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AND

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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.

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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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ERRATA.

Page 147, lines 28, 29, *for* and, on account of the disturbances, neglected, *read* and on account of disturbances neglected,

— 152, at foot, *for* $I=\frac{Wr}{L\gamma}g$ read $I=\frac{Wr}{Lb\gamma}g$

— 156, *instead of the equations*

$$q = AI = \&c.$$
$$q' = AI' = \&c.$$

read

$$q = \frac{AI}{R} = \&c.$$
$$q' = \frac{AI'}{R} = \&c.,$$

where R is the total resistance in circuit;

— 310, line 1, *for* friction *read* fiction

— 390, line 33, *for* a velocity g *read* an acceleration g

PLATES.

THE

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PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FIFTH SERIES.]

JULY 1883.

I. *On the Constant of Dielectricity and the Double Refraction of Insulating Fluids.* By G. Quincke*.

According to the views of Faraday†, we must assume that in every point of an insulating fluid which is electrified like the glass of a Leyden jar there is a pulling force in the direction of the lines of force, and a pushing force perpendicular to those lines. According to Maxwell‡, these pushing forces are given by the equation

$$p=\frac{K_1}{8\pi}\cdot\frac{P^2}{a^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

if P denotes the difference of electric potentials, a the distance between the plane metallic faces, or electrodes, between which the fluid is situated. K_1 would have for the forces parallel and perpendicular to the lines of electric force the same value and would be equal to the dielectricity-constant K, or the number which represents the increase of the capacity of a condenser when the plates are separated from one another by another insulating substance instead of air. According to the most recent theoretic investigations by Helmholtz§, the ratio between the amounts of the pressures and tensions per-

* Translated from the *Sitzungsberichte der Kön. Preuss. Akad. der Wissensch. Berlin*, 1883, April 5, pp. 413–420.

† Experimental Researches, §§ 1224 & 1297.

‡ Electricity and Magnetism, i. §§ 111 & 124.

§ Berlin *Monatsber.* xvii. 2, 1881.

pendicular and parallel to the lines of electric force may vary according to the nature of the insulating substance.

I have measured, for a series of insulating fluids, the value of K and K_1 for the pulling and pushing forces parallel and perpendicular to the lines of electric force. I will denote K_1 by K_p or K_s, according to whether the action of the forces is parallel or perpendicular to the lines of electric force.

I employed in those measurements an electric balance with plane horizontal condenser-plates of nickel-coated brass 8·530 centim. in diameter and distant 0·1597 centim., which stood in a larger glass vessel filled with air or the insulating fluid. The lower condenser-plate was connected with the inner coating of a large Leyden battery of eight flint-glass jars, the outer coating of which, as well as the upper plate of the condenser, was conducted to earth.

The plates of the condenser and the coatings of the battery were charged by a Holtz machine, and were maintained at a constant difference of potentials. The latter could be controlled by a reflecting electrometer described by M. Righi*, or measured by a screw-electrometer (long-range electrometer) of Sir William Thomson†.

A comparison of the indications of the screw-electrometer with the measurements in an electric balance in air, with condenser-plates of 85 millim. diameter and at from 1 to 2 millim. distance, showed that to one turn of the screw of the electrometer (of which 0·001 revolution could be determined by estimation) corresponded an alteration of the electric potential amounting to 1·1415 C.G.S., or the electromotive force of a Daniell battery of 305 cells.

With the screw-electrometer I determined the difference of electric potentials at which the condenser-plates in air attracted each other with a force G_I of 20 or 10 grammes.

I next sought to ascertain the weights which, with equal difference of electrical potentials, exactly counterbalanced the electric attraction of the condenser-plates (parallel to the lines of electric force) when the plates were entirely surrounded by the insulating fluid instead of air. These weights, G_{II}, varied between 21 and 100 grammes, and could be determined with precision to 0·01 grm. by a sensitive balance. The ratio of the weights G_{II} and G_I found for the fluid and air, with equal electric potential-difference, gives K_p.

When the condenser-plates of the electric balance were charged up to the same difference of electric potentials, and

* *Memorie dell' Accademia delle Scienze dell' Istituto di Bologna*, [3] vii. 2, p. 193 (1876).

† Papers on Electrostatics and Magnetism, p. 306, pl. ii. fig. 15.

discharged while in the insulating fluid or in air by a sensitive multiplier, the ratio of the deflections of the multiplier gave the ratio of the capacities of the condenser, or the dielectricity-constant K.

Finally the upper condenser-plate was removed, and replaced by a plate of equal dimensions with a short vertical metal tube in the centre, which was connected with a sulphide-of-carbon manometer and a long tube of caoutchouc furnished with a cock.

While both plates of the condenser were conducted to earth, dry air was blown through the caoutchouc tube into the space between the condenser-plates, and a flattened air-bubble produced, touching the plates in two equal surfaces of from 2 to 5 centim. diameter. Then, with the cock closed, the separating fluid showed in the two legs of the manometer a difference of heights that was constantly less than 1 centim. and depended on the hydrostatic and capillary pressure of the fluid upon the air-bubble.

If the two plates were now brought to the difference of electric potentials for which the weights G_I and G_{II} had been determined, the electrical transverse pressure in the interior of the fluid counteracted the electrical transverse pressure in the interior of the air-bubble, and the difference of heights of the fluid in the manometer received an increment h. This increase was independent of the diameter of the air-bubble, and proportional to the square of the electric potential-difference. It varied, with the different fluids, between 0·15 and 1 centim.

If σ denotes the specific gravity of the separating fluid, the difference of the electric transverse pressures upon unit surface in the interior of the fluid and of the air-bubble is

$$h\sigma = \frac{K_s - 1}{8\pi} \cdot \frac{P^2}{a^2}. \quad \ldots \ldots \quad (2)$$

For the same potential-difference P and the same distance a of the plates of the condenser from the surface O, the pulling force parallel to the lines of electric force in the interior of the fluid, for the unit of surface, had been found,

$$\frac{G_{II}}{O} = \frac{K_p}{8\pi} \cdot \frac{P^2}{a^2}, \quad \ldots \ldots \quad (3)$$

or, by division of both equations,

$$K_s = \frac{h\sigma \cdot O}{G_{II}} \cdot K_p \quad \ldots \ldots \quad (4)$$

This determines K_s, since K_p has been ascertained by means of the electric balance.

Special experiments showed that the amount of the surface-tension of the insulating fluids did not change during prolonged action of electric forces.

With the same fluid and at the same temperature the refraction-index for Fraunhofer's line D was measured by means of a Steinheil hollow prism and an Oertling circle, permitting readings of 2″.

According to Maxwell*, for light-waves of infinite length the constant K would be equal to the square of the index of refraction. This relation is not confirmed by my observations collected in the opposite Table. On the contrary, K_p is nearly equal to K_s, and constantly greater than K. Colza oil forms the only exception.

All the fluids were as pure and free from dust and contained as little water as possible, and insulated excellently. As, however, the observations took some time, a portion of the fluid evaporated, the temperature fell, and on opening the balance-case some water condensed on the surface of the fluid, through which the numbers found for the dielectricity-constants were too high. This source of error was especially great in the case of ether. Four successive determinations with ether which had stood for a long time over calcined marble, and was filtered as quickly as possible through a fan filter into the glass vessel of the electric balance, gave

$$K_p = 4{\cdot}399 \quad 4{\cdot}516 \quad 4{\cdot}739 \quad 4{\cdot}891.$$

The numbers in the Table therefore refer to ether which had already absorbed a trace of water.

In order to investigate the alteration of the optical properties of fluids by electric forces, I employed condensers with plane electrodes, or with electrodes consisting of two concentric cylinders. The electrodes or coatings, of nickel-covered brass, were insulated by flint-glass rods from one another, and from the wider glass tubes closed by plane-parallel glass plates which contained the fluid as pure and free from dust as possible. The wires, insulated by flint glass, which connected the electrodes with the earth or with the inner coating of a large battery of eight Leyden jars of flint glass, passed through two side tubes. The coatings of the fluid condenser and the Leyden battery were charged, as in the experiments with the electric balance, by a Holtz machine, and maintained at a constant difference of potentials, which was controlled by the

* Electricity and Magnetism, ii. § 788.

No.	Fluid.	Specific gravity	at 0° C.	Refraction-index, n_D.	Temperature.	Condenser-capacity, K.	Dielectricity-constant, determined by: Longitudinal pressure, K_p.	Dielectricity-constant, determined by: Transverse pressure, K_s.
1.	Ether	1·7205	14·90	1·3605	6°·60	3·364	4·851	4·672
2.	Ether (which had stood over calcined marble)	,,	,,	1·3594	8·37	3·323	4·623	4·660
3.	5 vol. ether + 1 vol. sulphide of carbon	0·8134	16·40	1·4044	8·50	2·871	4·136	4·392
4.	1 vol. ether + 1 vol. ,, ,, ...	0·9966	16·60	1·4955	10·50	2·458	3·539	3·392
5.	1 vol. ether + 3 vol. ,, ,, ...	1·1360	17·40	1·5677	5·30	2·396	3·132	3·061
6.	Sulphur in sulphide of carbon (19·5 p. c.)	1·3623	12·60	1·6797	8·68	2·113	2·870	2·895
7.	Sulphide of carbon (Kahlbaum).........................	1·2760	12·20	1·6386	7·50	2·217	2·669	2·743
8.	,, ,, (Heidelberg)	1·2796	10·20	1·6342	12·98	1·970	2·692	2·752
9.	1 vol. sulphide of carbon + 1 vol. oil of turpentine...	1·0620	17·80	1·5442	10·92	1·962	2·453	2·540
10.	Heavy benzol (from coal-tar)..............................	0·8825	15·91	1·5035	13·20	1·928	2·389	2·370
11.	Pure benzol (from benzoic acid)..........................	0·8822	17·64	1·5050	14·40	2·050	2·325	2·375
12.	Light benzol	0·7994	17·20	1·4535	11·60	1·775	2·155	2·172
13.	Colza oil	0·9159	16·40	1·4743	16·41	2·443	2·385	3·296
14.	Oil of turpentine	0·8645	17·10	1·4695	16·71	1·940	2·259	2·356
15.	Petroleum	6·8028	17·00	1·4483	16·62	1·705	2·138	2·149

reflection electrometer and measured by the screw-electrometer.

These fluid condensers, alone or with a Babinet compensator (employed by me in previous investigations*), placed between Nicol prisms, permitted the electric double refraction found by Mr. Kerr† to be measured.

If we name d the difference of rate, measured in wave-lengths, by which the light polarized parallel is accelerated with respect to the light polarized perpendicular to the electric lines of force, P the electric difference of potential (in the C.G.S. system) of the plane electrodes, l their length, and a the distance between them, then

$$d = \mathrm{B} \cdot \frac{l}{100} \cdot \frac{\mathrm{P}^2}{a^2} \text{ nearly.} \quad . \quad . \quad . \quad . \quad . \quad (5)$$

The quantity B diminishes as the wave-length increases, and is in general found to be smaller in proportion as the length of the electrized liquid is greater. Slight mixtures of foreign substances, even water condensed from the air, have a considerable influence on B.

As the means of a rather large number of series of experiments with plane electrodes of nickel of 10, 20, 46 centim. length and at from 0·1615 to 0·3230 centim. distance, I found for light corresponding to Fraunhofer's line D the following values of the constant B:—

Amount of the Electric Double Refraction for different Fluids between Plane Nickel Electrodes, measured by a Babinet Compensator. (Fraunhofer's line D.)

	$\frac{\mathrm{B} \cdot 10^6}{\lambda}$.
Sulphide of carbon (Kahlbaum)	32·798
" " (Heidelberg) . . .	31·984
3 vol. sulphide of carbon + 1 vol. ether .	27·252
1 " " " + 1 " " . .	19·476
1 " " " + 5 " " . .	4·422
Heavy benzol (from coal-tar)	4·460
Pure benzol (from benzoic acid) . . .	3·842
Light benzol	2·970
Oil of turpentine	0·109
Colza oil	−2·273
Ether (fresh distilled)	−6·400
" (having stood over calcined marble)	−6·685
Sulphur dissolved in sulphide of carbon (19·5 per cent.)	32·67

* Pogg. *Ann.* cxxvii. p. 206, pl. 1. figs. 4, 5 (1866).

† Phil. Mag. [4] l. pp. 337, 446 (1875); [5] viii. pp. 85, 229 (1879), ix. p. 159 (1880).

In sulphide of carbon the electric double refraction remained the same when the temperature varied 10°.

When insulating fluids are examined between two concentric cylinders with a Babinet compensator, so that the dark streak in the compensator appears parallel to the lines of electric force, on the electrification of the fluid the dark streak is the more displaced the nearer it lies to the inner cylinder. It frequently at the same time forms a curve with a turning-point, running to different sides according to whether the fluid possesses positive or negative double refraction. I have sometimes observed this turning-point even with strong electric forces between plane electrodes.

The difference of rate of light polarized parallel and that polarized perpendicular to the lines of electric force, at the surface of two cylinders with internal radius R_1 and external radius R_2, and length l, is given approximately by the equations

$$\left.\begin{aligned} d_1 &= B_1 \frac{l}{100} \cdot \frac{P}{\left(R_1 \log \text{nat.} \frac{R_2}{R_1}\right)^2}, \\ d_2 &= B_2 \frac{l}{100} \cdot \frac{P^2}{\left(R_2 \log \text{nat.} \frac{R_2}{R_1}\right)^2}. \end{aligned}\right\} \quad . \quad . \quad . \quad (6)$$

B_1 is mostly found smaller, B_2 greater, than the constant B, under otherwise like conditions. B_1 and B_2 are likewise in general found to be smaller the greater the length of fluid through which the light passes.

For a series of fluids with cylindrical electrodes of 10–47 centim. length and 0·3–0·5 centim. internal, 0·7 centim. external radius, I obtained, as means of several series of experiments, the following results :—

Electric Double Refraction for various Fluids between Cylindrical Electrodes, measured with a Babinet Compensator. (Fraunhofer's line D.)

	$B_1 . 10^6$. λ.	$B_2 . 10^6$. λ.
Iodine and sulphide of carbon . . .	53·69	
1 part sulphur + 4 parts sulphide of carbon	28·55	43·05
Sulphide of carbon	22·50	33·65
Heavy benzol	4·21	6·49
Oil of turpentine	3·15	4·55
Petroleum	0·60	
Colza oil	−2·81	−7·00
Oil of rape-seed	−2·07	
Ether	4·17	−6·82

Petroleum and oil of rape-seed show great variations of electric double refraction, according to the sort of oil examined. In a certain description of petroleum, d_1 at first increased with the increase of P, and then remained constant while P was raised from 55 to 72 C.G.S.

In rape-seed oil the difference of phases of the light polarized parallel and perpendicular to the lines of electric force was greater at the first moment after the sudden entrance of the electric forces than subsequently. At the same time the field appeared darkened for about half a minute. The phase-difference therefore consists of a variable and, on the action of the electric forces continuing, a constant portion. The former appears to arise from an electrical unequal expansion of the oil, in like manner as double refraction results from unequal heating.

The phenomena of double refraction are very striking when from any cause the electric forces in the interior of the insulating fluid undergo periodical variations and at the same time the dark streak in Babinet's compensator is displaced a little or starts. The greater the difference of the electric potentials of the electrodes the more frequent were these startings or pulsations. With cylindrical electrodes the dark streak in the Babinet compensator then appeared displaced to the same extent on the inner and outer faces of the cylinder. The constants B_1 and B_2 increased as the difference of potentials rose; and only with high potential-differences did they reach the value ordinarily found for them without pulsations. At the same time numerous air-bubbles are evolved from the fluid, and a clicking sound is heard proceeding from the apparatus, the pitch of which falls as the potential-difference diminishes.

In order to examine whether the mean index of refraction of insulating fluids is altered by electric forces, the fluid condensers were inserted in an interference-apparatus*, in which two pencils of rays reflected from two plane-parallel glass plates of equal thickness interfere and form a pure spectrum with Fraunhofer's lines, which is traversed by dark interference-streaks. One of the pencils passed through the fluid inside, the other outside the electrodes.

On the electrification of the fluid a displacement of the interference-streaks was observed, now in the direction of an increase, and now in the direction of a diminution of the refraction-index by the electric forces. Outcurvings or waves, as it were, rise above the interference-streaks. These wave-like displacements appear to point to periodic discharges or

* Quincke, Pogg. *Ann.* cxxxii. p. 53, pl. 2. f. 11 (1867).

a periodic variation of the electric forces within the insulating fluid between the electrodes.

The displacement in the direction of a diminution of the refraction-index corresponds to a raising of the temperature of the insulating fluid from 0°·0001 to 0°·1 C.; it increases with the difference of electric potentials of the electrodes and the viscosity of the fluid, and may be owing to a heating of the fluid by friction, since the fluid-particles are moved to and fro and stirred round by the electric forces between the electrodes.

The increase of the refraction-index occurs, in some fluids, only at the commencement of the electric action; in others it alternates with a diminution at short intervals during the entire continuance of the electric action.

II. *Upon a Photo-chemical Method for the Determination of Organic Matter in Potable Water.* *By* Dr. ALBERT R. LEEDS*.

WITHOUT attempting to revive at this moment the discussion as to the sources of error inherent in the present methods employed for the estimation of organic matter in potable waters, or as to the relative value of the inferences drawn from the results arrived at by the various analytical methods, I shall proceed to state the outlines of the new process, and the experiments thus far made to test its accuracy and applicability. The process is founded on the fact of the ready decomposability of certain salts of silver when exposed, in the presence of organic matter and in the state of solution, to the action of light. As a preliminary step it was needful to determine whether these salts were likewise reduced, even when organic matter was not present; and to this end the following experiments were tried:—

1: To 100 cubic centim. of ammonia-free distilled water, 5 cubic centim. of a decinormal solution of nitrate of silver was added, and the liquid exposed to the sunlight for 48 hours in a well-stoppered comparison-tube.

2. The same, but after addition of ammonia just sufficient to redissolve the precipitate first formed.

3. Same as 1, but the silver first precipitated as chloride, and then redissolved by ammonia.

4. Same as 1, but the silver precipitated as cyanide, and then redissolved in cyanide of potassium.

5. Same as 1, but the precipitate first formed with sodium hyposulphite just redissolved in excess of reagent.

* Communicated by the Author.

After two days' exposure, no precipitation of reduced silver occurred in the neutral solution of argentic nitrate, nor in that of the ammonio-argentic oxide, nor in the ammonio-argentic chloride, nor in the sodio-argentic hyposulphite.

Having thus established the fact that no reduction of silver occurs in case organic matter is rigidly excluded, even after prolonged exposure to sunlight, the next point of inquiry was to determine which of these five solutions was most affected by such organic matters as are ordinarily present in potable water. The inquiry as to the relative energy of action of different kinds of organic matter in general did not necessarily form a part of the present investigation.

In the study of this point, a sample of potable water was made use of from the river Schuylkill, the water-supply of the city of Philadelphia. This drinking-water had been affected by a sudden cachexy in the month of January, becoming most offensive to taste and smell; and the authorities of the city had requested me to investigate the causes of its non-potability. The scope of the inquiry being thus narrowed to the finding of specific causes, I was necessitated to tax the capabilities of the methods at present possessed by water-analysts, and was furthermore led to test this new one. Now on adding, to the five silver solutions prepared as in the first series of experiments, 100 cubic centim. of this Schuylkill water, it was found, after 5 hours' exposure to sunlight, that the ammonio-argentic oxide was slightly changed, the potassio-argentic cyanide was unaffected. Of the remaining three, the sodio-argentic hyposulphite threw down the heaviest precipitate, the ammonio-argentic chloride a precipitate nearly as great, and the neutral solution of argentic nitrate the least.

From these experiments it would appear that the solution of argentic hyposulphite in excess of sodium hyposulphite is the reagent best adapted for use in this actinic method. But subsequent experiments showed that the neutral solution of argentic nitrate fulfilled all needed requirements, and that the labour of preparing a special reagent was uncalled for. The method finally adopted is as follows:—

250 cubic centim. of the natural water is treated with 10 cubic centim. of decinormal silver solution in the tall glass-stoppered cylinders or bottles. The waters become turbid and frequently coloured; but after a moderate interval, usually less than two days, the turbidity entirely disappears and the entire precipitate collects at the bottom of the vessel. In case sufficient silver has been employed (and the amount recommended is ordinarily a large excess), this clarification indicates

an end-reaction, and it is useless to expect a renewed precipitation in case the filtrate be submitted to second exposure. The precipitates are collected on asbestos filters, similar to those used in the determination of carbon in steel, washed first with water, then with strong ammonia to redissolve co-precipitated chloride, dissolved in nitric acid, evaporated to dryness, redissolved in nitric acid, and the silver determined by Pisani's method. All these washings are but the work of a few minutes with the aid of a water-pump. The metallic silver may be weighed directly; but the method indicated is more rapid and equally accurate.

Six samples of the Philadelphia water (Nos. 230–235) were so treated; and duplicate determinations of the oxygen required to oxidize the organic matters were made by potassium permanganate. Finding that the results better accorded with the information imparted by the other data of water-analysis than those obtained by the permanganate, the method was systematically applied to the examination of the Passaic water (water-supply of Newark and Jersey City).

The results were as follows:—

	Silver precipitate. mgrm.	Corresponding to Oxygen. mgrm.	Oxygen by Permanganate mgrm.
Schuylkill River.			
Laboratory No. 230	1·47	0·218	0·18
" " 231	1·65	0·244	0·19
" " 232	1·91	0·283	0·18
" " 233	2·02	0·299	0·18
" " 234	1·56	0·231	0·16
" " 235	1·39	0·206	0·16
Passaic River.			
" " 242	2·26	0·33	0·34
" " 243	2·596	0·37	0·58
" " 244	2·3	0·34	0·56
" " 245	1·184	0·17	0·57
" " 246	2·296	0·34	0·59
" " 247	3·59	0·53	1·05
" " 248	2·074	0·31	0·33
" " 249	3·333	0·49	1·33
" " 250	2·592	0·37	0·37
" " 251	3·55	0·52	0·37
" " 252	3·2	0·47	0·38
" " 253	2·82	0·42	0·30
" " 255	2·29	0·34	0·35
" " 256	2·67	0·39	0·36
" " 257	2·29	0·34	0·36

It will be noted that whilst there is a close agreement in many cases between the results obtained by these utterly diverse methods, yet in others there is a striking difference—the higher result being uniformly obtained by the actinic method with the Schuylkill waters, but generally by the permanganate with the Passaic waters. Now investigation revealed that the non-potability of the former was connected with a state of retarded or arrested oxidation, the ice-covered stream having a striking deficiency of oxygen as compared with its normal percentage, and being laden with products of organic decomposition. In the Passaic, besides the sewage there is a great volume of trade pollution; and it is in connexion with such samples (like Nos. 247 and 249, which show the points where the refuse of the great manufacturing city of Paterson is discharged into the Passaic) that the discrepancy is greatest, and the oxygen as determined by the permanganate most exceeds the amount as determined by reduction of silver. The general inference from these experiments was that, while potassium permanganate at the boiling-point (the determinations were made at 98° C.) was decomposed to a greater or less extent by most organic substances soluble in water, the reduction of the silver was to be ascribed to easily decomposable organic bodies akin in their nature to sewage.

To test this point some weak hay-infusion and highly dilute urine were added to two portions of silver solution and exposed to sunlight. A dense mirror quickly formed on sides and bottom of vessels. Solutions of cane-sugar, moist sugar, and clear starch were added. No darkening of the liquid occurred in the first two cases, and after a short while the slight grey precipitate ceased to increase. This faint action was not due to the sugar, but to a minute amount of foreign organic matter contained in it. Starch produced no change, neither did nitrobenzol. Urea caused a scarcely perceptible precipitate. The powerful base aniline decomposed the silver salt, forming aniline nitrate; and afterwards the liberated argentic oxide was decomposed, its silver forming a beautiful mirror and the solution becoming dyed with rosaniline. This reduction by organic bases might indeed be employed for the silvering of mirrors and the interior of glass ornaments. But such bodies would be rarely present, even in trade-polluted streams; and if they were, would be readily detected, and their influence allowed for.

Of course, the reduction of ammoniacal solution of silver in presence of alcohol, naphtha, and essential oils like cassia, caraway, cloves, &c., will occur to the reader; so also will the reduction of the alkalized silver liquid by alcoholic solution of grape-sugar, or of milk- or cane-sugar, or by aldehydes &c. But the reductions thus effected bear little analogy to that occurring in a neutral solution, otherwise permanent, under the influence of light. In conclusion, I desire to add that this publication is in one sense premature; for while the process has been constantly employed in my own laboratory during the past four months, I have not as yet had the opportunity to apply it to so great a variety of waters as I had intended. But finding that various chemists to whom I have communicated it have always adopted the method, I publish it in order to obtain the benefit of wider and more severe criticism.

III. *On* Mr. Ferrel's *Theory of Atmospheric Currents.* *By* D. D. Heath, *Esq.**

IT has recently come to my knowledge that teachers of Physical Science who have not had a mathematical training (and the specialism of the day must be increasing the class) trust to Mr. Ferrel, of Cambridge in the United States, as a sound and accepted authority for dynamical principles and conclusions relating to atmospheric currents; and, not to quote other confirmations of this fact from communications to 'Nature' and elsewhere, I find that Dr. Haughton, of Dublin, thus speaks of him in some lectures on Physical Geography (1880):—

"We are not able, in the present state of our knowledge, to form a complete mathematical theory of atmospheric currents caused by the unequal expansion of the air by solar heat. . . . But we are able to satisfy the equations of motion by a special arrangement of all the atmospheric currents and barometric pressures, which gives us a solution of the problem that explains every important fact. . . . This solution is due to Mr. W. Ferrel, of the United-States Coast Survey, 'The Motions of Fluids and Solids relative to the Earth's Surface' (New York, 1860)."

This is a cautiously worded passage; and I suspect Dr. Haughton has not examined very carefully the connexion between Mr. Ferrel's premises and conclusions. I only quote it as a justification for my thinking it must be worth while to show the unsoundness of his work and the baselessness of his conclusions, if I am right in the estimate I have made of them. I may as well premise, however, that I possess no familiarity with the *facts* of meteorology, on which Mr. Ferrel may have every claim to speak; I only address myself to his principles and methods of proof.

* Communicated by the Author.

If I were mainly writing for mathematicians, I should at once begin on his treatise of 1860, on which Mr. Ferrel's other papers and communications to 'Nature' are founded; and my paper would not be long. But having rather in view readers who are not generally familiar with partial differential equations, and wishing to carry them along with me without impediment as far as I can, I will begin with a communication to 'Nature' (March 14th, 1872), which is in fact what was first brought to my notice and led me to further inquiry.

It is headed "Ocean Currents," and forms part of a discuscussion on the Gulf-stream and similar phenomena, in which Dr. Croll, Professor Everett, and a Dane, Professor Colding, are concerned. But it includes atmospheric motions in its scope.

He begins with warnings against taking on trust "all the principles and theories *based upon hypothetical forces* which have come down to us from preceding generations, however plausible and however much sanctioned by authority they appear to be," with more in the same strain. But, after all, he adduces no example of such hypothetical assumptions of forces from older writers; and we shall see that it is he who is rather obnoxious to such a charge.

He goes on to lament that Professor Colding has been "unsuspectingly led into error" by accepting from old textbooks the statement that "if a particle of air at the equator, relatively at rest and therefore having a linear velocity in space of about 1000 miles an hour, is *forced* to move towards the pole, it will, on arriving at the parallel of latitude where the earth's surface has a velocity of only 900 miles, still have its velocity of 1000 miles *in the case of no friction*" (which, here and elsewhere, I understand to include pressure of the surrounding air), "and consequently have a relative velocity of 100 miles; and, on arriving at the parallel of 60°, will have a relative velocity of 500 miles."

I should agree that a bare statement like this in a text-book would be objectionable as, at the least, misleading. Matter at the equator can no doubt be "forced" to move all the way to the pole. But the velocity and direction with and in which it arrives there will depend upon the means taken to force it. It would never get there by any natural impulse or pressure originating in the equatorial regions. For it is one of the plainest of fundamental facts in Dynamics (though we shall see that Mr. Ferrel denies it) that, taking gravity to be directed to the centre of the earth, as it is very nearly, a mass situated anywhere near the surface, but unconnected with it and unresisted by the air, if put in motion, will begin to move *in fixed* *space*, in a straight line, in a direction and with a velocity compounded of the velocity it had in common with the surface at that place and that of the impulse given to it, in accordance with the principle of the *Parallelogram of Velocities*, and will be drawn out of that line only downwards, by the force of gravity at the centre of the earth; and therefore it will never leave the *plane of the great circle*, fixed in space, which comprises the original direction and this centre. From this it will be seen that, if started due north from the equator with a velocity of even 1000 miles an hour—the same as the eastward velocity of the surface there—it will only reach the latitude of 45°. Up to that culminating point it will gain *apparent* or relative velocity, compared with the surface it is passing over, as explained in Mr. Ferrel's "text-book," and then will begin its descent, losing relative velocity, cross the equator at the point opposite to the one it started from, and complete its circuit in the other hemisphere, just as the moon does at a greater distance—that is, supposing gravity does not, long before then, pull it down to the earth's surface. This effect of equable motion combined with passage over different latitudes is precisely one part of the explanation of that variability of the sun's apparent motion which gives rise to the "Equation of Time," or difference between solar and clock time.

Sir J. Herschell, I may remark, in his 'Meteorology,' does not thus mislead his readers. He takes the lower current of cold air *from the north*, and traces it downwards towards the equator; and this, if unimpeded, it will reach with ever so little original impulse southward; and even with the actual resistance the impelling force arising from the difference of temperature will always be helping, and may prevail. When he comes to consider the upper northward current, he only conducts it "beyond the tropics," and there brings it down to earth.

I have gone at length into this very elementary exposition, because a clear conception of the facts will prevent a great deal of confusion from Mr. Ferrel's papers. But this is by no means the objection which he in this place goes on to bring against the "mischievous" teaching of the text-books. What he says, after the passage I have quoted, is:—"But this is at variance with a fundamental and well-established principle in mechanics. The force in this case is a central force; or at least the compound perpendicular to the earth's axis [*sic*, I do not understand what resolution of the force he has in his mind] can be neglected, since it can have nothing to do with any east or west motion. This being the case, the *principle of the conservation of areas* must be satisfied; and consequently, when it arrives at the parallel of 60°, where the earth's surface has a velocity of 500 miles, it must have a velocity of 2000 miles."

My first impression on reading this was that Mr. Ferrel could not mean to propound it as his own doctrine; but must have intended some *argumentum ad hominem* against Professor Colding, though I could not see it. But it appears to be his serious belief, stated in his treatise of 1860 (parag. 22), and repeated and made the basis of calculation in a summary of his doctrine, by himself, in Silliman's Journal (2nd ser. vol. xxxi.). Now this "principle," as applicable to this case, is that, when a body is moving subject only to the action of a force directed to a fixed centre, the imaginary line which joins it with that centre sweeps out equal areas round it in equal times, however much the body may approach to or recede from it in the course of its orbit. Here, the centre of force is the centre of the earth; and the body moving over the surface is always very nearly at the same distance from it; and so, to preserve the equality of areas, it must preserve very nearly an equable velocity. Mr. Ferrel seems to imagine that the force shifts along the axis of the earth so as always to keep abreast of the moving body, and so to give it a corkscrew motion round the axis, but drawing it into continually narrower and narrower circles, and so giving it greater and greater velocities. What gravity really does is to keep the moving body, together with air and sea and all loose matter, from flying off into space, leaving the solid part to spin by itself.

Mr. Ferrel goes on with his charge against Professor Colding:—"Adopting, thoughtlessly and very naturally, the erroneous principle which is usually taught" (that, namely, which he impugned above, that a body will keep the velocity and direction once given to it until interfered with) "he estimates the amount of *deflecting force due to the earth's rotation*; and the result is that his force is just one half of what it really is." I have not read Professor Colding's paper, and so cannot say whether he has made any mistake. And Mr. Ferrel seems to me to have confused himself about these "deflecting forces," and so not to be very intelligible to others. But I understand the charge to mean that the Professor has here estimated the *relative* eastward or westward motion as due to the earth's spinning under the moving body which *keeps* its original velocity, instead of *doubling* the effect by the imaginary "conservation of areas" principle. If so, he has certainly so far done the right thing.

But then he adds that the Professor has "entirely neglected one component of the force due to the earth's rotation," which Mr. Ferrel and Professor Everett have (independently as I understand it) discovered. "If he had taken in this latter component also, and resolved it in the direction of the line of motion and perpendicular to it, as he did the former he would have found that the parts in the direction of the motion *arising from both components* exactly cancel one another n all cases, and that the resultant of both components is a force perpendicular to the direction of motion,"—which of course "tends only to change the direction of the motion and *never to accelerate or retard it, in whatever direction it may be.*"

So that, on Mr. Ferrel's own showing, a body set in relative motion, towards the pole or in any other direction, will, after all, neither be accelerated nor retarded, in virtue of the "principle of conservation of areas," but will maintain its velocity and be deflected from (that is, never come near to) the pole. In truth it will, when supposed free in its motion, move equably in a great circle, as I have already explained, in opposition to Mr. Ferrel, who, not here only but in Silliman's Journal, explicitly denies it*. For these two forces which combine to "deflect" the moving body (that is, to alter the apparent direction of its motion) are the merely "hypothetical" forces which, I have above said, are introduced by Mr. Ferrel himself—justifiably enough, if he understood them to be such. To explain—

If a body moving uniformly in a straight line is viewed from an observatory itself in motion, it will appear to be moving in some curve and with varying velocity. And the observer may express this fact by saying that, relatively to him, it moves as if acted on by such and such forces. And this is our case. We are carried along by the earth's rotation from west to east; and if a mass is set moving, *in space*, equably along a great circle, it moves, as we estimate it, away from our latitude and our longitude, and unequably. "Great-circle sailing" is really straight sailing; but I dare say that many an uneducated steersman, when following the rules given him for that operation, thinks he is altering the straight course. I should here add that Mr. Ferrel does, in Silliman's Journal, when speaking of the resultant "force," remark that it "is not an absolute force, but relative; somewhat in the nature of centrifugal force." But this seems to have been a fleeting thought not duly followed out; or he would not, a dozen years later, have written what I have quoted.

I do not know what Professor Everett has written, or

* "If a body were set in motion upon the surface of the earth, it would not in general move in the circumference of a great circle."

whether he makes any claim to a discovery in this matter. When the mathematical investigation of the motions of a fluid surrounding the rotating earth is made to commence, as by Laplace and Airy, and by Mr. Ferrel in his paper of 1860, with the most general equations, it is at once evident to inspection that the terms which suggest these "deflecting forces" really express the effect of passing from formulas adapted to a fixed to those adapted to a revolving observatory, as I will point out more clearly below. But I have to confess that this simple interpretation had not occurred to me till I had pondered for some time over Mr. Ferrel's papers.

I wrote a paper in the Philosophical Magazine (March 1867) on Deep-Sea Tides, based on Airy's "Tides and Waves" in the *Encycl. Metrop.*, but intended to be less abstruse and formidable. And I deduced these two terms in the way indicated by Airy (par. 87 and 88, where he also calls them the representatives of "forces"), making one term the expression of the alteration of *relative* motion by the mere passage north or south, as explained in the passage first quoted from Mr. Ferrel above, and the other the expression of the effect of the impressed east or west velocity by virtue of which the mass is carried outward or inward in the plane of the small circle of latitude (a so-called "centrifugal force"), and so throwing it southward or northward along the surface. And though I had to prove in the course of the investigation that "these forces counteract each other" as regards acceleration and retardation, I did so without further reflection or inference.

I hope I have made it sufficiently plain that the two points Mr. Ferrel insists on in this communication—the positive one, that the principle of the conservation of areas requires an accelerated spiral motion in bodies impelled northwards, and the negative one, that an impulse given to a free body along the surface of the earth will not generally send it along a great circle—are utterly unfounded and erroneous. I now proceed to the paper of 1860 referred to by Dr. Haughton; and, still addressing myself mainly to the same class of readers, I will first notice some prominent points in the treatment and results, and then say a few words on the mathematical working*.

1. His purpose is not to trace the course and effect of any accidental disturbance in the atmosphere or ocean, but the permanent and necessary motions, pressures, and shapes which must arise and subsist in virtue of the causes of motion which are permanently at work. In an ideally "perfect" fluid, and

* It appears that the substance of this paper was distributed in pamphlet form by the Smithsonian Institute in Europe and America; which I suppose has helped Mr. Ferrel's reputation.

with no surface friction, any motion once started might indeed alter its character, but would never cease, and would blend with and affect the permanent phenomena. But with fluids and surfaces as they are, each such accidental disturbance will sooner or later be exhausted; and we may reasonably endeavour to find what *minimum* of motion is consistent with the existence of the permanent forces, and assume that this will be the *average* state. This Mr. Ferrel himself states. Now, as he takes no notice of tides, these permanent causes of motion are gravity and the unequal action of the sun's heat, according to season, latitude, and longitude (as fixing the time of day and night at each place), and height above the surface. And gravity of itself requires no permanent relative motion; for the 'Theory of the Figure of the Earth' shows that, where there is no inequality of temperature, the fluids can arrange themselves in a permanent shape adapted to a uniform rotation round the earth's axis. There will be relative motion of the parts until this shape be reached; afterwards, relative rest. Mr. Ferrel accordingly begins with supposing the heat does vary according to latitude, longitude, and height, though he takes no notice of seasons. But he is quite entitled to say the complete problem is too difficult, and to simplify it down as far as he thinks it necessary. If he could give us a satisfactory solution on the supposition that the localized and moving sun was replaced by a uniform equatorial ring of heat, I suppose he would have performed a great feat. But he does not start in any such way. He at first supposes the temperature to vary *somehow*: for aught that appears in his formula, the torrid zone might be the coolest, and the polar regions the warmest portions of the earth, and the air hotter 7 miles high than at the surface. But when he begins to get results (Sect. II.), he first supposes temperature to vary only with latitude, allowing no difference for height, and finally "supposes the density," and of course the temperature with it, "to be independent of the latitude." But this supposition amounts, as I have already pointed out, to supposing that there is no force at all to sustain any motion, and that consequently there are no permanent tendencies in the winds—that the average state is one of relative rest, the air arranging itself in conformity to the Theory of the Figure of the Earth. Thus all his work should be all to no purpose!

2. But he fancies he does get results. And this is the principal one:—"The fluid, *however deep it may be at the Equator, cannot exist at the Poles.*" And he draws a picture of the atmosphere as it should be, banked up from somewhere near the arctic circle towards the middle latitudes, but with a depression

about the equator. Is this one of the results which please Dr. Haughton as conformable to facts?

Some other curious results might be deduced from his numerical formulas; but I will not dwell on them. Mr. Ferrel himself is conscious that all this is not very like nature, and so suggests "the resistance of the earth's surface" may considerably modify the state of things. And having once called in aid this agency, like the god on the Athenian stage, he is free from all restraint from formulas, and rearranges all his shapes and currents—for aught I know in fair conformity with facts, but with hardly any resemblance to his first picture. One would think that friction would rather hinder than help the air's spreading towards the poles.

3. Another (a minor) point to observe is that, on commencing his investigations, he does not prove, but at once assumes that the upward and downward motions of the air, due to changes of temperature, which I suppose undoubtedly play a large part in these phenomena, are themselves everywhere insensible as wind.

4. I should mention that in his Fourth Section, long after he has deduced these results from the supposition of absolute uniformity of temperature, he does make a supposition of its varying with latitude; and from this, combined with deductions from barometrical observations, he draws some inferences as to the strength of the average east or west components of the wind at different places and different heights. How near they may be to actual fact I do not know; but I must warn my readers (not to go into more abstruse criticisms) that, no doubt from mere inadvertence, he works on the supposition that the hotter the air the smaller the pressure for a given density*, and, moreover, will not allow that the variation of temperature with the height of the strata can "produce any sensible effect" on the difference of velocity of the wind at different heights.

I hope I have, so far, succeeded in showing to those I have in view that they had better trust to the old text-books than to Mr. Ferrel, as far as fundamental principles are concerned, though I dare say his store of facts may be greater. I will now say a few words about his use of his mathematical tools.

Putting P for pressure, κ for density, and Ω for the gravity potential, he begins, as Laplace and Airy do, with the equations in fixed rectangular coordinates $\frac{d^2x}{dt^2}+\frac{d\Omega}{dx}+\frac{1}{\kappa}\frac{dP}{dx}=0$,

* He puts κ (density) $=\alpha$P, and then supposes α to vary *directly* as the "absolute temperature," usually written $\left(\frac{1}{\alpha}+t^\circ\right)$.

and so on, making the true remark that, "putting $P=0$, they are the equations to a projectile."

And he might have observed that (assuming symmetry round the axis of rotation) these equations are the same whether the earth is revolving or not, and that, when we put $P=0$, we get the path of the projectile in fixed space the same for a stone or for the moon.

He then, as Laplace and Airy do, transforms the equations for polar coordinates. And if he and they retained, for a while, ω for the fixed celestial longitude, we should have equations equally independent of rotation. Writing $-g$ for $\frac{d\Omega}{dr}$, they would be

$$\frac{1}{\kappa}\frac{dP}{dr} = -\frac{d^2r}{dt^2} + r\left(\frac{d\theta}{dt}\right)^2 + r(\sin\theta)^2\left(\frac{d\omega}{dt}\right)^2 - g,$$

$$\frac{1}{\kappa}\frac{dP}{d\theta} = -r^2\frac{d^2\theta}{dt^2} - 2r\frac{dr}{dt}\frac{d\theta}{dt} + r^2\sin\theta\cos\theta\left(\frac{d\omega}{dt}\right)^2,$$

$$\frac{1}{\kappa}\frac{dP}{d\omega} = -r^2(\sin\theta)^2\frac{d^2\omega}{dt^2} - 2r(\sin\theta)^2\frac{dr}{dt}\frac{d\omega}{dt} - 2r^2\sin\theta\cos\theta\frac{d\theta}{dt}\frac{d\omega}{dt}.$$

But when we put $\omega = nt + \phi$, we get equations adapted to a terrestrial, revolving meridian. And when we make n = the actual angular rotation, and suppose the figure of the earth, with the undisturbed sea and atmosphere, such as are adapted for keeping the whole system in relative rest (see Airy, paragraph 79), then we can separate what belongs to the undisturbed system and what to the motions, shapes, and pressures arising from the extraneous forces (here those due to change of temperature). And then the terms in question make their appearance. Mr. Ferrel makes, substantially, the same simplifications which Laplace and Airy make in the Tidal theory. They do not seem quite so legitimate here*, where there is no *à priori* ground for supposing the motions very small; but I am not quarrelling with this. His next, his first original step is to lay it down that "For a stratum of equal pressure P is constant, *and hence* $\frac{dP}{d\theta}=0$ and $\frac{dP}{d\phi}=0$;" and of course, by parity of reasoning, $\frac{dP}{dr}=0$. And all his formulas, I believe without exception, till he comes to those in the Fourth

* In my paper I ventured to suggest some doubts on the subject even as regards tidal motion near the poles.

Section, where he connects pressure and wind, are derived from these equations! He gives no equation of continuity. And in truth he does not want it. For his equations, as he had himself begun with observing, are the equations to the *motion of a free projectile*, not of a continuous fluid.

Or, rather, this is what they would have been but for his fantastic treatment of them. Taking the case of an elastic fluid, he puts $\kappa=\alpha P$ (in connexion with which I have mentioned the blunder he subsequently makes); and then he separates the right-hand side of the equation for $\frac{1}{\kappa}\frac{dP}{dr}$ into two parts (say $U-g$); and then the three equations become

$$\frac{1}{P}\frac{dP}{dr}=\alpha U-\alpha g, \quad \frac{1}{P}\frac{dP}{d\theta}=\text{(say) } \alpha V, \quad \frac{1}{P}\frac{dP}{d\phi}=\alpha W.$$

Then he supposes H to be the complete integral of

$$\alpha U dr+\alpha V d\theta+\alpha W d\phi,$$

without attempting to prove that it is a complete differential; and then he asserts that $P=H-\int g\alpha\, dr$, or say $H+K$; and *then* he applies his proposition as to the consequence of making P constant, and gets his wonder-working equations

$$0=\alpha V+\frac{dK}{d\theta}, \quad 0=\alpha W+\frac{dK}{d\phi},$$

in which, moreover, we find that the r which may be involved in α, and so in K, is treated as a function of θ and of ϕ in the differentiation, though θ and ϕ are not treated as similarly interdependent.

I think this is a sufficient specimen of Mr. Ferrel's way of dealing with symbols as soon as he leaves the beaten track, and that no one need attempt to follow him further.

Kitlands, Dorking.

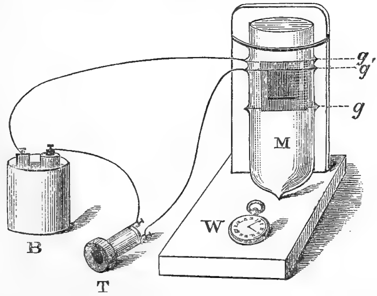
Postscript.—Just as I was about to send this communication to the Philosophical Magazine I became acquainted with Mr. Scott's 'Elementary Meteorology;' and I find from a passage in it that my warning against the misleading phraseology of the "old text-book," as quoted from Mr. Ferrel, is by no means superfluous. After explaining how the heated surfaces in the equatorial regions must cause an overflow of rarefied air northward and southward, he goes on (p. 241):—"As the form of the earth is spherical, the circumference of the equator, a great circle, exceeds that of the smaller circles of latitude, which gradually diminish to a point at the pole. The meridians therefore converge as the latitude increases; and the air, *flowing polewards,* is moving in a bed continually becoming narrower and narrower, and is finally *forced down to the surface* of the earth long before the pole is reached."

I do not understand this process of "forcing down." But this description of the air racing and crowding towards the pole is entirely imaginary. A mass of air, or anything else, pressed or impelled northwards in the equatorial regions is not in the least tending towards the pole, but does begin to move in a great circle slightly inclined to the equator. I am no meteorologist; but I presume that the northward motion is reinforced while it still moves in hot regions; and so the great circle in which the motion is supposed to take place may become more and more divergent; and the northward motion may be sensible up to the latitudes of 30° or 40° mentioned by Mr. Scott. But a general flow of air in the upper regions from west to east within a certain tropical belt is all that I can infer from the data.

I have also found that Professor Everett, in his latest edition of the 'Natural Philosophy,' at p. 525 accepts Mr. Ferrel's theory of the Conservation of Areas, and at p. 527 refers to Mr. Ferrel's paper of 1860 as "the most complete exposition we have seen of the general atmospheric circulation."

IV. *On Metal Microphones* in vacuo.
By J. Munro, *A.S.T.E.**

The consideration that a metal microphone would not oxidize to any serious extent in a vacuum, and also a desire to see how it would operate in exhausted air, led me to construct the apparatus illustrated below.



It consists of a tubular bulb of glass, M, into which the

* Communicated by the Author.

microphone is sealed by means of electrodes of stout platinum wire, g, g, g'. The two wires g, g have a plate or strip of iron-wire gauze, of about 80 meshes to the lineal inch, stretched tightly between them. This gauze forms the fixed contact of the microphone. The movable contact is formed of a smaller square of similar gauze hung or hinged on the third electrode, g', in such a manner that it rests lightly against the fixed piece behind. This metal piece is so light that it forms a microphonic contact with the fixed piece below; and when a circuit is made by a battery (B) and a telephone (T) through the gauze, the ticking of a watch, w, laid on the base-board is heard in the telephone.

The sensitiveness of the microphone is increased by bringing up the pole of a magnet *in front of* the movable piece so as to lighten the pressure of that piece on the fixed plate behind, and tend to draw, by the inductive attraction of the pole upon the iron, the movable piece away from the piece on which it rests. I have not been able to ascertain whether the movable piece is actually separated from the back piece by the approaching magnet, because in the case of metals, when the microphonic contacts are separated by an extremely minute space, the current is interrupted, whereas in the case of a carbon microphone Mr. Stroh has observed an actual separation of the contact-points by means of a microscope, whilst the current continued to flow and the microphone to act. It is certain, however, that as the pole of the magnet is gradually brought nearer to the movable gauze the ticking of the watch becomes gradually louder and better defined, as if the contact-points were able to vibrate in a freer manner and with a longer range. At last there comes a point at which the sonorous current, which had evidently increased till then, suddenly fails, and a sharp click in the telephone announces the fact that the electric flow is interrupted.

If, on the other hand, the pole of the magnet be brought up behind the fixed gauze, the sound in the telephone dies off, apparently because the movable gauze is pulled by its attraction into closer contact with the fixed piece behind.

On exhausting this bulb with a mercury-pump of the improved kind used for making electric incandescence lamps, the ticking of the watch becomes singularly clear and metallic in quality, the very ring of the hair-spring seeming to be audible. In fact a vacuum microphone of this kind is peculiarly sensitive; and the effect appears to be due, in part at least, to the rarity of the air within the bulb. Another form of the instrument, having loose iron grains contained in a vacuum-bulb between two electrodes of platinum, exhibits the same clearness of tone which distinguishes the same arrangement in air at the ordinary pressure from that in a vacuum. The degree of vacuum existing in the bulbs is that of the usual incandescence lamp, or about one millionth of an atmosphere.

It might be thought from M. Edlund's researches, recently referred to in the Philosophical Magazine, that polarization of the metal contacts would prevent the microphonic action *in vacuo*; but this is evidently not the case.

V. *On the Determination of Chemical Affinity in terms of Electromotive Force.*—Part VII. *By* C. R. Alder Wright, *D.Sc. (Lond.), F.R.S., Lecturer on Chemistry and Physics, and* C. Thompson, *Demonstrator of Chemistry, in St. Mary's Hospital Medical School*.*

On the Electromotive Force of Clark's Mercurous-Sulphate Cell; and on the Work done during Electrolysis.

On the E.M.F. of Clark's Cell.

133. In the course of the series of experiments partly described in Parts V. and VI. a large number of observations have been made with various cells after Clark's construction (Proc. Roy. Soc. xx. p. 444), in all cases compared with one another and with other cells by means of the quadrant-electrometer only, so that they never generated any current other than the minute leakage current through the not mathematically absolutely insulating materials between their poles.

