in the direction of the arrow about the axis o of the magnet. Let us then denote by R the distance from the element of the circuit beneath a to the axis of the magnet, taking the radius oc of the magnet equal to r, and supposing that the element Δs is at a distance H vertically below the horizontal plane first mentioned. Let us further suppose a vertical plane passing through the vertical element of the circuit Δs and the axis of the magnet, and that another vertical plane passes through the same element and the point c situated on the circumference of the magnet at the angular distance u from the line om. The first of these vertical planes will consequently intersect the horizontal plane mentioned along ao and the second along ac. The element of the circuit Δs moves in the direction $ag = vR$, and the point c in the direction $cn=vr$. Let us now suppose the magnet divided into elementary magnets, of which we have to consider only those situated at the circumference of the magnet, since all the others may be treated in a similar manner. If we call M the magnetic intensity per unit of length of the circumference, the intensity which corresponds to the differential of the angle u will be equal to $\mathrm{M}rdu$. We will now calculate by means of the formula (6) the magnitude of the induction which is produced in the element of the circuit Δs, when this element and the ring of the magnet are in rotation in the same direction with the angular velocity v.

Let us give to the element $\mathrm{M}rdu$ of the magnet a velocity vr in the opposite direction to the rotation, and to the element of circuit Δs a velocity ak of the same magnitude and in the same direction. The relative velocity between the two elements will not be thereby modified. The element of the magnet $\mathrm{M}rdu$ enters thus into rest, and Δs moves within the resultant of ag and ak. The resultant ah consequently represents the magnitude and direction of the velocity which the element of the circuit Δs receives in this manner. Since cn and ak are parallel, the angle $oak=cmo=90°-u$. But the angle oag being 90°, the angle kag ought consequently to be $=u$. The horizontal line ac is equal to $\sqrt{\mathrm{R}^2+r^2+2\mathrm{R}r\cos u}$, and the cosine of the angle cao or t' will consequently be equal to $\dfrac{\mathrm{R}+r\cos u}{\sqrt{\mathrm{R}^2+r^2+2\mathrm{R}r\cos u}}$. Since $ag=v\mathrm{R}$ and ak or $gh=vr$, the resultant ah will be equal to $v\sqrt{\mathrm{R}^2+r^2+2\mathrm{R}r\cos u}$, and the cosine of the angle hag will be equal to

$$\frac{v(\mathrm{R}+r\cos u)}{v\sqrt{\mathrm{R}^2+r^2+2\mathrm{R}r\cos u}}.$$

But since these two angles are both acute they ought to have the same magnitude. The line ag being at right angles to the vertical plane which passes through the line oa, the resultant ah must also, for the reason that the angles named are of equal magnitude, be perpendicular to the vertical plane which passes through ac, and consequently also perpendicular to the line uniting c to the element of circuit Δs. The distance between Δs and c is evidently $=\sqrt{\mathrm{H}^2+\mathrm{R}^2+r^2+2\mathrm{R}r\cos u}$. The element of the magnet $\mathrm{M}rdu$ acts normally to the plane which passes through this element, and the direction of motion of the element of circuit ah. To obtain from this the effect along the vertical element Δs, we must multiply by the cosine of the angle which the normal mentioned makes with the element Δs: now this angle is equal to that which the latter plane makes with the horizontal plane. The cosine of this angle is evidently equal to

$$\frac{\sqrt{\mathrm{R}^2+r^2+2\mathrm{R}r\cos u}}{\sqrt{\mathrm{H}^2+\mathrm{R}^2+r^2+2\mathrm{R}r\cos u}}.$$

The velocity of the element of circuit will be then

$$v\sqrt{\mathrm{R}^2+r^2+2\mathrm{R}r\cos u};$$

the angle which the direction of movement makes with the line of junction between the element of circuit and that of the magnet will be 90°, and consequently its sine $=1$; the distance between the two elements will then amount to

$$\sqrt{\mathrm{H}^2+\mathrm{R}^2+r^2+2\mathrm{R}r\cos u}.$$

Lastly, the cosine of the angle between the vertical element Δs and the normal to the plane passing through the direction of movement and the element of the magnet will be

$$\frac{\sqrt{\mathrm{R}^2+r^2+2\mathrm{R}r\cos u}}{\sqrt{\mathrm{H}^2+\mathrm{R}^2+r^2+2\mathrm{R}r\cos u}}.$$

If now we introduce these values for $\frac{\mathrm{M}}{\rho^2}$, β, and ψ into the formula (6), drawn from the mechanical theory of heat, we obtain for the inductive action of the element of the magnet $\mathrm{M}r du$, situated at c, upon the element of circuit Δs, when both are in rotation in the same direction and with the same angular velocity, the following expression:—

$$\frac{\mathrm{M}rv(\mathrm{R}^2+r^2+2\mathrm{R}r\cos u)\Delta s du}{(\mathrm{H}^2+\mathrm{R}^2+r^2+2\mathrm{R}r\cos u)^{\frac{3}{2}}}.$$

Multiplying by 2 the integral between the limits 0 and π of this expression, we obtain the sum of the whole inductive action upon the element of circuit Δs of the magnets situated upon the periphery of the magnet. But

$$\int_{u=0}^{u=\pi}\frac{(\mathrm{R}^2+r^2+2\mathrm{R}r\cos u)du}{(\mathrm{H}^2+\mathrm{R}^2+r^2+2\mathrm{R}r\cos u)^{\frac{3}{2}}}=\int_{u=0}^{u=\pi}\frac{du}{(\mathrm{H}^2+\mathrm{R}^2+r^2+2\mathrm{R}r\cos u)^{\frac{1}{2}}}$$
$$-\mathrm{H}^2\int_{u=0}^{u=\pi}\frac{du}{(\mathrm{H}^2+\mathrm{R}^2+r^2+2\mathrm{R}r\cos u)^{\frac{3}{2}}}.$$

If we remember that $\cos u=1-2\sin^2\frac{u}{2}$, and if we put $\frac{u}{2}=\phi$, and, lastly, $\frac{2\sqrt{\mathrm{R}r}}{[\mathrm{H}^2+(\mathrm{R}+r)^2]^{\frac{1}{2}}}=\sin\Theta$, we obtain for the two integrals of the right-hand member of the preceding equation,

$$\frac{2}{[\mathrm{H}^2+(\mathrm{R}+r)^2]^{\frac{1}{2}}}\int_{\phi=0}^{\phi=\frac{\pi}{2}}\frac{d\phi}{(1-\sin^2\Theta\sin^2\phi)^{\frac{1}{2}}}$$

$$-\frac{2\mathrm{H}^2}{[\mathrm{H}^2+(\mathrm{R}+r)^2]^{\frac{3}{2}}}\int_{\phi=0}^{\phi=\frac{\pi}{2}}\frac{d\phi}{(1-\sin^2\Theta\sin^2\phi)^{\frac{3}{2}}}$$

The elliptical integral of this will be

$$\frac{2}{[\mathrm{H}^2+(\mathrm{R}+r)^2]^{\frac{1}{2}}}\mathrm{F}'(\Theta)-\frac{2\mathrm{H}^2}{[\mathrm{H}^2+(\mathrm{R}+r)^2]^{\frac{1}{2}}[\mathrm{H}^2+(\mathrm{R}-r)^2]}\mathrm{E}'(\Theta). \quad (7)$$

3. We will take as example $\mathrm{H}=\mathrm{R}=10r$. Θ will then be $25^\circ{\cdot}18$, and the value of the formula (7) $\frac{0{\cdot}1107}{r}$. Multiplying this expression by $2\mathrm{M}rv\Delta s$, we have the total induction of the ring equal to $0{\cdot}2214\ \mathrm{M}v\Delta s$.

If the magnet were at rest, but Δs in rotation as before with the angular velocity v, and if the magnetism of the periphery were concentrated in the axis of the magnet, the calculation of the magnitude of the induction would give the following results :—The velocity of the element of the circuit would be $v\mathrm{R}$, the intensity of the magnetic pole would be $2\pi r\mathrm{M}$, and the cosine of the angle which the plane passing through the direction of motion and the pole makes with the horizontal plane would be $\frac{\mathrm{R}}{(\mathrm{R}^2+\mathrm{H}^2)^{\frac{1}{2}}}$. In virtue of formula (6) the induction would be

$$\frac{2\pi r\mathrm{M}v\mathrm{R}^2\Delta s}{(\mathrm{R}^2+\mathrm{H}^2)^{\frac{3}{2}}}.$$

If, as we have supposed above, $\mathrm{H}=\mathrm{R}=10r$, we obtain, as the value of the induction in Δs, $0{\cdot}2221\ \mathrm{M}v\Delta s$, a value which only differs from the former value by a little more than 0·3 per cent.

Let us take some other examples. Suppose that $\mathrm{H}=0$ and $\mathrm{R}=10r$. When the magnet and the element are in rotation in the same direction with the same angular velocity v, we obtain from formula (7) the magnitude of induction $=0{\cdot}630\ \mathrm{M}v\Delta s$. If the magnet is at rest and the element Δs moves with the angular velocity v, and if we suppose the magnetism concentrated in the axis of the magnet, the magnitude of the induction will be equal to $0{\cdot}628\ \mathrm{M}v\Delta s$, expressions which differ from one another by 0·25 per cent. Let us suppose $\mathrm{H}=0$ and $\mathrm{R}=20r$. The elliptical integral gives in this case the value of the induction as equal to $0{\cdot}3144\ \mathrm{M}v\Delta s$.

If, on the contrary, the magnet is at rest and the magnetism is concentrated in the axis of the magnet, the value of the induction when Δs has the angular velocity v will be 0·3142 $\mathrm{M}v\Delta s$; and these two values are almost identical.

Let us put $\mathrm{H}=2r$ and $\mathrm{R}=5r$. When the magnet and the element of the circuit are in common rotation, we obtain from formula (7) the value of induction $=1·002\,\mathrm{M}v\Delta s$; and when the element of the circuit only is in rotation we obtain 1·004 $\mathrm{M}v\Delta s$. The difference is therefore about 0·2 per cent.

For the case where the magnet is at rest and the element alone is in rotation, we have supposed in our calculation that all the magnetism of the ring is concentrated in the axis of the magnet. In this manner, of course, a sufficiently exact result is obtained only if the element Δs is at a sufficient distance from the surface of the magnet, as was the case in the preceding examples. But we must not forget that all the magnetism of the magnet is not found upon its surface; but that the subjacent layers, of which the distance from the axis is only a fraction of r, are also magnetic, although their distance from Δs is greater than that of the superficial layer. The mode of calculation employed, which may of course also be applied to the subjacent layers, consequently gives a more exact result for the magnet as a whole than for the superficial layers.

4. Let us now suppose the element Δs situated at the surface of the magnet so that $r=\mathrm{R}$; and let us further suppose that H also is equal to r. When the element is in rotation with the same angular velocity and in the same direction as the magnet, we obtain, by employing formula (7), 1·926 $\mathrm{M}v\Delta s$ for the value of the induction.

Let us suppose, finally, that the element Δs is situated within the magnetic ring at the distance $\frac{1}{2}r$ from the axis of the magnet; that the magnet and the element are in rotation in the same direction with the same angular velocity; and that $\mathrm{H}=r$.

In conformity with formula (7) the induction will now be 1·038 $\mathrm{M}v\Delta s$.

§ 6.

The old theory supposes that a magnet in rotation about its axis produces an induction in an element of circuit at rest and at a distance the same as if the magnet were at rest, but the element rotated in the opposite direction but with an equal angular velocity. We will now examine if this result conforms to the requirements of the mechanical theory of heat.

1. Suppose the vertical element of the circuit at rest and

situated in the same horizontal plane as the pole. The sine of the angle which the direction of motion makes with the line of junction between the element of the magnet and the element of the circuit (the angle designated by β in the formula (6)) will be in this case (fig. 4) equal to $\cos t=(u-t')$

$$=\frac{\cos u(\mathrm{R}+r\cos u)}{\sqrt{\mathrm{R}^2+r^2+2\mathrm{R}r\cos u}}+\frac{r\sin^2 u}{\sqrt{\mathrm{R}^2+r^2+2\mathrm{R}r\cos u}}.$$

Consequently $\sin\beta$

$$=\frac{\mathrm{R}\cos u+r}{\sqrt{\mathrm{R}^2+r^2+2\mathrm{R}r\cos u}}.$$

The angle denoted by ψ in the formula (6) is equal to zero, and its cosine is equal to 1. The inductive action of the element of the magnet $\mathrm{M}rdu$ situated at the point c upon the element of circuit Δs at rest will be then

$$\frac{\mathrm{M}r^2v(\mathrm{R}\cos u+r)\Delta s du}{(\mathrm{R}^2+r^2+2\mathrm{R}r\cos u)^{\frac{3}{2}}}.$$

If we multiply by 2 the integral of this expression between the limits zero and π, we shall have the value of the induction which the sum of the elementary poles situated in the periphery of the magnet is able to produce by the rotation of the magnet round its axis in the element at rest Δs.

$$2\mathrm{M}r^2v\Delta s\int_{u=0}^{u=\pi}\frac{(\mathrm{R}\cos u+r)du}{(\mathrm{R}^2+r^2+2\mathrm{R}r\cos u)^{\frac{3}{2}}}$$

$$=\mathrm{M}rv\Delta s\int_{u=0}^{u=\pi}\frac{(\mathrm{R}^2+r^2+2\mathrm{R}r\cos u)du+(r^2-\mathrm{R}^2)du}{(\mathrm{R}^2+r^2+2\mathrm{R}r\cos u)^{\frac{3}{2}}}.$$

Putting in this expression $1-2\sin^2\frac{u}{2}$ in place of $\cos u$, and ϕ instead of $\frac{u}{2}$, and lastly $\sin\Theta$ for $\frac{2\sqrt{\mathrm{R}r}}{\mathrm{R}+r}$, we have

$$\frac{2\mathrm{M}rv\Delta s}{\mathrm{R}+r}\int_{\phi=0}^{\phi=\frac{\pi}{2}}\frac{d\phi}{\sqrt{1-\sin^2\Theta\sin^2\phi}}$$

$$+\frac{2\mathrm{M}rv\Delta s\,.\,(r-\mathrm{R})}{(\mathrm{R}+r)^2}\int_{\phi=0}^{\phi=\frac{\pi}{2}}\frac{d\phi}{(1-\sin^2\Theta\sin^2\phi)^{\frac{3}{2}}},$$

from which we obtain the elliptical integral

$$2r\mathrm{M}v\Delta s\left[\frac{1}{\mathrm{R}+r}\mathrm{F}'(\Theta)+\frac{1}{r-\mathrm{R}}\mathrm{E}'(\Theta)\right]. \quad . \quad . \quad (8)$$

2. If we suppose that the element of the circuit only is in

rotation whilst the magnet is at rest, the effect of the magnet situated at c upon the element of the circuit Δs will be

$$\frac{\mathrm{M}r\mathrm{R}v(\mathrm{R}+r\cos u)\Delta s du}{(\mathrm{R}^2+r^2+2\mathrm{R}r\cos u)^{\frac{3}{2}}},$$

which will give therefore for the whole of the ring,

$$2\mathrm{M}rv\Delta s\int_0^\pi\frac{(\mathrm{R}^2\ +r\mathrm{R}\cos u)du}{(\mathrm{R}^2+r^2+2\mathrm{R}r\cos u)^{\frac{3}{2}}}.$$

The function under the sign of the integration becomes equal to that of the preceding case if we replace r by R, and R by r. The elliptic integrals would then also become equal in consequence of this modification. The value of the total induction will then be from that time

$$2\mathrm{M}rv\Delta s\left\{\frac{1}{r+\mathrm{R}}\mathrm{F}'(\Theta)+\frac{1}{\mathrm{R}-r}\mathrm{E}'(\Theta)\right\}. \quad . \quad . \quad (9)$$

3. Let us suppose that $\mathrm{R}=10r$. For the case when the magnet is in rotation and Δs at rest, the induction will be, according to formula (8), $=-0{\cdot}00316\mathrm{M}v\Delta s$. If, on the contrary, it is the magnet which is at rest, whilst Δs is in rotation, the magnitude of the induction will be, according to formula (9), equal to $0{\cdot}6331\ \mathrm{M}v\Delta s$. The first of these values is only about 0·5 per cent. from the second.

Let us suppose that $\mathrm{R}=20r$. If it is the magnet that is in rotation whilst Δs is at rest, we shall have, from formula (8), a value of induction equal to $-0{\cdot}0004\ \mathrm{M}v\Delta s$. In conformity with formula (9), the induction will amount to $0{\cdot}3148\ \mathrm{M}v\Delta s$. For the case when the magnet is at rest and Δs in rotation, the first of these values is not quite 0·13 per cent. of the second.

Let us suppose, lastly, that $\mathrm{R}=2r$. Then the induction will be, according to formula (8), $-0{\cdot}5416\ \mathrm{M}v\Delta s$; and according to formula (9) it would amount to $3{\cdot}9132\ \mathrm{M}v\Delta s$. The first of these numbers gives nearly 13·8 per cent. of the second.

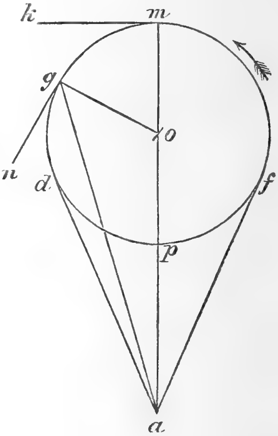
The result of the preceding investigation is that the values per cent. increase in proportion as the distance between Δs and the axis of the magnet diminishes; and that in the last case, where $\mathrm{R}=2r$, the value per cent. has become considerable. We must, however, remember that in the preceding calculations we have only taken into consideration the magnetism of the external layer of the magnet. But the magnet is equally magnetic in the layers at a shorter distance from the axis, and the inductive power of these layers may be calculated in a similar manner as the effect of the layer limited by the circumference of the magnet. As the values per cent. will become lower for the interior layers than for the exterior ones, it is evident that this will also be the case for the total effect of the magnet.

4. It has been supposed above that the element of the circuit at rest Δs was at a distance, greater or less, outside of the exterior layer of the magnet in rotation; and it has been shown at the same time that the induction of this element was in general very feeble. We will now suppose that the vertical element of the circuit is in the axis of the magnet and in the same horizontal plane as the pole.

The distance between the elementary pole $Mrdu$ situated at c and the element of the circuit Δs is then equal to r. The angle β is evidently a right angle, and ψ is equal to zero. In virtue of formula (6), the induction of the element of the magnet will consequently be equal to $\frac{Mrvr\Delta sdu}{r^2}$ when the magnet is in rotation with the angular velocity v; and as this expression is equal for all values of the angle u, the induction of the whole layer will be equal to $6{\cdot}2832\ Mv\Delta s$. This value is the same as that we obtain when the element Δs is in rotation at a distance r round a magnetic pole of which the intensity is equal to that of the exterior of the magnet, and is consequently $2\pi rM$.

5. It is easy to explain in an elementary way why, when the element is outside of the external layer, the induction becomes so feeble in comparison with that produced when the element is situated in the axis of the ring.

Fig. 5.



Let the circle $dpfmg$ (fig. 5) represent the horizontal plane in which one of the poles of the vertical magnet is situated, and let us suppose that the element Δs at rest is placed at the point a. The magnet is in rotation in the direction indicated by the arrow. An elementary magnet at m moves then in the direction mk, and the angle which this direction of motion makes with the line of junction between m and a (the angle β of the formula 6) is a right angle. When the elementary magnet reaches g the angle in question is acute, and it becomes zero at d, from which point a line drawn to a is a tangent to the circle. Sin β

consequently changes its sign at *d*, and it retains this change of sign until it comes to *f*. It follows from this that the elementary magnets upon the arc *d p f* produce an induction of opposite sense to those upon the arc *f m g d*. If these contrary inductions become of equal value, no induction will be produced in the element of circuit at rest Δs. It is moreover clear that if the element Δs were situated at a vertical distance H above or below the horizontal plane in question, the induction would change its sign when the elementary magnet passed the points *d* and *f*. If, on the contrary, the element of the circuit is situated at the centre *o* of the circle, the inductions of all the elementary magnets would act in the same direction, and the result would consequently be equal to their arithmetical sum.

6. In the preceding explanation we have taken into consideration only the induction of the elementary magnets upon the periphery of the magnet. But it is evident that the same deductions are equally applicable to each ring of elementary magnets of which the radius is less than the radius of the magnet. The difference is this: that, in consequence of the rotation of the magnet on its own axis, the induction produced by these rings or layers in an element of the circuit situated at a distance is less than the effect of the ring or layer of elementary magnets at the periphery of the magnet. In the same way no account has been taken of the one pole of the magnet; but it is evident that the same proof applies equally to the second. It follows, therefore, that the preceding demonstration applies to the whole magnet.

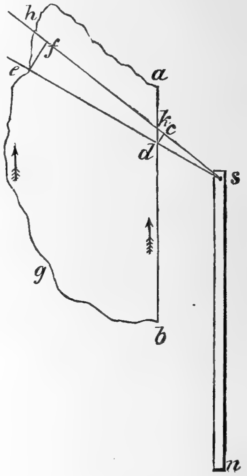
§ 7.

1. The experimental result given under No. 3 of § 2 is, as already mentioned, explained on the old theory by the supposition that an electromotive force is produced in the jacket of the same magnitude as if the magnet were at rest but the jacket in rotation in the opposite direction with the same angular velocity, and that an electromotive force of equal magnitude was produced in the metallic wire. Where these two forces of equal magnitude neutralized each other, the intensity of the current is equal to zero. According to the requirements of the mechanical theory of heat, this explanation is erroneous. It is true that the rotation of the magnet causes the production of an electromotive force in the jacket at rest; but, as we have seen, this force is very small. It produces in the same way a feeble electromotive force in the metallic wire. These two forces are opposed, but they

are not of equal magnitude. They are in any case so small that it would be impossible for them to produce an appreciable current, even if they acted in the same direction.

2. In experiment No. 4 of § 2, the jacket and the metallic wire were in rotation with the same angular velocity. As it would be impossible to use a galvanometer to measure the current, recourse was had to chemical tests to show the existence of a current. It is easy to deduce the results of formula (6) mentioned above, drawn from the mechanical theory of heat. Let *s n* (fig. 6) represent a vertical magnet, of which the poles are situated at *s* and *n*, and let *a b* be a concentric jacket encircling the magnet, and put in contact at the points *a* and *b* with the metallic wire *a h e g b*. We will suppose now that the jacket *a b* and the wire *a h e g b* are put into rotation round the axis of the magnet, so that each part of the circuit formed moves with the same angular velocity v. The lines *s e* and *s h* denote two planes passing through the magnetic pole, and through the direction of motion of the two points *e* and *h*. We will suppose the angle *f s e* between the two planes to be very small. If the distance from *s* to *e* is denoted by the velocity of the point, *e* would be denoted by pvr_1 where p is a constant. The length of the normal *e f* drawn to the plane *s h* is equal to $r_1 \sin(fse)$. The element of the circuit *e h* ($=\Delta s$ of formula 6) multiplied by $\cos(feh)$ ($=\cos\psi$ of formula 6) is equal to $ef = r_1 \sin(fse)$. If the closed circuit seen from above is in rotation in a direction opposite to that of the hands of a watch, and if *s* is the south pole, there is produced in *e h* an electromotive force tending to produce a current in the direction of the arrow. Sine β of formula (6) is equal to unity, since the direction of motion makes a right angle with the line joining the element to the pole. If we denote by M the intensity of the magnetic pole, and consequently by $\frac{M}{r_1^2}$ the intensity of the magnetic field at the point *e*, we shall obtain from formula (6), for the induction in the element of circuit *e h*, the expression

Fig. 6.



$$\frac{M}{r_1^2} p r_1 v r_1 \sin (fse) = M p v \sin (fse).$$

It is of course the same for the element of circuit *dk*, situated between two planes, making the same angle with each other and with the horizontal plane as those just mentioned passing through the pole, and the direction of motion of the points *d* and *k*.

The induction in this element of the circuit will consequently be also $M p v \sin (fse)$, and its effect will be upwards in the direction of the arrow. But, since these conditions are applicable to all the positions which these planes can take, it follows that the electromotive force produced in a part of the closed circuit is equal to the force which is produced in the rest of the circuit. As these two forces neutralize each other, there can be no current produced in this case.

3. We will now suppose that the jacket is removed, and that the ends of the metallic wire *ahegb* are in contact, one with one of the poles, and the other with the centre of the magnet. If now the wire is in rotation with the magnet round the axis of the latter, it evidently follows from the preceding that in this case also no current can be produced. When the magnet is in rotation round its axis it produces, according to 4 of § 5, an electromotive force at each of the points which receive a motion of translation, and in conformity with 4 of § 6, a like force is produced in the axis itself, although it does not receive any motion of translation, but simply one of rotation. The sum of these forces is equal to that which would be produced in a jacket in rotation round the magnet. As experiment shows, no current can therefore be produced in a closed circuit.

If, on the contrary, the magnet is at rest whilst the metallic wire only is in rotation, no electromotive force will of course be produced in the magnet, and consequently the electromotive forces produced in the metallic wire may in this case give rise to a current.

4. I have had occasion to verify, in the preceding work, formula 6, given above, by a great number of experiments on unipolar induction, and I have always been able to observe that it furnishes a true explanation. I therefore consider it superfluous to submit it to new experiments of verification. We are, moreover, entitled to affirm that the formula in question ought to give a correct result, since it is an immediate consequence of the mechanical theory of heat.

§ 8.

Several physicists * have recently undertaken the defence of the old theory on the nature of unipolar induction against the objections which I have brought against it. I shall therefore conclude this memoir by calling the attention of the reader to the following results of the above-described researches.

The assertion of the old theory that a magnet is incapable of producing induction in a conductor with which it is fixedly united is in opposition to the requirements of the mechanical theory of heat, and cannot therefore be sustained.

The old theory supposes that a magnet in rotation about its axis produces in a conductor at rest placed at a distance, an induction of the same magnitude as if the conductor were in rotation with the same angular velocity in the opposite direction round the magnet at rest.

This method of regarding it is in opposition to the mechanical theory of heat, and must therefore be erroneous; the induction produced in the conductor at rest when the magnet is in rotation amounts generally only to an insignificant fraction of the induction due to the rotation of the conductor with the same velocity round the magnet at rest.

If a magnet and a conductor at some distance are in rotation in the same direction, with the same angular velocity round the axis of the former, according to the old theory no induction should be produced in the conductor. But this is also in opposition to the mechanical theory of heat, which shows us that the induction in the conductor is of nearly equal amount, whether the magnet is in rotation or whether it is not. The rotation of the magnet is in fact without sensible effect upon the magnitude of the induction.

LIV. *On the Cause of Iridescence in Clouds.*

By James C. McConnel, *M.A.*†

IN a recent number of this Magazine‡, Dr. Johnstone Stoney has suggested an explanation of the iridescent hues sometimes seen tinging the edges and thinner portions of the clouds. During my residence last winter in the Engadine, I

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* Hoppe, Wied. *Ann.* t. xxviii. p. 478; t. xxix. p. 544. F. Exner, *Sitzungsber. der Kais. Akad. der Wiss. in Wien,* 8 Juli 1886. For the refutation of these articles see Edlund, Wiedemann's *Annalen,* t. xxix. p. 420, t. xxx. p. 655; *Sitzungsber. der K. Akad. der Wissensch. in Wien,* 13 Januar 1887.

† Communicated by the Author.

‡ Phil. Mag. July 1887, p. 87; Trans. Roy. Dublin. Soc. Feb. 16 and March 23, 1887.

was greatly struck both by the splendour and the frequency of this phenomenon, and I availed myself of the opportunity of watching the general character of the display, and securing a number of measurements of the angular distances of the various colours from the sun. I was thus led to an explanation totally different to that advanced by Dr. Stoney. He considers that we have here an example of the colours of thin plates, while I believe the colours to originate in diffraction by fine filaments of ice. Spider-threads, streaming from the trees in full sunshine, often glow with most brilliant hues. Replace the spider-threads by similar filaments of ice, increase their number to billions, carry them to the distance of a mile, and we should, under favourable circumstances, have a cloud shining with all the colours of the spectrum.

In the following paper I shall compare the results of each theory with my observations, and show that, in the light of this fuller series of facts, the theory of thin plates proves unsatisfactory, while diffraction appears competent to explain the whole of the phenomena. It may be that there are other kinds of iridescence, which I have not been fortunate enough to witness, and for which the theory of diffraction fails. But even so, I shall have done good service in this inquiry by pointing out both the province and the limitations of diffraction. Before discussing the two theories, I will give an account of the phenomena I have observed, so that my readers may understand precisely what the appearances are that I shall attempt to explain.

During the winter in the Engadine, the colours are frequently brilliant enough to attract the notice of the most indifferent, and on almost any day, when there are broken clouds near the sun, they can be made out with the aid of dark grey spectacles. The action of the spectacles depends on the physiological fact, that in intense light the eye loses, to a considerable extent, its power of distinguishing colour.

Within a circle round the sun, radius about 2°, the clouds are white, or faintly tinged with blue. This circular space is surrounded by a ring of yellow, passing into orange. The region of most vivid hues is comprised between 3° and 7°, the most striking being purple, blue, orange, green, and red. These are generally scattered at random over the thinner parts of the clouds. Further out, the only colours visible are green and pink, becoming rapidly fainter with increasing distance. These greens and pinks are often arranged in bands parallel to the edge of a cloud, sometimes as many as three bands of each being visible. Broad alternate bands of colour are easy to detect, even when very faint, and after some practice I have thus distinguished colour as far from the sun as 23°. For this purpose I have found the naked eye most efficient in winter, though not in summer.

It is thus obvious that the distribution of colour depends mainly on the distance from the sun when that distance is small, the colours being arranged in circles. But further out it depends more on the variation of some property of the cloud, since the colours follow its edge. In both theories this property is the average size of the particles, which is no doubt generally greater in the interior of a cloud. So far we have but little evidence as to the proper order of the colours. But along the edge of a large cloud we may expect the particles to be of tolerably uniform size, and the disturbing element to be thereby removed. On one occasion, when such an edge passed almost through the sun, I noted down the colours in order: white, yellow, red; blue, green, yellow, pink; green, pink. This list consists evidently of three successive spectra similar to those seen in Newton's rings or in various diffraction experiments. Its accuracy was fully borne out by a number of similar observations. The blue, however, is often replaced by a brilliant purple, due to the first and second spectra overlapping.

We may now, I think, fairly conclude that, if the sky were overspread by a thin cloud of particles of uniform size, and the eye were not troubled by the glare, we should see a series of coloured rings—blue, white, yellow, red; blue, green, yellow, pink; green, pink; green, pink; &c. The nearest approach to this in my experience was in April at Bern. With the aid of dark spectacles, I saw nearly complete circles of yellow, orange, red, purple, and green.

In the summer I have rarely seen colour with the unaided eye, and even with spectacles the iridescence, though seldom absent, is generally insignificant. The most vivid effects are given by unmistakable ice-clouds, while unmistakable water-clouds often show no effects at all. In the high altitudes, in which I have been living since I began to pay special attention to these appearances, the majority of the clouds are no doubt ice; and for some time I was uncertain whether the colours were ever shown by clouds composed entirely of water. However, one warm day in June, these doubts were set at rest. I was standing on a high ridge, the temperature perhaps being 60° Fahr., when some thin clouds came drifting over, perhaps a hundred feet above my head; I put on dark spectacles, and immediately circles of colour appeared—yellow, orange, red, faint green, in order outwards. Unluckily I took no measures, but the colours were about the usual distance from the sun. On another occasion, with similar clouds, I found the boundary between red and green to be about 6° from the sun.

The peculiar brilliance of the effects during the Engadine winter is, no doubt, partially due to a subjective cause. The eye, continually exposed to the reflexion from the snow, accomodates itself to the intense light, and is better fitted to deal with the bright glare near the sun. Some eyes, perhaps, have not this power of adaptation, for, even at St. Moritz in the winter, some persons are quite unable, without glasses, to see the colours. The near proximity also of the ice-clouds adds greatly to the display, for, though the brightness is not increased, each colour is spread over a larger portion of the sky, and thereby rendered much more effective.

Having now put the reader in possession of the facts to be explained, I will give my reasons for thinking Dr. Stoney's explanation untenable in the face of these facts. In the first place, it does not apply to the very similar phenomena seen in water-clouds, so that in this case, at any rate, he would have to resort to diffraction. Secondly, though it gives the colours in their proper order, it leads to a hopelessly exaggerated idea of scale.

Let ϕ be the angle of incidence on the plate of ice,
ϕ' „ „ refraction,
ψ the angular distance of the cloud from the sun.

Then we have

$$\left.\begin{aligned}\psi&=180^\circ-2\phi\\ \sin\phi&=\mu\sin\phi'\\ \cos\phi'&=\frac{n\lambda}{2t\mu}\end{aligned}\right\}$$

where n is the order of the spectrum and t the thickness of the plate. Let us suppose the sky to be covered with a thin cloud, composed of plates of such thickness as to give the blue of any order at $\psi=5^\circ$; then taking $\mu=1\cdot3$, and the wave-length of red light half as great again as that of blue, we find for the red of the same order $\psi=136^\circ$. And this is neglecting the variation of μ with λ, so that really ψ would be greater still. Thus a single spectrum would be spread over almost the whole expanse of the sky. It is needless to say that this is utterly at variance with the facts I have described.

Thirdly, the brightness of the colours does not fall off in the theory of thin plates nearly as rapidly as we find in observation.

As the plate is inclined at different angles to the sun, the quantity of light reflected passes through a series of maxima, alternating with vanishing-points. But for the present let us make abstraction of these variations. Let $2\gamma=\psi$, so that γ is the complement of the angle of incidence of the light from the centre of the solar disc, and let q be the area of the plate, A the intensity of sun-light, and ω the angular area of the sun. The light that falls on the plate is $q \sin\gamma . \mathrm{A}$. This light is distributed over an area $r^2\omega$ near the observer, at the distance r from the plate. Hence the intensity due to one plate at that distance is $q \sin\gamma . \mathrm{A}/r^2\omega$. If there are n suitable plates within the area ω, the brightness of the cloud is, in terms of that of the sun, $nq \sin\gamma/r^2\omega$. We proceed to investigate the value of n.

With the aid of a little spherical trigonometry, we may show that every plate will reflect light to the eye, whose normal lies within an angular space of approximate area $\omega/4 \sin\gamma$. Now we assume for simplicity that the plates are arranged at random, so out of the whole number a fraction, $\omega/8\pi \sin\gamma$, will send light to the eye. For it must be remembered that each face is competent to reflect. The average apparent area of a plate is $q/2$; so, if the plates occupy a fraction α of the field of view, the whole number in the area ω is $2\omega r^2\alpha/q$, and the number in position to reflect light to the eye is $n=\omega^2 r^2\alpha/4\pi q \sin\gamma$. Hence the brightness of the cloud is $\omega\alpha/4\pi$ that of the sun. Inserting numerical values we have

$$\cdot 0000045\alpha. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

We must now take into account the varying intensity of the reflected light according to the angle of incidence. It is only with the maxima that we are concerned. At each maximum we have for this intensity

$$\mathrm{I}=\frac{4b^2}{(1+b^2)^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where b^2 is the proportion of light reflected from a single surface, and the intensity of the incident light is unity. By Fresnel's laws, which are sufficiently accurate for our present purpose,

$$2b^2=\frac{\sin^2(\phi-\phi')}{\sin^2(\phi+\phi')}+\frac{\tan^2(\phi-\phi')}{\tan^2(\phi+\phi')}. \quad . \quad . \quad . \quad (3)$$

From these formulæ I have calculated the following table :—

Table I.