In some instances the mercurous sulphate was purchased (from Messrs. Hopkin and Williams), and was well washed before use by numerous boilings with distilled water and decantations. In other cases the mercurous sulphate was prepared by heating twice-distilled mercury (previously purified by nitric acid) with pure sulphuric acid at as low a temperature as possible consistent with any action taking place, and thoroughly washing the resulting sulphate by repeatedly boiling with distilled water and decantation. The action was never allowed to go on until more than a fraction of the mercury used was converted into sulphate, in order to reduce the amount of mercuric sulphate formed to a minimum.

The cells were made out of pieces of ordinary combustion-tubing (selected on account of the absence of lead in the glass)

* Communicated by the Physical Society, having been read May 12, 1883.

drawn out before the blowpipe into the U-shape represented on about two thirds scale in the cut (fig. 1). The glass being perfectly dry and hot, pure recently-boiled still hot mercury was poured into them so as to form a layer about half an inch (10 to 15 millimetres) deep, *a*; on the top of this was then poured a boiling paste of thoroughly well-washed mercurous-sulphate and zinc-sulphate solution, containing so much of the latter salt as to be slightly supersaturated when cold, so as to crystallize on standing. It was found convenient to make the paste not too thick, and to let the solid matter subside in the cell, the supernatant comparatively clear fluid being sucked out by a clean pipette, so as finally to leave on the top of the mercury a layer of particles of mercurous sulphate wetted with zinc-sulphate solution some 15 to 20 millimetres deep, *b*. Pieces of zinc rod (cast in glass tubes from pure metal fused in a porcelain crucible), well brightened by a file that had never touched any other metal, were then placed in the cells so as to dip into the paste some 4 or 5 millimetres, and project out of it about twice as much, *c*. The zincs were kept from falling by pieces of cork, *d*, cut as represented in fig. 2, and previously immersed in hot paraffin-wax so as to expel air and moisture; to the ends projecting from the paste were previously soldered copper wires, *e*. Melted paraffin-wax was then poured into the cell so that all air was expelled, rising through the perforations in the edges of the cork disks, and so that the upper two thirds of the zinc and the soldering were completely covered, *f*. Finally, a piece of platinum wire, *g*, or a strip of foil was passed down the narrow limb of the cell so as to make contact with the mercury: it was found convenient to amalgamate the tip of the platinum by moistening it and immersing it in freshly made sodium amalgam, all sodium being removed from the adherent film of mercury by subsequent immersion in water for some hours. The cells thus prepared, being wanted for use only and not being required to be externally well finished, were not mounted in the neat brass cases with ebonite tops and binding-screws usually employed, but were simply fixed in a beaker, or any other convenient holder, by pouring in melted paraffin-wax around them. When used in connexion with the electrometer, the copper wire soldered to the zinc and a similar wire soldered

Fig. 1.

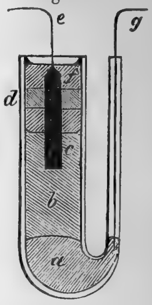


Fig. 2.

to the strip of platinum (and secured by a turn round the upper end of the narrow limb and a drop of sealing-wax) were bent over so as to dip into mercury-cups, a number of which were arranged in the arc of a circle round two others, like those figured in Part V.; so that any consecutive pair of cups could at will be connected with the electrometer by the double switch.

Either through a natural repulsion between bright zinc and the mercurous-sulphate paste, or through the formation of a faint film of grease &c. on the zinc from the file used to brighten it, it sometimes happened that the cell when finished would not work, contact not existing between the zinc and paste. It was found that this never occurred when the brightened zincs were washed successively with ether, alcohol, and saturated zinc-sulphate solution just before immersion in the paste.

134. On comparing together a moderately large number of cells (upwards of fifty) thus prepared with different specimens of mercurous sulphate, readings being taken two or three times a week for some three months, the following results were obtained:—A slight rise in E.M.F. was often observed during the first few days after construction; but at the end of a week at most the values *became constant, and remained so (the temperature being constant) for long periods of time.* The maximum variations observed between the average results of the series of observations for any two given cells were slightly less than that found to exist by Clark (whose highest and lowest values are respectively 1·4651 and 1·4517 volt, giving a difference of ·0134 volt, or upwards of 0·9 per cent.). Taking the average of the whole set as 100·00, the maximum variation between two single cells did not exceed ·010 volt, or 0·7 per cent., each cell possessing a value lying between 99·65 and 100·35. Even amongst cells set up at the same time from absolutely the same materials, extreme differences of as much as 0·005 volt = 0·35 per cent. were sometimes observed, although usually the difference did not exceed ·002 or ·003 volt and was frequently almost inappreciable.

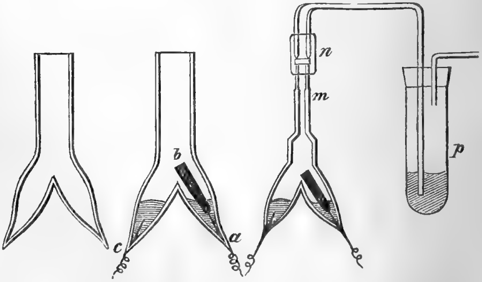
Much greater differences, however, were found to exist when the zinc-sulphate solution was not completely saturated with that salt, the variation produced being of this kind, that *the weaker the solution the higher the E.M.F. of the cell*, the difference being approximately proportionate to the amount of dilution, and amounting to upwards of 2·0 per cent. of the value when considerably dilute zinc-sulphate solution was used. The details of these observations will be discussed in a future paper, along with those of similar experiments made with other cells. It may, however, be here noticed that, so long as a cell containing unsaturated zinc-sulphate solution was protected against concentration by evaporation, and was only used in connexion with a quadrant-electrometer, its indications remained perfectly constant for many months (the temperature being the same), precisely as was found with cells set up with saturated zinc-sulphate solution.

Effect of Dissolved Air on the E.M.F. of Clark's Cell.

135. Two series of experiments were made with the object of finding out how far the boiling of the mercurous-sulphate paste (as recommended by Clark) in order to remove dissolved air is essential. In one series a number of cells were set up, using fully aerated zinc-sulphate solution and unboiled mercury (exposed to the air under a glass shade for several days since preparation and distillation respectively); in the other the paste was boiled in a Sprengel vacuum produced in the cell itself for some time, the cell being then hermetically sealed, so as to reduce the amount of residual air to a minimum. In each case the average E.M.F. of the combination *was sensibly identical with that of an average ordinary Clark cell* prepared as above described and containing zinc-sulphate solution of the same strength as that contained in the combination.

In order to prepare these hermetically-sealed cells a rather troublesome process was employed. First a piece of glass tubing, about 10 or 12 millim. in bore, was blown into a Y-shape, and the two limbs of the Y drawn out as represented in fig. 3; a zinc rod was then cast so that a thin platinum wire was imbedded in one end; this zinc rod was brightened and

Fig. 3. Fig. 4. Fig. 5.



sealed up on the tube so that the platinum wire projected (fig. 4, *a*). By the aid of a glass funnel with a flexible capillary stem (made by drawing out a piece of tubing before the blowpipe) paraffin-wax was introduced into the sealed-up limb so as to cover up completely the platinum wire and lower half of the zinc, leaving the other half exposed, *b*. In a similar way, recently boiled mercury was run into the other limb, previously sealed up with a second platinum wire passing through the glass, *c*. The stem of the inverted Y-tube was then carefully drawn out before the blowpipe (fig. 5, *m*), and connected by means of a short piece of india-rubber tubing, *n*, with the end of a glass tube projecting from the little flask, *p*, containing mercurous sulphate paste, and connected with the Sprengel pump. When a fairly good vacuum was obtained, the paste was made to boil by applying a very gentle heat; after about half an hour's boiling (the pump being at work the whole time) the connexion between the pump and flask was suddenly severed, when the sudden access of atmospheric pressure drove the paste into the cell, completely filling it: the pump was then again connected, and the boiling carried out again in the cell itself, and so on as before. Finally, by means of a blowpipe the drawn-out stem was sealed at *m*. To prevent the paste blocking up this drawn-out part, it was found necessary to use levigated particles of mercurous sulphate with a large proportion of zinc-sulphate solution; so that ultimately the cell contained much more fluid than solid matter. In order to use the cell, copper wires were soldered to the platinum wires projecting from the sealed ends of the inverted Y and bent over so as to dip into mercury-cups, the Y being either held by a clamp or imbedded in paraffin-wax, and of course being never allowed to be upset or shaken up so that the mercury could pass into the limb containing the zinc, which is otherwise liable to occur and spoil the cell.

136. The following numbers may be quoted as illustrations of the practical absence of any effect on the E.M.F. of the cell caused by the presence or otherwise of dissolved air. The values cited are the average readings, during a period of several months, of a dozen cells set up with cold-saturated zinc-sulphate solution well aerated, and *not* sealed up with melted paraffin-wax, but only loosely corked to avoid entrance of dust. Each cell during this period remained sensibly constant. All the values are reduced to the average reading (taken as 100·00), during the same period, of a yet larger number of cells prepared hot and sealed up precisely in accordance with Clark's directions—this average reading being the standard employed in the previous portions of these experiments, and especially in Parts V. and VI.

Batch of four cells made from mercurous sulphate purchased from Messrs. Hopkin and Williams	No. 1.	99·90
	" 2.	99·94
	" 3.	99·95
	" 4.	99·99
Batch of four cell made from mercurous sulphate prepared specially by ourselves for this purpose	" 5.	99·75
	" 6.	99·95
	" 7.	99·99
	" 8.	100·02
Batch of four cells made from another specimen of mercurous sulphate prepared by ourselves	" 9.	99·97
	" 10.	100·11
	" 11.	100·18
	" 12.	100·19
General average		99·995

Precisely analogous figures were obtained with several vacuum-prepared cells, no one of which gave a value outside of the limits 99·7 and 100·3, *i. e.* outside of the limits of fluctuation of the ordinary Clark's cells compared with them. On opening one of these vacuum-cells so as to admit air, a distinct fall in E.M.F., amounting to 0·25 volt, was observed; this behaviour, however, was not shown by other similar cells on opening.

Influence of Mercuric Sulphate in the Mercurous Sulphate.

137. However carefully the mercurous sulphate may be prepared, it is almost impossible to obtain it without some admixture of mercuric sulphate. During the boiling and washing by decantation this latter becomes a basic salt, the so-called "turpeth mineral," which possesses a bright yellow tint, and communicates to the mercurous sulphate a more or less pronounced yellowish tinge. In order to see how far the presence of varying quantities of this compound might possibly affect the E.M.F. of Clark's cell, several cells were set up in the same way as the hot-prepared cells described above, but using turpeth mineral only instead of mercurous sulphate. Two samples of turpeth mineral were employed:—one purchased (from Messrs. Hopkin and Williams), and well washed by boiling up many times with water and decantation before use; the other prepared by boiling mercury with a large excess of pure sulphuric acid, evaporating off most of the acid (which process converts all mercurous sulphate present into mercuric), adding to a large bulk of boiling water and washing many times the yellow heavy powder formed, by boiling up with water and decanting, so as to remove all traces of free sulphuric acid, and of the soluble acid mercuric sulphate also

formed. On taking a long series of readings of these cells, it was found that whilst the E.M.F. was, when the cell was newly set up, close to that of an average mercurous-sulphate cell, on standing a few days a distinct fall was observable, which went on progressively until, after some weeks, a diminution in the E.M.F. of between 3 and 4 per cent. was brought about, after which the fall ceased or became very languid. Thus the following average readings were obtained as before, the average E.M.F. of the hot-prepared cells containing saturated zinc-sulphate solution being taken as 100 when at the same temperature as the cells examined: cells A, B, C, and D were set up simultaneously with turpeth mineral prepared by ourselves; cells E and F simultaneously with the purchased substance. The zinc, zinc sulphate, and mercury used were the same as those used for the hot-prepared cells. Notwithstanding, however, that all the cells were as alike as possible, yet the rate of fall during the first few weeks was by no means identical.

Age of cell...	1 day.	2 to 6 days.	1 to 2 weeks.	6 weeks.	2 to 4 months.	6 to 20 months.
Cell A	100·6	100·12	99·95	98·35	97·04	97·00
„ B	100·3	100·31	100·11	99·65	98·18	97·64
„ C	100·4	100·22	99·90	99·19	97·38	96·96
„ D	100·6	100·46	99·80	97·88	97·39	97·44
Average	100·5	100·28	99·93	98·77	97·50	97·26
Cell E	99·4	98·85	97·27	......	96·78	95·80
„ F	99·5	99·41	98·13	......	97·11	96·00
Average......	99·45	99·13	97·70	......	96·95	95·90

It is evident from these figures that the effect of the presence of turpeth mineral in the mercurous sulphate used for Clark's cells is in the direction of decreasing the value; but inasmuch as the decrease is progressive, whilst no such alteration was observed in the Clark's cells examined, at any rate during several months after construction, it appears doubtful whether the variations in the E.M.F. of different Clark's cells set up at various times can be attributed to this cause.

Permanence of Clark's Cells.

138. A number of cells prepared in various ways (paste boiled and cells sealed with paraffin-wax; paste boiled *in vacuo* and cells hermetically sealed; set up with saturated zinc-sulphate solution, or with weaker solutions) were kept for periods of time ranging from a few months to two or three years, and

checked against one another from time to time, or compared with Daniell cells set up as described in Part V., with amalgamated pure zinc and electro-copper plates, and pure zinc- and copper-sulphate solutions of the same molecular strength*. No permanent changes in the values were observed (outside of the limit of the errors of observation) in the case of those cells which were so well sealed that neither evaporation took place, nor passage outwards of the fluid by capillary action through cracks in the sealing material. Vacuum-cells were thus kept unchanged for upwards of two years, as also were some normal Clark cells that were completely imbedded in paraffin-wax. In several cases, however, where the cells were not completely imbedded, but were only sealed up by a plug of paraffin-wax poured in at first round the zinc plate and the cork &c. supporting it, cracks formed sooner or later either in the paraffin-wax itself or between the glass and the wax, so that the fluid passed out through the cracks by capillary action and formed an efflorescence outside the cell. In some cases the action went on to such an extent as to leave the zinc wholly exposed, no contact finally existing between it and the paste: such cells were of course utterly spoilt. In other instances the zinc was only partially bared: in these cases the E.M.F. of the cell remained almost unaltered when saturated zinc-sulphate solution was employed in the first instance, but was lessened when unsaturated solution was originally used, owing to the evaporation and concentration which went on simultaneously with the capillary action, or subsequently to the commencement thereof. For example, two cells set up

* A large number of observations on the E.M.F. of Daniell cells have shown that, when proper precautions are taken in setting up the cells, a very considerable degree of constancy in value is attainable, so that such cells serve as good practical standards; but that if these precautions are neglected, *variations amounting to 5 per cent., and even more, may ensue.* The essential precautions are:—first, that pure solutions of zinc and copper sulphates containing no free acid should be used, each being of the same molecular strength (*i. e.* practically of the same specific gravity; conveniently the molecular strength may be near to $MSO_4, 50H_2O$); secondly, that the solutions should be in separate vessels, united when required by an inverted U-tube, the mouths of which are covered with thin bladder (Raoult's form of cell); thirdly, that the plates should be pure zinc amalgamated with pure mercury, and copper recently electro-deposited from pure sulphate solution—the wires serving as electrodes, and their junctions with the plates being coated with gutta-percha, so that no part of the plate or wire is simultaneously in contact with both fluid and atmosphere; and fourthly, that, if used to generate a current, the current-density must not exceed some 5 microampères per square centimetre, so that with plates exposing 20 square centimetres the total resistance in circuit must be *at least* 10,000 ohms, if exposing 10 square centimetres 20,000 ohms, and so on.

with zinc-sulphate solution about two thirds saturated gave the values 101·07 and 100·92 during the first few weeks after construction; cracks then formed, and efflorescence and evaporation took place, so that the zincs became partially bared, during which time the electromotive forces gradually sank. After some months the paste became covered with crystals of zinc sulphate, indicating that the residual solution moistening the mercurous sulphate was saturated: the electromotive forces were then 100·13 and 99·73 respectively, which values were subsequently retained almost constant for several months longer, notwithstanding that a considerable portion of each zinc rod was out of the paste and exposed to the air.

A number of observations made with cells containing zinc rods partly immersed in the paste and partly exposed to the air, gave sensibly the same average result as another series of observations made with the same cells when the zinc rods were pushed down so as to be wholly immersed (the upper end and the wire serving as electrode being protected from contact with the paste by gutta-percha).

It is specially to be noticed, in connexion with the question of the permanence of Clark's cells, that the cells experimented with were only used in connexion with the quadrant-electrometer; so that from first to last they *never generated any continuous current, nor had any current (however small) sent through in the inverse direction*—conditions impossible completely to realize in practice when working by the "method of opposition" or with the potentiometer.

Effect of Temperature on the E.M.F. of Clark's Cell.

139. According to Clark (Proc. Roy. Soc. xx. p. 444), the E.M.F. of a hot-prepared mercurous zinc-sulphate cell diminishes at an approximately constant rate of 0·06 per cent. per degree rise in temperature between 15°·5 and 100°; he states, however, that this figure might be verified with advantage. A number of observations having indicated, as a preliminary result, that this value is considerably too high between the temperature-range (10° to 25°) most frequently obtaining in practice, and that fairly constant results are given with different cells, the following experiments were made in order to determine more exactly the mean coefficient of alteration per degree between these temperature-limits, with the result of showing that, instead of Clark's number (0·0006) being deduced, a value but little above two thirds of this figure was obtained, viz. ·000411, as the average of ten experiments with five cells.

Let the E.M.F. of a given cell, taken temporarily as a standard, be 1 at temperature t_1 (near to 15°), and let the E.M.F. of a second cell compared therewith be a_1 when the cell compared is at a temperature t_2, the standard being still at t_1. In another experiment, when the standard is at a temperature t_3 not far from t_1, let the E.M.F. of the second cell be a_2, this cell being at the temperature t_4. Now let x be the mean coefficient of variation for 1° between t_2 and t_4 for the second cell, whilst x' is the analogous coefficient between t_1 and t_3 for the temporary standard. Then, since the E.M.F. of the standard at t_1 is unity, its E.M.F. at t_3 is $1-(t_3-t_1)x'$, whence the E.M.F. of the second cell at t_4 is $a_2\{1-(t_3-t_1)x'\}$. The E.M.F. of this second cell at t_4, however, is also

$$a_1\{1-(t_4-t_2)x\};$$

so that

$$a_1\{1-(t_4-t_1)x\}=a_2\{1-(t_3-t_1)x'\}.$$

Now, from Clark's experiments and certain preliminary observations made by ourselves, it results that x is approximately equal to x'; whilst if the temperatures are suitably chosen so that the mean of t_1 and t_3 is sensibly the same as the mean of t_2 and t_4, it must result that the difference between x and x' is very small; and, finally, if t_1 and t_3 differ but little in comparison with the difference between t_2 and t_4, any errors in the valuation of x' will be but small relatively. Hence, taking $x=x'$, it results that

$$x=\frac{a_1-a_2}{a_1(t_4-t_2)-a_2(t_3-t_1)}.$$

In order, then, to determine x, it is only necessary to determine the relative readings of two cells, first when one is at t_1 and the other at t_2 (say at 15° and 0° respectively), and secondly when the first is at t_3 and the second at t_4 (say at 14° and 30° respectively), the temperatures being such that t_1+t_3 approximately equals t_2+t_4 (as in the case of the supposed numbers).

To carry out this principle two water-jacketed metal chambers were constructed, furnished with delicate thermometers reading to 0°·01 C., and containing respectively the sets of cells to be compared, the poles of the cells being connected with the mercury-cup arrangement applied to the electrometer by means of covered wires passing through narrow glass tubes fixed in the double lids of the chambers, so that no conducting contact between the wires themselves or between the wires and lids &c. was possible. One of the water-jackets was filled with water at near 15°, the other with water either at or near 0° or at or near 30°, as the case might be; the masses of fluid (agitated from time to time with a peculiar stirrer) were so large that the temperature of the chamber-spaces varied but little during the progress of the series of readings ultimately made. The mean temperatures indicated by the thermometers during the series were taken as the mean temperatures of the cells (placed in the chambers some time before the readings were commenced, so as to attain sensibly the temperatures of the chamber-spaces). The readings were carried out in systematic order; so that the average reading for each cell should be exactly comparable with that of any other, notwithstanding any possible running-down of the electrometer-scale during the progress of the readings. For instance, if in the first chamber two cells (A and B) were placed, and in the second two others (C and D), the readings were alternately taken in the orders A, B, C, D and D, C, B, A, or C, D, A, B and B, A, D, C; so that the average reading for each cell was identical with that which would have been observed had the electrometer-scale value been absolutely constant throughout at its mean value (the actual variation of the electrometer-scale during any set of readings was considerably under 1 per cent.).

Thus, for instance, the following numbers were obtained in two experiments, in each of which the same two cells A and B were placed in the first chamber, and the same two (C and D) in the second:—

	1st experiment.	2nd experiment.
t_1	16°·84	17°·04
t_2	1°·08	3°·30
t_3	9°·72	10°·98
t_4	26°·02	25°·12
Average scale-reading for A and B taken together at t_1	159·94	159·62
Average reading for C at t_2	161·00	160·75
Average reading for A and B taken together at t_3 .	153·00	152·56
Average reading of C at t_4	152·25	152·00
a_1	$\frac{161}{159·94}=1·0066$	$\frac{160·75}{159·62}=1·0071$
a_2	$\frac{152·25}{153}=·9951$	$\frac{152}{152·56}=·9963$
$x=\frac{a_1-a_2}{a_1(t_4-t_2)-a_2(t_3-t_1)}$ . . .	·000358	·000386

Mean value of x = ·000372.

Similarly the values ·000439 and ·000428 (mean = ·000434) were obtained for x in the case of cell D simultaneously examined. The following Table exhibits in brief these figures and those obtained in six other experiments with three other different cells:—

	1st experiment.	2nd experiment.	Mean.
1st cell	·000358	·000386	·000372
2nd "	·000439	·000428	·000434
3rd "	·000480	·000481	·000481
4th "	·000436	·000397	·000417
5th "	·000364	·000336	·000350
	General average . . .		·000411

Hence, finally, it results that the E.M.F. of a Clark's cell set up with saturated zinc-sulphate solution is, at a temperature t not more than 10° or 12° above or below 15°·5 C.,

$$1{\cdot}457\{1-(t-15^\circ{\cdot}5)\times 0{\cdot}00041\} \text{ volt;}$$

it being admitted that Clark's valuation is exact, viz. 1·457 volt at 15°·5.

On the Work done during Electrolysis.

140. The experiments described in the previous portions of these researches have shown that, when a current is passed through an electrolytic cell, the amount of energy expended (positively or negatively) during the passage in performing a given amount of chemical work (apart from that transformed into heat in consequence of the resistance proper of the cell in accordance with Joule's law) is not constant, but *increases algebraically with the current-density*, in such wise that when the cell is an ordinary decomposing cell (*e. g.* a voltameter) the "counter electromotive force" of the cell increases in arithmetical value with the current-density, whilst when the cells is an electromotor (*i. e.* such a cell as to yield a *negative* counter E.M.F.), the arithmetical value of the negative counter E.M.F. (*i. e.* the direct E.M.F. of the cell) decreases with the current-density. The extra work done by a stronger current as compared with a weaker one in the former case, and the deficiency of work corresponding with the fall in direct E.M.F. in the latter case, make their appearance in the form of sensible heat in the cell.

Experiments have been published by Favre (*vide* Part I. §§ 14 and 15) which appear to show that certain forms of electromotor-cells can generate currents capable of doing more work externally to the cell than corresponds with the net chemical action taking place, this extra work being gained at the expense of the sensible heat of the cell, which becomes *cooled* by the passage of a current of too small magnitude to generate, in accordance with Joule's law, sufficient heat in the cell to overpower this cooling action. Inasmuch, however, as the mercury-calorimeter was employed in these experiments of Favre, whilst, from the nature of the case, but feeble currents passed, so that the total amount of chemical action in a given time could be but small, it seems not unlikely that an excessively large probable error attends the numerical values obtained. In point of fact, one of the cells found by Favre to behave in this way was Grove's cell; and his results in this repect are totally at variance with all other experiments on the subject (compare H. F. Weber, Phil. Mag. 1878, v. p. 195), leading to the conclusion that the supposed cooling action was not a real effect, but simply the result of the accumulation of experimental errors. In order to see whether this was also the case with the other cells examined by Favre, the following experiments were made.

These other cells were simple voltaic couples of zinc and platinum or cadmium and platinum immersed in dilute hydrochloric acid; the numbers obtained by Favre as the cooling effects per gramme equivalent of metal dissolved were respectively 1051 and 1288 gramme-degrees, corresponding with ·046 and ·057 volt*. On the other hand, with dilute sulphuric acid in lieu of hydrochloric, Favre found that no cooling action was traceable, but that the cells were always warmed by the passage of a current. Now these results, if correct, must imply that the E.M.F. of a zinc-platinum or a cadmium-platinum cell, when generating only a minute current, is above the value corresponding with the heat-development due to the net chemical action taking place when hydrochloric-acid solution is the exciting fluid, and below that value when dilute sulphuric acid is used instead; *i. e.* the electromotive forces of cells containing dilute hydrochloric acid must be above ·754 and ·388 volt respectively with zinc-platinum and cadmium-platinum couples, and the electromotive forces of cells containing dilute sulphuric acid must be below ·835 and ·470 volt respectively with these same couples, these being the values in E.M.F. corresponding respectively with the heat-developments per gramme-equivalent in the reactions

* For the sake of comparison with the experiments described in the previous portions of these researches, the factor 4410 for converting gramme-degrees into volts is adhered to, notwithstanding that the balance of evidence now seems to indicate that the value of J hitherto assumed (42 megalergs) is somewhat too high, and that the B.A. unit of resistance is upwards of 1 per cent. below its intended value, instead of being exact as hitherto assumed.

$$Zn + 2HCl = ZnCl_2 + H_2,$$
$$Cd + 2HCl = CdCl_2 + H_2,$$
$$Zn + H_2SO_4 = ZnSO_4 + H_2,$$
$$Cd + H_2SO_4 = CdSO_4 + H_2,$$

these heat-developments being, per gramme-molecule, as follows*:—

Zn, Cl_2, aq.	=112840	Cd, Cl_2, aq.	=96250
H_2, Cl_2, aq. . . .	= 78640	H_2, Cl_2, aq. . . .	=78640
Difference . .	34200	Difference . .	17610
Diff. per gramme-equivalent	17100	Diff. per gramme-equivalent	8805
Corresponding with volt	·754	Corresponding with volt	·388
Zn, O, SO_3, aq. . .	=106090	Cd, O, SO_3, aq. . .	=89500
H_2, O	= 68200	H_2, O	=68200
Difference . .	37890	Difference . .	21300
Diff. per gramme-equivalent	18945	Diff. per gramme-equivalent	10650
Corresponding with volt	·835	Corresponding with volt	·470

141. In order to see whether the electromotive forces actually developed by these four voltaic combinations are really above the calculated values in the first two cases and below in the second two instances, when the disturbing effects of dissolved air are eliminated, cells were set up like those described in § 85, and caused to generate feeble currents by employing large external resistances. In all cases it was found that when the errors due to dissolved air were eliminated and the readings became constant, the E.M.F. actually developed *invariably fell short of the value corresponding with the net chemical action* by an amount which increased with the current-density until the reduction became a large fraction of the E.M.F. observed with the smallest possible densities. With hydrochloric-acid cells the deficiency was not so great in the first instance, and

* These figures are deduced from Julius Thomsen's thermochemical data and the mean value for the heat of formation of water arrived at in § 31. Thomsen's values relate to the degree of dilution MCl_2, 400 H_2O, and MSO_4, 400 H_2O. Some experiments made by us on the amounts of heat evolved on diluting stronger solutions of zinc and cadmium chlorides and sulphates indicate that these values require slight corrections for stronger solutions than those used by Thomsen; but the alterations thus produced in the net heat-development and in the E.M.F. corresponding thereto is but small.

the rate of increase in deficiency was not so rapid, as with sulphuric-acid cells. Thus the following four experiments may be cited as illustrations of the results obtained in numerous cases:—

Current, in microampères, =C.	Current-density, in microampères, per square centimetre.	Observed differences of potential between plates =E.	Value of CR.	E.M.F. of cell, e=E+CR.
Hydrochloric Acid: Zinc and Platinum.				
12·6	1·6	·633	......	·633
23·4	2·9	·628	·001	·629
55·1	6·9	·609	·002	·611
102·4	12·8	·585	·003	·588
224·5	28·1	·545	·007	·552
		Calculated E.M.F. .... = ·754		
Hydrochloric Acid: Cadmium and Platinum.				
6·5	0·8	·347	......	·347
11·0	1·4	·291	·001	·292
14·8	1·85	·249	·001	·250
33·7	4·2	·161	·003	·164
54·3	6·8	·130	·005	·135
97·8	12·2	·103	·010	·113
		Calculated E.M.F. .... = ·388		
Sulphuric Acid: Zinc and Platinum.				
12·6	1·6	·626	......	·626
23·4	2·9	·540	·001	·541
55·1	6·9	·492	·002	·494
102·4	12·8	·439	·003	·442
224·5	28·1	·353	·007	·360
		Calculated E.M.F. .... = ·835		
Sulphuric Acid: Cadmium and Platinum.				
6·5	0·8	·301	......	·301
11·0	1·4	·259	·001	·260
14·8	1·85	·211	·001	·212
33·7	4·2	·080	·003	·083
54·3	6·8	·033	·005	·038
97·8	12·2	·019	·010	·029
		Calculated E.M.F. = ... ·470		

In each of these experiments the plate-surface was constantly 8 square centimetres; the hydrochloric-acid solution was close to $2\,HCl, 50\,H_2O$ and $2\,HCl, 100\,H_2O$ in the first and second experiments respectively, and the sulphuric-acid $H_2SO_4, 50\,H_2O$ and $H_2SO_4, 100\,H_2O$ in the third and fourth experiments respectively. The zinc plates were amalgamated, the cadmium ones not.

142. The experiments described in Parts IV., V., and VI. indicate that the amount of diminution brought about in the E.M.F. of an electromotor (either a simple cell, or one after Daniell's construction) by an increase in the current-density may readily greatly exceed any possible effect due to the accumulation round the two plates of fluids of widely different molecular strength, and, further, that, as a general rule, the effect of diminishing the area of the plate on which the metal is deposited is considerably greater than that of a similar diminution in the area of the other plate, although this is not invariably the case. It is hence evident that the chief source of nonadjuvancy especially lies in the incomplete manifestation as electricity of the energy due, after the elimination by the action of the current of the deposited metal (or body equivalent thereto) in the nascent form, to the subsequent transformation thereof into the permanent form. Clearly the same kind of thing must be equally true for the other products of electrolysis evolved at the other electrode. Hence the reason why a less amount of non-adjuvancy is brought about at this side is presumably the greater amount of attraction exercised by the material of the electrode for the nascent product ("sulphion" of Daniell in the case of cells containing sulphates) here evolved, owing to their opposite chemical characters, than is observable at the other electrode. Admitting this to be so, it should result that the more oxidizable the metal dissolved (*i. e.* the greater the heat of formation of the compound produced by its solution), the less will be the amount of nonadjuvancy due to the incomplete conversion into electricity at this plate of the energy due to transformation of nascent into final products. The results of the experiments hitherto described, however, being complicated by the formation of solutions of different strengths around the two plates, are not sufficiently precise to show that, under given conditions, a zinc plate, for example, causes less nonadjuvancy than a cadmium one, and so on. Accordingly the following experiments on the point were made, the result of which is to show indisputably that the more oxidizable the metal the less the nonadjuvancy.

An electrolytic cell was constructed, consisting of a wide glass tube closed by india-rubber bungs through which passed wires terminating interiorly in the plates to be experimented with, the opposed plate-surfaces being perpendicular to the axis of the tube and therefore parallel to one another, and the anterior portions of the plates and the wires being thickly coated with gutta-percha. The tube was then filled, for instance, with concentrated zinc-sulphate solution, with plates of zinc at an accurately known distance apart, and was kept at a temperature sensibly uniform. A series of currents of various strengths was then passed through the cell, and the difference of potential subsisting between the plates determined in each case. These values represented the numerical values of $e_1 + CR$, where e_1 is the counter E.M.F. set up during the electrolysis, C the current, and R the resistance of the cell; and from them the values of this expression for definite values of C (50, 100, 200 microampères, &c.) were readily calculated by interpolation. The + zinc electrode was then removed, and a copper plate exposing exactly the same area placed in precisely the same position. The observations were then repeated, the temperature being the same as before, and a new series of values, $e_2 + CR$, calculated, e_2 being the counter E.M.F. now set up for a given value of C. Since R is constant throughout, it is evident that the difference between the two values for a given current obtained, first with a zinc, and secondly with a copper + electrode, represents $e_2 - e_1$. Now necessarily both e_1 and e_2 increase with the value of C in accordance with the general law to that effect deduced from all the previous observations (§ 133); but if it be true that a less production of heat instead of electricity is brought about when nascent sulphion is liberated in contact with zinc than when in contact with copper, e_1 must increase less rapidly with the current than e_2, and hence the value of $e_2 - e_1$ *must rise with the current-strength.* Precisely this result was observed in every case: for example, the following numbers were obtained in a pair of sets of observations carried out as described, the area of the plates being 0·50 square centim. throughout.

+ Zinc electrode.		+ Copper electrode.	
C.G.S. current.	Observed potential-difference.	C.G.S. current.	Observed potential-difference.
·00000436	·018	00000466	1·073
·00000866	·039	00000883	1·089
·00001432	·062	00001460	1·127
·00002130	·091	00002275	1·159
·00004160	·174	00004450	1·251

From these figures the following are obtained by interpolation:—

Current.	Potential-difference.		
	+ Zinc.	+ Copper.	e_2-e_1.
·000005	·021	1·075	1·054
·00001	·045	1·101	1·056
·00002	·085	1·147	1·062
·00004	·168	1·232	1·064

Precisely similar results were obtained in numerous other analogous experiments. Thus the following Table illustrates some of the figures obtained, the + zinc plate originally employed being replaced by a plate of the same size, I. of bright copper, II. of electro-copper, III. of amalgamated copper, IV. of bright cadmium, V. of bright silver.

C.G.S. current.	Values of e_2-e_1 obtained.				
	I.	II.	III.	IV.	V.
·000005	1·063	1·054	1·065	......	1·486
·00001	1·067	1·057	1·076	......	1·498
·00002	1·073	1·061	1·084	·315	1·503
·00004	1·075	1·068	1·099	·324	1·512

143. A still better illustration of the regular rise in value of e_2-e_1 with the current is afforded by the following series of numbers obtained as the average results of several sets of observations very carefully made—A with a bright zinc + electrode, B with one of bright cadmium, C with one of bright copper, and D with one of bright silver. In every case the mean temperature was the same within two or three tenths of a degree (varying from 17°·55 to 17°·9 throughout). In the last case it was found that, whilst perfectly steady readings could be obtained with current-strengths up to something like ·0007, with higher strengths this was no longer the case, silver peroxide being apparently formed instead of silver sulphate. In these experiments all the plates exposed an area of 1·5 square centim., the solution electrolyzed being a nearly saturated one of pure zinc sulphate, renewed for each series; the plates were about 5 centim. apart, the tube holding them being 3 centim. in internal diameter.

C.G.S. current.	Difference of potential set up.			
	A.	B.	C.	D.
·00002	·029	·317	1·069	1·490
·00005	·044	·334	1·086	1·509
·0001	·063	·354	1·107	1·530
·0002	·084	·381	1·139	1·562
·0005	·146	·451	1·210	1·636
·001	·230	·547	1·310	
·0015	·311	·636	1·403	
·002	·389	·730	1·498	
·0025	·476	·830	1·598	

These figures yield the following six sets of values of e_2-e_1 for the corresponding pairs of + electrodes compared.

Current.	Zinc-cadmium.	Zinc-copper.	Zinc-silver.	Cadmium-copper.	Cadmium-silver.	Copper-silver.
·00002	·288	1·040	1·461	·752	1·173	·421
·00005	·290	1·042	1·465	·752	1·175	·423
·0001	·291	1·044	1·467	·753	1·176	·423
·0002	·297	1·055	1·478	·758	1·181	·423
·0005	·305	1·064	1·490	·759	1·185	·426
·001	·317	1·080	......	·763		
·0015	·325	1·092	......	·767		
·002	·341	1·109	......	·768		
·0025	·354	1·122	......	·768		

Not only does the value of e_2-e_1 increase with the current-density in every case, but, further, the rate of increase is greater when zinc is compared with silver than with copper, and greater then than when compared with cadmium; similarly the rate of increase with cadmium and silver is greater than with copper and silver, and so on. It is further noticeable that in each case a value of e_2-e_1 with some particular current-strength is deducible which is sensibly the same as the E.M.F. of a cell after Daniell's construction containing the same metals and sulphate solutions of equal molecular strength; so that in general it may be said that, for a current-density below a particular limit, the value of e_2-e_1 is less than that of the corresponding Daniell form of cell, whilst for a current-density above this limit it is greater.

144. The following experiment seems to show that the substitution of dilute sulphuric acid for zinc-sulphate solution as the electrolyte makes no material difference in the end result, the − electrode being made of platinum, and the disturbing influence of dissolved air being eliminated. Two precisely similar U-tube cells (§ 85) were filled with recently boiled dilute sulphuric acid (11·5 grammes H_2SO_4 per 100 cubic centim.), and

fitted with uniformly sized plates (8 square centim. total surface in each case) at an equal distance asunder, so that the resistance of the cell should be sensibly the same in each case. In the first cell the plates were of zinc (amalgamated) and platinum, and in the second of copper and platinum respectively; the two were arranged in series with a couple of Leclanché cells, so that the platinum plates were necessarily the — electrodes; a large variable resistance being included in the circuit, the current could be regulated at pleasure. A current of some fifty microampères being sent through for three days, the readings became steady when all the dissolved air around the platinum plates was eliminated; the current was then varied from time to time, and a series of readings of the potential-difference between each pair of plates taken. By interpolation as before, the following figures were then deduced from the average values.

Current in micro-ampères.	Micro-ampères per square centim.	Difference of potential.		e_2-e_1.
		+ zinc.	+ copper.	
20	2·5	−·552	+·449	+1·001
40	5·0	−·558	+·448	+1·006
80	10·0	−·498	+·521	+1·019

The value of e_2-e_1 consequently increases with the current-density as before. The numerical values observed in this experiment are somewhat lower than those found in the experiments above described, as might be expected, since the largest current-density employed in this case, being only 10 microampères per square centim., is considerably below the smallest cited in the previous observations, in the last of which a minimum current of ·00002 C.G.S. units (or 200 microampères) was employed with plate-surfaces of 1·5 square centim., giving a density of 133·3 microampères per square centim., in which case the value of e_2-e_1 was 1·040; whilst in the former experiments a minimum current of ·000005 C.G.S. unit (50 microampères) was employed with a plate-surface of ·50 square centim., giving a density of 100 microampères per square centim., when values of from 1·054 to 1·065 were observed.

145. Some experiments were also made with analogous pairs of cells in which the + electrodes were made of metals not attacked by the nascent products arising from the electrolysis of sulphates, *e. g.* gold and platinum. In these instances it was found that platinum behaved in reference to gold just

as a more readily to a less readily oxidizable metal, this result being evidently brought about by the superior surface condensing-power possessed by platinum, in virtue of which a greater proportion of the energy due to the transformation of the nascent into the final products of electrolysis evolved at the + electrode becomes adjuvant. For instance, the following numbers were obtained with a pair of precisely similar cells containing the same copper-sulphate solution and copper − electrodes.

Current-density, microampères per square centimetre.	Difference of potential between plates.		Difference.
	+Platinum.	+Gold.	
3·0	1·500	1·555	·055
7·0	1·534	1·591	·057
11·0	1·570	1·630	·060

Even with the lowest current-density and with platinum as + electrode the total amount of nonadjuvancy was here considerable; for the E.M.F. corresponding with the net chemical action is only 1·234 volt ($\frac{1}{2}$[Cu, O, SO_3 aq] = 27,980 gramme-degrees = 1·234 volt); and the minimum difference of potential set up, after correction for the resistance of the cell (*i. e.* the counter E.M.F. set up, or the value of the term e in the expression $E = e + CR$), exceeds 1·490, since the term CR in this case was much less than ·010 volt.

In just the same kind of way, when platinum and gold were respectively made the − electrodes in similar pairs of cells containing dilute sulphuric acid and a constant oxidizable + electrode, the superior surface condensing-power possessed by platinum caused a less degree of nonadjuvancy during the transformation of nascent into free hydrogen. Thus, for example, the following numbers were obtained with a copper + electrode and acid containing 10 per cent. of H_2SO_4.

Current-density, microampères per square centimetre.	Difference of potential between plates.		Difference.
	−Platinum.	−Gold.	
2·5	·449	·575	·126
5·0	·488	·619	·131
10·0	·521	·661	·140

Here again, even in the most favourable instance, with the smallest current-density and platinum as — electrode, a conderable amount of nonadjuvancy subsisted; for the value of CR in this case was not greater than ·001; so that the minimum counter E.M.F. set up was at least ·448 volt, whilst the E.M.F. corresponding to the net chemical action is only ·270 volt, the heat-development being per gramme equivalent

$$\begin{array}{lrl} \tfrac{1}{2}(H_2, O) \quad . \quad . & = 34100 & \text{gramme-degrees.} \\ \tfrac{1}{2}(Cu, O, SO_3\,aq) & = 27980 & \quad ,, \qquad ,, \\ \hline & 6120 & \quad ,, \qquad ,, \quad = \cdot 270 \text{ volt.} \end{array}$$

It is hence evident, *à fortiori*, that when acidulated water is decomposed with two gold electrodes, the counter E.M.F. set up must be much greater for a given current-density than when two platinum electrodes are used, the deficiency in condensing-power being then manifest at both electrodes simultaneously. The experiments described in Part IV. § 90 have shown that this is the case.

146. In addition to the experiments above described as examples, a large number of analogous observations have been made with varying kinds of electrolytic solutions and electrodes, and with varying strengths of solutions. The general results of these experiments, so far as at present completed, may be thus summarized.

(1) When an electrolytic cell is of such a nature that the counter E.M.F. set up is negative (*i. e.* when the cell is an electromotor), it is always found that *the E.M.F developed is less the greater the density of the current generated.* With very small current-densities the E.M.F. has a maximum value which in certain cases (*e. g.* Daniell's cell and the analogous zinc-cadmium and cadmium-copper cells described in Part VI.) is substantially identical with the E.M.F. corresponding with the heat-development due to the net chemical action taking place in the cell, *i. e.* with the E.M.F. representing the algebraic sum of the chemical affinities involved. In certain other cases (*e. g.* the zinc-silver, cadmium-silver, and copper-silver cells described in Part VI.) the maximum E.M.F. developed is sensibly *below* that due to the net chemical action.

(2) Some kinds of combinations have been found to be capable of existing which can develop a *greater* E.M.F. than that due to the net chemical action (although the particular cells described by Favre as possessing this property are not really cases in point, Favre's results being due to experimental errors); amongst such combinations may be mentioned several where *lead* is the metal dissolved, *i. e.* lead-copper cells

charged with solutions of acetates. It is noticeable that in such cases Volta's law of summation holds, the sum of the electromotive forces of two cells, one containing zinc and lead and the other lead and copper, being equal to the E.M.F. of a zinc-copper cell, the E.M.F. of the first cell being just as much below the amount calculated from the heat-development as that of the second is above the amount similarly calculated. This class of cells is now undergoing careful examination, and will be dealt with in a subsequent paper. Unfortunately, progress in this direction during the last fifteen months has been greatly retarded by the refusal of the Administrators of the Government Fund of £4000 to continue the grants by the aid of which the previous portions of these researches have mainly been made, on account of which circumstance numerous other points of interest that have cropped up have necessarily remained uninvestigated *.

(3) When the electrolytic cell is not an electromotor, the counter E.M.F. set up (positive) always *increases in amount with the current-density*. When the + electrode is of such a nature as to combine with the products of electrolysis evolved thereat, other things being the same, *the rate of increase is slower the greater the chemical affinity* between the nascent products of electrolysis evolved at the + electrode and the material of which that electrode is composed; *i. e.* the greater the affinity, the less the degree of nonadjuvancy brought about at the + electrode.

(4) Whether the cell be an electromotor or not, there is always (with currents not so small as to be practically infinitesimal) a greater or less degree of nonadjuvancy brought about at the − electrode, owing to the development of heat in lieu of electricity during the transformation of nascent into ultimate permanent products of electrolysis. In many cases this source of nonadjuvancy decidedly predominates over that at the + electrode.

(5) The particular extent to which the nonadjuvancy reaches at either electrode appears to be a complex function not only of the chemical nature of the electrode, the physical conditions of its surface, and the character of the nascent products of electrolysis evolved thereat, but also of the temperature, and the degree of concentration of the solution electrolyzed, and

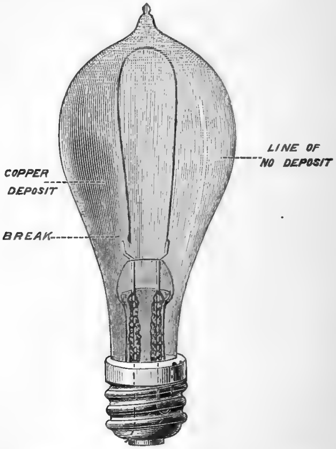
* Since the presentation to the Physical Society of Part VI. of these researches, a paper has appeared by F. Braun (*Annalen der Phys. u. Chem.* xvi. p. 561), in which the author shows that various combinations examined by him give electromotive forces sensibly the same as those calculated from thermochemical data, whilst others fall short of, and some exceed, the calculated values.

possibly of other conditions besides. Other things being equal, it appears to be a general rule that *the weaker the solution, the greater the degree of nonadjuvancy.* When a gas is one of the permanent products of electrolysis at either electrode, *the greater the surface condensing-power of the material of which the electrode is composed, the less is the degree of nonadjuvancy.*

VI. *On a Phenomenon of Molecular Radiation in Incandescence Lamps.* *By* J. A. Fleming, *B.A., D.Sc.**

NOT long ago a curious phenomenon came under my notice in connexion with the burning of Edison incandescence lamps, which presents sufficient interest to warrant my drawing the attention of physicists to it.

As is well known, the carbon filament in the Edison lamp is of a horse-shoe form. The two extremities of the loop are



clamped into small copper clamps on the ends of the platinum wires, which are sealed through the glass. The ends of the carbon loop are electroplated over with copper at the place where they are connected to the clamp in order to make a

* Communicated by the Physical Society, having been read at the Meeting on May 26, 1883.

good contact. If this precaution is omitted, a loose contact may be formed, the result of which will be a generation of heat at that point.

In the ordinary working the life-history of a carbon filament is something as follows:—

At some point or other the filament is probably thinner than at other places. At this place there will be a greater generation of heat and a higher temperature; volatilization of the carbon ensues, and the vapour condenses on the sides of the glass bulb, as far as I have observed, uniformly. If, however, the point of greatest resistance occurs on the copper clamp, then it is found that copper volatilizes and deposits on the inside of the glass.

But what is most curious is, that in this case an examination of the glass envelope shows that there is a narrow line along which no copper has been deposited. This is seen best by holding the lamp up before the light and slowly turning it round. In one particular position, easily found, it is best seen. Now, on examining carefully the position of the line of no deposit as compared with the position of the carbon filament, it will be seen that it lies in the plane of the loop, and on the opposite side to that nearest to which the break of the loop has occurred. It is in fact *a shadow of the loop*.

The conclusion which must be arrived at, then, is that the copper molecules are shot off in straight lines; otherwise it is impossible that there should be this line of no deposit.

The most noticeable thing is, that it occurs only when the deposition of copper takes place; I have never noticed it in an ordinary carbon deposit.

Hence there must be some essential difference between the vaporization of the carbon and that of the copper. The carbon deposit resembles more the condensation of a vapour and is uniformly distributed; but the copper deposit exhibits the character of a molecular radiation or shower taking place from a certain point.

The whole phenomenon calls at once to mind the beautiful researches of Mr. Crookes with vacuum-tubes. Here, however, we are dealing not with an induction-coil discharge, but with a comparatively low potential.

I have never failed to see the effect in any lamp which has had a deposition of copper on its interior.

It is interesting to note how nearly the colour of transparent copper resembles that of transparent gold. The similarity of the surface-colour of pure unoxidized copper and of gold is accompanied by a near resemblance in colour of the two metals in thin films.

VII. *On the Crispations of Fluid resting upon a Vibrating Support.* *By* Lord Rayleigh, *D.C.L., F.R.S., Cavendish Professor of Physics in the University of Cambridge**.

If a glass plate, held horizontally, and made to vibrate as for the production of Chladni's figures, be covered with a thin layer of water or other mobile liquid, the phenomena in question may be readily observed. Over those parts of the plate which vibrate sensibly the surface of the liquid is ruffled by minute waves, the degree of fineness increasing with the frequency of vibration. The same crispations are observed on the surface of liquid in a large wine-glass or finger-glass which is caused to vibrate in the usual manner by carrying the moistened finger round the circumference. All that is essential to the production of crispations is that a body of liquid with a free surface be constrained to execute a vertical vibration. It is indifferent whether the origin of the motion be at the bottom, as in the first case, or, as in the second, be due to the alternate advance and retreat of a lateral boundary, to accommodate itself to which the neighbouring surface must rise and fall.

More than fifty years ago the nature of these vibrations was examined by Faraday with great ingenuity and success. His results are recorded in an Appendix to a paper on a Peculiar Class of Acoustical Figures†, headed "On the Forms and States assumed by Fluids in Contact with vibrating Elastic Surfaces." In more recent times Dr. L. Matthiessen has travelled over the same ground‡, and on one very important point has recorded an opinion in opposition to that of Faraday. In order more completely to satisfy myself, I have lately repeated most of Faraday's experiments, in some cases with improved appliances, and have been able to add some further observations in support of the views adopted.

The phenomenon to be examined is evidently presented in its simplest form when the motion of the vibrating horizontal plate on which the liquid is spread is a simple up-and-down motion without rotation. To secure this, Faraday attached the plate to the centre of a strip of glass or lath of deal, supported at the nodes, and caused to vibrate by friction. In my experiments an iron bar was used about 1 metre long and ·0064 metre thick (in the plane of vibration). The bar was supported horizontally at the nodes; and to its centre a glass plate was attached by gutta-percha and carefully levelled.

* Communicated by the Author.

† Phil. Trans. 1831.

‡ Pogg. *Ann.* t. cxxxiv. 1868, t. cxli. 1870.