When	$\psi=0°$	$\phi=90°$	$b^2=1$	and	$\mathrm{I}=1$
,,	$\psi=20°$	$\phi=80°$	$b^2=\cdot 327$	,,	$\mathrm{I}=\cdot 76$
,,	$\psi=30°$	$\phi=75°$	$b^2=\cdot 20$	,,	$\mathrm{I}=\cdot 56$
,,	$\psi=50°$	$\phi=65°$	$b^2=\cdot 08$	,,	$\mathrm{I}=\cdot 275$

In discussing the variation of intensity with distance, we have merely to consider the march of the function I, since the factor (1) is constant. And it appears from the table that, even at 30° from the sun, the intensity has more than half its maximum value. Now it is matter of common observation, that a thin cloud near the sun glows with an intensely bright white light, due probably to irregular diffraction. This white light diminishes rapidly with increasing distance. Hence, on this theory, so far from expecting the brilliance to fall off rapidly from 5° outwards, we might reasonably look for the most obvious colour-effects at 20° or 30°. But we should look in vain.

The utmost distance at which I have detected the faintest trace of colour is 23°, and this limit is not approached on one day in twenty.

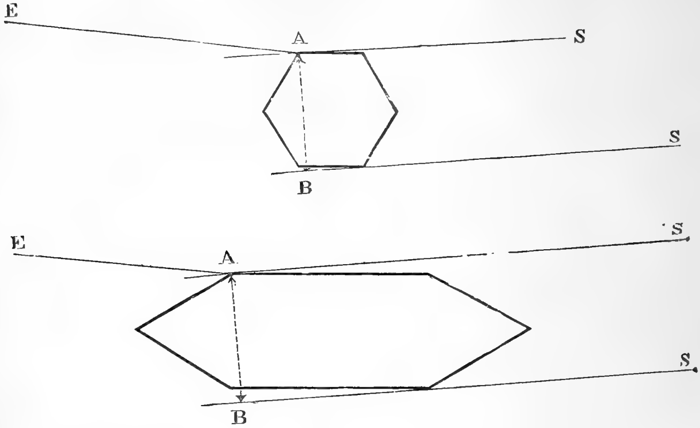
It is now, I think, clear that Dr. Stoney's is not the true explanation of the whole, or even of the most striking part of the phenomena I have described. Whether it accounts for any part, is a question which can be more satisfactorily discussed after I have set forth my own theory in fuller detail.

One essential feature in any theory is that the colour of the light sent by the particles should depend on their size only, and not on their orientation. The only form of diffracting particle which satisfies this condition accurately is the sphere. But the long circular cylinder and forms similar thereto, such as the long hexagonal prism, are sufficiently satisfactory for practical purposes. The sphere of course diffracts light in the same way, however it be turned. The prism, on the contrary, to send light at all, must deviate but slightly from the "reflecting plane," but within this plane it can be turned between wide limits without materially affecting the quality of its light. (By the reflecting plane is meant the plane in which a small mirror must lie so as to reflect sunlight to the observer.)

Let us examine this very important point more closely. If the axis of the prism or filament lie in the reflecting plane, the light from all points of the axis takes equal time from the sun to the eye. So, as far as relative retardation is concerned, we may consider all points on a line parallel to the axis as equivalent. We may with sufficient accuracy replace our filament by a long narrow plate, whose plane passes through the axis and is inclined at the greatest possible angle to the sun's rays. This plate, in turn, may by Babinet's principle be replaced by an equal and similarly situated slit in an infinite opaque screen. As all points in a line parallel to the axis are equivalent, we need only consider the trace on the plane of diffraction, *i. e.* the plane through the sun and the eye. The breadth of the representative plate or slit, measured on this plane, determines the diffraction of the various spectra, and is the greatest breadth of the section of the filament measured across the sun's rays.

In fig. 1 is given the case of a hexagonal filament, whose

Fig 1:



minor diameter is perpendicular to the "reflecting plane," when the angle of diffraction is 10°; first when the axis is at right angles to the plane of diffraction, and secondly when it is inclined at only 20° to that plane.

AS,BS are drawn towards the sun, AE towards the observer, and AB is the breadth of the representative slit. It will be noticed how slightly AB is altered even by this extreme inclination. If the major diameter had been perpendicular to the reflecting plane, there would have been no alteration. The difference between the major and minor diameters themselves is of somewhat more importance, but each differs by only about 7 per cent. from the mean.

Among the numerous crystalline forms met with in falling snow, and therefore presumably in the clouds, there is none approaching to a sphere. But thin hexagonal filaments are common, either separate, or forming part of more elaborate structures; so it is these which we must regard as the "active principle" in the coloured clouds. Let us then consider the spectacle to be expected in a sky covered with thin clouds composed of filaments of one uniform diameter a. The following table, deduced from figures quoted by Verdet*,

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* *Leçons d'Optique Physique,* tome i. §§ 69, 77.

gives the angular distance and relative brightness of the first four maxima for wave-length λ.

Table II.

Order of Spectrum.	Angular Distance.	Brightness.
1	$1·43\,\lambda/a$	1
2	$2·46\,\lambda/a$	·37
3	$3·47\,\lambda/a$	·185
4	$4·48\,\lambda/a$	·109
9	$9·50\,\lambda/a$	·024

For the sake of comparison I give the corresponding figures for spheres of diameter D.

1	$1·64\,\lambda/D$	1
2	$2·67\,\lambda/D$	·24
3	$3·69\,\lambda/D$	·095
4	$4·72\,\lambda/D$	·045

Though the first ring is rather further out, the succeeding intervals are much the same, but the intensity falls off much more rapidly.

To return to the filaments, (1) if the first red were at 4° from the sun, the second and third would be at 7° and $9\frac{3}{4}$°. Observation points to something of this kind. (2) In observation we find the spectrum colours show themselves tolerably distinct, in favourable cases, as far as the green of the second order; but outside this there is decided evidence of overlapping. And this is what we might expect from the theory, when we remember, on the one hand, that only the brighter parts of the spectrum need be considered, and, on the other, that there are three important causes of blurring—the finite diameter of the source of light, the spreading out of the diffracted light on either side of the maximum, and the want of uniformity among the particles, both as regards size, and, though of less importance, as regards orientation.

It will be remarked that only a small proportion of the filaments are sufficiently near the "reflecting plane" to send diffracted light. And it might be concluded that the light sent would be very feeble. But this conclusion is by no means borne out by some rough calculations I have made, and which lead to the result that a cloud of ice filaments is but little inferior in diffracting power to a cloud of water drops. (This surprising result is chiefly due to the advantage a filament gains from its length. The following is a general explanation :—If a long filament and a sphere give equal diffraction to the first spectrum, the area of the filament must be many times greater than that of the sphere. But the intensity of diffracted light is proportional to the square of the area of the diffracting body. So the single filament sends more intense light than a number of spheres, which together occupy the same apparent area.) The steps of the calculation for the filaments may be indicated as follows:—

The filament, by reflecting and refracting in different directions part of the light that falls upon it, and greatly retarding the remainder, behaves in diffraction like an opaque body, and, by Babinet's principle, may be replaced by a similarly oriented slit in an opaque screen. If the axis of the slit lie in the "reflecting plane," light from every point of any line parallel to the axis will reach the retina in the same phase. The oblique slit will therefore diffract light in the same manner as a slit at right angles to the sun's rays, except that we have to take as effective area the projection of its area perpendicular to the sun's rays. But filaments lying within a small angle of the reflecting plane will also contribute light, and it may be shown that we get a fair approximation to the amount of the total light, by supposing all filaments, for which the retardation of one end of the axis relative to the other does not exceed π, to contribute the maximum light, and other filaments to contribute nothing[*]. Further it may be shown, with the aid of spherical trigonometry, that such filaments must lie within an angle $\chi=\lambda/4b\sin\gamma$ on either side of the "reflecting plane," where b is the length of the filament, and γ is half the angular distance from the sun. So of the whole number we may consider only the fraction $\lambda/4b\sin\gamma$ to send light, for we suppose the filaments to lie at random in all directions.

When the slit is perpendicular to the sun's rays, the intensity of light at a distance r in the direction opposite the

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[*] Let x denote the retardation of phase of any point in the axis relative to the middle point for any given direction of diffraction, and R the value of x for one end. Then the amplitude in that direction is

$$\int_{-\mathrm{R}}^{+\mathrm{R}} \cos x\, dx/2\mathrm{R} = \sin \mathrm{R}/\mathrm{R},$$

that for $\mathrm{R}=0$ being taken as unity. If the whole of the light for positive values of R be supposed compressed between the limits $\mathrm{R}=0$, $\mathrm{R}=\frac{\pi}{2}$, the average value over this range is

$$\int_0^\infty \frac{\sin^2 \mathrm{R}}{\mathrm{R}^2}\, d\mathrm{R} \div \frac{\pi}{2}.$$

It is easy to verify by a rough numerical calculation that this last expression is approximately equal to unity.

sun is $a^2b^2 . \mathrm{A}/\lambda^2r^2$ by Stokes's formula*, and the intensity of the first spectrum is then, according to Verdet†,

$$\cdot 046\, a^2b^2 . \mathrm{A}/\lambda^2r^2.$$

While, if the slit or filament be inclined to the sunlight at an angle β, the intensity is

$$\cdot 046\, a^2b^2 \sin^2\beta . \mathrm{A}/\lambda^2r^2.$$

By what we have just shown, we need only consider a fraction, viz. $\lambda/4b \sin\gamma$, of the filaments to send light, and, since the effectiveness of each is proportional to $\sin^2\beta$, we must diminish the final product in the ratio $2:1$. (The correct ratio is $2:1+\sin^2\gamma$, but in our applications γ is small.) Hence the average filament sends light of intensity

$$\cdot 046 \frac{a^2b^2}{\lambda^2r^2} . \mathrm{A} \frac{\lambda}{8b\sin\gamma}.$$

Now we want to compare the brightness of the cloud with that of the sun. Let ω be the angular magnitude of the sun, and let the filaments occupy a fraction α of the field of view. The average apparent size of a filament is $\pi ab/4$. So, if n be the number of filaments in the area ω, we have

$$\alpha r^2\omega = n\pi ab/4.$$

Hence the intensity of the light from the n filaments is

$$\cdot 023\, a\omega\alpha . \mathrm{A}/\lambda\pi \sin\gamma,$$

and the brightness of the cloud, in terms of that of the sun, is

$$\cdot 023\, a\omega\alpha/\lambda\pi \sin\gamma.$$

Now for the first spectrum,

$$a/\lambda = 1\cdot 43/\sin\psi,$$

where ψ is the angular distance from the sun, and so $\psi = 2\gamma$. So, inserting numerical values, the last expression takes the form

$$\cdot 0041\, \alpha/\psi^2,$$

where ψ is supposed to be less than 30°, and is expressed in degrees.

In the same way we find for the second and fourth spectra the expressions

$$\cdot 0026\, \alpha/\psi^2$$
$$\cdot 0015\, \alpha/\psi^2.$$

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* Stokes, "On the Dynamical Theory of Diffraction," Trans. Camb. Phil. Soc. vol. ix. p. 1; or 'Math. and Phys. Papers,' vol. ii. p. 243; or Glazebrook, "On Optical Theories," B. A. Report, 1885.

† *Loc. cit.*

(3) These figures exhibit a rapid diminution in brightness with increasing ψ, such as is found in observation, but is wanting in the theory of thin plates.

For the brightness of the first spectrum at 5° we obtain the value

$$\cdot000164\,\alpha,$$

and at the same distance for a cloud of thin plates,

$$\cdot0000045\,\alpha.$$

Though the ice-crystals which fall as snow give us some clue to the forms of those suspended in the clouds, we are ignorant, at any rate, as to which is there the predominating form. But, if we assume that plates and filaments occupy nearly the same fraction of the field of view, these figures show that the colours due to the filaments must far outweigh the others in the near neighbourhood of the sun. So we are in a position, not merely to declare that the observed facts about the brighter colours are inconsistent with the hypothesis of thin plates, but also to show cause why ice-laminæ should not produce these brighter colours.

Granting, however, that thin plates are not the principal, they might still be a subsidiary cause. Though incompetent to produce the bright colours near the sun, they might yet give rise to the faint tints at a distance. But I do not think this probable. On days when the colours are visible unusually far out, I have generally noticed several cases all at about the same distance, *e. g.* on one day 21°, 18½°, 21°, 21°, 19°, on another 20°, 20°, 22½°. This points to a rapid falling-off of intensity with distance, and therefore to the hypothesis of diffraction. Some slight additional evidence may be drawn from the figures given below. I have thought it probable that the colours at 20° belonged to the fourth spectrum. On this assumption, for $\psi=20°$ the brightness is $\cdot0000037\alpha$, while for any spectrum on the hypothesis of thin plates it is $\cdot0000017\alpha$.

The average diameter of the filaments that produce the brighter colours I find to be about ·013 millim. It might be thought the chief cause of the brighter tints appearing at about 5° was, that at this angle the colours escape, on the one hand, the overpowering white glare which suffuses clouds in the neighbourhood of the solar disc, and, on the other, the enfeebling influence of greater distance. But I cannot hold this view. There is one characteristic colour, which always occurs at the boundary between the first two spectra, I mean purple. Out of some thirty or forty measurements of this colour, taken at various times and places, I have never found a case within 3°, or without 7°. The first gives a diameter ·019 millim., the second ·0085 millim. So, whatever may be the cause, we are driven to conclude that filaments outside these limits are not found in the clouds in sufficient numbers and with sufficient uniformity of size to produce the brighter iridescences. (Coronæ, mentioned below, are seen in quite a different type of cloud. The colours form regular circles, and are of a different character, being dull and blurred.)

Dr. Stoney's description of the phenomena agrees, as far as it goes, fairly well with mine, though I have not noticed that iridescence is more frequent when the sun is low. Indeed, it has seemed to me to be less frequent, owing to the greater thickness of the cloud-layer, measured in the direction of the sun. But near the horizon the colours, of course, are more likely to catch the eye.

If he is correct in supposing that the laminæ would generally be but slightly inclined to the horizontal, the thin-plate colours would be seen only above and below the sun, and not at either side. I have never seen any sign of such a tendency.

He says :—" Some few times in one's life the display may be seen in all quarters of the sky, and with the sun well up in the heavens." If the phrase "in all quarters of the sky" is to be taken literally, the phenomenon is quite different from anything I have seen, and can, indeed, be scarcely explained on my hypothesis.

Another phenomenon, nearly related to iridescence, is that of coronæ. I have seen these in the winter at St. Moritz, when the sky was covered with a white haze, gathered here and there into more definite wisps. The colours were much fainter, and were arranged in regular rings. I have distinguished two complete spectra, the size of the rings pointing to filaments about ·04 millim. in diameter.

Both coronæ and iridescence are also produced by water-drops, and I have calculated the brightness of the cloud in this case also. The results for the first, second, and fourth spectra are :—

$$\cdot 0071\alpha/\psi^2,$$
$$\cdot 0046\alpha/\psi^2,$$
$$\cdot 0027\alpha/\psi^2,$$

where ψ, as before, is expressed in degrees, and the unit of brightness is that of the sun. These are only slightly greater than the corresponding figures for ice-clouds, and the difference is no doubt considerably more than compensated by the tendency of water-drops to coalesce and lose their uniformity of size.

Coronæ, too, are sometimes seen in water-mists, surrounding the point opposite the sun. In this case the expression no longer contains the variable $1/\psi^2$, and for the first spectrum has the value $\cdot 000002\alpha$. Theory renders it highly improbable that such coronæ should ever be seen in ice-clouds. For we have for the first spectrum the very small value $\cdot 00000001\alpha/\psi$. It is curious that the brightness increases with the size of the ring. But even with such a large ring as $\psi = 20°$, the brightness is only one tenth of that for a water-cloud. The reason is that comparatively few filaments are in a position to send light in the required direction.

Since the above was set up in type I have come across, in a paper by Lord Rayleigh, a reference to a very important proposition proved by Verdet*. This is to the effect that, owing to the finite angular magnitude of the sun, we can only consider an uniform plane wave of direct sunlight to extend over a circle about 100λ in diameter. At points separated by a greater distance than this there is no permanent relation of phase between the vibrations.

Now the ice-filaments are no doubt often much longer than 100λ. So we must divide them into sections, each somewhat shorter than 100λ, and treat each section as a separate filament. This will make no difference to the result; for, on referring to the argument above, it will be seen that the length b disappears in the course of the proof. It is true that the light from each filament is diminished, the diffracted light being proportional to the square of the area; but, on the other hand, a greater latitude of orientation is permissible.

In the case of the coronæ cited above, the thickness of the filaments must have nearly reached the theoretical limit. And it is at least probable that coronæ of that size would be seen in clouds in which the filaments were of very irregular thickness. Those of smaller diameter would be ineffective owing to their weaker diffracting-power, while those of greater diameter would be incapacitated by being outside the theoretical limit.

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* Tome i. § 28; and Strutt, "On Coronas," London Math. Soc. vol. iii. No. 36.

LV. *On Peculiarities observed in Iron and Steel at a Bright-Red Heat.* *By* H. F. Newall, *M.A., Demonstrator of Experimental Physics, Cambridge University**.

In the September number (p. 256) of the Philosophical Magazine, which I am sorry not to have seen till last week (Oct. 6), there appears a paper by Mr. Herbert Tomlinson on a "Remarkable Effect on raising Iron when under Temporary Stress or Permanent Strain to a Bright-red Heat." I have been engaged for some time past on experiments bearing on the subject of his paper, and as I hope before long to be able to publish a full account of my experiments, I will not at present do more than put down a few notes on such of the more definite results that I have obtained as seem to throw light on the interesting phenomena recorded in Mr. Tomlinson's paper.

First I will describe what I observe as to Reglow or Recalescence. A wire (about 1 mm. diam.) steel or iron, of certain composition, is heated gradually by a strong current or in the flame of a Bunsen burner; it becomes luminous; the luminosity increases as the temperature rises, until a certain point is reached, when in some cases it pauses in its increase, in others it actually diminishes: this I will refer to as the Darkening; and then it increases again until the wire melts. However, let the heating be stopped before melting takes place and let the wire cool gradually, or, rather, let it be placed in such circumstances as one would naturally expect would promote gradual cooling: the luminosity decreases until a certain point is reached, roughly speaking about dull red heat, when it ceases to decrease and in some cases only pauses, in others it actually increases: this is spoken of as the Reglow; and then it diminishes again until the wire is no longer luminous.

I am still working with a view to finding out to what differences in composition are to be attributed the differences observed in the effects just described. I am fortunate enough to have enlisted the interest of Col. Dyer, of Lord Armstrong's firm, and of Mr. Jenkins, of the Consett Iron Works, and of Mr. Firth, of Sheffield, who have most kindly supplied me with many specimens of steel of known composition. After a great many experiments and observations,—many of them merely careful repetitions and extensions of Gore's results and of Barrett's, many of them, however, new,—I

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* Communicated by the Author.

incline to regard the change going on in iron and steel at high temperatures as partaking of the nature of an explosion, in that once started it continues throughout the mass of the iron, and is evidenced amongst other phenomena by the reglow. Until the "darkening" has taken place the iron is not in a state fit to explode, so to speak; and hence it is necessary to raise the iron above this temperature in order that reglow may be possible. But before the iron can be raised above this temperature, heat must have passed into it in sufficient quantity to separate the elements producing the explosion. My evidence for this view of the matter I hope to publish in full.

Briefly, I have shown that reglow is not due to chemical action at the surface of the iron; that it is not due to occlusion of gases; that it is not due to differences in conductivity in iron at different temperatures, as suggested by Forbes; that there is a rise of temperature not only at the surface, as has been shown by Barrett, but also throughout the mass; that this rise of temperature will partly account for peculiarities—(1) in the thermoelectric properties, observed by Tait; (2) in the electrical conductivity, observed by Smith, Knott, and Macfarlane; (3) in the thermal expansion, observed by Gore and by Barrett; (4) in the rigidity, observed by Tomlinson and independently by myself; (5) in the viscosity, observed by myself, and probably by Barus and Strouhal, though I cannot find mention of it; (6) in the possibility of hardening, observed by myself; (7) in the return of magnetic properties, recorded by many observers.

My present aim is to find out the cause of this rise in temperature; and my belief is that it is an internal chemical action; and the question is, What is this action? I hope to clear the matter up by experimenting upon specimens of known composition. So far as I have gone I have come across very curious differences in the effects shown by different chance specimens. In some specimens the reglow takes place much more leisurely than in others; sometimes it is practically a flash, and at other times it is not perceptible at all with the most careful observation. I reserve a detailed account of my observations, and at present will only mention in this connexion that I am not inclined to attribute any important part in the phenomena to the appearance of magnetic properties; and I realize how, if my view of the cause of reglow is correct, a difficult field for inquiry is opened out, necessitating the production of evidence that the properties of iron are different above and below the temperature of the reglow phenomena, or, rather, that the temperature-coefficients of the various physical properties suddenly change at that critical temperature.

I will now return to the subject of Mr. Tomlinson's paper; and as I have independently made observations on similar points, I will give my view of the case, as applied to Mr. Tomlinson's experiments. In Experiment I. a wire was heated under torsional stress, and when a certain temperature was reached the rigidity gave way somewhat suddenly and the wire was permanently twisted. "The slight check in the rate at which the wire was permanently twisting" is to be attributed to a similar check in the rate of decrease of rigidity at the "darkening." The wire was allowed to cool; the rigidity increases until the reglow takes place, and this involves a sudden rise in temperature with corresponding fall in rigidity, and the wire twists further.

I have been nonplussed by what appears to be an inconsistency in the results recorded in Experiments I. and II. In Experiment I. the stress produces permanent strain; in II. a very feeble stress (magnet deflected through 10°) produces a permanent strain; but an intermediate stress (magnet deflected through 90°) is not stated to have produced anything but a temporary effect at the higher temperature; and this effect—untwisting during the heating of the wire—is such as to overcome the external stress, as if we had to deal with some instability. I hesitate to attempt an explanation of the first part of Experiment II.; the second part admits of an attempt, suggesting further tests.

In Experiment II. (second part), then, we have to deal with the case of a wire under very feeble external stress without permanent strain; as the temperature rises there is a sudden untwist, that is, the needle is deflected further from the meridian, and also permanent twist; and as the wire cools, there is a sudden temporary twist at nearly the same temperature. I think it possible to explain the untwist by attributing it to a pause in the permanent twisting, resulting in a swing of the magnet temporarily from the meridian; the pause being due to the fact that the "darkening" is taking place, and with it a pause in the decrease of rigidity. It would be interesting to repeat this experiment with a view to testing how far all the movements of the needle actually represent stresses in the wire.

Experiments IV. and V. are of great interest as showing both the effect of permanent stress (I leave out of the case the magnetic effects observed), and the failure of a very common method of annealing.

With permanent torsional strain are introduced internal torsional stresses, which differ not only in magnitude but even in direction in different annuli of the wire's section when the external stress is removed; that is to say, if we regard the wire in this condition as divided into two parts, a core and an enclosing tube or rind, we may choose the radius of the core so that the internal stresses in the core are opposed to those in the rind. If by any means we can weaken the rigidity or elasticity in one part more quickly than that in the other, it is clear that we should get the permanent strain either increased or diminished accordingly. Here, then, we have another way in which the results recorded in Mr. Tomlinson's paper, "On the Effect of Change of Temperature in Twisting or Untwisting Wires" (p. 253), may be regarded. I will not dwell upon these, but will deal with Experiment IV., in which a wire permanently twisted is heated, and shows twist in heating and untwist in cooling. We have then to show that in heating the inner core is heated more quickly than the outer rind, in which case the wire will untwist. This case presents little difficulty, except in that at one point during the heating the core may be cooler than the rind, namely, if the wire shows "darkening," with a fall of temperature. Next we have to show that at a point in the cooling the outer rind can become hotter than the core. Now in cooling, the rind reaches the temperature at which reglow occurs before the core; and so it is possible that the rind may become considerably hotter than the core, its rigidity also becoming less, and the wire therefore is untwisted. The suddenness of the twist or untwist depends on the shortness of the period (in both heating and cooling) during which the rigidities of the core and rind differ markedly. At low temperatures the rigidity apparently does not vary much with temperature, or at least there is not much greater variation of rigidity in the permanently strained rind than in the core, and the internal couples are almost balanced. But at a certain temperature the rigidity suddenly falls, as is shown by Experiment I., and the balance of couples in the unequally heated wire is destroyed until the wire is heated so much above this critical temperature that an approximate balance of weaker couples is again established. If the temperature of the wire is kept at about a dull red heat (as seen from outside), it is possible to keep the balance of couples so far disturbed as to get nearly all the permanent twist out of the wire.

The explanation above suggested is based on (1) the fact that the reglow is connected with a rise of temperature—this is established by my own experiments; (2) the fact that there is a sudden fall in rigidity at a temperature about red heat—this is established by Mr. Tomlinson's Experiment I. as well as by similar experiments of my own; (3) the assumption (? fact) that a wire of even 1 mm. diam. is not heated uniformly throughout its thickness, but that the inner part is hotter than the parts near the surface; and lastly that the rigidity of the iron at the high temperature of reglow is less than the rigidity of iron at not so high a temperature before reglow.

A similar line of argument will explain the results recorded in Experiments VI. and VII.

I have made a great number of experiments on the return of magnetic properties in cooling iron; and can at present only say with certainty that the matter is not so simple as appears from Experiment IV. of Mr. Tomlinson's paper. The relation between the reglow and return of magnetic properties varies with the specimens of iron and steel used. I have found some few specimens in which the reglow takes place in the middle of the return of magnetic properties, so that the galvanometer indication is *two* kicks, the first very small, just before reglow, the second and main kick after reglow, as if the metal was becoming magnetic, when the rise of temperature, coincident with reglow, caused it to pause for an instant. In general, however, the reglow precedes the return of magnetic properties, though by intervals which vary with the specimens observed. I hope to have more definite results when I have tested the numerous specimens of steel and iron which I have now in my possession.

LVI. *On "Random Scattering" of Points on a Surface.* By JOSEPH KLEIBER, *Privat-Docent in the Imperial University of St. Petersburg.*[*]

IN a paper published in the Philosophical Magazine [ser. 3], vol. xxxvii. pp. 401–427 [†], Prof. Forbes set forth some objections against the application of the principles of the mathematical theory of probabilities to the question of the distribution of stars in the sky. Mr. Todhunter quotes this paper in his well-known 'History of the Theory of Probabi-

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[*] Communicated by the Author.

[†] "On the Alleged Evidence for a Physical Connexion between Stars forming Binary or Multiple Groups, deduced from the Doctrine of Chances."

lities,' agreeing apparently with the views of its author. He writes :—

"The late Prof. Forbes wrote a very interesting criticism on Mitchell's memoir. He objects with great justice to Mitchell's mathematical calculations, and he altogether distrusts the validity of the inference drawn from these calculations" (History &c. p. 334).

Relying on so high an authority, the same paper is referred to by J. Jevons in his 'Principles of Science.' He says :—

"The calculations of Mitchell have been called in question by the late James D. Forbes, and Mr. Todhunter vaguely countenances his objections, otherwise I should not have thought them of much weight" (Principles &c. vol. i. p. 286).

Now the article in question contains a very erroneous conception of the "law of great numbers," and its application to the investigation of accidental distribution, and the objections of Prof. Forbes against this application, together with the experiments made by him on the random distribution of grains thrown on a chess-board, furnish, on the contrary, a very good argument for and illustration of the very same views they are intended to invalidate.

Misinterpretations of the laws of "random scattering" like those of Prof. Forbes are not very rare, especially in the writings of statisticians, so I thought it worth while to show the error of these misconceptions.

It is a common error to confound random scattering with uniform distribution. It is true that the most probable result of a series of drawings from an urn containing an equal number of black and white balls will be an equal number of both; but this result is in itself very improbable, because this is but one out of a great number of possible events, all giving unequal distribution, less probable individually, but more probable in sum, than the most probable result. Also, the most probable distribution of points on a surface, if scattered at random, is a uniform one, but this is very improbable. The *probable result* of a random scattering is therefore *not a uniform* distribution.

Let n points be distributed on a surface divided into m equal parts (squares). Then, although the probable number of points in each square is $\frac{n}{m}$, it is very probable that there will be squares containing less than $\frac{n}{m}$ points, while others will contain more.

Let us find the probable number of squares containing a given number of points. The probability of a given point being in a given square is $\frac{1}{m}$; the inverse probability of its not being in this square is

$$1-\frac{1}{m}.$$

Hence the probability that this square contains i points and no more is

$$p_i=\frac{n!}{i!(n-i)!}\left(\frac{1}{m}\right)^i\left(1-\frac{1}{m}\right)^{n-i};$$

and the probable number of squares containing i points will be mp_i, or

$$m\cdot\frac{n!}{i!\,(n-i)!}\left(\frac{1}{m}\right)^i\left(1-\frac{1}{m}\right)^{n-i}. \quad . \quad . \quad . \quad (1)$$

We will apply these formulæ to the experiments described by Prof. Forbes. He writes:—

"I have thought it worth while to test a little by simple experiment the differences to which 'mere chance' gives rise in the grouping of bodies dispersed over a surface, by a method of 'random scattering,' which I conceive to be as nearly as possible analogous to Mitchell's idea of chance as affecting the placing of the stars. . . . I placed a chess-board, having, as usual, sixty-four squares, on the floor, and I provided a large sieve into which I put a quantity of grains of rice, which did not fall through the sieve until it was somewhat shaken. I then shook the sieve at a considerable height above the chess-board until it was pretty well scattered over with grains. . . . The following diagrams contain the results of five experiments, the number of grains which fell on each of the sixty-four squares being counted and registered. . . . In these experiments we observe that the most loaded squares contain from nearly two to more than four times the *average* number of grains, whilst in four out of five experiments one or more squares were vacant. . . . If we were to take any one of these experiments, and attempt to calculate the antecedent probability of the grains so arranging themselves on Mitchell's supposition, we should unquestionably find numerical chances far greater against these configurations being the result of accident, than those on which we are told that we form our most certain ordinary judgments. Thus, if an experimental argument may be admitted, the reasoning of Mitchell and his followers is altogether fallacious."

But if we calculate the probable number of squares containing a given number of grains in these experiments, putting in our formula (1) $m=64$, $n=$ total number of grains, we find, on the contrary, deductively, a distribution very similar to that obtained by Prof. Forbes in his experiments.

The following diagram represents the results of Prof Forbes's first experiment.

Experiment I.

4	3	3	6	2	4	2	1
4	4	4	3	1	4	3	1
6	2	3	2	3	5	3	2
3	0	4	3	4	7	1	3
0	4	4	4	0	3	4	4
3	2	3	3	2	5	5	5
2	3	3	0	3	5	1	1
1	3	3	2	1	2	0	2

In the following table I give the comparisons between the theoretical and observed numbers of squares containing given numbers of points.

Number of grains in a square.	Number of squares.	
	Observed.	Calculated.
0	5	3·6
1	8	10·4
2	11	15·1
3	19	14·4
4	13	10·3
5	5	5·8
6	2	2·8
7	1	1·1
Above . . .	0	0·4

The observed and calculated values agree sufficiently. A still more close agreement is presented by Experiment II.

Experiment II.

2	7	4	1	6	5	5	3
5	3	3	5	6	7	2	8
7	6	3	2	7	6	1	4
4	8	8	3	4	5	2	4
0	5	6	7	4	2	3	7
1	6	3	4	6	8	5	2
2	4	4	7	5	3	4	8
2	4	5	5	3	3	3	4

Number of grains in a square.	Number of squares.	
	Observed.	Calculated.
0	1	0·8
1	3	3·4
2	8	7·6
3	11	11·2
4	12	12·4
5	10	10·9
6	7	8·0
7	7	5·0
8	5	3·0
Above	0	1·7

We see from these diagrams that the experiments of Prof. Forbes by no means contradict the formulæ of probabilities applied to the question of distribution by chance.

But these experiments are not quite free from objections. It might be stated that by throwing grains of rice on a chess-board we must obtain a *more uniform* rather than an accidental distribution, as it is evident that the number of grains being augmented so as to cover the whole surface of the chess-board, we shall have a pretty equal number of grains on each square. Another arrangement of the same experiment may be less objectionable, and I tried it, in order to give a more complete illustration of the application of the principle of random scattering.

Taking a table of logarithms with seven places, I considered the last two figures of the logarithms of a series of consecutive numbers, as designating the coordinates of points, which I supposed distributed within a square divided, for convenience, into 100 equal parts ; so that the sixth place of the mantissa designated the line, the seventh place the column in which the point was to be put. Two hundred and fifty points were distributed in this manner, and the result was as follows :—

6	2	1	2	1	1	3	2	2	4
3	0	2	3	4	2	5	1	2	5
2	1	5	3	1	3	5	0	1	2
3	7	4	6	1	1	4	1	0	0
1	1	2	3	3	2	4	0	6	2
1	4	2	3	2	1	3	2	2	1
3	4	2	4	3	5	4	2	1	1
1	2	2	2	2	2	1	3	1	4
1	5	2	5	1	3	3	4	0	1
0	2	4	3	5	2	2	6	4	2

Applying to this case the formula above, in which we put

$$m=0, \quad n=50,$$

we obtain the following result :—

Number o points in a square.	Number of squares.	
	Observed.	Calculated.
0	7	8·1
1	23	20·5
2	28	25·7
3	17	21·5
4	13	13·4
5	7	6·7
6	4	2·8
7	1	1·0
Above	0	0·4

Summing up our conclusions, we see that, contrary to the opinion of Prof. Forbes, the theory of probabilities does not affirm that a "perfectly uniform and symmetrical disposition of the stars over the sky would (if possible) be that which could alone afford no evidence of causation or any interference with the laws of 'random.'" On the contrary, I have endeavoured to show that this theory gives a sufficient account of the possible irregularities of distribution such as those observed by Prof. Forbes in his experiments, or those presented by the stars in the sky. The ingenious argumentation of Mitchell respecting the improbability of the observed groups of stars being merely accidental is by no means in contradiction with the true principles of probability, and the exact meaning of the "law of great numbers."

St. Petersburg, October 2, 1887.

LVII. *Intelligence and Miscellaneous Articles.*

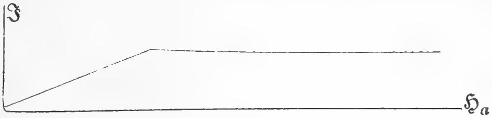
MAGNETIC CIRCULAR POLARIZATION IN COBALT AND NICKEL.

BY H. E. J. G. DU BOIS.

If we consider an infinitely extended plate of any thickness in a uniform magnetic field normal to its plane sides; the material being supposed homogeneous, isotropic, and devoid of retentiveness, the induced magnetization $\mathfrak{J}$ is given by the equation

$$\mathfrak{J}=\frac{1}{4\pi+1/\kappa}\mathfrak{H}_a,$$

where $\mathfrak{H}_a$ is the original field, κ the susceptibility. From Rowland's data for "Burden's best" iron, cast cobalt, and cast nickel the relation between κ (an essentially variable quantity in the case of these metals), and $\mathfrak{J}$ is known, and curves are accordingly plotted corresponding to the above equation. Their shape is represented with great approximation in the annexed diagram. The ordinate of the singular point is the maximum $\mathfrak{J}_m$ given by Rowland, its abscissa is $4\pi\,\mathfrak{J}_m$; the absolute values stand as follows:—



	"Burden's best" iron.	Cast cobalt.	Cast nickel.
Ordinate	1390	880	494 C.G.S.
Abscissa	17440	11060	6200 C.G.S.

Next follows an experimental investigation of transparent films of cobalt and nickel prepared galvanoplastically. These, as was discovered by Kundt in 1884, rotate the plane of polarization of transmitted radiations when placed equatorially in a magnetic field. No residual rotation, corresponding to any residual magnetism on the suppression of the magnetizing field, could be detected.

The rotation was found by Kundt to be proportional to the thickness in the case of iron; as was to be expected, this was verified for cobalt and nickel.

The maximum "twist" (*i. e.* rotation per unit thickness) occurring in a field of infinite intensity was found to be, for red light:—

In Nickel	89000° per centimetre.
In Cobalt	198000° per centimetre.
Iron (Kundt)	209000° per centimetre.

The curves obtained by Kundt, in which the rotation in iron films is plotted as a function of the field which magnetizes them, were also experimentally determined for cobalt and nickel.

For all three metals it was found that these experimental curves have ordinates proportional to those of the magnetic curves above illustrated, the abscissa of the singular point being always the same for a given metal; proportionality existing within the limits of experimental error and due regard being paid to the difference in chemical and physical constitution of Rowland's material and the electrolysed films employed.

The magnetic rotation is therefore directly proportional to the magnetization $\mathfrak{J}$.—Wiedemann's *Annalen*, vol. xxxi. p. 941 (1887).

ON THE SOLIDIFICATION OF LIQUIDS BY PRESSURE.

BY E.-H. AMAGAT.

Theoretically, the hypothesis of J. Thomson enables us to predict that, at a given temperature, the solidification of a body will be possible under a sufficient pressure, on condition that its density be greater in the solid than in the liquid state.

It has been verified, for ice by Sir W. Thomson and by Mousson, and for various solid bodies by Bunsen, Hopkins, and, recently, by M. Batelli; but no example is known of liquids properly so-called which have been reduced to the solid state by pressure alone, even among those which are very readily solidified by cold, such, for example, as benzene.

In my researches on the expansion and compressibility of liquids, I have examined, between 0° and 50° and at pressures increasing to above 3000 atmospheres, a fairly large number of substances belonging either to inorganic or to organic chemistry. None of these had shown signs of solidification, when the idea struck me to examine bichloride of carbon (C_2Cl_4). In my first trials, I was stopped by difficulties which led me to suspect at once that this body (unknown, moreover, in the solid state) was solidified by pressure; I forthwith made the following experiment, which is only the reverse of that of Mousson with ice.

The liquid is compressed in a bronze cylinder, of which the upper part is closed by a kind of plug of soft iron, which is at the same time the prolongation of the pole of an electromagnet. In the liquid a small cylinder of soft iron is free to move, sinking by its own weight, and which, at the moment of closing the current, is attracted and, traversing the liquid, strikes the plug. Under a sufficient pressure, the noise produced by the collision, which is audible at several metres distance, ceases to be heard; it is heard anew as soon as the pressure is sufficiently diminished. The pressure at which the little cylinder ceased to move was, in this experiment, about 1500 atmospheres.

I have since carried out the following arrangement, which has enabled me not only to see the solidification take place, but also to obtain perfectly regular crystals and to photograph them. The chloride of carbon is enclosed in a steel chamber having, (one in front the other behind,) two horizontal holes formed by small cones of annealed glass, making a smooth joint by means of a conical envelope of very thin ivory. A ray of electric light traverses these two cones (and the liquid compressed between them) parallel to their common axis, and falls upon a telescope fixed in the same direction: we can, under these conditions, easily follow the course of the phenomenon. The apparatus is, moreover, arranged in such a manner as to permit of its being maintained at a constant temperature by a current of water, by ice, or by a freezing-mixture.

The observations are made by bringing into focus the back face of the cone turned towards the telescope; the solidification in taking place presents very different appearances according to the degree of quickness with which the pressure is produced. If this is done rapidly, there is seen to spring up suddenly at the periphery of the luminous field a wreath of crystals more and more closely packed and opaque; this quickly and regularly spreads to the centre, which soon becomes reduced to a luminous point and disappears in its turn. If we continue to compress it, the field remains for some time completely obscured, then little by little it is illumined and the mass becomes transparent again: one could believe that it had again become liquid. If the pressure be then gradually diminished, the tangle of crystals reappears and the field again becomes obscure; at length, the pressure diminishing continuously, the light reappears anew, and the crystals dissolve and dislodge themselves from the mass, they are seen to sink across the liquid portion; thus they are, agreeably to hypothesis, heavier than the latter.

By taking certain precautions it is possible to obtain and to preserve for some time crystals detaching themselves very regularly from the portion which remains liquid; these can then be photographed. Parallelipipeds and right octahedra, which appear to belong to the cubic system, are readily distinguished.

The determination of the pressure under which the solidification takes place at different temperatures presents some difficulties; the liquid is warmed by the compression, it must be compressed slowly, and the moment of crystallization is more difficult to hit upon. The inverse phenomenon taking place with diminution of pressure, we obtain a pressure of fusion and a pressure of solidification, the interval between which is made as small as possible and the mean taken.

I have thus found that chloride of carbon solidifies:—

At −19°·5 under a pressure of 210 atmospheres.
„ 0° „ „ „ 620 „
„ 10° „ „ „ 900 „
„ 19°·5 „ „ „ 1160 „

I have made one experiment with protochloride of carbon (C_4Cl_4); at 0° it is not solidified under a pressure of 900 atmospheres.

I have likewise examined benzene, which, at 0°, solidifies under the normal pressure; but an accident having interrupted these researches, I have only been able to prove that at 22° it crystallizes in beautiful feathers under a pressure of about 700 atmospheres (approximate).

The question arises whether there is not, for each liquid, a temperature above which solidification cannot take place under any pressure, that is to say a critical temperature of solidification, in the same way that there appears to be a temperature below which the body remains solid under the feeblest pressures. This I must leave for further investigation.—*Comptes Rendus*, July 18, 1887.

ON A NEW VOLTAIC BATTERY.

BY F. FRIEDERICHS IN STÜTZERBACH.

The novelty in this battery consists in the mode of filling and emptying the exciting liquid, by an arrangement met with in some forms of apparatus for generating sulphuretted hydrogen. A series of tubulated bell-glasses, supported with their necks downwards, are connected by means of caoutchouc-tubes with a horizontal glass tube containing as many tubulures as there are glass vessels. One end of this horizontal tube is connected by means of an india-rubber tube with a tubulure in a large glass reservoir containing the exciting liquid. This vessel can be raised or lowered to any desired extent, and thus the cells can be filled to any height or can be emptied. The emptying is facilitated by a stopcock in the other end of the tube.

If it is wished to use a smaller number of cells the india-rubber tube connecting the cell and the horizontal tube can be clamped by a stopcock.—Wiedemann's *Annalen*, No. 9, 1887.

THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FIFTH SERIES.]

DECEMBER 1887.

LVIII. *On the Relative Motion of the Earth and the Luminiferous Æther.* *By* Albert A. Michelson *and* Edward W. Morley*.

THE discovery of the aberration of light was soon followed by an explanation according to the emission theory. The effect was attributed to a simple composition of the velocity of light with the velocity of the earth in its orbit. The difficulties in this apparently sufficient explanation were overlooked until after an explanation on the undulatory theory of light was proposed. This new explanation was at first almost as simple as the former. But it failed to account for the fact proved by experiment that the aberration was unchanged when observations were made with a telescope filled with water. For if the tangent of the angle of aberration is the ratio of the velocity of the earth to the velocity of light, then, since the latter velocity in water is three-fourths its velocity in a vacuum, the aberration observed with a water telescope should be four-thirds of its true value†.

On the undulatory theory, according to Fresnel, first, the

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* Communicated by the Authors.

This research was carried out with the aid of the Bache Fund.

† It may be noticed that most writers admit the sufficiency of the explanation according to the emission theory of light; while in fact the difficulty is even greater than according to the undulatory theory. For on the emission theory the velocity of light must be greater in the water telescope, and therefore the angle of aberration should be less; hence, in order to reduce it to its true value, we must make the absurd hypothesis that the motion of the water in the telescope carries the ray of light in the opposite direction!

æther is supposed to be at rest, except in the interior of transparent media, in which, secondly, it is supposed to move with a velocity less than the velocity of the medium in the ratio $\frac{n^2-1}{n^2}$, where n is the index of refraction. These two hypotheses give a complete and satisfactory explanation of aberration. The second hypothesis, notwithstanding its seeming improbability, must be considered as fully proved, first, by the celebrated experiment of Fizeau*, and secondly, by the ample confirmation of our own work†. The experimental trial of the first hypothesis forms the subject of the present paper.

If the earth were a transparent body, it might perhaps be conceded, in view of the experiments just cited, that the intermolecular æther was at rest in space, notwithstanding the motion of the earth in its orbit; but we have no right to extend the conclusion from these experiments to opaque bodies. But there can hardly be any question that the æther can and does pass through metals. Lorentz cites the illustration of a metallic barometer tube. When the tube is inclined, the æther in the space above the mercury is certainly forced out, for it is incompressible‡. But again we have no right to assume that it makes its escape with perfect freedom, and if there be any resistance, however slight, we certainly could not assume an opaque body such as the whole earth to offer free passage through its entire mass. But as Lorentz aptly remarks: "Quoi qu'il en soit, on fera bien, à mon avis, de ne pas se laisser guider, dans une question aussi importante, par des considérations sur le degré de probabilité ou de simplicité de l'une ou de l'autre hypothèse, mais de s'addresser a l'expérience pour appendre à connaître l'état, de repos ou de mouvement, dans lequel se trouve l'éther à la surface terrestre." §

In April, 1881, a method was proposed and carried out for testing the question experimentally‖.

In deducing the formula for the quantity to be measured,

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* *Comptes Rendus,* xxxiii. p. 349 (1851); Pogg. *Ann.* Ergänzungsband, iii. p. 457 (1853); *Ann. Chim. Phys.* [3], lvii. p. 385 (1859).

† "Influence of Motion of the Medium on the Velocity of Light." Am. J. Sci. [3], xxxi. p. 377 (1886).

‡ It may be objected that it may escape by the space between the mercury and the walls; but this could be prevented by amalgamating the latter.

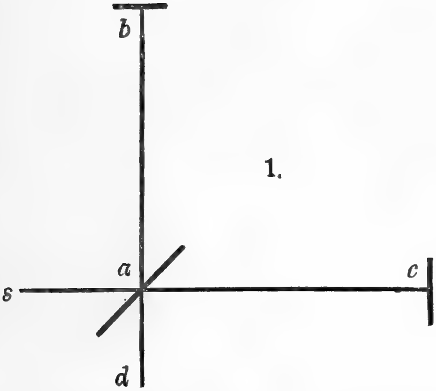
§ *Archives Néerlandaises,* xxi. 2^{me} livr. Phil. Mag. [5], xiii. p. 236.

‖ "The Relative Motion of the Earth and the Luminiferous Æther,' by Albert A. Michelson. Am. J. Sci. [3], xxii. p. 120.

the effect of the motion of the earth through the æther on the path of the ray at right angles to this motion was overlooked*. The discussion of this oversight and of the entire experiment forms the subject of a very searching analysis by H. A. Lorentz†, who finds that this effect can by no means be disregarded. In consequence, the quantity to be measured had in fact but half the value supposed, and as it was already barely beyond the limits of errors of experiment, the conclusion drawn from the result of the experiment might well be questioned ; since, however, the main portion of the theory remains unquestioned, it was decided to repeat the experiment with such modifications as would insure a theoretical result much too large to be masked by experimental errors. The theory of the method may be briefly stated as follows :—

Let *sa*, fig. 1, be a ray of light which is partly reflected in *ab*, and partly transmitted in *ac*, being returned by the mirrors *b* and *c* along *ba* and *ca*. *ba* is partly transmitted along *ad*, and *ca* is partly reflected along *ad*. If then the paths *ab* and *ac* are equal, the two rays interfere along *ad*. Suppose now, the æther being at rest, that the whole apparatus moves in the direction *sc*, with the velocity of the earth in its orbit, the directions and distances traversed by the rays will be altered thus :—The ray *sa* is reflected along *ab*, fig. 2 ;

Fig. 1.



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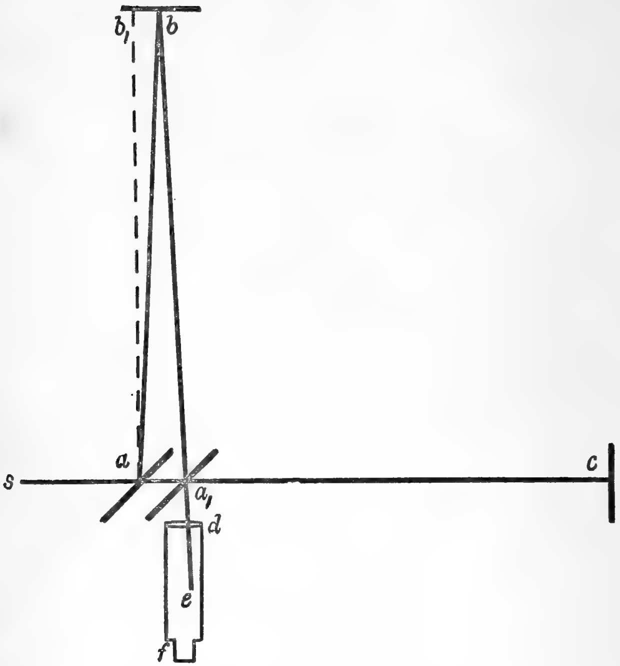
* It may be mentioned here that the error was pointed out to the author of the former paper by M. A. Potier, of Paris, in the winter of 1881.

† "De l'Influence du Mouvement de la Terre sur les Phen. Lum." *Archives Néerlandaises,* xxi. 2^me^ livr. (1886).

the angle $bab_{,}$ being equal to the aberration $=\alpha$, is returned along $ba_{,}$, $(aba_{,}=2\alpha)$, and goes to the focus of the telescope, whose direction is unaltered. The transmitted ray goes along ac, is returned along $ca_{,}$, and is reflected at $a_{,}$, making $ca_{,}e$ equal $90-\alpha$, and therefore still coinciding with the first

Fig. 2.



ray. It may be remarked that the rays $ba_{,}$ and $ca_{,}$ do not now meet exactly in the same point $a_{,}$, though the difference is of the second order; this does not affect the validity of the reasoning. Let it now be required to find the difference in the two paths $aba_{,}$ and $aca_{,}$.

Let V = velocity of light.
v = velocity of the earth in its orbit.
D = distance ab or ac, fig. 1.
T = time light occupies to pass from a to c.
$T_{,}$ = time light occupies to return from c to $a_{,}$ (fig. 2).

Then

$$T=\frac{D}{V-v} \qquad T_{,}=\frac{D}{V+v}$$

The whole time of going and coming is

$$T+T_{,}=2D\frac{V}{V^2-v^2},$$

and the distance travelled in this time is

$$2D\frac{V^2}{V^2-v^2}=2D\left(1+\frac{v^2}{V^2}\right),$$

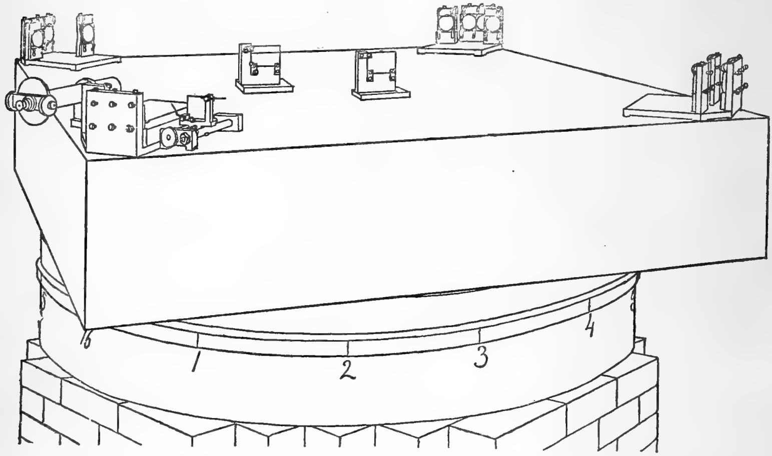
neglecting terms of the fourth order. The length of the other path is evidently $2D\sqrt{1+\frac{v^2}{V^2}}$, or to the same degree of accuracy, $2D\left(1+\frac{v^2}{2V^2}\right)$. The difference is therefore $D\frac{v^2}{V^2}$. If now the whole apparatus be turned through 90°, the difference will be in the opposite direction, hence the displacement of the interference-fringes should be $2D\frac{v^2}{V^2}$. Considering only the velocity of the earth in its orbit, this would be $2D\times10^{-8}$. If, as was the case in the first experiment, $D=2\times10^6$ waves of yellow light, the displacement to be expected would be 0·04 of the distance between the interference-fringes.

In the first experiment, one of the principal difficulties encountered was that of revolving the apparatus without producing distortion; and another was its extreme sensitiveness to vibration. This was so great that it was impossible to see the interference-fringes except at brief intervals when working in the city, even at two o'clock in the morning. Finally, as before remarked, the quantity to be observed, namely, a displacement of something less than a twentieth of the distance between the interference-fringes, may have been too small to be detected when masked by experimental errors.

The first-named difficulties were entirely overcome by mounting the apparatus on a massive stone floating on mercury; and the second by increasing, by repeated reflexion, the path of the light to about ten times its former value.

The apparatus is represented in perspective in fig. 3, in plan in fig. 4, and in vertical section in fig. 5. The stone *a* (fig. 5) is about 1·5 metre square and 0·3 metre thick. It rests on an annular wooden float *bb*, 1·5 metre outside diameter, 0·7 metre inside diameter, and 0·25 metre thick. The float rests on mercury contained in the cast-iron trough *cc*, 1·5 centimetre thick, and of such dimensions as to leave a clearance of about one centimetre around the float. A pin d, guided by arms $g\,g\,g\,g$, fits into a socket e attached to the float. The pin may be pushed into the socket or be withdrawn, by a lever pivoted at f. This pin keeps the float concentric with the trough, but does not bear any part of the weight of the stone. The annular iron trough rests on a bed of cement on a low brick pier built in the form of a hollow octagon.

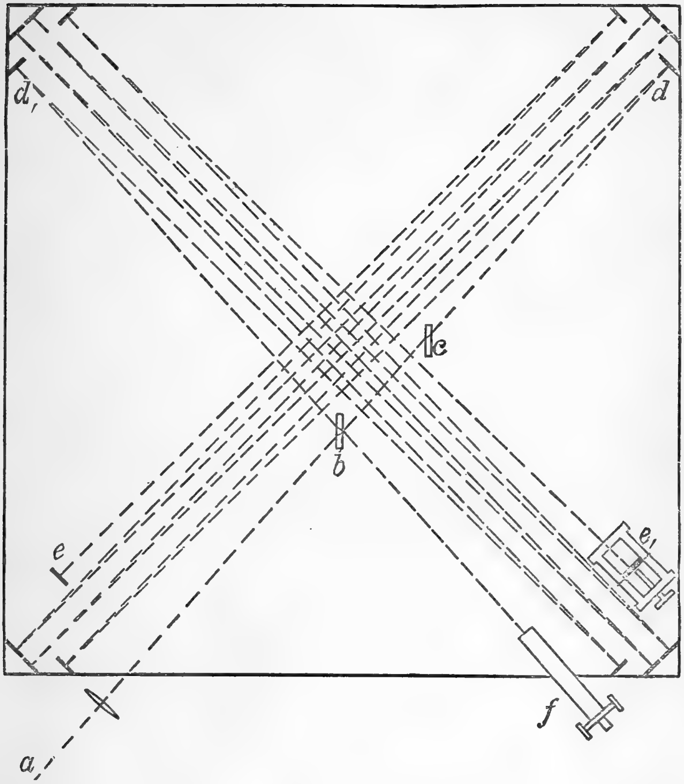
Fig. 3.



At each corner of the stone were placed four mirrors $d\,d\,e\,e$, fig. 4. Near the centre of the stone was a plane parallel glass b. These were so disposed that light from an argand burner a, passing through a lens, fell on b so as to be in part reflected to d_1; the two pencils followed the paths indicated in the figure, $b\,d\,e\,d\,b\,f$ and $b\,d_1\,e_1\,d_1\,b\,f$ respectively, and were observed by the telescope f. Both f and a revolved with the stone. The mirrors were of speculum metal carefully worked to optically plane surfaces five centimetres in diameter, and the glasses b and c were plane parallel of the same thickness, 1·25 centimetre; their surfaces measured 5·0 by 7·5 centimetres. The second of these was placed in the path of one of the pencils to compensate for the passage of the other through the same thickness of glass. The whole of the optical portion of the apparatus was kept covered with a wooden cover to prevent air-currents and rapid changes of temperature.

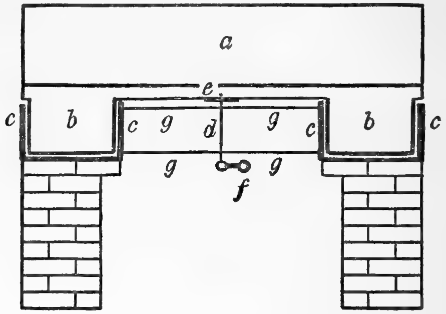
The adjustment was effected as follows:—The mirrors having been adjusted by screws in the castings which held the mirrors, against which they were pressed by springs, till light from both pencils could be seen in the telescope, the lengths of the two paths were measured by a light wooden rod reaching diagonally from mirror to mirror, the distance being read from a small steel scale to tenths of millimetres. The difference in the lengths of the two paths was then annulled by moving the mirror $e_{\prime}$. This mirror had three adjustments: it had an adjustment in altitude and one in azimuth, like all the other mirrors, but finer; it also had an adjustment in the direction of the incident ray, sliding forward or backward, but keeping very accurately parallel to its former plane. The three adjustments of this mirror could be made with the wooden cover in position.

Fig. 4.



The paths being now approximately equal, the two images of the source of light or of some well-defined object placed in front of the condensing lens, were made to coincide, the telescope was now adjusted for distinct vision of the expected interference-bands, and sodium light was substituted for white light, when the interference-bands appeared. These were now made as clear as possible by adjusting the mirror $e_{,}$; then white light was restored, the screw altering the length of path was very slowly moved (one turn of a screw of one hundred threads to the inch altering the path nearly 1000 wave-lengths) till the coloured interference-fringes reappeared in white light. These were now given a convenient width and position, and the apparatus was ready for observation.

Fig. 5.



The observations were conducted as follows :—Around the cast-iron trough were sixteen equidistant marks. The apparatus was revolved very slowly (one turn in six minutes) and after a few minutes the cross wire of the micrometer was set on the clearest of the interference-fringes at the instant of passing one of the marks. The motion was so slow that this could be done readily and accurately. The reading of the screw-head on the micrometer was noted, and a very slight and gradual impulse was given to keep up the motion of the stone ; on passing the second mark, the same process was repeated, and this was continued till the apparatus had completed six revolutions. It was found that by keeping the apparatus in slow uniform motion, the results were much more uniform and consistent than when the stone was brought to rest for every observation ; for the effects of strains could be noted for at least half a minute after the stone came to rest, and during this time effects of change of temperature came into action.

The following tables give the means of the six readings ; the first, for observations made near noon, the second, those near six o'clock in the evening. The readings are divisions of the screw-heads. The width of the fringes varied from 40 to 60 divisions, the mean value being near 50, so that one division means 0·02 wave-length. The rotation in the observations at noon was contrary to, and in the evening observations, in the same direction as, that of the hands of a watch.

NOON OBSERVATIONS.

	16.	1.	2.	3.	4.	5.	6.	7.	8.
July 8	44·7	44·0	43·5	39·7	35·2	34·7	34·3	32·5	28·2
July 9	57·4	57·3	58·2	59·2	58·7	60·2	60·8	62·0	61·5
July 11	27·3	23·5	22·0	19·3	19·2	19·3	18·7	18·8	16·2
Mean	43·1	41·6	41·2	39·4	37·7	38·1	37·9	37·8	35·3
Mean in w.l. ...	·862	·832	·824	·788	·754	·762	·758	·756	·706
	·706	·692	·686	·688	·688	·678	·672	·628	·616
Final mean	·784	·762	·755	·738	·721	·720	·715	·692	·661

	9.	10.	11.	12.	13.	14.	15.	16.
July 8	26·2	23·8	23·2	20·3	18·7	17·5	16·8	13·7
July 9	63·3	65·8	67·3	69·7	70·7	73·0	70·2	72·2
July 11	14·3	13·3	12·8	13·3	12·3	10·2	7·3	6·5
Mean	34·6	34·3	34·4	34·4	33·9	33·6	31·4	30·8
Mean in w.l.	·692	·686	·688	·688	·678	·672	·628	·616
Final mean	...	...	...	...	...	...	...	...

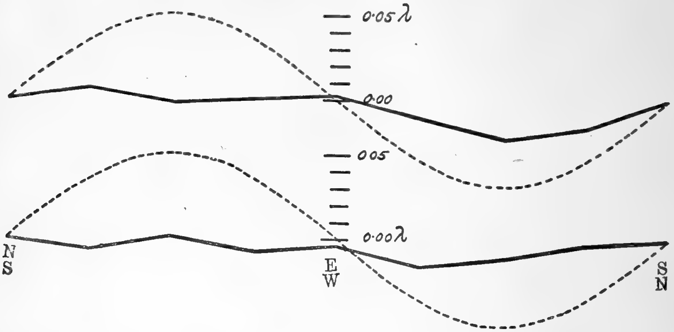
P.M. OBSERVATIONS.

	16.	1.	2.	3.	4.	5.	6.	7.	8.
July 8	61·2	63·3	63·3	68·2	67·7	69·3	70·3	69·8	69·0
July 9	26·0	26·0	28·2	29·2	31·5	32·0	31·3	31·7	33·0
July 12	66·8	66·5	66·0	64·3	62·2	61·0	61·3	59·7	58·2
Mean.........	51·3	51·9	52·5	53·9	53·8	54·1	54·3	53·7	53·4
Mean in w.l.	1·026	1·038	1·050	1·078	1·076	1·082	1·086	1·074	1·068
	1·068	1·086	1·076	1·084	1·100	1·136	1·144	1·154	1·172
Final mean .	1·047	1·062	1·063	1·081	1·088	1·109	1·115	1·114	1·120

	9.	10.	11.	12.	13.	14.	15.	16.
July 8	71·3	71·3	70·5	71·2	71·2	70·5	72·5	75·7
July 9	35·8	36·5	37·3	38·8	41·0	42·7	43·7	44·0
July 12	55·7	53·7	54·7	55·0	58·2	58·5	57·0	56·0
Mean	54·3	53·8	54·2	55·0	56·8	57·2	57·7	58·6
Mean in w.l.......	1·086	1·076	1·084	1·100	1·136	1·144	1·154	1·172
Final mean	...	...	...	...	...	...	...	...

The results of the observations are expressed graphically in fig. 6. The upper is the curve for the observations at noon, and the lower that for the evening observations. The dotted curves represent *one eighth* of the theoretical displacements. It seems fair to conclude from the figure that if there is any displacement due to the relative motion of the earth and the luminiferous æther, this cannot be much greater than 0·01 of the distance between the fringes.

Fig. 6.



Considering the motion of the earth in its orbit only, this displacement should be

$$2D\frac{v^2}{V^2}=2D\times 10^{-8}.$$

The distance D was about eleven metres, or 2×10^7 wave-lengths of yellow light; hence the displacement to be expected was 0·4 fringe. The actual displacement was certainly less than the twentieth part of this, and probably less than the fortieth part. But since the displacement is proportional to the square of the velocity, the relative velocity of the earth and the æther is probably less than one sixth the earth's orbital velocity, and certainly less than one fourth.

In what precedes, only the orbital motion of the earth is considered. If this is combined with the motion of the solar system, concerning which but little is known with certainty, the result would have to be modified; and it is just possible that the resultant velocity at the time of the observations was small, though the chances are much against it. The experiment will therefore be repeated at intervals of three months, and thus all uncertainty will be avoided.

It appears from all that precedes reasonably certain that if there be any relative motion between the earth and the luminiferous æther, it must be small; quite small enough entirely to refute Fresnel's explanation of aberration. Stokes has given a theory of aberration which assumes the æther at the earth's surface to be at rest with regard to the latter, and only requires in addition that the relative velocity have a potential; but Lorentz shows that these conditions are incompatible. Lorentz then proposes a modification which combines some ideas of Stokes and Fresnel, and assumes the existence of a potential, together with Fresnel's coefficient. If now it were legitimate to conclude from the present work that the æther is at rest with regard to the earth's surface, according to Lorentz there could not be a velocity potential, and his own theory also fails.

Supplement.

It is obvious from what has gone before that it would be hopeless to attempt to solve the question of the motion of the solar system by observations of optical phenomena *at the surface of the earth.* But it is not impossible that at even moderate distances above the level of the sea, at the top of an isolated mountain-peak, for instance, the relative motion might be perceptible in an apparatus like that used in these experiments. Perhaps if the experiment should ever be tried under these circumstances, the cover should be of glass, or should be removed.

It may be worth while to notice another method for multiplying the square of the aberration sufficiently to bring it within the range of observation which has presented itself during the preparation of this paper. This is founded on the fact that reflexion from surfaces in motion varies from the ordinary laws of reflexion.

Let ab (fig. 1, p. 461) be a plane wave falling on the mirror mn at an incidence of 45°. If the mirror is at rest, the wave-front after reflexion will be ac.

Now suppose the mirror to move in a direction which makes an angle α with its normal, with a velocity ω. Let V be the velocity of light in the æther, supposed stationary, and let cd be the increase in the distance the light has to travel to reach d. In this time the mirror will have moved a distance $\dfrac{cd}{\sqrt{2}\cos\alpha}$.

We have

$$\frac{cd}{ad}=\frac{\omega\sqrt{2}\cos\alpha}{V},$$

which put $= r$, and

$$\frac{ac}{ad}=1-r.$$

In order to find the new wave-front, draw the arc *fg* with *b* as a centre and *ad* as radius; the tangent to this arc from *d* will be the new wave-front, and the normal to the tangent from *b* will be the new direction. This will differ from the direction *ba* by θ, which it is required to find. From the equality of the triangles *adb* and *edb* it follows that $\theta = 2\phi$, $ab = ac$,

$$\tan adb = \tan\left(45^\circ - \frac{\theta}{2}\right) = \frac{1 - \tan\frac{\theta}{2}}{1 + \tan\frac{\theta}{2}} = \frac{ac}{ad} = 1 - r,$$

or, neglecting terms of the order r^3,

$$\theta = r + \frac{r^2}{2} = \frac{\sqrt{2\omega}\cos\alpha}{V} + \frac{\omega^2}{V^2}\cos^2\alpha.$$

Now let the light fall on a parallel mirror facing the first, we should then have

$$\theta_{,} = \frac{-\sqrt{2\omega}\cos\alpha}{V} + \frac{\omega^2}{V^2}\cos^2\alpha,$$

and the total deviation would be

$$\theta + \theta_{,} = 2\rho^2\cos^2\alpha,$$

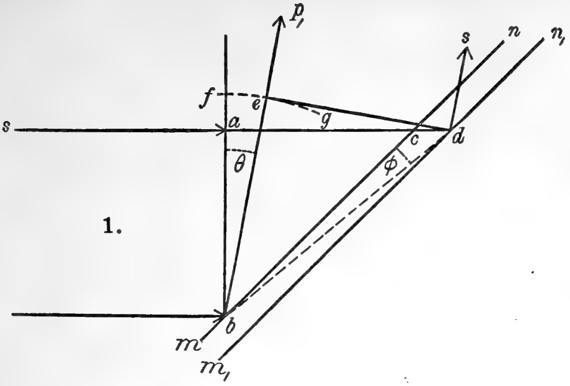
where ρ is the angle of aberration, if only the orbital motion of the earth is considered. The maximum displacement obtained by revolving the whole apparatus through 90° would be

$$\Delta = 2\rho^2 = 0{\cdot}004''.$$

With fifty such couples the displacement would be 0·2″. But astronomical observations in circumstances far less favourable than those in which these may be taken have been made to hundredths of a second; so that this new method bids fair to be at least as sensitive as the former.

The arrangement of apparatus might be as in fig. 2; *s*, in the focus of the lens *a*, is a slit. *bb*, *cc*, are two glass mirrors optically plane, and so silvered as to allow say one twentieth of the light to pass through, and reflecting say ninety per cent. The intensity of the light falling on the observing telescope *df* would be about one millionth of the original intensity, so that if sunlight or the electric arc were used it could still be readily seen. The mirrors *bb*, and *cc*, would differ from parallelism sufficiently to separate the successive images. Finally, the apparatus need not be mounted so as to revolve, as the earth's rotation would be sufficient.

If it were possible to measure with sufficient accuracy the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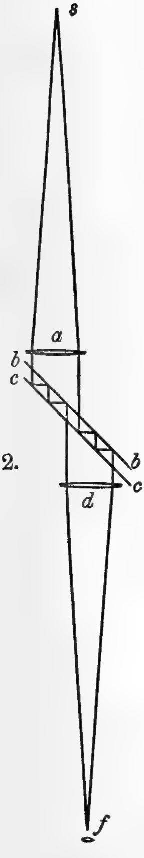
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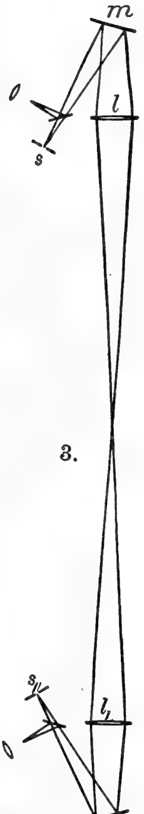
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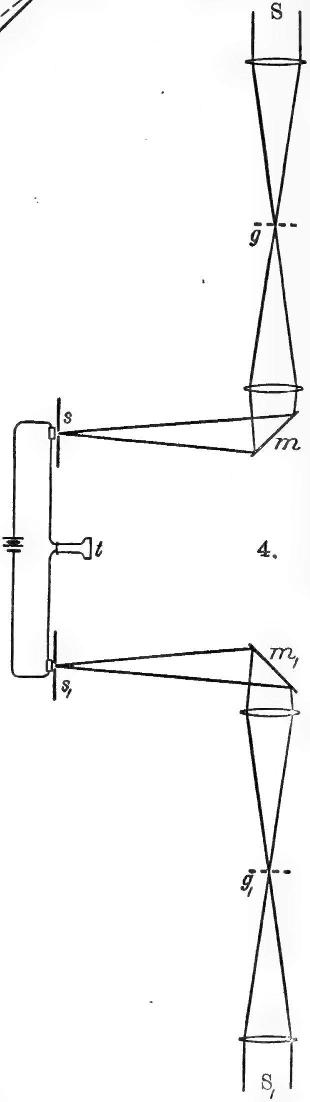
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velocity of light without returning the ray to its starting point, the problem of measuring the first power of the relative velocity of the earth with respect to the æther would be solved. This may not be as hopeless as might appear at first sight, since the difficulties are entirely mechanical and may possibly be surmounted in the course of time.

For example, suppose m and $m_{\prime}$ (fig. 3) two mirrors revolving with equal velocity in opposite directions. It is evident that light from s will form a stationary image at $s_{\prime}$ and similarly light from $s_{\prime}$ will form a stationary image at s. If now the velocity of the mirrors be increased sufficiently, their phases still being exactly the same, both images will be deflected from s and $s_{\prime}$ in inverse proportion to the velocities of light in the two directions; or, if the two deflections are made equal, and the difference of phase of the mirrors be simultaneously measured, this will evidently be proportional to the difference of velocity in the two directions. The only real difficulty lies in this measurement. The following is perhaps a possible solution.