The vibrations of the bar were maintained electromagnetically, as in tuning-fork interrupters, with the aid of an electro-magnet placed under the centre, the circuit being made and broken at a mercury-cup by a dipper carried at one end of the bar. By calculation from the dimensions*, and without allowance for the load at the centre, the frequency of (complete) vibration is 33. Comparisons with a standard tuning-fork gave more accurately for the actually loaded bar a frequency of 31.

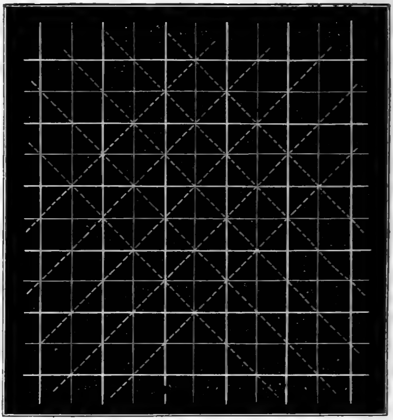
The vibrating liquid standing upon the plate presents appearances which at first are rather difficult to interpret, and which vary a good deal with the nature of the liquid in respect of transparency or opacity, and with the incidence of the light. The vibrations of the liquid, whether at the rate of 31 per second, or, as in fact, at the rate of $15\frac{1}{2}$ per second, are too quick to be followed by the eye; and thus the effect observed is an average, due to the superposition of an indefinite number of components corresponding to the various phases of vibration.

The motion of the liquid consists of two sets of stationary vibrations superposed, the ridges and furrows of the two sets being perpendicular to one another, and usually parallel to the edges of the (rectangular) plate. Confining our attention for the moment to one set of stationary waves, let us consider what appearance it may be expected to present. At one moment the ridges form a set of parallel and equidistant lines, the interval being the wave-length. Midway between these are the lines which represent at that moment the position of the furrows. After the lapse of $\frac{1}{4}$ period, the surface is flat; after another $\frac{1}{4}$ period, the ridges and furrows are again at their maximum development, but the *positions are exchanged*. Now, since only an average effect can be perceived, it is clear that no distinction can be recognized between the ridges and the furrows, and that the observed effect must be periodic within a distance equal to *half* a wave-length of the real motion. If the liquid on the plate be rendered moderately opaque by addition of aniline blue, and be seen by diffused transmitted light, the lines of ridge and furrow will appear bright in comparison with the intermediate nodal lines where the normal depth is preserved throughout the vibration. The gain of light when the thickness is small will, in accordance with the law of absorption, outweigh the loss of light which occurs half a period later when the furrow is replaced by a ridge.

The actual phenomenon is more complicated in consequence

* 'Theory of Sound,' § 171.

of the coexistence of the two sets of ridges and furrows in perpendicular directions (x, y). In the adjoining figure the thick



lines represent the ridges, and the thin lines the furrows, of the two systems at a moment of maximum excursion. One quarter period later the surface is flat, and one half a period later the ridges and furrows are interchanged. The places of maximum elevation and depression are the intersections of the thick lines and of the thin lines, not distinguishable by ordinary vision; and these regions will appear like holes in the sheet of colour. The nodal lines, where the normal depth of colour is preserved, are shown dotted; they are inclined at 45°, and pass through the intersections of the thick lines with the thin lines. The pattern is recurrent in the directions of both x and y, and in each case with an interval equal to the real wave-length (λ). The distance between the bright spots measured parallel to x or y is thus λ; but the shortest distance between these spots is in directions inclined at 45°, and is equal to $\frac{1}{2}\sqrt{2}\,.\,\lambda$.

In order to determine the relation of the frequency of the liquid vibrations to that of the bar, an apparatus was fitted up capable of giving an intermittent view of the vibrating system. This consisted of a blackened paper disk pierced with three sets of holes, mounted upon an axle, and maintained in rotation by a small electromagnetic engine of Apps's construction. The whole was fastened to one base-board, and could be moved about freely, the leading wires from the battery being flexible.

The current was somewhat in excess; so that the desired speed could be attained by the application of moderate friction. At a certain speed of rotation the appearances were as follows. Through the set of four holes (giving four views for each rotation of the disk) the bar was seen double. Through the set of two holes the bar was seen single, and the water-waves were seen double. Through the single hole the bar was seen single, and the waves also were seen single. From this it follows that the water vibrations are not, as Matthiessen contends, synchronous with those of the bar, but that there are two complete vibrations of the support for each complete vibration of the water, in accordance with Faraday's original statement.

An attempt was made to calculate the frequency of liquid vibration from measurements of the wave-length and of the depth. The depth (h), deduced from the area of the plate and the whole quantity of liquid, was ·0681 centim.; and by direct measurement $\lambda = {\cdot}848$ centim. Sir W. Thomson's formula connecting the velocity of propagation with the wave-length, when the effect of surface-tension is included, is

$$v^2 = \frac{\lambda^2}{\tau^2} = 982\left(\frac{\lambda}{2\pi} + \frac{{\cdot}074 \times 2\pi}{\lambda}\right) \times \frac{e^{\alpha} - e^{-\alpha}}{e^{\alpha} + e^{-\alpha}}, \quad . \quad . \quad \text{(A)}$$

where $\alpha = 2\pi h/\lambda$. With the above data we find for the frequency of vibration (τ^{-1}) 20·8. This should have been 15·5; and the discrepancy is probably to be attributed to friction, whose influence must be to diminish the efficient depth, and may easily rise to importance when the total depth is so small.

Another method by which I succeeded in determining the frequency of these waves requires a little preliminary explanation. If $n = 2\pi/\tau$, and $\kappa = 2\pi/\lambda$, the stationary waves parallel to y may be expressed as the resultant of opposite progressive waves in the form

$$\cos(\kappa x + nt) + \cos(\kappa x - nt) = 2\cos\kappa x \cos nt. \quad . \quad . \quad (1)$$

This represents the state of things referred to an origin fixed in space. But now let us refer it to an origin moving forward with the velocity (n/κ) of the progressive waves, so as to obtain the appearance that would be presented to the eye, or to the photographic camera, carried forward in this manner. Writing $\kappa x' + nt$ for κx, we get

$$\cos(\kappa x' + 2nt) + \cos\kappa x'. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Now the average effect of the first term is independent of x', so that what is seen is simply that set of progressive waves which moves with the eye. In this way a kind of resolution of the stationary wave into its progressive components may be effected.

In the actual experiment two sets of stationary waves are combined; and the analytical expression is

$$\cos(\kappa x+nt)+\cos(\kappa x-nt)+\cos(\kappa y+nt)+\cos(\kappa y-nt), \quad . \quad (3)$$

which is equal to

$$2\cos\kappa x\ \cos nt+2\cos\kappa y\ \cos nt, \quad . \quad . \quad . \quad . \quad (4)$$

or to

$$4\cos\frac{\kappa(x+y)}{2}\cos\frac{\kappa(x-y)}{2}\cos nt. \quad . \quad . \quad . \quad . \quad (5)$$

If, as before, we write $\kappa x'+nt$ for κx, we get

$$\cos(\kappa x'+2nt)+\cos\kappa x'+2\cos\kappa y\cos nt. \quad . \quad . \quad (6)$$

The eye, travelling forward with the velocity n/κ, sees mainly the corresponding progressive waves, whose appearance, however, usually varies with y, i. e. along the length of a ridge or furrow. If the effect could be supposed to depend upon the *mean* elevation only, this complication would disappear, as we should be left with the term $\cos\kappa x'$ standing alone. With the semi-opaque coloured water the variation along y is evident enough; but the experiment may be modified in such a manner that the ridges and furrows appear sensibly uniform. For this purpose the coloured water may be replaced by milk, lighted from above, but very obliquely. The appearance of a set of (uniform) ridges and furrows varies greatly with the direction of the light. If the light fall upon the plate in a direction nearly parallel to the ridges, the disturbance of the surface becomes almost invisible; but if, on the other hand, the incidence be perpendicular to the line of ridges, the disturbance is brought into strong relief. The application of this principle to the case before us shows that, when the eye is travelling parallel to x, the ridges and furrows will look nearly uniform if the incidence of the light be also nearly parallel to x; but if the incidence of the light be nearly parallel to y, the ridges will show marked variations along their length, and in fact be resolved into a series of detached humps. The former condition of things is the simplest, and the most suitable as the subject of measurement.

In order to see the progressive waves it is not necessary to move the head as a whole, but only to turn the eye as when we look at an ordinary object in motion. To do this without assistance is not at first very easy, especially if the area of the plate be somewhat small. By moving a pointer at various speeds until the right one is found, the eye may be guided to do what is required of it; and after a few successes repetition becomes easy. If we wish not merely to see the progressive waves, but to measure the velocity of propagation with some approach to accuracy, further assistance is required. In my experiments an endless string, passing over pulleys and driven by a small water-engine, travelled at a small distance above the plate so that its length was in the direction of wave-propagation. A piece of wire was held at one end by the fingers, and at the other rested upon the travelling string and was carried forward with it. In this way, by adjusting the water supply, the speed of the string could be made equal to that of wave-propagation; and the former could easily be determined from the whole length of the string, and from the time required by a knot upon it to make a complete circuit. Thus (on February 7) the velocity of propagation was found to be 5·4 inches per second. At the same time, by measurement of the pattern as seen by ordinary vision, $14\lambda = 4\frac{7}{8}$ inches. Hence frequency $= \frac{5 \cdot 4}{\lambda} = 15 \cdot 5$ per second; exactly one half the observed frequency of the bar, viz. 31.

In addition to the phantoms which may be considered to represent the four component progressive waves, others may be observed travelling in directions inclined at 45°. If we take coordinates ξ, η in these directions, (5) may be written

$$4 \cos \frac{\kappa\xi}{\sqrt{2}} \cos \frac{\kappa\eta}{\sqrt{2}} \cos nt \, ; \quad . \quad . \quad . \quad . \quad . \quad (7)$$

in which if we put

$$\frac{\kappa\xi}{\sqrt{2}} = \frac{\kappa\xi'}{\sqrt{2}} + nt$$

(*i. e.* if we suppose the eye to travel with velocity $\sqrt{2} \, . \, n/\kappa$), we get

$$2 \cos \frac{\kappa\eta}{\sqrt{2}} \cos \frac{\kappa\xi'}{\sqrt{2}} + \text{ terms in } 2nt.$$

The non-periodic part may be supposed roughly to represent the phenomenon.

In order if possible to settle the question beyond dispute, I made yet another comparison of the frequencies of vibration of the fluid and of the support, using a plan not very different from that originally employed by Faraday. A long plank was supported on trestles at the nodes, and could be tuned within pretty wide limits by shifting weights which rested upon it near the middle and ends. At the centre was placed a beaker $4\frac{1}{4}$ inches in diameter, and containing a little mercury. The plank was set into vibration by properly timed impulses with the hand, and the weights were adjusted until the period corresponded to one mode of free vibration of the pool of mercury. When the adjustment is complete, a very small vibration of the plank throws the mercury into great commotion, and unless the vessel is deep there is risk of the fluid being thrown out. The question now to be decided is whether, or not, the vibrations of the mercury are executed in the same time as those of the plank.

On March 18 the plank was adjusted so as to excite that mode of vibration of the mercury in which there are two nodal diameters. Two other diameters bisecting the angles between these give the places of maximum vertical motion. At one moment the mercury is elevated at *both* ends of one diameter and depressed at both ends of the perpendicular diameter; half a period later the case is reversed. The frequency of the fluid vibrations could be counted by inspection, and was found to be 30 (complete) vibrations in 15 seconds, or exactly two vibrations per second. The vibrations of the plank were counted by allowing it to tap slightly against a pencil held in the hand. In five seconds there were 21 complete vibrations, *i. e.* $4\frac{1}{5}$ vibrations per second, almost exactly twice as many as was found for the mercury. The measurements were repeated several times; and the general result is beyond question.

On another occasion the mode of fluid vibration was that in which there is but one nodal diameter, the fluid being most raised at one end of the perpendicular diameter and most depressed at the other end. The frequency of fluid vibration was $30/22 = 1{\cdot}36$; while that of the plank was $27/10 = 2{\cdot}7$. Here again the fluid vibrations are proved to be only half as quick as those of the support.

The mechanics of the question are considered in a communication to the Philosophical Magazine for April 1883, to which reference must be made. Merely to observe the phenomenon, it is sufficient to take a porcelain evaporating-dish containing a shallow pool of mercury 2 or 3 inches in diameter, and, holding it firmly with both hands, to impose upon it a vertical vibratory motion. After a few trials of various speeds it is possible to excite various modes of vibration, including those referred to in connexion with the plank. The first (with two nodal diameters) is more interesting in itself, and is more certainly due to a vertical as opposed to a horizontal vibration of the support. The gradually shelving bank presented by the dish adds to the beauty of the experiment by its tendency to prevent splashing.

Dr. Matthiessen, in the papers referred to, records a long series of measurements of the wave-lengths of crispations corresponding to various frequencies of vibration, not only in the case of water, but also of mercury, alcohol, and other liquids. He remarks that the nature of the liquid affects the relation in a marked manner, contrary to the theoretical ideas of the time, which recognized gravity only as a "motive" for the vibrations. In the following year Sir W. Thomson gave the complete theory of wave-propagation*, in which it is shown that in the case of wave-lengths so short as most of those experimented upon by Matthiessen, the influence of cohesion, or capillary tension, far outweighs that of gravity. In general, if T be the tension, $\kappa=2\pi/\lambda$, the velocity of propagation (v) is given by

$$v=\sqrt{\left\{\frac{g}{\kappa}+\mathrm{T}\kappa\right\}}; \quad . \quad . \quad . \quad . \quad . \quad (8)$$

or, when λ is small enough,

$$v=\sqrt{(\mathrm{T}\kappa)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

Since $\lambda=v\tau$, the relation between τ and λ is, by (9),

$$2\pi\mathrm{T}\tau^2=\lambda^3; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

or, if N be the frequency of vibration,

$$\mathrm{N}^{\frac{3}{2}}\lambda=\text{constant}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

Dr. Matthiessen's results agree pretty well with (11), much better in fact than with the formula proposed by himself.

There is another point of some interest on which the views expressed by Matthiessen call for correction. It was observed by Lissajous some years ago, that if two vibrating tuning-forks of slightly different pitch are made to touch the surface of water, the nearly stationary waves formed midway between the sources of disturbance travel slowly towards the graver. We may take as the expression for the two progressive waves

$$\cos(\kappa x-nt)+\cos(\kappa' x+n't),$$

or, which is the same,

$$2\cos\{\tfrac{1}{2}(\kappa+\kappa')x+\tfrac{1}{2}(n'-n)t\}\times\cos\{\tfrac{1}{2}(\kappa'-\kappa)x+\tfrac{1}{2}(n'+n)t\}.$$

The position at any time of the crests of the nearly stationary waves is given by

$$\tfrac{1}{2}(\kappa+\kappa')x+\tfrac{1}{2}(n'-n)t=2m\pi,$$

where m is an integer. The velocity of displacement V is thus

$$\mathrm{V}=\frac{n-n'}{\kappa+\kappa'}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

* Phil. Mag. Nov. 1871.

from which it appears that in every case the shifting is in the direction of propagation of waves of higher pitch, or towards the source of graver pitch.

According to Matthiessen, the shifting takes place with a velocity equal to half the difference of velocities of the component trains, *i. e.*

$$2V=\frac{n}{\kappa}-\frac{n'}{\kappa'}, \quad \ldots \ldots \ldots \quad (13)$$

and in the direction of that component train which moves with greatest velocity. So far as regards the direction merely, the two rules come to the same thing for the range of pitch used by Lissajous and Matthiessen, since over this range the velocity increases with pitch. If, however, we have to deal with waves longer than the critical value (1·7 centim. for water), the two rules are at issue, since now the velocity increases as the pitch diminishes. The following are a few corresponding values, in C.G.S. measure, of wave-length, velocity, and frequency of vibration calculated by Thomson's formula (A).

Wave-length...	·5	1·0	1·7	2·5	3·0	5·0
Velocity	31·48	24·75	23·11	23·94	24·92	29·54
Frequency ...	62·97	24·75	13·60	9·579	8·306	5·908

I have examined the matter experimentally with the aid of vibrators making from 12 to 7 complete vibrations per second, and therefore well below the critical point, with the result that the transference is towards the source of graver pitch, although this is the direction of propagation of the component which travels with the smaller velocity. I reserve for the present a more detailed description of the apparatus, as I propose to apply it to the general verification of Thomson's law of velocities.

VIII. *An Illustration of the Crossing of Rays.* *By* Walter Baily*.

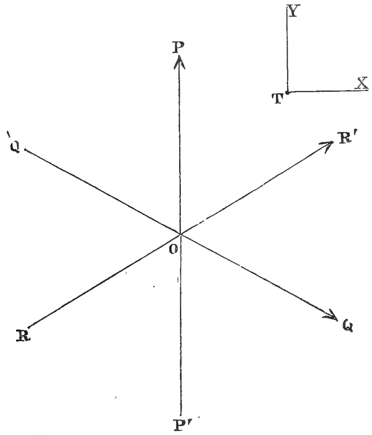
[Plate I.]

When rays of light are passing through a point, the resultant motion of the æther is in general far too complicated to be conceived; but if the light is homogeneous, it can readily be shown that the motion at each point is simply harmonic motion in an ellipse; so that in that case the

* Communicated by the Physical Society; read May 26, 1833.

complication consists only of the change in this ellipse in passing from one point to another. Hence a model might be constructed to represent the crossing of homogeneous rays by placing a number of ellipses to represent the motion at a number of separate points, through which the light might be supposed to be passing. If we further simplify the case by considering only rays parallel to one plane, and suppose them to be plane-polarized so that the vibrations are parallel to the same plane, the whole motion will be parallel to that plane, and might be represented by means of diagrams.

The case worked out in this paper is that of three rays of equal intensity parallel to one plane, plane-polarized so that the vibrations are parallel to that plane, and meeting one another at equal angles.



Take any point O, and let P′ O P, Q′ O Q, R′ O R be the rays through O. Take any other point T in the same plane; draw T X, T Y perpendicular and parallel respectively to P′ O P. Let p, q, r be the distances from O of the feet of the perpendiculars drawn from T on P′ O P, Q′ O Q, R′ O R respectively; these distances being considered positive if drawn towards P, Q, R, and negative if drawn towards P′, Q′, R′. Then it may be shown that

$$p+q+r=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The position of T may be defined by any two of these quantities. The equations $p=$ const., $q=$ const., $r=$ const., are equations to straight lines perpendicular to P′ O P, Q′ O Q, R′ O R respectively; and the equations $q-r=\text{const}$, $r-p=\text{const.}$, $p-q=\text{const.}$ are equations to lines parallel to P′ O P, Q′ O Q, R′ O R respectively. When the constant is zero, the lines pass through O.

If we take any point in Q′ Q and move perpendicularly to Q′ Q from this point, we can, without altering the phase of the vibration of the ray Q, reach a point at which the phase of the vibration of the ray R is the same. If we now move from this latter point in a direction parallel to P′ P, we shall keep the phases of Q, R equal to one another, and we can reach a point at which the phase of the ray P is equal to either of them. Take this point as the origin, and let the phases be zero at the initial time. Then at a time t the displacements due to the three rays at the point T will be $\sin 2\pi(t-p)$, $\sin 2\pi(t-q)$, $\sin 2\pi(t-r)$, the wave-length being taken as the unit of length, and the period as the unit of time.

Let x be the amount of displacement along TX and y that along TY, at the time t. Then

$$x=\sin\frac{\pi}{2}\sin 2\pi(t-p)+\sin\left(\frac{\pi}{2}+\frac{2\pi}{3}\right)\sin 2\pi(t-q)$$
$$+\sin\left(\frac{\pi}{2}-\frac{2\pi}{3}\right)\sin 2\pi(t-r),$$

$$y=\cos\frac{\pi}{2}\sin 2\pi(t-p)+\cos\left(\frac{\pi}{2}+\frac{2\pi}{3}\right)\sin 2\pi(t-q)$$
$$+\cos\left(\frac{\pi}{2}-\frac{2\pi}{3}\right)\sin 2\pi(t-r).$$

By means of (1) these equations may be written

$$x=\sin 2\pi(t-p)-\cos\pi(q-r)\sin 2\pi\left(t+\frac{p}{2}\right), \quad . \quad (2)$$

$$y=\sqrt{3}\sin\pi(q-r)\cos 2\pi\left(t+\frac{p}{2}\right). \quad . \quad . \quad . \quad . \quad . \quad (3)$$

In general the calculation of the phase and the ellipse would be laborious; but it may be readily effected along lines parallel to P′OP, Q′OQ, R′OR at distances $\frac{1}{\sqrt{3}}$ from one another as follows:—We have as equation to such lines parallel to P′OP, $q-r=n$, where n is an integer. Hence

$$y=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

$$x=\sin 2\pi(t-p)-\sin 2\pi\left(t+\frac{p}{2}\right)\cos n\pi.$$

If n is even,

$$x=-\sqrt{2-2\cos 3\pi p}\,.\cos 2\pi\left(t-\frac{p}{4}\right). \quad . \quad . \quad (5)$$

If n is odd,

$$x=\sqrt{2+2\cos 3\pi p}\,.\sin 2\pi\left(t-\frac{p}{4}\right). \quad . \quad . \quad . \quad (6)$$

Equation (4) shows that along these lines the vibrations are rectilinear, and perpendicular to direction of the ray.

Putting $p=\frac{m}{3}$, m being an integer, we see from (5) and (6) that there are points of no motion when m and n are both even or both odd. These conditions will be satisfied if p, q, and r are multiples of $\frac{1}{3}$. In order to satisfy (1), one of the quantities must be an even multiple, and the other two must be both even or both odd.

We may obtain similar equations in relation to Q′OQ and R′OR; and the points of no motion will be the same as those already obtained. If we draw the three sets of lines above considered, we shall form a series of triangles whose sides are parallel to the rays, each side being equal to $\frac{2}{3}$. These triangles will have the properties, that their angles will be nodes, and that the vibrations along their sides will be perpendicular to the sides, the displacement being given by equations (5) and (6) and the corresponding equations for the rays Q and R. The form of these triangles under displacement, when $t=0$, is shown in Pl. I. fig. 1.

The motion may be also readily obtained along lines perpendicular to the direction of the rays, at distances $\frac{1}{3}$ from each other, one of each set passing through the origin. p must be a multiple of $\frac{1}{3}$; and there are six different forms of equations (2) and (3) for six consecutive values of p, which are given in the following Table (n being an integer):—

p.	x.	y.
$2n+1$	$(1+\mathrm{A})\sin 2\pi t$	$-\mathrm{B}\cos 2\pi t$
$2n+\frac{2}{3}$	$(1-\mathrm{A})\sin 2\pi(t+\frac{1}{3})$	$+\mathrm{B}\cos 2\pi(t+\frac{1}{3})$
$2n+\frac{1}{3}$	$(1+\mathrm{A})\sin 2\pi(t-\frac{1}{3})$	$-\mathrm{B}\cos 2\pi(t-\frac{1}{3})$
$2n$	$(1-\mathrm{A})\sin 2\pi t$	$+\mathrm{B}\cos 2\pi t$
$2n-\frac{1}{3}$	$(1+\mathrm{A})\sin 2\pi(t+\frac{1}{3})$	$-\mathrm{B}\cos 2\pi(t+\frac{1}{3})$
$2n-\frac{2}{3}$	$(1-\mathrm{A})\sin 2\pi(t-\frac{1}{3})$	$+\mathrm{B}\cos 2\pi(t-\frac{1}{3})$

where $\quad \mathrm{A}=\cos\pi(q-r), \quad \mathrm{B}=\sqrt{3}\sin\pi(q-r).$

These lines intersect the triangles (fig. 1) at their angles, and also at the bisection of their sides. At these points the motion has been already determined. The motion is circular if p is an even multiple of $\frac{1}{3}$, at the points for which $1-A=\pm B$ —that is, where $q-r=2m\pm\frac{2}{3}$ (m being an integer); and if p is an odd multiple of $\frac{1}{3}$, at the points for which $1+A=\pm B$ —that is, where $q-r=2m\pm\frac{1}{3}$.

These conditions are satisfied at the middle points of the triangles. In fig. 2 are shown the nodes and the circular points, the arrows indicating the phase when $t=0$. It will be noticed that at adjacent circular points the motion is in opposite directions.

It would be possible to construct a piece of apparatus to exhibit the motion approximately. A piece of elastic membrane, sufficiently stretched in all directions, should be fastened at a set of points corresponding to the points of rest, and the middle points of the triangles should then be displaced according to the phase (see fig. 2), and carried round their original positions in circles of equal size and period, the adjacent motions being in opposite directions—an arrangement which might easily be effected by a series of cogged wheels. We should then have a number of points fixed, and the correct motion given at other points where the motion is greatest. The motion of the rest of the membrane except near the edges would then be approximately correct.

In fig. 3 is given an enlarged view of one of the triangles, showing some of the points where the motion is elliptic, and the displacement of the lines through the nodes parallel and perpendicular to the rays.

IX. *On the Conservation of Solar Energy.* *Reply by* Sir WILLIAM SIEMENS *to* Mr. E. H. Cook*.

ARTICLE LX. in the June Number of the 'Philosophical Magazine,' by E. H. Cook, B.Sc., calls for a reply to some of the objections raised against my Solar hypothesis, which I am the more readily disposed to give, inasmuch as they differ from those already raised by others, and involve moreover questions of general interest. Mr. Cook proves that CO_2 is distributed uniformly throughout our atmosphere; and concludes that the power of gaseous diffusion is such that, admitting (as he does) a universal plenum, the same gaseous proportion must prevail throughout space—that, in short, there must be as large a proportion of CO_2 and N in space

* Communicated by the Author.

and in the solar photosphere as in our atmosphere, and for the same reason no more hydrogen in those regions than we can detect in the atmosphere.

While admitting the uniform distribution of CO_2 in our atmosphere, which is subject to powerful circulating currents, I cannot agree with Mr. Cook in ascribing to gaseous diffusion any considerable influence upon the constitution of a great solar inflow and outflow current. Mr. Cook points out very properly that the density of some of the metallic vapours known to exist in the sun is either inferior to, or does not materially exceed that of, carbon dioxide; and these, he concludes, would mix rapidly by diffusive action with the photospheric current, doing away with its distinctive character; regarding stellar material, he says, towards the end of his article, "any difference in composition would be rapidly removed by the action of gaseous diffusion." Mr. Cook here appears to fall into a common error of interpreting Dalton's expression, that "one gas diffuses into the space occupied by another as though the latter had no existence," into an action comparable to the rush of a gas into a vacuum; whereas in reality the rate of the diffusion of gases at equal pressures does not exceed a few feet per hour, and that of fluids not a few feet per annum, as proved by Sir William Thomson's experiments now going on at Glasgow University. The effect of such slow action must be inappreciable upon gaseous currents depending upon solar rotation, amounting to 23,760,000 feet per second, or 4500 miles an hour. The metallic vapours in the photosphere must therefore be attributable chiefly to mechanical intermixture, which no doubt is considerable, not indeed on the polar surfaces, where the tangential motion is not great, nor at the equator, where the photosphere has acquired the solar rotation, but in the intermediate zones of great differential velocity, rendered visible to us by the occurrence of sun-spots, the result, as is now largely admitted, of cyclonic action. The metallic vapours thus introduced into the photospheric current will be burnt and projected outwards into space, not indeed in the vaporous condition as supposed by Mr. Cook, but as a metallic dust (several thousand times denser than the surrounding medium), the greater portion of which will probably soon return to the sun by virtue of solar gravitation, there to be dissociated on reaching the great gaseous metallic sea underlying the photosphere.

It is of course important to consider to what extent the density of this metallic sea is likely to exceed that of the photosphere. Assuming the latter to contain, besides the light combustible gases, a large proportion of oxygen and nitrogen, we shall be safe in estimating its mean density as equal to that of nitrogen, or equal to 14; and assuming the metallic sea below to consist of both the heavy and light metallic vapours in reasonable proportions, we shall probably be near the truth in assuming its mean vapour-density as being equal to that of iron, or equal to 56. The photospheric current would therefore sweep over an ocean only four times denser than itself, which, under terrestrial conditions, would give rise to very active mechanical admixture; but this tendency is counteracted upon the solar surface by a force of gravity 27 times greater, exercising a separating influence analogous to that upon which the action of the ingenious cream-separator depends, in which the force of gravitation is replaced by centrifugal action. To get the parallel of terrestrial conditions, we should have to assume the solar ocean to be $4 \times 27 = 108$ times denser than the atmospheric current sweeping over it. The light metallic vapours will be held in suspension in the vast metallic ocean in the same way as oxygen and carbonic anhydride are retained in sea-water.

Mr. Cook thinks that the preponderance of hydrogen and CO in meteorites is inconsistent with the idea that the meteoric gases have been absorbed in space; and he puts forward a theory making them the result of decomposition of solid hydrocarbon and oxides by heat while passing through our atmosphere perhaps more than once. A shell of fused magnetic oxide of iron is supposed to be formed, retaining these gases bottled as it were; but it may fairly be objected that such a fused shell, if it were really formed, would crack in cooling and allow the gas to escape through innumerable fissures. It is sufficient for us to know, I think, that the meteorites contain all the constituents of our atmosphere except hydrogen, which latter occurs in our atmosphere combined with oxygen, whereas in space and in the solar photosphere we have evidence of its separate existence or in combination with carbon—a difference which is to my mind the necessary consequence of dissociation of carbonic acid and aqueous vapour in space, and of ignition and slow combustion, effecting their oxidation upon our earth.

I cannot agree with Mr. Cook in supposing that a terrestrial polar inflow current would be inconsistent with the direction of the "return trade" winds, of which it would simply form part, the direction of both being necessarily from the north-east, in the northern hemisphere.

Mr. Cook has evidently not followed me in my description of the equatorial outflow as a balanced current depending upon solar gravitation only for its density and rate of flow;

he reintroduces the effect of projection into space by centrifugal force, and makes the remarkable statement that "if owing to centrifugal force H is projected x miles into space, then sodium vapour, which is 23 times as heavy, will be projected $\frac{x}{23}$ miles." Surely a pound of one substance is as good as another as regards *vis inertiæ* and gravitation; and it seems an unfair proceeding on the part of the latter force to recall the one sooner than the other simply because it is the denser of the two. Considering, however, that the force of gravity is 46,800 times greater than centrifugal force upon the solar equator, no substance could be projected outward a single inch by centrifugal force, although that force is capable of determining the outflow of a gaseous column, balanced by an inflowing polar current of nearly the same density, thus giving rise to what I termed the solar "fan action."

Mr. Cook remarks very properly, that my hypothesis, if applicable to the Sun, must be equally applicable to Sirius with its bluish-white and to Arcturus with its reddish light; and he says "it is difficult to see how these differences are to be accounted for " on the principle of combustion, "the same atmosphere supplying them all." But surely the temperature of terrestrial coal-furnaces is not always the same, depending, as they do, upon the intensity of the draught and the density of the fuel employed. In the case of the photosphere the richness of the gaseous fuel is determined by its density (that is, by the magnitude of the solar body), and the intensity of the combustion by the draught (that is, by its tangential velocity), both of which may vary between wide limits; and the latter may be slowly diminishing, thus giving force to Dr. Huggins's suggestion of the effect of age. The brilliancy of a fixed star must depend moreover upon our relative position towards it, being partially obscured perhaps, if seen equatorially, by planetary matter.

In conclusion, allow me to refer Mr. Cook to the last issue of the Proceedings of the Berlin Academy. In it one of its members (my brother, Dr. Werner Siemens) gives a full investigation of the nature and causes of the electric potential supposed to exist between the Sun and our planet. After some hesitation, my brother has arrived at the conclusion that this electrical connexion cannot be satisfactorily explained except by adopting my hypothesis of a material equatorial outflow, sweeping out from the Sun past our Earth, and carrying with it particles of dust. When writing this article, my brother appears to have overlooked the most interesting observations that have been made for some years at the Greenwich observatory, showing a remarkable coincidence between terrestrial magnetic storms and solar disturbances observable as sun-spots. These observations completely confirm my brother's views on this subject, and agree also with those I ventured to put forward in my original paper.

X. *Notices respecting New Books.*

Sir William Hamilton: the Man and his Philosophy. Two Lectures delivered before the Edinburgh Philosophical Institution, January and February, 1883. *By* JOHN VEITCH, *LL.D.* (W. Blackwood and Sons; 1883: pp. 68.)

"I HAVE got but one hour this evening to put before you the method and the main results of a great philosophy, the work of a man's lifetime." So opens the second lecture, which gives a rapid sketch of a philosophy, admittedly put forth to the world in fragments; but "from the stately parts" of which "we can imagine the greatness of the whole, had the master's hand given them union and cohesion." Lectures written under the condition indicated above cannot be expected to go deep down into the subject; and all the reader can reasonably look for is a clear and accurate presentment of facts. This he will find furnished in this interesting sketch. Students who want fuller details will find these in the same writer's larger work in Messrs. Blackwood's 'Philosophical Classics for English Readers.' With one of Hamilton's opponents we say, "Long live the memory of William Hamilton, good, learned, acute, and disputatious."

On the Motion of a Projectile in a Resisting Medium, and particularly when the Resistance varies as the Cube of the Velocity. By A. G. GREENHILL. (Woolwich; printed at the Royal Artillery Institution, 1882: pp. 32.)

Mr. Bashforth's "Motion of Projectiles" contains a lengthy set of tables from which by laborious processes the path of the projectile may be constructed with almost any desired amount of accuracy *. Prof. Greenhill comes to the consideration of the problem with his almost unequalled mastery of elliptic-function methods, and, as a result *inter alia*, appears to have simplified the calculation of some of Mr. Bashforth's tables. At the close are appended some practical tables, drawn up by Captain P. A. Macmahon, R.A., worked out from values given in Mr. Bashforth's "final report on experiments with the Bashforth chronograph," 1880. Though we cannot pronounce upon the technical applications of the paper, we have read Prof. Greenhill's elegant analysis with interest.

* Encycl. Met., art. Gunnery. The term *laborious* does not, of course, imply that blame rests upon the writer of this classical treatise: even the cubic law, the simplest tc work with, is fraught with difficulty.

XI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xv. p. 437.]

May 9, 1883.—J. W. Hulke, Esq., F.R.S., President, in the Chair.

THE following communications were read :—

1. "The Age of the newer Gneissic Rocks of the Northern Highlands." By C. Callaway, Esq., D.Sc., F.G.S. With Notes on the Lithology of the specimens collected by Prof. T. G. Bonney, M.A., F.R.S., Sec. G.S.

The object of the author was to prove that the eastern Gneiss of the Northern Highlands, usually regarded as of "Lower Silurian" age, was to be placed in the Archæan. While admitting that this gneiss frequently overlies the quartzo-dolomitic group of Eribol and Assynt, he held that this relation was due to dislocation accompanied by powerful thrust from the east, which had squeezed both formations into a series of folds thrown over towards the west, so as to cause a general easterly dip. As a preliminary to his demonstration, the author gave the following classification of the quartzo-dolomitic series, which, in the absence of clear proof of its age, he called the "Assynt Group," the subdivisions being taken in ascending order :—

C_1. Torridon Sandstone and Ben More Grit.
C_2. Quartzite.
  C_2 l. Seamy. C_2 u. Annelidan.
C_3. Brown Flags.
C_4. Salterella Grit and Quartzite.
C_5. Dolomite.
  C_5 l. Dark. C_5 u. White.

For the eastern gneiss the author proposed the term "Caledonian."

Taking the country examined from south to north, Loch Broom was first described. Here the author considered there was clear proof of dislocation. Between the Torridon and the Caledonian there were several subparallel faults, which increased in throw from west to east, Torridon Sandstone being first brought up through the quartzite, then further east through the dolomite, while still further east the Hebridean was thrown up, the Caledonian appearing east of the Hebridean. This Hebridean was the "porphyry" of Nicol.

In Assynt the "Upper Quartzite" was first discussed. The author described several sections which he considered to prove that this band was the ordinary quartzite repeated east of a great fault which brought up the Hebridian, in one place (Glen Coul) the quartzite being conformably succeeded by the brown flags and dolomite.

The "igneous rocks" of Nicol ("Logan Rock" of Dr. Heddle) were regarded as the old gneiss brought up by a fault and thrown over onto the Assynt group to the maximum breadth of more than a mile.

The "Upper Limestone" of authors was described as either outliers of the dolomite or a part of the Caledonian series.

The Caledonian rocks were seen in Glen Coul to be immediately overlying the Hebridian, the Assynt group being caught in the angle between the two gneisses, and bent back in overthrown folds.

The mountain-groups of Assynt were described as usually consisting of cores of Hebridian gneiss swathed in or capped by sheets of quartzite. In the former case the quartzite on the western slopes was contorted into overthrown folds by the thrust from the east.

In the Loch-Eribol district, the "granulite" of Nicol was considered to be a lower division of the Caledonian gneiss, though bearing some resemblances to the Hebridian. In other respects, the views of Nicol were regarded as substantially correct. Along the entire length of Loch Eribol, a distance of about twelve miles, the thrust from the east had bent back the Assynt group into overthrown folds, and pushed the Caledonian gneiss onto the top of the inverted quartzite. This had produced the appearance of an "upper" quartzite passing "conformably" below the eastern gneiss. The superior antiquity of the Caledonian was confirmed by the occurrence of outliers of quartzite upon the Arnabol (Lower Caledonian) series, and by the fact that the granite, which sent numberless veins into the gneiss, never penetrated the quartzite and associated rocks.

2. "On a Group of Minerals from Lilleshall, Salop." By C. J. Woodward, Esq., B.Sc., F.G.S.

The minerals noticed in this paper occur in a bed of grey limestone in the Carboniferous Limestone at Lilleshall. They occur in vertical joints in the upper subdivisions of the bed. The following list gives them arranged in order of frequency, the least common being placed first:—quartz, bornite, towanite, iron-pyrites, hæmatite, barytes, calcite, dolomite (ankerite?). Of the first, the author has only met with a single minute crystal. Iron-pyrites is by no means common; hæmatite is more abundant. Analyses of two specimens of the "dolomite," show that one agrees very nearly with ankerite, while another, identical in aspect, exhibits considerable differences, being only a ferriferous dolomite. The author suggests that it is doubtful whether ankerite should be retained as a mineral species.

3. "Fossil Chilostomatous Bryozoa from Muddy Creek, Victoria." By A. W. Waters, Esq., F.G.S.

May 23.—J. W. Hulke, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "On the Basalt-glass (Tachylyte) of the Western Isles of Scotland." By Prof. J. W. Judd, F.R.S., Sec. G.S., and G. A. J. Cole, Esq., F.G.S.

Basalt-glass or tachylyte is a rare rock, although very widely distributed.

In the Western Isles of Scotland it has, by the authors of the paper, been detected in five localities only, namely Lamlash (Holy Isle) near Arran, the Beal near Portree in Skye, Gribun and Sorne in Mull, and Screpidale in Raasay.

Basalt-glass is always found in the Hebrides as a selvage to dykes, though elsewhere it has been described as occurring under other conditions, where rapid cooling of basaltic lava has taken place. Some of the varieties of basalt-glass in the Hebrides differ from any hitherto described by their high specific gravity (2·8 to 2·9) and by their low percentage of silica (45 to 50).

This basalt-glass is frequently traversed by numerous joints; it is occasionally finely columnar, and sometimes perlitic in structure.

From the acid glasses (obsidian) it is distinguished by its density, its opacity, its magnetic properties, and especially by its easy fusibility, from which the name of tachylyte is derived. By its greater hardness it is readily distinguished form its hydrated forms (palagonite, &c.).

In its microscopic characters basalt-glass is found to resemble other vitreous rocks; thus it exhibits the porphyritic, the banded and fluidal, the spherulitic, and the perlitic structures. In the gradual transition of this rock into basalt, all the stages of devitrification can be well studied.

The difference between these locally developed basalt-glasses and the similar materials forming whole lava-streams in the Sandwich Islands was pointed out in the paper, and the causes of this difference were discussed.

It was argued that the distinction between tachylyte and hyalomelane, founded on their respective behaviour when treated with acids, must be abandoned, and that these substances must be classed as rocks and not as mineral species; the name basalt-glass was adopted as best expressing their relations to ordinary basalt, the term tachylyte being applied to all glasses of basic composition and being used in contradistinction to obsidian.

2. "On a Section recently exposed in Baron Hill Park, near Beaumaris." By Prof. T. G. Bonney, M.A., F.R.S., Sec. G.S.

The author, about three years since, observed some imperfect exposures of a felsitic grit in the immediate vicinity of the normal schists of the district in a road which leads from Beaumaris cemetery to Llandegfan; but last summer he had the opportunity, through the courtesy of Sir R. B. Williams, of examining the cuttings made in constructing a new drive, which runs through Baron Hill Park very near the above outcrops. After tracing the normal schists along the steep scarp of the hill, he came, after an interval of about 60 yards, covered by soil and vegetation, to a massive grey grit consisting of quartz, felspar, and minute fragments of compact felsite, which now and then attain a larger size, being an inch or so across. These grits, which pass occasionally into hard compact mudstones (probably more or less of volcanic origin), can be traced for some 350 yards to the neighbourhood of the above-mentioned road, which is crossed by a bridge; and a short distance on the other side of this is a considerable outcrop of the grit, which in places becomes coarsely conglomeratic, containing large fragments of the reddish quartz felsite so common on the other side of the straits in the beds at or below the base of the Cambrian series. The schists appear to dip about 20° E.S.E., the grits about 25° E.

The author, after describing the microscopic structure of the various rocks noticed, pointed out that this section, though the junction of the two rocks is probably a faulted one, has an important bearing on the question of the age of the Anglesey schists, micaceous and chloritic. The Survey regards them as altered Cambrian; it has even been suggested that they may be of Bala age; others have regarded them as Pebidian. Now the felsitic grits and conglomerates cannot be newer than the Cambrian conglomerate of the mainland, regarded by Prof. Hughes as the base of the true Cambrian, and are probably older, corresponding with some part of the series between it and the great masses of quartz felsite which are developed near Llyn Padarn and Port Dinorwig, which series lithologically and stratigraphically corresponds with the typical Pebidian of Pembrokeshire. Hence, as the Anglesey schists are in the full sense of the term metamorphic rocks, and the "Pebidian" but slightly altered, this section shows that the former must be much older than the latter, and so be distinctly Archæan.

3. "On the Rocks between the Quartz Felsite and the Cambrian Series in the neighbourhood of Bangor." By Prof. T. G. Bonney, M.A., F.R.S., Sec. G.S.

This district has already been the subject of papers by the author (Q. J. G. S. vol. xxxiv. p. 137) and by Prof. Hughes (vol. xxxv. p. 682), who differs from him in restricting the series between the quartz felsite and Cambrian conglomerate to little more than the bastard slates and green breccias of Bangor mountain. The author has traced on the S.E. side of the Bangor-Caernarvon road a well-marked breccia containing fragments of purple slate mixed with volcanic materials below the above-named Bangor series for more than a mile; at a lower level he has traced another well-marked breccia, chiefly of volcanic materials, for half a mile, and, lastly, a grit and conglomerate, apparently resting on the quartz felsite named above, composed of materials derived from it. This has been traced on both sides of the road mentioned above for nearly two miles. For these and for other reasons given in the paper, the author is of opinion that, as he formerly maintained, there is a continuous upward succession on the S.E. side of the road, from the quartz felsite at Brithdir to the Cambrian conglomerate on Bangor mountain. The district on the N.W. side of the road is so faulted that he can come to no satisfactory conclusions. The author is in favour of incorporating the above-named quartz felsites with the overlying beds as one series, corresponding generally with the Pebidian of South Wales—older than the Cambrian, though probably not separated from it by an immense interval of time. An analysis of the Brithdir quartz felsite by Mr. J. S. Teall was given, from which it appeared that the rock corresponds very closely with the "devitrified pitchstone" of Lea rock in the Wrekin district, described by Mr. Allport, but differs considerably in composition from those in the Ordovician rocks of North Wales.

XII. *Intelligence and Miscellaneous Articles.*

ON THE CRITICAL POINT OF LIQUEFIABLE GASES. BY J. JAMIN.

ATTENTION having been called to the liquefaction of gases, I proceed to lay before the Academy the reflections which the subject has suggested to me. The remarkable paper published by Andrews in 1870 * demonstrated that, in order to liquefy carbonic acid, pressures increasing up to 78 atmospheres must be employed when the temperature amounts to 31°. At that limit the phenomenon changes: certainly a rapid diminution of volume is still seen, and undulating and moving striæ are observed as in a mixture of two liquids of different densities; but there is no longer any liquefaction, however great may be the pressure exerted. In short, below 31° the gas is liquefiable; above, it is so no longer. This is the *critical point.*

These are incontestable facts; and the notion of the critical point has rendered great services; but it is unexplained, and perhaps inaccurately interpreted. I believe that gases are liquefiable at any temperature when the pressure is sufficient, but that an unperceived circumstance has prevented the liquefaction being seen. To make this intelligible, let us take the experiments of Cagniard-Latour.

A thick glass tube was half or two-thirds filled with water under the pressure of its vapour only; it was hermetically sealed, and was then heated to 300° or 400°. According to known laws the quantity of vapour superposed to the liquid increases

* *Ann. de Chimie et de Physique,* [4] xxi. p. 208.

rapidly; its density increases, in the same ratio as its weight, without any known limit. On the other hand, the portion which has remained liquid undergoes an expansion that increases until at last it exceeds that of gases (Thilorier). It is clear that, by the effect of these reverse changes, a limiting temperature is finally attained, at which liquid and vapour have the same weight under the same volume.

At that moment they are no longer separate: the vapour does not escape to the top; the liquid does not sink to the bottom. First of all the meniscus is seen to disappear, the surface of separation ceases to be distinct (Drion); then the entire mass is mingled, with undulating and moving streaks giving evidence of a mixture of different densities; and finally the whole assumes a homogeneous state which is supposed to be gaseous. The critical point is reached, which may be defined as *the temperature at which a liquid and its saturated vapour have the same density.* But, for all that, the general law of vaporization is not suddenly interrupted; the liquid continues to be at its boiling-point and maximum tension. If it is no longer visible, that is because it is mixed with the gas, in which it floats on account of the equalization of the densities; and when the temperature continues to be augmented, the tension continues to increase, remaining a maximum, until the liquid is entirely volatilized; after which, but not till then, the space ceases to be saturated and the pressure to be limited: there is no longer any thing but dry vapour, a gas far from its liquefying-point.

It is necessary to rightly account for this very peculiar state of the liquid while it is thus invisible and floats in its vapour. In general a saturated vapour is distinguished from the generating liquid by two conditions—its less density, and its latent heat; here, as we have seen, the densities are equal, and there is no latent heat, since the volume is unchanged and there is no work of expansion. Hence comes it that in Cagniard-Latour's experiment the liquid is seen so suddenly to reappear or disappear through the least depression or elevation of temperature. In short, at its critical point nothing differentiates the liquid from its vapour—neither tension, nor density, nor heat of constitution, nor aspect, nor any property that could distinguish them.

If instead of Cagnaird-Latour's experiment we follow in detail those of Andrews, they can be summed up as follows:—When a gas confined in a closed space is gradually compressed,

(1) Its pressure increases up to a fixed limit, the *maximum tension*; if we try to go beyond that, the gas condenses: it is the *liquefaction-point*; it is the *boiling-point* under that pressure.

(2) The liquefaction-pressure augments rapidly with the temperature, without any known limit, but without ceasing to exist and without changing its character at any critical point whatever.

(3) At low temperatures the density of saturated vapour is less than that of the generating liquid. Starting from a determined

limit, it becomes and remains equal to the latter: this is the critical point.

(4) At the critical point and beyond it the liquid is mixed and blended with its saturated vapour.

(5) At the critical point and beyond it there is no longer any latent heat.

(6) At the critical point and beyond it the liquid and gaseous states are blended.

From my point of view it is seen that all the laws of the formation and condensation of vapours are maintained, and that the exception of the critical point is explained by the equalization of the densities. We shall see that the same theory accounts for facts hitherto incomprehensible, and enables us to predict others which are not without interest. M. Cailletet in 1880 * made the following experiment, which has not received the attention it deserves, and has been forgotten because it was not understood:— Having compressed in his apparatus a mixture of 1 part air and 5 parts carbonic acid, M. Cailletet first saw the latter gas assume the liquid state under a moderate pressure; then, without changing the temperature, but raising the pressure to 150 or 200 atmospheres, he saw the whole of the liquid which had been formed disappear. One would say that the increase of pressure gives rise to a critical point, like the elevation of the temperatures, which is scarcely admissible. The theory which I have just established accounts for this curious phenomenon very easily as follows:—

By a moderate pressure carbonic acid is brought to its condensing-point and is at first partially liquefied. If the reduction of volume is continued, the pressure of the carbonic acid is no longer augmented, because it had already attained its maximum; but that of the air continues to increase indefinitely, and with it the total density of the gaseous atmosphere. This density soon becomes equal to that of the liquid already formed, which then does not remain at the bottom of the vessel, but diffuses into the whole space, having, according to the principle of Archimedes, lost all its weight.

It is by the accuracy of its previsions that the truth of a theory is judged. Now, if it be true that the disappearance of the liquefied acid is due to the equalization of the densities, it will be retarded by substituting for the air of the mixture a gas of less density, hydrogen.

I forwarded this conclusion to M. Cailletet, who hastened to submit it to experiment. He made two mixtures, the first containing 5 volumes of carbonic acid to 1 volume of air, and the second containing 5 volumes of carbonic acid to 1 volume of hydrogen. In both cases he obtained the liquefaction of the carbonic acid under moderate, and its total disappearance with more powerful pressures. In both cases, first the meniscus was effaced; this was at the moment when the densities of the liquid and

* *Comptes Rendus*, Feb. 2, 1880.

the atmosphere became equal. Afterwards the liquid disappeared; and, in conformity with the theory, it disappeared at very different and more considerable pressures for the mixture of hydrogen than for that of air.

Here are M. Cailletet's results reduced, by a graphic construction, to the same temperature. It will be seen that at 20° it requires about 200 atmospheres for hydrogen, and only 100 for air.

Temperature.	Disappearance-pressure for the mixtures of carbonic acid and	
	air.	hydrogen.
15°	135	245
16	130	236
17	125	227
18	120	218
19	114	208
20	108	199
21	102	190
22	96	181
23	90	172
24	85	163
25	79	153

From these experiments, which shall certainly be continued, it will be easy to institute a comparison between the densities of air and hydrogen at different temperatures and under one and the same pressure.