$gg_{\prime}$ (fig. 4) are two gratings on which sunlight is concentrated. These are placed so that after falling on the revolving mirrors m and $m_{\prime}$, the light forms images of the gratings at s and $s_{\prime}$, two very sensitive selenium cells in circuit with a battery and telephone. If everything be symmetrical, the sound in the telephone will be a maximum. If now one of the slits s be displaced through half the distance between the image of the grating bars, there will be silence. Suppose now that the two deflections having been made exactly equal, the slit is adjusted for silence. Then if the experiment be repeated when the earth's rotation has turned the whole apparatus through 180°, and the deflections are again made equal, there will no longer be silence, and the angular distance through which s must be moved to restore silence will measure the required difference in phase.

There remain three other methods, all astronomical, for attacking the problem of the motion of the solar system through space.

1. The telescopic observation of the proper motions of the stars. This has given us a highly probably determination of the direction of this motion, but only a guess as to its amount.

2. The spectroscopic observation of the motion of stars in the line of sight. This could furnish data for the relative motions only, though it seems likely that by the immense improvements in the photography of stellar spectra, the information thus obtained will be far more accurate than any other.

3. Finally there remains the determination of the velocity of light by observations of the eclipses of Jupiter's satellites. If the improved photometric methods practised at the Harvard observatory make it possible to observe these with sufficient accuracy, the difference in the results found for the velocity of light when Jupiter is nearest to and farthest from the line of motion will give, not merely the motion of the solar system with reference to the stars, but with reference to the luminiferous æther itself.

LIX. *On a Method of making the Wave-length of Sodium Light the actual and practical Standard of Length.* By ALBERT A. MICHELSON *and* EDWARD W. MORLEY[*].

THE first actual attempt to make the wave-length of sodium light a standard of length was made by Peirce[†]. This method involves two distinct measurements: first, that of the angular displacement of the image of a slit by a diffraction-grating, and second, that of the distance between the lines of the grating. Both of these are subject to errors due to changes of temperature and to instrumental errors. The results of this work have not as yet been published; but it is not probable that the degree of accuracy attained is much greater than one part in fifty or a hundred thousand. More recently, Mr. Bell, of the Johns Hopkins University, using Rowland's gratings, has made a determination of the length of the wave of sodium light which is claimed to be accurate to one two hundred thousandth part[‡]. If this claim is justified, it is probably very near the limit of accuracy of which the method admits. A short time before this, another method was proposed by Macé de Lepinay[§]. This consists in the calculation of the number of wave-lengths between two surfaces of a cube of quartz. Besides the spectroscopic observations of Talbot's fringes, the method involves the measurement of the index of refraction and of the density of quartz, and it is not surprising that the degree of accuracy attained was only one in fifty thousand.

Several years ago, a method suggested itself which seemed likely to furnish results much more accurate than either of the foregoing, and some preliminary experiments made in

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[*] Communicated by the Authors.

[†] 'Nature,' xx. p. 99 (1879); Amer. Journ. Sci. [3], p. 51 (1879).

[‡] "On the Absolute Wave-lengths of Light," Amer. Journ. Sci. [3], xxxiii. p. 167 (1887); Phil. Mag. [5], xxiii. p. 365.

[§] *Comptes Rendus*, cii. p. 1153 (1886); *Journ. de Phys.* [2], v. p. 411 (1886).

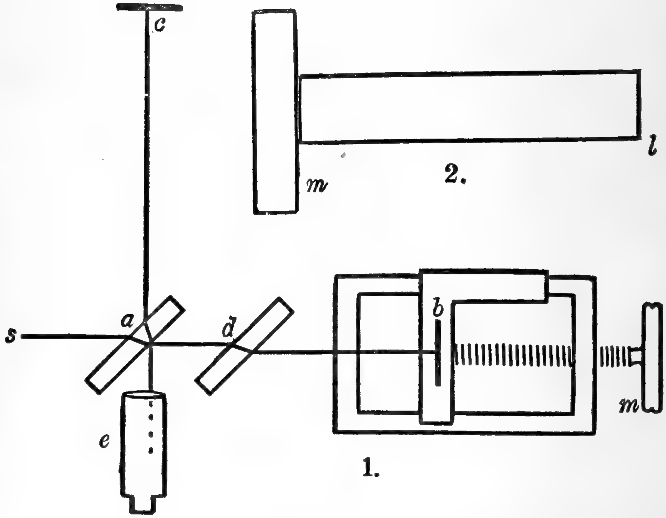
June have confirmed the anticipation. The apparatus for observing the interference phenomena is the same as that used in the experiments on the relative motion of the earth and the luminiferous æther.

Light from the source at *s* (fig. 1), a sodium-flame, falls on the plane parallel glass *a*, and is divided, part going to the plane mirror *c*, and part to the plane mirror *b*. These two pencils are returned along *cae* and *bae*, and the interference of the two is observed in the telescope at *e*. If the distances *ac* and *ab* are made equal, the plane *c* made parallel with that of the image of *b*, and the compensating glass *d* interposed, the interference is at once seen. If the adjustment be exact, the whole field will be dark, since one pencil experiences external reflexion, and the other internal.

If now *b* be moved parallel with itself a measured distance by means of the micrometer-screw, the number of alternations of light and darkness is exactly twice the number of wave-lengths in the measured distance; thus the determination consists absolutely of a measurement of a length and the counting of a number.

The degree of accuracy depends on the number of wave-lengths which it is possible to count. Fizeau was unable to observe interference when the difference of path amounted to 50,000 wave-lengths. It seemed probable that with a smaller density of sodium vapour this number might be increased, and

Figs. 1 and 2.



the experiment was tried with metallic sodium in an exhausted tube provided with aluminium electrodes. It was found possible to increase this number to more than 200,000. Now it is very easy to estimate tenths or even twentieths of a wave-length, which implies that it is possible to find the number of wave-lengths in a given fixed distance between two planes with an error less than one part in two millions and probably one in ten millions. But the distance corresponding to 400,000 wave-lengths is roughly a decimetre, and this cannot be determined or reproduced more accurately than, say, to one part in 500,000. So it would be necessary to increase this distance. This can be done by using the same instrument together with a comparer.

The intermediate standard decimetre *lm* (fig. 2) is put in place of the mirror *b*. It consists of a prism of glass one decimetre long with one end *l* plane, and the other slightly convex, so that when it touches the plane *m*, Newton's rings appear, and these serve to control any change in the distance *lm*, which has been previously determined in wave-lengths.

The end *l* is now adjusted so that coloured fringes appear in white light. These can be measured to within one twentieth of a wave-length, and probably to within one fiftieth. The piece *lm* is then moved forward till the fringes again appear at *m*; then the refractometer is moved in the same direction till the fringes appear again at *l*, and so on till the whole metre has been stepped off. Supposing that in this operation the error in the setting of the fringes is always in the same direction, the whole error in stepping off the metre would be one part in two millions. By repetition this could of course be reduced. A microscope rigidly attached to the carriage holding the piece *lm* would serve to compare, and a diamond attached to the same piece would be used to produce copies. All measurements would be made with the apparatus surrounded by melting ice, so that no temperature corrections would be required.

Probably there would be considerable difficulty in actually counting 400,000 wave-lengths, but this can be avoided by first counting the wave-lengths and fractions in a length of one millimetre, and using this to step off a centimetre. This will give the nearest whole number of wave-lengths, and the fractions may be observed directly. The centimetre is then used in the same way to step off a decimetre, which again determines the nearest whole number, the fraction being observed directly as before.

The fractions are determined as follows: the fringes observed in the refractometer under the conditions above mentioned can readily be shown to be concentric circles. The centre has the minimum intensity when the difference in the distances *ab ac* is an exact number of wave-lengths. The diameters of the consecutive circles vary as the square roots of the corresponding number of waves. Therefore, if x is the fraction of a wave-length to be determined, and y the diameter of the first dark ring, d being the diameter of the ring corresponding to one wave-length, then $x = \frac{y^2}{d^2}$.

There is a slight difficulty to be noted in consequence of the fact that there are two series of waves in sodium-light. The result of the superposition of these is that, as the difference of path increases, the interference becomes less distinct and finally disappears, reappears, and has a maximum of distinctness again, when the difference of path is an exact multiple of both wave-lengths. Thus there is an alternation of distinct interference-fringes with uniform illumination. If the length to be measured, the centimetre for instance, is such that the interference does not fall exactly at the maximum—to one side by, say, one tenth the distance between two maxima, there would be an error of one twentieth of a wave-length requiring an arithmetical correction.

Among other substances tried in the preliminary experiments were thallium, lithium, and hydrogen. All of these gave interference up to fifty to one hundred thousand wave-lengths, and could therefore all be used as checks on the determination with sodium. It may be noted, that in case of the red hydrogen-line, the interference phenomena disappeared at about 15,000 wave-lengths, and again at about 45,000 wave-lengths; so that the red hydrogen-line must be a double line with the components about one sixtieth as distant as the sodium-lines.

LX. *On* Mr. Edgeworth's *Method of Reducing Observations relating to several Quantities. By* H. H. Turner, *M.A., B.Sc., Fellow of Trinity College, Cambridge*[*].

In the Philosophical Magazine for August 1887, Mr. F. Y. Edgeworth invites attention to a method of reducing observations relating to several quantities, which he has suggested as a substitute for the ordinary process of the "Method of Least Squares." I have applied this method to an example for a particular case of two variables, and venture to offer the following remarks and suggestions for consideration.

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[*] Communicated by the Author.

Mr. Edgeworth thus describes his method in the case of two variables x and y :—" Find an approximate solution by some rough process (such as simply adding together several of the equations so as to form two independent simultaneous equations). Take the point thus determined as a new origin, and substitute in the n (transformed) equations for one of the variables x a series of values $\pm\delta$, $\pm 2\delta$, &c. Corresponding to each of these substitutions we have n equations for y. For each of these systems determine the *Median* according to Laplace's *Method of Situation.* This series of Medians forms one locus for the sought point. A second locus is found by transposing x and y in the directions just given. The intersection of these loci is the required point."

Some of the labour of this process, and sometimes the preliminary search for an approximate solution, may be avoided by the use of a graphical method, which will be best described by considering first a simple case. Suppose we have five equations,

$$\begin{aligned} x + \cdot 01\, y &= a_1, \\ x + \cdot 01\, y &= a_2, \\ x + y &= b_1, \\ x + y &= b_2, \\ x + y &= b_3. \end{aligned}$$

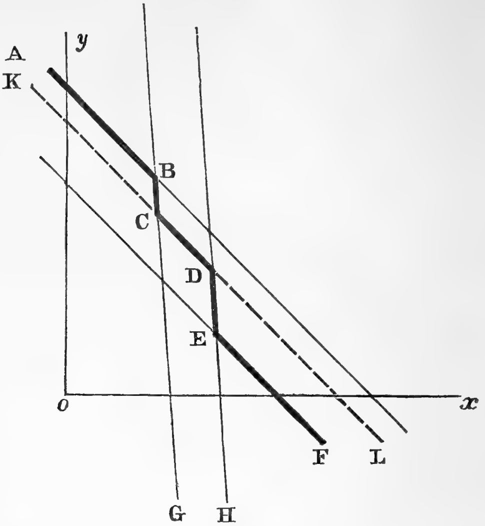
Geometrically these represent two lines nearly parallel to the axis of y, and three inclined at 45° to it.

Now in forming the first normal equation according to the Method of Least Squares, we should multiply each of the equations by the coefficient of x, which is unity in each case. In Mr. Edgeworth's method we are to find the median line, *weighting* all the equations according to the coefficient of x, *i. e.* equally. This is evidently the broken line A B C D E F; for an ordinate drawn through any point of it cuts the system of five lines in five points, one of which is on the locus A B C D E F and two others are on either side.

It is obviously very easy to draw this locus when once we have any portion of it; for we simply traverse the network, changing our line at every corner.

The second median locus is obtained by weighting the equations, or lines according to the coefficients of y. The first two count for very little, and the last three again count equally. The median is thus the line K L throughout, for the crossing of the slightly weighted lines does not disturb the balance of weights.

We are now to take the point of intersection of these loci as the final solution of the equations. It will be noticed that this leaves the solution somewhat indeterminate; for any point of the portion C D satisfies the required condition.



This special case is in many respects an unfavourable example of the method under consideration; but it illustrates sufficiently well the following points :—

(1) The two median loci are broken lines which follow the lines of the network formed by the separate observation-lines (except in one very special instance mentioned below), and formed according to the following rule. Suppose the lines all labelled with the coefficients of x in the equations representing them. At any point of the locus let A be the sum of all the labels to the left (looking along the locus), and B the sum of all those to the right; l the label of the line with which the locus coincides. Then $A+l>B$ and $B+l>A$.

The locus continues to coincide with the line l until it is crossed by another, say from the right, weight m. Then if the locus is to change to this new line, the sum of labels on the left is still A, but on the right is $B-m+l$: thus we must have

$$A+m>(B-m+l),$$

and

$$(B-m+l)+m>A.$$

The second condition is the same as one of the former; but if the first is not fulfilled, the locus continues to travel along the line l. By this rule the two loci can be traced with great ease. It is necessary, however, to select the scale of the diagram with some care, so as to prevent confusion when there are many equations.

If the final solution is roughly known, the origin should be taken in the neighbourhood; and substitution of this rough solution in the equations will enable us to leave out of consideration those with large residuals, the lines representing which will often not fall within the limited area of paper at command. [If it should happen, when the line m crosses l, that

$$\mathrm{A}+m=\mathrm{B}-m+l,$$

the locus does not follow either line, but consists of the whole space between them up to the next crossing. This is the special case referred to above.]

(2) In the method of least squares the normal equations give a unique solution; but the intersection of two broken lines may be a series of points, and the two median loci may also have a common portion. The solution then becomes to some extent indeterminate. It is difficult to decide whether this is generally the case; but in all the simple examples which I have tried, and in two real examples of 93 and 67 equations respectively, the intersection of the loci consisted of a finite line and one or more points: and there are also cases where the loci approach very closely, and where they would have again met had one only of the observations been almost infinitesimally different. For instance, in the longest example tried, I took ninety-three equations of the form $x+\mathrm{A}y=\mathrm{B}$. B is the tabular error in the semidiameter of Venus, as obtained by a certain observer at Greenwich. This is supposed divisible into two parts; one constant, x, and the other, y, varying as the diameter A. The values of x and y obtained by the method of least squares were

$$x=1''{\cdot}22 \ \pm 0''{\cdot}222,$$

$$y=0''{\cdot}024\pm 0''{\cdot}011.$$

By the method of median loci, I obtain

$$x=0''{\cdot}93, \quad y=0''{\cdot}037,$$

or $$x=1''{\cdot}13, \quad y=0''{\cdot}032;$$

or any value on the line joining these two points. There is also a point of meeting of the loci at

$$x=1''{\cdot}15, \quad y=0''{\cdot}029;$$

and a point of near approach at

$$x=1''{\cdot}37, \quad y=0''{\cdot}017.$$

These equations were discussed by Mr. Thackeray in the Monthly Notices of the Royal Astronomical Society; and his solution, obtained by simple addition of the equations with large and those with small coefficients of y, was

$$x = 1''{\cdot}432,$$

$$y = 0''{\cdot}014.$$

It is possible that the number and distribution of these points of intersection afford real information as to the value and accordance of the observations. But, in practice, a single solution, although its singularity may be somewhat fictitious, is preferable to a variety; and unless some additional criterion for extracting a single solution from the median loci can be obtained, it is to be feared that we have here a somewhat serious objection to this method on the score of convenience.

The extension to the case of three variables is obvious: the three normal equations, or rather the planes which they represent, are replaced by three broken-plane loci, made up of individual planes corresponding to the separate observations. These loci may intersect in a finite portion of a plane, one or more finite lines, and one or more points: and the multiplicity of solutions is obviously liable to increase largely with the number of variables.

Mr. Edgeworth claims as advantages for the new method that

(1) It is considerably less laborious than the Method of Least Squares.

(2) In the case of Discordant Observations it is theoretically better.

So far as my slight experience entitles me to express an opinion on these points, I should say that

(1) is very doubtful. In trying a new method much time is liable to be wasted; but there would, I imagine, never be quite the same straightforwardness about the new method which makes the method of least squares so easy, although somewhat long.

(2) is somewhat counterbalanced by the failure to give a *unique* solution.

LXI. *On the Diffusion of Gases.—A simple case of Diffusion.* *By* S. H. Burbury[*].

1. LET two reservoirs be connected by a uniform horizontal tube. In the left-hand reservoir shall be a mixture of two gases, gas I. and gas II., in certain proportions ; and in the right-hand reservoir a mixture of the same two gases in different proportions. The temperature and also the pressure of the mixture shall be the same in either reservoir. Then a stream of gas I. will flow through the tube, say from left to right, and a stream of gas II. from right to left.

We will suppose the proportions in which the gases are mixed, as well as the total pressure and temperature, to be maintained constant in each reservoir, as if, for instance, the reservoirs be of infinite extent. Under these circumstances the motion through the tube will become steady, and the temperature and pressure of the combined gases will be the same throughout. The problem of diffusion is to find what, under these circumstances, the stream-motion of either gas will be for given proportions of the mixture at the extremities of the tube.

It is assumed that the stream-velocity is very small compared with the mean square velocity of either gas required by the kinetic theory. This must be the case if the tube be long enough. It is assumed, further, that in calculating the result of encounters between the molecules they are to be regarded as elastic spheres.

If n_1 denote the number of molecules of gas I. in unit of volume at any point, n_2 the same for gas II., then by Avogadro's law n_1+n_2 is constant throughout the system. We will take the axis of the tube for axis of x. Then at any point,

$$\frac{dn_1}{dx}+\frac{dn_2}{dx}=0.$$

2. Professor Tait has recently ("On the Foundations of the Kinetic Theory of Gases," Transactions of the Royal Society of Edinburgh, 1887) given what he considers to be the solution of the problem in a slightly more general form, assuming the tube vertical. He assumes that the molecules of the diffusing gas have, in addition to their ordinary velocities required by the kinetic theory, a common velocity α of translation along the tube, very small compared with the velocity of mean square. In his view this common translation-velocity

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[*] Communicated by the Author.

is the same for all classes of molecules of the same gas whatever their absolute speeds in space, no distinction being drawn between those classes which have great and those which have small absolute speeds.

I propose to show (as has been already proved by Boltzmann) that, in order to represent the true motion of the diffusing gas, it is necessary to make α, the small additional translation-velocity, a function of the absolute velocity of the molecules, those classes which have greater absolute velocity having greater translation-velocity; and that Professor Tait's hypothesis, making α constant, is inconsistent with steady motion. It is sufficient to prove this for the imaginary case of two gases, of which the molecules of one have the same mass and the same diameter as those of the other.

3. At any point in the tube let us take two parallel sections, A and A′, each of unit area, distant δx from each other. The volume of the cylinder whose bases are A and A′ is δx. We will call it our element of volume. At A let n_1 be the number of molecules of gas I., n_2 the number of molecules of gas II., per unit of volume. Then at A′ the respective numbers will be $n_1+\frac{dn_1}{dx}\delta x$, and $n_2+\frac{dn_2}{dx}\delta x$; that is, since $\frac{dn_1}{dx}=-\frac{dn_2}{dx}$,

$$n_1-\frac{dn_2}{dx}\delta x \text{ for gas I.,}$$

and

$$n_2+\frac{dn_2}{dx}\delta x \text{ for gas II.}$$

At A the number of molecules of gas I. in unit volume whose velocities, irrespective of direction, lie between v and $v+dv$, shall be denoted by $n_1 f(v)dv$. We will call them the class v. Those members of the class v whose directions of motion make with the axis of the tube angles between ψ and $\psi+d\psi$ shall be called the class (v, ψ). Their number in the gas at rest would be

$$n_1 f(v)dv\,\tfrac{1}{2}\sin\psi\,d\psi.$$

In the moving gas it will be

$$n_1 f(v)\,dv\,\tfrac{1}{2}\sin\psi\,d\psi$$

$$+\tfrac{3}{2}n_1 f(v)\,dv\,\frac{\alpha}{v}\cos\psi\sin\psi\,d\psi\,;$$

α being the mean translation-velocity for the class v, whether that be a function of v or not.

4. Let us now find the number of molecules of the class

(v, ψ) that cross the section A per unit of time. It is

$$n_1 f(v)\, dv\, \tfrac{1}{2} v \cos\psi \sin\psi\, d\psi$$
$$+ n_1 f(v)\, dv\, \tfrac{3}{2} \alpha \cos^2\psi \sin\psi\, d\psi.$$

Of course if $\cos\psi$ is positive they enter the element of volume; if $\cos\psi$ is negative they leave it.

5. We next consider the *momentum* in direction x which is carried across the section A by the class (v, ψ). If m be the mass of a molecule, it is

$$mn_1 f(v)\, dv \{\tfrac{1}{2} v^2 \cos^2\psi + \tfrac{3}{2} v \alpha \cos^3\psi\} \sin\psi\, d\psi.$$

If we integrate this expression according to ψ from π to 0, we obtain the momentum in direction x which the whole class v brings into our element of volume through the section A in unit time, negative momentum carried out of the element by molecules having $\cos\psi$ negative being treated as positive momentum carried into the element. The result is

$$mn_1 f(v)\, dv\, \frac{v^2}{2} \int_0^\pi \cos^2\psi \sin\psi\, d\psi, \quad . \quad . \quad . \quad . \qquad \text{(A)}$$

the term in $\cos^3\psi$ disappearing.

6. We will now consider what happens at the other section A′.

The momentum in direction x of the class v which is carried out of our element of volume at A′ per unit of time is found from the expression A by writing $n_1 + \frac{dn_1}{dx}\delta x$, that is, $n_1 - \frac{dn_2}{dx} dx$ for n_1. It is therefore

$$m\left(n_1 - \frac{dn_2}{dx}\delta x\right) f(v)\, dv\, \frac{v^2}{2} \int_0^\pi \cos^2\psi \sin\psi\, d\psi. \quad . \quad . \quad . \qquad \text{(A}'\text{)}$$

7. Comparing the expressions A and A′, we see that the momentum of the class v in the direction of positive x which enters the element of volume in unit time exceeds the momentum which leaves the element of volume by the quantity

$$m \frac{dn_2}{dx} \delta x\, \frac{v^2}{2} f(v)\, dv \int_0^\pi \cos^2\psi \sin\psi\, d\psi;$$

that is,

$$m \frac{dn_2}{dx} \delta x\, f(v)\, dv\, \frac{v^2}{3}.$$

If therefore there were no encounters, the class v within our element of volume would gain x momentum per unit time by the amount

$$m\frac{dn_2}{dx}\delta x f(v)\,dv\frac{v^2}{3};$$

or would gain x velocity $\frac{dn_2}{dx}\delta x f(v)\,dv\frac{v^2}{3}$ per unit time. This is the effect of the variation of density of gas I. from point to point along the tube.

8. But the motion is in fact steady. Therefore this gain in translation-velocity, which would accrue to the class v in the absence of encounters, is exactly compensated by the loss which is due to encounters. A certain number of the class undergo encounter, and are knocked out of the class in a unit of time. An equal number of molecules come out of encounters with velocity between v and dv, that is enter the class v, in a unit of time. And these, as they enter the class, have less average translation-velocity than the continuing members of the class have.

Let B be the average number of encounters with molecules of either gas which a molecule of class v undergoes in unit time. In the special case we are now treating, B is the same for all directions of motion of the molecule; because in whatever direction there are more encounters with one gas, there are by the same number fewer with the other. It follows that those members of the class v which undergo encounter have before such encounter the same mean translation-velocity which the other members of the class have. Now $n_1 f(v)\,dv\,\mathrm{B}$ molecules per unit volume and unit time undergo encounters, and cease to be members of the class. Were they not replaced, the aggregate translation-velocity of the class would be diminished from this cause by $n_1\mathrm{B}f(v)\,dv\,.\,\alpha$ per unit of volume and time. But an equal number of molecules come out of encounters with velocity between v and $v+dv$; and these have some mean translation-velocity in x, which we will call α'. The effect of encounters on the translation-velocity of the class v is to substitute per unit volume and unit time $n_1\mathrm{B}f(v)\,dv$ molecules with mean translation-velocity α' for the same number with mean translation-velocity α.

This diminishes the translation-velocity of the class v within our element of volume δx by

$$n_1 f(v)\,dv\,\mathrm{B}(\alpha-\alpha')\,\delta x$$

in unit time.

9. For steady motion we equate this expression to the gain of translation-velocity which, as we have seen, arises from the variation of density. We thus obtain the equation

$$\frac{v^2}{3}\frac{dn_2}{dx}\delta x f(v)\,dv = n_1 B(\alpha-\alpha')\,\delta x f(v)\,dv,$$

or

$$\frac{v^2}{3}\frac{dn_2}{dx} = n_1 B(\alpha-\alpha'). \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

This equation must in steady motion be satisfied for every class of molecules. And it cannot be satisfied without making α a function of v. Were α made constant, molecules with high values of v would begin to gain more translation-velocity than they lose. The arrangement could not be permanent.

10. It is of course true that the effect of encounters between the different classes of molecules of the same gas is to equalize the translation-velocity. A levelling-process is always going on. It is also true, as Professor Tait shows (page 83), that this process goes on very fast; because B, the number of encounters per second, is a very large quantity. The larger you make B, the smaller you make α and α'. But the equation (1) remains none the less true, and none the less must α be a function of v.

Nor is it a matter of indifference whether we treat α as constant or not. The greater the absolute speed the greater the number of encounters per unit time. Therefore, for given stream-velocity of the whole gas, the resistance will be greater if that stream-velocity be all attributed to the higher classes than if it be the same for all classes. Therefore, in order to get the same resistance with α constant, we must increase the stream. Any result obtained with α constant can be no more than a superior limit of the stream-velocity; and the value of such a solution depends on the limits of error being ascertained.

I think the same error vitiates Professor Tait's treatment of Viscosity and Thermal Conductivity.

11. As a step towards the true solution, I will endeavour to obtain inferior and superior limits for the stream-velocity in this imaginary case, the molecules of one gas having equal mass and diameter with those of the other. I will further suppose that there exists a point C in the tube at which $n_1=n_2$. This must be the case if $n_1 > n_2$ in one reservoir, and $n_1 < n_2$ in the other. If we can find the stream-velocity at C, we know it at every point in the tube.

12. The number of encounters per unit of time for each molecule of the class v is B. I shall assume that at the point C, where $n_1=n_2$, there are for each molecule of the class $\frac{1}{2}$B encounters with molecules of gas II., and $\frac{1}{2}$B with other molecules of gas I.

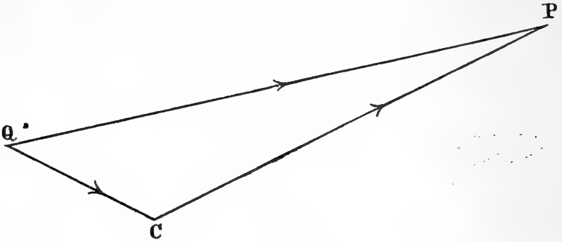
13. A molecule of gas I. coming out of encounter with

velocity between v and $v+dv$ has on an average, if we include encounters of both kinds, some translation-velocity in direction x, which we have called α'. This will have altogether different values according as the encounter is with another molecule of gas I. or with a molecule of gas II. Let it be α_1' in the first case, and α_2' in the second. Then, at the point C, our equation (1) becomes

$$\frac{v^2}{3}\frac{dn_2}{dx}=\tfrac{1}{2}\mathrm{B}(\alpha-\alpha_1')+\tfrac{1}{2}\mathrm{B}(\alpha-\alpha_2'). \quad . \quad . \quad . \quad (2)$$

14. We can now prove that at C, where $n_1=n_2$, $\alpha_2'=0$. This can be done as follows:—Let p, q be two velocities such that $p^2+q^2>v^2$. Then, for an encounter from which a molecule of gas I. issues with velocity v, the velocities before encounter may be either p for the molecule of gas I., and q for that of gas II., or *vice versâ*. There are just as many encounters one way as the other; and the chance of the given event happening, namely that the molecule of gas I. comes out with velocity v, is the same in either case.

Let V′ be the velocity of the centre of gravity of the two molecules engaged. Then, taking the two groups $p\,q$ and $q\,p$ together, V′ can have no average component in x. This follows from the symmetry of the circumstances, n_1 being equal to n_2. If ϕ be the angle made by V′ with x, $\overline{\cos\phi}=0$.



In the figure let C P denote V′, and Q C=V denote the half relative velocity, and Q P denote v. Then Q P must be some radius vector of the cone described about C P as axis with semivertical angle $\mathrm{QPC}=\cos^{-1}\dfrac{v^2+\mathrm{V}'^2-\mathrm{V}^2}{2v\mathrm{V}'}$. And since all directions of V are equally probable after encounter, Q P may with equal probability be any radius vector of that cone. Therefore if θ be the angle made by Q P with x, and ϕ that made by C P with x, the mean value of $\cos\theta$, given ϕ, is $\cos\mathrm{QPC}\cos\phi$. And the mean value of $\cos\theta$ for all directions of CP is cos QCP $\overline{\cos\phi}$; that is, zero, since $\overline{\cos\phi}=0$. Therefore molecules of gas I. issuing from encounter with molecules of gas II. with velocity v have no average translation-velocity; that is, $\alpha_2'=0$.

15. Our equation (2) is thus reduced to

$$\frac{v^2}{3}\frac{dn_2}{dx}=\tfrac{1}{2}n_1\mathrm{B}(\alpha-\alpha_1')+\tfrac{1}{2}n_1\mathrm{B}\alpha,$$

or

$$\alpha=\frac{1}{3n_1}\frac{dn_2}{dx}\frac{v^2}{\mathrm{B}}+\tfrac{1}{2}\alpha_1';$$

where α_1' is the mean projection on x of the velocity v of a molecule of gas I. as it comes with that velocity out of an encounter with another molecule of gas I. Now there can be no gain or loss of x velocity to gas I. as a whole from encounters between its own molecules, although some classes may gain and others lose. It follows that if we integrate for all values of v from ∞ to 0,

$$\int f(v)\mathrm{B}(\alpha-\alpha_1')\,dv=0,$$

or

$$\int f(v)\mathrm{B}\alpha\,dv=\int f(v)\mathrm{B}\alpha_1'\,dv.$$

But the effect of these encounters is to equalize wholly or partially the translation-velocities of the different classes; so that α_1' is less than α for high, greater than α for low, values of v. Also B increases with v. Therefore, since

$$\int_0^\infty f(v)\mathrm{B}\alpha\,dv=\int_0^\infty f(v)\mathrm{B}\alpha_1'\,dv,$$

$\int_0^\infty f(v)\alpha_1'\,dv$ must be greater than $\int_0^\infty f(v)\quad dv$.

16. We have then

$$\alpha=\frac{1}{3n_1}\frac{dn_2}{dx}\frac{v^2}{\mathrm{B}}+\tfrac{1}{2}\alpha_1',$$

$$\int_0^\infty f(v)\alpha\,dv=\frac{1}{3n_1}\frac{dn_2}{dx}\int_0^\infty f(v)\,\frac{v^2}{\mathrm{B}}\,dv+\tfrac{1}{2}\int_0^\infty f(v)\alpha'\,dv;$$

$$\therefore\int_0^\infty f(v)\,\alpha\,dv>\frac{1}{3n_1}\frac{dn_2}{dx}\int_0^\infty f(v)\,\frac{v^2}{\mathrm{B}}\,dv+\tfrac{1}{2}\int_0^\infty f(v)\,\alpha\,dv.$$

Therefore, taking mean values, $\overline{n_1}$ is the stream of gas I., and

$$\overline{n_1}>\frac{2}{3}\frac{dn_2}{dx}\,\overline{\frac{v^2}{\mathrm{B}}}\Big|,$$

where $\left.\frac{\overline{v^2}}{B}\right|$ denotes the mean for all classes of molecules of the function $\frac{2}{B}$. We may write vl for $\frac{v^2}{B}$, if l be the mean free path for a v. We have thus obtained an inferior limit for $n_1\bar{\alpha}$.

17. We have now to ascertain the superior limit. Let us suppose α to be for an instant constant. The gain of x velocity to the class v per unit of time due to variation of density is in this case, as before,

$$f(v)\,dv\,\frac{v^2}{3}\frac{dn_2}{dx}.$$

And there is a loss due to encounters. We cannot with α constant make the loss equal to the gain for each class separately; but we can so choose the constant α as that, for an instant, the loss to the whole gas per unit time shall be equal to the gain for the whole gas. That is,

$$\int_0^\infty f(v)\,dv\,\frac{v^2}{3}\frac{dn_2}{dx}=\int_0^\infty f(v)n_1\mathrm{B}(\alpha-\alpha')dv$$
$$=\int_0^\infty dv\,f(v)n_1\{\tfrac{1}{2}\,\mathrm{B}\overline{\alpha-\alpha_1'}+\tfrac{1}{2}\,\mathrm{B}\overline{\alpha-\alpha_2'}\}.$$

Then, as before,

$$\alpha_2'=0;$$

and, as before,

$$\int_0^\infty f(v)\mathrm{B}(\alpha-\alpha_1')\,dv=0.$$

Our equation is thus reduced to

$$\frac{1}{3}\frac{dn_2}{dx}\int_0^\infty dv\,f(v)\,v^2=\frac{n_1}{2}\int_0^\infty dv\,f(v)\mathrm{B}\alpha=n_1\,\frac{\alpha}{2}\int_0^\infty dv\,f(v)\,\mathrm{B}\,;$$

and therefore

$$n_1\bar{\alpha}=\frac{2}{3}\frac{dn_2}{dx}\,\overline{v^2}/\overline{\mathrm{B}}.$$

Here we have $\overline{v^2}/\overline{\mathrm{B}}$, the quotient of the means, instead of $\left.\frac{\overline{v^2}}{\mathrm{B}}\right|$, the mean of the quotient.

Also for given stream, the loss of translation-velocity by encounters is less when α is constant than when α is variable, and increases with v. Therefore, to make the loss equal to the gain for the whole gas, we must make $\bar{\alpha}$ greater in the former than in the latter case. And therefore the value of $\bar{\alpha}$ last found must be greater than the true stream-velocity. We may then say that the stream $\overline{n_1\alpha}$ lies between

$$\frac{2}{3}\frac{dn_2}{dx}\frac{\overline{v^2}}{\text{B}}\bigg| \quad \text{and} \quad \frac{2}{3}\frac{dn_2}{dx}\overline{v^2}/\overline{\text{B}}.$$

18. The value of the stream which we have found at C must be the same at every point in the tube. This depends on $\frac{dn_2}{dx}$ being constant. Now from equation (1) we deduce

$$\frac{1}{3}\frac{\overline{v^2}}{\text{B}}\bigg|\frac{dn_2}{dx}=n_1\overline{\alpha}-n_1\overline{\alpha'}.$$

and $n_1\overline{\alpha}$ is constant. In the case we are now treating, all molecules having the same mass and diameter, the relation between $\overline{\alpha'}$ and $\overline{\alpha}$ is the same at all points of the tube. Therefore $n(\overline{\alpha}-\overline{\alpha'})$ is constant, and $\frac{dn_2}{dx}$ is constant.

19. It would appear from this investigation that if the molecules of gas I. could be so guided as never to collide with each other, but only with the common enemy, the molecules of the other gas (the motion being in other respects unaltered), then the stream-velocity would assume $\frac{2}{3}\frac{dn_2}{dx}\frac{\overline{v^2}}{\text{B}}\bigg|$ as its limiting value. We might, on the other hand, keep continually changing the directions, so as to make and maintain α the same for all classes without changing its mean value $\overline{\alpha}$, or in other respects altering the motion. In this case the stream-velocity would assume the limiting value $\frac{2}{3}\frac{dn_2}{dx}\overline{v^2}/\overline{\text{B}}$.

LXII. *On Resistance and Conductance Operators, and their Derivatives, Inductance and Permittance, especially in connexion with Electric and Magnetic Energy.* *By* OLIVER HEAVISIDE[*].

1. IF we regard for a moment Ohm's law merely from a mathematical point of view, we see that the quantity R, which expresses the resistance, in the equation V=RC, when the current is steady, is the operator that turns the current C into the electromotive force V. It seems, therefore, appropriate that the operator which takes the place of R when the current varies should be termed the resistance-operator. To formally define it, let any self-contained electrostatic and

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[*] Communicated by the Author.

electromagnetic combination be imagined to be cut anywhere, producing two electrodes or terminals. Let the current entering at one and leaving at the other terminal be C, and let the potential-difference be V, this being the fall of potential from where the current enters to where it leaves. Then, if $V = ZC$ be the differential equation (ordinary, linear) connecting V and C, the resistance-operator is Z.

All that is required to constitute a self-contained system is the absence of impressed force within it, so that no energy can enter or leave it (except in the latter case by the irreversible dissipation concerned in Joule's law) until we introduce an impressed force; for instance, one producing the above potential-difference V at a certain place, when the product VC expresses the energy-current, or flux of energy into the system per second.

The resistance-operator Z is a function of the electrical constants of the combination and of d/dt, the operator of time-differentiation, which will in the following be denoted by p simply. As I have made extensive use of resistance-operators and connected quantities in previous papers*, it will be sufficient here, as regards their origin and manipulation, to say that resistance-operators combine in the same way as if they represented mere resistances. It is this fact that makes them of so much importance, especially to practical men, by whom they will be much employed in the future. I do not refer to practical men in the very limited sense of anti- or extra-theoretical, but to theoretical men who desire to make theory practically workable by the simplification and systematization of methods which the employment of resistance-operators and their derivatives allows, and the substitution of simple for more complex ideas. In this paper I propose to give a connected account of most of their important properties, including some new ones, especially in connexion with energy, and some illustrations of extreme cases, which are found, on examination, to "prove the rule."

2. If we put $p=0$ in the resistance-operator of any system as above defined, we obtain the steady resistance, which we may write Z_0. If all the operations concerned in Z involve only differentiations, it is clear that when C is given completely, V is known completely. But if inverse operations (integrations) have to be performed, we cannot find V from C completely; but this does not interfere with the use of the resistance-operator for other purposes.

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* Especially Part III., and after, "On the Self-Induction of Wires," Phil. Mag. Oct. 1886 and after. Also 'Electrician,' Dec. 1884.

It is sometimes more convenient to make use of the converse method. Thus, let Y be the reciprocal of Z, so that $C=YV$. If we make p vanish in Y, the result, say Y_0, is the conductance of the combination. Therefore Y is the conductance-operator.

The fundamental forms of Y and Z are

$$Z=R+Lp, \qquad (1)$$

$$Y=K+Sp. \qquad (2)$$

In the first case, it is a coil of resistance R and inductance L that is in question, with the momentum LC and magnetic energy $\frac{1}{2}LC^2$. In the second case, it is a condenser of conductance K and permittance S, with the charge SV and electric energy $\frac{1}{2}SV^2$; or its equivalent, a perfectly non-conducting condenser having a shunt of conductance K.

In a number of electromagnetic problems (no electric energy) the resistance-operator of a combination, even a complex one, reduces to the simple form (1). The system then behaves precisely like a simple coil, so far as externally impressed force is concerned, and is indistinguishable from a coil, provided we do not inquire into the internal details. I have previously given some examples*. Substituting condensers for coils, permittances for inductances, we see that corresponding reductions to the simple form (2) occur in electrostatic combinations (no magnetic energy).

But such cases are exceptional; and, should a combination store both electric and magnetic energy, it is not possible to effect the above simplifications except in some very extreme circumstances. There are, however, two classes of problems which are important practically, in which we can produce simplicity by a certain sacrifice of generality. In the first class the state of the whole combination is a sinusoidal or simple harmonic function of the time. In the second class we ignore altogether the manner of variation of the current, and consider only the integral effects in passing from one steady state to another, which are due to the storage of electric and magnetic energy.

3. If the potential-difference at the terminals be made sinusoidal, the current will eventually become sinusoidal in every part of the system, unless it be infinitely extended, when consequences of a singular nature result. At present we are concerned with a finite combination. Then, if $n/2\pi$ be the frequency of the alternations, we have the well-known pro-

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* "On the Self-Induction of Wires," Parts VI. and VII. (Phil. Mag. [5], vols. xxiii. & xxiv.)

perty $p^2=-n^2$; which substitution, made in Z and Y, reduces them to the forms

$$Z=R'+L'p, \quad \ldots \quad (3)$$

$$Y=K'+S'p; \quad \ldots \quad (4)$$

where R′, L′, K′, S′ are functions of the electrical constants and of n^2, and are therefore constants at a given frequency.

In the first case we compare the combination to a coil whose resistance is R′ and inductance L′, so that R′ and L′ are the effective resistance and inductance of the combination, originally introduced by Lord Rayleigh* for electromagnetic combinations. In my papers, however, there is no limitation to cases of magnetic energy only†, and it would be highly inconvenient to make a distinction.

In a similar way, in the second case we compare the combination to a condenser, and we may then call K′ the effective conductance and S′ the effective permittance at the given frequency. R′ reduces to Z_0, and K′ to Y_0 at zero frequency. But it is important to remember that the two comparisons are of widely different natures: and that the effective resistance is not the reciprocal of the effective conductance.

Y and Z in (3) and (4) are reciprocal, or $YZ=1$, just as the general Y and Z of (1) and (2) are reciprocal.

If (V) and (C) denote the amplitudes of V and C, we have, by (3) and (4),

$$(V)/(C)=(R'^2+L'^2n^2)^{\frac{1}{2}}=I, \text{ say}, \quad \ldots \quad (5)$$

$$(C)/(V)=(K'^2+S'^2n^2)^{\frac{1}{2}}=J, \text{ say}. \quad \ldots \quad (6)$$

I and J are also reciprocal. The former, I, being the ratio of the force to the flux (amplitudes), is the impedance of the combination. It is naturally suggested to call J the "admittance" of the combination. But it is not to be anticipated that this will meet with so favourable a reception as impedance, which term is now considerably used, because the methods of representation (1), (3), and (5) are more useful practically than (2), (4), and (6); although theoretically the two sets are of equal importance‡.

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* Phil. Mag. May 1886.

† In Part V. of "On the Self-Induction of Wires" I have given a few examples of mixed cases of an elementary nature, in connexion with the problem of finding the effect of an impressed force in a telegraph circuit.

‡ The necessity of the term impedance (or some equivalent) to take the place of the various utterly misleading expressions that have been used, has come about through the wonderful popularization of electromagnetic knowledge due to the dynamo, and its adoption to Sir W. Thomson's approval of it and of one or two other terms.

To obtain the relations between R' and K', and L' and S', we have

$$Y=(R'+L'p)^{-1}=(R'-L'p)I^{-2}, \quad . \quad . \quad . \quad . \quad . \quad (7)$$

$$Z=(K'+S'p)^{-1}=(K'-S'p)J^{-2}; \quad . \quad . \quad . \quad . \quad . \quad (8)$$

from which we derive

$$\left.\begin{array}{ll} I^2K'=R', & J^2R'=K', \\ -I^2S'=L', & -J^2L'=S', \\ L'/R'=-S'/K'. & R'/K'=I^2=-L'/S', \end{array}\right\} \quad . \quad (9)$$

all of which are useful relations.

4. By (3) and (4) we have the equations of activity

$$VC=R'C^2+p(\tfrac{1}{2}L'C^2), \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

$$VC=K'V^2+p(\tfrac{1}{2}S'V^2), \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

in general. Now, if we take the mean values, the differentiated terms go out, leaving

$$\overline{VC}=R'\overline{C^2}=K'\overline{V^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

the bars denoting mean values. The three expressions in (12) each represent the mean dissipativity, or heat per second. R' and K' are therefore necessarily positive. It should be noted that $R'C^2$ or $K'V^2$ do not represent the dissipativity at any moment. The dissipativity fluctuates, of course, because the square of the current fluctuates; but besides that, there is usually a fluctuation in the resistance, because the distribution of current varies, and it is only by taking mean values that we can have a definite resistance at a given frequency.

If the combination be electromagnetic, and T denote the magnetic energy, its mean value is given by

$$\overline{T}=\tfrac{1}{2}L'\overline{C^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

so that L' is necessarily positive and S' negative. But $\frac{1}{2}L'C^2$ is not usually the magnetic energy at any moment.

If the combination be electrostatic, and U denote the electric energy, its mean value is

$$\overline{U}=\tfrac{1}{2}S'\overline{V^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

so that S' is positive and L' negative. The electric energy at any moment is not usually $\frac{1}{2}S'V^2$.

But, in the general case of both energies being stored, we have

$$\overline{T}-\overline{U}=\tfrac{1}{2}L'\overline{C^2}=-\tfrac{1}{2}S'\overline{V^2}. \quad . \quad . \quad . \quad . \quad . \quad (15)$$

If the mean magnetic energy preponderates, the effective inductance is positive, and the permittance negative; and conversely, if the electric energy preponderates. If there be no condensers, the comparison with a coil is obviously most suitable, and if there be no magnetic energy we should naturally use the comparison with a condenser; but when both energies coexist, which method of representation to adopt is purely a matter of convenience in the special application concerned.

If the mean energies, electric and magnetic, be equal, then

$$\left.\begin{aligned} L'=0=S', \quad R'K'=1, \\ I=R', \qquad J=K'. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (16)$$

That is, by equalizing the mean energies we bring the current and potential-difference into the same phase, annihilate the effective inductance (and also permittance), and make the effective conductance the reciprocal of the effective resistance, which now equals the impedance itself. It should be noted that the vanishing of the energy-difference only refers to the mean value. The two energies are not equal and do not vanish simultaneously. Sometimes, however, their sum is constant at every moment, but this is exceptional. [Example, a coil and a condenser in sequence.]

5. Passing now to the second class referred to in § 2, imagine, first, the combination to be electromagnetic, and that V is steady, producing a steady C, dividing in the system in a manner solely settled by the distribution of conductivity. Although we cannot treat the combination as a coil as regards the way the current varies when the impressed force is put on, we may do so as regards the integral effect at the terminals produced by the magnetic energy. This last is the well-known quadratic function of the currents in different parts of the system,

$$T=\tfrac{1}{2}L_1C_1^2+MC_1C_2+\tfrac{1}{2}L_2C_2^2+\dots \quad . \quad . \quad (17)$$

Now put every one of these C's in terms of *the* C, the total current at the terminals, which may be done by Ohm's law. This reduces T to

$$T=\tfrac{1}{2}L_0C^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

where L_0 is a function of the real inductances, self and mutual, of the parts of the system, and of their resistances. This L_0 may be called the impulsive inductance of the system. For although it is, in a sense, the effective steady inductance, taking the current C at the terminals as a basis, being, in fact, the value of the sinusoidal inductance L′ at zero frequency; yet, as it is only true for impulses that the combination behaves as a coil of inductance L_0, it is better to signify this fact in the name, to avoid confusion. This will be specially useful in the more general case in which both energies are concerned.

Secondly, let the system be electrostatic. Then, in a similar way, we may write the electric energy in the form

$$U = \tfrac{1}{2} S_0 V^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (19)$$

in terms of the V at the terminals, where S_0 is a function of the real permittances and of the resistances. S_0 is the impulsive permittance of the combination. It is also the sinusoidal S' at zero frequency.

In (18) L_0 is positive, and in (19) S_0 is positive. The momentum or electromotive impulse at the terminals in the former case is L_0C, and in the latter case is $-S_0RV$, where R is the steady resistance. The true analogue of momentum, however, is charge, or time-integral of current, and this, at the terminals, is $-S_0V$, corresponding to L_0C.

6. Passing to the general case, and connecting with the resistance-operator, let Γ be the current at the terminals at time t when varying, so that

$$V = Z\Gamma = (Z_0 + pZ_0' + \tfrac{1}{2}p^2Z_0'' + \ldots)\Gamma, \quad . \quad . \qquad (20)$$

where the accents denote differentiations to p, and the zero suffixes indicate that the values when $p=0$ are taken. The coefficients of the powers of p are therefore constants. Integrating to the time,

$$\int V\,dt = \int Z_0\Gamma\,dt + Z_0'[\Gamma] + \tfrac{1}{2}Z_0''[\dot{\Gamma}] + \ldots . \quad . \qquad (21)$$

If the current be steady at beginning and at end,

$$\int (V - Z_0\Gamma)\,dt = Z_0'[\Gamma], \quad . \quad . \quad . \quad . \qquad (22)$$

and if the initial current be zero, and the final value be C,

$$\int (V - Z_0\Gamma)\,dt = Z_0'C\,; \quad . \quad . \quad . \quad . \quad . \qquad (23)$$

so that $Z_0'C$ is the electromotive impulse employed in setting up the magnetic and the electric energy of the steady state due to steady V at the terminals. Thus

$$L_0 = Z_0' \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (24)$$

finds the impulsive inductance from the resistance-operator. Or,

$$L_0 = (Z - Z_0)\,p^{-1} \text{ with } p=0. \quad . \quad . \quad . \quad . \qquad (25)$$

In a similar manner, we may show that

$$S_0 = Y_0' = -Z_0^{-2}Z_0' \quad . \quad . \quad . \quad . \quad . \quad . \qquad (26)$$

finds the impulsive permittance from the conductance-operator. L_0C and $-S_0Z_0V$ are equivalent expressions for the electromotive impulse.

If Z_0 should be infinite, then use Y. For instance, the insertion of a nonconducting condenser of permittance S_1 in the main circuit of the current makes Z_0 infinite, since the resistance-operator of the condenser is $(S_1p)^{-1}$. There is no final steady current, and L_0 is infinite. We should then use (26) instead of (24), especially as the energy is wholly electric in the steady state.

7. To connect with the energy, multiply (23) by C, the final current, and, for simplicity, let V be steady; giving

$$\int(V-R\Gamma)C\,dt=Z_0'C^2=\int V(C-\Gamma)\,dt. \quad . \quad . \quad (27)$$

It may be anticipated from the preceding that these equated quantities express twice the excess of the magnetic over the electric energy.

In connexion with this I may quote from Maxwell, vol. ii. art. 580. A purely electromagnetic system is in question. "If the currents are maintained constant by a battery during a displacement in which a quantity of work, W, is done by electromotive force, the electrokinetic energy of the system will be at the same time increased by W. Hence the battery will be drawn upon for a double quantity of energy, or 2W, in addition to that which is spent in generating heat in the circuit. This was first pointed out by Sir W. Thomson. Compare this result with the electrostatic property in art. 93." The electrostatic property referred to relates to conductors charged by batteries. If "their potentials are maintained constant, they tend to move so that the energy of the system is increased, and the work done by the electrical forces during the displacement is equal to the increment of the energy of the system. The energy spent by the batteries is equal to double of either of these quantities, and is spent half in mechanical, half in electrical work."

Although of a somewhat similar nature, these properties are not what is at present required, which is contained in the following general theorem given by me*:—Let any steady impressed electric forces be suddenly started and continued in a medium permitting linear relations between the two forces, electric and magnetic, and the three fluxes—conduction current, electric displacement, and magnetic induction (but with no rotational property allowed, even for conduction current); the whole work done by the impressed forces during the establish-

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* 'Electrician,' April 25, 1885, p. 490.

ment of the steady state exceeds what would have been done had this state been instantly established (but then without any electric or magnetic energy) by twice the excess of the electric over the magnetic energy. That is,

$$\int_0^\infty dt\,\Sigma\,\mathbf{e}(\boldsymbol{\Gamma}-\boldsymbol{\Gamma}_0)dt=2(\mathrm{U}-\mathrm{T}), \quad . \ . \ . \ . \quad (28)$$

where $\mathbf{e}$ stands for an element of impressed force, $\boldsymbol{\Gamma}$ the current-density at time t, $\boldsymbol{\Gamma}_0$ the final value, and Σ the space-integration to include all the impressed forces. (Black letters for vectors.) The theorem (28) seems the most explicit and general representation of what has been long recognized in a general way, that permitting electric displacement increases the activity of a battery, whilst permitting magnetization decreases it. The one process is equivalent to allowing elastic yielding, and the other to putting on a load (not to increasing the resistance, as is sometimes supposed).

Applying (28) to our present case of one impressed force V, producing the final current C, we obtain

$$\int dt\,\mathrm{V}(\Gamma-\mathrm{C})dt=2(\mathrm{U}-\mathrm{T}), \quad . \ . \ . \ . \quad (29)$$

comparing which with (27), we see that

$$\mathrm{T}-\mathrm{U}=\tfrac{1}{2}\mathrm{Z}_0'\mathrm{C}^2=\tfrac{1}{2}\mathrm{L}_0\mathrm{C}^2=-\tfrac{1}{2}\mathrm{S}_0\mathrm{V}^2, \quad . \ . \ . \quad (30)$$

confirming the generality of our results.

8. It is scarcely necessary to remark that the properties of Z and Z′ previously discussed do not apply merely to combinations consisting of coils of fine wire and condensers; the currents may be free to flow in conducting masses or dielectric masses. Solid cores, for example, may be inserted in coils within the combination. The only effect is to make the resultant resistance-operator at a given place more complex.

But a further very remarkable property we do not recognize by regarding only common combinations of coils and condensers. If we, in the complex medium above defined, select any unclosed surface, or surface bounded by a closed line, and make it a shell of impressed force (analogous to a simple magnetic shell), thereby producing a potential-difference V between its two faces, and C be the current through the shell in the direction of the impressed force, there must be a definite resistance-operator Z connecting them, depending upon the distribution of conductivity, permittivity, and inductivity through all space, and determinable by a sufficiently exhaustive analysis. The remarkable property is that the resistance-operator is the same for any surfaces having the same bounding-edge. For a closed shell of impressed force of uniform strength can produce no flux whatever. This is instructively shown by the equation of activity,

$$\Sigma \mathbf{e}\boldsymbol{\Gamma} = Q + \dot{U} + \dot{T}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (31)$$

indicating that the sum of the activities of the impressed forces, or the energy added to the system per second, equals the total dissipativity Q, *plus* the rate of increase of the stored energies, electric and magnetic, throughout the system. Now here $\boldsymbol{\Gamma}$ consists of closed tubes; if, therefore, the distribution of $\mathbf{e}$ be polar, or $\mathbf{e}$ be the vector space-variation of a single-valued scalar potential, of which a simple closed shell of impressed force is an example, the left member of (31) vanishes, so that the dissipation, if any, is derived entirely from the stored energy. Start, then, with no electric or magnetic energy in the system; then the positivity of Q, U, and T ensures that there never can be any, under the influence of polar impressed force. Hence two shells of impressed force of equal uniform strength produce the same fluxes if their edges be the same; not merely the steady fluxes possible, but the variable fluxes anywhere at corresponding moments after commencing action. The only difference made when one shell is substituted for the other is in the manner of the transfer of energy at the places of impressed force; for we have to remember that the effective force producing a flux, or the "force of the flux," equals the sum of the impressed force and the "force of the field;" whereas the transfer of energy is determined by the vector product of the two forces of the field, electric and magnetic respectively. In (31) no count is taken of energy transferred from one seat of impressed force to another, reversibly, all such actions being eliminated by the summation.

It is well to bear in mind, when considering the consequences of this transferability of impressed force, especially in cases of electrolysis or the Volta force, not only that the three physical properties of conductivity, permittivity, and inductivity, though sufficient for the statement of the main facts of electromagnetism, are yet not comprehensive, but also that they have no reference to molecules and molecular actions; for the equations of the electromagnetic field are constructed on the hypothesis of the ultimate homogeneity of matter, or, in another form, only relate to elements of volume large enough to allow us to get rid of the heterogeneity.

As the three fluxes are determined solely by the vorticity (to borrow from liquid motion) of the vector impressed force, we cannot know the distribution of the latter from that of the former, but have to find where energy transformations are going on; for the denial of the law that $\mathbf{e\Gamma}$ not only measures the activity of an impressed force $\mathbf{e}$ on the current $\mathbf{\Gamma}$, but represents energy received by the electromagnetic system at the very same place, lands us in great difficulties.

Again, as regards the "electric force of induction." We cannot find the distribution through space of this vector from the Faraday law that its line-integral in a closed circuit equals the rate of decrease of induction through the circuit. We may add to any distribution satisfying this law any polar distribution without altering matters, except that a different potential function arises. In this case we do not even alter the transfer of energy. The electric force of the field is always definite; but when we divide it into two distinct distributions, and call one of them the electric force of induction, and the other the force derived from electric potential, it is then quite an indeterminate problem how to effect the division, unless we choose to make the quite arbitrary assumption that the electric force of induction has nothing of the polar character about it (or has no divergence anywhere), when of course it is the other part that possesses the whole of the divergence. This fact renders a large part of some mathematical work on the electromagnetic field that I have seen redundant, as we may write down the final results at the beginning. In the course of some investigations concerning normal electromagnetic distributions in space I have been forcibly struck with the utter inutility of dividing the electric field into two fields, and by the simplicity that arises by not doing so, but confining oneself to the actual forces and fluxes, which describe the real state of the medium and have the least amount of artificiality about them. Similar remarks apply to Maxwell's vector-potential **A**. Has it divergence or not? It does not matter in the least, on account of the auxiliary polar force. When the electric force itself is made the subject of investigation, the question of divergence of the vector-potential does not present itself at all.

The lines of vorticity, or vortex-lines of the vector impressed force, are of the utmost importance, because they are the originating places of all disturbances. This is totally at variance with preconceived notions founded upon the fluid analogy, which is, though so useful in the investigation of steady states, utterly misleading when variable states are in question, owing to the momentum and energy belonging to the magnetic field, not to the electric current. Every solution involving impressed forces consists of waves emanating from the vortex lines of impressed force (electric or magnetic as the case may be, but only the electric are here considered), together with the various reflected waves produced by change of media and other causes. At the first moment of starting an impressed force the only disturbance is at the vortex lines, which are the first lines of magnetic induction.

(*a*) Thus a uniform field of force suddenly started over all space can produce no effect. For, either there are no vortex lines at all, or they are at an infinite distance, so that an infinite time must elapse to produce any effect at a finite distance from the origin.

(*b*) Copper and zinc put in contact. Whether the Volta-force be at the contact or over the air-surfaces away from and terminating at the contact (if perfectly metallic), the vortex-line is the common meeting-place of air, zinc, and copper ; the first line of magnetic force is there, and from it the disturbance proceeds into the metals and out into the air, which ends in the steady electric field*.

Since the vortex-lines or tubes are closed, we need only consider one at present—say, that due to a simple shell of impressed force. If it be wholly within a conductor, the initial wave emanating from it is so rapidly attenuated by the conductivity (the process being akin to repeated internal reflexions, say reflexion of 9 parts and transmission of 1 part, repeated at short intervals) that the transmission to a distance through the conductor (if good) becomes a very slow process, that of diffusion. Consequently, when the impressed force is rapidly alternated, there is no sensible disturbance except at and near the vortex-line.

But if there be a dielectric outside the conductor, the moment disturbances reach it, and therefore instantly if the vortex-line be on the boundary, waves travel through the dielectric at the speed of light unimpeded, and without the attenuating process within the conductor, which therefore becomes exposed to electric force all over its boundary in a very short time ; hence diffusion inward from the boundary. The electric telegraph would be impossible without the dielectric. It would take ages if the wire itself had to be the seat of transfer of energy.

(*c*) In the electromagnetic theory of the rise of current in a wire we have, at first sight, an exception to the law that at the first moment there is no disturbance except at the vortex-lines of impressed force. But it is that theory which is incorrect, in assuming that there is no displacement. This is equivalent to making the speed of propagation through the dielectric infinitely great ; so that we have results mathe-

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* "Some Remarks on the Volta Force," Journal S. T. E. & E., 1885.

matically equivalent to distributing the impressed force throughout the whole circuit, and therefore its vortex-lines over the whole boundary*. In reality, with finite speed, the disturbances come from the real vortex-lines in time.

There is still a limitation of the disturbances to the neighbourhood of the vortex-lines when they are on the boundary of the conductor, and the frequency of alternations is sufficiently great, the impressed force being within the conductor.

But in a non-conducting dielectric this effect does not occur, at least in any case I have examined. On the contrary, as the frequency is raised, there is a tendency to constancy of amplitude of the waves sent out from the edge of a simple sheet of impressed force, or from a shell of vortex-lines of the same, in a dielectric. Very remarkable results follow from the coexistence of the primary and reflected waves. Thus :—

(*d*) If a spherical portion of an infinitely extended dielectric have a uniform field of alternating impressed force within it, and the radius a, the frequency $n/2\pi$, and the speed v be so related that

$$\tan\frac{na}{v} = \frac{na}{v},$$

there is no disturbance outside the sphere. There are numerous similar cases ; but this is a striking one, because, from the distribution of the impressed force, it looks as if there *must* be external displacement produced by it. There is not, because the above relation makes the primary wave outward from the surface of the sphere, which is a shell of vorticity, be exactly neutralized by the reflexion, from the centre, of the primary wave inward from the surface.

(*e*) If, instead of alternating, the uniform field of impressed force in (*d*) be steady, the final steady electric field due to it takes the time $(r+a)/v$ to be established at distance r from the centre. The moment the primary wave inward reaches the centre, the steady state is set up there ; and as the reflected wave travels out, its front marks the boundary between the steady field (final) and a spherical shell of depth $2a$, within which is the uncancelled first portion of the primary wave outward from the surface ; which carries out to an infinite distance an amount of energy equal to that of the final steady electric field. This is the loss by radiation. [The magnetic energy in this shell equals half the final

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* 'Electrician,' June 25, 1886, p. 129.

electric energy on the whole journey; the electric energy in the shell is greater, but ultimately becomes the same.] In practical cases this energy would be mostly, perhaps wholly dissipated in conductors.

(*f*) If a uniformly distributed impressed force act alternatingly longitudinally within an infinitely long circular cylindrical portion of a dielectric, the axis is the place of reflexion of the primary wave inward, and the reflected wave cancels the outward primary wave when

$$J_1(na/v) = 0\,;$$

so that there is no external disturbance, except at first.

(*g*) There is a similar result when the vorticity of impressed force takes the place of impressed force in (*f*).

(*h*) If the alternating impressed force act uniformly and longitudinally in a thin conducting-tube of radius a, with air within and without, then

$$J_0(na/v) = 0$$

destroys the external field and makes the conduction-current depend upon the impressed force only. And if we put a barrier at distance x to serve as a perfect reflector, that is, a tube of infinite conductivity,

$$J_0(nx/v) = 0$$

makes the electric force of the field in the inner tube to be the exact negative of the impressed force; so that there is no conduction-current. The electromagnetic field is in stationary vibration. If the inner tube be situated at one of the nodal surfaces of electric force, the vibrations mount up infinitely.

(*i*) If, in case (*h*), the impressed force act circularly about the axis of the inner tube (which may be replaced by a solenoid of small depth),

$$J_1(na/v) = 0$$

destroys the external field, and

$$J_1(nx/v) = 0$$

makes the electric force of the field the negative of the impressed force, and so destroys the conduction-current.

(*j*) We can also destroy the longitudinal force of the field in a conductor without destroying the external field. Let it be a wire of steady resistance in a dielectric, and the impressed force in it be

$$e = e_0 \cos mx \cos nt$$

per unit length. Then $m=n/v$ makes e be the force of the flux, in the wire; so that the current is Ke, if K be the conductance of unit length.

These examples are mostly selected from a paper I am now writing on the subject of electromagnetic waves, which I hope to be permitted to publish in this Journal.

If the electric and magnetic energies, and the dissipation of energy, in a given system be bounded in their distribution, it is clear that the resistance-operator is a rational function of p. But should the field be boundless, as when conductors are contained in an infinitely extended dielectric, then just as complete solutions in infinite series of normal solutions may become definite integrals by the infinite extension, so may the resistance-operator become irrational. We may also have to modify the meaning of the sinusoidal R$'$ from representing mean resistance only, on account of the never-ceasing outward transfer of energy so long as the impressed force continues.

9. Returning to a finite combination represented by $V=ZC$, there are at least three kinds of induction-balances possible. First, true balances of similar systems, where we balance one combination against another which either copies it identically or upon a reduced scale, without any reference to the manner of variation of the impressed force. Along with these we may naturally include all cases in which the Z of a combination, in virtue of peculiar internal relations, reduces to a simpler form representing another combination, equivalent so far as V and C are concerned. The telephone may be employed with great advantage, and is, in fact, the only proper thing to use, especially for the observation of phenomena.

There are, next, the sinusoidal current-balances. These are also true, in being independent of the time, so that the telephone may be used; but are of course of a very special character otherwise. Here any combination is made equivalent to a mere coil if L$'$ be positive, or to a condenser if S$'$ be positive [§§ 3 and 4], and so may be balanced by one or the other. But intermittences of current cannot be safely taken to represent sinusoidality, and large errors may result from an assumed equivalence.

In the third kind of balances it is the impulsive inductance that is balanced against some other impulsive inductance, positive or negative as the case may be; or perhaps the impulsive inductance of a combination is made to vanish, by equating the electric and magnetic energies in it when its state is steady. The rule that the impulsive balance in a Christie arrangement without mutual induction between the four sides is given by equating to zero the coefficient of p in the expansion of $Z_1Z_4-Z_2Z_3$ in powers of p, where Z_1 &c. are the resistance-operators of the four sides*, is in agreement with the rule derived from (24) or (25) above, to make the impulsive inductance of one combination vanish. Impulsive, or "kick" balances, naturally require a galvanometer. Even then, however, the method is sometimes unsatisfactory, when the opposing influences which make up the impulse are not sufficiently simultaneous, as has been pointed out by Lord Rayleigh†.

There is also the striking method of cumulation of impulses employed by Ayrton and Perry‡, employing false resistance-balances. It seems complex, and of rather difficult theory; but, just as a watch is a complex piece of mechanism, and is yet thoroughly practical, so perhaps the secohmmeter may have a brilliant career before it.

Several interesting papers relating to the comparison of inductances and permittances have appeared lately. It is usually impulsive balances that are in question, probably because it is not the observation of phenomena that is required, but a direct, even if rough, measurement of the inductance or permittance concerned, often under circumstances that do not well admit of the use of the telephone. Only one of these papers, however, contains anything really novel, scientifically, viz. that of Mr. W. H. Preece, F.R.S.§, who concludes, from his latest researches, that the "coefficient of self-induction" of copper telegraph-circuits is nearly zero, the results he gives being several hundred times smaller than the formula derived from electromagnetic principles asserts it to be. Here is work for the physicist.

10. To equate the expressions for the electric and magnetic energies of a combination is, I find, in simple cases, the easiest and most direct way of furnishing the condition that the impulsive inductance shall vanish. Thus, if there be but one condenser and one coil, $SV^2=LC^2$ is the condition, S and L being the permittance and the inductance respectively, V the potential-difference of the condenser, and C the current in the coil. The relation between V and C will be, of course, dependent upon the resistances concerned‖. But in complex cases, and to obtain the value of the impulsive inductance when it is not zero, equation (24) is most useful.

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* "On the Self-Induction of Wires," Part VI., Phil. Mag. Feb. 1887.

† 'Electrical Measurements,' p. 65.

‡ Journ. Soc. Tel. Engineers and Electricians, 1887.

§ B.A. Meeting, 1887: "On the Coefficient of Self-Induction of Iron and Copper Wires."

‖ If the condenser shunts the coil, making $V=RC$, we get the case brought before the S. T. E. & E. by Mr. Sumpner, with developments.

The following illustration of the properties of Z and Z_0' is a complex one, but I choose it because of its comprehensive character, and because it leads to some singular extreme cases, interesting both mathematically and in the physical interpretation of the apparent anomalies. Let the combination be a telegraph-circuit, say a pair of parallel copper wires, of length l; resistance R, permittance S, inductance L, and leakage-conductance K, all per unit length, and here to be considered strictly constants, or independent of p. Let the two wires be joined through an arrangement whose resistance-operator is Z_1 at the distant end B; then the resistance-operator at the beginning A of the circuit is given by*

$$Z=\frac{(R+Lp)l\{(\tan ml)/ml\}+Z_1}{1+(K+Sp)lZ_1\{(\tan ml)/(ml)\}}, \quad . \quad . \quad (32)$$

if

$$-m^2=(R+Lp)(K+Sp). \quad . \quad . \quad . \quad . \quad (33)$$

Take $Z_1=0$ for the present, or short-circuit at B. This makes

$$Z=(R+Lp)l(\tan ml)/ml, \quad . \quad . \quad . \quad (34)$$

and the steady resistance at A is therefore

$$Z_0=Rl(\tan m_0l)/m_0l, \quad . \quad . \quad . \quad . \quad . \quad (35)$$

if $-m_0^2=RK$. Also, differentiating (34) to p, and then making $p=0$, we find

$$Z_0'=L_0=\tfrac{1}{2}l\frac{\tan m_0l}{m_0l}\left(L-\frac{RS}{K}\right)+\tfrac{1}{2}l\sec^2 m_0l\left(L+\frac{RS}{K}\right). \quad (36)$$

represents the impulsive inductance.

If we put $S=0$ in (36) we make the arrangement electromagnetic, and then L_0 is positive. If we put $L=0$, we make it electrostatic, and L_0 is negative, or S_0, the impulsive permittance, is positive. It is to be noticed that there is no confusion when both energies are present; that is, there are no terms in Z_0' containing products of real permittances and inductances, which is clearly a general property of resistance-operators, otherwise the two energies would not be independent.

We may make L_0 vanish by special relations. Thus, if there be no leakage, or $K=0$, (36) is

$$L_0=Ll-\tfrac{1}{3}Rl\,.\,RSl^2; \quad . \quad . \quad . \quad . \quad . \quad (37)$$

so that the electromagnetic must be one third of the electrostatic time-constant to make the "extra-current" and the static charge balance. (The length of the circuit required for

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* "On the Self-Induction of Wires," Part IV., Phil. Mag. Nov. 1886.

this result may be roughly stated as about 60 kilometres if it be a single copper wire of 6 ohms per kilometre, 4 metres high, with return through the ground; but it varies considerably, of course.)

But if leakage be now added, it will increase the relative importance of the magnetic energy, so that the length of the circuit requires to be increased to produce a balance. This goes on until K reaches the value RS/L, when, as an examination of (36) will show, the length of the circuit needs to be infinitely great. The same formula also shows that if K be still greater, L_0 cannot be made to vanish at all, being then always positive.

11. Now let the circuit be infinitely long. Equation (35) reduces to the irrational form

$$Z = \pm (R + Lp)^{\frac{1}{2}}(K + Sp)^{-\frac{1}{2}}, \quad . \quad . \quad . \quad (38)$$

with ambiguity of sign. Of course the positive sign must be taken. The negative appears to refer to disturbances coming from an infinite distance, which are out of the question in our problem, as there can be no reflexion from an infinite distance. But equation (38) may be obtained directly in a way which is very instructive as regards the structure of resistance-operators. Since the circuit is infinitely long, Z cannot be altered by cutting-off from the beginning, or joining on, any length. Now first add a coil of resistance R_1 and inductance L_1 in sequence, and a condenser of conductance K_1 and permittance S_1, in bridge, at A, the beginning of the circuit. The effect is to increase Z to Z_2, where

$$Z_2 = \{K_1 + S_1 p + (R_1 + L_1 p + Z)^{-1}\}^{-1}; \quad . \quad . \quad (39)$$

i. e. the reciprocal of the new Z_2, or the new conductance-operator, equals the sum of the conductance-operators of the two branches in parallel, one the conducting condenser, the other the coil and circuit in sequence. (39) gives the quadratic

$$Z_2^2 + (R_1 + L_1 p)Z_2 = (R_1 + L_1 p)(K_1 + S_1 p)^{-1}. \quad . \quad (40)$$

Now choose R_1, L_1, K_1, S_1 in exact proportion to R, L, K, and S, and then make the former set infinitely small. The result is that we have added to the original circuit a small piece of the same type, so that Z_2 and Z are identical, and that the coefficient of the first power of Z_2 in (40) vanishes. Therefore (40) becomes

$$Z = (R + Lp)^{\frac{1}{2}}(K + Sp)^{-\frac{1}{2}}. \quad . \quad . \quad . \quad . \quad (41)$$

This fully serves to find the sinusoidal solution. Differentiating it, we find

$$L_0=\tfrac{1}{2}\frac{L}{(RK)^{\frac{1}{2}}}\left(1-\frac{RS}{KL}\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad (42)$$

corroborating the previous result as to the vanishing of L_0 when the circuit is infinitely long by equality of RS and KL and the positivity of L_0 when $KL > RS$.