When the mixture is less rich in carbonic acid, M. Cailletet has remarked that the liquefaction of this gas is always retarded, and sometimes impossible. In fact, when the total volume is reduced by pressure to one tenth or one hundredth, both gases undergo the same reduction but not the same increase of pressure: that of the air is multiplied by 10 or 100; that of the carbonic acid increases less quickly, since Mariotte's law is no longer applicable. Hence results a retardation of liquefaction; and when at length the carbonic acid has reached its maximum tension and passes into the liquid state, its density may have become equal to, and sometimes less than, that of the superposed atmosphere. In this case it mixes with, floats in the latter, leading one to believe that it has lost the property of liquefying under pressure, when it has only lost the property of collecting at the bottom of the vessel from excess of density.

Is it not permissible to expect another ending to this experiment? If the total pressure were augmented indefinitely, the carbonic acid would continue to liquefy, and its density would change but little, while that of the gaseous atmosphere would be indefinitely increased and would become superior to that of the liquid, which would perhaps separate; but then it would collect at the top of the tube instead of sinking to the bottom. This second essay I proposed to M. Cailletet, who hastened to try it. He did not succeed; but I do not despair.—*Comptes Rendus de l'Académie des Sciences*, May 21, 1883, pp. 1448-1452.

ON THE LIQUEFACTION OF OXYGEN AND THE CONGELATION OF CARBON DISULPHIDE AND ALCOHOL. BY PROFESSORS SIGM. VON WROBLEWSKI AND K. OLSZEWSKI.

The results at which Cailletet and Raoul Pictet arrived in their beautiful investigations on the liquefaction of gases permitted the hope that the time was not distant when liquid oxygen would be observed in a glass tube as easily as liquid carbonic acid now is. The only condition for this was the attainment of a sufficiently low temperature. In a memoir published twelve months since*, Cailletet recommended liquid ethylene as a means for attaining a very low temperature; for the liquefied gas boils at $-105°$ C. under the pressure of the atmosphere, the temperature being measured with a carbon-disulphide thermometer. Cailletet himself compressed the oxygen in a very narrow glass tube which was cooled in that liquid to $-105°$ C. At the moment of the expansion he saw "a tumultuous ebullition, which persists during an appreciable time and resembles the projection of a liquid into the cooled portion of the tube. This ebullition takes place at a certain distance from the bottom of the tube. I have not been able to ascertain," he continues, "if this liquid preexists, or if it is formed at the moment of the expansion; for I have not yet been able to see the plane of separation of the gas and liquid."

As one of us† had recently constructed a new apparatus for high pressures, with which comparatively large quantities of gas can be subjected to the pressure of 200 atmospheres, we employed it to study the temperatures at the moment of the expansion. These experiments soon led to the discovery of a temperature at which carbon disulphide and alcohol congeal and oxygen is with great facility completely liquefied. *This temperature is reached when liquid ethylene is permitted to boil in a vacuum.* The boiling-temperature in this case depends on the goodness of the vacuum obtained. With the greatest rarefaction which it has hitherto been possible for us to attain, the temperature descended to $-136°$ C. This, as well as all the other temperatures, we measured with the hydrogen thermometer.

The critical temperature of oxygen is lower than that at which liquid ethylene boils under the pressure of one atmosphere. The latter is not $-105°$ C. (as has hitherto been assumed), but lies between $-102°$ and $-103°$ C. (as we have found with our thermometer).

From a series of observations made by us on the 9th April of this

* *Comptes Rendus*, t. xciv. pp. 1224-1226.
† S. v. Wroblewski.

year, we take as an example the following numbers in order to give an idea of the state of things:—

Temperature, Centigrade.	Pressure at which oxygen began to liquefy.
°	atmospheres.
−131·6	26·5
−133·4	24·8
−135·8	28·5

We reserve the communication of the definitive numbers.

Liquid oxygen, like liquid carbonic acid, is colourless and transparent. It is very movable, and forms a fine meniscus.

Carbon disulphide congeals at about −116° C. Absolute alcohol at −129° C. becomes viscid like oil, and congeals to a solid mass at about −130°·5 C. Of these also we reserve the communication of the definitive numbers.—*Anzeiger der kaiserlichen Akademie der Wissenschaften in Wien*, 1883, no. ix. pp. 74, 75.

ON THE LIQUEFACTION OF NITROGEN AND CARBONIC OXIDE. BY PROFESSORS S. VON WROBLEWSKI AND K. OLSZEWSKI.

Having succeeded in completely liquefying oxygen*, we tried in the same manner to bring nitrogen and carbonic oxide into the liquid state. The liquefaction of both these gases is considerably more difficult than that of oxygen, and takes place under conditions so similar that it is at present impossible for us to say which of the two gases liquefies more readily.

At the temperature of about −136° C., and under the pressure of about 150 atmospheres, neither nitrogen nor carbonic oxide liquefies; the glass tube containing the gas remains perfectly transparent, and not a trace of liquid can be perceived. If the gas is *suddenly* released from the pressure, in the nitrogen-tube is seen a violent effervescence of liquid, comparable only to the effervescence of the liquid carbonic acid in Natterer's tube when the latter is put into a glass containing hot water. With the carbonic oxide the ebullition is not so strong.

But if the expansion is not effected *too suddenly* and the pressure is not allowed to fall below 50 atmospheres, both nitrogen and carbonic oxide are liquefied completely; the liquid shows a distinct meniscus, and evaporates very briskly. Therefore neither of the two gases can be kept more than a few seconds as liquids in the static condition; to retain them longer in that state a somewhat lower temperature would be necessary than the minimum which up to the present it has been possible for us to attain.

Nitrogen and carbonic oxide in the liquid state are colourless and transparent.—*Anzeiger der kaiserlichen Akademie der Wissenschaften in Wien*, 1883, no. xi. pp. 91, 92.

* See preceding article.

THE

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[FIFTH SERIES.]

AUGUST 1883.

XIII. *Improved Construction of the Movable-coil Galvanometer for determining Current-strength and Electromotive Force in Absolute Measure.* *By* Dr. EUGEN OBACH*.

SOME years ago I showed that the tangent-galvanometer of ordinary dimensions may be employed as a measuring instrument for very strong currents if the ring is made movable around its horizontal diameter †, a principle already adopted before that time by Prof. Trowbridge, of Harvard College, Massachusetts ‡; a little later I described a galvanometer based upon that principle, and constructed by Messrs. Siemens Brothers §.

I now propose to give a brief account of several alterations which have since been introduced by that firm, and which I venture to think render the instrument more sensitive and more convenient for use, besides creating for it a wider field. As the galvanometer, in the complete form in which I shall presently describe it, is not so much destined to meet the daily want of the practical electrician, but is rather intended for measurements where greater accuracy and trustworthiness than usual is necessary, I thought myself justified in bringing the subject before the Physical Society, particularly as the kindness

* Communicated by the Physical Society; read June 9, 1883.

† 'Nature,' xviii. p. 707 (1878); *Repertor. für Exper. Physik.* xiv. p. 507 (1878).

‡ Amer. Journ. of Arts and Science, vol. ii. (August 1871).

§ *Zeitschrift für angewandte Electricitätslehre,* i. p. 4 (1879).

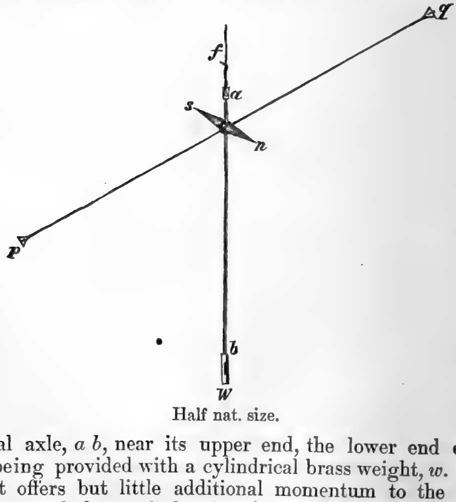
of Messrs. Siemens Bros. & Co. at the same time enables me to place the instruments before you for inspection.

Ere proceeding further, allow me to say that I shall not on this occasion touch upon the theory of the instrument, which is already given elsewhere, but confine myself wholly to describing the recent improvements in its construction, adding a few series of measurements in order to prove the high degree of accuracy obtainable.

I propose to deal with the different parts of the instrument under separate headings; and will first speak of

The Magnetic Needle and its Pointer.

The older instruments had a flat magnetic needle fixed to a light vertical axle, pivotted at both ends between jewels to prevent any dipping, which the needle would otherwise experience with great inclinations of the ring. This arrangement answered sufficiently well with ordinary care; but still the delicate pivots were likely to be damaged, thus impairing the sensitiveness of the needle. As now constructed, the dipping of the needle is completely avoided in the manner illustrated by the annexed figure. The needle, *n s*, is fixed to a thin



Half nat. size.

vertical axle, *a b*, near its upper end, the lower end of the axle being provided with a cylindrical brass weight, *w*. This weight offers but little additional momentum to the whole system round the vertical axis, whereas the movement round the horizontal axis is completely prevented. The aluminium pointer, pq, is situated in the same plane as the scale; the ends are flattened and provided with a fine slit, which serves as an index for reading the deflections, the bottom of the needle-box being blackened. The reading can thus be taken without parallax, and therefore very accurately. The magnetic needle has a biconical shape, which entirely prevents the shifting of the magnetic axis from its original position, as was sometimes found to be the case with the old broad needles. Adjustments are provided by which the cocoon-fibre, f, serving to suspend the needle, can be raised or lowered, as well as accurately centred.

The Damping of the Oscillations.

Numerous experiments were undertaken to ascertain a convenient method for damping the oscillations of the needle, and to arrive, if possible, at a perfectly aperiodical movement. After trying large masses of copper placed in the immediate neighbourhood of the swinging magnet, as well as liquid damping, without decided success, air-damping was resorted to, and finally adopted. It will be remembered that Sir William Thomson used air-damping for the light-mirror of his dead-beat galvanometer, and Prof. Töpler * for other galvanometric apparatus. In our case the air-chamber consists of a shallow cylindrical box, about 8 centim. in diameter, $1\frac{1}{4}$ centim. high, provided with two radial partitions which can be slid in or out; the axle of the needle, passing through the centre of this box, carries a light and closely fitting vane. By sliding the partitions more or less into the box various degrees of damping can be obtained; and if they are right in, the motion is practically dead-beat.

The Scales.

Declination-scale.—This scale, engraved on a horizontal ring, was formerly divided into degrees, as usually done; but now one semicircle is provided with divisions corresponding to the natural tangents. The interval between each two divisions must of course vary for different parts of the scale, and is arranged as follows:—

Table I.

Values of tangent.	Interval.
0 to 1	0·01
1 „ 2	0·02
2 „ 3	0·05
3 „ 5	0·10

* *Repert. f. exp. Phys.* ix. p. 259 (1873).

It will be noticed that the value of the interval only changes at those places where the tangent is equal to a whole figure, thus making a mistake in reading less likely. Looking at this scale no gaps are conspicuous, and the divisions are everywhere pretty evenly distributed. Tangent-scales have been employed by Joule, Sir William Thomson, and others; but the one now described seems well to satisfy all the requirements.

Inclination-scale.—This scale, engraved on a vertical quadrant divided into degrees, can accurately be read to one tenth by means of a vernier. The zero division was formerly that to which the index pointed when the ring was horizontal. In this case the tangent of the deflections had to be divided by the sine of the angles. For convenience' sake, the places were specially marked on the scale at which the sines corresponded to whole figures. The new inclination-scale has the zero at the vertical or normal position of the ring; and instead of the sines, the *secants* are specially marked which are represented by whole figures. With these secants the tangents of the deflections must be multiplied; and they can therefore be termed *multiplying powers,* analogous to the multiplying power of shunts. The instruments intended only for the measurement of current-strength have the quadrant bearing the secant-scale fixed outside the ring, whilst the others, measuring also electromotive force, have it situated between the needle-box and the ring, where it is better protected from injury.

If the deflections of the needle are read on the tangent-scale and the positions of the ring on the secant-scale, the aid of trigonometrical tables may be entirely dispensed with, as the product of the two figures represents the quantity to be measured, irrespective of a constant.

THE SOLID RING AND THE COIL.

If the galvanometer has to serve only for the measurement of currents, the gun-metal ring is of a rectangular cross section; but if it is at the same time destined to measure difference of potential, the cross section is V-shaped, the groove being filled with numerous turns of G.S. wire. If the number of convolutions is known, and if a simple relation exists between that number and the resistance of the wire, a great advantage may be derived therefrom. For instance, if there are one thousand convolutions on the coil, offering a resistance of exactly one thousand ohms, the current due to the difference of potential of one volt at the ends of the coil would produce the same deflection of the needle as the current of one ampère flowing through the solid metal ring. That this must be so is evident, if it is remembered that the weak current of one thousandth of an ampère flows round the needle one thousand times, but the stronger current of one ampère only once. The solid ring and the convolutions are thus arranged that their cross sections have a common centre of gravity, thus both acting exactly in the same way upon the magnetic needle. If this simple plan is adopted, the calibration of the galvanometer for difference of potential in volts, which is readily performed with a few cells of known E.M.F., at the same time gives the graduation of the instrument for strength of current in ampères. I have been using various modifications of the Daniell cell with solutions of copper and zinc sulphate of equal specific gravity. At present I am engaged in constructing a standard cell for such purposes, which is always at disposal; and, as far as the preliminary experiments show, the E.M.F. of the new cell will closely approach one volt. Further on I shall communicate some measurements, by which I intend to show how accurately the calibration of the fine-wire coil in volts can serve for that of the solid ring in ampères.

The Constant Shunt and Leading Wires.

With the size of the ring usually employed, viz. 30 centim. diameter, and our horizontal component of the earth's magnetism, currents of greater strength than about 50 ampères would require the ring to be at the multiplying powers 9 or 10, *i. e.* near the horizontal position. If, as a rule, such currents have to be measured, it is desirable to raise the constant of the galvanometer, say two or threefold, without a proportionate increase of the dimensions. This can be done by the use of a so-called "*constant shunt*," thereby allowing only half or one third of the current to flow round the needle. In our case the shunt is made of exactly the same metal as the solid ring itself; it has no soldering-places, and consists in fact of three or four little bridge pieces left standing instead of cutting the ring quite open where the terminals join. By comparison with an instrument of the same description having an open ring, the shunt can be adjusted to any power desirable.

However, by the introduction of the "constant shunt" the accuracy of the measurements is somewhat impaired. Experiments in which the shunt-pieces were touched with a thin stick of low-melting material during the passage of very strong currents, proved that they did not become hot, on account of their extremely low absolute resistance and their contact with the large metallic mass of the ring conducting away the heat. Variations of temperature, to which both the ring and the shunt are subjected, do not of course in the least disturb the ratio of their resistance, since they both consist of the very same alloy.

As the current only passes round the needle once and, if powerful enough, produces deflections even if the ring is almost horizontal, it is hardly necessary to call attention to the fact that the wires leading the current to the instrument should be so arranged that they cannot act upon the needle; still I have seen instances where this simple and almost self-evident precaution has been strangely neglected. I thought it therefore best to have special leading wires provided which are absolutely inactive upon the needle, and may therefore be named "*adynamic leads.*" These leads consist of a number of well-insulated copper wires stranded together in a peculiar manner, and covered with a cotton braiding, similar to the ordinary speaking-tubes. The cable thus formed is quite flexible, and without the slightest action upon a magnetic needle. I sent a strong current through several turns of such a cable, and held it close to a delicately suspended magnetic needle, but could not detect any effect whatever upon it. One half of the wires is covered with a differently coloured material to the other half; and the wires of each colour are united at both ends of the cable, and there soldered to a stout piece of copper wire. The adynamic cable can be made in any length and for different current-strengths; and as it offers only a small resistance, it can be employed to convey the current to be measured to a locality where the needles are not disturbed by machines or wires.

The Adjusting Parts and the Compensating Magnet.

With regard to the adjustment of the instrument, it suffices to say that, besides the necessary levelling-arrangements, it is provided with clamp-rings for tightly holding the pillar as well as the movable coil without interfering with previous adjustments, and in the same manner as is often done with mathematical apparatus. The final adjustment of the axis of the coil into the meridian is performed by means of a fine screw, which proves very useful for correcting, during a series of measurements, the occasional variations of the zero position.

As long as the galvanometer stands in the same position, the "*constant*," as a rule, changes but little even from day to day. If, however, the instrument is taken from one place to another, great changes in that respect will occur, amounting sometimes to many per cents. These changes do not of course interfere with the accuracy of the measurements, because the "constant" can easily be redetermined with a known E.M.F.

at any place, as we have seen; but it would undoubtedly be very convenient to have the galvanometer of *equal sensibility* everywhere. For that purpose an auxiliary magnet is placed east or west from the needle in a plane parallel to the meridian, which can turn round a horizontal axis passing through its neutral point and the centre of the needle, and being at right angles to the diameter on which the coil is turned. This magnet does not affect the zero position, and moreover, if placed exactly vertical with its magnetic axis, it does not alter the original constant, which then only depends upon the horizontal terrestrial component, more or less modified by the surroundings; but if it is dipped, the horizontal force acting on the needle is either augmented or diminished, according to the direction in which the magnet is turned and to the amount of dip given. It is easily seen that the magnetic influence of the surroundings upon the needle may now greatly vary from one place to another and still be compensated by the magnet, thus keeping the so-called "constant" of the galvanometer actually at a *constant value*, adjusting it, for instance, always so that the unit deflection of 45° with the vertical ring corresponds to a round number of volts and ampères, say five or ten. Under such conditions the deflection-scale could at once give the current or E.M.F. in ampères or volts. A gradual change of magnetism in the compensating magnet does not affect the measurements. I shall not further enter into this subject here, as I intend discussing it more fully on a future occasion.

Methods of Measurement.

For measuring current-strength or electromotive force either of the following four methods can be employed according to circumstances, viz.:—

I. *The General Method.*—Applicable under almost any conditions. The coil is placed in such a position that the deflection attains a proper value. If α is the deflection of the needle, and ϕ that of the coil from the meridian, the formula is

$$x = \tan\alpha \times \sec\phi \times \text{const.};$$

or in case the multiplying powers P on the quadrant are used, the formula becomes

$$x = \tan\alpha \times \mathrm{P} \times \text{const.}$$

II. *Method of Equality.*—With this method the coil is each time placed in such a position that the needle is deflected exactly to the *same angle* ψ to which the coil is inclined, giving the formula

$$x = \tan\psi \times \sec\psi \times \text{const.}$$

Having then only to deal with a single angle for a particular measurement, these products of tangents and secants may be calculated beforehand. For this purpose the natural sines are sufficient, because $\tan \times \sec = \frac{\sin}{\cos^2}$.

The following Table gives, for easy comparison, the values of tangents, secants, and their products at ten and multiples of ten degrees. These products, like the tangents, range from nil to infinity, but increase more rapidly.

TABLE II.

Angle.	0°	10°	20°	30°	40°	50°	60°	70°	80°	90°
tan	0	·176	·364	·577	·839	1·192	1·732	2·747	5·671	∞
sec	1·0	1·015	1·064	1·155	1·305	1·556	2·000	2·924	5·759	∞
tan × sec	0	·1786	·3873	·6664	1·095	1·855	3·464	8·032	32·66	∞

III. *Method of Constant Deflection.*—Here the coil is each time inclined until the needle reaches a certain deflection, say $26\frac{1}{2}°$, $45°$, or $63\frac{1}{2}°$, of which the corresponding tangents are $\frac{1}{2}$, 1, and 2 respectively. This figure then enters the constant, giving the simpler formula

$$x = \sec \phi \times \text{const.},$$

the instrument acting as a *secant-galvanometer*. For a given constant deflection the secant-measurements range between unity and infinity, as the above little table shows. This method has the peculiarity that the needle occupies a fixed position in space during the measurements, which in some instances may be found of advantage.

IV. *Method of Constant Inclination.*—In this case the coil is fixed at a given inclination, and $\sec \phi$ enters the constant; thus the formula is reduced to that of the ordinary tangent-galvanometer,

$$x = \tan \alpha \times \text{const.}$$

As compared with other galvanometers proposed for a similar purpose, the one here described offers the great advantage that the magnetic needle has not to be shifted from one measurement to another, whereby the magnetic field may sometimes considerably alter; furthermore it does not depend upon the constancy of permanent magnets, which, to say the least, is rather precarious*.

Numerical Results of Measurements.

I shall now communicate some measurements and tests to which the latest forms of instruments have been subjected, in order to illustrate the degree of accuracy obtainable. The first set was undertaken to ascertain the relation actually existing between the solid ring for currents and the coil of wire for E.M.F., which, it will be remembered, was intended to be such that ampères with the solid ring should accurately correspond to volts with the wire coil. The experiment was conducted as follows:—A current, from my constant battery with acid flow†, was sent through the solid ring and a copper voltameter in circuit for a certain time, the deflections to the right and left being observed every five minutes. The mean of these deflections was taken as corresponding to the amount of copper deposited. The copper-sulphate solution and the electrodes consisted of pure materials. The mean of the gain of the kathode and the loss of the anode was taken. The amount corresponding to one ampère per hour is 1·164 gramme of copper or 3·96 grammes of silver, the latter figure being that adopted by Messrs. Siemens and Halske, of Berlin‡. The calibration of the fine-wire coil for volts was performed by means of a number of Daniells, each compared with a Raoult's standard cell filled with pure sulphate solutions and having the E.M.F. 1·115 volt, according to Dr. Alder Wright's experiments §. The figures obtained were as follows:—

a. With the solid Ring.

Copper obtained $=11{\cdot}435$ grammes.
Time of electrolysis $=60$ minutes.
Mean deflection $=47^\circ{\cdot}3$.
Position of ring P $=2$.

Hence the current corresponding to the unit deflection of 45° with the ring vertical $=4{\cdot}531$ ampères.

b. With the fine-wire Coil.

Number of Daniells $=4$.
E.M.F. thereof $=4\times 1{\cdot}109$.
Deflection obtained $=44^\circ{\cdot}6$.
Position of coil P $=1$.

* Another advantage undoubtedly is that the galvanometer requires no variable shunt, by which errors may very easily be introduced.

† *Rep. f. exp. Phys.* xviii. p. 633 (1882).

‡ See latest instructions for use of their torsion galvanometers.

§ Proc. Phys. Soc. v. p. 80 (1882).

Hence the E.M.F. corresponding to the deflection of 45° with the ring vertical $=$**4·525** *volts*, the correction for the resistance of the cells being applied.

These two results agree very closely indeed, showing only a difference of 0·13 per cent. This is the more remarkable as the two kinds of measurements have nothing in common, being in fact based upon data quite independent of each other, thus proving that it is admissible to substitute for such instruments the calibration in volts for that in ampères.

A further series given in Tables III. and IV., and carried out with great care, clearly show that, for any given current-strength or E.M.F., the result of the measurement is almost identical in whatever region the readings are taken.

Table III. is obtained with currents from my constant battery passed through the solid ring. The six different current-strengths were obtained by the insertion of suitable resistances, and were as nearly as possible in the proportion of the whole figures 1 to 6.

Table IV. contains measurements with the fine-wire coil, thereby using the E.M.F. of ordinary Bunsen cells connected in series, and varying in number from 2 to 12.

From these Tables it will be seen that, on the one hand, the deflections extend over the greater part of the tangent-scale, *i. e.* from 3°·4 till 78°·2, and, on the other, the position of the coil varies from the multiplying power 1 to 10—the quantities measurable being therefore in the proportion of 1 to 500, yet the accuracy arrived at may be pronounced fully satisfactory. Combining the results of all these measurements with the solid ring and with the wire coil, the mean error of a single reading becomes 0·35 per cent., and the probable error 0·24 per cent.

The last set of measurements had for its object to show that a compensating-magnet of the description proposed does not affect the readings. The results are embodied in Table V.; they were obtained with one of the older forms of solid-ring galvanometers provided with a sine-scale. The curved controlling magnet of a mirror-galvanometer, 20 centim. long and 2 centim. broad, was strongly magnetized and placed at a distance of 24 centim. in the manner formerly specified, and so arranged that it could be turned on a horizontal axis. Three different positions were given to the magnet—viz. one, in which it assisted the earth's magnetism, another, in which it did not act upon the needle, and a third, in which the earth's magnetism was partly neutralized. By altering the resistance in circuit, the deflections with the vertical ring were made equal in all three cases, viz. 63°·5.

Table III.—Measurements with the Solid Ring.

Multiplying powers corresponding to the position of the ring.	Current-strength.																	
	C_1.			C_2.			C_3.			C_4.			C_5.			C_6.		
	a.	tan a.	P×tan a	a.	tan a.	P×tan a	a.	tan a.	P×tan a	a.	tan a.	P×tan a	a.	tan a.	P×tan a	a.	tan a.	P×tan a
P=1	30·7	·5938	·594	49·95	1·190	1·19	60·65	1·778	1·78	66·8	2·333	2·33	71·35	2·963	2·96	74·7	3·655	3·66
2	16·6	·2981	·596	30·7	·5938	1·19	41·6	·8878	1·78	49·5	1·171	2·34	56·15	1·491	2·98	61·25	1·823	3·65
3	11·2	·1980	·594	21·65	·3969	1·19	30·6	·5914	1·77	37·9	·7785	2·34	44·75	·9913	2·97	50·55	1·215	3·65
4	8·4	·1477	·591	16·65	·2991	1·20	23·85	·4421	1·77	30·4	·5867	2·35	36·7	·7454	2·98	42·5	·9163	3·67
5	6·75	·1183	·592	13·4	·2382	1·19	19·5	·3541	1·77	25·1	·4684	2·34	30·8	·5961	2·98	36·1	·7292	3·65
6	5·7	·0998	·599	11·15	·1971	1·18	16·4	·2943	1·77	21·3	·3899	2·34	26·35	·4953	2·97	31·35	·6092	3·66
7	4·8	·0840	·588	9·65	·1700	1·19	14·15	·2521	1·77	18·4	·3327	2·33	23·0	·4245	2·97	27·5	·5206	3·64
8	4·25	·0743	·594	8·4	·1477	1·18	12·5	·2217	1·77	16·3	·2924	2·34	20·35	·3709	2·97	24·5	·4557	3·65
9	3·8	·0664	·598	7·5	·1317	1·19	11·05	·1953	1·76	14·5	·2586	2·33	18·3	·3307	2·98	21·95	·4030	3·63
10	3·35	·0585	·585	6·7	·1175	1·18	10·0	·1763	1·76	13·1	·2327	2·33	16·5	·2962	2·96	20·0	·3640	3·64
Mean of P×tan a	...	...	·593	...	...	1·187	...	...	1·769	...	...	2·336	...	...	2·973	...	...	3·647
Mean error of obs.	±0·0043=·72 p. c.			±0·0060=·51 p. c.			±0·0064=·36 p. c.			±0·0069=·29 p. c.			±0·0074=·25 p. c.			±0·0101=·28 p. c.		
Prob. error of obs.	±0·0029=·49 ,,			±0·0040=·34 ,,			±0·0043=·24 ,,			±0·0047=·20 ,,			±0·0050=·17 ,,			±0·0068=·19 ,,		
Mean cur. in amp. (P×tan a×c*)	2·69			5·39			8·03			10·61			13·50			16·56		
Ratio	1			2			3			3·9			5			6·1		

* The galvanometer constant c =4·54 ampères.

Table IV.—Measurements with the fine-wire Coil.

Multiplying powers corresponding to the position of the coil.	Electromotive force.																	
	E_1.			E_2.			E_3.			E_4.			E_5.			E_6.		
	a.	tan a.	P×tan a	a.	tan a.	P×tan a	a.	tan a.	P×tan a	a.	tan a.	P×tan a	a.	tan a.	P×tan a	a.	tan a.	P×tan a
P=1	39·2	·8156	·816	58·2	1·613	1·61	67·4	2·402	2·40	72·7	3·211	3·21	76·0	4·011	4·01	78·2	4·787	4·79
2	22·2	·4081	·816	38·55	·8084	1·62	50·3	1·205	2·41	58·05	1·603	3·21	63·5	2·006	4·01	67·4	2·402	4·80
3	15·25	·2727	·818	28·3	·5384	1·62	38·85	·8055	2·42	46·9	1·069	3·21	53·2	1·337	4·01	58·1	1·607	4·82
4	11·55	·2044	·818	22·0	·4040	1·62	31·1	·6032	2·41	38·7	·8012	3·21	45·15	1·005	4·03	50·2	1·200	4·80
5	9·3	·1638	·819	18·0	·3249	1·63	25·8	·4834	2·42	32·7	·6420	3·21	38·7	·8012	4·01	43·8	·9590	4·80
6	7·8	·1370	·822	15·1	·2698	1·62	21·9	·4020	2·41	28·0	·5317	3·19	33·65	·6657	3·99	38·6	·7983	4·79
7	6·7	·1175	·823	13·0	·2309	1·62	19·0	·3443	2·41	24·5	·4557	3·19	29·75	·5716	4·01	34·45	·6860	4·80
8	5·8	·1016	·813	11·35	·2007	1·61	16·8	·3019	2·42	21·8	·4000	3·20	26·5	·4986	3·99	30·9	·5985	4·79
9	5·2	·0910	·819	10·1	·1781	1·60	15·0	·2679	2·41	19·55	·3551	3·20	24·0	·4452	4·01	28·15	·5351	4·82
10	4·7	·0822	·822	9·15	·1611	1·61	13·55	·2410	2·41	17·7	·3191	3·19	21·7	·3979	3·98	25·55	·4781	4·78
Mean of P×tan a	...	...	·819	...	...	1·615	...	...	2·413	...	...	3·201	...	...	4·003	...	...	4·798
Mean error of obs.	±0·0032=·39 p. c.			±0·0064=·40 p. c.			±0·0044=·18 p. c.			±0·0083=·26 p. c.			±0·0119=·30 p. c.			±0·0129=·27 p. c.		
Prob. error of obs.	±0·0021=·26 „			±0·0043=·27 „			±0·0029=·12 „			±0·0056=·17 „			±0·0084=·21 „			±0·0087=·18 „		
Mean E.M.F. in volts. (P×tan a ×e*.)	3·72			7·34			10·95			14·53			18·17			21·78		
Ratio	1			2			2·9			4			5			6		

* The galvanometer constant e =4·54 volts.

Table V.

Position of compensating magnet.	Extra resistance inserted.	Sine of inclination.						Mean of $\frac{\tan \alpha}{\sin \phi}$.	Mean error of observation.
		$\sin \phi$	·2	·4	·6	·8	1·0		
Dipped to augment earth's magnetism.	0	deflect. α	22°·1	38°·9	50°·4	58°·2	63°·5	2·020	$\pm$0·005 or 0·25 p. c.
		$\tan \alpha$	·4061	·8069	1·209	1·613	2·006		
		$\frac{\tan \alpha}{\sin \phi}$	2·03	2·02	2·02	2·02	2·01		
Vertical, having no action.	0·13 ohm.	deflect. α	21°·9	38°·7	50°·3	58°·0	63°·5	2·006	$\pm$0·004 or 0·20 p. c.
		$\tan \alpha$	·4020	·8012	1·205	1·600	2·006		
		$\frac{\tan \alpha}{\sin \phi}$	2·01	2·00	2·01	2·00	2·01		
Dipped to reduce earth's magnetism.	0·56 ohm.	deflect. α	21°·9	38°·9	50°·4	58°·2	63°·5	2·016	$\pm$0·004 or 0·20 p. c.
		$\tan \alpha$	·4020	·8069	1·209	1·613	2·006		
		$\frac{\tan \alpha}{\sin \phi}$	2·01	2·02	2·02	2·02	2·01		

Table V. shows that the degree of accuracy did not materially differ under the three varying conditions. The magnet therefore does not appreciably interfere with the measurements. The mean error of all three positions of the magnet is 0·22 per cent., which is very low.

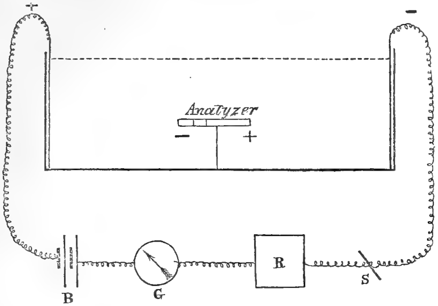
In conclusion, I may mention that a smaller model of the galvanometer, intended for practical use, is now being made, which will contain all the recent improvements, viz. the fine-wire coil besides the solid ring, the tangent-scale, the secant-marks, the air-damping, and the compensating-magnet. The latter will be so arranged that the "constant" will be considerably increased as compared with that due to the earth's magnetism alone; thus the needle should be much less influenced by outer disturbances than before.

Woolwich, June 1883.

XIV. *The Influence of Current, Temperature, and Strength of Electrolyte on the Area of Electrification. By* Alfred Tribe, *F.Inst.C., Lecturer on Chemistry in Dulwich College.*

In the study of the subject of this paper I have employed the "electro-chemical method" described by me in the Philosophical Magazine, 1881, xi. p. 446. It will be remembered that by this method images of the electrifications produced on metallic plates are rendered evident to the eye, and also that I have almost invariably employed as *analyzers* plates of silver, and as the electrolyte a solution of copper sulphate. The areas of the electrifications were found by measuring the copper and peroxide of silver respectively deposited or formed on the analyzing plate, the copper giving the area of the − electrification, and the silver peroxide that of the + electrification. The sum of the two electrifications subtracted from the total area of the plate, it will be seen, gives the area of non-electrification, *i. e.* the intermedial space, or that part of the plate which separates the + and −, and where the electromotive force is incapable of initiating electro-chemical action. These measurements are of course only relative.

The following conditions were common to the several experiments to be described:—an electrolytic cell 120 millim. broad, 128 deep and 305 long, filled to within 8 millim. with copper-sulphate solution; copper electrodes of the same area as the ends of the cell; a plate (analyzer) of fine silver, 67 × 7 millim., weighing 0·75 gram, placed lengthwise midway between the electrodes, and between the surface of the liquid and the bottom of the cell. The time of each experiment was six minutes. The diagram shows the general arrangement—B being the battery; G, tangent-galvanometer; R, rheostat; and S, switch.



I. *Influence of Current.*—In this series of experiments a 5-per-cent. solution of copper sulphate was employed. The results obtained and the current employed are set out in the following table. The numbers refer to lengths in millimetres of the several electrifications and intermedial spaces.

Current in ampères.	− electrification.	Intermedial space.	+ electrification.	+ referred to − as unity.
0·1	4	...	nil.	......
0·2	13	...	signs at end edges.	......
0·4	18	30	19	1·05
0·6	18	18	31	1·72
0·8	16	16	35	2·19
1·0	17	12	38	2·23
1·2	18	11	38	2·11
1·4	19	10	38	2·00
1·6	21	8	38	1·81
1·8	22	7	38	1·72
2·0	23	6	38	1·65

The usual sign of + electrification was invisible on the analyzer in the first experiment, but the − was sufficiently marked to permit of measurement.

The relation which I have already shown to exist between the magnitude of the intermedial space on an analyzer and the force required to separate an electrolyte into its constituent ions (Phil. Mag. Oct. 1881) naturally led me to look for some well-defined relation between the intermedial space on the analyzers and the current. The electromotive force between the electrodes, and therefore, as it may be supposed, of the molecules of the electrolyte, varied of course with the strength of the current; and the electromotive force set up on the analyzers might consequently also be supposed to vary in the same way. Now as the force required to overcome the affinity of the ions was constant, and as the electromotive force decreases from the ends of an analyzer in the position of the ones employed, it might be expected that the intermedial space would decrease in proportion as the electromotive force increased between the electrodes. That such a relation exists would appear to be fairly well established by the results of this series of experiments. In the annexed table the intermedial spaces are given in millimetres side by side with the numbers calculated on the supposition that the spaces vary inversely as the electromotive force of the electrolytic molecules.

Current.	Found.	Calculated.
0·4	30	30
0·6	18	20
0·8	16	15
1·0	12	12
1·2	11	10
1·4	10	8·5
1·6	8	7·5
1·8	7	6·7
2·0	6	6

There are two other points brought out by these experiments to which I would direct attention. The one is the want of uniformity in the increase of the opposite electrifications with the increase of current; and the other is the remarkable constancy of the + electrification from one to two ampères.

II. *Influence of Strength of Electrolyte.*—In this series of experiments the current was constant, one ampère being employed. The strengths of copper sulphate used for the several experiments are given in the annexed table of results.

Percentage of $CuSO_4$.	− electrification.	Intermedial space.	+ electrification.	+ referred to − as unity.
1	27	7	33	1·14
2	25	8	34	1·36
4	18	12	37	2·05
6	15	14	38	2·53
8	14	16	37	2·64
10	12·5	18·5	36	2·88
12	12	22	33	2·75
14	10	28	29	2·9
16	9·5	29·5	28	2·94

As already mentioned, the same strength of current flowed for equal times in the several experiments. It may therefore, I think, be assumed that the same quantity of electrification was set up on the analyzers, and, further, that the differences in the magnitude of the electrifications were determined by an influence or influences other than that of the quantity of electricity which flowed into the electrolyte. The resistance of a solution of copper sulphate increases, as is well known, with its dilution; and as the current was constant throughout, the electromotive force of the molecules of the several solutions necessarily increased in the same proportion as their resistances. The variations in the electromotive force must therefore again be regarded as the chief cause of the differences in the magnitude of the electrifications, and consequently also of the intermedial spaces. It is certain that the electrolytic radicals of sulphate of copper were separated on the analyzers in each of the experiments; but it by no means follows that the molecular decompositions were identical in each case; for it is known that the molecular constitution of salts in solution varies within certain limits with variations in the proportion of water. The relation already shown to obtain between the intermedial space and the electromotive force of similarly constituted molecules would lead to the conclusion that the intermedial spaces in this series of experiments would be inversely proportional to the resistances of the solutions, supposing that their molecular constitutions were similar. I have not the resistances for the exact strengths of the solutions; but, taking approximations to these, I find the relation referred to holds good in solutions varying from 6 to 12 per cent. of copper sulphate—*i. e.* where the viscosity on the one hand, and the tendency to decomposition of the metallic salt on the other, perhaps differ so little as to be practically inappreciable. The data &c. are set out in the annexed table:—

Strength of copper-sulphate solution.	Approximate resistance.	Intermedial space.	Resistance × by intermedial space.
6 per cent.	1·6	14	22·4
8 "	1·3	16	20·8
10 "	1·16	18·5	21·5
12 "	1·04	22	22·8

III. *Influence of Temperature.*—In this series of experiments the current was also constant, an ampère being again employed. The strength of electrolyte was likewise constant, a 5 per cent. solution of copper sulphate being employed. The temperatures are given in the annexed table of results:—

Temperature.	— electrification.	Intermedial space.	+ electrification.	+ referred to — as unity.
4° C.	19	12	36	1·90
8°	17	12	38	2·23
16°	16·5	14·5	36	2·18
24°	16·5	16·5	34	2·06
32°	17	18	32	1·88
40°	19	20	28	1·47
56°	21	27	19	0·90
72°	22	31	14	0·63
88°	25	36	6	0·24

As a rule, the boundaries of the electro-deposits are nearly or quite straight; the peroxide-of-silver boundaries, however, in the experiments above 32° C. were concave. Thinking that this concavity arose from the time being insufficient, I repeated some of the experiments, prolonging the time to 20 minutes; but the results were practically the same. The intermedial spaces in the higher-temperature experiments also exhibited signs of slight oxidation, which I am inclined to attribute to the disturbing influence of convection-currents.

It will be observed that the intermedial space increased as the temperature increased. This, again, agrees with the principle connecting the area of the spaces with electromotive force; for as the temperature increased, of course the resistance of the solution decreased; the electromotive force of the molecules necessarily also decreased; and the magnitude of the intermedial spaces increased, in accordance with the principle referred to. I have not the resistances of the solutions at the temperatures of my experiments, and cannot, therefore, institute a numerical comparison.

The very small + electrifications at the high temperatures is very noticeable; and I have thought that this may be not unconnected with differences in the action of heat itself on the opposite electricities.

It will be noticed that one feature common to the experiments described is the inequality in the magnitude of the electrifications of opposite sign on the several analyzing plates. This fact naturally attracted my attention some years ago (Proc. Roy. Soc. no. 181, 1877); and the work of this paper was undertaken mainly in the hope of throwing light on the subject. I cannot say it has brought about the desired result. It has, however, cleared away one or two speculations, suggested the direction of further inquiry, and shown, among other things, that the condition of the electrolytic field affects not only the area of the electrifications, as this was to be foreseen, but also the ratio of the − to the + electrification. The numbers in the fifth columns in the above tables show the great diversity in this respect; and in my recent paper (Phil. Mag. June 1883) I referred to more examples, under conditions, too, where it appeared very difficult to imagine the nature of the difference in the condition of the surrounding molecules. As to the meaning of this disparity in the ratio of the electrifications, I am unable to form a conception satisfactory to myself.

The work has moreover established the definite relation already pointed out between intermedial space and electromotive force. And on an examination into the effects of what I have named "Dissymmetry in the Electrolytic Discharge," in the light of this relation, I believe I observe a difference of electromotive force of the molecules where the dissymmetry manifests itself, *i. e.* on the + and − sides of non-uniform fields. The evidence consists in differences in the area of the spaces on analyzers in corresponding parts of the field relative to the electrodes, or sides of the trough. I find that the area of the intermedial space on an analyzer on the + side is invariably smaller than that on the − side, from which it would follow that the electromotive force of the molecules on the + side is invariably the greater. This conclusion is supported by another observation of a somewhat different character. On the analyzers *c* and *d*, shown in fig. 1, p. 393 of my paper in the June number of the Philosophical Magazine, it is evident that a greater amount of electrochemical action was set up on *c*; and *c*, it will be observed, is on the + side of the trough, where it has been inferred that the electromotive force of the molecules is the greater. I think it not improbable that this difference in electromotive force is related to Faraday's observation ('Experimental Researches,' Series xii., xiii.) that "Negative electricity discharges into air at a somewhat lower tension than positive electricity." The molecules surrounding the electrode discharging + electricity would, of course, be differently affected electrically than when the electrode was discharging − electricity; and if this be true of molecules of air, why may it not be equally true of the molecules of a solution of copper sulphate or other electrolyte. A difference in the electromotive force of molecules in corresponding positions would appear, then, to be a feature of dissymmetry in certain electrolytic fields; but the more noteworthy effect of this dissymmetry is seen in the differences in the area of like electrifications on the correspondingly placed analyzers.

June 25, 1883.

XV. *On the Change in the Double Refraction of Quartz produced by Electrical Force.* By W. C. Röntgen*.

IN a previous paper † I have described the electro-optical behaviour of plates of quartz cut parallel to the optical axis, through which the rays of light passed at right angles to this axis; this I proposed to supplement by a description of experiments with plates cut at right angles to the optical axis. I had already observed most of the optical phenomena described in the following paper; but the explanation of them was not clear. This is now given by a piezoelectric examination of the quartz which I have carried out. The present paper therefore consists of two parts—the first describing the piezoelectric, the second the electro-optical experiments.

Two pieces of quartz were, for the most part, employed for the piezoelectric experiments. The one is a circular plate cut exactly at right angles to the optical axis, having a thickness of 0·58 centim. and a diameter of 1·8 centim.; the other is a sphere of 3 centim. diameter. Both are of good quality, as shown by optical examination. It will be clearly seen, in what follows, how advantageous it is to employ pieces of quartz of this form, and not crystals with natural faces.

The plate was employed to subject the piezoelectric behaviour of quartz described in the first paper to a direct experimental proof. The result obtained was that there are in reality three directions in quartz, all at right angles to the principal axis, and making angles of 120° with each other, which possess the property that pressure exerted upon the crystal in any one of these directions produces no electricity at the points of pressure: these are the three axes of no piezoelectricity. A pressure exerted in any other direction at right angles to the principal axis produces electricity at the points of pressure, the greatest manifestation taking place in the lines bisecting the angles between the axes named. These lines I called axes of maximum piezoelectricity. The sign of the electricity produced is in accordance with the previous description.

In order to exert a pressure upon the plate in a definite direction a compression-apparatus of ordinary construction was employed, the opposing steel cheeks of which were faced with ebonite plates upon which pieces of silver wire 0·06 centim. thick were cemented. These silver wires were exactly opposite and parallel to each other, and the quartz plate was

* Translated from the *Ber. der Oberh. Ges. für Natur- und Heilkunde,*
† Phil. Mag. [5] xv. p. 132.

compressed between them. This arrangement ensured that the points of pressure formed a narrow line upon the plate, parallel to the axis of the disk, and that the pressure was exerted upon the disk exactly in the direction of a diameter.

The disk was attached by means of a straw about 4 centim. long to a divided circle, so that its axis coincided with the axis of rotation of the divided circle. Consequently by rotating the circle the direction in which the pressure was exerted upon the disk could be changed and the angle of rotation read off on the divided circle.

The frame of the compression-apparatus and the mounting in which the divided circle turned were rigidly attached by means of clamps to a stand. To ensure that the pressure was always applied in the direction of a diameter the following plan was adopted. The quartz plate was covered with a thin layer of wax, in which, by means of a fine needle, a considerable number of diameters were traced. With the aid of these lines it was easy to ensure that the direction of pressure coincided with a diameter. One of the two silver wires was connected with the delicate electroscope previously mentioned; the other was put to earth. The experiment, then, consisted in determining the direction of no piezoelectricity. The results of a single series of experiments will serve as example :—

Reading of Divided Circle.	Piezoelectricity due to increased pressure.
0°	Strongly negative.
20	Less strongly negative.
35	Feebly positive.
27	Feebly negative.

Having thus found that one of the directions sought lies between the last two positions of the disk, the following experiments were made :—

Reading of circle.	Piezoelectricity due to increased pressure.	Reading of circle.	Piezoelectricity due to increased pressure.
29°	Trace of negative electricity.	209°	Trace of positive electricity.
30	Nothing.	210	Nothing.
31	Trace of positive electricity.	211	Trace of negative electricity.
89	Trace of positive electricity.	269	Trace of negative electricity.
90	Nothing.	270	Nothing.
91	Trace of negative electricity.	271	Trace of positive electricity.
149	Trace of negative electricity.	329	Trace of positive electricity.
150	Trace of positive electricity.	330	Nothing.
151	Trace of positive electricity.	331	Trace of negative electricity.

On releasing the compressing-screw electricity of the opposite kind was obtained in every case.

These experiments leave no doubt that the angle between the three axes of no piezoelectricity is 120° ; the single deviation observed, which did not amount to 1°, may very well be set down to experimental error.

Pressures exerted upon the plate in directions lying between the axes of no piezoelectricity produced a powerful evolution of electricity. Since the electroscope employed was not adapted for quantitative experiments, I was not able to determine how the quantity of piezoelectricity varied with the direction; it is, however, certain that the largest quantity was produced in these intermediate directions. For an accurate quantitative determination it would be absolutely necessary to take account of the inductive action which the electricity excited on the surfaces near the points of pressure would exert upon the electrode connected with the electroscope. This action will be considered later on.

The experiments with the sphere of quartz required a somewhat more complicated apparatus. Here also it was necessary that pressure should be exerted upon the sphere as nearly as possible along a diameter, and that the resulting piezoelectricity should be observed. After numerous experiments with different forms of apparatus, the following arrangement was adopted as the most convenient. The sphere was placed upon the stage of an old and large microscope, and pressure exerted upon it by lowering the heavy body of the microscope down upon it.

The stage is provided with the necessary screws for exact adjustment; in the circular opening of the stage can be placed either a round thick disk of brass or a disk of ebonite which fits it exactly. The disk of brass is provided at its centre with a depression 0·25 centim. wide, which gives better support to the sphere laid upon it; the sphere therefore does not touch the disk in one point only, but in a small circle. A small thick brass cylinder 1 centim. long, 0·4 centim. thick, is cemented on to the ebonite disk, which is also provided at the top with a small depression which lies exactly in the axis of the disk of rubber and upon which the sphere rests. The brass disk was used when the lower side of the sphere did not require to be insulated, and the rubber disk when insulation was necessary.

The microscope-tube, which moved accurately in a solid stand by means of rack-work, was fitted at its lower end with a piece of brass rod rounded at the lower end and insulated by means of ebonite, by means of which the pressure upon the sphere is exerted. It is unnecessary to explain how the adjustment is effected, and the means adopted to ensure that the pressure was actually exerted along a diameter. The tube of the microscope was weighted with 2 kilogrammes; there was no perceptible friction in lowering it, between the brass rod and the sphere; any such must be carefully avoided, since it would cause an energetic excitement of electricity.

As I designed to test other portions of the sphere for piezoelectricity besides those upon which the pressure was directly exerted, the gold leaf of the electroscope was not always connected directly with the brass rod by means of which the pressure was exerted, but often with a small metallic holder fastened to an ebonite rod, in which could be placed at pleasure either a simple brass wire, or a wire provided with a small metallic disk*. The ebonite rod was held by a jointed stand, so that the wire or metal disk which served as electrode could be placed in contact with any point of the quartz sphere.

The following experiments were made with this sphere:—

(1) Besides the three axes of no piezoelectricity, the principal axis of the quartz also possesses the property that pressure exerted upon the crystal in this direction produces no electricity at the points of pressure. The question then arises whether there exist also other directions having the same property.

Without having previously determined by optical means the position of the principal axis, I placed the sphere upon the previously described brass disk of the stage, connected the rod producing the pressure with the electroscope, and determined by experiment the points on the surface of the sphere which did not become piezoelectric when pressure was exerted upon them in the direction of a diameter. When such a point had been found, it was marked upon the sphere by moistening the brass rod with india-ink and then gently lowering it upon the sphere. In this way the sphere was marked with 40 or 50 small dots. It is easily understood that in this examination a somewhat systematic method had to be adopted. We find, for example, very soon that certain portions of the surface of the sphere become only very feebly electric ; these, as seen afterwards, were the portions of the surface in the neighbourhood of the ends of a diameter parallel to the principal axis. It is difficult here to say exactly where no piezoelectricity at all results ; hence these places are not

* These parts of the apparatus were so constructed as to have as small a capacity as possible. The chief advantage of Fechner's Electroscope over Thomson's Electrometer, apart from its better insulation, consists in its extremely small capacity. The instrument is thus well adapted for the present investigation.

marked. On examining the quartz sphere marked with ink-spots, the result obtained was that the position of these points is simply determined by three planes which cut each other at an angle of 120°, in the diameter parallel to the principal axis, and contain the three axes of no piezoelectricity. The direction of the principal axis determined in this way agreed very well with that subsequently found by optical means.

It hence follows that all straight lines lying in any of the three planes specified are directions of no piezoelectricity; a pressure exerted upon the crystal in one of these directions produces no electricity at the points of pressure. These planes are therefore called planes of no piezoelectricity.

The following values were obtained with the sphere described, by measurement of the six angles included between the three planes :—

$$58°, 61°, 60°, 60°, 59°, 62°.$$

These angles ought to be exactly 60°. The deviations from this number may result in part from experimental errors; they are, however, probably also a consequence of small deformations and irregularities of the quartz, the presence of which could not be determined by optical methods in consequence of the spherical form of the crystal. Such deformations have considerable influence on the distribution of piezoelectricity, as Hankel has shown with crystals having natural faces. Thus, for example, with another sphere which plainly showed visible irregularities, I obtained the following angles :—

$$51°, 54°, 69°, 57°, 64°, 65°.$$

The experiments with the first crystal were several times repeated—thus, for example, once at a temperature of about 10° C., a second time at about 31°C. I found always the same position of the three planes.

(2) After the position of the planes of piezoelectricity on the sphere had been determined and marked, I examined in what way the fields lying between these meridians were piezoelectric. The result was that the electricity excited at all points of pressure lying in any one of the six fields was of the same kind, but that it changed on passing from one field into the next. The whole sphere may therefore be divided into six fields alternately positively and negatively piezoelectric. For the clearer understanding of what follows, it will be well to designate them 1, 2, 3, 4, 5, 6, and to assume that at points of pressure lying in the first field positive electricity was produced: consequently the fields must be marked in order +, −, +, −, +, −.

The further result was obtained that the most energetic evolution of electricity took place when pressure was exerted in directions at right angles to the principal axis, and bisecting the angles between the planes of no piezoelectricity; these are the directions already designated as axes of maximum piezoelectricity.

I scarcely need remark that diminution of pressure produces electricity opposite in kind to that produced by increase of pressure; the same holds good for all the following experiments.

(3) A pressure exerted in the direction of an axis of no piezoelectricity produces no electricity at the points of pressure. The question, however, arises whether under these circumstances there is no piezoelectricity at other points of the sphere*. To answer this question, the sphere was so placed upon the metal disk that the principal axis was at right angles to the direction of pressure, and so that the pressure was exerted in the direction of an axis of no piezoelectricity. The electroscope was connected with the insulated electrode previously described, which was successively placed in contact with different points of the sphere, the direction of pressure remaining unaltered.

The following results were obtained:—The plane of no piezoelectricity in which the pressure is exerted divides the sphere into two halves, which both become electric all over, the one half being positive, the other half negative. The sign of the electricity produced upon each half is determined by the sign of the electricity excited on that one of the two fields, divided by the direction of pressure, which is situated on that half. Thus, for example, if the pressure takes place in the plane dividing the field 1 from 6, and 3 from 4, then the half upon which the fields 1 and 3 lie becomes positive, and the other half negative.

Maximum electricity is evolved at the ends of an axis of maximum piezoelectricity lying at right angles to the direction of pressure—in the above example therefore in the middle of fields 2 and 5. There is no electricity produced upon the circle in which the plane of no piezoelectricity containing the direction cuts the sphere.

The same result was obtained when the sphere was placed upon the insulating brass cylinder, or if the support and the brass rod exerting the pressure were connected with the earth.

* Messrs. J. and P. Curie have found that a pressure exerted in the direction of an axis of no piezoelectricity upon a parallelepiped of quartz produces electricity at the ends of the axis of maximum piezoelectricity at right angles to the direction of pressure.

(4) The further question arises, how the free surface of the sphere would be affected if the pressure acted along an axis of maximum piezoelectricity.

The quartz sphere was placed in the proper position on the insulated brass cylinder, the brass rod being also insulated. The electrical examination of the surface showed then that the sphere was again divided into two halves charged with opposite electricities, separated by a plane of no piezoelectricity at right angles to the direction of pressure. The sign of the electricity was determined by the sign of the fields as found above in which the points of pressure lay. If, for example, field 1 was above and consequently field 4 below, the upper half became positive, and the lower half negative. The maximum of electricity appeared at the points of pressure, and no electricity at all along the circle in which the plane of no piezoelectricity at right angles to the direction of pressure cuts the sphere.

If the two points of pressure were connected with the earth, essentially the same distribution of electricity was observed.

(5) The sphere was subjected to pressure in a direction intermediate between an axis of no piezoelectricity and the nearest axis of maximum piezoelectricity, but still at right angles to the principal axis; both points of pressure were insulated. The sphere was again found to be divided by a plane passing through the principal axis into two halves having opposite electricities; but the plane was no longer parallel or at right angles to the direction of pressure, but lay in the acute angle formed by the direction of pressure with the next but one axis of no piezoelectricity. The nearer the direction of pressure lay to the axis of no piezoelectricity the smaller was the acute angle between the direction of pressure and the plane of bisection. If in passing from one experiment to another the direction of pressure was changed so that, to begin with, it coincided with a direction of maximum piezoelectricity, and, to end with, coincided with the next axis of no piezoelectricity, then the position of the plane of bisection also revolved through an angle of 90° about the principal axis.

The sign of the electricity evolved was determined from the sign of the fields in which the points of pressure lay. If, for example, the point of pressure was situated in field 1, but nearer to field 2 than to field 6, then the half of the sphere which contained a part of field 2, the whole of fields 6 and 1, and a part of field 5 became positively, and the other half of the sphere became negatively electric.

If the pressure acted in the line bisecting the angle between an axis of maximum and an axis of no piezoelectricity, then the plane of bisection was inclined to the direction of pressure at an angle which at any rate did not differ much from 45° ; a more accurate determination of this angle was not possible with the apparatus at my disposal.

The maximum piezoelectricity was found at the ends of the diameter at right angles to the plane of bisection—consequently, in the last mentioned case, at the ends of a diameter at right angles to the axis and inclined at 45° to the direction of pressure—that is, at the ends of an axis of no piezoelectricity.

In the three following series of experiments the quantities of electricity produced were so small that I cannot regard the results communicated as absolutely certain. I do not, however, believe that any different result would be obtained by the use of more delicate apparatus; but such a confirmation would be desirable.

(6) A direction lying between an axis of maximum piezoelectricity and the principal axis was chosen as the direction of pressure. The sphere was divided into two oppositely electrified halves by that plane of no piezoelectricity which was at right angles to the plane passing through the direction of pressure and the principal axis. The sign of the electricity is determined by that of the fields in which the points of pressure are situated. If, for example, they lay in fields 1 and 4, then fields 6, 1, and 2 were positive, and fields 3, 4, and 5 were negative.

(7) If the pressure was exerted in any direction different from those already considered and not coincident with the principal axis, the sphere was always divided into two oppositely electrified halves by a plane which in all cases passed through the principal axis.

(8) Increase of pressure in the direction of the principal axis produced small quantities of electricity in the six fields, the sign of which corresponded to the sign of the fields as given above: the points of pressure were not electrified.

It follows from the above that, whatever the direction of pressure, there is never any perceptible evolution of electricity at the ends of a diameter parallel to the principal axis. It should be mentioned, that in the dry air of a room the piezoelectric experiments take place extremely regularly and with certainty.

An objection may be raised to the results described on the ground that they are influenced by induction or possibly by surface-conduction of electricity. It is not to be denied that these causes, and in particular induction, had a part to play ; but I have repeatedly convinced myself that these results remained qualitatively the same when such disturbing causes were removed. I will mention here only a few of the observations leading to this conclusion, which will serve to show how carefully it is necessary to proceed and how the results may be confirmed.

Two opposite fields of the sphere, *e.g.* 2 and 5, were completely covered with tin-foil, but so that the two pieces of tin-foil did not touch each other; then the sphere was so placed upon the insulated brass cylinder that the pressure took place in the direction of an axis of no piezoelectricity joining the bounding line between fields 1 and 6 with that between fields 3 and 4. The brass rod by which the pressure is exerted was in connexion with the electroscope. If then all the fields remained insulated, the electroscope showed no electricity upon change of pressure; but if field 2 were connected with the ground, positive electricity was observed, produced by the induction of field 5, which was strongly charged with positive electricity. The electroscope showed a negative charge if field 5 alone were connected with the ground.

If, when fields 2 and 5 were connected with the ground, field 1 was also put to earth by being touched with the finger, I obtained a feeble negative deflection of the gold leaf due to induction by the negative electricity produced on field 6.

In order to show that in the position of the sphere indicated it was actually the whole halves of the sphere, and not alone the fields 2 and 5, which became electrified, these fields were connected with the earth, and fields 1, 3, 4, and 5 in order were tested with the electrode connected to the electroscope. They were found to be, upon increase of pressure, feebly but decidedly electrified, positively or negatively as the case might be.

If the sphere were placed upon the brass disk connected with the earth, with the covered portions above and below, and if pressure were exerted upon it in the direction of an axis of maximum piezoelectricity, positive electricity was obtained upon the whole sphere if the positive field 5 was uppermost, but, on the other hand, negative electricity if the negative field 2 was uppermost. In these cases the inductive action of the piezoelectricity produced above overpowered the feeble action of the opposite electricity present upon the lower half of the sphere. Normal conditions resulted when the lower field was also insulated. In this position of the sphere it could be shown that electricity was present upon the whole whole upper and lower halves, and not only at the points of pressure.

If the coated fields were connected with the earth, the other fields showed qualitatively the same distribution of electricity as already described.

I pass on now to describe the electro-optic experiments. I employed for these experiments the square quartz plate described in the first paper cut parallel to the side faces, as well as the parallelepiped marked II. and a small quartz cylinder. A fuller description of the two first mentioned crystals will be found in my first paper. The plate was examined both in parallel and in convergent light. For the experiments with parallel light, the plate was placed in a horizontal position in the flask filled with benzol; the lower electrode was the brass disk provided with two glass strips; the upper electrode a brass wire projecting into the depression in the plate (compare the former arrangement of apparatus).

Sodium-light linearly polarized at an angle of 45° to a horizontal plane traversed the plate parallel to the principal axis; and the analyzer was turned so as to give a dark field. When electrification took place, the centre of the field of view —the place beneath the depression—became markedly bright; the arrangement of the bright places being different according as the upper or lower electrode was made positive.

This result surprised me, since electrical forces acting along an axis of no piezoelectricity ought to produce no compression or dilatation in this direction.

The behaviour of the plate in convergent light was just as surprising to me. It certainly brought me nearer to an explanation of the first experiment; but a complete explanation only became clear after carrying out the piezoelectric experiments described above.

In order to examine the plate in convergent light, glass tubes 0·7 centim. wide, bent upwards at right angles, were cemented to the square faces of the plate, and were filled with mercury. Then the plate was so placed under a Steeg's polarizing microscope that the axial image of concentric circles could be observed, illumination being obtained from a sodium-flame.

The tubes containing mercury were severally connected with the electrodes of the electrical machine. At the moment that the electricity began to act upon the plate, the circles became changed into ellipses, the major axis of which, longer than the diameter of the corresponding circle, made an angle of 45° with the lines of force (which were at right angles to the square faces of the plate); the minor axis was shorter than the diameter of the circle. If the plate was so placed under the microscope that the side becoming positive under pressure was on the right hand, and consequently the side becoming negative under pressure was on the left, and if positive electricity was conducted to the side turned towards the observer, and negative electricity to the side turned from him, then the major axis ran from above on the left, down towards the right.

It is known* that a mechanical compression or dilatation of a quartz plate cut at right angles to the axis, in a direction at right angles to the axis, produces a change in the system of rays which is similar to that just described. The diameters of the circles become lengthened in the direction of pressure and shortened in the direction at right angles to this; if, on the other hand, the plate is dilated, then the major axis of the ellipse is at right angles to the direction of expansion.

The results of the two last experiments would be at once explained, if it were allowable to assume that electrical forces acting in the direction of an axis of no piezoelectricity produce indeed no change of form in the direction of this axis, but compression or dilatation, or both at the same time, in directions at right angles to the principal axis, and inclined at an angle of 45° to the corresponding axis of no piezoelectricity. How far this assumption is justified could not be decided until the piezoelectric experiments which have been described had been made; and on that account I have delayed the publication of my electro-optical experiments with plates cut at right angles to the axis till now.

The confirmation of the accuracy of this assumption follows from the piezoelectric experiments described under (5), and from the law of reciprocity of compression and electric charge enunciated by Lippmann, and with it the complete explanation of the above experiments. He found, in the place referred to, that a pressure exerted upon the quartz at an angle of 45° to an axis of no piezoelectricity and at right angles to the principal axis produces piezoelectricity at the ends of that axis, which is also greater in quantity than that produced at other points. If, then, we communicate to the ends of the axis electricity of the same kind as would be produced by pressure in the given direction, a dilatation of the quartz in this direction must take place; and, conversely, if electricities are communicated of the opposite kinds to those resulting from pressure, compression in this direction must take place. There are, now, two such directions (which are at right angles to the principal axis and inclined at 45° to an axis of no piezoelectricity) for each of the axes of no piezoelectricity, viz. one on each side of the axis. From what was said under (5), it further follows that the electricities which are produced when a pressure is exerted in one of these two directions are opposite to the electricities produced by a pressure in the other

* Pfaff, Pogg. *Ann.* cvii. p. 133 (1859).

direction. If, then, positive electricity be communicated to one end of an axis of no piezoelectricity and negative electricity to the other, this electrification must produce a compression in the one direction and simultaneously an expansion in the other. If the electricities communicated are exchanged, then there will be also an exchange of compression for dilatation. But what has just been said is nothing else than was assumed above in explanation of the electro-optical experiments.

The parallelepiped of quartz designated II. in the first paper was now examined in parallel as well as in convergent sodium-light. At first it was placed in the flask filled with benzol just as before, except that now the rays of light traversed it parallel to the principal axis. If, then, the analyzer was adjusted so as to give a dark field, then upon electrification the place between the perforations became very bright; the distribution of the bright parts changed upon reversal. Both these results were to be expected, since the perforations were made in the direction of an axis of maximum piezoelectricity. In order to investigate the nature of the double refraction produced, the method previously used of an interposed glass plate which was compressed in a vertical or horizontal direction could not be employed, since, in consequence of the rotation of the plane of polarization in the direction of the principal axis, the conditions were more complicated than before; and therefore I made the experiments in convergent light.

For this purpose glass tubes bent upwards at right angles were cemented to the end surfaces of the parallelepiped, and these, together with the communicating perforations, were filled with mercury. The change in the system of rings lying in the middle between the perforations produced by electrification consisted again in the transformation of circles into ellipses having major axes longer and minor axes shorter than the diameters of the corresponding circles. The major axes lay in the direction of a line joining the perforations, and therefore parallel to the direction of the corresponding axis of maximum piezoelectricity, if the marked end of the crystal was positively, and the unmarked end negatively electrified. This axis, on the other hand, was at right angles to that direction, if the marked end were negatively electrified and the unmarked end positively. If we refer to the piezoelectric behaviour of the crystal used, described in the first paper, we easily find, by the aid of the piezoelectric experiments given under 3 and 4, that the optical phenomena observed may be completely explained by the simultaneous action of an electrical contraction in one direction, and an expansion in the direction at right angles to it.

I come now to the experiments with the small quartz cylinder. The axis of the cylinder is parallel to the principal axis. The cylinder is 0·5 centim. in height and 0·45 centim. in diameter, and has an exactly centrical perforation in the direction of the axis 0·08 centim. wide; the end surfaces are polished. I examined first the piezoelectric behaviour of the cylinder, and marked the direction of the three axes of no piezoelectricity. Then the cylinder was cemented by its end faces with Canada balsam to two glass plates, each 4·5 centim. long and 1·5 centim. wide, each of which was provided with a perforation also 0·08 centim. wide. The perforation in the one plate coincided exactly with the perforation in the cylinder; the perforation in the other was at a distance of 2 centim. from that in the cylinder. The edges of the glass plates, which were parallel two and two, were then cemented together with strips of glass so as to form a glass box, which was filled with mercury through the hole in the one plate. This mercury, completely surrounding the curved surface of the cylinder, formed the outer coating of the cylinder, and during the experiments was connected by means of a wire with the one electrode of the Holtz machine. The inner coating was formed by a thin wire passing through the hole in the other glass plate into the perforation in the cylinder, and connected with the other electrode, which was put to earth.

The apparatus described was placed under the Steeg's polarizing microscope, so that the first circle of the system of rings was concentric with the perforation in the cylinder, and could be distinctly observed with sodium-light; it is necessary that the upper system of lenses of the apparatus should be adjusted at a certain distance, easily found, from the cylinder.

The electrification of the two coatings of the quartz cylinder gave rise to the following phenomena. Only the six portions of the circle which lay in the direction of the three axes of no piezoelectricity passing through the centre retained their position; at all other points there was a displacement either towards the centre or away from it: this was greatest in the directions of the three axes of maximum piezo-electricity passing through the centre. In each of these directions there was upon one side a displacement inwards, and on the opposite side a displacement outwards, corresponding in each case to the piezoelectric and to the previously observed electro-optic behaviour of quartz in these three directions. These displacements give to the ring a shape resembling an equilateral triangle with rounded corners. Exchange of electricities changes the position of the triangle, so that the new position would result by turning the triangle round from its former position through an angle of 180° in its own plane. The arrangement of the experiment described is further worth notice, since it shows at one glance the changes which take place in the six directions to which special attention has been directed. The result obtained moreover gave occasion for further electrical experiments with quartz, the results of which I hope to describe in a further communication*.

It is, I think, unnecessary to detail further electro-optical experiments with quartz, since all the phenomena I have hitherto observed may be deduced from the piezoelectric behaviour. How far the hypotheses which form the starting-point of my investigation will hold good in all cases yet remains to be tested by calculation.

XVI. *Mica Films and Prisms for Polarizing-Purposes.* *By* Lewis Wright.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE letter of Mr. H. G. Madan leads me to hope that some interest may be taken in the construction of mica designs and combinations; and I therefore give a few hints from an experience which has been rather tiresome. I also have used the photographic films spoken of; but, except for a few special quarter-wave plates, these would soon be expensive, and the carte-de-visite size are not large enough for work which requires many squares of equal thickness. Moreover it is very tedious to find and mark the "axis" on each separate film; I much prefer to select a fairly large slab, of the best quality obtainable, which will provide a quantity at a tithe of the cost of the films, and quite as clear. This should first be split into moderately thick sheets, say two to four waves in thickness, from which the films can be split afterwards; they do not split so evenly direct from the thick slab. The process described for splitting would, by its tedium, deter many from doing much with mica films; and it is not necessary. The edge being sprung with the point of a sharp needle, the sheet is laid on a perfectly flat surface, and the film separated by a thin and smooth paper-cutter of ivory or tortoise-shell. This is pushed and "coaxed" in gently, when, in most cases, the film will come evenly off. The main secret is to have a flat surface to work upon, and to keep the

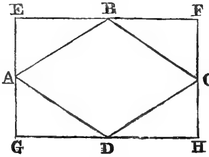
* I have commissioned Messrs. Steeg and Reuter, in Homburg vor der Höhe, to prepare quartz suitable for electro-optical experiments, which, if desired, can be examined under my direction before delivery.

paper-cutter flat down to it. In this simpler way I have split many $\frac{1}{8}$-wave sheets, measuring, say, 13 inches by 5 or 7 inches wide, and plenty of smaller sheets, which by reflection only show nine or ten Talbot lines throughout the whole spectrum. Coloured films could probably only be obtained by Mr. Madan's method. Even the smooth paper-cutter will cause signs of bruising or scratching; but this is of no consequence, as every mark totally disappears when cemented with balsam. At the same time I should much like to know how the trade splitters manage to get their sheets apart without this. The best mica I have found was Indian. Mica must sometimes occur perfectly colourless, as Mr. Fox gave me a small sample; but where more can be got no one seems able to say. Such mica would be a great boon in mica-selenite or crossed-mica combinations; but what the trade call "white" mica is simply ordinary mica which, owing to countless air-bubbles, shows pearly iridescence. When the original slab is split into sheets the mica "axis" can be most carefully ascertained and marked on one of these, and then transferred without further trouble to all the others.

Since my paper was published I have devised designs showing, even more beautifully than the "optical chromotrope," the rotational colours of circularly-polarized films. In their startling kaleidoscopic changes on the screen they almost baffle description, and would certainly puzzle any one not thoroughly acquainted with the subject. To such it will be sufficient to say that they consist practically of *two* or more superposed circularly-polarized designs, planned to go thus together, in appropriate positions. As design, polarizer, or analyzer is rotated, the variety of effect is almost endless, and beyond any thing I have seen yet in spectacular demonstration.

One word further as to prisms. In the note to his able paper, Mr. Glazebrook apparently refers to a few remarks I made at the Meeting of the Physical Society where it was read. In those remarks I referred expressly to the paper by Hartnack and Prazmowski, of which a translation had been sent me by the Rev. P. R. Sleeman; and I am glad to see that this fine paper has *at last* been published in English in the current number of the Journal of the Royal Microscopical Society. It seems strange that Hartnack should have chosen the plane of section, and not direction of the ray, at right angles to the optic axis, as the best theoretic position, since, without Mr. Glazebrook's elegant demonstration, it appears at once that the latter position must give even the greatest difference in refractive indices—the main object Hartnack had in view. Some years ago I asked Messrs. Darker to cut me a prism with its axis at right angles to the optic axis, when they informed me that they believed their late father, so well known for polariscope apparatus, had cut some such prism to order many years before. They had an impression it was for Professor Stokes; but they had no clear recollection about it. If it did fail, as they implied, it was probably for want of a cementing medium which could take advantage of the full interval between the indices, as explained by Hartnack in his admirable essay. I trust that Professor Thompson, in his forthcoming paper, will collect for English readers what has been done abroad, and describe in an accessible form the prisms of Senarmont, Rochon, Glan, and others.

Meantime I wish chiefly to point out that, if the full advantage of these improved prisms is to be had, undue length must be avoided. Most Nicol prisms are longer than they need be; and Hartnack by no means does justice to his own prism in this respect, giving its length as over three times the width. The Rev. P. R. Sleeman possesses a prism only 2·75 times its width, cut as below, which performs admirably in projection, transmitting a much larger pencil and covering more field. Again, a polarizer can and ought to be made shorter than an analyzer, since it only has to transmit rays approximately parallel; hence the reason we can use a Foucault as polarizer but not as analyzer, owing to its small angle of 8°. But a more important point still in getting field, especially for microscope polarizers, is this:—A Nicol section is made from the *corner* B of the spar to the opposite corner, the end elevation being the rhombus A B C D. For this there are practical reasons, the cut being made through a *natural* piece of the spar. But in the Hartnack, Glazebrook, or Thompson prisms, where all the sides and cuts are artificial, this reason does not exist. Hence Prazmowski, in cutting his prisms from corner to corner like a Nicol, threw away a great deal of field. This is not theory only; it has been seen by Steeg and Reuter, who make their Hartnack prism of the very same length and angle, with the end elevation E F G H, the section being from the *edge* E F to the opposite one. A glance will show that a pencil of double the sectional area will thus be passed through a prism of the same section-angle and length. Apologizing for drawing attention to these practical considerations,



I am &c.,

Wellfield, Ashley Road,
Crouch Hill, N.

LEWIS WRIGHT.

XVII. *On a General Theorem of the Stability of the Motion of a Viscous Fluid.* By D. J. Korteweg, *Professor of Mathematics at the University of Amsterdam*.*

The interesting experimental investigation of Prof. O. Reynolds† on sinuous fluid-motion and the origin of eddies induces me to communicate a very general theorem on the stability of fluid-motion, which I published some months ago in the Transactions of the Royal Academy of Amsterdam. Conceiving that the origin of eddies was to be explained by the existence of unstable solutions of the equations of motion, I endeavoured to find such solutions by means of the well-known equations for slow motion of viscous fluid, till I found out that *in any simply connected region, when the velocities along the boundary are given, there exists, as far as the squares and products of the velocities may be neglected, only* one *solution of the equations for the steady motion of an incompressible viscous fluid, and that this solution is always stable.*

The first part of this theorem is due to Helmholtz‡. He shows it to be a very simple consequence of another theorem, stating that "*if the motion be steady, the currents in a viscous fluid are so distributed that the loss of energy due to viscosity is a minimum, on the supposition that the velocities along the boundaries of the fluid are given.*"

Though my demonstration of this theorem is somewhat less general than that of Helmholtz, I will write it down here, as it leads to a very simple and symmetrical expression for the difference between the real dissipation of energy by internal friction with any motion and the minimum dissipation consistent with the same velocities at the boundary, which expression may be useful in other cases.

Theorem I.—*Let* M_0 *represent a mode of motion of an incompressible fluid answering to the equations*

$$\left.\begin{aligned}\mu\nabla^2 u_0 &= \frac{\delta(V\rho+p_0)}{\delta x},\\ \mu\nabla^2 v_0 &= \frac{\delta(V\rho+p_0)}{\delta y},\\ \mu\nabla^2 w_0 &= \frac{\delta(V\rho+p_0)}{\delta z}.\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (1)$$

M *another mode of motion of incompressible fluid, consistent*

* Communicated by the Author.

† Proceedings of the Royal Society, vol. xxxv. 1883, p. 84.

‡ *Verh. des naturh.-med. Vereins zu Heidelberg*, Bd. v. S. 1–7; Collected Works, i. p. 223.

with the same velocities along the boundary, then

$$A=A_0+4\mu\int(\Omega')^2.\,dx\,dy\,dz; \quad . \quad . \quad . \quad . \quad (2)$$

where A_0 *is the dissipation of energy in unit of time at the mode of motion* M_0, A *at the mode of motion* M, Ω' *the angular velocity corresponding at any point of the region to the mode of motion indicated by* $M-M_0$.

Proof. "Let u', v', w' represent the component velocities, A' the dissipation of energy by friction at the mode of motion $M-M_0$, then at every point of the region occupied by the fluid we have by definition,

$$\left.\begin{aligned} u' &= u - u_0, \\ v' &= v - v_0, \\ w' &= w - w_0. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

"Along the boundary,

$$u'=0, \quad v'=0, \quad w'=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

"Substituting the values of u, v, w from (3) in the well-known expression for the dissipation-function,

$$\left.\begin{aligned} A=\mu\int\Big[2\left(\frac{\delta u}{\delta x}\right)^2+2\left(\frac{\delta v}{\delta y}\right)^2+2\left(\frac{\delta w}{dz}\right)^2+\left(\frac{\delta w}{\delta y}+\frac{\delta v}{\delta z}\right)^2 & \\ +\left(\frac{\delta u}{\delta z}+\frac{\delta w}{\delta x}\right)^2+\left(\frac{\delta v}{\delta x}+\frac{\delta u}{\delta y}\right)^2\Big]\,dx\,dy\,dz & \\ =2\mu\int\Big[\left\{\left(\frac{\delta u}{dx}\right)^2+\left(\frac{\delta u}{\delta y}\right)^2+\left(\frac{\delta u}{\delta z}\right)^2\right\}+\left\{\left(\frac{\delta v}{\delta x}\right)^2+\left(\frac{\delta v}{\delta y}\right)^2+\left(\frac{\delta v}{\delta z}\right)^2\right\} & \\ +\left\{\left(\frac{\delta w}{\delta x}\right)^2+\left(\frac{\delta w}{\delta y}\right)^2+\left(\frac{\delta w}{\delta z}\right)^2\right\}\Big]\,dx\,dy\,dz & \\ -\mu\int\Big[\left(\frac{\delta w}{\delta y}-\frac{\delta v}{\delta z}\right)^2+\left(\frac{\delta u}{\delta z}-\frac{\delta w}{\delta x}\right)^2+\left(\frac{\delta v}{\delta x}-\frac{\delta u}{\delta y}\right)^2\Big]\,dx\,dy\,dz, & \end{aligned}\right\} \quad (5)$$

we find

$$\left.\begin{aligned} A=A_0+A'+4\mu\int\Big[\left(\frac{\delta u_0}{\delta x}\cdot\frac{\delta u'}{\delta x}+\frac{\delta u_0}{\delta y}\cdot\frac{\delta u'}{\delta y}+\frac{\delta u_0}{\delta z}\cdot\frac{\delta u'}{\delta z}\right) & \\ +\left(\frac{\delta v_0}{\delta x}\cdot\frac{\delta v'}{\delta x}+\frac{\delta v_0}{\delta y}\cdot\frac{\delta v'}{\delta y}+\frac{\delta v_0}{\delta z}\cdot\frac{\delta v'}{\delta z}\right) & \\ +\left(\frac{\delta w_0}{\delta x}\cdot\frac{dw'}{\delta x}+\frac{\delta w_0}{\delta y}\cdot\frac{\delta w'}{\delta y}+\frac{\delta w_0}{\delta z}\cdot\frac{\delta w'}{\delta z}\right)\Big]\,dx\,dy\,dz & \\ -2\mu\int\Big[\left(\frac{\delta w_0}{\delta y}-\frac{\delta v_0}{\delta z}\right)\left(\frac{\delta w'}{\delta y}-\frac{\delta v'}{\delta z}\right)+\left(\frac{\delta u_0}{\delta z}-\frac{\delta w_0}{\delta x}\right)\left(\frac{\delta u'}{\delta z}-\frac{\delta u'}{\delta x}\right) & \\ +\left(\frac{\delta v_0}{\delta x}-\frac{\delta u_0}{\delta y}\right)\left(\frac{\delta v'}{\delta x}-\frac{\delta u'}{\delta y}\right)\Big]\,dx\,dy\,dz. & \end{aligned}\right\} \quad (6)$$

"To the third term of the right-hand side of this equation we apply the theorem of Green. With respect to the boundary conditions (4), it then takes the form

$$-4\mu\int(u'\nabla^2 u_0+v'\nabla^2 v_0+w'\nabla^2 w_0)\,dx\,dy\,dz.$$

"In virtue of (1), this may be written

$$-4\int\left(u'\frac{\delta(\text{V}\rho+p_0)}{\delta x}+v'\frac{\delta(\text{V}\rho+p_0)}{\delta y}+w'\frac{\delta(\text{V}\rho+p_0)}{\delta z}\right)dx\,dy\,dz,$$

or, by partial integration,

$$4\int(\text{V}\rho+p_0)\left(\frac{\delta u'}{\delta x}+\frac{\delta v'}{\delta y}+\frac{\delta w'}{\delta z}\right)dx\,dy\,dz,$$

which expression is identically zero, having regard to the equation of continuity.

"In the last term of the equation (6) the multiplications must be effected. By partial integration we then can give it the form

$$\begin{aligned}-2\mu\int\Big[&u'\left(\frac{\delta^2 w_0}{\delta x\,\delta z}+\frac{\delta^2 v_0}{\delta x\,\delta y}-\frac{\delta^2 u_0}{\delta z^2}-\frac{\delta^2 u_0}{\delta y^2}\right)\\&+v'\left(\frac{\delta^2 u_0}{\delta y\,\delta x}+\frac{\delta^2 w_0}{\delta y\,\delta x}-\frac{\delta^2 v_0}{\delta x^2}-\frac{\delta^2 v_0}{\delta z^2}\right)\\&+w'\left(\frac{\delta^2 v_0}{\delta z\,\delta y}+\frac{\delta^2 u_0}{\delta z\,\delta x}-\frac{\delta^2 w_0}{\delta y^2}-\frac{\delta^2 w_0}{\delta x^2}\right)\Big]\,dx\,dy\,dz.\end{aligned}$$

"By means of the equation of continuity this may be reduced to

$$2\mu\int(u'\nabla^2 u_0+v'\nabla^2 v_0+w'\nabla^2 w_0)\,dx\,dy\,dz,$$

which expression has already been seen to vanish. Therefore

$$\text{A}=\text{A}_0+\text{A}'. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

"Now, if we effect quite similar transformations with the terms of the expression for A′,

$$\begin{aligned}\text{A}'=2\mu\int\Big[&\left\{\left(\frac{\delta u'}{\delta x}\right)^2+\left(\frac{\delta u'}{\delta y}\right)^2+\left(\frac{\delta u'}{\delta z}\right)^2\right\}\\&+\left\{\left(\frac{\delta v'}{dx}\right)^2+\left(\frac{\delta v'}{\delta y}\right)^2+\left(\frac{\delta v'}{\delta z}\right)^2\right\}\\&+\left\{\left(\frac{\delta w'}{\delta x}\right)^2+\left(\frac{\delta w'}{\delta y}\right)^2+\left(\frac{\delta w'}{\delta z}\right)^2\right\}\Big]\,dx\,dy\,dz\\-\mu\int\Big[&\left(\frac{\delta w'}{\delta y}-\frac{\delta v'}{\delta z}\right)^2+\left(\frac{\delta u'}{\delta z}-\frac{\delta w'}{\delta x}\right)^2+\left(\frac{\delta v'}{\delta x}-\frac{\delta u'}{\delta y}\right)^2\Big]\,dv\,dy\,dz,\end{aligned}$$

the first may be reduced to

$$-2\mu\int(u'\nabla^2u'+v'\nabla^2v'+w'\nabla^2w')\,dx\,dy\,dz,$$

the second to

$$+\ \mu\int(u'\nabla^2u'+v'\nabla^2v'+w'\nabla^2w')\,dx\,dy\,dz;$$

but then it is obvious that A′ may be put at choice under one of two forms:—

$$A'=-\mu\int(u'\nabla^2u'+v'\nabla v'+w'\nabla^2w')\,dx\,dy\,dz, \quad . \quad (8)$$

or

$$A'=\mu\int\left[\left(\frac{\delta w'}{\delta y}-\frac{\delta v'}{\delta z}\right)^2+\left(\frac{\delta u'}{\delta z}-\frac{\delta w'}{\delta x}\right)\right.$$

$$\left.+\left(\frac{\delta v'}{\delta x}-\frac{\delta u'}{\delta y}\right)^2\right]dx\,dy\,dz=4\mu\int(\Omega')^2\,dx\,dy\,dz.\text{"} \quad (9)$$

Theorem II.—*The mode of motion* M_0 *is such that the dissipation of energy by internal friction is the least possible consistent with the same velocities along the boundary. It is unique in every simply connected region when these velocities are given.*

Proof. "The first part of this theorem follows immediately from the equation (2), the second term of the right-hand side of this equation being necessarily positive or zero.

"Now let M'_0 be a second mode of motion, consistent with the given velocities along the boundary and satisfying the equations (1). Then, as in both motions the dissipation of energy must be the least possible, A'_0 is equal to A_0. This can be so only when, at every point,

$$\Omega'=0;$$

but then $M'_0 - M_0$ should represent an irrotational motion with zero-velocities all over a closed boundary; and such a motion is known to be impossible."

Theorem III.—*When in a given region occupied by viscous incompressible fluid, there exists at a certain moment a mode of motion* M, *which does not satisfy the equations* (1), *then, the velocities along the boundary being maintained constant, the change which must occur in the mode of motion will be such (neglecting squares and products of velocities) that the dissipation of energy by external friction is constantly decreasing till it reaches the value* A_0 *and the mode of motion becomes identical with* M_0.

Proof. "The change occurring at any moment in the mode of motion is determined by the equations

$$\left.\begin{aligned}\rho\frac{\delta u}{\delta t}-\mu\nabla^2 u+\frac{\delta(\mathrm{V}\rho+p)}{\delta x}=0,\\ \rho\frac{\delta v}{\delta t}-\mu\nabla^2 v+\frac{\delta(\mathrm{V}\rho+p)}{\delta y}=0,\\ \rho\frac{\delta w}{\delta t}-\mu\nabla^2 w+\frac{\delta(\mathrm{V}\rho+p)}{\delta z}=0.\end{aligned}\right\} \quad . \quad . \quad . \quad (10)$$

"Along the boundary,

$$\frac{\delta u}{\delta t}=\frac{\delta v}{\delta t}=\frac{\delta w}{\delta t}=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

" By means of these relations we have to prove that $\frac{\delta \mathrm{A}}{\delta t}$ is constantly *negative*.

" Now, in virtue of (5),

$$\left.\begin{aligned}\frac{\delta \mathrm{A}}{\delta t}=4\mu\int\Big[&\left(\frac{\delta u}{\delta x}\cdot\frac{\delta^2 u}{\delta x\,\delta t}+\frac{\delta u}{\delta y}\cdot\frac{\delta^2 u}{\delta y\,\delta t}+\frac{\delta u}{\delta z}\cdot\frac{\delta^2 u}{\delta z\,\delta t}\right)\\ &+\left(\frac{\delta v}{\delta x}\cdot\frac{\delta^2 v}{\delta x\,\delta t}+\frac{\delta v}{\delta y}\cdot\frac{\delta^2 v}{\delta y\,\delta t}+\frac{\delta v}{\delta z}\cdot\frac{\delta^2 v}{\delta z\,\delta t}\right)\\ &+\left(\frac{\delta w}{\delta x}\cdot\frac{\delta^2 w}{\delta x\,\delta t}+\frac{\delta w}{\delta y}\cdot\frac{\delta^2 w}{\delta y\,\delta t}+\frac{\delta w}{\delta z}\cdot\frac{\delta^2 w}{\delta z\,\delta t}\right)\Big]\,dx\,dy\,dz\\ -2\mu\int\Big[&\left(\frac{\delta w}{\delta y}-\frac{\delta v}{\delta z}\right)\left(\frac{\delta^2 w}{\delta y\,\delta t}-\frac{\delta^2 v}{\delta z\,\delta t}\right)+\left(\frac{\delta u}{\delta z}-\frac{\delta w}{\delta x}\right)\left(\frac{\delta^2 u}{\delta z\,\delta t}-\frac{\delta^2 w}{\delta x\,\delta t}\right)\\ &+\left(\frac{\delta v}{\delta x}-\frac{\delta u}{\delta y}\right)\left(\frac{\delta^2 v}{\delta x\,\delta t}-\frac{\delta^2 u}{\delta y\,\delta t}\right)\Big]\,dx\,dy\,dz.\end{aligned}\right\} \quad (12)$$

" Applying the theorem of Green to the first term of the right-hand side of this equation, we get

$$-4\mu\int\left(\frac{\delta u}{\delta t}\cdot\nabla^2 u+\frac{\delta v}{\delta t}\nabla^2 v+\frac{\delta w}{\delta t}\nabla^2 w\right)dx\,dy\,dz.$$

" With respect to (10), this may be written

$$-4\rho\int\left[\left(\frac{\delta u}{\delta t}\right)^2+\left(\frac{\delta v}{\delta t}\right)^2+\left(\frac{\delta w}{\delta t}\right)^2\right]dx\,dy\,dz$$

$$-4\int\left(\frac{\delta u}{\delta t}\cdot\frac{\delta(\mathrm{V}\rho+p)}{\delta x}+\frac{\delta v}{\delta t}\cdot\frac{\delta(\mathrm{V}\rho+p)}{\delta y}+\frac{\delta w}{\delta t}\cdot\frac{\delta(\mathrm{V}\rho+p)}{\delta z}\right)dx\,dy\,dz,$$

the second term of which expression vanishes after partial integration by virtue of the equation of continuity.

" As for the second term of the right-hand side of (12), effecting the multiplications and applying partial integration, it takes the form

$$-2\mu\int\left(\frac{\delta u}{\delta t}\left(\frac{\delta^2 w}{\delta x\,\delta z}+\frac{\delta^2 v}{\delta x\,\delta y}-\frac{\delta^2 u}{\delta z^2}-\frac{\delta^2 u}{\delta y^2}\right)\right.$$
$$+\frac{\delta v}{\delta t}\left(\frac{\delta^2 u}{\delta y\,\delta x}+\frac{\delta^2 w}{\delta y\,\delta z}-\frac{\delta^2 v}{\delta x^2}-\frac{\delta^2 v}{\delta z^2}\right)$$
$$\left.+\frac{\delta w}{\delta t}\left(\frac{\delta^2 v}{\delta z\,\delta y}+\frac{\delta^2 u}{\delta z\,\delta x}-\frac{\delta^2 w}{\delta y^2}-\frac{\delta^2 w}{\delta x^2}\right)\right)dx\,dy\,dz$$
$$=2\mu\int\left(\frac{\delta u}{\delta t}\,\nabla^2 u+\frac{\delta v}{\delta t}\,\nabla^2 v+\frac{\delta^2 w}{\delta t}\,\nabla^2 w\right)dx\,dy\,dz$$
$$=2\rho\int\left(\frac{\delta u}{\delta t}\right)^2+\left(\frac{\delta v}{\delta t}\right)^2+\left(\frac{\delta w}{\delta t}\right)^2\Big]\,dx\,dy\,dz.$$

"Combining the values of both terms, we have

$$\frac{\delta \mathrm{A}}{\delta t}=-2\rho\int\left[\left(\frac{\delta u}{\delta t}\right)^2+\left(\frac{\delta v}{\delta t}\right)^2+\left(\frac{dw}{dt}\right)^2\right]dx\,dy\,dz. \quad . \quad . \quad (13)$$

"This expression remains negative, and therefore the dissipation of energy is decreasing, till everywhere in the fluid

$$\frac{\delta u}{\delta t}=\frac{\delta v}{\delta t}=\frac{\delta w}{\delta t}=0;$$

but then the motion has become steady, and is necessarily identical with the motion represented by M_0."

THEOREM IV.—*The mode of motion represented by* M_0 *is always stable as far as squares and products of velocities may be neglected.*

Proof. "Let the mode of motion M_0 be disturbed by any cause whatever. Then the dissipation of energy by internal friction is necessarily increased (Theorem II.); but as soon as the cause of disturbance ceases it must decrease again (Theorem III.) till it reaches the value A_0, and then the mode of motion M_0 is restored."

From this theorem I draw the following conclusions:—

1st. That the existence of unstable modes of fluid-motion and the origin of eddies cannot be explained without taking into account squares and products of velocities; for that the equations (1) for steady motion with low velocities cannot lead directly to eddying fluid-motion, whatever the velocities along the boundary be, is a consequence of the well-known relations

$$\nabla^2\xi=0,\quad \nabla^2\eta=0,\quad \nabla^2\zeta=0.$$

According to these relations, ξ, η, and ζ can have no maximum or minimum values in the interior of the fluid; but then the angular velocity Ω cannot have there a maximum value, which, taking the axis of x parallel to the direction of the rotation-axis, would correspond obviously with a maximum value of ξ.

2nd. That though the idea of the possible existence of unstable solutions of the equations of motion (alluded to, as far as I know, for the first time by Prof. Stokes*) was very just and fertile in itself, yet the case of motion which suggested it to him is not one of unstable motion, at least not so unless the squares and products of velocities be taken into account. It is perfectly true that, when a cylinder of infinite length moves with uniform velocity through an incompressible viscous fluid, the state of steady motion never can be reached, and an ever increasing quantity of fluid will be carried on by the cylinder. Yet as the dissipation of energy will be ever decreasing, and even, as may be proved, tending to zero, as the motion proceeds, such a change in the state of motion as Prof. Stokes alludes to, and by which the dissipation of energy could only be augmented, cannot occur.

When, on the contrary, the squares and higher powers of the velocities are taken into account, I have my reasons for supposing that, even in the case of a sphere moving with uniform velocity, no state of steady motion can be reached, and the motion must finally become unstable.

Amsterdam, June 4, 1883.

XVIII. *On the Critical Point of Liquefiable Gases.* *By* William Ramsay.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the July number of this Magazine there is a translation of a memoir by M. J. Jamin, presented by him to the French Academy. As he has taken no notice of the views expressed by me in the 'Proceedings of the Royal Society' for 1880, April 22nd and December 16th, I think it right to point out that the substance of this memoir has been anticipated; and in support of this statement let me quote the following passages. M. Jamin states (1) that the critical point of a liquid is not to be regarded, as hitherto, as the temperature at

* Trans. of the Camb. Phil. Soc. vol. ix. 1849, "On the Effect of the Internal Friction of Fluids on the Motion of Pendulums," p. 56.

which a liquid changes completely into gas, independently of the extent to which the pressure to which it is exposed is increased, but as the point at which the specific gravity of a liquid becomes equal, through rise of temperature, to that of the gas above and in contact with it. At the top of p. 326 of the 'Proceedings of the Royal Society' I remark:—"The critical point [of a liquid] is that point at which the liquid, owing to expansion, and the gas, owing to compression, acquire the same specific gravity, and consequently mix with one another. From the first experiment it is seen that on cooling the liquid contracts more rapidly than the gas, and consequently separates as a mist through the whole of the tube, and, from its gravity, separates at the lower half." M. Jamin again states that if a gas be compressed in a closed space, the tension of the gas reaches a maximum, and the gas then liquefies at its boiling-point under the pressure at which liquefaction takes place. Again let me quote:—"If the deductions from the above experiments be correct, it follows that that form of matter which we call gas may be converted into liquid by pressure alone; but the meniscus will never become visible, for the process of change is a gradual one. To render the meniscus visible it is necessary to take advantage of the fact that liquids under such circumstances have a much greater coefficient of expansion by heat, and, conversely, a much greater contraction on withdrawal of heat, than gases. It therefore becomes necessary to lower the temperature until the liquid by contraction acquires a specific gravity greater than that of its gas; and then, and not till then, does the phenomenon of a meniscus become observable." Again (Dec. 16th, 1880), "When a mixture of liquid and gas is maintained at a certain volume, the expansion of the liquid, in raising the temperature, *so long as it is possible to distinguish liquid from gas*, points to the ultimate occupying of the space by liquid at temperatures above which the meniscus becomes invisible;" and again, "Under such circumstances the liquid retains its solvent powers, while the gas is incapable of dissolving a solid." For the experimental data on which these assertions are based, reference must be made to the original paper. M. Jamin next quotes experiments of Cailletet, in which the latter shows that by decreasing the volume of carbonic anhydride in the liquid state in contact with gaseous matter, oxygen or hydrogen, the meniscus gradually disappears when the pressure is increased sufficiently to cause the mixture of unliquefied carbonic anhydride and "permanent" gas to have a density greater than that of the liquid anhydride. Again M. Jamin has been anticipated. "But I venture to think that the possession of surface-tension is not a criterion of the existence of a liquid. And a most striking argument in support of this theory has lately been furnished by M. Cailletet (*Compt. Rend.* xc. 210). He found that carbonic anhydride at a temperature of $5^{\circ}{\cdot}5$, when the lower portion of his experimental tube was filled with liquid, the upper portion being filled with a mixture of gaseous carbonic anhydride with air, mixed with the air when a pressure of 130 atmospheres was applied. The question is a simple one, Does the gas become liquid, or the liquid become gas? Or do they both enter a state to be called neither liquid nor gas?" For the theory brought forward I must refer to the original memoirs in the Royal Society's Proceedings; but if I have not already trespassed on your space, let me finish with the last paragraph of my second memoir.

"The views expressed in this paper are :—(1) That a gas may be defined as a body whose molecules are composed of a small number of atoms; (2) a liquid may be regarded as formed of aggregates of gaseous molecules forming a more complex molecule; and (3) that above the critical point the matter may consist wholly of gas, if a sufficient volume be allowed; wholly of liquid, if that volume be diminished sufficiently; or of a mixture of both at intermediate volumes. . . . . When prevented from mixing by interposing a capillary tube between the two, the liquid and gas retain their several properties."

I am much interested to find that the further experiments made by M. Cailletet at the instance of M. Jamin appear to substantiate these views; and I cannot refrain from expressing my gratification that they are shared by such a distinguished physicist as M. Jamin.

It may be of interest to add that, in one or two experiments, I have noticed that amyl alcohol near its critical point forms globules which rise comparatively slowly in the tube in which it is confined and then disappear. These globules appear to float, and would present the phenomenon of a liquid lighter than a gas.

I am, Gentlemen,
Your obedient servant,
William Ramsay.

XIX. *The Molecular Volumes of Salt-Solutions.* By W. W. J. NICOL, *M.A., B.Sc., F.R.S.E., &c., Lecturer on Chemistry, Mason College, Birmingham* *.

HITHERTO but little attention has been paid to the molecular volumes of salt-solutions; the phrase, so far as I am aware, occurs only in the papers of Berthelot, Thomsen, and Ostwald. The first of these examined the relation of the heat evolved by the solution of the haloid acids in water to the molecular volume of the resulting solutions †; while Thomsen, in addition to experiments with iodic acid similar to those of Berthelot, has pointed out the close connexion existing between the molecular heat and molecular volume of various salt-solutions ‡. The experiments of Ostwald §, quoted by Thomsen (*loc. cit.*) in support of the theory of the "Avidity of Acids," are misleading, as the solutions he employed contained two equivalents of the salt or acid in every 1000 grms.; as a consequence, they contained different quantities of water, and therefore did not really admit of comparison.

The molecular volume of a solution is a measure of the space occupied by the molecules of salt and water forming the unit of the solution, along with the intermolecular spaces separating them. It is obtained by dividing the molecular weight of the unit of the solution by its specific gravity; or, generally,

$$\text{Mol. vol.} = \frac{x\text{M} + n18}{\delta},$$

where M is the molecular weight of the salt, x and n the numbers respectively of salt and water molecules, and δ the specific gravity of the solution.

I hope in the following pages to be able to show that an exact and extended knowledge of the molecular volumes of salt-solutions will throw much light on the constitution of such solutions, and will probably prove a powerful instrument in investigating the constitution of the salts themselves. With this object in view, I give first the results of the more complete and important of my experiments in this direction, and shall then proceed to state the conclusions I feel warranted in drawing from them. My method of experiment is the same as that described in my former paper ‖; but, inasmuch as

* Communicated by the Author.

† *Comptes Rendus*, lxxvi. p. 679.

‡ *Thermochemische Untersuchungen*, Band i. p. 52, ii. p. 427.

§ Wiedemann's *Annalen*, ii. p. 429.

‖ Phil. Mag. 1883, xv. p. 91.

these data are to be used for mutual comparison, I have in many cases taken the mean of two or more independent determinations, as likely to possess greater accuracy than the results of single experiments.

Table I. contains a complete list of the solutions experimented on, with their specific gravities and molecular volumes. For convenience of reference these are numbered.

TABLE I.

Water at 20°=1. Strength x molec. salt to 100 H_2O.