12. Now, in this singular case, we have, by (41) and (42),

$$Z=Lv, \quad L_0=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (43)$$

if $v=(LS)^{-\frac{1}{2}}$, the speed of transmission of disturbances along the circuit. The resistance-operator has reduced to an absolute constant, and the current and potential-difference are in the same phase, altogether independent of the frequency of alternations, or indeed of the manner of variation. The quantity Lv, or $L\times 30$ ohms, approximately, if the dielectric be air, is strictly, and without any reservation, the impedance of the circuit at A, but it is only exceptionally the resistance.

Make $V=f(t)$, at A, an arbitrary function of the time; then, if V_x and C_x are the potential-difference and the current at distance x from A at time t, respectively, we shall have

$$\left.\begin{aligned} V_x&=f(t-x/v)\,\epsilon^{-Rx/Lv} \\ C_x&=V_x/Lv \end{aligned}\right\}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (44)$$

or all disturbances originating at A are transmitted undistorted along the circuit at the speed v, attenuating at a rate indicated by the exponential function. (I have elsewhere* fully developed the properties of this non-distortional circuit, and only mention such as are necessary to understand the peculiarities connected with the present subject-matter.) The electric and magnetic energies are always equal, not only on the whole, but in any part of the circuit; this accounts for the disappearance of L_0, and the bringing of V_x and C_x to the same phase, as we should expect from § 4. But in the present case Z_0, or Lv, or R', for they are all equal, is only the resistance when the steady state due to the steady V at A is arrived at (asymptotically), or the effective resistance at a given frequency when V is sinusoidal, and sufficient time has elapsed to have allowed V_x and C_x to become sinusoidal to such a distance from A that we can neglect the remainder of the circuit into which greatly attenuated disturbances are still being transmitted.

13. Now, since the impedance is unaltered by joining on at A any length of circuit of the same type, and is a constant,

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* "Electromagnetic Induction and its Propagation," Arts. XL. to L., 'Electrician,' 1887.

it follows that the impedance at A of a non-distortional circuit as above described, but of *finite* length, stopping at B, where $x=l$, with a *resistance* of amount Lv inserted at B, is also a constant, viz. the same Lv. To corroborate, take RS=KL and $Z_1=Lv$ in the full formula (32). The result is $Z_1=Lv$. The interpretation in this case is that all disturbances sent from A are absorbed completely by the resistance at B immediately on arrival, so that the finite circuit behaves as if it were infinitely long. The permanent state due to a steady V at A is arrived at in the time l/v. The impedance and the resistance then become identical.

14. If, in the case of § 12, we further specialize by taking R=0, K=0, producing a perfectly insulated circuit of no resistance, the impedance is, as before, Lv; but no part of it is resistance, or ever can be, in spite of the identity of phase of V and C. However long we may keep on a steady V at A, we keep the impressed force working at the same rate, the energy being entirely employed in increasing the electric and magnetic energies at the front of the wave, which is unattenuated, and cannot return.

But if we cut the circuit at B, at a finite distance l, and there insert a resistance Lv, the effect is that, as soon as the front of the wave reaches B, the inserted resistance immediately becomes the resistance of the whole combination; or the impedance instantly becomes the resistance, without change of value.

15. As a last example of singularity, substitute a short-circuit for the terminal resistance Lv just mentioned. Since there is now no resistance in any part of the system, if we make the state sinusoidal everywhere, by V sinusoidal at A, R′ must vanish, or V and C be in perpendicular phases, due to the infinite series of to-and-fro reflexions. We now have, by (32),

$$Z'=\mathrm{L}pl\frac{\tan(pli/v)}{pli/v}=\mathrm{L}lp\frac{\tan(nl/v)}{nl/v}, \quad \ldots \quad (45)$$

if $n/2\pi=$ frequency, and R′ has disappeared.

If, on the other hand, V be steady at A, the current increases without limit, every reflexion increasing it by the amount V/Lv at A or at B (according to which end the reflexion takes place at), which increase then extends itself to B or A at speed v. The magnetic energy mounts up infinitely. On the other hand, the electric energy does not, fluctuating perpetually between 0 when the circuit is uncharged, and $\frac{1}{2}\mathrm{S}l\mathrm{V}^2$ when fully charged. The impedance of the circuit to the impressed force at A is Lv for the time $2l/v$

after starting it; then $\frac{1}{3}\mathrm{L}v$ for a second period $2l/v$; then $\frac{1}{5}\mathrm{L}v$ for a third period, and so on.

It will have been observed that I have, in the last four paragraphs, used the term impedance in a wider sense than in § 3, where it is the ratio of the amplitude of the impressed force to the amplitude of the flux produced at the place of impressed force when sufficient time has elapsed to allow the sinusoidal state to be reached, when that is possible. The justification for the extension of meaning is that, since in the non-distortional circuit of infinite length, or of finite length with a terminal resistance to take the place of the infinite extension, we have nothing to do with the frequency of alternations, or with waiting to allow a special state to be established, it is quite superfluous to adhere to the definition of the last sentence; and we may enlarge it by saying that the impedance of a combination is simply the ratio of the force to the flux, when it happens to be a constant, which is very exceptional indeed. I may add that R, L, K, and S need not be constants, as in the above, to produce the propagation of waves without tailing. All that is required is $\mathrm{R}/\mathrm{L}=\mathrm{K}/\mathrm{S}$, and $\mathrm{L}v=$ constant; so that R and L may be functions of x. The speed of the current, and the rate of attenuation, now vary from one part of the circuit to another.

16. In conclusion, consider the application of the resistance-operator to normal solutions. If we leave a combination to itself without impressed force, it will subside to equilibrium (when there is resistance) in a manner determined by the normal distributions of electric and magnetic force, or of charges of condensers and currents in coils; a normal system being, in the most extended sense, a system that, in subsiding, remains similar to itself, the subsidence being represented by the time-factor ϵ^{pt}, where p is a root of the equation $\mathrm{Z}=0$. It is true that each part of the combination will usually have a distinct resistance-operator; but the resistance-operators of all parts involve, and are contained in, the same characteristic function, which is merely the Z of any part cleared of fractions. It is sometimes useful to remember that we should clear of fractions, for the omission to do so may lead to the neglect of a whole series of roots; but such cases are exceptional and may be foreseen; whilst the employment of a resistance-operator rather than the characteristic function is of far greater general utility, both for ease of manipulation and for physical interpretation.

Given a combination containing energy and left to itself, it is upon the distribution of the energy that the manner of subsidence depends, or upon the distribution of the electric and magnetic forces in those parts of the system where the permittivity and the inductivity are finite, or are reckoned finite for the purposes of calculation. Thus conductors, if they be not also dielectric, have only to be considered as regards the magnetic force, whilst in a dielectric we must consider both the electric and the magnetic force. [The failure of Maxwell's general equations of propagation arises from the impossibility of expressing the electric energy in terms of his potential function. The variables should always be capable of expressing the energy.] Now the internal connexions of a system determine what ratios the variables chosen should bear to one another in passing from place to place in order that the resultant system should be normal; and a constant multiplier will fix the size of the normal system. Thus, supposing u and w are the normal functions of potential-difference and current, which are in most problems the most practical variables, the state of the whole system at time t will be represented by

$$V=\Sigma A u \epsilon^{pt}, \quad C=\Sigma A w \epsilon^{pt}; \quad . \quad . \quad . \quad (46)$$

V being the real potential-difference at a place where the corresponding normal potential-difference is u, and C the real current where the normal current is w, the summation extending over all the p roots of the characteristic equation. The size of the systems, settled by the A's (one for each p) are to be found by the conjugate property of the vanishing of the mutual energy-difference of any pair of p systems, applied to the initial distributions of V and C.

17. To find the effect of impressed force is a frequently recurring problem in practical applications; and here the resistance-operator is specially useful, giving a general solution of great simplicity. Thus, suppose we insert a steady impressed force e at a place where the resistance-operator is Z, producing $e=ZC$ thereafter. Find C in terms of e and Z. The following demonstration appears quite comprehensive. Convert the problem into a case of subsidence first, by substituting a condenser of permittance S, and initial charge Se, for the impressed force. By making S infinite later we arrive at the effect of the steady e. In getting the subsidence solution we have only to deal with the energy of the condenser, so that a knowledge of the internal connexions of the system is quite superfluous.

The resistance-operator of the condenser being $(Sp)^{-1}$, that of the combination, when we use the condenser, is Z_1, where

$$Z_1=(Sp)^{-1}+Z. \quad . \quad . \quad . \quad . \quad . \quad (47)$$

Let V and C be the potential-difference and the current, respectively, at time t after insertion of the condenser, and due entirely to its initial charge. Equations (46) above express them, if u and w have the special ratio proper at the condenser, given by

$$w=-\mathrm{S}pu, \qquad \ldots\ldots \qquad (48)$$

because the current equals the rate of decrease of its charge. Initially, we have $e=\Sigma \mathrm{A}u$ and $\Sigma \mathrm{A}w=0$. So, making use of the conjugate property*, we have

$$\mathrm{S}eu=2(\mathrm{U}_p-\mathrm{T}_p)\mathrm{A}, \qquad \ldots\ldots \qquad (49)$$

if U_p be the electric and T_p the magnetic energy in the normal system. But the following property of the resistance-operator is also true *,

$$\mathrm{T}_p-\mathrm{U}_p=\frac{d\mathrm{Z}_1}{dp}w^2\,; \qquad \ldots\ldots \qquad (50)$$

that is, $d\mathrm{Z}_1/dp$ is the impulsive inductance in the p system at a place where the resistance-operator is Z_1, p being a root of $\mathrm{Z}_1=0$; just as $d\mathrm{Z}_1/dp$ with $p=0$ is the impulsive inductance (complete) at the same place. Using (50) in (49) gives

$$\mathrm{A}=-(\mathrm{S}eu)\div\left(w^2\frac{d\mathrm{Z}_1}{dp}\right). \qquad \ldots\ldots \qquad (51)$$

Now use (48) in (51) and insert the resulting A in the second of (46), and there results

$$\mathrm{C}=\Sigma\frac{e}{p\mathrm{Z}_1'}\epsilon^{pt}, \qquad \ldots\ldots \qquad (52)$$

where the accent means differentiation to p. This is the complete subsidence solution. Now increase S infinitely, keeping e constant. Z_1 ultimately becomes Z; but, in doing so, one root of $\mathrm{Z}_1=0$ becomes zero. We have, by (47), and remembering that $\mathrm{Z}_1=0$,

$$p\mathrm{Z}_1'=-(\mathrm{S}p)^{-1}+p\mathrm{Z}'=\mathrm{Z}+p\mathrm{Z}'\,; \qquad \ldots \qquad (53)$$

so, when $\mathrm{S}=\infty$ and $\mathrm{Z}=0$, we have $p\mathrm{Z}_1'=p\mathrm{Z}'$ for all roots except the one just mentioned, in which case p tends to zero and Z' is finite, making in the limit $p\mathrm{Z}_1'=\mathrm{Z}_0$, by (53), where Z_0 is the $p=0$ value of Z, or the steady resistance. Therefore, finally,

$$\mathrm{C}=\frac{e}{\mathrm{Z}_0}+\Sigma\frac{e}{p\mathrm{Z}'}\epsilon^{pt}, \qquad \ldots\ldots \qquad (54)$$

where the summation extends over the roots of $\mathrm{Z}=0$, shows the manner of establishment of the current by the impressed force e. The use of this equation (54), even in comparatively elementary problems, leads to a considerable saving of labour, whilst in cases involving partial differential equations it is invaluable*. To extend it to show the rise of the current at

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* "On the Self-Induction of Wires," Phil. Mag. Oct. 1886.

any other part of the system than where the impressed force is, it is necessary to know the connexions, so that we may know the ratio of the current in a normal system at the new place to that at the old; inserting this ratio in the summation, and modifying the external Z_0 to suit the new place, furnishes the complete solution there. Or, use the more general resistance-operator Z_{xy}, such that $e_x = Z_{xy}C_y$, connecting the impressed force at any place x with the current at another place y.

18. When the initial current is zero, as happens when there is self-induction without permittance at the place of e, and in other cases, (54) gives

$$\frac{1}{Z_0} = \Sigma \frac{1}{-pZ'}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (55)$$

showing that the normal systems may be imagined to be arranged in parallel, the resistance of any one being $(-pZ')$.

To express the impulsive inductance Z_0' in terms of the normal Z's, multiply (54) by e and take the complete time-integral. We obtain

$$\int e\left(C - \frac{e}{Z_0}\right) dt = 2(U - T) = -\Sigma \frac{e^2}{p^2 Z'}, \quad . \quad . \quad (56)$$

remembering (29). Or, using (26),

$$Y_0' = \Sigma \frac{1}{p^2 Z'}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (57)$$

In electrostatic problems the roots of $Z=0$ are real and negative, as is also the case in electromagnetic problems. There are never any oscillatory results in either case, and the vanishing of Z' is then accompanied by vanishing of the corresponding normal functions, to prevent the oscillations which seem on the verge of occurring by the repetition of a root which $Z'=0$ implies. When both energies are present, the real parts of the imaginary roots are always compelled to be negative by the positivity of U, T, and of Q the dissipativity.

When Z is irrational, it is probable that the complete solution corresponding to (54) might be immediately derived from Z. In the case of (41), however, the application is not obvious, although there is no difficulty in passing from the (54) solution to the corresponding definite integrals which arise when the length of the circuit is infinitely increased.

October 15, 1887.

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* In Part III. of "On the Self-Induction of Wires" I employed the Condenser Method, with application to a special kind of combination; but, as we have seen from the above proof, (54) is true for any electrostatic and electromagnetic combination provided it be finite.

LXIII. *On the Division of Space with Minimum Partitional Area.* *By* Sir WILLIAM THOMSON*.

1. THIS problem is solved in foam, and the solution is interestingly seen in the multitude of film-enclosed cells obtained by blowing air through a tube into the middle of a soap-solution in a large open vessel. I have been led to it by endeavours to understand, and to illustrate, Green's theory of "extraneous pressure" which gives, for light traversing a crystal, Fresnel's wave-surface, with Fresnel's supposition (strongly supported as it is by Stokes and Rayleigh) of velocity of propagation dependent, not on the distortion-normal, but on the line of vibration. It has been admirably illustrated, and some elements towards its solution beautifully realized in a manner convenient for study and instruction, by Plateau, in the first volume of his *Statique des Liquides soumis aux seules Forces Moléculaires.*

2. The general mathematical solution, as is well known, is that every interface between cells must have constant curvature† throughout, and that where three or more interfaces meet in a curve or straight line their tangent-planes through any point of the line of meeting intersect at angles such that equal forces in these planes, perpendicular to their line of intersection, balance. The *minimax* problem would allow any number of interfaces to meet in a line; but for a pure minimum it is obvious that not more than three can meet in a line, and that therefore, in the realization by the soap-film, the equilibrium is necessarily unstable if four or more surfaces meet in a line. This theoretical conclusion is amply confirmed by observation, as we see at every intersection of films, whether interfacial in the interior of groups of soap-bubbles, large or small, or at the outer bounding-surface of a group, never more than three films, but, wherever there is intersection, always *just three films*, meeting in a line. The theoretical conclusion as to the angles for stable equilibrium (or pure minimum solution of the mathematical problem) therefore becomes, simply, that every angle of meeting of film-surfaces is exactly 120°.

3. The rhombic dodecahedron is a polyhedron of plane sides between which every angle of meeting is 120°; and space can

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* Communicated by the Author.

† By "curvature" of a surface I mean sum of curvatures in mutually perpendicular normal sections at any point; not Gauss's "curvatura integra," which is the product of the curvature in the two "principal normal sections," or sections of greatest and least curvature. (See Thomson and Tait's 'Natural Philosophy,' part i. §§ 130, 136.)

be filled with (or divided into) equal and similar rhombic dodecahedrons. Hence it might seem that the rhombic dodecahedron is the solution of our problem for the case of all the cells equal in volume, and every part of the boundary of the group either infinitely distant from the place considered, or so adjusted as not to interfere with the homogeneousness of the interior distribution of cells. Certainly the rhombic dodecahedron *is a solution of the minimax, or equilibrium-problem*; and certain it is that no other plane-sided polyhedron can be a solution.

4. But it has seemed to me, on purely theoretical consideration, that the tetrahedral angles of the rhombic dodecahedron*, giving, when space is divided into such figures, twelve plane films meeting in a point (as twelve planes from the twelve edges of a cube meeting in the centre of the cube) are essentially unstable. That it is so is proved experimentally by Plateau (vol. i. § 182, fig. 71) in his well-known beautiful experiment with his cubic skeleton frame dipped in soap-solution and taken out. His fig. 71 is reproduced here in fig. 1. Instead of twelve *plane* films stretched inwards from the twelve edges and meeting in the centre of the cube, it shows twelve films, of which eight are slightly curved and four are plane†, stretched from the twelve edges to a small central plane quadrilateral film with equal curved edges and four angles each of 109° 28′. Each of the plane films is an isosceles triangle with two equal curved sides meeting at a corner of the central curvilinear square in a plane perpendicular to its plane. It is in the plane through an edge and the centre of the cube. The angles of this plane curvilinear triangle are respectively 109° 28′, at the point of meeting of the two curvilinear sides: and each of the two others half of this, or 54° 44′.

5. I find that by blowing gently upon the Plateau cube into any one of the square apertures through which the little central quadrilateral film is seen as a line, this film is caused

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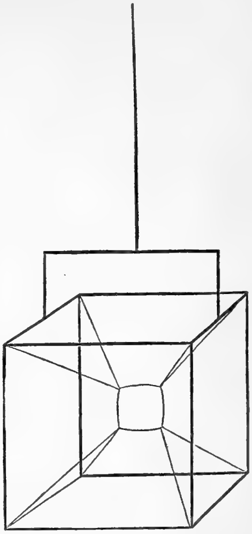
* The rhombic dodecahedron has six tetrahedral angles and eight trihedral angles. At each tetrahedral angle the plane faces cut one another successively at 120°, while each is perpendicular to the one remote from it; and the angle between successive edges is $\cos^{-1}\frac{1}{3}$, or 70° 32′. The obtuse angles (109° 28′) of the rhombs meet in the trihedral angles of the solid figure. The whole figure may be regarded as composed of six square pyramids, each with its alternate slant faces perpendicular to one another, placed on six squares forming the sides of a cube. The long diagonal of each rhombic face thus made up of two sides of pyramids conterminous in the short diagonal, is $\sqrt{2}$ times the short diagonal.

† I see it inadvertently stated by Plateau that all the twelve films are "légèrement courbées."

Fig. 1.



to contract. If I stop blowing before it contracts to a point, it springs back to its primitive size and shape. If I blow still very gently but for a little more time, the quadrilateral contracts to a point, and the twelve films meeting in it immediately draw out a fresh little quadrilateral film similar to the former, but in a plane perpendicular to its plane and to the direction of the blast. Thus, again and again, may the films be transformed so as to render the little central curvilinear square parallel to one or other of the three pairs of square apertures of the cubic frame. Thus we see that the twelve plane films meeting in the centre of the cube is a configuration of unstable equilibrium which may be fallen from in three different ways.

6. Suppose now space to be filled with equal and similar ideal rhombic dodecahedrons. Draw the short diagonal of every rhombic face, and fix a real wire (infinitely thin and perfectly stiff) along each. This fills space with Plateau cubic frames. Fix now, ideally, a very small rigid globe at each of the points of space occupied by tetrahedral angles of the dodecahedrons, and let the faces of the dodecahedrons be realized by soap-films. They will be in *stable* equilibrium, because of the little fixed globes ; and the equilibrium would be stable without the rigid diagonals which we require only to help the imagination in what follows. Let an exceedingly small force, like gravity*, act on all the films everywhere perpendicularly to one set of parallel faces of the cubes. If this force is small enough it will not tear away the films from the globes ; it will only produce a very slight bending from the plane rhombic shape of each film. Now annul the little globes. The films will instantly jump (each set of twelve which meet in a point) into the Plateau configuration (fig. 1), with the little curve-edged square in the plane perpendicular to the determining force, which may now be annulled, as we no longer require it. The rigid edges of the cubes may also be now annulled, as we have done with them also ; because each is (as we see by symmetry) pulled with equal forces in opposite directions, and therefore is not required for the equi-

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* To do for every point of meeting of twelve films what is done by blowing in the experiment of § 5.

librium, and it is clear that the equilibrium is stable without them*.

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* The corresponding two-dimensional problem is much more easily imagined; and may probably be realized by aid of moderately simple appliances.

Between a level surface of soap-solution and a horizontal plate of glass fixed at a centimetre or two above it, imagine vertical film-partitions to be placed along the sides of the squares indicated in the drawing (fig. 2):

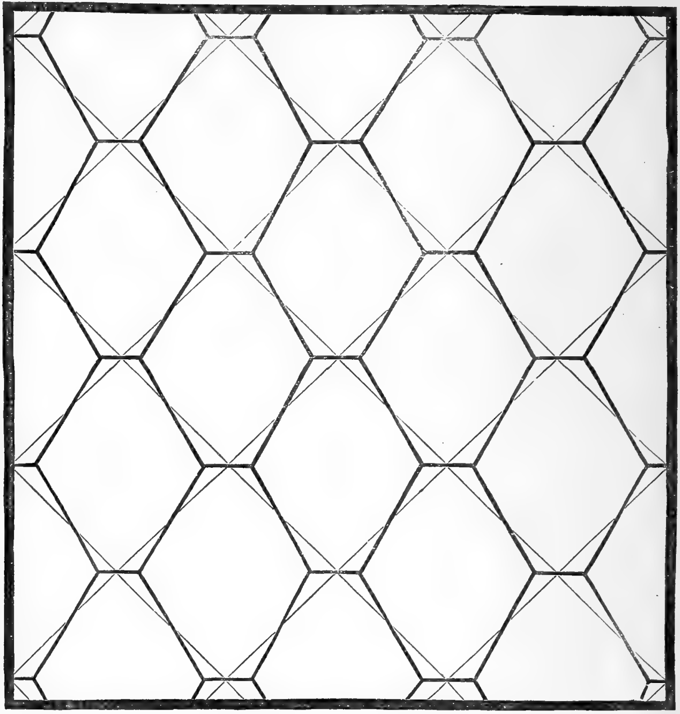

Fig. 2.

these will rest in stable equilibrium if thick enough wires are fixed vertically through the corners of the squares. Now draw away these wires downwards into the liquid: the equilibrium in the square formation becomes unstable, and the films instantly run into the hexagonal formation shown in the diagram; provided the square of glass is provided with vertical walls (for which slips of wood are convenient), as shown in plan by the black border of the diagram. These walls are necessary to maintain the inequality of pull in different directions which the inequality of the sides of the hexagons implies. By inspection of the diagram we see that the pull is T/a per unit area on either of the pair of vertical walls which are perpendicular to the short sides of the hexagons; and on either of the other pair of walls $2 \cos 30° \times T/a$; where T denotes the pull of the film per unit breadth, and a the side of a square in the original formation. Hence the ratio of the pulls per unit of area in the two principal directions is as 1 to 1·732.

7. We have now space divided into equal and similar tetrakaidecahedral cells by the soap-film ; each bounded by

(1) Two small plane quadrilaterals parallel to one another ;

(2) Four large plane quadrilaterals in planes perpendicular to the diagonals of the small ones ;

(3) Eight non-plane hexagons, each with two edges common with the small quadrilaterals, and four edges common with the large quadrilaterals.

The films seen in the Plateau cube show one complete small quadrilateral, four halves of four of the large quadrilaterals, and eight halves of eight of the hexagons, belonging to six contiguous cells ; all mathematically correct in every part (supposing the film and the cube-frame to be infinitely thin). Thus we see all the elements required for an exact construction of the complete tetrakaidecahedron. By making a clay model of what we actually see, we have only to complete a symmetrical figure by symmetrically completing each half-quadrilateral and each half-hexagon, and putting the twelve properly together, with the complete small quadrilateral, and another like it as the far side of the 14-faced figure. We thus have a correct solid model.

8. Consider now a cubic portion of space containing a large number of such cells, and of course a large but a comparatively small number of partial cells next the boundary. Wherever the boundary is cut by film, fix stiff wire ; and remove all the film from outside, leaving the cubic space divided stably into cells by films held out against their tension by the wire network thus fixed in the faces of the cube. If the cube is chosen with its six faces parallel to the three pairs of quadrilateral films, it is clear that the resultant of the whole pull of film on each face will be perpendicular to the face, and that the resultant pulls on the two pairs of faces parallel to pairs of the greater quadrilaterals are equal to one another and less than the resultant pull on the pair of faces parallel to the smaller quadrilaterals. Let now the last-mentioned pair of faces of the cube be allowed to yield to the pull inwards, while the other two pairs are dragged outwards against the pulls on them, so as to keep the enclosed volume unchanged ; and let the wirework fixture on the faces be properly altered, shrunk on two pairs of faces, and extended on the other pair of faces, of the cube, which now becomes a square cage with distance between floor and ceiling less than the side of the square. Let the exact configuration of the wire everywhere be always so adjusted that the cells throughout the interior remain, in their altered configuration, equal and similar to one another. We may thus diminish, and if we

please annul, the difference of pull per unit area on the three pairs of sides of the cage. The respective shrinkage-ratio and extension-ratio, to exactly equalize the pulls per unit area on the three principal planes, (and therefore on all planes), are $2^{-\frac{1}{3}}$, $2^{\frac{1}{6}}$, $2^{\frac{1}{6}}$, as is easily seen from what follows.

9. While the equalization of pulls in the three principal directions is thus produced, work is done by the film on the moving wire-work of the cage, and the total area of film is diminished by an amount equal to W/T, if W denote the whole work done, and T the pull of the film per unit breadth. The change of shape of the cage being supposed to be performed infinitely slowly, so that the film is always in equilibrium throughout, the total area is at each instant a minimum, subject to the conditions

(1) That the volume of each cell is the given amount;
(2) That every part of the wire has area edged by it; and
(3) That no portion of area has any free edge.

10. Consider now the figure of the cell (still of course a tetrakaidecahedron) when the pulls in the three principal directions are equalized, as described in § 8. It must be perfectly isotropic in respect to these three directions. Hence the pair of small quadrilaterals must have become enlarged to equality with the two pairs of large ones, which must have become smaller in the deformational process described in § 8. Of each hexagon three edges coincide with edges of quadrilateral faces of one cell; and each of the three others coincides with edges of three of the quadrilaterals of one of the contiguous cells. Hence the 36 edges of the isotropic tetrakaidecahedron are equal and similar plane arcs; each of course symmetrical about its middle point. Every angle of meeting of edges is essentially 109° 28′ (to make trihedral angles between tangent planes of the films meeting at 120°). Symmetry shows that the quadrilaterals are still plane figures; and therefore, as each angle of each of them is 109° 28′, the change of direction from end to end of each arc-edge is 19° 28′. Hence each would be simply a circular arc of 19° 28′, if its curvature were equal throughout; and it seems from the complete mathematical investigation of §§ 16, 17, 18 below, that it is nearly so, but not exactly so even to a first approximation.

Of the three films which meet in each edge, in three adjacent cells, one is quadrilateral and two are hexagonal.

11. By symmetry we see that there are three straight lines in each (non-plane) hexagonal film, being its three long diagonals; and that these three lines, and therefore the six angular points of the hexagon, are all in one plane. The arcs

composing its edges are not in this plane, but in planes making, as we shall see (§ 12), angles of 54° 44′ with it. For three edges of each hexagon, the planes of the arcs bisect the angle of 109° 28′ between the planes of the six corners of contiguous hexagons; and for the other three edges are inclined on the outside of its plane of corners, at angles equal to the supplements of the angles of 125° 16′ between its plane of corners and the planes of contiguous quadrilaterals.

12. The planes of corners of the eight hexagons constitute the faces of an octahedron which we see, by symmetry, must be a regular octahedron (eight equilateral triangles in planes inclined 109° 28′ at every common edge). Hence these planes, and the planes of the six quadrilaterals, constitute a plane-faced tetrakaidecahedron obtained by truncating the six corners[*] of a regular octahedron each to such a depth as to reduce its eight original (equilateral triangular) faces to equilateral equiangular hexagons. An orthogonal projection of this figure is shown in fig. 3. It is to be remarked that space can be filled with such figures. For brevity we shall call it a plane-faced isotropic tetrakaidecahedron.

13. Given a model of the plane-faced isotropic tetrakaidecahedron, it is easy to construct approximately a model of the *minimal tetrakaidecahedron*, thus:—Place on each of the six square faces a thin plane disk having the proper curved arcs of 19° 28′ for its edges. Draw the three long diagonals of each hexagonal face. Fill up by little pieces of wood, properly cut, the three sectors of 60° from the centre to the overhanging edges of the adjacent quadrilaterals. Hollow out symmetrically the other three sectors, and the thing is done. The result is shown in orthogonal projection, so far as the edges are concerned, in fig. 4; and as the orthogonal projections are equal and similar on three planes at right angles to one another,

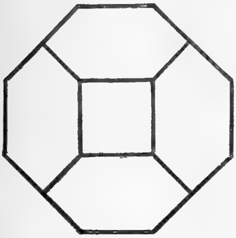

Fig. 3.

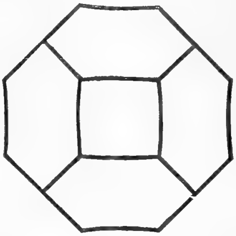

Fig. 4.

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[*] This figure (but with probably indefinite extents of the truncation) is given in books on mineralogy as representing a natural crystal of red oxide of copper.

this diagram suffices to allow a perspective drawing from any point of view to be made by "descriptive geometry."

14. No shading could show satisfactorily the delicate curvature of the hexagonal faces, though it may be fairly well seen on the solid model made as described in § 12. But it is shown beautifully, and illustrated in great perfection, by making a skeleton model of 36 wire arcs for the 36 edges of the complete figure, and dipping it in soap solution to fill the faces with film, which is easily done for all the faces but one. The curvature of the hexagonal film on the two sides of the plane of its six long diagonals is beautifully shown by reflected light. I have made these 36 arcs by cutting two circles, 6 inches diameter, of stiff wire, each into 18 parts of 20° (near enough to 19° 28′). It is easy to put them together in proper positions and solder the corners, by aid of simple devices for holding the ends of the three arcs together in proper positions during the soldering. The circular curvature of the arcs is not mathematically correct, but the error due to it is, no doubt, hardly perceptible to the eye.

15. But the true form of the curved edges of the quadrilateral plane films, and of the non-plane sufaces of the hexagonal films, may be shown with mathematical exactness by taking, instead of Plateau's skeleton cube, a skeleton square cage with four parallel edges each 4 centimetres long: and the other eight, constituting the edges of two squares each

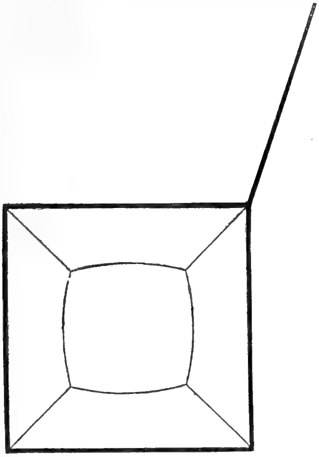

Fig. 5.

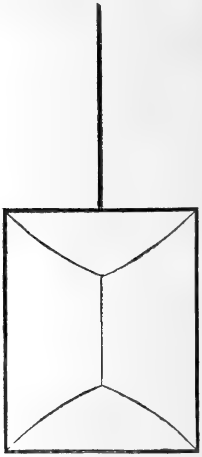

Fig. 6.

$\sqrt{2}$ times as long, or 5·66 centim. Dipped in soap-solution and taken out it always unambiguously gives the central quadrilateral in the plane perpendicular to the four short edges. It shows with mathematical accuracy (if we suppose the wire edges infinitely thin) a complete quadrilateral, four half-quadrilaterals, and four half-hexagons of the minimal tetrakaidecahedron. The two principal views are represented in figs. 5 and 6.