No.	x.	Salt.	δ.	Molec. vol.	Remarks.
1.	5·0	KCl	1·11445	1949·84	
2.	4·0	„	1·09415	1917·84	
3.	2·0	„	1·04959	1857·12	Mean of 2.
4.	1·0	„	1·02568	1827·67	„ 3.
5.	0·5	„	1·01310	1813·54	„ 2.
6.	5·0	NaCl	1·10276	1897·42	„ 6.
7.	4·0	„	1·08408	1876·23	„ 3.
8.	2·0	„	1·04393	1836·29	„ 3.
9.	1·0	„	1·02255	1817·52	„ 5.
10.	0·5	„	1·01145	1808·54	„ 2.
11.	5·0	KNO_3	1·14888	2006·74	„ 2.
12.	4·0	„	1·12264	1963·53	„ 2.
13.	2·0	„	1·06524	1879·58	
14.	1·0	„	1·03373	1839·07	„ 4.
15.	0·5	„	1·01730	1819·03	
16.	5·0	$NaNO_3$	1·13813	1954·96	
17.	2·0	„	1·05980	1858·85	
18.	1·0	$KClO_3$	1·04122	1846·49	
19.	1·0	$NaClO_3$	1·03844	1835·93	
20.	1·0	K_2SO_4	1·06744	1840·10	Mean of 2.
21.	0·5	„	1·03758	1818·76	„ 2.
22.	1·0	Na_2SO_4	1·06744	1819·31	
23.	0·5	„	1·03466	1808·32	Above diluted.
24.	2·0	KHO	1·05325	1815·52	From K.
25.	2·0	NaHO	1·04712	1795·40	From Na.
26.	0·5	$MgSO_4$ 7 aq.	1·03201	1863·35	
27.	0·5	$FeSO_4$ 7 aq.	1·04050	1863·53	
28.	0·5	$NiSO_4$ 7 aq.	1·04296	1860·43	
29.	0·5	$CoSO_4$ 7 aq.	1·04303	1860·30	
30.	0·5	$CuSO_4$ 5 aq.	1·04268	1863·22	101 H_2O.
31.	0·5	$ZnSO_4$ 7 aq.	1·04367	1862·18	

When a salt is in the solid state, its molecular volume evidently expresses the space occupied by its molecule along with that fraction of the intermolecular space that clearly belongs to that molecule. Only, therefore, on the supposition that the intermolecular spaces, in different salts in the solid state, are coextensive, or nearly so, does the molecular volume give any clue to the constitution of the salt; but I venture to submit that the molecular volumes of salt-solutions do more than this, and that it is possible, by the comparison of similarly constituted solutions, and the effect of heat and concentration on their molecular volumes, to determine, not only the molecular volumes of the salts themselves, but also that of each of their constituents, as has, to some extent, been accomplished in the case of organic liquids.

If we compare the molecular volumes (Table I.) of the salts of the same acid-radical with different metals, and also those of the same metal with different acid-radicals, we obtain the following results:—

A. The Alteration in the Molecular Volume of a Salt-Solution resulting from the replacement of Potassium by Sodium.

(1) *In Combination with Chlorine.*

Table II. contains the data necessary for this comparison. It is evident from it that, when solutions of the same strength are compared, the volume-change as above is 10 to 10·48, or

$$(K-Na)Cl=10·0 \text{ to } 10·48=\Delta.$$

TABLE II.

No. in Table I.	x.	KCl.	NaCl.	Diff.	Δ.
1– 6.	5·0	1949·84	1897·42	52·42	10·48
2– 7.	4·0	1917·84	1876·23	41·61	10·40
3– 8.	2·0	1857·12	1836·29	20·83	10·41
4– 9.	1·0	1827·67	1817·52	10·15	10·15
5–10.	0·5	1813·54	1808·54	5·00	10·00

It is to be noted here that Δ increases with the concentration. I shall return to this later.

(2) *In Combination with* (SO_4).

$$(K-Na)\left(\frac{SO_4}{2}\right)=10·39 \text{ to } 10·44=\Delta.$$

TABLE III.

No. in Table I.	x.	K_2SO_4.	Na_2SO_4.	Diff.	Δ.
20–22.	1·0	1840·10	1819·31	20·79	10·39
21–23.	0·5	1818·77	1808·32	10·44	10·44

The effect of concentration is here the reverse of that in the case of the chlorides.

(3) *In Combination with* NO_3.

$(K-Na)\,NO_3 = 10{\cdot}36 = \Delta$.

(4) *In Combination with* (ClO_3).

$(K-Na)\,ClO_3 = 10{\cdot}56 = \Delta$.

(5) *In Combination with* (OH).

$(K-Na)\,OH = 10{\cdot}06 = \Delta$.

Table IV. contains all the above.

TABLE IV.

No. in Table I.	x.	KNO_3.	$NaNO_3$	Diff.	Δ.
11–16.	5·0	2006·74	1954·96	51·78	10·36
13–17.	2·0	1879·58	1858·85	20·73	10·36
		$KClO_3$	$NaClO_3$		
18–19.	1·0	1846·49	1835·93	10·56	10·56
		KOH.	NaOH.		
24–25.	2·0	1815·52	1795·40	20·12	10·06

The above results may be summed up as follows :—The value of $(K-Na)\,R$, where $R = Cl$, $\left(\frac{SO_4}{2}\right)$. NO_3. ClO_3, or OH, is a number lying between 10·0 and 10·56 ; and it is to some extent dependent on the strengths of the solutions compared.

B. The Alteration in the Molecular Volume of a Salt-Solution resulting from the replacement of one Acid-radical by another.

(α) *The replacement of* NO_3 *by* Cl.

(1) In combination with Potassium.
$K(NO_3-Cl) = 10{\cdot}98$ to $11{\cdot}4 = \Delta$.

(2) In combination with Sodium.
$Na\,(NO_3-Cl) = 11{\cdot}28$ to $11{\cdot}51 = \Delta$.

TABLE V.

No. in Table I.	x.	KNO_3.	KCl.	Diff.	Δ.
11—1.	5·0	2006·74	1949·84	56·90	11·40
12—2.	4·0	1963·53	1917·84	45·69	11·42
13—3.	2·0	1879·58	1857·12	22·46	11·23
14—4.	1·0	1839·07	1827·67	11·40	11·40
15—5.	0·5	1819·03	1813·54	5·49	10·98
		$NaNO_3$.	NaCl.		
16—6.	5·0	1954·96	1897·42	57·54	11·51
17—8.	2·0	1858·85	1836·29	22·56	11·28

(β) *The replacement of* Cl *by* $\left(\frac{SO_4}{2}\right)$.

(1) In combination with Potassium.

$$K\left(Cl-\frac{SO_4}{2}\right)=8{\cdot}55 \text{ to } 8{\cdot}91=\Delta.$$

(2) In combination with Sodium.

$$Na\left(Cl-\frac{SO_4}{2}\right)=8{\cdot}49 \text{ to } 9{\cdot}2=\Delta.$$

TABLE VI.

No. in Table I.	x.	$(KCl)_2$.	K_2SO_4.	Diff.	Δ.
3—20.	1·0	1857·12	1840·10	17·02	8·50
4—21.	0·5	1827·67	1818·76	8·91	8·91
		$(NaCl)_2$.	Na_2SO_4		
8—22.	1·0	1836·25	1819·31	16·94	8·49
9—23.	0·5	1817·52	1808·32	9·20	9·20

Finally :—

(γ) *The replacement of* (ClO_3) *by* Cl.

(1) In combination with Potassium.

$$K(ClO_3-Cl)=18{\cdot}82=\Delta.$$

(2) In combination with Sodium.

$$Na(ClO_3-Cl)=18{\cdot}41=\Delta.$$

Thus we find, in all the above cases, that the substitution of one metal for another in combination with the same acid-radical, or the replacement of one acid-radical by another under the same conditions, is attended by an alteration in the molecular volume of the solution, which, in each case, is nearly a constant quantity. The volumes, therefore, of the above elements and groups of elements are independent of the manner in which they may be combined together, provided only they be determined in solution in water and under the same conditions. Now such is not the case in the solid state. Taking the molecular volumes of the above salts, calculated by Schröder* from the most reliable determinations of the specific gravity of the solid salts, we have the following :—

$$(K-Na)\,Cl = 37{\cdot}4-27{\cdot}1 = 10{\cdot}3,$$
$$(K-Na)\,NO_3 = 48{\cdot}5-37{\cdot}6 = 10{\cdot}9,$$
$$(K-Na)\left(\frac{SO_4}{2}\right) = \left(\frac{32{\cdot}8-26{\cdot}7}{2}\right) = 3{\cdot}1.$$
$$K\left(Cl-\frac{SO_4}{2}\right) = 37{\cdot}4-16{\cdot}4 = 21,$$
$$Na\left(Cl-\frac{SO_4}{2}\right) = 27{\cdot}1-13{\cdot}3 = 13{\cdot}8.$$
$$K\,(NO_3-Cl) = 48{\cdot}5-37{\cdot}4 = 11{\cdot}1,$$
$$Na\,(NO_3-Cl) = 37{\cdot}6-27{\cdot}1 = 10{\cdot}5.$$

All attempts to extend the experiments of Kopp on liquids to the case of solid salts have, I believe, failed, or at the best been attended by only partial success, owing doubtless to the impossibility of obtaining these under such conditions that their molecular interspaces are even approximately comparable. This, however, is not the case with some isomorphous salts examined by Thorpe† and others; but I shall return to this point later on. Meanwhile I believe that I am justified in concluding, from my experiments above, *that, when salts are dissolved in water, the molecular interspaces in various solutions are approximately coextensive.* Hence it is possible to ascertain the relative molecular volumes of the salts themselves with a degrée of accuracy equal, at least, to that with which the molecular volumes of liquids have been determined.

I will now consider the causes which lead to the variations in the difference between the molecular volumes of two salt-solutions, and which thus affect the accuracy of the above determinations. Putting the errors of experiment on one side, we have the main cause in the effect of concentration on the volumes of the solutions compared, or, what is the same thing (as I hope to be able to show), in the different solubilities of the two salts. Among the best-marked instances given above, we find two cases in which Δ increases with the concentration,

* Poggendorff's *Annalen*, cvi. p. 242.
† Journal of Chemical Society, 1880, p. 102.

viz. Tables II. and V., where the values of (K−Na) Cl and $K(NO_3-Cl)$ are least with the most dilute solution; and two cases where the reverse is the case, Table VI., where $K\left(Cl-\frac{SO_4}{2}\right)$ and $Na\left(Cl-\frac{SO_4}{2}\right)$ have the greatest value when the solution is most dilute. If we compare the most concentrated and the most dilute solutions of the same salt, we can ascertain what effect concentration has on the molecular volume of the salt. This is done in Table VII., where the volume of the water is assumed constant =1800.

TABLE VII.

x.	Mol. vol. KCl.	Diff.	Mol. vol. NaCl.	Diff.	Mol. vol. KNO_3.	Diff.	Mol. vol. KCl.	Diff.
0·5	27·08		17·08		38·06		27·08	
5·0	29·97	2·89	19·48	2·40	41·35	3·29	29·97	2·89
	Mol. vol. KCl.		Mol. vol. $\left(\frac{K_2SO_4}{2}\right)$.		Mol. vol. NaCl.		Mol. vol. $\left(\frac{Na_2SO_4}{2}\right)$.	
1·0	27·67		18·76		17·52		8·32	
2·0	28·56	0·89	20·05	1·29	18·14	0·62	9·66	1·34

The numbers in the columns headed Mol. vol. are the molecular volumes of *one* molecule of the salt in a solution which contains x molecules to $100H_2O$; the difference is the increase of the molecular volume of each molecule produced by the increase in concentration shown by the column headed x.

A glance at these differences will show the immediate reason of the increase or the diminution, as the case may be, of the differences between the pairs of salts, when the solution is concentrated. In the first two cases, (K−Na) Cl and $K(NO_3-Cl)$, the salt with the larger molecular volume has it increased by concentration at a rate faster than that of the other; and the converse is true with regard to $K\left(Cl-\frac{SO_4}{2}\right)$ and $Na\left(Cl-\frac{SO_4}{2}\right)$, for here it is the one with the smaller molecular volume on which the effect of concentration is more marked.

To find, however, the real cause of this we must bear in mind that, in the first instances, it is the first member of each pair of salts that is least soluble in water, while in the latter it is the second member of each pair of which this is true. The solubilities in terms of salt-molecules to 100 water-molecules are approximately:—

KCl	8·4,	$NaCl$	10·99,
KNO_3	5·6,	K_2SO_4	1·0+,
Na_2SO_4	2·0+.		

All at 20° C.

We have therefore arrived at the following:—

In the case of salts when compared in pairs, the increase of molecular volume by concentration is greatest with the less-soluble salt. Why is this? When viewed in the light of the theory of solution which I recently stated in a paper* read before the Royal Society of Edinburgh, the reason is evident. In that paper I suggested that the solubility of a salt in water was due to "the attraction of the molecules of water for a molecule of salt exceeding the attraction of the molecules of salt for one another;" and that "as the number of dissolved salt-molecules increases, the attraction of the *dissimilar* molecules is more and more balanced by the attraction of the *similar* molecules;" and that this last increases until the two forces balance, when saturation takes place. I also showed that the rate of increase in the density of a salt-solution is less than the rate at which it becomes more concentrated, but that when a solution is sufficiently dilute no further dilution affects its specific gravity; as a consequence, the molecular volume of the salt dissolved is constant. It is evident from the above that the effect of concentration in increasing the molecular volume of a solution is due to the molecules of salt coming more and more within the sphere of one another's attraction, and being thus brought nearer and nearer to the point at which crystallization takes place. Now what is true of the same salt in solutions of different strengths is equally true of solutions of the same strength of salts whose solubilities are different; and it follows that the less soluble salt will have its molecular volume increased by concentration faster than the more soluble salt. Such, I believe, is the explanation of the variations in the differences above observed; and, as a consequence, if the salt-solution be sufficiently dilute, the differences between their molecular volumes are constant.

The next point to be considered is the influence of rise of temperature on the molecular volumes of salt-solutions. Table VIII. contains the requisite data. These are the results of single experiments, and are therefore not of equal accuracy with the data in Table I., nor, in consequence, do the values at 20° correspond exactly with those given in that table; but they are accurate in general to $\pm$·00005 in the specific gravity.

The column headed Difference contains the increase of molecular volume due to heating from 20° to 40°. This refers to the salt alone; for water $=1$, both at 20° and 40°.

* Phil. Mag. 1883, xv. p. 91.

TABLE VIII.

x.	Salt.	$t°$.	δ.	Mol. vol.	Diff.	D.
5·0	NaCl	20°	1·10292	1897·25		
,,	,,	40	1·10032	1901·73	4·48	0·89
2·5	,,	20	1·05426	1846·09		
,,	,,	40	1·05270	1848·82	2·73	1·09
2·0	,,	20	1·04396	1836·29		
,,	,,	40	1·04266	1838·57	2·28	1·14
1·0	,,	20	1·02259	1817·44		
,,	,,	40	1·02190	1818·68	1·24	1·24
5·0	KCl	20	1·11445	1949·84		
,,	,,	40	1·11278	1952·77	2·93	0·58
2·0	,,	20	1·04961	1857·08		
,,	,,	40	1·04864	1858·80	1·72	0·86
1·0	,,	20	1·02561	1827·79		
,,	,,	40	1·02507	1828·76	0·97	0·97
5·0	$NaNO_3$	20	1·13790	1955·37		
,,	,,	40	1·13359	1962·79	7·42	1·48
2·5	,,	20	1·07366	1874·42		
,,	,,	40	1·07086	1879·34	4·92	1·97
2·0	,,	20	1·05980	1858·85		
,,	,,	40	1·05738	1863·09	4·24	2·12
5·0	KNO_3	20	1·14917	2006·23		
,,	,,	40	1·14556	2012·55	6·32	1·26
2·0	,,	20	1·06524	1879·58		
,,	,,	40	1·06333	1882·95	3·37	1·68
1·0	,,	20	1·03373	1839·07		
,,	,,	40	1·03233	1841·56	2·49	2·49
2·0	Na_2SO_4	20	1·06744	1819·31		
,,	,,	40	1·06594	1821·87	2·56	1·28
1·0	,,	20	1·03466	1808·32		
,,	,,	40	1·03385	1809·74	1·42	1·42
2·0	K_2SO_4	20	1·07286	1840·12		
,,	,,	40	1·07172	1842·08	1·96	0·98
1·0	,,	20	1·03762	1818·69		
,,	,,	40	1·03694	1819·87	1·18	1·18
5·0	NH_4Cl	20	1·03880	1990·27		
,,	,,	40	1·03814	1991·55	1·28	0·25
2·5	,,	20	1·02130	1893·43		
,,	,,	40	1·02093	1894·12	0·69	0·28
2·0	,,	20	1·01741	1874·36		
,,	,,	40	1·01711	1874·93	0·57	0·29
5·0	NH_4NO_3	20	1·07685	2043·00		
,,	,,	40	1·07372	2049·00	6·00	1·20
2·5	,,	20	1·04137	1920·54		
,,	,,	40	1·03957	1923·87	3·33	1·33
2·0	,,	20	1·03389	1895·76		
,,	,,	40	1·03238	1898·53	2·77	1·39

The last column (D) contains the difference divided by x, and thus gives the increase of volume for each molecule of salt present.

A glance at the table will show that, in every case, the increase of the molecular volume by heating from 20° to 40° is inversely proportional to the strength of the solution. In other words, the more dilute a solution is the greater the increase of its molecular volume by heat per molecule of dissolved salt. This is, I believe, inexplicable by the hydrate theory of solution. It appears impossible that a solution which contains only *one* molecule of salt to 100 molecules of water should experience a greater change of molecular volume per salt-molecule than a solution of the same salt containing *two* salt-molecules. This is, however, in accordance with the theory * of expansion of salt-solutions which I put forward at the end of my paper on the Nature of Solution. At that time the only data at my disposal were the results obtained by Kremers and others, whose experiments were made with non-molecular solutions. The above table, however, fully bears out my previous statement that the expansion of a solution is the result of the action of heat on the resultant of the three forces—the attraction of water for water, that of water for salt, and that of salt for salt, the last two being inversely proportional to one another.

In the cases considered in the first part of this paper, it is clear that a variation in the composition of the salt necessitates a corresponding change in the molecular volume of the solution containing that salt. This, however, holds true only of salts non-isomorphous in the strict sense of the term—that is, of heteromorphous and homeomorphous salts. When, however, two salts are strictly isomorphous, that is isotomous, they preserve their isotomy in solution. The last six salts given in Table I. come under this head; they are the members of the so-called magnesian-sulphate group, which crystallize (except $CuSO_4$) with $7H_2O$, and then possess approximately identical molecular volumes †. This is also true to some extent of their solutions, which have nearly the same molecular volumes. What the slight variations may be due to I am not in a position at present to prove; but enough has been said to show that the intermolecular spaces in isomorphous salts are comparable, and that true isomorphism is conditioned by the molecule itself; while with salts which are isomorphous in the wider sense (homeomorphous), such as the chlorides, bromides,

* Phil. Mag. 1883, xv. p. 99.

† Thorpe, Journ. of Chem. Soc. 1880, p. 102.

&c. of sodium and potassium, the apparent isomorphism is the result, not of the isomorphism of the molecules themselves, but of the mutual relations of the molecules and intermolecular spaces being such as to produce the same crystalline form.

In conclusion, I may be allowed to point out that this method of investigating the molecular volumes of salts is, in all probability, capable of extension to organic substances; and that, by comparing solutions of various organic bodies which differ by one or more CH_2-groups, or in other respects, it may be possible to determine the volume of these differences. Such solutions need not necessarily be aqueous. At present, I have made only a few experiments in this direction; these were with formiate, acetate, and butyrate of sodium; the results are given in Table IX. Analysis showed that, while the formiate and acetate of sodium were pure, the butyrate contained 21·7 p. c. of sodium, theory requiring 20·9 p. c. All attempts to purify the small quantity at my disposal were vain; the impurity was probably acetate; this would tend to reduce the molecular volume by increasing the specific gravity.

TABLE IX.

x.	Salt.	$t°$.	δ.	Mol. vol.	Diff. per CH_2.
2·0	Butyrate	20°	1·04349	1935·82	
2·0	Acetate	20	1·04380	1881·58	13·74
2·0	Formiate	20	1·04561	1851·58	15·00
1·0	Butyrate	20	1·02278	1867·46	
1·0	Acetate	20	1·02280	1840·05	13·70
1·0	Formiate	20	1·02345	1825·19	14·86
2·0	Butyrate	40	1·04173	1939·08	
2·0	Acetate	40	1·04279	1883·41	13·92
2·0	Formiate	40	1·04436	1853·82	14·81
1·0	Butyrate	40	1·02184	1869·18	
1·0	Acetate	40	1·02218	1841·16	14·07
1·0	Formiate	40	1·02276	1826·44	14·72

I hope soon to be able to carry out further experiments in the field of inquiry thus opened up.

XX. *Note on the Measurement of the Electric Resistance of Liquids. By* Professors W. E. Ayrton, *F.R.S., and* John Perry, *M.E.**

[Plate II.]

SOME time back a paper was communicated by Prof. Reinold to this Society on the Resistance of Liquid Films, which had a double interest, arising from the great value of the results arrived at and from the method employed to obtain them. It is of course well known that the great difficulty in measuring the resistance of a liquid arises from the polarization of, or actual deposit of gases on, the anode and cathode, which makes the apparent resistance of the liquid far greater than the true value. To overcome this difficulty Kohlrausch employed rapidly alternating currents; and Dr. Guthrie, with Mr. Boys, dispensed altogether with the anode and cathode by observing the amount of twist produced in a fine steel wire supporting a vessel of liquid when a magnet was rotated at a fixed speed in the neighbourhood.

But there is another method of measuring the resistance of a liquid independently of its polarization—the one so successfully employed by Prof. Reinold, and which consists in measuring by means of an electrometer the potential-difference at two fixed points in a column of the liquid when a current of known strength is passing through it.

At the time Prof. Reinold communicated his paper, we mentioned that some years previously certain experiments had been conducted in our laboratory in Japan for the purpose of ascertaining how far the electrometer method of measuring the resistance of a liquid was entirely independent of polarization; and as we have since come across the results of these experiments in turning over some papers, we have thought that the information may possess some interest for the Members of this Society. The experiments were made at the commencement of 1878 by some of our students; and the first part of the investigation was for the purpose of ascertaining how the resistance of water varied with the electromotive force employed and with the temperature of the water when, first, the resistance was measured by the current which a known electromotive force could send between platinum plates of known size and at fixed distances apart in the water, and, secondly, when the resistance was measured by a comparison of the potential-differences of two platinum wires placed in the water at fixed distances apart, with the potential-differ-

* Communicated by the Physical Society, having been read June 9, 1883.

ences when the same current was being sent through a known resistance.

Figs. 1 and 2, Pl. II., show the arrangement of the apparatus used in the experiments. B is the battery producing the current passing between the platinum plates P and P′. G is a delicate reflecting galvanometer measuring the current. E is a quadrant-electrometer which measures the difference of potentials between the two wires W and W′. These two platinum wires W and W′ were immersed in glass tubes; and their ends were above the bottom of the glass tubes as shown. Figure 1 shows the connexions when the differences of potentials between W and W′ were being measured by the electrometer, and figure 2 when the differences of potentials at the two ends of the known resistance-coil, of 10,000 ohms, were being measured.

The following Table gives the dimensions of the various parts of the apparatus:—

Diameter of the beaker at water-line	8·5	centim.
Height of water-line above the bottom ...	5·76	"
Distance between centres of wire tubes (W, W′ in fig. 2)	4·88	"
Distance between the platinum plates......	7·3	
Part of the glass tube surrounding the wire dipped in water....................	2·14	"
Part of the platinum wire in water.........	0·91	"
Outside diameter of the glass tube	0·87	"
Size of platinum plate : height	3·28	"
" " " : width	2·29	"

Before each experiment, when no current was passing, the difference of potentials between the plates and wires was reduced to 0, if not 0 already. The wires W and W′ were heated to redness before each experiment, and the platinum plates cleaned.

At the beginning pure distilled water was used; and this water was not added to all the time: it therefore lost a little by evaporation during the course of the experiment, and may have become a little dusty; but as the main object of the investigation was to examine the method of testing, and not for the purpose of measuring the specific resistance of water or of any other particular liquid, this result was of little consequence.

The following is a sample of the experiments made:—

January 25, 1878.—Battery-power employed $\frac{1}{6}$ of 23 Daniell's cells, having an E.M.F. of 4·08 volts, and which gave a deflection of 468 divisions on the galvanometer when shunted with the $\frac{1}{749}$ shunt, and when a resistance of 10,000 ohms was introduced in the circuit.

Time after putting on battery.	Galvanometer-deflection.	Electrometer-deflection.		Temperature.
		Figure 1.	Figure 2.	
1^m	99	10	...	59°·5 F.
2	96	10		
3	94	10		
Plates and wires thoroughly discharged.				
1^m	99	......	14	61° F.
2	98	......	14	
3	90	......	14	

Time after putting on battery.	Resistance as determined by the galvanometer.	Resistance as determined by the electrometer.
1^m	37000	7100
2	38000	7100
3	39000	7100

The annexed Table gives the results of a long series of experiments:—

	Total electromotive force, in volts.	Temperature.	Resistance as determined by galvanometer at end of one minute.	Ratio of resistance at the end of second minute to the resistance at the end of the first.	Ratio of resistance at the end of third minute to the resistance at the end of the first.	Resistance as determined by electrometer.	Ratio of resistance determined by galvanometer to resistance as determined by electrometer.	Date.
	0·93	58° F.	93900	1·27	1·4	15000	6·26	23rd Jan.
		60	133000	1·12	1·37	15000	8·87	23rd „
A	0·93	100	56000	1·16	1·27	8000	7·00	24th „
	1·86	63	53000	1·10	1·14	10670	5	24th „
	1·86	62	51500	1·12	1·18	10300	5	24th „
B	1·86	102	32000	1·17	1·27	9670	3·31	24th „
	4·08	59·5	37000	1·03	1·05	7100	5·21	25th „
	4·08	102	19800	1·01	1·06	3270	6·06	28th „
	6·17	60	21000	1·0	1·0	4450	4·72	28th „
C	6·17	106	12000	1·08	1·08	2700	4·44	31st „
	16·45	62	12700	1·01	1·99	3170	4·01	31st „
D	16·45	107	7700	1·04	1·04	1953	3·94	31st „

Only one number is given for the resistance as determined by the electrometer in each case, because it was found not to vary much during the time of electrification; whereas the resistance as determined by the galvanometer, as will be seen, increased in the earlier experiments 30 to 40 per cent. during the three minutes' electrification.

The total electromotive force in each case was determined by making a comparison by means of the electrometer with one of Clark's standard cells.

From these observations the following conclusions may be drawn:—First, the resistance measured by the galvanometer is much greater when using about 1 volt than when using nearly 2, at the same temperature (compare observations A and B), whereas the electrometer-measurements altered very little at all. Again, comparing C and D, we see that the resistance measured by the galvanometer is much greater when using 6 volts than when using 16. In this case, however, the measurements of the electrometer are also considerably greater in the first case than in the second, the temperature being the same. Secondly, if the electromotive force is less than the decomposing electromotive force, then the smaller it is the more does the resistance alter from one to two minutes' electrification, and from two to three minutes'. Whenever, however, the electromotive force is sufficiently high for decomposition to take place, the electrification seems to produce but little change in the resistance. The resistance of the water diminishes as the temperature rises, the electromotive force being kept constant.

The following experiments were made preliminarily to explorations of the region between the two platinum plates in the water, for determining what were the directions of the lines of flow of current. We desired to see if there was any chance of being able to use platinum wires in glass tubes connected with the electrometer, as previously described.

In the following cases a long trough of water was used instead of the beaker.

The sensibility of the galvanometer was nearly the same throughout all the experiments, and was such that $\frac{1}{20}$ of the whole electromotive force employed produced a deflection of about 500 divisions when there was an external resistance of 10,000 ohms and when the multiplying-power of the shunt employed was 100·7, which shunt was used throughout all the experiments.

Four Menotti cells, having an electromotive force of 3·7 volts, were employed in each of the following experiments. In A, B, C, D, E, F, and G the two platinum plates were placed parallel to one another at a distance of 90 centimetres apart. The two wires and their glass tubes were placed to commence at a distance of 80 centim.—that is, each being 5 centim. from the platinum plate. The lower ends of the platinum wires were each $1\frac{1}{2}$ centim. above the lower ends of the glass tubes; and the lower ends of the glass tubes were 1 centim. below the surface of the water. The two platinum plates and one of the platinum wires were kept immovable, while the other platinum wire was moved along the trough.

Distance between platinum wires.	Time after putting on battery.	Galvanometer-deflection.	Electrometer-deflection.	Temperature of water.
February 21, 1878.				
centim.	m s			
A 80	1 0	669	53	13° C.
A 60	1 30	662	40	
A 40	1 50	658	27	
A 20	2 15	654	15	
B 80	1 0	667	52	13° C.
B 60	1 25	663	39	
B 40	1 45	657	27	
B 20	2 10	653	15	
February 22, 1878.				
C 80	1 0	680	50	13° C.
C 60	1 35	675	37	
C 40	2 10	672	26	
C 20	2 30	670	15	
D 80	1 0	685	49	13° C.
D 60	1 20	680	37	
D 40	1 50	677	24	
D 20	2 10	674	12	
E 80	1 0	697	48	13° C.
E 60	1 20	692	36	
E 40	1 50	688	24	
E 20	2 20	685	13	
F 80	1 0	699	50	13° C.
F 60	1 35	694	37	
F 40	1 50	690	25	
F 20	2 15	687	13	
G 80	1 0	over 717	51	13° C.
G 60	0 30	"	38	
G 40	2 0	"	25	
G 20	2 20	"	15	

The object, of course, of taking the galvanometer-readings was to ensure that no material change was taking place in the current through the weakening of the battery or otherwise while the experiment was being made.

The experiments E and F appear most satisfactory of this set; and from these it seems that the resistance of the upper layer of water-column is nearly proportional to the distance between the platinum wires, except for the nearest distance, in which case the column seems to have a slightly larger resistance than it ought to have. This perhaps arose from the fact that, although the platinum plates nearly filled up the entire section of the trough, still the lines of flow at the platinum wire, which was kept stationary at a distance of 5 centimetres from one of the plates, were not quite parallel to the edge of the trough.

The two following sets of experiments, H and I, differ from the preceding only in that the lower end of the glass tube was one centimetre above the bottom of the trough; and from these two sets of experiments we see that the resistance of the lower layer of water-column, as measured by the electrometer, is nearly proportional to the distance between the wires, except, again, for the shortest distance.

	Distance between platinum wires.	Time after putting on battery.	Galvanometer-deflection.	Electrometer-deflection.
		m s		
H	80	1 0	709	50
	60	1 35	703	37·5
	40	2 0	700	26
	20	2 20	698	13
I	80	1 0	704	49
	60	1 20	700	37
	40	1 45	796	25
	20	2 20	792	13

The next experiments were for the purpose of seeing whether the potential, as measured by the electrometer, would come out uniform at all points in one vertical transverse section of the trough as well as at all points in one of the glass tubes.

	Distance between the platinum wires W and W′, in centimetres.	Position of the lower end of one of the tubes.	Galvanometer-deflection.	Electrometer-deflection.
J	80	Up.	696	49
	80	Down.	692	49
	80	Up.	689	49

"Up" means that the lower end of the glass tube was about 1 centim. below the surface of the water; and "Down" that it was about 1 centim. above the bottom of the glass trough. The platinum wire was now raised about 4 centim. above the bottom of the glass tube when the glass tube was down and the electromotive force was unaltered. The potential therefore, at all points in a vertical transverse section as well as at all points in the glass tube, is the same as measured by the electrometer.

The next set of experiments, K and L, were made under exactly the same conditions as A, B, C, D, E, F, and G, with the exception that the terminal platinum plates were now perpendicular to each other, the plate towards which the wire was moved being parallel to the long side of the trough.

	Distance between the platinum wires, in centimetres.	Time after putting on battery.	Galvanometer-deflection.	Electrometer-deflection.	Temperature.
		m s			
K	80	1 0	706	51	13° C.
	60	0 30	701	38	
	40	0 50	699	27	
	20	2 0	697	15	
L	80	1 0	705	51	13° C.
	60	0 30	701	40	
	40	0 55	698	27	
	20	2 20	697	15	

The resistance of the longer column of the water as measured by the electrometer is about the same as before, whereas that of the shorter is even greater; so that the resistance for the 80-centimetre column is even still less than four times that for the 20-centimetre one. But since the platinum plate near the stationary platinum wire was in these last two sets of experiments K and L kept parallel to the trough (that is, parallel to the mean direction of the lines of flow), it follows that any want of parallelism of the lines of flow to the edge of the trough at the point where was the stationary wire would be exaggerated by this mode of placing the plate; and since we observe that the error in the proportional law for distance is also increased, we may conclude that the explanation given above of the want of perfect accuracy in the proportional law being due to want of perfect parallelism in the lines of flow is the correct one.

In all the previous experiments the distance between the electrometer-wires only was altered; but in the next set the distance between the platinum plates as well as that between the platinum wires was altered, the distance between each plate and wire being kept constant. Further, the resistance determined from the electrometer was calculated, not, as before, by comparing the electrometer-deflection when its electrodes were attached to the platinum wires with the deflection obtained when its ends were attached to a known resistance traversed by the same current, but by first determining the absolute value, in volts, of the electrometer-scale with the absolute value, in ampères, of the galvanometer-scale, and by observing the electrometer- and galvanometer-deflections in each experiment.

Battery-power employed, 4 Menotti's cells. Temp. 14° C.

Time after putting on battery, in minutes.	Galvanometer-deflection.	Electrometer-deflection.	Resistance, as determined by galvanometer, in ohms.	Resistance, as determined by electrometer, in ohms.
Distance between the platinum plates 20 centim. ,, ,, ,, wires 10 ,,			Shunt $\frac{1}{990 \cdot 2}$.	
1	230	19	27000	11340
2	220	19	28000	11760
3	214	18·5	29000	11900
4	210	18	29600	11800
5	206	17	30000	11400
Distance between the platinum plates 90 centim. ,, ,, ,, wires 80 ,,			Shunt $\frac{1}{100 \cdot 7}$.	
1	634	42	97000	90200
2	621	42	98980	92070
3	614	42	99960	92900
4	608	41	100940	91900
5	602	41	111960	101900
Distance between the platinum plates 90 centim. ,, ,, ,, wires 80 ,,			Shunt $\frac{1}{100 \cdot 7}$.	
1	633	43	97000	93100
3	621	42	98000	91100
4	614	42	99000	92100
5	609	42	100000	93000
6	604	41	101000	92000
Distance between the platinum plates 20 centim. ,, ,, ,, wires 10 ,,			Shunt $\frac{1}{990 \cdot 2}$.	
1	245	21	25500	11900
2	235	20	26600	11700
3	230	20	27200	11960
4	225	19	27800	11670
5	220	19	28400	11930

The resistance, therefore, as measured by the galvanometer, does not increase as rapidly as the distance separating the plates, while that as measured by the electrometer is fairly in proportion to the distance. The explanation of the former is probably due to the fact that, since the electromotive force employed in all these four sets of experiments was constant, a greater current flowed when the plates were nearer than when they were far apart, hence that the resistance due to the layer of gas was greater when the plates were near than when they were far.

And this leads to a simple method of accurately measuring the resistance of liquids by using a galvanometer. The method, which was independently arrived at by one of our assistants (Mr. Mather), is now employed in our laboratory, and is so simple that we feel it can hardly be novel. It is as follows :—In a long vertical glass tube containing the liquid there are two metallic disks, not necessarily of platinum, and of about the same diameter as the tube. One of these can slide up and down the tube, so as to be able to be set at any fixed distance from the other. The disks are first put tolerably far apart, and a certain convenient current made to flow, which is measured on a galvanometer in the circuit. The plates are now made to approach and the current kept exactly the same by the insertion of an external resistance; whence it follows that the resistance of the column of liquid which has been subtracted from that originally separating the plates is equal exactly to the external resistance necessary to be inserted to keep the current constant.

February 28, 1878.

The next set of experiments was made to determine the alteration in resistance of a long trough of water when the distance between the centres of the platinum plates was kept constant at 90 centimetres, and the positions of the platinum plates varied as shown in the figures.

Galvanometer-Constant.—4 Menotti's cells with an E.M.F. 3·8 volts gave a deflection of 618 when a resistance of 10,000 ohms was in circuit and the galvanometer shunted with the $\frac{1}{999 \cdot 2}$ shunt.

The 4 Menotti's cells were employed and the galvanometer shunted with the $\frac{1}{100 \cdot 7}$ shunt, and the readings were in each case taken one minute after the application of the battery.

	Position of plates.	Galvanometer-deflection.	Temperature.
I.	— —	642	16° C.
	\	640	
	| |	622	
II.	| |	634	16° C.
	\ \	643	
	— —	647	

Two sets of experiments in the reverse order were taken to eliminate any change that might take place in the deflection from weakening of the battery, or from polarization of the plates, or from set of the galvanometer-fibre. The constant distance between the centres of the plates was now diminished to 20 centimetres, when the following results were obtained, the $\frac{1}{990\cdot 2}$ shunt being employed.

	Position of plates.	Galvanometer-deflection.	Temperature.
III.	| |	245	16° C.
	\ \	291	
	— —	304	
IV.	— —	315	16° C.
	\ \	285	
	| |	239	

Both therefore at the greater and at the less distance the resistance is least when the platinum plates are edge on ; a result that could hardly have been expected for the longer distance, considering that the width of each plate was only about 6 centimetres.

March 1, 1878.

In the following experiments one plate only was turned. The galvanometer had about the same sensibility as before. The $\frac{1}{100\cdot 7}$ shunt was used when the distance between the centre of the plates was 90 centimetres, and the $\frac{1}{990\cdot 2}$ shunt when it was 20 centimetres. An electromotive force of 3·8 volts was employed in each test.

Distance between the centres of the plates 90 centimetres.

	Position of plates.	Galvanometer-deflection.	Temperature.
V.	│ │	633	15° C.
	│ ╲	634	
	│ —	637	
VI.	│ —	660	15° C.
	│ ╲	655	
	│ │	651	
	Distance between the centres of the plates 20 centimetres.		
VII.	│ —	403	15° C.
	│ ╲	368	
	│ │	358	
	Battery reversed.		
VIII.	│ │	208	
	│ ╲	211	
	│ │	229	

Here again, then, the resistance is least with the plate end on, even when the distance between the centres of the plates is as much as 80 centimetres.

This apparent anomaly of the smaller resistance obtained when one or both plates is put end on is, as was pointed out by Mr. Boys, probably due to the smaller density of the gas which is deposited on a plate when it is put end on (in consequence of the current flowing from both sides of the plate into the liquid under these circumstances) more than compensating for the want of parallelism of the lines of flow when one or both of the plates are put end on.

XXI. *On Mr. Ferrel's Theory of Atmospheric Currents.* *By* Professor J. D. Everett, *F.R.S.**

Mr. D. D. HEATH, in his attack upon Mr. Ferrel in the July number of the Philosophical Magazine, overlooks the well-known principle of Conservation of Areas for the case of a particle acted on by a force which always passes through a fixed line. In such a case, the projection of the particle on a plane perpendicular to the fixed line describes equal areas in equal times about the fixed line.

If the particle is constrained to remain on the surface of a smooth sphere, and is acted on by a force always directed to the centre of the sphere, the property holds with respect to

* Communicated by the Author.

any fixed line through the centre. If the sphere be rotating about this line, its rotation (its surface being supposed frictionless) will not affect the motion.

This result is not in contradiction (as Mr. Heath maintains) to the fact that the particle moves uniformly in a great circle; as the following proof shows.

Take the particular case discussed by Mr. Heath—that of a particle projected from the equator along a meridian towards the north. The relative initial velocity here is northward, and must be compounded with the westward velocity of the equator to get the absolute initial velocity. Let ϕ denote the angle which this resultant velocity makes with west, and Ω the constant angular velocity with which the particle moves in its own great circle—a circle which cuts the equator at the angle ϕ. Also let λ, l denote the absolute latitude and longitude of the particle, measured from the starting-point. Then the particle in time t describes an arc Ωt which is the hypotenuse of a spherical right-angled triangle having λ and l for sides, and ϕ is the angle opposite to the side λ.

Hence, by spherical trigonometry,

$$\cos \Omega t = \cos \lambda \cos l, \quad \ldots \ldots \quad (1)$$

$$\tan l = \cos \phi \tan \Omega t. \quad \ldots \ldots \quad (2)$$

Differentiating (2), we obtain

$$\frac{dl}{dt} = \Omega \cos \phi \sec^2 \Omega t \cos^2 l;$$

that is, by (1),

$$\frac{dl}{dt} = \Omega \cos \phi \sec^2 \lambda.$$

This is the angular velocity round the earth's axis. Let R denote the earth's radius; then the distance from the axis is $R \cos \lambda$. The linear velocity of rotation is therefore

$$R\Omega \cos \phi \sec \lambda,$$

and the rate of describing area is

$$\tfrac{1}{2} R^2 \Omega \cos \phi,$$

which is constant. Q. E. D.

In the meteorological application which is under discussion, the moving air receives not merely an initial impulse but a steadily applied force directed towards the north, as it travels from lower to higher latitudes. Mr. Heath's argument to show that it can never reach the polar regions is therefore beside the mark.

In spite of the very bitter tone which Mr. Heath has chosen to adopt towards Mr. Ferrel, he is thus clearly wrong in the main point of his criticism.

I may refer to my paper in the Philosophical Magazine for September 1871 for some investigations which confirm Mr. Ferrel's results.

Bushmills, co. Antrim,
July 20, 1883.

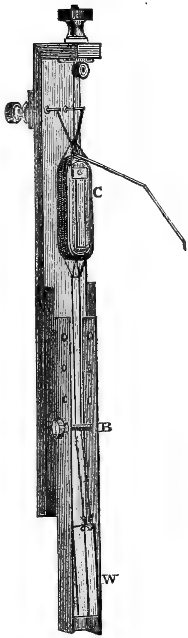
XXII. *On the Determination in Absolute Units of the Intensities of Powerful Magnetic Fields. By* A. Gray, *M.A., F.R.S.E., Chief Assistant to the Professor of Natural Philosophy in the University of Glasgow*.*

SO far as I know, no method has yet been published by which the intensity of a powerful magnetic field, such as that in the space in which the armature-coils of a dynamo move between the poles of the field-magnets, can be determined in absolute measure; and inventors of dynamos and other experimenters in practical electricity have hitherto, at a great expense of time and money, had to construct their actual apparatus, and find by the results obtained in actual practice the efficiency or non-efficiency of their arrangements. So much so has this been the case that it has even been held that the absolute value of the intensity of a magnetic field is a thing with which practical electricians have no concern, that, although theoretically it may have some importance, it has no application in practice. That all measurements, whether of electric or of magnetic quantities, should be expressed in absolute units—that is, in units altogether independent of the locality, surroundings, and apparatus of the experimenter—is, I think, of very great importance, if these results are to be of any service to others. And it would, I feel sure, be a great benefit to all engaged in practical electrical work, if the intensities of the magnetic fields obtainable with various forms of electromagnets, made with different kinds of iron, were determined in absolute units and published with full particulars of the apparatus. But failing the knowledge which would be derived from experiments such as these, a very great saving of time and money might be made if the inventor of a dynamo, for example, were to first make models of his magnets on a small scale, and determine the magnetic-field intensities obtainable in them with different current-strengths, and then to reason from the results in the model to those in the full-sized machine. The object of the present paper is to describe some methods by which the intensity of a powerful magnetic field may be determined in absolute units. The methods are wholly due to Sir William Thomson; but for the sketch of theory given

* Communicated by the Author.

below for each, the details of the experimental arrangements, and the results I am alone responsible.

(α) The first method, which is somewhat interesting theoretically, is one which was used in the determination of the magnetic field-intensities in the space in which the signal-coil is suspended in some of Sir William Thomson's Siphon Recorders made with permanent magnets. In it advantage is taken of the signal-coil, which consists of a rectangular coil a little more than 5 centim. long and 2 centim. broad, made of thin wire and supported by a silk thread above, so as to hang in a vertical plane round a rectangular core of iron, which nearly fills, but nowhere touches, the coil. To the lower end of the coil two silk threads are attached, as shown in the diagram, and are stretched against a bridge B by two weights resting on the inclined plane W. This bifilar arrangement gives a directive force, tending to bring the plane of the coil into parallelism with that of the bifilar threads; so that when the coil is disturbed from that position, which is one of stable equilibrium, and then left to itself, it will, if the circuit be not closed, vibrate about the position of equilibrium with a determinate period of oscillation, with slowly diminishing range, until at last it comes to rest.



But if the circuit be closed through a high resistance, the coil will come more rapidly to rest; and if we gradually diminish this resistance, deflecting the coil through the same angle and noting its subsidence at each diminution, we shall find it come more and more quickly to rest, until a resistance is obtained with which in circuit it just returns to the position of equilibrium without passing that position. When this resistance has been determined, the strength of the field can be calculated.

Let θ be the deflection of the coil from the position of equilibrium at time t, and T its period of oscillation when the circuit is not closed. We have then, neglecting the resistance of the air and other disturbances, for the equation of motion,

$$\frac{d^2\theta}{dt^2}+\frac{4\pi^2}{\mathrm{T}^2}\theta=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Let now the circuit of the coil be closed; a retarding force due to the current induced in the wire, and, if the effect of self-induction be neglected, proportional to the angular velocity, will act on the coil; and the equation of motion for this case will be of the form

$$\frac{d^2\theta}{dt^2}+k\frac{d\theta}{dt}+\frac{4\pi^2}{T^2}\theta=0. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

For let I be the mean intensity of the magnetic field over the space occupied by the coil at time t, M the electromagnetic inertia (coefficient of self-induction) of the circuit for that position of the coil, R the total resistance in the circuit, μ the moment of inertia of the coil round a vertical axis passing through its centre, L the effective length of wire in the coil (that is, the length of wire in its two vertical sides), and b the mean half-breadth of the coil. If we call N the number of lines of force which pass through the coil at time t, and γ the strength of the induced current in the coil at that instant, we have plainly

$$N=bIL\sin\theta-M\gamma.$$

The rate at which N increases per unit of time is therefore

$$\frac{dN}{dt}=bIL\cos\theta\frac{d\theta}{dt}+bL\sin\theta\frac{dI}{dt}-\frac{d}{dt}(M\gamma);$$

and if θ be small, and I be therefore supposed the same for every position of the coil, we have approximately

$$\frac{dN}{dt}=bIL\frac{d\theta}{dt}-\frac{d}{dt}(M\gamma).$$

But $\frac{dN}{dt}$ is the electromotive force due to the inductive action; hence the current γ is by Ohm's law given by the equation

$$\gamma=\frac{bIL}{R}\frac{d\theta}{dt}-\frac{1}{R}\frac{d}{dt}(M\gamma).$$

It was assumed that the second term of this expression for γ would prove negligible in comparison with the first; and this assumption was so far justified by the results of the experiments, which agreed fairly well with results obtained, for other instruments of the same pattern, by a modification of the second method described below.

The couple due to the action of the field on the current is $bIL\gamma$; and therefore, on the supposition of negligible self-induction, the retardation of the angular velocity of the coil at time t is

$$k\frac{d\theta}{dt}=\frac{b^2I^2L^2}{\mu R}\frac{d\theta}{dt}.$$

Hence (2) becomes

$$\frac{d^2\theta}{dt^2}+\frac{b^2\mathrm{I}^2\mathrm{L}^2}{\mu\mathrm{R}}\frac{d\theta}{dt}+\frac{4\pi^2}{\mathrm{T}^2}\theta=0. \quad . \quad . \quad . \quad . \quad (3)$$

The motion represented by this differential equation will be oscillatory or non-oscillatory, according as the roots of the auxiliary quadratic are imaginary or real—that is, according as $\frac{4\pi}{\mathrm{T}} >$ or $< \frac{b^2\mathrm{I}^2\mathrm{L}^2}{\mu\mathrm{R}}$. Hence, if R be the critical resistance at which the motion just ceases to be oscillatory, we have

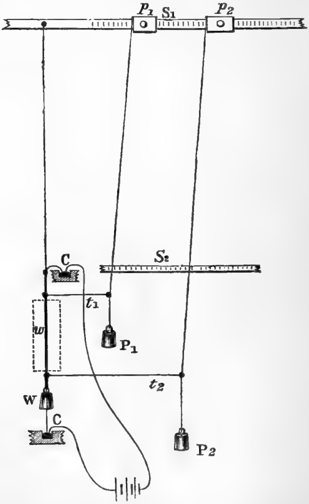
$$\mathrm{I}^2=\frac{4\pi\mathrm{R}\mu}{\mathrm{T}b^2\mathrm{L}^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

When L and b are expressed in centimetres, μ in grammes and centimetres, T in seconds, and R in centims. per second, I is given by this equation in absolute C.G.S. units of magnetic field-intensity.

The method of experimenting consisted in first finding the value of T, the free period of vibration of the coil with its circuit uncompleted, then finding the resistance which, being placed in circuit with the coil, just brought the needle to rest without oscillation. This resistance was conveniently obtained by means of a resistance-box included in the circuit, and therefore added no self-induction to that in the coil. An aluminium arm attached to the coil, and carrying the siphon, served as an index to render the motions of the coil visible. The resistance R was first made much too great, so as to give a slow subsidence, then gradually diminished until the value which just prevented oscillation was reached; and it was found that this value could be determined easily within 50 ohms, and, with great care, to 20 ohms. As the experiments on the recorders had to be made somewhat hurriedly, and, on account of the disturbances, neglected, and, further, as μ was taken as equal to $\mathrm{W}b^2$, where W is the mass of the coil, the results could not be taken as giving more than a rough approximation to I; but those for two instruments are given below in illustration of the method. For both instruments the values of W, L, and b were the same, and were respectively taken as 3·343 grammes, 3338 centim., and ·95 centim. Each coil had a mean vertical length of 5·3 centim., a mean breadth of 1·9 centim., and contained 45·72 metres of fine wire arranged in 290 turns, and had a resistance of about 500 ohms.

	T.	R.	I.
(1)	·465 sec.	3330×10^9 centim. per sec.	5150 C. G. S.
(2)	·500 „	3530×10^9 „ „	5120 „

(β) The second method is one which was adopted in measurements which I made of the fields of some experimental electromagnets, and it depends on the action between a magnetic field and a conductor in the field carrying a constant current. The ends of the cores of the electromagnets were long straight surfaces of iron, separated from the surface of iron placed opposite to them by only a narrow space at each pole. In the experiments, of which I give some results below, the intensity of the field was found, in a space about 2 millim. wide, between one end of the iron core of an electromagnet and the surface of a thick piece of iron. The length of the pole-face was 30 centim. The magnet and corresponding iron were placed with their faces vertical; and a copper wire was suspended vertically in the space between them, and insulated from their surfaces. The lower end of the wire dipped into a mercury-cup, and its upper end into a second mercury-cup on the top of the electromagnet, without touching the sides or bottoms of the cups. By means of these cups the wire could either be put in series with the coil of the electromagnet or in circuit with an independent battery or generator. The wire was hung in a vertical position (as shown in the diagram)



from the lower end of a cord about two metres long, attached to a firm support above, and was kept taut by two half-pound weights (represented by the single weight W in the diagram) hung at the ends of a cross bar firmly attached to the wire. The position of the wire was adjusted by shifting the point of attachment of the cord above until the wire was exactly opposite two marks, one at the upper end and one at the lower end of the vertical central line of the pole-face. A thin thread, t_1, was attached to the wire just above the electromagnet, and carried out to one side nearly horizontally, and attached by its other end at a point near the lower end of the cord of the pendulum, P_1. A second thread, t_2, was attached to the lower end of the wire, and carried out parallel to the thread t_1, and attached to a second pendulum, P_2, at a somewhat greater distance from the wire. The pendulums, P_1, P_2, were simply weights, in some cases of half a pound each, and in other cases 100 grammes each, hung by fine cords from one to two metres long from blocks, p_1, p_2, which could be moved along a horizontal scale, S_1, placed nearly in a vertical plane passing through the wire at right angles to the coil of the electromagnet, and therefore parallel to the pole-face. A second scale, S_2, placed just above the point of attachment of the upper thread, and nearly in the same vertical plane as S_1, gave the position at any time of a point in each of the pendulum-cords at the same distance below the point of suspension.

The method of performing an experiment was as follows:—The vertical positions of the cords of the pendulums when the threads t_1, t_2 were quite slack and the wire was in position were read off from the scales. A current was then sent through the wire and coil in series. The electromagnetic action between the current in the wire and the magnetic field caused the wire to move bodily to one side across the lines of force; and the direction of the current in the wire was so arranged that the side towards which the wire moved was that remote from the pendulums. By moving out the blocks, p_1, p_2, pull (in consequence of the deviation of the pendulums from the vertical) was brought to bear through the threads t_1, t_2 on the upper and lower ends of the wire; and this was gradually increased until the wire was brought back as nearly as possible to its former vertical position. The readings given by the pendulums on the two scales were then taken, and at the same time a reading of the current-strength on the current-galvanometer. The difference between the two motions on S_1 and S_2 for either pendulum, divided by the distance between the scales, gave the tangent of the inclination of the pendulum to the vertical.

Let, now, I be the mean intensity for the position of the wire of the magnetic field in C.G.S. units, γ the current flowing on the wire also in C.G.S., L the length of the wire in the field in centims., F the force per unit of length on the wire in dynes, we have

$$I\gamma = F.$$

But the total horizontal force exerted by the pendulums is $W_1 g \tan\theta_1 + W_2 g \tan\theta_2$, where W_1, W_2 are taken in grammes, and g, the acceleration produced by gravity, as approximately 981 centim. per second per second. Hence we have

$$I = \frac{g}{\gamma L}(W_1 \tan\theta_1 + W_2 \tan\theta_2). \quad . \quad . \quad . \quad (5)$$

From this formula the values of I can be calculated from the values of the other quantities found by experiment. The following are the results of some actual experiments made in September last:—

	W_1, in grammes.	W_2, in grammes.	γ, in C.G.S.	I, in C.G.S.	I/γ*.
(1)	100	100	·09	2210	24555
(2)	,,	,,	·132	3410	26590
(3)	,,	,,	·140	4170	29785 (?)
(4)	,,	,,	·156	4373	28032
(5)	,,	,,	·172	4864	28283

The same method was applied to find the value of I for different parts of the field on the two sides of the middle position. With two operators (one to move the pendulums, the other to watch the wire and take the galvanometer-readings) the experiments were quickly and satisfactorily performed.

The following modification of the same method has been used for the determination of I for several recorder magnets, with results agreeing well with those obtained (for other instruments) by the first method described above. A horizontal bar, carrying a divided scale, was fixed parallel to the equilibrium position of the plane of the coil, in a vertical plane passing through the glass siphon a little beyond the point of the aluminium arm supporting it. A single block, to which the upper end of a pendulum-thread was attached, was made movable along the horizontal bar, so that any displacement of the point of suspension could be observed by means of the divided scale. The pendulum was made of a brass weight, generally of one gramme, as bob, hung by a very fine silk thread long enough to allow the bob to hang a few centimetres below the siphon. When the coil was in the position of equilibrium, the pendulum was moved until the thread just rested against the siphon without deflecting it, and the reading on the scale was noted. A current from one or two Daniell's cells was then sent through the coil so as to deflect it from the equilibrium

* This column is given, as γ was also the magnetizing current.

position in the direction to cause the siphon to press against the thread. The point of suspension of the pendulum was then moved so as to restore the coil to its initial position, and the distance through which it was moved noted. This distance, divided by the vertical height of the point of suspension above the point at which the thread touched the siphon, gave the tangent of the inclination of the thread to the vertical; and this multiplied by the downward pull in dynes on the weight gave the horizontal force restoring the coil to its equilibrium position.

When equilibrium had been obtained, the difference of potentials between the terminals of the coil was immediately measured in volts by means of a potential-galvanometer previously arranged in readiness; and from the result and the known resistance of the coil the current flowing was deduced in C.G.S. units. The mean intensity I of the field over the space occupied by the coil in the equilibrium position was then easily calculated. For let γ be the current in the coil, θ the inclination of the pendulum-thread above the siphon to the vertical, l the distance in centimetres of the point at which the thread touched the siphon from the vertical axis round which the coil turned, and W the pendulum-weight in grammes. Using L and b with the same meaning as before, we have plainly the equation of equilibrium

$$\mathrm{IL}b\gamma = \mathrm{W}gl \tan\theta,$$

and therefore

$$\mathrm{I} = \frac{\mathrm{W}gl}{\mathrm{L}b\gamma}\tan\theta. \quad . \quad . \quad . \quad . \quad . \quad (6)$$

The method of determining a magnetic field-intensity by measuring the electromagnetic action on a conductor carrying a current in the field, may be very conveniently applied to the space between the opposite poles of a field magnet in which the armature-coils of a dynamo are made to revolve. A rectangular frame of stout copper wire or narrow copper strip, containing one or more turns (insulated from one another if there are more than one), is constructed of such a size that two opposite sides are at a distance apart equal to the diameter of the armature, and of length equal to the length of the pole-faces. The rectangle should begin and end near the middle of the two other sides; and the two ends of the wire should be brought out side by side, insulated from one another, at right angles to the side, a distance of two or three inches. A pair of knife-edges, which may be made for ease of a piece of hardened copper or brass wire filed so as to have a nearly triangular section, are soldered, one at the centre of the side where the ends of the wire are brought out, the other at the centre of the opposite side; so that the rectangle is symmetrical on the two sides of the knife-edges, and is horizontal when the knife-edges are turned downwards. The sides on which the knife-edges are placed may conveniently be slightly bent so as to have an upward convexity, in order that the line of knife-edges may, when the wire frame is made to rest on them, be a little above the centre of inertia of the system, in order that the frame may rest stably in the horizontal position; or the frame may be loaded so as to have sufficient stability by pieces attached below the knife-edges. The ends of the wire are to be bent first a little up, then downwards so that the point of one is about an inch nearer to the frame than the other, and both points are nearly in the line through the knife-edges.

The field-magnets are set up so that the plane cutting their pole-faces in which the field-intensity is to be measured is horizontal; and two supports, carrying metal V-shaped rests for the knife-edges, are arranged so that the frame when resting on its knife-edges has the two sides lying along the pole-faces in the horizontal plane through the two parts of these faces near which the field is to be determined. Two cups containing mercury are arranged on a support so placed that the ends of the wire of the frame dip into the mercury, without touching the sides or bottoms of the cups. One terminal of a convenient battery is connected to one of the cups; and the other terminal is so arranged that the circuit can be completed by depressing the terminal into the other cup, or broken by raising it, when required.

When an experiment is made, the magnets are excited by a current of the required amount, and a measured current is sent round the frame, which is made to turn on its knife-edges by the electromagnetic action. A weight which can be slided along the frame is hung on one of the sides which carry the knife-edges, and moved to such a distance from the line of knife-edges that the frame is brought back to its initial position. This distance in centimetres and the mass of the weight in grammes are carefully determined, with the strength of the current flowing in the frame; and the mean intensity of the field over the space occupied by the part of the frame in the field calculated from the results. Let I be this mean intensity, L the length of wire of the frame in the field, b the horizontal distance in centimetres of each side from the line of knife-edges, W the mass of the equilibrating weight in grammes, and r its distance from the line of knife-edges, g the acceleration produced by gravity (= 981 centim. per second per second), and γ the strength of the current in C.G.S. units; we have by equating moments,

$$\mathrm{I}=\frac{\mathrm{W}r}{\mathrm{L}\gamma}g. \qquad (7)$$

If instead of a single weight, W, several weights, W_1, W_2, &c., at distances r_1, r_2, &c. be used to restore equilibrium, we have of course, instead of Wr in this formula, simply the sum $W_1r_1 + W_2r_2 +$ &c.

Another modification of this method may be convenient in some cases. The field-magnets may be so placed that the plane cutting the poles in which the intensity is to be determined is vertical, and a rectangle made of the proper dimensions and hung by a torsion-thread or wire, or by means of a bifilar, so that it is in equilibrium in the required position, and turns when deflected round a vertical axis bisecting it. The ends of the wire forming the rectangle are carried down, insulated from one another, from the middle of the lower side, and bent so that their points are in the vertical line bisecting the rectangle, and dip, like the terminals of an Ampère frame, into two mercury-cups placed one above the other on supports arranged below. Two pendulums are arranged so that their points of suspension can be moved along horizontal bars at right angles to the equilibrium position of the frame, one in the vertical plane through each side of the rectangle. These pendulums carry at the ends of light but strong threads equal known weights; and each pulls horizontally on the corresponding side of the rectangle by means of a thread attached at one end to the middle point of the side, at the other to the pendulum-cord. The pendulums and threads are so arranged that the pulls are applied in opposite directions; and thus when the pulls are equal they give a couple tending to turn the frame round a vertical axis. Corresponding to each pendulum are a pair of horizontal scales, arranged like those shown in the second diagram above, by means of which, in the manner already described, the tangent of the inclination of the pendulum to the vertical is at once read off.

The method of experimenting is similar to that last described. The magnets are excited, and a measured current sent round the suspended rectangle, from a battery connected with the mercury-cups, in the direction to cause the rectangle to turn round in the direction opposite to the motion which the pendulums are arranged to produce. The points of suspension of the pendulums are then moved so as to restore the frame to the initial position, and the tangents of the inclinations of the cords obtained by means of the scales. If the field be symmetrical about the axis of suspension of the coil, the electromagnetic action of the frame will be a pure couple, and the equilibrating forces will form also a couple; but in general this will not be exactly the case. Let W be the mass, in grammes, of each pendulum-weight, θ_1, θ_2 the inclinations of the pendulum-cords to the vertical, and let L, b, γ have the same meaning as before; we get by equating moments of forces,

$$I = \frac{Wg}{L\gamma}(\tan\theta_1 + \tan\theta_2). \quad . \quad . \quad . \quad . \quad (8)$$

(γ) The third method is one which has been frequently used in the Physical Laboratory of the University of Glasgow, and consists in exploring the magnetic field by means of the induced current in a wire moved quickly across the lines of force over a definite area in the field. The wire is in circuit with a reflecting "ballistic" galvanometer—that is, a galvanometer the system of needles of which has so great a moment of inertia that the whole induced current due to the motion of the wire has passed through the coil before the needle has been sensibly deflected. The deflection thus obtained is noted, and compared with the deflection obtained when, with the same circuit, a portion of the conductor is made to sweep across the lines of force over a definite area of a uniform field of known intensity, such as that of the earth or its horizontal or vertical component.

In performing the experiments, it is necessary to take precautions to prevent any action except that between the definite area of the field selected and the wire cutting its lines of force. For this purpose the conducting-wire, which is covered with insulating material, is bent so as to form three sides of a rectangle, one of which is of the length of the portion of field to be swept over. This side is placed along one side of the space over which it is about to be moved so that the connecting-wires lie along the ends of the space; and the open rectangle is then moved in the direction of its two sides until the opposite side of the space is reached. The connecting-wires thus do not cut the lines of force, and the induced current is wholly due to the closed end of the rectangle.

Instead of a single wire cutting the lines of force, a coil of proper dimensions (for many purposes conveniently of rectangular shape), the mean area of which is exactly known, may be suspended in the field with its plane parallel to the lines of force, and turned quickly round through a measured angle of convenient amount not exceeding 90°; or it may be suspended with its plane at right angles to the lines of force and turned through an angle of 180°. If n be the number of turns, A their mean area, and I the mean intensity of the field over the area swept over in each case, then, in the first case, if θ be the angle turned through, the area swept over is $n\mathrm{A}\sin\theta$, and the number of lines cut is $n\mathrm{IA}\sin\theta$; in the second, the area is $2n\mathrm{A}$, and the number of lines cut is $2n\mathrm{IA}$.

In order that with the feeble intensity of the earth's field a sufficiently great deflection for comparison may be obtained, it is necessary that a relatively large area of the field should be swept over by the conductor. One convenient way is to mount on trunnions a coil of moderately fine wire of a considerable number of turns wound round a ring of large radius, like the coil of a standard tangent-galvanometer, and arranged with stops so that it can be turned quickly round a horizontal axis through an exact half-turn, from a position in which its plane is exactly at right angles to the dip. This coil, of course, always remains in the circuit of the ballistic galvanometer. The change in the number of lines of force passing through the coil in the same direction relatively to the coil, produced by the half-turn, is plainly equal to twice as many times the area of the turn of mean area as there are turns in the coil (the effective area swept over) multiplied by the intensity of the field.

A sufficiently large area of the earth's field, for comparison, may otherwise be obtained very readily by carrying the wire along a rod of wood (say two or three metres long), and suspending this rod in a horizontal position by the continuations of the conductor at its ends from two fixed supports in a horizontal line at a distance apart equal to the length of the rod, and securing the remaining wires in circuit so that they may not cause disturbance by their accidental motion. The rod will thus be free to swing like a pendulum by the two suspending-wires. The pendulum thus made is slowly deflected from the vertical until it rests against stops arranged to limit its motion. It is then quickly thrown to the other side against similar stops there, and caught. The straight conductor thus sweeps over an area of the vertical component of the earth's field equal to the product of the length of the rod into the horizontal distance between the two positions of the conductor at the extremities of its swing. The rod may be placed at any azimuth, as the suspending portions of the conductor in circuit, moving in vertical planes, can cut only the horizontal lines of force; and the induced currents thus produced have opposite directions and neutralize one another.

The calculation of the results is very simple. By the theory of the ballistic galvanometer (the same *mutatis mutandis* as that of the ballistic pendulum), if q be the whole quantity of electricity which passes through the circuit, and if θ be the angle through which the needle has been deflected, or the "throw," we have

$$q=\frac{2}{\mathrm{G}}\sqrt{\frac{\mu\mathrm{H}}{m}}\sin\frac{\theta}{2};$$

where μ is the moment of inertia of the needle and attachments, m the magnetic moment of the needle, H the earth's horizontal magnetic force, and G the constant of the galvanometer. If θ be small, as it generally has been in these expe-

riments, we have
$$q=\frac{1}{G}\sqrt{\frac{\mu H}{m}}\,.\,\theta,$$
and the quantities of electricity produced by sweeping over two areas, A and A′, are directly as the deflections.

Let A be the area of the field or portion of field the mean intensity (I) of which is being measured, A′ and I′ the same quantities for the known field, q, q' the quantities of electricity generated in the two cases, θ, θ' the corresponding deflections supposed both small; we have
$$q=AI=\frac{1}{G}\sqrt{\frac{\mu H}{m}}\,\theta,$$
$$q'=A'I'=\frac{1}{G}\sqrt{\frac{\mu H}{m}}\,\theta',$$
and therefore
$$I=\frac{A'\theta}{A\theta'}I'. \quad \ldots \ldots \quad (9)$$
If convenient, θ and θ' may be taken as proportional to the numbers of divisions of the scale traversed by the spot of light in the two cases.

I may remark, in conclusion, that the method described in (α) above gives, when the intensity of the field has been accurately measured by another method, theoretically a method of determining a resistance in absolute electromagnetic measure, This method is nearly the converse of that given by Weber, in which a magnet is made to oscillate within a coil of wire the circuit of which is closed. What I venture to propose is to hang a coil, the constants of which are known, in a sufficiently intense and uniform magnetic field, and find the decrement of the oscillatory motion produced by the induction: for it would scarcely be possible, I think, to observe with sufficient accuracy the point at which the coil just became "dead beat." How far this method may be practicable can hardly be affirmed without experiment; but I think the necessary calculations and corrections would be in some degree at least simpler than those required for the method given by Weber.

XXIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 71.]

June 6, 1883.—J. W. Hulke, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "The Estuaries of the Severn and its Tributaries, an Inquiry into the Nature and Origin of their Tidal Sediment and Alluvial Flats." By Prof. W. J. Sollas, M.A., F.R.S.E., F.G.S.

Various sources have been ascribed to the mud which is so characteristic of the estuaries of the Severn and its tributaries, such as the rivers themselves, the waste of mud shoals, or of bordering cliffs, or the sea. The author considered the effect of these sources of supply, and showed that, although the first three are doubtless to a certain extent correct, they are inadequate to account for some very important phenomena. The tidal silt, on microscopic examination, is found to consist of both inorganic and organic materials, the former being argillaceous granules, grains of quartz, flint, &c.; the latter, coccoliths, coccospheres, Foraminifera, occasional sclerites of Alcyonaria, fragments of Echinodermata, and triradiate spicules of Calcispongia, together with numerous spicules of siliceous sponges, a few Radiolaria, and a variable quantity of Diatoms. These organisms (described in detail by the author) are marine, and yet they occur on the banks of rivers at a great distance from a truly marine area. The author showed it to be improbable that they can have been derived, at any rate to a considerable extent, either from the older formations through which the Severn flows, or from the alluvial flats of its estuary; for although the latter do contain marine organisms of a generally like kind, the spicules &c. indicate corrosion, and are generally not so well preserved as those which occur in the tidal silt. It seems therefore necessary to conclude that a considerable proportion of the organisms now present in this have been brought from the sea; but sponges are not known to grow in any quantity nearer Bristol than the coasts of Devon and Pembrokeshire. It would therefore appear that these organisms, contrary to what might have been expected, have been drifted up into the tidal estuaries of the river for a very considerable distance. The author concluded by describing in detail the alluvial tracts of the Severn, which he considers to have been formed (with certain differences of level) much as tidal deposits are formed at the present day; and by pointing out the bearing of his investigations on the question of the probable results of the discharge of sewage into tidal rivers.

2. "Notes on a Collection of Fossils and Rock-specimens from West Australia, north of the Gascoyne River." By W. H. Hudleston, Esq., M.A., F.G.S.

3. "Notes on the Geology of the Troad." By J. S. Diller, Esq. Communicated by W. Topley, Esq., F.G.S.

This paper gave a brief account of the results obtained by the author whilst attached to the United-States Assos Expedition. Together with a geological map (scale 1:100,000), this was sent to Mr. Topley for the service of the new Geological Map of Europe (and its borders), which is now being prepared by a Committee of the International Geological Congress.

The country described is that lying south and west of the river Menderé (Scamander). The sedimentary rocks may be divided into three great groups:—

(1) An old, possibly Archæan, highly crystalline series, forming the mountainous lands of the Ida range (5750 feet), but also

appearing in smaller detached areas to the W. and N.W. Probably these have existed as islands from early time, and around these the later rocks have accumulated. Mt. Ida itself is almost a dome, the lowest rocks (talc schists) occupying the summit. On the northern slopes there is true gneiss. No igneous rocks enter into the structure of this mountain. At different horizons there are bands of coarsely crystalline limestone; and, as far as can be seen, this series is conformable throughout.

(2) Resting on these old rocks and in part made up of their remains is a series of partially crystalline rocks, chiefly limestone. It is probable that this series is in large part of Cretaceous age; but it contains rocks which are older, possibly Palæozoic. Eocene fossils have lately been discovered by Mr. Frank Calvert, which also may have come from this series. The rocks in the south of the Troad, hitherto supposed to be Lower Tertiary, are now known to be of later date. Sharply marked off from these older rocks are the Upper Tertiaries: these are of two ages, occurring in two distinct areas.

(3) The *Upper Miocene*, which fringes the western shores of the Troad, and forms a broader band at the north-west corner in the lower course of the Menderé. Hissarlik is built on this. These beds are marine, and belong to the *Sarmatian Stage*. The Troad is the most south-westerly point at which the *Mactrakalk* is yet known.

(4) Freshwater beds, which occur in force in the interior of the country, between the Menderé and the south coast, and in patches near the coast. These are *Upper Miocene* or *Lowest Pliocene*.

Later than these are the *Pliocene beds* of the great plain of Edsemet.

The igneous rocks are of various ages; but most are of Tertiary date. The oldest is a *granite* which intrudes through and alters the oldest (? Archæan) crystalline rocks. This is invaded by dykes of *Quartz-porphyry*.

Quartz-diorite invades and alters the group of partially crystalline rocks.

The oldest rocks in the newer series are the *Andesites* and *Liparites*. These, in part, are older than the Sarmatian stage, as the conglomerate at its base contains fragments of these rocks. But they are also in part of later date. Where they can be studied together the Liparite is the later of the two, as it flows through and carries up fragments of the Andesite. The Andesite (unlike the Liparite) seems to have reached the surface, in some cases, through volcanic vents.

Basalts and Nepheline-basalts are of late Tertiary date; possibly they are the latest volcanic rocks of the district; but their relation to the other eruptive rocks of the Troad cannot be definitely determined.

The volcanic rocks in the isolated area between Alimadja and Lyalar are interesting because their relative ages are here well seen. The earliest was melaphyre; this was followed by mica-andesite, hornblende-andesite, augite-andesite, basalt, and late (if not last) by liparite.

Mr. Topley, who, in the absence of the author, read the paper, explained the objects of the Assos expedition and the geological results obtained by Mr. Diller. He gave a short account of previous literature, and mentioned some of the main points in which our knowledge of the Troad is now advanced. Mr. Topley briefly described the physical geography and general structure of the country, illustrating this by means of a section which he had prepared from Mr. Diller's map and paper.

XXIV. *Intelligence and Miscellaneous Articles.*

ON EFFECTS OF RETENTIVENESS IN THE MAGNETIZATION OF IRON AND STEEL.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the April number of your Journal I observe a letter from Prof. Warburg showing that a preliminary communication of mine to the Royal Society, on the above subject, was in some points anticipated by a paper of his, published a year before. I regret very much that I have not been sooner acquainted with Prof. Warburg's investigations, to which even now I am unable to obtain access in Japan. Those of my results which have been previously stated by him may therefore be regarded as an independent confirmation of his work. The Preliminary Notice (published in the Proceedings of the Royal Society, vol. xxxiv. no. 220) refers to no more than a small part of an extended investigation of the relations of stress, permanent and induced magnetism, magnetizing force, and thermoelectric quality (in iron), with which I have been engaged for three years. I have recently learnt that some of the work has been previously done by E. Cohn, and, as it now appears, some by Warburg; and in the full account, which has yet to be published, I hope to be able to refer fully to the results of these observers, of which my own are in part a rediscovery, in part a supplement, and in part entirely independent.

I have, &c.,
J. A. Ewing.

The University, Tokio, Japan,
May 30, 1883.

ON DRY CHARGING-PILES. BY JULIUS ELSTER AND HANS GEITEL.

In Zamboni's piles phenomena can be observed which unequivocally indicate polarization of their plates. If the copper pole of such a pile be connected with the positive, and the tin pole with the negative discharger of a Holtz machine, the pile, on being again disconnected after only a few minutes' action of the machine, shows a considerably strengthened tension, which, even after repeated discharging, resumes almost equal intensity. Only after the lapse of some hours does the pile get back to its initial condition.

For experiments of this kind a form of the pile is adapted which is also in other respects to be recommended for the purpose of demonstration: the pairs of plates are strung, by means of a needle, upon a strong silk thread. In this form it can be easily placed between the dischargers of the machine, and disturbing influences, such as appear when a glass envelopment is employed, are avoided. A pile of 11,000 pairs of plates of 1 square centim. surface, after ten minutes' charging, gave sparks of about 1 millim. length, between two metal balls connected with the poles, and rendered a small Geissler tube at first continuously, afterwards intermittently luminous. After this it was to be expected that dry charging-piles could also be constructed of one metal only, according to the analogy of Planté's battery. Plates of lead foil were coated on both sides with tissue paper, which was made to adhere by means of potash waterglass to which a little oxide of lead was added. The plates were then cut into pieces of 1 centim. square, and, as above prescribed, strung upon a silk thread. A pile of 7000 such plates, between the poles of a Holtz machine, likewise assumed a very powerful polarization. The action was perhaps still more energetic than that of the before-described pile. In order that the experiment may be successful, it is requisite that the separating layers of paper have a certain degree of moisture, which will be produced spontaneously if the pile be left for 24 hours, under a glass bell, by the side of water.

The tension falls pretty quickly after charging; so that sparks of 1 millim. length can be obtained only within the first ten minutes; but even after 24 hours free electricity can still be distinctly demonstrated at the poles of the pile with the aid of a not particularly sensitive gold-leaf electroscope.

Here also it appears that the products electrolytically deposited on the plates by the current (in the present case superoxide of lead) act considerably more powerfully than when deposited in any other way. A pile consisting of 1000 pairs of lead plates coated on one side with chemically produced superoxide and on the other with protoxide of lead, gave in the electroscope a proportionally much feebler tension.

As the electromotive force of dry charging-piles never remains constant, but, in accordance with the nature of the thing, constantly diminishes from the beginning, they can never possess any practical importance. Nevertheless they are excellently adapted to illustrate to a large audience the principle of accumulators. The Holtz machine then represents the dynamo-electric machine, and the dry pile Planté's or Faure's accumulator: the tension-phenomena at the poles are so energetic that they can be seen from a great distance.—Wiedemann's *Annalen*, 1883, No. 7, pp. 489–491.

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[FIFTH SERIES.]

SEPTEMBER 1883.

XXV. *On the Admissibility of the Assumption of a Solar Electric Potential, and its Importance for the Explanation of Terrestrial Phenomena.* By WERNER SIEMENS*.

MY brother, Sir William Siemens, in his memoir "On the Conservation of Solar Energy," has set up the hypothesis that the sun possesses a high electric potential, which possibly produces the phenomenon of the zodiacal light. He accounts for the rise and maintenance of this electric potential by friction of the matter which, according to his theory, having been dissociated by the light- and heat-rays emitted from the sun, flows in from cosmical space to its polar regions. This would, after condensation had set in, again undergo combustion and then flow towards the sun's equator. In doing this it would be electrified by friction with the rotating body of the sun, and then be afresh diffused in cosmical space, in its electrified state, by the centrifugal force of the rotation.

If this much-controverted theory be admitted as correct, it follows that the phenomenon in question is really similar to the electrization (described by me†) of the apex of the pyramid of Cheops by the whirling dust-clouds of the desert. We could then assume that the body of the sun, supposed conductive and insulated by the sea of flame enveloping it, the

* Translated from a separate impression, communicated by Sir Charles Siemens, from the *Sitzungsberichte der königlich preussischen Akademie der Wissenschaften zu Berlin*, 1883, XXVI.

† Pogg. *Ann.* cix. p. 355, 1860.

photosphere, keeps one of the electricities which have been separated by the friction, while the other is spread by convection in cosmical space. As it must then be also assumed that this convection extends far beyond the orbits of the planets, the body of the sun must be regarded as having an electric potential with respect to these and acting distributively upon them. I will not enter further upon the controversy respecting the admissibility of the theory in question. I do not fail to perceive the weight of many of the reasons which have been brought to bear against it; but I am of the opinion that the possibility of the assumption of a solar electric potential, given by the theory, tells much in its favour, since some of the most important terrestrial phenomena would find in it an explanation, which has hitherto been sought in vain, and since, on the other hand, in the present position of physical knowledge the finding of any other explanation of the presence of a solar electric potential will scarcely be possible. For, up to the present time, no process is known to us in which only one electricity is called forth. We know only separations of the two electricities; and although, according to all expectation, with the intensely powerful mechanical and chemical actions which are set up at the surface of the sun's body, such separations take place on a very large scale, they must be again compensated by conduction within the same; and even if there were a lasting separation of the two electricities in the body of the sun, action at a distance of one of them could not possibly take place. So long, therefore, as no new and at present quite unknown facts appear, my brother's convection theory remains inseparable from the assumption of a solar electric potential.

I must not, however, omit briefly to go into the weightiest objection to this theory, urged against it by MM. Faye and Hirn. It is that the invariability of the periods of revolution of the planets about the sun logically excludes the admission that space is filled with matter—that astronomical observations unconditionally demand the assumption that cosmical space is an absolute vacuum, since, with the prodigious velocity of the motion of the planets, even the most highly rarefied atmosphere must bring about a measurable diminution of the planetary velocities, and consequently a shortening of their periods. This would be true, on the hypothesis that the atmosphere of cosmical space is at relative rest. But this cannot be the case if the circulation assumed by my brother really takes place. It must be assumed that the sun's atmosphere has approximately the same rotation-period as the body of the sun. Any difference in the velocities of rotation, brought about by the powerful ascending and descending currents in the sun's atmosphere which must arise from the combustion of the elements cooled by expansion and the cooling of the burnt outermost layers of the photosphere by radiation, will continually be equalized again by friction against each other of the parts of the sun's atmosphere rotating with different velocities. About the height of this atmosphere rotating equally with the sun's body nothing is yet known. According to Ritter's calculations *, indeed, the density of the solar atmosphere diminishes very quickly (following the sudden alteration of the adiabatic curve) in the region of the photosphere, in which the supply of heat from combustion considerably retards the diminution of temperature corresponding to the progressive rarefaction; but we do not yet know the limit of rarefaction up to which Mariotte and Gay-Lussac's law holds good. If, however, the atmosphere reaches the limit at which the force of attraction and the centrifugal force counterbalance each other, every material molecule passing beyond this must henceforth revolve round the sun like a planet. If new particles of the mass were constantly arriving at and entering this limit, a progressive condensation of matter would of necessity take place and a ring be formed here, which would rotate round the sun in accordance with Kepler's laws. Presupposing, however, the continuity of the sun's atmosphere, this ring-formation cannot occur, since the mutual friction of the strata of gas continues even beyond the surface of equilibrium, and consequently those which are already in planetary motion are subjected to an acceleration. The result must be that with the increase of velocity the distance of each of these microplanets from the sun is perpetually increasing; consequently the constant outflow from the solar atmosphere into cosmical space, assumed by my brother, must actually take place. And it can only take place in the zone of the sun's equator, since here at equal distance from the centre the centrifugal force is the greatest. It must also be admitted that the density of this atmosphere, everywhere rotating in accordance with Kepler's laws, in the plane of the sun's equator remains constant up to great distances from the sun, since the solar gravitation is throughout equilibrated by the velocity of revolution. In the directions perpendicular to that plane, on the contrary, the density must decrease, since the solar attraction diminishes as the distance from the plane of the sun's equator increases.

It follows from this consideration that a material current emanating from the sun coincident with the phenomenon of the

* Wied. *Ann.* v. p. 405, 1878.

zodiacal light must have everywhere the period of revolution that planets would have at the distance in question from the sun. A resistance experienced by planets from the material parts of interplanetary space, moving nearly uniformly with them round the sun, is therefore out of the question. Only a resistance (here negligible) must take place in consequence of the inclination of their ecliptic to the plane of the sun's equator, to which the observed lessening of this angle of inclination may perhaps be traced. The satellites, too, in revolving round their planets, must experience a resistance from the atmosphere of cosmical space; while the extreme limit of the atmosphere of the planets, rotating with them, must undergo a frictional resistance. With respect to the moon M. Hirn is perhaps right in maintaining that, the celestial bodies moving with such prodigious velocity, even the most rarefied resisting medium would sweep away their atmospheres.

Numerous observations make it highly probable that cosmical space, at least within the region of the solar system, is filled with combustible matter. This also indirectly tells very decidedly for my brother's hypothesis that the products of combustion, in a state of extreme rarefaction and at very low temperature, are again dissociated by the sun's rays. The objection which has been made, that the work of dissociation would absorb the energy of the luminous rays and thereby render cosmical space opaque, could be set aside by supposing that the work of dissociation is done by the invisible chemically acting rays only. But it can also be assumed that, in the course of the ages, the work of dissociation is already accomplished, and that now only the chemically combined mass continually emanating from the sun is still to be dissociated by his rays, for which only a part of the light-energy would be expended.

Without the hypothesis of dissociation it would be difficult to explain why cosmical space is not filled, like the earth's atmosphere, essentially with oxygen, nitrogen, and aqueous vapour. It cannot be assumed that the composition of the body of the sun is essentially different from that of the earth, if both have come out of the same rotating nebulous mass, since to suppose a separation of matter in the gaseous state according to specific gravity is inadmissible. Hence, at least in the solar system, the electronegative substances must everywhere predominate; and it is to be assumed that even the cold burnt-up sun in the future will be surrounded by an atmosphere containing an excess of oxygen. But if cosmical space is filled with highly rarefied dissociated products of combustion, these must become subject to the attraction of the sun wherever they are not, as in the vicinity of the equatorial plane, withdrawn from that attraction by planetary revolution. Hence a constant inflow of dissociated matter to the sun must take place, especially in the polar regions, where there is a total absence of centrifugal force, as my brother supposes. If the mass of the sun, as may well be assumed, remains invariably the same, this means that a state of equilibrium has entered, in which just as much burnt matter flows out from the sun's equatorial zone as is again conveyed by attraction, in a state of dissociation, to its polar regions. Then the flow from the pole to the equator, as well as the proved lower angular velocity of rotation of the gaseous mass of the sun in its higher latitudes, would consequently follow.

Although, however, this gives the possibility of the production of a solar electric potential by friction and continual removal from the sun of the parts charged with one of the electricities, yet the mechanism of this electrification still remains very obscure. The light of the sun proceeds from a sea of flame which, according to Ritter's beautiful calculations, must have a thickness of about 25 kilometres. Whether a flame of burning gases of this thickness will still transmit much of the heat- and light-rays from a hotter source of emission, how much of them it will absorb or, like a layer of cloud, will reflect, we cannot know. I have recently* shown that gases heated to from 1500° to 2000° C. still appear perfectly dark, while even at a lower temperature they emit the more slowly vibrating heat-rays. Whether gases become self-luminous on being raised to a much higher temperature has not yet been determined by experiment. As, however, a small flame in a brighter light casts a shadow, it seems unlikely that many of the light- and heat-rays from the deeper and hotter strata of the sun can traverse the huge photosphere. The observed temperature and light of the sun are then phenomena originating essentially in the chemical action that goes on in the solar atmosphere. This requires that that atmosphere ascending in a state of dissociation and at the same time cooled by increase of volume, shall begin to burn when the limit of the temperature of dissociation for the respective compound is passed, and that this combustion shall continue until the loss of heat by expansion is equal to the heat liberated by combustion. The apparent temperature of the sun will hence be approximately the dissociation-temperature of those compounds which have the highest chemical heat-equivalent, consequently of water, the elements of which will burn at the greatest altitude, while the masses which are heavier and at the same time

* *Sitzungsb. der Akad. der Wiss.*, Wied. *Ann.* xviii. pp. 311–316, 1883.

possess a higher temperature of dissociation are already ignited in lower regions. In order to keep going this upward motion of the dissociated elements, the final products of the combustion must return to the main body of the sun. As Faye, Ritter, and others have shown, this takes place, first, because those products possess greater specific gravity than the unburnt gases, and, secondly, in consequence of the cooling of the higher strata of the photosphere by radiation of heat and light. This disturbs the adiabatic equilibrium of the over- and underlying strata of gases, and compels the higher, having become relatively heavier, to return in descending currents to the solar depths. The reason that these descending currents become visible as sun-spots only in middle solar latitudes is that there only are the conditions present for a rotating motion of the descending current, by which a vertical direction is given to it. The funnel-shaped diminution of the diameter of a sun-spot is the result of the great diminution of volume effected by the rapidly increasing pressure. The interior of the funnel must be relatively dark, since there the formation of luminous flame fails, as the temperature must be lower by the amount of the heat of dissociation than that of the surrounding unburnt solar substance, and perhaps products of condensation already occur, which act as a screen in keeping back the radiation of the more brightly luminous deeper strata of the sun. On the other hand, it is not improbable that the high-blazing faculæ consist of bubbles of hydrogen and oxygen in the proportion to form explosive gas, or of coal-gas mixed in the right proportion with oxygen, which, in consequence of their less specific gravity and greater liberation of heat in combustion, bursting through the penumbra and the photosphere, soar aloft, and, the elements of luminous flame being absent, transmit in part the rays of the hotter deeper layers of the sun. The enormous velocity of the coruscations of many faculæ, scarcely admissible as a mechanical effect, might then find its explanation in this radiation from the solar depths. My brother, in a recently published supplement to his theory of the sun, assumes that the main body of the sun itself may not be hotter than about 3000° C., since at a higher temperature the chemical rays would be the predominant ones, and at a very much higher temperature the sun would actually cease to give light. This might be true if the photosphere did not as a screen keep back the hotter rays of the sun's body, as it probably does. In fact, from analogies to the observations we cannot draw any safe conclusion as to whether a body heated to hundreds of thousands or millions of degrees would still be luminous. Only rays of so minute a wave-length might emanate from it that they would be incapable of performing any chemical work! The apparently dark nucleus of the sun-spots might then be accounted for thus:—The flameless products of combustion, relatively cooled by commencing dissociation, returning to the sun, remain transparent and permit the deeper layers of the sun, too highly heated for luminous radiation, to radiate through them. The violet colour of the nuclei of the sun-spots would tell in favour of this. For attainable temperatures, it is true, the law holds good that, besides the rapid undulations of the æther corresponding to the higher temperature, the entire scale of the slower undulations comes in also; but whether it is or is not different with temperatures so enormously higher cannot, at all events, be known.

It was necessary to enter somewhat more into particulars respecting the constitution of the main body of the sun and its light- and heat-radiating envelope, in order to get a foundation for an answer to the question whether, with the present extent of our knowledge, the hypothesis of a solar electric potential appears admissible. As I have already insisted, its conception is only possible if a separation of the two electricities goes on at the sun's surface and if one of the separated electricities is simultaneously conducted away. As flame is a good conductor of electricity, the entire photosphere and the penúmbra (which probably take part in the process of combustion) may be regarded as a conducting mantle enveloping the hotter body of the sun. As, further, flames have, like points, the property of transmitting electricity to their surroundings (here, therefore, to their gaseous combustion-products), the photosphere must be continually discharged by a partial outflow of the products of combustion into cosmical space. If, therefore, the photosphere were insulated from the deeper body of the sun not yet included in the combustion, the latter, if it should be regarded as a conductor of electricity, could be charged with electricity by friction or chemical processes taking place between the conducting body of the sun and the photosphere. The question whether hot gases are conductors of electricity, even when no flames appear in them, has not yet been decided by direct experiments. That gases, like all bodies, become conductors of electricity when the dielectric polarization of their molecules has reached its maximum, and that this maximum diminishes proportionally with the rarefaction of the gases, consequently also with their heating reckoned from absolute zero, I have already shown, when describing my ozone-apparatus, in the year 1857*. Accordingly conductors are

* Pogg. *Ann.* cii. p. 66.

distinguished from nonconductors only by the maximum of polarization of the former being vanishingly small. That in very highly heated gases the maximum of polarization would become, as in metallic conductors, vanishingly small, can scarcely be admitted. Direct experiments on the dielectric properties of highly heated flameless gases are not known to me; but the phenomena of the electric spark, as well as the luminous appearances in the ozone-apparatus and Geissler tubes, and the beautiful experiments of Hittorf *, can be explained even without assuming that highly heated conduct differently from cold gases of equal density. Hence the high temperature of the solar gases appears at present to be no obstacle to ascribing to them insulating properties. Indeed their maximum of polarization will, in correspondence with the density of the sun's atmosphere, be greater than that of our cold atmospheric air, notwithstanding their elevated temperature.

Very different relations, however, may appear on the appearance of the critical state at greater depths in the sun. For the electrical property of the critical state we have neither experiments nor analogies: hence we can assume that the interior of the sun is also a metallically conducting mass—that is, having a vanishingly small polarization-maximum. The surface of this mass of the sun in the critical state might then have an electric potential. The question, however, would have to be taken into consideration whether the conducting photosphere might not on the face turned towards the interior of the sun become electric by distribution, so that the sun with its enveloping photosphere would form a

* M. Hittorf, in a communication in vol. xix. of Wiedemann's *Annalen*, p. 73, says that what I communicated to the Academy on the 9th November, 1882, viz. that gases at temperatures from 1500° to 2000° C. still appear perfectly dark if they are quite flameless, and that the luminosity of gases on the passing through them of an electric current is a similar process to the shining of a flame that separates no solid components, had been previously made known by himself and others. I willingly grant this with respect to the nonluminosity of hot gases; but I made no claim to priority in this communication: I believe, however, that I first proved by experiment that gases so highly heated actually appear perfectly dark, although the hot layer of air be over a metre thick and the eye has been rendered in the highest degree sensitive by complete darkness. Hittorf's experiments proved only that hot gases are relatively dark. As to the conductivity of gases, assumed by Faraday for high tensions, in my paper above cited, about 25 years older, I already set up the general law according to which the conductivity of gases commences. To this I might also refer M. Eilhard Wiedemann, who claims priority for his explanation of gases becoming luminous on an electric current passing through them as a result of dielectric polarization.

prodigiously powerful Leyden jar, by which action at a distance of the electricity of the conducting nucleus of the sun would be in a great measure excluded. This cannot be assumed at once, since the conductivity of flame has quite different causes, directly connected with the combustion-process itself, than that of conductive bodies not in chemical action; so that an analogy between the two is hardly to be inferred with respect to their capacity for electric distribution. I have therefore made some experiments on the question whether a flame is subject to the action of induction in the same manner as other conductors; and these experiments have confirmed the hypothesis. According to this, two flames insulated from each other can act as the coatings of a charged Leyden jar, in like manner as other conductors*. According to this it must be assumed that the seat of the sun's electricity is chiefly to be sought in the photosphere, and not in the body itself of the sun. The electric properties of flame are still very obscure, notwithstanding all the experiments hitherto made on the subject. In particular, it is not yet decisively determined whether a difference of potentials, spontaneously produced, between the different zones of the flame, especially between that where the combustion commences and that where it goes out, exists or not. If this were the case, as appears probable from some experiments by Riess and others, the cause of the sun's electricity might be sought therein, considering the prodigious dimensions of the sea of flame

* The experiment was made thus:—A ring-shaped gas-burner was insulated. On opening the cock a cylindrical flame of about 2 centim. diameter rose above it to the height of about 15 centim. The flame passed through a metallic cylinder of about 8 centim. diameter, placed so as to be insulated and to surround it concentrically. To produce conducting connexion with the flame, an insulated platinum wire, bent into a circular shape, was placed in the lower part of the flame. The charge produced between this platinum wire and the cylinder by a galvanic series of fifty Daniell cells was now measured, by means of my rapidly oscillating electromagnetic switch, with the gascock alternately nearly closed and quite open. The difference between the deflections of the mirror-galvanometer was then a measure of the capacity of the Leyden jar formed by the flame and the cylinder. The results obtained are collected in the following table:—

Number of oscillations of the switch per minute.	Difference of deflection between low and high flame.	Amount of a discharge.
310	3	96
600	6	100
700	8	115
1000	12	120

The increasing numbers of the last column show that with slow oscillations a part of the charge was lost by conduction.

surrounding the sun, and the corresponding great differences in temperature and density, since the electricity of the outer layers of the photosphere would then pass over to the products of combustion, and with them, according to my brother's theory, would in part be spread in the direction of the plane of rotation of the sun in cosmical space. Even supposing, however, that the process of electrification may have to be sought in the solar combustion itself, in the friction of the matter flowing in from cosmical space, or in other causes yet unknown, the possibility of the existence of an electric potential of the sun is given by the equatorial diffusion of solar products of combustion in cosmical space.

And this possibility rises to the rank of high probability when one considers the facility with which some difficult and hitherto unsolved problems of terrestrial phenomena can be solved with the aid of a solar electric potential. If the sun possesses a high electric potential, he must act distributively upon all the celestial bodies, consequently upon the earth also. But an accumulation of electricity upon the entire solar surface can only take place when the opposite electricity liberated is conducted away; and the only conceivable way in which it can be so conducted is by being diffused in cosmical space. The process is approximately the same as that which takes place when an insulated sphere is placed opposite to a larger charged spherical conductor. The sphere then gradually takes an opposite charge, while the same electricity is lost by dissipation in space. With the earth this dissipation of the so-called free electricity arising from the sun's distribution is, moreover, greatly favoured by the extreme rarefaction of the upper strata of air and by the ascending and descending currents of air loaded with moisture, since by these the free electricity is conveyed to the upper strata of highly rarefied air. That in this rarefied upper air electric currents take place is proved by the auroræ boreales and australes. They might be regarded as the electrical compensation taking place at the boundary of the earth's atmosphere, between the matter flowing out from the sun charged with negative electricity and the liberated positive induced electricity of the earth. This compensation must occur whenever by a change in the sun's potential that of the earth is also changed. For the restoration of equilibrium, positive or negative electricity must then flow out from the earth; consequently either a compensation must take place at the boundary of the atmosphere with the negative electricity flowing out from the sun, or this must flow to the earth. The reason that this exchange takes place preeminently in the earth's polar regions may be that the polar air is more strongly electric, as it is continually displaced by the more strongly electrified air brought by the equatorial current in the upper regions of the atmosphere, and must therefore receive in its entire mass the electricity of the highest strata of air of the lower latitudes. Earth-currents, standing in intimate connexion with the northern and southern lights, are then to be considered a necessary consequence of the compensation of the variations of intensity of the electricity of the earth and the sun taking place preeminently in the polar regions. These compensation-currents must on their part affect the magnetic needle by their electrodynamic action.

Here, however, the question intrudes, is not the earth's magnetism itself to be considered an electrodynamic action of the electrical charge of the earth? According to the beautiful investigation instituted in Helmholtz's laboratory, under his direction, by Mr. Rowland*, it is to be regarded as demonstrated that stationary electricity mechanically moved exerts electrodynamic actions in a similar manner as an electric current. Accordingly the earth must, if its surface is charged with electricity of great density, exhibit, in consequence of its rotation, magnetic phenomena, in like manner as if electric currents encircled it, carrying round it in each latitude, during the time of a rotation, just so much electricity as the static electricity present on the respective surface-rings amounts to. What density of electricity on the earth's surface would be required, in order to produce the magnetism of the earth by its rotation, will not be difficult to practised mathematicians to calculate. As the magnetic moment of a circular current is proportional to the surface flowed round, we may foresee that, considering the dimensions of the earth, that density will not prove to be inadmissibly great. Further, with the colossal dimensions of the sun, whose surface contains 11,483 times the surface of the earth, while the distance of the sun amounts to only 22,934 earth's semidiameters, the density of the sun's electricity need be only about twice that of the earth in order to call forth the latter by electric distribution. Were the whole of the earth's surface equally charged with electricity, the magnetic pole would necessarily coincide with the pole of rotation of the earth. Since it is not so, and as, on the whole, great irregularities take place in the distribution of the earth's magnetism over its surface, the distribution of the static electricity on that surface must be irregular. This also appears probable when it is remembered that about one third of the earth's surface consists of

* *Monatsberichte der Berliner Akademie der Wissenschaften.*

continent, which mostly has a rocky bottom only thinly covered with badly conducting earth. The accumulation of the induced electricity will therefore have to be sought rather on the surface of the incandescent well-conducting interior of the earth, by the greater distance of which from the outer surface the preponderant influence of the masses of electricity in convective motion lying nearest is diminished. Whether it will be possible to deduce the existing distribution of the earth's magnetism, as well as the observed periodic and irregular disturbances of it, from this theory of the cause of the phenomenon of terrestrial magnetism, must be decided by subsequent special investigation. The daily regular disturbances might be accounted for by the fact that the density of the induced electricity on the side turned away from must be somewhat less than that on the side turned towards the sun. This unequal density of the earth's electricity depending on the position of the sun, must proceed *pari passu* with the rotation of the earth, and may therefore be the cause of the regular equatorial earth-currents discussed by Lamont. The magnetic disturbances produced by the moon may likewise find their explanation in the reaction of lunar electricity upon the distribution of the induced electricity of the earth. On the other hand, the secular alteration of the situation of the magnetic pole can, in all probability, only be referred to yet unrecognized cosmic causes.

Although this theory may still leave much unexplained, it yet affords at least the possibility of giving an explanation of the origination of the earth's magnetism fitting in with our present experience. This is not the case with any previous theory. The hypothesis of a central magnet in the interior of the earth is contradicted by the universal experience that a red heat destroys the magnetism of all bodies. Hence that hypothesis cannot be maintained without entirely losing sight of the foundation of experience. The hypothesis of a stratum of magnetic ore in the crust of the earth as the seat of terrestrial magnetism is contradicted, in the first place, by calculation, since the magnetism of such a stratum, even if it be supposed of the greatest possible thickness and magnetized to the maximum, would not suffice to produce the existing terrestrial magnetism; and, secondly, by the impossibility of finding a cause for the magnetization of that stratum of ore, since the magnetism cannot have been present from the beginning, but must have first arisen after the cooling of the earth.

The same might be maintained of the theory advanced after Faraday's discovery of the magnetic properties of the oxygen of the air, that this oxygen is the seat of the earth's magnetism, if calculation had not already shown that the seat of this magnetism cannot be outside of the surface of the earth. Just as little can those theories of the earth's magnetism meet with consideration which rest upon thermoelectric currents or, as Zöllner attempted to base his, on convection-currents in the liquid interior of the earth, since in a medium conducting equally well in all directions such currents can never take place. Moreover no cause can be found for the existence of permanent regular currents of the liquid interior of the earth.