16. The mathematical problem of calculating the forms of the plane arc-edges, and of the curved surface of the hexagonal faces, is easily carried out to any degree of approximation that may be desired ; though it would be very laborious, and not worth the trouble, to do so further than a first approximation, as given in § 17 below. But first let us state the rigorous mathematical problem ; which by symmetry becomes narrowed to the consideration of a 60° sector B C B′ of our non-plane hexagon, bounded by straight lines C B, C B′, and a slightly curved edge B E B′, in a plane, Q, through B B′, inclined to the plane B C B′ at an angle of $\tan^{-1}\sqrt{2}$, or 54° 44′. The plane of the curved edge I call Q, because it is the plane of the contiguous quadrilateral. The mathematical problem to be solved is *to find the surface of zero curvature edged by* B C B′ *and cutting at* 120° *the plane* Q *all along the intersectional curve* (fig. 7). It is obvious that this problem is

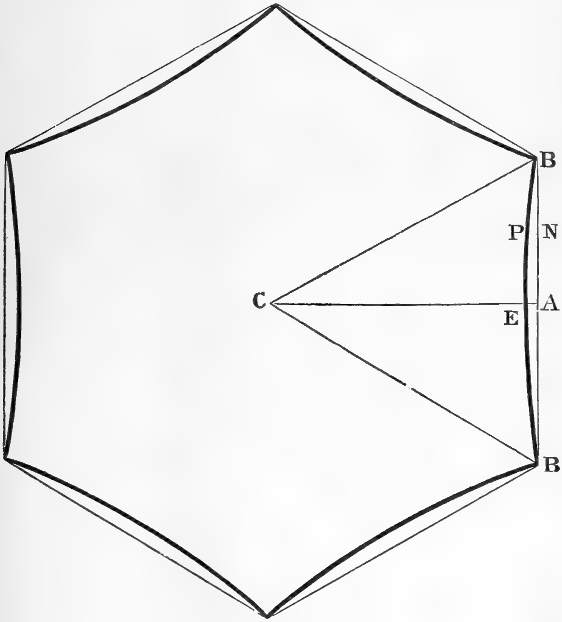

Fig. 7.

determinate and has only one solution. Taking C A for axis of x; and z perpendicular to the plane B C B′: and regarding z as a function of x, y, to be determined for finding the form of the surface, we have, as the analytical expression of the conditions

$$\frac{d^2z}{dx^2}\left(1+\frac{dz^2}{dy^2}\right)-2\frac{dz}{dx}\frac{dz}{dy}\frac{d^2z}{dx\,dy}+\frac{d^2z}{dy^2}\left(1+\frac{dz^2}{dx^2}\right)=0 \quad . \quad (1);$$

and

$$\left.\begin{array}{l}\left(1+\dfrac{dz^2}{dx^2}+\dfrac{dz^2}{dy^2}\right)^{-\frac{1}{2}}\left(\sqrt{\tfrac{1}{3}}-\dfrac{dz}{dx}\sqrt{\tfrac{2}{3}}\right)=\tfrac{1}{2}\\ \qquad\qquad\qquad\qquad \text{when } z=(a-x)\sqrt{2}\end{array}\right\} \quad (2).$$

17. The required surface deviates so little from the plane B C B′ that we get a good approximation to its shape by neglecting dz^2/dx^2, $dz/dx\,.\,dz/dy$, and dz^2/dy^2, in (1) and (2), which thus become

$$\nabla^2 z=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3),$$

and

$$\frac{dz}{dx}=\frac{\sqrt{2}}{2}-\sqrt{\tfrac{3}{8}}=\cdot094735,\ \text{when } x=a-z/\sqrt{2}, \quad . \quad (4),$$

∇^2 denoting $(d/dx)^2+(d/dy)^2$. The general solution of (3), in polar coordinates (r, ϕ) for the plane (x, y), is

$$\Sigma(\mathrm{A}\cos m\phi+\mathrm{B}\sin m\phi)r^m, \quad . \quad . \quad . \quad . \quad . \quad (5),$$

where A, B, and m are arbitrary constants. The symmetry of our problem requires $\mathrm{B}=0$, and $m=3\,.\,(2i+1)$, where i is any integer. We shall not take more than two terms. It seems not probable that advantage could be gained by taking more than two, unless we also fall back on the rigorous equations (1) and (2), keeping dz^2/dx^2 &c. in the account, which would require each coefficient A to be not rigorously constant but a function of r. At all events we satisfy ourselves with the approximation yielded by two terms, and assume

$$z=\mathrm{A}r^3\cos 3\phi+\mathrm{A}'r^9\cos 9\phi \quad . \quad . \quad . \quad . \quad (6);$$

with two coefficients A, A′ to be determined so as to satisfy (4) for two points of the curved edge, which, for simplicity, we shall take as its middle, E ($\phi=0$); and end, B ($\phi=30°$). Now remark that, as z is small, even at E, where it is greatest, we have, in (4), $x\doteqdot a$ or $r\doteqdot a\sec\phi$. Thus, and substituting for dz/dx its expression in polar (r, ϕ) coordinates, which is

$$\frac{dz}{dx}=\frac{dz}{dr}\cos\phi-\frac{dz}{rd\phi}\sin\phi \quad . \quad . \quad . \quad . \quad (7),$$

we find, from (4) with (6),

$$\text{(by case } \phi=0\text{)} \quad \mathrm{A}+3a^6\mathrm{A}'=\cdot 031578a^{-2} \quad . \quad . \quad . \quad (8),$$

$$\text{(and by case } \phi=30^\circ\text{)} \quad \mathrm{A}-\tfrac{64}{9}a^6\mathrm{A}'=\cdot 031578\,.\,\tfrac{3}{2}\,.\,a^{-2} \quad . \quad (9);$$

whence

$$\begin{aligned}\mathrm{A}' &= -\tfrac{1}{2}\,.\,\tfrac{9}{91}\times\cdot 031578\,.\,a^{-8}=-9\times\cdot 0001735\,.\,a^{-8}\\ &= -\cdot 001561\,.\,a^{-8},\\ \mathrm{A} &= \tfrac{1}{2}(3-\tfrac{64}{91})\times\cdot 031578\,.\,a^{-2}=209\times\cdot 0001735\,.\,a^{-2}\\ &= \cdot 036261\,.\,a^{-}\ ;\end{aligned}$$

and for required equation of the surface we have (taking $a=1$ for brevity)

$$\left.\begin{aligned} z &= \cdot 03626\,.\,r^3\cos 3\phi-\cdot 001561\,r^9\cos 9\phi\\ &= \cdot 03626\,.\,r^3(\cos 3\phi-\cdot 043\,.\,r^6\cos 9\phi)\end{aligned}\right\} \quad . \quad . \quad (10).$$

18. To find the equation of the curved edge BEB′, take, as in (4),

$$x=1-z\sqrt{}2=1-\xi,\text{ where } \xi \text{ denotes } z\sqrt{}2 \quad . \quad . \quad (11).$$

Substituting in this, for z, its value by (10), with for r its approximate value sec ϕ, we find

$$\xi=\frac{1}{\sqrt{}2}(\cdot 03626\sec^3\phi\cos 3\phi-\cdot 001561\sec^9\phi\cos 9\phi)\,. \quad (12)$$

as the equation of the orthogonal projection of the edge, on the plane BCB′, with

$$\mathrm{AN}=y=\tan\phi;\quad\text{and}\quad \mathrm{NP}=\xi \quad . \quad . \quad . \quad (13).$$

The diagram was drawn to represent this projection roughly, as a circular arc, the projection on BCB′ of the circular arc of 20° in the plane Q, which, before making the mathematical investigation, I had taken as the form of the arc-edges of the plane quadrilaterals. This would give 1/35 of CA, for the sagitta, AE; which we now see is somewhat too great. The equation (12), with $y=0$, gives for the sagitta

$$\mathrm{AE}=\cdot 0245\times\mathrm{CA} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14),$$

or, say, 1/41 of CA. The curvature of the projection at any point is to be found by expressing $\sec^3\phi\cos 3\phi$ and $\sec^9\phi\cos 9\phi$ in terms of $y=\tan\phi$ and taking d^2/dy^2 of the result.

By taking $\sqrt{}(3/2)$ instead of $\sqrt{}(1/2)$ in (12), we have the equation of the arc itself in the plane Q.

19. To judge of the accuracy of our approximation, let us find the greatest inclination of the surface to the plane BCB′. For the tangent of the inclination at (r,ϕ) we have

$$\left(\frac{dz^2}{dr^2}+\frac{dz^2}{r^2\,d\phi^2}\right)^{\frac{1}{2}}=\cdot 1088\,.\,r^2(1-2\times\cdot 129\,.\,r^6\sec 6\phi+\cdot 129^2r^{12})^{\frac{1}{2}} \quad (15).$$

The greatest values of this will be found at the curved bounding edge, for which $r \doteq \sec\phi$. Thus we find

$$\left.\begin{array}{l} \left(1+\dfrac{dz^2}{dr^2}+\dfrac{1}{r^2}\dfrac{dz^2}{d\phi^2}\right)^{\frac{1}{2}} \\ \left\{\begin{array}{l} =\cdot0948, \text{ and therefore inclination} = 5^\circ\, 25' \text{ at E} \\ =\cdot1892, \quad ,, \quad ,, \quad ,, \quad =10^\circ\, 43' \text{ at B} \end{array}\right\} \end{array}\right\} \quad (16).$$

Hence we see that the inaccuracy due to neglecting the square of the tangent of the inclination in the mathematical work cannot be large. The exact value of the inclination at E is $\tan^{-1}(-\sqrt{2})-120^\circ$, or 5° 16′, which is less by 9′ than its value by (16).

LXIV. *On the Application of the Deci-ampere or the Centi-ampere Balance to the Determination of the Electromotive Forces of Voltaic Cells. By* Sir William Thomson, *F.R.S.*[*]

THE method described in this paper for the determination, in absolute measure, of the electromotive forces of voltaic cells, consists in the use of one of my standard ampere-balances instead of the tangent-galvanometer in the method given in the following statement, which I quote from Kohlrausch's 'Physical Measurements,' pp. 223, 224, 230:—"The only methods applicable to inconstant elements, of which the electromotive force varies with the current-strength, is to bring the current to zero by opposing an equal electromotive force. Poggendorff's method, which is very convenient, as it involves no measurement of internal resistance, requires the use of a galvanoscope, G, a galvanometer, T, and a rheostat, R, and, in addition, that of an auxiliary battery, S, of constant electromotive force, greater than either of those which are to be compared. The arrangement of the experiment is shown in the figure. In the left division of the circuit are the galvanoscope G, and the electromotive force E to be measured; in the right, the auxiliary battery S and the galvanometer T. E and S are so placed that their similar poles are turned towards each other. In the middle part of the circuit, which is common to both batteries, is the rheostat R.

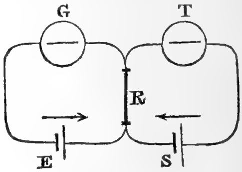

Fig. 1.

"As much rheostat resistance W must now be intercalated

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[*] Communicated by the Author, being a paper read at the Manchester Meeting of the British Association, with additions.

as will cause the current in E G to vanish, and the current-strength J in T must then be observed. . . . . If J = current-strength in T, the electromotive force of the battery E is E=WJ."

The deci-ampere balance, or, when a sufficient number of battery-cells is available, the centi-ampere balance, answers well for the current measurements here required. An arrangement of the circuit which is convenient for most purposes is shown in the diagram (fig. 2); but it may be remarked that the reversing-keys there shown may be replaced by ordinary make-break keys. Referring to the diagram, a battery of a sufficient number of cells is joined in circuit through a reversing-key with a rheostat, a deci-ampere balance, and a standard resistance. The poles of the cell to be tested are connected in circuit with a key and a sensitive mirror-galvanometer to the two ends of the standard resistance in such a way that both the battery and the cell to be tested tend to send a current in the same direction through that resistance. Care should be taken that the circuit of the cell to be tested is well insulated, and that both it and the standard resistance are free from

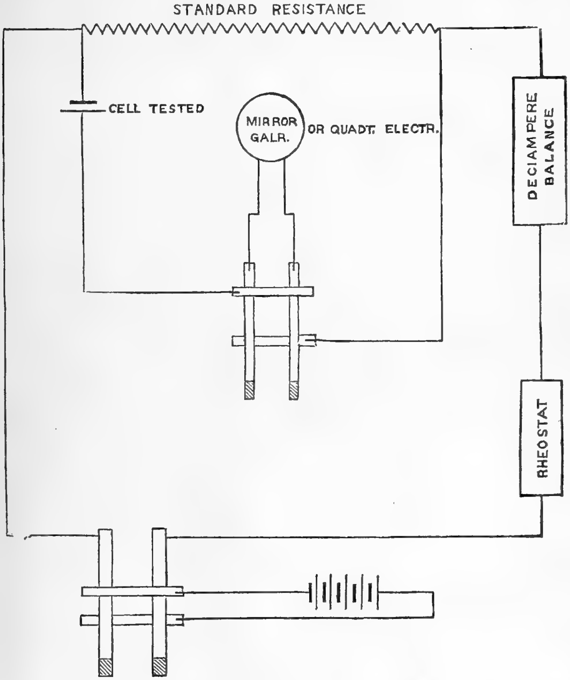

Fig. 2.

other electromotive force. When, as in the case of Clark-standard cells, the cell is incapable of maintaining a current, a high-resistance galvanometer or an additional resistance should be included in its circuit. If very great sensibility is not required, a quadrant-electrometer may in such cases be substituted for the galvanometer. The standard resistance must be of such a form that no sensible error is introduced through heating by the passage of the current. A good plan is to wind well-insulated platinoid wire in one layer on the outside of a brass or copper cylindrical vessel which can be filled with water. The temperature of the water, when it is nearly the same as that of the air outside, will be very approximately the temperature of the coil. For still greater accuracy the cylinder may be fitted with a jacket and immersed in a vessel of water, and appliances introduced for changing the water in each part and keeping account of its temperature. For use with the deci-ampere balance, a platinoid resistance of two ohms is sufficient for any single cell. A resistance of two ohms, made of insulated platinoid wire one millimetre in diameter, and wound on a brass tube capable of holding half a litre of water and simply exposed to the air outside, will carry a current of one ampere for an hour without changing its resistance more than a tenth per cent. The water should be stirred when the readings are taken, and, if necessary, the change of resistance can be approximately allowed for by taking its temperature. To measure by means of the deci-ampere balance an electromotive force of from one to two volts, a battery of two small secondary cells or four Daniell cells, a resistance of two ohms such as has just been described, with the other appliances as indicated in the figure, are all that is necessary.

The method has been applied in my laboratory by Mr. Thomas Gray for the measurement of the electromotive forces of standard and other cells, and has been found very convenient. The results obtained for four Clark-standard cells set up by Mr. J. T. Bottomley in March last were almost identical with one another, and gave 1·439 Rayleigh, or 1·442 legal, volts at 11° C. The variation of the electromotive force of these cells with temperature has not yet been determined; but assuming the average value obtained for this variation by Lord Rayleigh, namely a fall of ·077 per cent. per degree centigrade rise of temperature, and correcting to 15° C., we obtain slightly under 1·436 Rayleigh volts at that temperature. This result is interesting as showing a difference of less than $\frac{1}{15}$th per cent. from that obtained by Lord Rayleigh for similar cells, which was 1·435 at 15° C.

LXV. *Notices respecting New Books.*

A Treatise on the Integral Calculus.—Part I. *Containing an elementary account of Elliptic Integrals and applications to Plane Curves, with numerous Examples. By* RALPH A. ROBERTS, *M.A.* Dublin: Hodges, 1887; pp. viii+368.

MR. ROBERTS, who has written a number of papers on subjects intimately connected with the Calculus, has now embodied some of these results, with the necessary elementary details, in the present treatise. He has not seen fit to introduce his work to his readers with any forewords, so we are left to infer that he considers such a work is required to meet a want not met by existing treatises. The contents, so far, do not differ materially from those of Dr. Williamson's treatise, but the treatment is different, and the illustrations and examples are very full and important. The main difference is in the amount of space (Chapter v.) devoted to the discussion of Elliptic Integrals; here our author is specially strong. We have noticed but few typographical or other errors, and as the whole discussion is admirably conducted throughout, we need only append the headings of the chapters. These are:—Elementary Integrals, Integration of Rational Functions, Integration by Rationalization, Integration by Successive Reduction, Elliptic Integrals, Definite Integrals, Areas of Plane Curves, and Rectification of Plane Curves. From the titlepage we gather that at least another volume is to follow. The book is of handy form and clearly printed.

LXVI. *Intelligence and Miscellaneous Articles.*

INFLUENCE OF MAGNETISM ON THE THERMOELECTRIC BEHAVIOUR OF BISMUTH. BY DR. GIOVANNI PIETRO GRIMALDI.

THE remarkable influence which magnetism exerts on the electrical resistance of bismuth led me to suppose that it would not be uninteresting to investigate whether the thermoelectric behaviour of bismuth in the magnetic field was different to what it is usually.

In this preliminary note I give the results hitherto obtained. The experiment was made as follows:—A cylinder of bismuth, 5 centim. in length and 1 centim. in diameter, with copper wires soldered at the ends, was placed equatorially between the poles of a Faraday's electromagnet. The solderings were immersed in two baths of such a form that they could be close to the poles of the magnet. One of the baths was full of melting snow, and the other of water at the ordinary temperature, which flowed from a large reservoir.

The bismuth-copper couple was introduced into a circuit of copper wire of about 2 millim. in diameter, which remained constant. In this circuit two copper-wire couples of about the same E.M.F. as the couple under experiment were arranged in opposition. The circuit also contained a Wiedemann's galvanometer (Edelmann's large pattern) astaticized as completely as possible, and whose coils were of greatest sensitiveness for thermoelectric currents. The deflections were read with a telescope on a scale a metre in length divided in millimetres, and arranged at a distance of about two metres from the mirror of the galvanometer.

The current due to the difference between the electromotive force of the bismuth-copper couple and the compensating-couple was so weak as to produce a deflection of a few centimetres only in the scale of the telescope; but either of the batteries would have sent the spot of light off the scale.

The galvanometer was at a distance of about twenty metres from the electromagnet.

By means of two keys, one in the thermoelectric circuit A and one in the magnetizing current B, arranged near the telescope, either of the two circuits could be opened or closed.

Opening first the circuit A, I investigated the influence of the electromagnet on the galvanometer, which, notwithstanding the distance of the two apparatus, was sufficiently apparent, and then tried the position at which the least action was exerted. It was thus found that the deflection of the needle when the electromagnet was excited was only two millim. It remained constant for the entire duration of the experiment. This was made as follows.

The galvanometer being at zero, the circuit A was first closed; then the circuit B was opened, and the deflection of the galvanometer read off (first reading). Then A was opened to avoid induction-currents, which, notwithstanding the most minute precautions, were sufficiently apparent; then B was closed, and immediately afterwards A, to make the second reading; then A being opened, and afterwards B, A was again closed, and the third reading made. Lastly, it was ascertained if the needle returned to zero of the scale, those experiments being rejected in which this was not the case. The difference between the second reading and the mean of the first and third, which generally were but little different, corrected for the above-mentioned influence of the electromagnet on the galvanometer, measured the intensity of the phenomenon I investigated. Many measurements were lost owing to the action on the galvanometer of carriages passing in the neighbouring street, an influence which made itself more felt when the galvanometer was arranged in a room of the laboratory, than when it was on the first floor in the courtyard.

With a magnetizing current of twelve Bunsen's elements, freshly filled, the following result was obtained:—The thermoelectric force *of bismuth with respect to copper was considerably weakened by magnetism*; the difference between the second reading and the mean of the first and third, which, as mentioned above, measured the intensity of the said diminution, amounted ultimately to 45 millim. of the scale.

In order to be certain that the phenomenon observed was solely due to the influence in question, and was not caused by any disturbing influences, I made the following experiment.

Adjusting both the solderings of the bismuth-copper couple to zero, so as to annul the electromotive force, I left the bismuth cylinder in the same position as before between the poles of the electromagnet, and then weakened the compensating-pile so as to have in the circuit a current of about the same intensity as in the first experiment. Experimenting then as in the first case, I found that the magnetism exerted no influence on the intensity of the thermoelectric current.

In these preliminary experiments I made no measurements which would enable me to give definite values of the variation found. In order to furnish an approximate idea, I may mention that in the conditions in which I experimented the diminution of the thermoelectric electromotive force was about $\frac{1}{30}$. The variations in question are perhaps of the same order of magnitude as those found by Righi for the electrical resistance.

In a more complete investigation of the phenomenon, which I hope presently to carry out, I shall examine the relation between the intensity of the magnetic field and the thermoelectric power of pure and commercial bismuth in various states of aggregation, and of the alloys of bismuth.

I propose to make an analogous investigation on antimony.—*Rendiconti della R. Accademia dei Lincei*, February 1887.

ON THE ACTION OF LIGHT ON A MIXTURE OF HYDROGEN AND CHLORINE. BY E. PRINGSHEIM.

The author sums up as follows the results of his investigation:—

(1) The proof attempted by Bunsen and Roscoe that in the photochemical combination of chlorine and hydrogen, an amount of work is done by light for which an equivalent quantity of light disappears does not hold. The proof for that assertion lies in the fact that the quantity of the hydrochloric acid formed is proportional to the intensity of the light.

(2) The first apparent action of light on chlorine and hydrogen consists in a sudden increase of volume of the gaseous mixture, which as suddenly disappears; the magnitude of this increase is proportional to the intensity of the acting light, but is independent of the condition of the induction and of the quantity of hydrochloric acid formed.

(3) This sudden increase of volume is produced by a sudden dissociation of the molecules, which goes on at the moment of chemical change.

(4) In this, no hydrochloric acid is formed in the first case, but an intermediate substance.

(5) The slow formation of hydrochloric acid by light takes place solely with moist chlorine and hydrogen; dry gas is not affected by light which is not very intense.

(6) Under the influence of strong sources of light or great heat, dry hydrogen and chlorine gases explode as well as moist.

(7) The intermediate product which the photochemical indication produces, and the assumption of which completely explains this phenomenon, arises very probably from decomposition of aqueous vapour.—Wiedemann's *Annalen*, November 1887.

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Fig. 1.

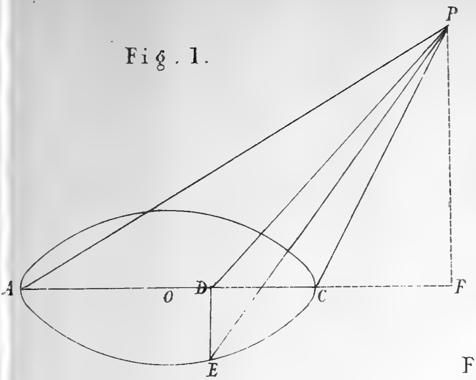
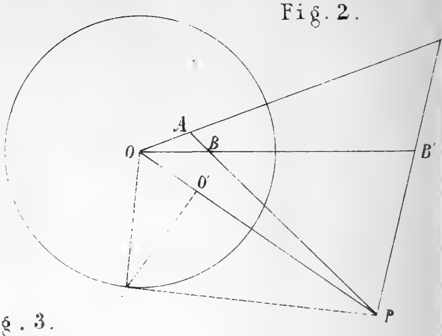


Fig. 2.



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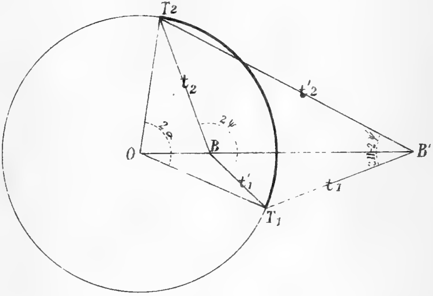

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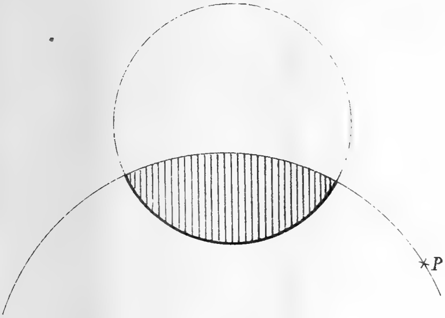

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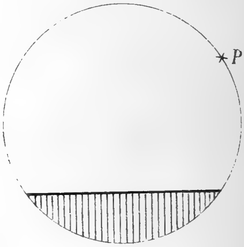

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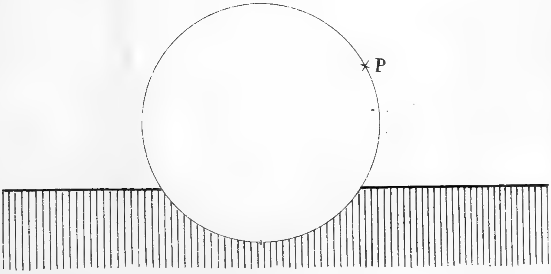

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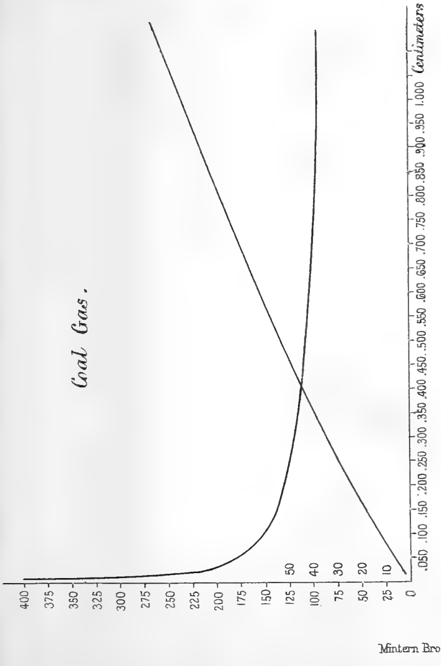

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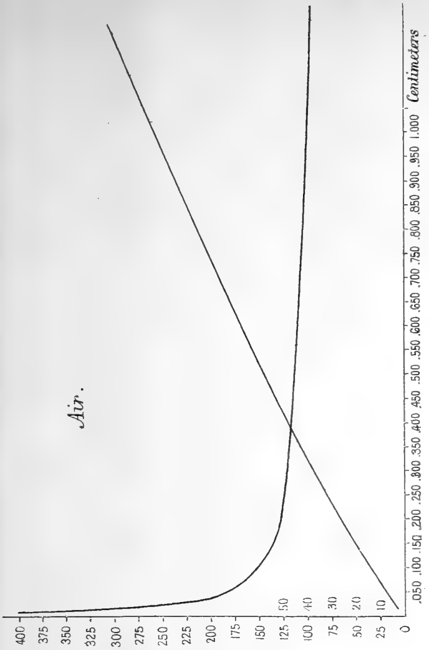

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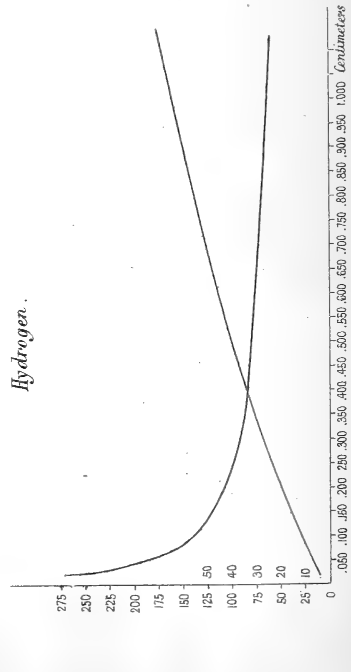

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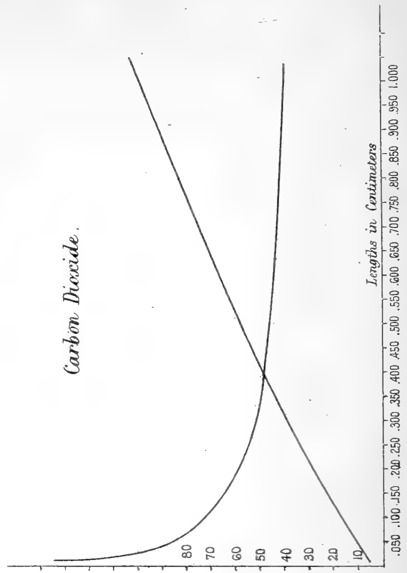

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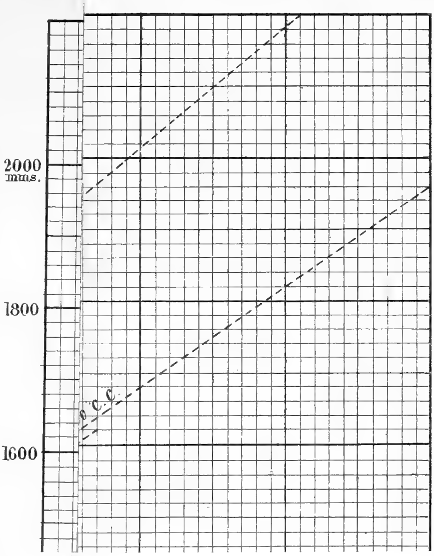
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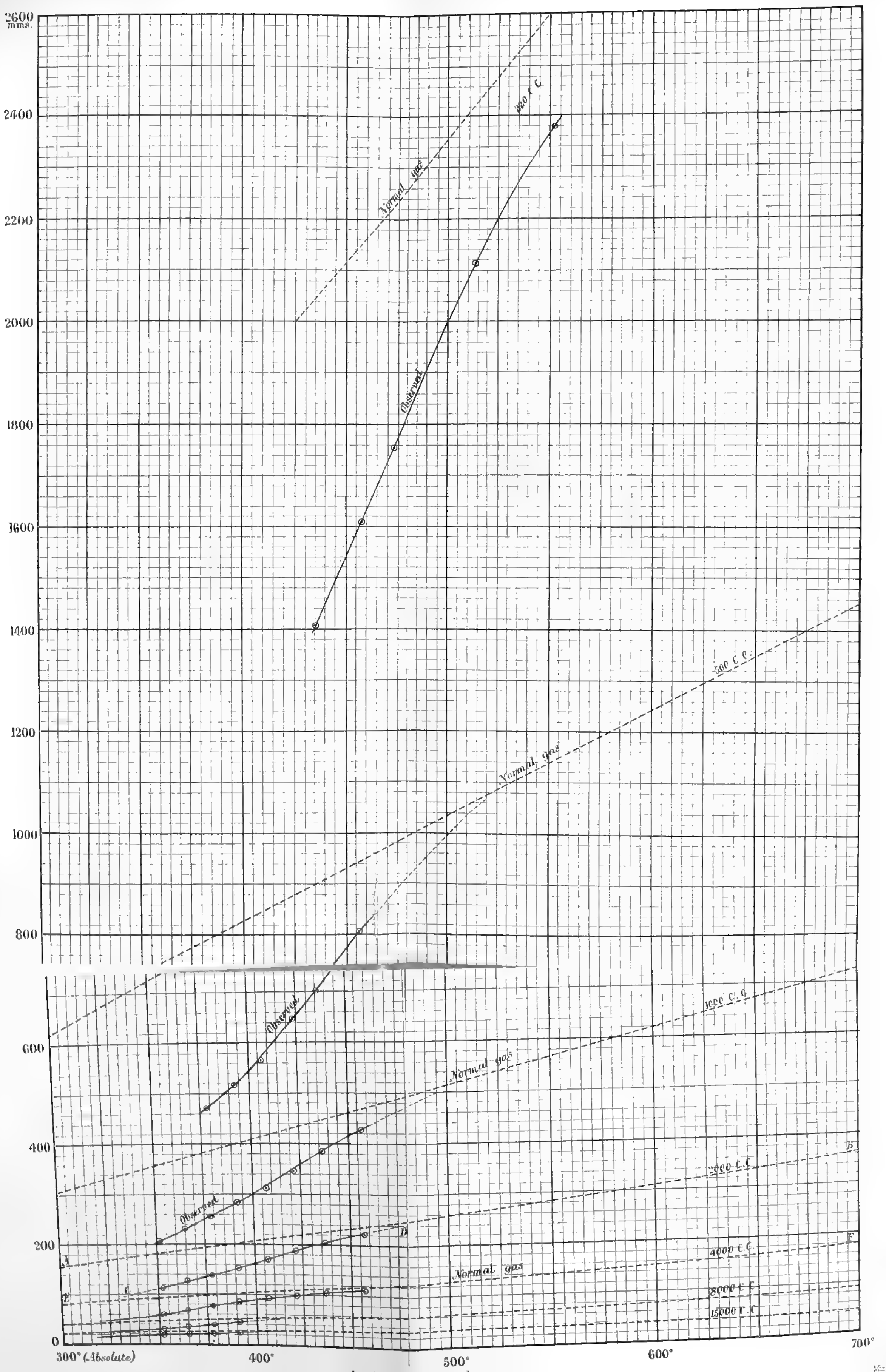

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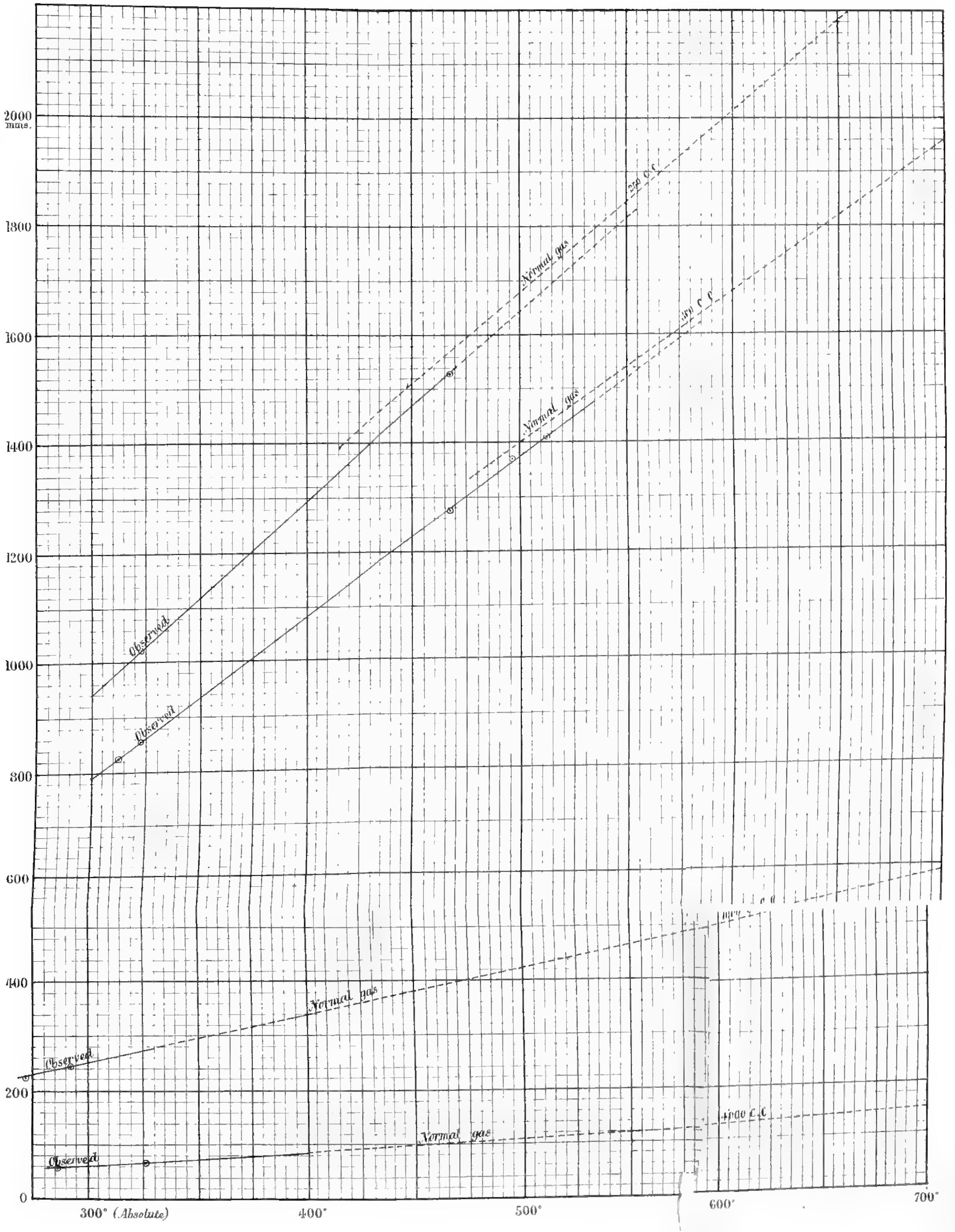

Ether Isochors.

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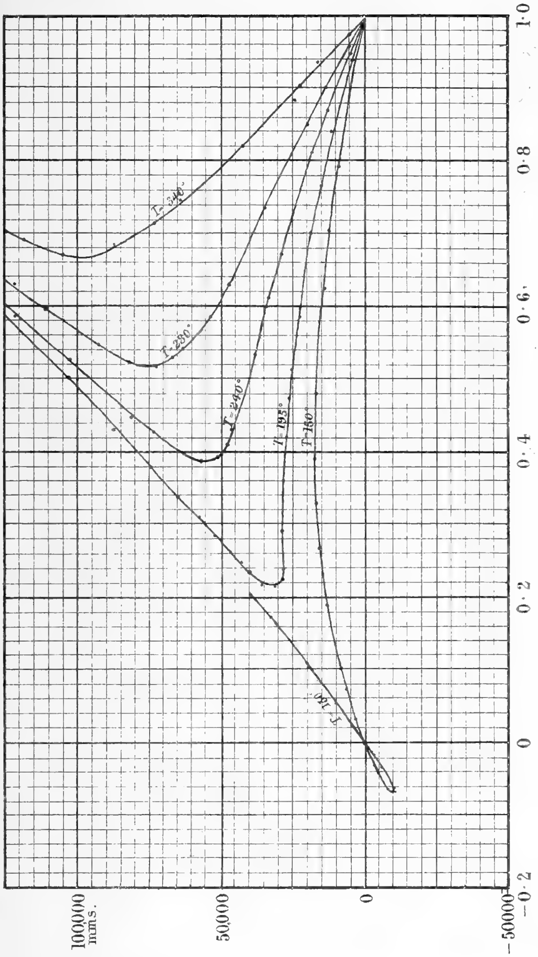
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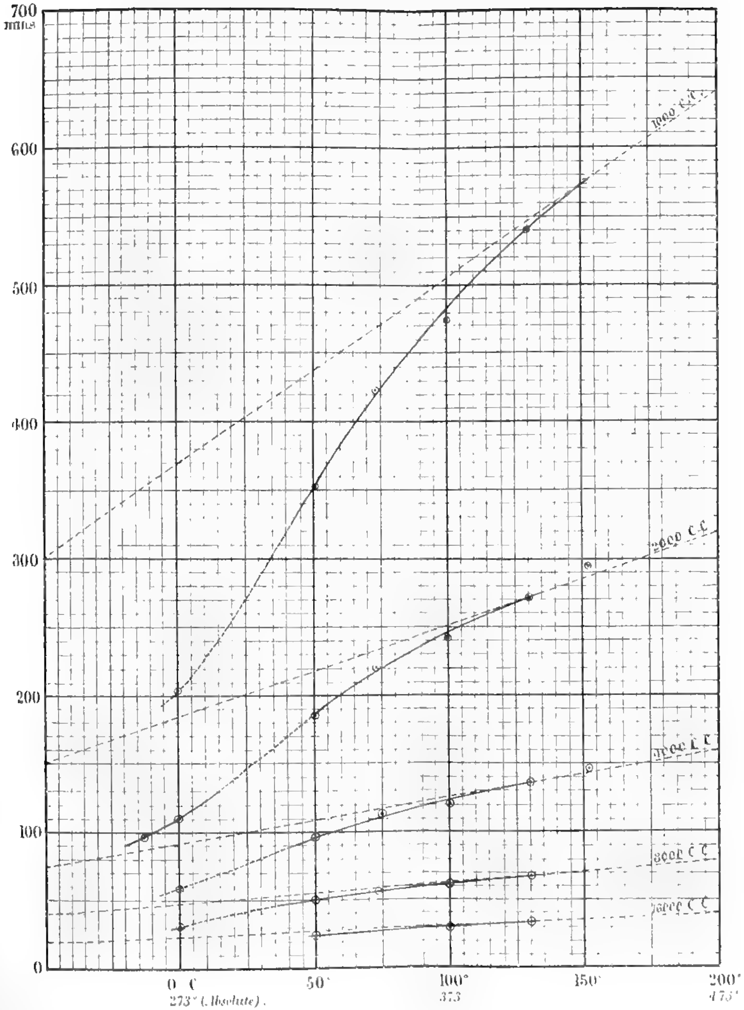

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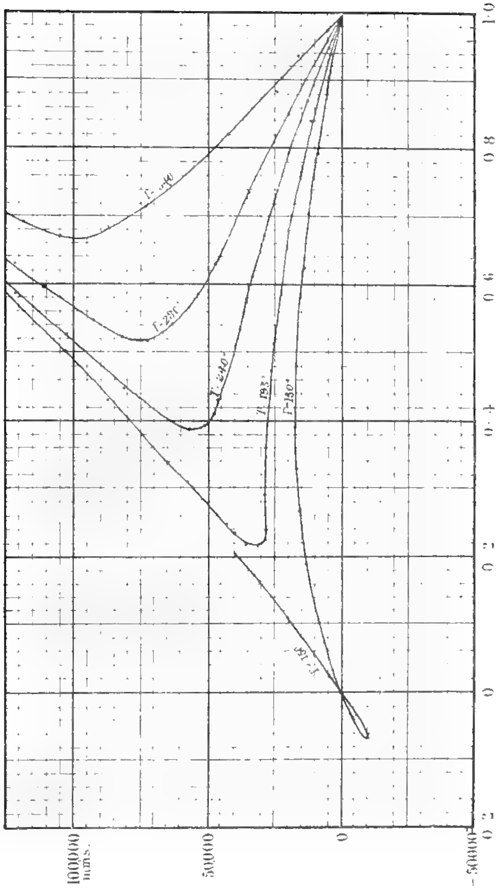

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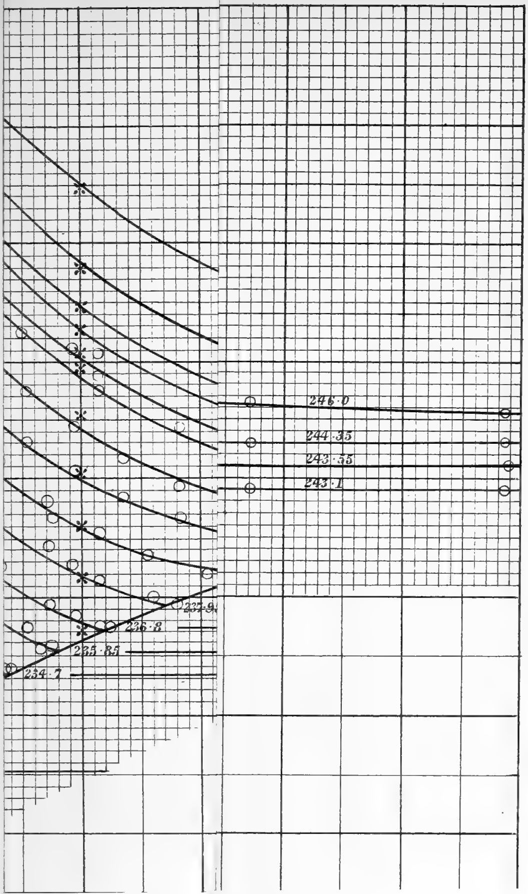
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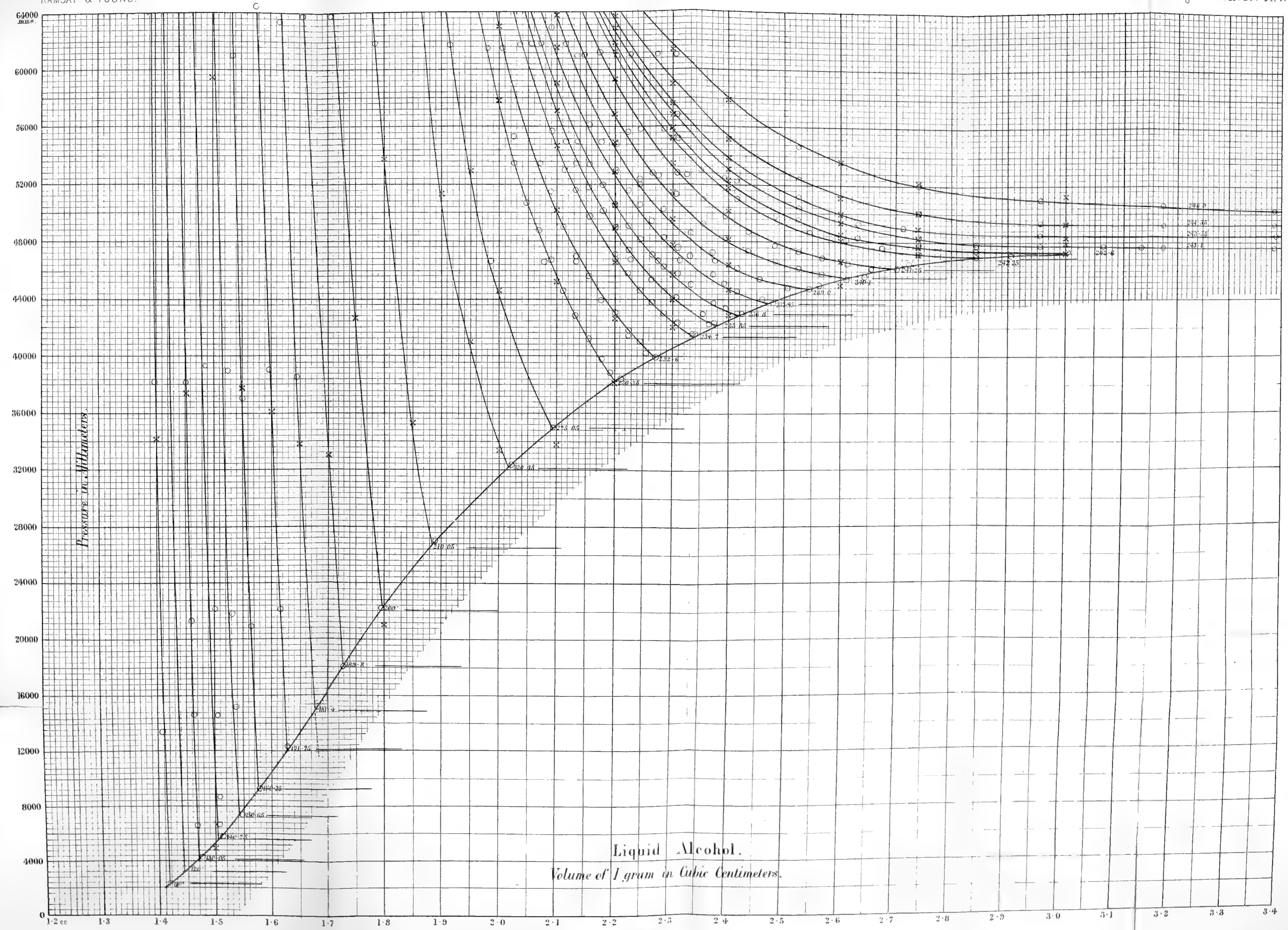
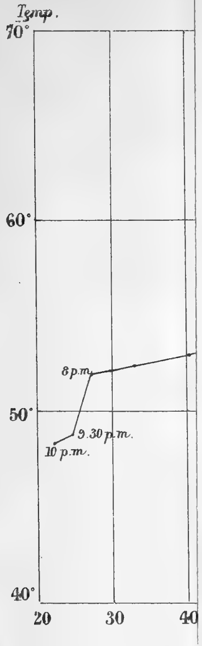


Liquid Alcohol.

Volume of 1 gram in Cubic Centimeters.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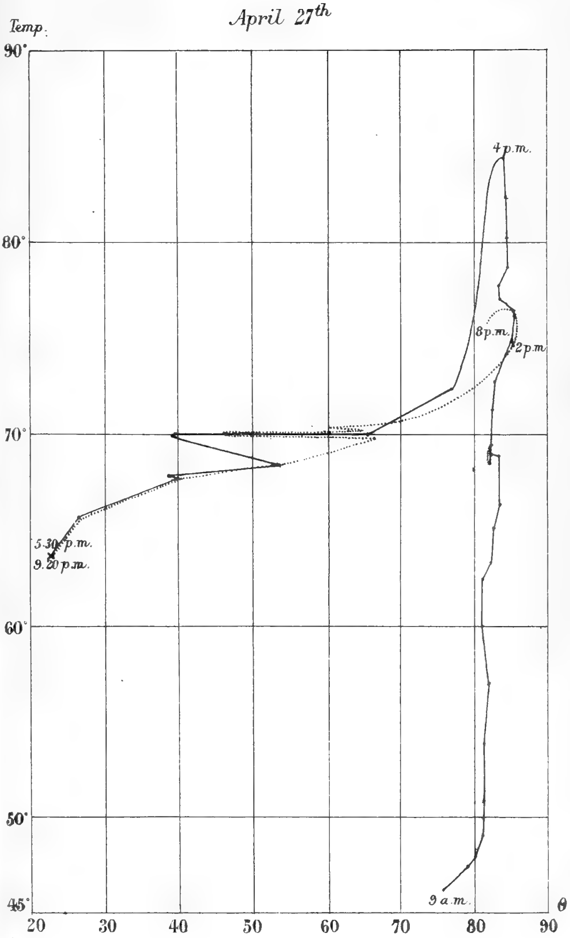
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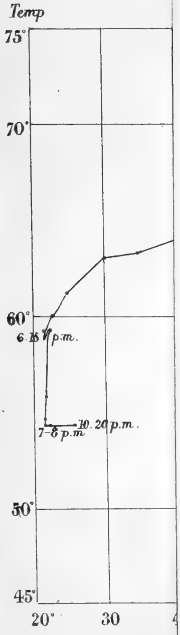
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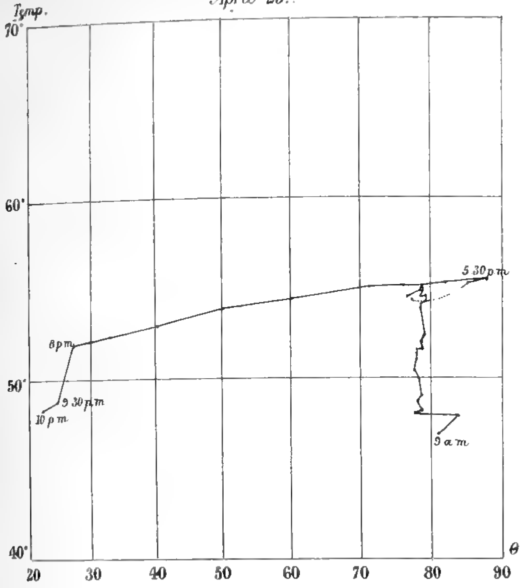

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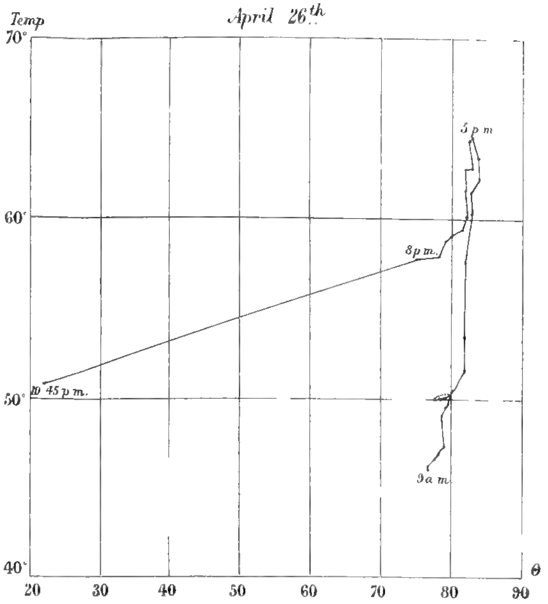

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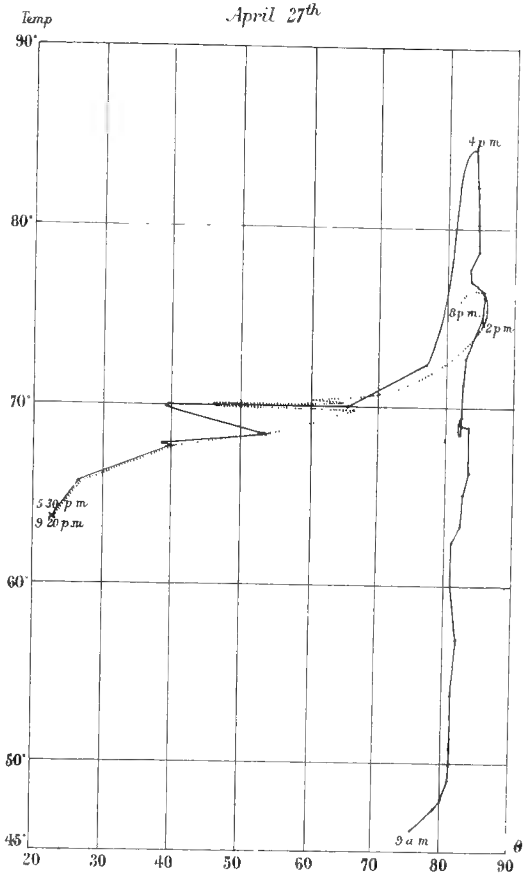

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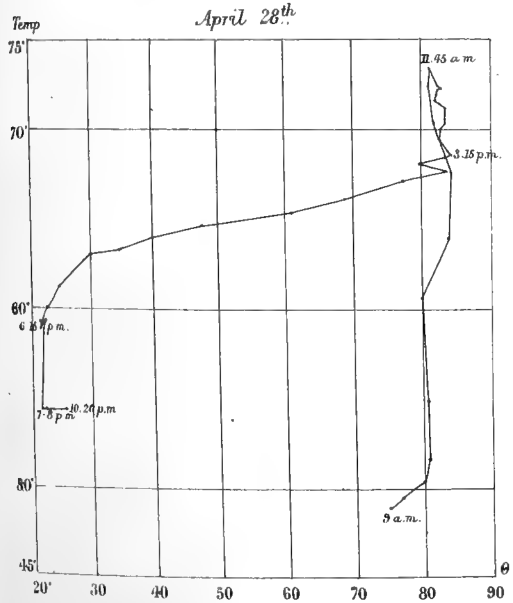

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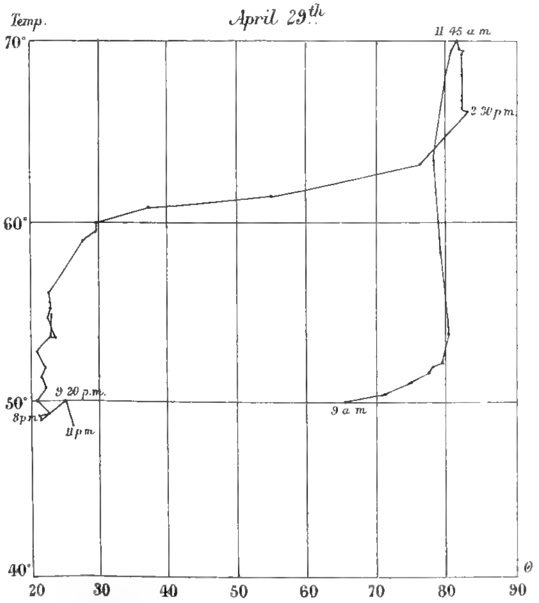

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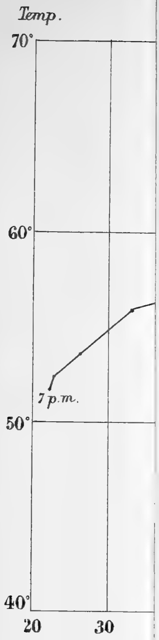
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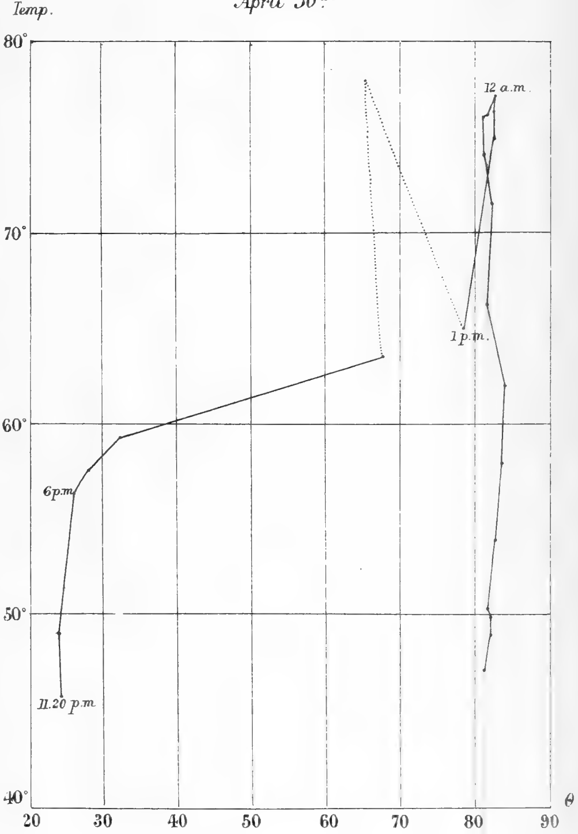
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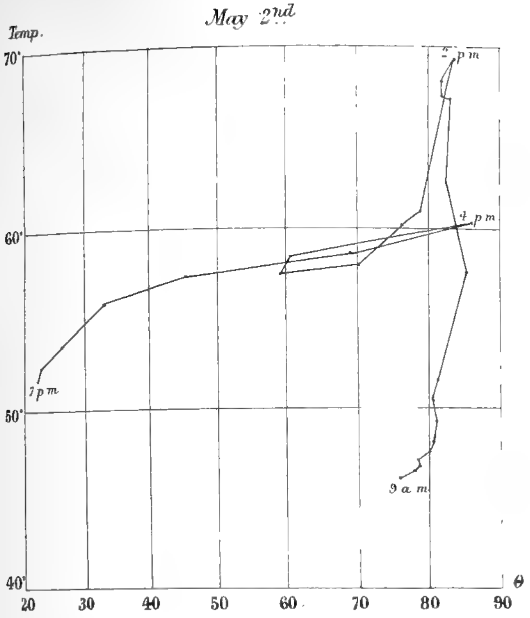
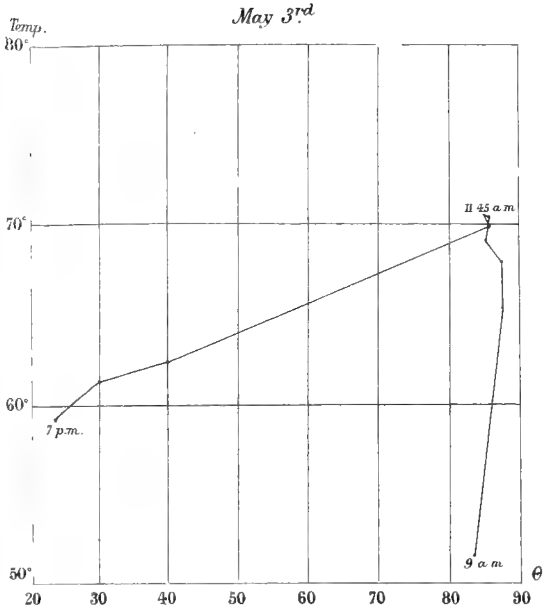


May 2nd



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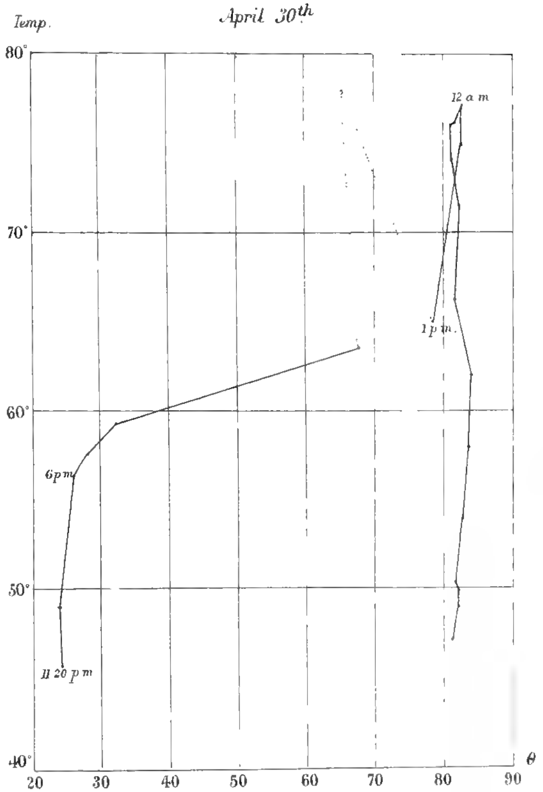

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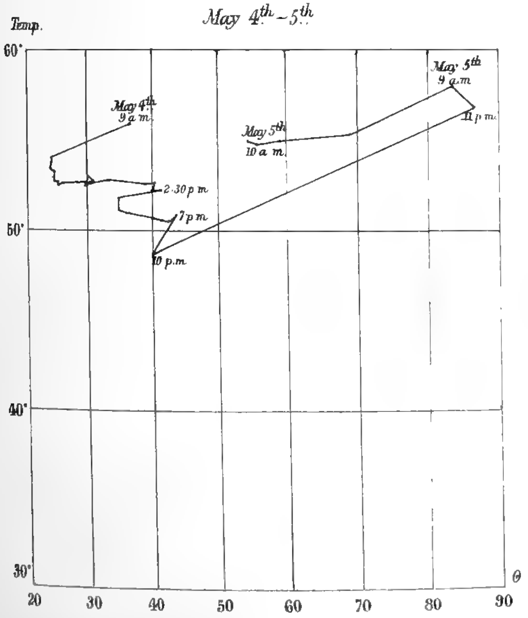

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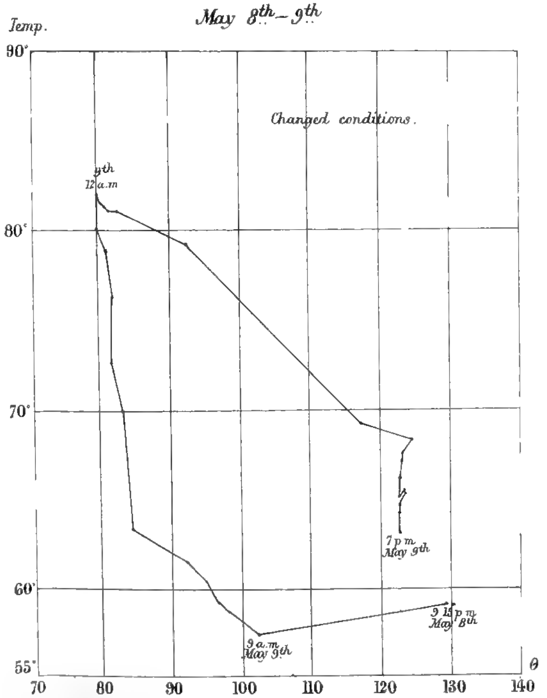

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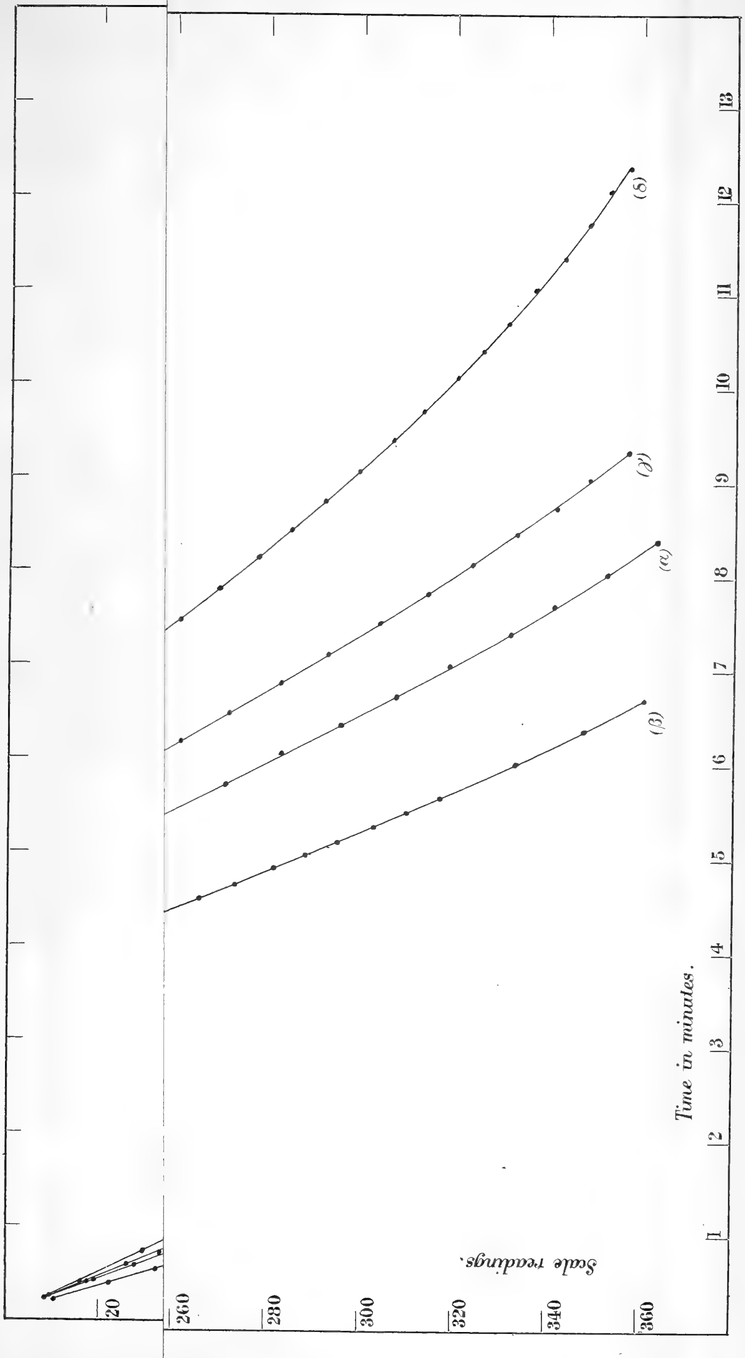

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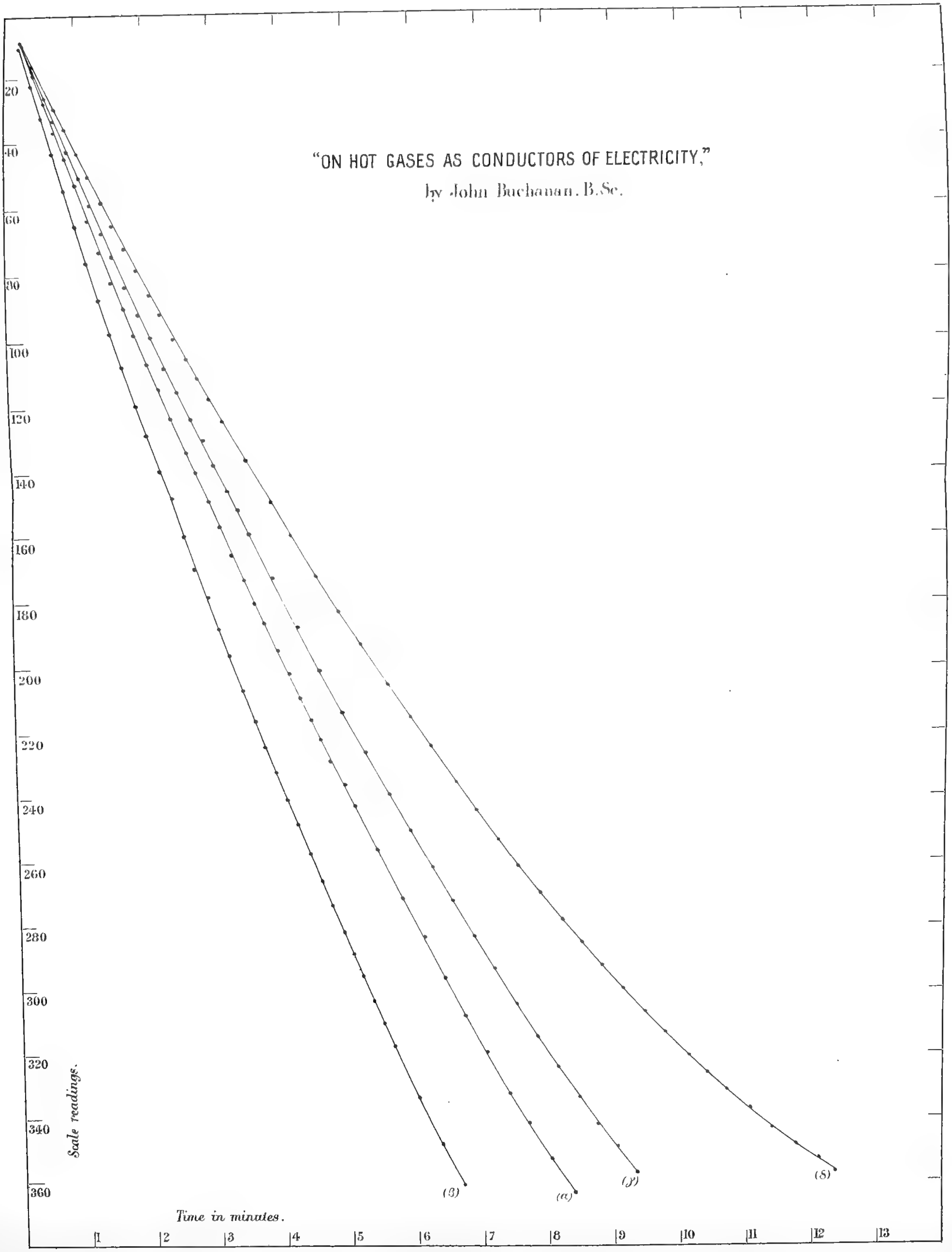

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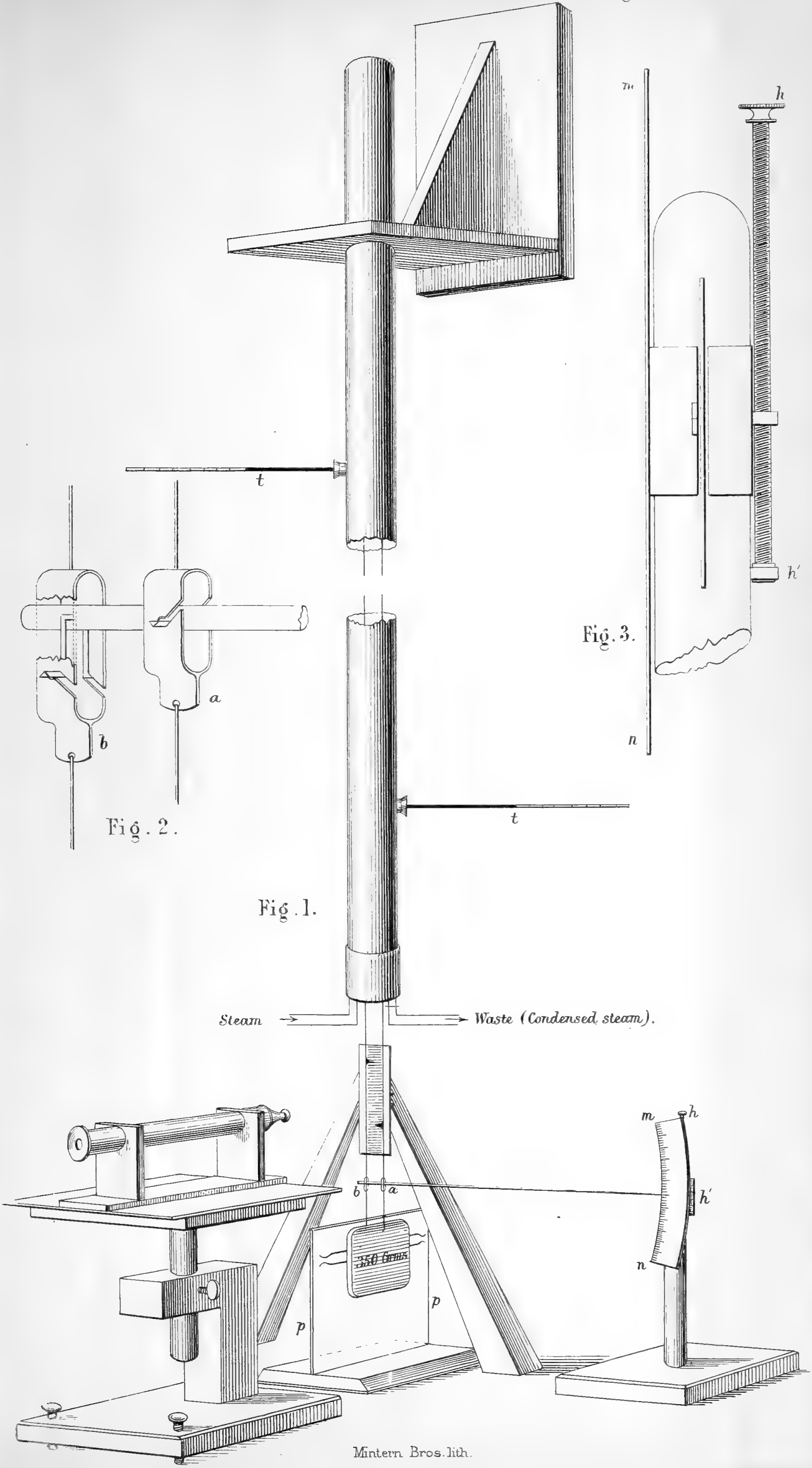

Fig. 3.

Fig. 2.

Fig. 1.

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$$\frac{1}{2\sqrt{(m^2+q^2)}}\Big[\epsilon^{y\sqrt{(m^2+q^2)}}\int_0^y dy\epsilon^{-y\sqrt{(m^2+q^2)}}\big(\mathrm{L}f(y)+\mathrm{MF}(y)\big)$$
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