In like manner as an electric potential of the sun affords the possibility of accounting for the earth's magnetism, together with the related phenomena of the auroræ and earth-currents, it also gives a handle for the explanation of the electricity of the air and the phenomena of thunder-storms. That the earth must be charged with negative electricity was assumed already by Lamont for the explanation of the perpetually changing atmospheric electricity. His view, however, that this electric charge is to be accounted for by thermoelectric differences, is no more tenable than the view that friction-processes can generate an electric potential of the earth. Such a potential can only arise from cosmic influence and the removal of the liberated same electricity by diffusion in space, or its neutralization by the oppositely charged matter which flows out from the sun in the direction of the plane of his equator. If, however, we assume this to be the case, that consequently the earth together with the sun forms an electric accumulation-apparatus the separating dielectric of which is the atmosphere of the sun and of the earth and the interplanetary space filled with most highly rarefied matter, all the conclusions drawn by Lamont and others from the electric charge of the earth are justified. But to account for the electricity of thunder-storms, the trifling and varying electricity of the atmosphere, to which it has hitherto been attributed, does not seem sufficient. The sudden appearance of such vast masses of electricity as arrive especially in tropical storms repels the supposition that they have had their seat in the feeble electric charge of the comparatively small quantity of air that carries the thunder-clouds. The sources from which it springs must be more productive. Such a source, of inexhaustible vastness, is found in the electric charging of the earth by solar influence. When a conducting object is brought near a large sphere charged with electricity, it is subjected to the distributive action of the electricity existing on the surface of the sphere.

If the similar electricity accumulated in the part of the conductor most distant from the sphere finds a means of transmission to still more distant conductors, the first conductor becomes lastingly charged with electricity the polarity of which is the opposite of that of the sphere. But if the elevation of the conductor above the surface is but little in proportion to the diameter of the sphere, then the difference of tension between the surface of the sphere and the most distant point of the elevation can only be small. For this reason, even with a great density of the electricity on the surface of the earth no electrical repulsion can take place there, and even on mountain-tops it cannot be very noticeable. But the ratio takes another form when a sphere is charged by induction from a distant electrical sphere. The lines of force which, according to Faraday's molecular-distribution theory, go from the charging to the charged sphere from which the electricity is carried away, meet the latter everywhere almost perpendicularly and, with a great distance between the influencing spheres acting upon each other in proportion to the diameters of the spheres, in almost equal number on the side turned towards the distributing sphere and on that turned away from it. If now an insulated conductive screen that covers a part of the surface of the influenced sphere be brought near the latter, if it be thin it will not become perceptibly electric. But as soon as the screen is touched so as to conduct away its electricity, it takes the opposite electricity to that of the sphere, while the like electricity is carried away. The behaviour is the reverse of this when the screen is conductively connected with the sphere; the screen then forms part of the surface of the sphere and receives its electric charge, while the part of the surface under the screen becomes nonelectric. Now the thunder-clouds appear in the character of such screens upon the surface of the earth. If such a screen of cloud be imagined included in the formation over a part of the earth's surface, it will remain unaffected by the earth's electricity so long as the conductive particles of water are insulated and at a considerable distance from one another. Hence mist and light clouds will not become electric. But as soon as the mist has so far condensed that its conductive parts come into contact with one another, or the distance between them becomes so small that electricity of very slight tension can overleap the intervals, the cloud is subjected to the distribution process. This can be initiated by its being put into conducting connexion with clouds situated in very elevated regions by ascending cloud vortices. This conductively connected cloud is then electrified in its lower part with the contrary electricity to that of the earth, while the upper part is electrified similarly to the latter. But a dense conductive bank of clouds may in one or more places come into conductive connexion with the earth itself. It then forms a part of the earth's conductive surface, and takes up its electricity*. The latter process most readily occurs at the declivity of steep mountains against which the layers of cloud rest. Hence mountains frequently occasion tempests. In the generation of the clouds that carry the electricity of storms electricity does

* During a voyage on the Mediterranean in the winter of 1865, in the vicinity of the Spanish coast, between Cartagena and Almeria I had an opportunity of observing the course of the phenomenon of a waterspout, which appears to me to tell decidedly in favour of this conception.

Between the ship and the coast in the vicinity of Almeria, with a vigorously agitated so-called dead sea, without any considerable motion of the air, a dense but apparently not high bank of black clouds was seen, under which the sea seemed to be in the wildest commotion. It appeared there like a roundish white spot, the diameter of which the seamen estimated at from two to three [German] nautical miles, foaming up to a great height, while the sea around it showed only smooth waves without any breakers. In spite of the considerable distance of the ship from the place of violent agitation, amounting to several leagues, it could be distinctly seen through the telescope that the angry surges rose several metres above the sharply defined surface of the relatively calm mirror of the sea. The cloud descended at one place in the shape of a funnel, forming a streak of cloud curved like an elephant's trunk, reaching down nearly to the foaming surface of the sea, and apparently joining below. Perfect contact with the foaming surface could not be perceived; and, what was surprising, no greater foaming took place under the trunk-shaped cloud than in other places. The trunk itself slowly rotated, if I remember rightly, in the direction of the hands of a watch, over the white spot; and its place of junction with the cloud took part in the motion, though not to an equal extent. Unfortunately, night coming on and the increasing distance, after about half an hour's observation, during which the trunk had made a turn and a half, keeping its point constantly at about one third of the radius of the white spot from its margin, deprived us of the further contemplation of this interesting phenomenon, which had been followed with the closest attention by myself, my brother William and his wife, and the naval officers belonging to the company of the French cable-ship, on board of which we were. No whirling motion was perceptible. Almost a dead calm prevailed. It can only have been a purely electrical phenomenon, which must have consisted in an electric current from the earth to the cloud. If we assume that this current had at one place become so strong that, by electric conveyance of the liquid, a conductive water communication between the sea and the cloud was formed, the rotation of the trunk under the influence of the earth's magnetism is also explained. During the night a storm raged on the Spanish coast, which probably originated in the waterspout observed by us. The latter, however, appeared later to have directed its course from the Spanish to the African coast; for towards the end of the night our ship near the latter coast encountered so fearful a storm of only a few minutes' duration that it was in the greatest peril, and the seamen were firmly of opinion that the waterspout had passed over the ship.

not appear to play any essential part. The cause of the formation of clouds is, as a rule, to be sought in the ascending and descending motion of the air, to which not only this and the rain falling from the clouds, but also the letting loose of storms is almost exclusively to be ascribed. The views on this subject which still prevail to a great extent in meteorology need, in my opinion, correction in some points. Were the equilibrium of the aerial ocean not constantly disturbed by unequal heating and cooling of the air by radiation, the temperature and density of the air could not but be in so-called indifferent equilibrium up to the greatest altitude, and in such wise that the loss of temperature with increasing height would be everywhere equivalent to the work of expansion of the gas. The higher temperature of the air of the lower latitudes would be equalized by slowly coursing whirlwinds with a horizontal axis of rotation, as is shown on a large scale by the trade winds, and finally the entire sea of air would possess equal temperature at equal hights. This indifferent or adiabatic equilibrium is now continually disturbed by extra heating of the earth's surface and of the lower strata of the air by solar radiation, by absorption of the same on passing through the atmosphere, and by the extra cooling of the higher strata by radiation outwards. Through this the lower strata become lighter and the upper heavier than the adiabatic equilibrium requires; and this disturbance must be compensated by ascending and descending currents in the atmosphere. As the ascending air, which has become warmer on the ground, in correspondence with the adiabatic curve of temperature preserves this excess of heat on ascending, but the upward impulse increases with the increasing height of the ascending current because the succeeding layers of air at the ground have always the same excess of temperature, the upcurrent must continue, in the places where it has once been produced by favourable local conditions, until the difference of temperatures is equalized. The work done by the upcurrent of the relatively lighter air and the descent in other places of the relatively heavier air cooled by radiation must be converted into *vis viva*, as it puts the air into quicker motion. This is effected essentially by the volume of the ascending air being increased by the diminution of pressure. Since, the diameter of the terrestrial globe being great, the air-space becomes only imperceptibly larger with the height, the velocity of the ascending air must even from this cause increase nearly proportionally to the decrease of pressure. In the highest regions of the air to which each upcurrent formed will reach, the velocity of the air must therefore be very considerable; and there with the same velocity the surrounding calm air must be pushed aside in order to make room for that which has arrived. This displacement will take place chiefly in the direction to where a descending current has been formed to replace the overheated air flowing at the surface of the earth to the place of the upcurrent. This downflowing air now becomes denser again correspondingly to its altitude, it is true; *but at the same time it retains the velocity it acquired in the upper regions.* It is evident that the final result may be a very great velocity of air at the earth's surface, if the disturbance of the adiabatic equilibrium was qualitatively and quantitatively considerable. These local storms, the direction of which is modified according to Dove's law of rotation by the rotation of the earth, must become peculiarly violent if the upcurrent itself is confined within narrow limits, since then the compensating process, *i. e.* the conversion of the energy accumulated in the disturbance of equilibrium into air-velocity, is confined to a proportionately small quantity of air. Yet powerful storms, passing over whole continents, may also be produced by ascending currents of air of wide local extent. That the descending current produces an increase of pressure upon the ground, and the ascending current a decrease, follows from the laws of mechanical motion. The mere motion of the air, however, must of itself always occasion a fall of the barometer, since the moved air carries away with it the still air at the contact boundary and consequently produces a rarefaction. The final result of the compensation of the disturbance will therefore be to put greater and greater masses of air into whirling motion, and last of all to bring the *vis viva* back, by friction, into the form of heat.

It follows from these considerations that the aqueous vapour in the air does not play the great part in the movement of the air that is usually attributed to it, since the phenomena of the motion and pressure of the air can be accounted for without the water contained in the air. The origin of storms, *i. e.* in this case the place of the acceleration of the masses of air, must only be sought, not at the surface of the earth, but essentially in the highest regions of the air. If the atmosphere consisted of aqueous vapour only, the phenomena would be altogether similar. Aqueous vapour is as subject to the law of adiabatic expansion as air; only its density and temperature decrease with increasing height much less than those of the permanent gases of the atmosphere. According to Ritter a vapour atmosphere would be about thirteen times as high as an atmosphere of air. It is true that, according to Clausius and Sir William Thomson, with the adiabatic expansion of vapour a continual condensation takes place; but, at the heights in which according to experience the formation of clouds occurs, it must still be too inconsiderable to bring about the observed precipitation. The reason of the condensation that takes place in ascending air-currents lies essentially in this—that the aqueous vapour is intimately mixed with the air, and that in the ascending current it does not take the adiabatic temperature belonging to itself, but that of the greatly preponderating mass of air with which it is mixed. Now, as the air is cooled much more quickly with increasing height than the vapour, the latter is cooled below the adiabatic temperature belonging to it; and this diminution of temperature gives rise to its condensation if the point of saturation of the vapour is overpassed.

This conception is apparently contradicted by the circumstance that aeronauts have repeatedly proved that strata of warmer air frequently overlie colder ones, while the law of adiabatic expansion requires a continuous decrease of pressure and temperature. But this is easily explained by the dissimilar constitution of the earth's surface, owing to which the ascending current frequently, and in many places, has a much higher temperature and contains a far greater quantity of aqueous vapour than in others. If the amount of water held by such a hot ascending mass of air is so great that part of it is separated during the ascent and falls as rain, the air met with in the upper strata of the atmosphere is still further heated by taking up the latent heat of the aqueous vapour; and thereby its volume and upward impulse are augmented; and the final result must be a stratum of relatively warm and comparatively waterless air, which is then pushed by its expansion over colder air, but which, from containing more aqueous vapour, is lighter *. These departures from the rule that the temperature and density of the atmosphere decrease as the height increases, while the contained water must increase, are easily accounted for. The latter must be the rule for the higher latitudes at least, since the masses of warm air containing relatively much water continually ascending in the calms, on their way to higher latitudes, after loss of their greater heat by radiation, it is true for the most part sink to earth again as descending currents; but they must also in part reach the high latitudes as an upper equatorial current.

* Krönig has already proved that the aqueous vapour mixed with an ascending current produces by its condensation no diminution, but an augmentation of volume, as the latent heat of the vapour enlarges the volume of the air by a much greater amount than the volume of the condensed vapour before its condensation (*Fortschr. d. Phys.* xx. p. 626).

In this greater humidity of the higher strata of the atmosphere is to be sought the reason why even with descending currents of air falls of rain may occur. If the temperature of a very moist upper current be cooled by radiation below the point of saturation of the vapour, cirrus clouds will be formed, probably consisting of ice crystals *. The latent heat of the vapour and water thereby set free will again heat these strata of the air and protract the process of formation of heavier snow-clouds; but if by continued loss of heat by radiation the process be completed, the weight of the ice, no longer filling any considerable space, must disturb the adiabatic equilibrium, and a sinking of the mass of cloud commence. The condensation and heating then taking place melt the snow again; and the requisite latent heat is withdrawn from the air. The adiabatic equilibrium is hence still further disturbed; and the final result will be a cold descending current with rain. The density of these slowly descending rain-clouds, however, will not be great enough to make the cloud electrically conductive; consequently there will be no formation of electricity by distribution. The course of things, however, will be different when by local overheating of the air in the vicinity of the ground a local upcurrent with rainfall is produced. The upcurrent may then acquire a velocity greater than that of the falling drops formed in the resisting air; hence these will be whirled with the current into the upper regions, the temperature of which lies far below the freezing-point, and frozen to hailstones. By the rapid increase of volume, and the corresponding lateral expansion of the accelerated current, the next higher strata of air, which are relatively moist and cold, are set whirling with a horizontal axis of rotation; and these whirlwinds combine with that which is ascending and rotating about a vertical axis. The violent whirling motion into which

* It is extremely probable, however, that in the high regions of the atmosphere both water and vapour retain their state of aggregation down to far below the temperature of their points of freezing and condensation respectively. That water without the presence of solid bodies to induce crystallization, and without violent agitation, can be cooled far below $-20°$ C. without freezing is a fact. That steam in like manner can retain the form of vapour below its point of condensation has not yet been proved experimentally. We know only the retardation of boiling, which so frequently gives rise to steam-boiler explosions. It is at all events not improbable that this retardation of boiling also stands opposed to a retardation of condensation. This can only with difficulty be ascertained by experiments, since means are wanting to cool a mass of steam out of contact with solid or liquid bodies. Without assuming this it cannot well be explained why the sky is not always entirely covered with cirrus clouds; for it would have to be assumed that water particles liquefied in the great rarefaction of the higher strata of air do not appear as clouds.

the hitherto calm overcooled aerial sea is thrown will now induce in it a sudden formation of water and ice. The whirlings with horizontal axis of rotation may at the same time acquire a great diameter and hurl up the grains of ice repeatedly into the ice-region, until they have grown too heavy and fall to the ground as hailstones or, after passing through lower warm air, as cold drops of rain. By this copious formation of rain in a short time the water particles of the path of cloud are brought so close together up to the highest air-strata, that it becomes a conductor of electricity and is consequently exposed to electrical distribution. If at any place it is in conductive connexion with the earth, the earth's electricity must flow into it; and it then receives the same electricity. If it is not so, it becomes charged in the vicinity of the earth with the contrary electricity, while the similar electricity escapes by the conductive vortex-cloud into the higher regions. Where the conduction of the cloud is imperfect, it is temporarily restored by lightning-flashes springing between the layers of cloud insulated from one another, or between cloud and earth; and finally, on the whirling storm passing away, and the cloud formed by it breaking up, the whole of the electricity will again equalize itself with the earth's electricity by flashes of lightning, or will in part pass into the air as atmospheric electricity.

Many observations of the formation of thunder-storms have been made from the summits of high mountains or from balloons; and almost all of the observers have spoken of several layers of clouds one above another, either joined together or between which flashes of lightning sprang. The most instructive description is that given by M. Wite*, who observed from a balloon the rise of a heavy thunder-storm. He saw "two layers of cloud, one about 2000 feet above the other, of which the upper one sent snow, rain, and hail to the lower. *Between the two passed noiseless undulating masses of yellowish light.* Electric discharges, with lightning and thunder, occurred always in the lower layer; yet the thunder-storm was far more violent *above both layers* than below them. The upper layer was strongly agitated by a west wind." That the observer could see only two clouds, one above the other, is explicable, since his balloon was at the altitude of the interval. It is to be presumed that several more such layers of cloud, up to the highest region of the atmosphere, were present, between which the observed precipitation and processes of electric conduction took place. By the heavy rain falling from the upper cloud layer, especially from its middle, the two were conduc-

* *Fortschr. der Physik,* 1852, p. 762.

tively connected, and thereby submitted to the process of electric distribution.

To the theory of the sun's electric potential might still be opposed the objection that the electric attraction between the sun and the planets, and the repulsion which the latter would necessarily exert upon one another and upon their satellites, would modify the basis of the astronomical calculations, since then, besides gravitation, an additional force, the electrical, would have to be taken into account.

This objection is perfectly legitimate. But as electric force, equally with gravitation, stands in the ratio of the square of the distance of the centres, the paths of the planets would remain unaltered if a part of the gravitational were replaced by an electrical attraction. Only the calculated ratio of the masses of the sun and planets to that of the earth would be changed. These alterations would be sensible, especially in the case of the small planets and the satellites, since electric force is a function of the surface. On the other hand, however, the disturbing influences exerted by the planets and their satellites upon one another's paths must be changed if gravitation be diminished by electric repulsion.

Perhaps it is reserved for astronomy to bring out from the perturbations of the paths of Mercury, the asteroids, and the satellites the demonstration of the existence or nonexistence of an electric potential of the sun.

XXVI. *On Porous Bodies in relation to Sound. By* Lord Rayleigh, *D.C.L., F.R.S., Cavendish Professor of Physics in the University of Cambridge**.

In Acoustics we have sometimes to consider the incidence of aerial waves upon porous bodies, in whose interstices some sort of aerial continuity is preserved. Tyndall has shown that in many cases sound penetrates such bodies, *e. g.* thick pieces of felt, more freely than would have been expected, though it is reflected from quite thin layers of continuous solid matter. On the other hand, a hay-stack seems to form a very perfect obstacle. It is probable that porous walls give a diminished reflection, so that within a building so bounded resonance is less prolonged than would otherwise be the case.

When we inquire into the matter mechanically, it is evident that sound is not destroyed by obstacles as such. In the absence of dissipative forces, what is not transmitted must be reflected. Destruction depends upon viscosity and upon

* Communicated by the Author.

conduction of heat; but the influence of these is enormously augmented by the contact of solid matter exposing a large surface. At such a surface the tangential as well as the normal motion is hindered, and a passage of heat to and fro takes place, as the neighbouring air is heated and cooled during its condensations and rarefactions. With such rapidity of alternations as we are concerned with in the case of audible sounds, these influences extend to only a very thin layer of the air and of the solid, and are thus greatly favoured by attenuation of the masses.

I have thought that it might be interesting to consider a little more definitely a problem sufficiently representative of that of a porous wall, in order to get a better idea of the magnitudes of the effects to be expected. We may conceive an otherwise continuous wall, presenting a flat face, to be perforated by a great number of similar small channels, uniformly distributed, and bounded by surfaces everywhere perpendicular to the face. If the channels be sufficiently numerous, the transition from simple plane waves outside to the state of aerial vibration corresponding to the interior of a channel of infinite length, occupies a space which is small relative to the wave-length of the vibration, and then the connexion between the condition of things inside and outside admits of simple expression.

Considering first the interior of one of the channels, and taking the axis of x parallel to the axis of the channel, we suppose that as functions of x the velocity-components u, v, w, and the condensation s are proportional to $e^{i\kappa x}$, while as functions of t everything is proportional to e^{int}, n being real. The relationship between κ and n depends on the nature of the gas and upon the size and form of the channel, and must be found in each case by a special investigation. Supposing it known for the present, we will go on to show how the problem of reflection is to be dealt with.

For this purpose consider the equation of continuity as integrated over the cross section of the channel σ. Since the walls are impenetrable,

$$\frac{d}{dt}\iint s\, d\sigma + \frac{d}{dx}\iint u\, d\sigma = 0,$$

so that

$$n\iint s\, d\sigma + \kappa\iint u\, d\sigma = 0. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

This result is applicable at points distant from the open end more than several diameters of the channel.

Taking now the origin of x at the face of the wall, we have to form corresponding expressions for the waves outside; and we may here neglect the effects of friction and heat-conduction. If a be the velocity of sound in the open, and $\kappa_0 = n/a$, we may write

$$s = (\quad e^{i\kappa_0 x} + \mathrm{B}e^{-i\kappa_0 x})e^{int}, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$u = a(-e^{i\kappa_0 x} + \mathrm{B}e^{-i\kappa_0 x})e^{int}; \quad . \quad . \quad . \quad . \quad . \quad (3)$$

so that the incident wave is

$$s = e^{i(nt+\kappa_0 x)}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

or, on throwing away the imaginary part,

$$s = \cos(nt + \kappa_0 x). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

These expressions are applicable when x exceeds a moderate multiple of the distance between the channels. Close up to the face the motion will be more complicated; but we have no need to investigate it in detail. The ratio of u and s at a place near the wall is given with sufficient accuracy by putting $x=0$ in (2) and (3),

$$\frac{u}{s} = \frac{a(-1+\mathrm{B})}{1+\mathrm{B}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

We now assume that a region about $x=0$, on one side of which (6) is applicable and on the other side of which (1) is applicable, may be taken so small relatively to the wave-length that the mean pressures are sensibly the same at the two boundaries, and that the flow into the region at the one boundary is sensibly equal to the flow out of the region at the other boundary. The equality of flow does not imply an equality of mean velocities, since the areas concerned are different. The mean velocities will be inversely proportional to the corresponding areas—that is, in the ratio $\sigma : \sigma + \sigma'$, if σ' denote the area of the unperforated part of the wall corresponding to each channel. By (1) and (6) the connexion between the inside and outside motion is expressed by

$$-\frac{n}{\kappa}\sigma = \frac{(\mathrm{B}-1)a}{\mathrm{B}+1}(\sigma + \sigma').$$

We will denote the ratio of the unperforated to the perforated parts of the wall by g, so that $g = \sigma'/\sigma$. Thus,

$$\frac{1-\mathrm{B}}{1+\mathrm{B}} = \frac{\kappa_0}{\kappa(1+g)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

If $g=0$, $\kappa=\kappa_0$, there is no reflection; if there are no perforations, $g=\infty$, and then $\mathrm{B}=1$, signifying a complete reflection. In place of (7) we may write

$$\mathrm{B} = \frac{\kappa(1+g) - \kappa_0}{\kappa(1+g) + \kappa_0}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

which is the solution of the problem proposed. It is understood that waves which have once entered the wall do not return. When dissipative forces act, this condition may always be satisfied by supposing the channels long enough. The necessary length of channel, or thickness of wall, will depend upon the properties of the gas and upon the size and shape of the channels.

Even in the absence of dissipative forces there must be reflection, except in the extreme case $g=0$. Putting $\kappa=\kappa_0$ in (8), we have

$$B=\frac{g}{2+g}. \qquad \dots\dots\dots \qquad (9)$$

If $g=1$ (that is, if half the wall be cut away), $B=\frac{1}{3}$, $B^2=\frac{1}{9}$, so that the reflection is but small. If the channels be circular, and arranged in square order as close as possible to each other, $g=(4-\pi)/\pi$, whence $B=\cdot121$, $B^2=\cdot015$, nearly all the motion being transmitted.

It remains to consider the value of κ. The problem of the propagation of sound in a circular tube, having regard to the influence of viscosity and heat-conduction, has been solved analytically by Kirchhoff*, on the suppositions that the tangential velocity and the temperature-variation vanish at the walls. In discussing the solution, Kirchhoff takes the case in which the dimensions of the tube are such that the immediate effects of the dissipative forces are confined to a relatively thin stratum in the neighbourhood of the walls. In the present application interest attaches rather to the opposite extreme, viz. when the diameter is so small that the frictional layer pretty well fills the tube. Nothing practically is lost by another simplification which it is convenient to make (following Kirchhoff)—that the velocity of propagation of viscous and thermal effects is negligible in comparison with that of sound.

One result of the investigation may be foreseen. When the diameter of the tube is very small, the conduction of heat from the centre to the circumference of the column of air becomes more and more free. In the limit the temperature of the solid walls controls that of the included gas, and the expansions and rarefactions take place isothermally. Under these circumstances there is no dissipation due to conduction, and everything is the same as if no heat were developed at all. Consequently the coefficient of heat-conduction will not appear in the result, which will involve, moreover, the Newtonian value of the velocity of sound (b) and not that of Laplace (a).

Starting from Kirchhoff's formulæ, we find as the value of

* Pogg. *Ann.* cxxxiv. 1868.

κ^2 applicable when the diameter ($2r$) is very small,

$$\kappa^2 = -\frac{8in\mu'}{b^2r^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

μ' being the kinematic coefficient of viscosity. The wave propagated into the channels is thus proportional to

$$e^{px} \cos(nt+px+\epsilon), \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

where

$$p = \frac{\kappa}{1-i} = \frac{2\sqrt{(n\mu')}}{br} = \frac{2\sqrt{(n\gamma\mu')}}{ar}, \quad . \quad . \quad . \quad (12)$$

γ being the ratio of the specific heats, equal to 1·41. In the derivation of (10), $nr^2/(8\nu)$, ν being the thermometric coefficient of conductivity, is assumed to be small.

To take a numerical example, suppose that the pitch is 256 (middle c of the scale), so that $n=2\pi\times 256$. The value of μ' for air is ·16 C.G.S. (Maxwell), and that of ν is ·256. If we take $r=\frac{1}{1000}$ centim., we find $nr^2/8\nu$ equal to about $\frac{1}{1000}$. If r were 10 times as great, the approximation would perhaps still be sufficient.

From (12), if $n=2\pi\times 256$,

$$p = \frac{1{\cdot}15\times 10^{-3}}{r}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

so that if $r=\frac{1}{1000}$, $p=1{\cdot}15$. In this case the amplitude is reduced in ratio $e:1$ in passing over the distance p^{-1}—that is, about one centimetre. The distance penetrated is proportional to the radius of the channel.

The amplitude of the reflected wave is, by (8),

$$\mathrm{B} = \frac{p(1+g)(1-i)-\kappa_0}{p(1+g)(1-i)+\kappa_0},$$

or, as we may write it,

$$\mathrm{B} = \frac{p'(1-i)-1}{p'(1-i)+1} = \frac{p'-1-ip'}{p'+1-ip'}, \quad . \quad . \quad (14)$$

where

$$p' = (1+g)p/\kappa_0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

If I be the intensity of the reflected sound, that of the incident sound being unity,

$$\mathrm{I} = \frac{2p'^2-2p'+1}{2p'^2+2p'+1}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

The intensity of the intromitted sound is given by

$$\mathrm{I}' = 1-\mathrm{I} = \frac{4p'}{2p'^2+2p'+1}. \quad . \quad . \quad . \quad . \quad . \quad (17)$$

By (12), (15),

$$p'=\frac{2(1+g)\sqrt{(\mu'\gamma)}}{r\sqrt{n}}. \quad . \quad . \quad . \quad (18)$$

If we suppose $r=\frac{1}{1000}$ centim., and $g=1$, we shall have a wall of pretty close texture. In this case, by (18), $p'=47{\cdot}4$, and $\mathrm{I}'={\cdot}0412$. A four-per-cent. loss may not appear to be much; but we must remember that in prolonged resonance we are concerned with the accumulated effects of a large number of reflections, so that rather a small loss in a single reflection may well be material. The thickness of the porous layer necessary to produce this effect is less than one centimetre.

Again, suppose $r=\frac{1}{100}$ centim., $g=1$. We find $p'=4{\cdot}74$, $\mathrm{I}'={\cdot}342$, and the necessary thickness would be less than 10 centimetres.

If r be much greater than $\frac{1}{100}$ centim., the exchange of heat between the air and the walls of the channels is no longer sufficiently free for the expansions to be treated as isothermal. When r is so great that the thermal and viscous effects extend only through a small fraction of it, we have the case discussed by Kirchhoff. If we suppose for simplicity $g=0$ (a state of things, it is true, not strictly consistent with channels of circular section*), we have

$$\mathrm{I}=\frac{\gamma'^2}{4nr^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

in which

$$\gamma'=\sqrt{\mu'}+\left(\frac{a}{b}-\frac{b}{a}\right)\sqrt{\nu}. \quad . \quad . \quad . \quad . \quad (20)$$

The incident sound is absorbed more and more completely as the diameter of the channels increases; but at the same time a greater thickness becomes necessary in order to prevent a return from the further side. If $g=0$, there is no theoretical limit to the absorption; and, as we have seen, a moderate value of g does not by itself entail more than a comparatively small reflection. A loosely compacted hay- or straw-stack would seem to be as effective an absorbent of sound as anything likely to be met with.

In large spaces bounded by non-porous walls, roof, and floor, and with few windows, a prolonged resonance seems inevitable. The mitigating influence of thick carpets in such cases is well known. The application of similar material to the walls, or to the roof, appears to offer the best chance of further improvement.

* The problem in two dimensions is somewhat simpler than that treated by Kirchhoff. Although it would allow us without violence to suppose $g=0$, it seems scarcely worth while to enter upon it here, as the results are of precisely the same character. The principal difference is that the hyperbolic functions cosh &c. replace that of Bessel.

XXVII. *On the Size of Conductors for the Distribution of Electric Energy.* By THOMAS GRAY, *B.Sc., F.R.S.E.**

THE principal questions which have to be considered in the determination of the proper size of conductors for the distribution of electric energy are:—

(1) *Economy* †, or the amount of metal which must be put into the conductor in order that the sum of the annual cost for interest on capital, for depreciation, and for energy lost in consequence of heat generated in the conductor by the current may be a minimum.

(2) *Safety*, or the amount of metal necessary to prevent excessive heating.

(3) *Regulation*, or the amount of metal required to prevent too great a variation of potential along the leads.

In the consideration of the question of economy, let I be the fraction of the original cost of the conductors to be allowed annually for interest and depreciation combined, let P be the price per unit volume of the conductor, A its sectional area, and l its length. Then the cost per annum of possessing the conductor is

$$\mathrm{IPA}l.$$

Again, if E be taken as the cost of one erg of electric energy, c the current flowing in the conductor at any time t, and S the specific resistance of the material, the energy lost in heat will cost per annum

$$\mathrm{ES}\frac{l}{\mathrm{A}}\int_0^{\mathrm{T}} c^2\,dt,$$

where T is the number of seconds in a year. In most cases c will be a complicated and somewhat uncertain function of t, in consequence of which it may only be possible to form a rough estimate of the value of the integral. Let C be such a quantity that

$$\mathrm{ES}\frac{l}{\mathrm{A}}\int_0^{\mathrm{T}} c^2\,dt=\mathrm{EC^2S}\frac{l}{\mathrm{A}}\mathrm{T},$$

and we get for the total annual expenditure due to the conductors the expression

$$\mathrm{IPA}l+\mathrm{EC^2S}\frac{l}{\mathrm{A}}\mathrm{T}. \quad \cdots\cdots \quad (\alpha)$$

* Communicated by the Author.

† This problem for the case of a constant current flowing during the same portion of each day, has already been treated by Sir William Thomson, in a paper communicated to the British Association at the York Meeting 1881, and printed in 'Nature' for September 1881.

The expression (α) has a minimum value when

$$A=C\sqrt{\frac{EST}{IP}}=C\sqrt{\frac{ES}{iP}}, \quad . \quad . \quad . \quad (1)$$

where i is the fraction of the original cost to be allowed per second for interest and depreciation.

Equation (1) shows that, from a purely economical point of view, the section of the conductor should be directly proportional to the quantity C, which, when the current is continuous and of constant value, is the current-strength in C.G.S. units, but which, when the current is variable, has such a value that the heat which would be generated by a continuous and constant current of that value is equal to the actual heat generated. The section should also be directly proportional to the square root of the product of the cost of energy and the specific resistance of the conductor, and inversely proportional to the square root of the product of the first cost and the rate of interest and depreciation to be allowed, while it is independent of the length of the circuit.

Equation (1) gives in all cases an inferior limit below which it is not advisable to reduce the section of the conductor. It should be noticed, however, that the investigation proceeds on the assumption that the whole of the energy wasted is due to the heat generated in the main conductor by the current. This assumption may be, in many cases, inadmissible, owing to the varying E.M.F. at which the electricity would be supplied; and in such cases the economy problem assumes different forms depending on the conditions imposed.

Practical Example.

Let us suppose, for the sake of illustration, that the cost of one erg, or E, is, reckoned in pounds sterling,

$$E=\frac{1}{10^9T},$$

that the conductors are of copper having a specific resistance

$$S=1700,$$

that the price of copper conductors manufactured is

$$P=\frac{1}{1200},$$

and that the rate of interest and depreciation is

$$I=7{\cdot}5 \text{ per cent.}$$

Then we have

$$A=C\sqrt{\frac{1700\times 1200\times T}{7\cdot 5\times 10^7\times T}}$$

$$\doteq\frac{C}{\cdot 6},$$

or, when C is reckoned in amperes,

$$A=\frac{C}{60}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

This gives 60 amperes per square centimetre of section as the proper relation between the current and the section of the conductor. It must be remembered, however, that this calculation is based on the assumption of bare copper conductors and on a particular estimate of the cost of energy, both of which may have to be greatly modified in particular cases.

If, for example, covered cable costing, say, five times as much as bare copper be used, the proper ratio of the section of the conductor to the current would be

$$\frac{A}{C}=\frac{1}{60\sqrt{5}}=\frac{1}{134}; \quad . \quad . \quad . \quad . \quad . \quad (3)$$

or for every 134 ampères of current there should be one square centimetre of section in the copper conductor.

Safety.

We have next to consider whether the size of the conductors given by the above considerations will be sufficient for safety. The answer to this question depends on the rise of temperature which will be produced by the greatest current which is to be sent through the conductor. (This greatest current must not be confounded with C in equation (1), as the strength of the current may occasionally greatly exceed that which it is proper to use according to that equation.) It is easy to calculate to a considerable degree of approximation the rise of temperature which will be produced in a conductor by an electric current when we know the specific resistance of the material, the emissivity of the surface, and, in the case of covered wire, the thermal conductivity of the covering.

If we assume the rate of cooling for any conductor per unit length per unit difference of temperature to be r, we have for the total rise of temperature t the equation

$$c^2\frac{S+\alpha t}{A}=tr, \text{ or } t=\frac{c^2 S}{rA-C^2 S\alpha}, \quad . \quad . \quad (4)$$

where α is the coefficient of change of specific resistance with temperature. That is to say, for any conductor the rise of temperature depends directly on the square of the current and on the specific resistance of the material, and, if we neglect the variation of specific resistance with temperature, inversely on the rate of cooling and the section of the conductor.

If we assume 50° C. as the maximum rise of temperature, we have for the section of the conductor,

$$A = \frac{c^2 S(1+50\alpha)}{50r}.$$

For bare copper rod we may take r as being very nearly $\frac{J}{5000}$ multiplied by the number of units of radiating surface in the unit length, or

$$r = \frac{\pi d J}{5000},$$

where d is the diameter of the rod, and J Joule's mechanical equivalent of heat, which is, approximately, $4 \cdot 2 \times 10^7$ in C.G.S. units. Hence, by substitution,

$$r = \frac{3 \cdot 14 \times 4 \cdot 2 \times 10^7}{5000} d = 2 \cdot 64 \times 10^4 d. \quad . \quad . \quad (5)$$

Now, as S is 1700 and α nearly $\cdot 0039$, we have

$$A = c^2 \frac{2030}{13 \cdot 2 \times 10^5 d} = \frac{1 \cdot 54}{10^3} \times \frac{c^2}{d}.$$

It must be remembered that c is here taken in C.G.S. units; and hence, putting for A $\frac{\pi d^2}{4}$, and taking c in amperes and reducing, we get

$$c = \frac{10^3}{4 \cdot 42} d^{\frac{3}{2}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Now the value of C found from equation (2) is for bare copper 60 amperes per square centimetre. Let us assume that the maximum value of c will be 100 amperes per square centimetre, and we have to find for what value of d or c we have

$$A = \frac{\pi d^2}{4} = \frac{c}{100}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

From (6) and (7) we get at once

$$d^{\frac{1}{2}} = \frac{4 \times 10^3}{\pi \times 442},$$

or

$$d \doteq 8 \cdot 5.$$

From this we see that the conductor will only rise 50° C. in temperature when the current is of such strength as to require a conductor eight and a half centimetres in diameter. The question of safety, then, may, at least in the case of uncovered copper rod, be left out of consideration when the current is less than 5000 amperes.

Next take the case of a conductor covered with a substance whose thermal conductivity is k. In this case r depends both on the rate of cooling from the surface, or the emissivity, and on the conductivity k.

Let e = emissivity of the surface,

T = difference of temperature between the two surfaces of the covering,

T' = the difference of temperature between the external surface of the covering and the surrounding atmosphere,

r_2 = the external diameter of the covering,

q = the quantity of heat conducted through the covering per unit of time.

Then, since for constant temperature we must have the quantity conducted through the covering equal to the quantity radiated from the surface, we have

$$2\pi r_2 e\mathrm{T}' = q. \qquad (8)$$

But

$$q = -2\pi kx\,\frac{dt}{dx}, \qquad (9)$$

where x is any radius. Hence

$$2\pi r_2 e\mathrm{T}' = -2\pi kx\frac{dt}{dx},$$

or

$$\frac{dt}{dx} = -\frac{e}{k}\frac{r_2}{x}\mathrm{T}'. \qquad (10)$$

But the total difference of temperature between the two sides of the covering is

$$\mathrm{T} = -\int_{r_1}^{r_2}\frac{dt}{dx}dx$$

$$= \mathrm{T}'r_2\frac{e}{k}\int_{r_1}^{r_2}\frac{dx}{x}$$

$$= \frac{e}{k}\mathrm{T}'r_2\log_e\frac{r_2}{r_1}. \qquad (11)$$

Now, by the conditions of the question,

$$\mathrm{T}+\mathrm{T}' = 50;$$

that is to say,

$$\mathrm{T}'\left\{1+\frac{e}{k}r_2\log_e\frac{r_2}{r_1}\right\}=50. \quad . \quad . \quad . \quad . \quad (12)$$

Assuming, what are probably not far from the true values for gutta percha, e and k to be $\frac{1}{4000}$ and $\frac{1}{2000}$ respectively, we get

$$\mathrm{T}'=\frac{50}{1+\frac{1}{2}r_2\log_e\frac{r_2}{r_1}}. \quad . \quad . \quad . \quad . \quad . \quad (13)$$

Again, since the amount of heat generated by the current must be equal to the quantity radiated from the surface of the covering,

$$\frac{c^2\mathrm{S}(1+50\alpha)}{\mathrm{AJ}}=\frac{2\pi r_2}{4000}t_1;$$

by equation (13),

$$=\frac{2\pi r_2}{4000}\times\frac{50}{1+\frac{1}{2}r_2\log_e\frac{r_2}{r_1}}$$

$$=\frac{\pi r_2}{40\left\{1+1\cdot15\,r_2\log_{10}\frac{r_2}{r_1}\right\}}. \quad . \quad . \quad . \quad (14)$$

Take, for example, $r_1=1$ and $r_2=1\cdot4$. Then

$$c^2\frac{2030}{\pi\times4\cdot2\times10^7}=\frac{\pi\times1\cdot4}{40\{1+1\cdot15\times1\cdot4\log1\cdot4\}};$$

$$\therefore\ c=\pi\sqrt{\frac{1\cdot4\times4\cdot2\times10^7}{40\times2030\{1+1\cdot15\times1\cdot4\log1\cdot4\}}}$$

$$=\pi\times22\cdot3.$$

That is to say, a rod 1 centimetre in radius and covered with gutta percha to a thickness of 4 millimetres requires a current 223 amperes per square centimetre to raise its temperature 50° C.

If we suppose $r_1=1$ and $r_2=5$, we find $c=\pi\times21$, or a current of 210 amperes would heat the conductor 50° C. Increased thickness, therefore, in the covering does not greatly alter the capacity of the conductor.

Regulation.

It is of great importance, especially in electric-lighting circuits, that the E.M.F. at which the electricity is supplied should be nearly constant. Various means may be resorted to for the purpose of securing this; but we do not propose to go further into the matter at present than to investigate what the maximum difference of E.M.F. will be between different parts of the systems when the conductors are of the size which we should be inclined, from economical considerations, to adopt.

Let the E.M.F. between the conductors at any point P of the system (say at the poles of the generator) be kept at the constant value e volts. Then, if the current in amperes per square centimetre section of the conductor be c, we have

$$C=10^9\frac{x}{S},$$

where x is the E.M.F. in volts per unit length along the conductor. Now x is half the variation of E.M.F. per unit length along the system; and hence, if v be this variation,

$$v=\frac{2}{10^9}\,CS. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

Let us assume, as before, that the maximum value of c is 100, and we get

$$v=\frac{2\times 100\times 1700}{10^9}=\frac{34}{10^5} \quad . \quad . \quad . \quad . \quad (16)$$

If we assume that the greatest percentage variation of E.M.F. allowable is p, we get for the distance d of the furthest point of the system from the point P,

$$d=\frac{1000}{34}ep$$

$$\doteq 30ep. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

When $p=5$, which produces a very inconvenient variation in the illuminating-power of the incandescent lamps at present made, we get with

$$e=\ 50, \qquad d=\ 75 \text{ metres},$$
$$e=100, \qquad d=150 \quad ,,$$
$$e=200, \qquad d=300 \quad ,,$$

and so on.

XXVIII. *On the Thermoelectric, Actinoelectric, and Piezoelectric Properties of Quartz.* By W. C. Röntgen*.

THE last of the experiments described in my second communication on the optical behaviour of quartz in the electrical field induced me (as already mentioned†) to investigate experimentally the electrical properties of quartz, especially its thermoelectric and actinoelectric properties. In this investigation I very soon arrived at the conclusion that it is possible to refer to a common cause the evolution of electricity brought about in very different ways, whether by conduction of heat, radiation, or alteration of pressure—namely, to a change in the strains produced in the crystal by any means. I therefore consider it unnecessary to distinguish the three kinds of electricity named as differing from each other in their mode of production, and should propose to retain only the name piezoelectricity, if further investigation should show that the method of explanation indicated is applicable in all cases and sufficient. I have delayed the publication of this view and of these experiments, in the first place because the experiments were not altogether complete, and, secondly, because my theory is so entirely at variance with that proposed by so experienced and skilful an experimenter as Herr Hankel. It seems to me now, however, not possible to wait longer; and in the following I briefly describe the most important of my experiments.

I think we may conclude, from the electro-optical experiments described, that increase of a uniform surface-pressure exerted all over a quartz cylinder or quartz sphere produces such an evolution of electricity that the surface is divided by the three planes of no piezoelectricity into six electrical fields, which have the same position and sign as those obtained by the method of increase of pressure in one direction, described on p. 100 of my second paper (Phil. Mag. August 1883).

If we bring a non-electrified and hot sphere, having a uniform temperature all over, into a colder space (so that it can cool uniformly), the external layers, which cool first, will exert a pressure upon the inner ones everywhere in a radial direction, which will increase rapidly at first: consequently during this period, which we will call the first, we shall have the same distribution of electricity upon the sphere which corresponds to an increase of surface-pressure mechanically exerted upon it. After some time, when the cooling has

* Translated from a separate impression from the *Ber. der Oberh. Ges. für Natur-und Heilkunde*, communicated by the Author.

† Phil. Mag. August 1883, p. 109.

advanced further, the pressure exerted by the outer layers does not increase any more, but begins to diminish; but then the sign of the piezoelectricity evolved changes also, and the electricity produced during the first period becomes more and more weakened. During this second period the sphere becomes continually less strongly electric; and it may happen, especially if a part of the electricity produced has disappeared by conduction during the first period, that the all but cold sphere shows an electrification which is the opposite of that found when the cooling commenced.

These conclusions I have been able to verify repeatedly by observing the electricity appearing upon a sphere of quartz freely suspended, during cooling. Herr Hankel * has also observed the phenomena just described with quartz crystals. He, however, calls the electricity which makes its appearance at first actinoelectricity, and that remaining at the last thermoelectricity.

By heating a quartz sphere as uniformly as possible, I have obtained phenomena altogether analogous to the preceding; but the electricities are now of the opposite sign.

If we observe that the outer layers which receive the heat first exert upon the inner ones a radially directed tension, and that, by increase of a tension exerted upon the quartz, the same piezoelectricity is produced as by the decrease of pressure acting in the same direction, the explanation becomes easy.

Local cooling of a previously heated crystal by means of a cold stream of air directed against the crystal produces an energetic evolution of electricity at the points cooled, if these points did not lie exactly in a plane of no piezoelectricity; the nature of the resulting electricity was the same as that which would be produced at the same place by the increase of a pressure exerted there in the direction of a diameter. Local heating by a current of warm air produced, on the other hand, electricity of the opposite kind. In the first case we have a rapid increase of the pressure exerted by the outer layers upon the inner ones; in the second case, where the outer layers tend to raise themselves above the inner ones, the resulting tension increases very rapidly. I therefore consider the electricity produced to be simply piezoelectric. The electricity which Herr Friedel observed to be produced by placing a heated metal ball upon a quartz crystal is identical, both in sign and in mode of production, with that produced by a hot current of air: it is therefore piezoelectricity. Herr Friedel calls it thermoelectricity; Herr Hankel actinoelectricity.

* Hankel, 'Electrical Researches,' Abhandlung 15, p. 530.

The following experiments seem to me peculiarly adapted to support my theory.

A ring of tinfoil, of internal diameter 2 centim. and exterior diameter 4 centim., was cemented to a homogeneous plate of quartz cut at right angles to the principal axis; the ring was then cut through radially at six points in the direction of the axis of no piezoelectricity, so that six pieces of the ring, insulated from each other, were obtained. The first, third, and fifth pieces were connected by wires with one half-ring of a Kirchhoff-Thomson electrometer, the second, fourth, and sixth pieces and the other half-ring of the electrometer being connected with the earth.

If then, the plate possessing, to begin with, the temperature of the room, the central uncovered portion was heated by placing a warm metallic cylinder upon it, or by radiation from a flame or from a heated piece of metal, or by a current of hot air, or in any other way, then the portions of the ring became electrified, so that each portion of the ring acquired the same electricity as the end of the secondary axis lying next it would have acquired if there had been increase in a pressure acting in the direction of the corresponding secondary axis. Cooling of the central portion produced in any way, on the other hand, always produced the opposite electricities.

If now, in a subsequent experiment, it was not the central portion of the plate, but that surrounding the tinfoil ring which was heated or cooled as the case might be, then, in case of heating, the electrometer showed the presence of the same electricity previously found by cooling the centre, and conversely, in case of cooling, the same electricity as was produced by heating the centre.

These results are not surprising if we start from the view that changes in tension in the crystal are the cause to which the evolution of electricity is due. In the first and fourth cases, for example, heating at the centre, or cooling at the periphery, produces tensions in the plate which are of the same kind as those produced by a uniformly distributed pressure exerted upon the edge. In the second and third cases central cooling or peripheral heating produces a condition of tension analogous to the condition brought about by a uniformly distributed tension exerted upon the edge. In all cases, during the time immediately following the heating or cooling as the case may be, a rapid increase of the tensions produced takes place. Consequently in the two cases first mentioned there must be the same distribution of electricity as would correspond to an increase of the pressure exerted upon the edge of the plate; in the two last-mentioned cases, the distribution corresponding to a diminution of pressure.

We see from these experiments that the nature of the electricity produced is not dependent upon the particular method by which a local heating or warming is produced, but depends essentially upon the position in the crystal of the point where these changes in temperature take place. From the result that heating the peripheral portions of the plate and heating the central portions produce opposite electrical effects, I am disposed to draw a conclusion, which indeed has not yet been experimentally demonstrated but which seems to me tolerably safe. If we suppose it possible to warm a plate so uniformly that no perceptible differences of temperature or tensions should be produced, then I believe that this heating would produce no electricity or relatively very little, although the particles of the plate suffer considerable displacement amongst themselves. If, now, we consider that even very small displacements of the particles produce very considerable quantities of electricity, if these displacements are accompanied by changes of tension in the crystal (as is the case, for example, with irregular heating of a plate), the assumption seems to be justified that change of temperature and position of the particles in itself produces no electricity, but that, on the other hand, the real cause of the evolution of electricity is to be found in changes of tension.

In what has been described I have made a first attempt to explain the electricity produced in quartz by change of temperature as due to stress produced in the crystal. I am well aware that the explanation given in the separate cases is here and there defective, and that further investigations are necessary in order to establish the exact connexion between changes of temperature and evolution of electricity.

Giessen, March 20, 1883.

XXIX. *On Concave Gratings for Optical Purposes.* By HENRY A. ROWLAND, *Professor of Physics, Johns Hopkins University, Baltimore**.

General Theory.

HAVING recently completed a very successful machine for ruling gratings, my attention was naturally called to the effect of irregularity in the form and position of the

* An abstract of this Paper with some other matter was given at the Physical Society of London in November last, the Paper being in my hand in its present shape at that time. As I wished to make some addi-

lines and the form of the surface on the definition of the grating. Mr. C. S. Peirce has recently shown, in the 'American Journal of Mathematics,' that a periodic error in the ruling produces what have been called "ghosts" in the spectrum. At first I attempted to calculate the effect of other irregularities by the ordinary method of integration; but the results obtained were not commensurate with the labour. I then sought for a simpler method. Guided by the fact that inverse methods in electrical distribution are simpler than direct methods, I soon found an inverse method for use in this problem.

In the use of the grating in most ordinary spectroscopes the telescopes are fixed as nearly parallel as possible, and the grating turned around a vertical axis to bring the different spectra into the field of view. The rays striking on the grating are nearly parallel; but for the sake of generality I shall assume that they radiate from a point in space, and shall investigate the proper ruling of the grating to bring the rays back to the point from which they started. The wave-fronts will be a series of spherical shells at equal distances apart: if these waves strike on a reflecting surface they will be reflected back, provided they can do so all in the same phase. A sphere around the radiant-points satisfies the condition for waves of all lengths; and this gives the case of ordinary reflection. Let any surface cut the wave-surface in any manner, and let us remove those portions of the surface which are cut by the wave-surface. The light of that particular wave-length can then be reflected back along the same path and in the same phase; and thus, by the above principle, a portion will be thus sent back. But the solution only holds for one wave-length; and so white light will be drawn out into a spectrum. Hence we have the important conclusion that a theoretically perfect grating for one position of the slit and eyepiece can be ruled on any surface, flat or otherwise. This is an extremely important practical conclusion, and explains many facts which have been observed in the use of gratings. For we see that errors of the dividing-engine can be counterbalanced by errors in the flatness of the plate; so that a bad

tions, for which I have not yet had time, I did not publish it at the time. I was much surprised soon after to see an article on this subject, which had been presented to the Physical Society and was published in the Philosophical Magazine. The article contains nothing more than an extension of my remarks at the Physical Society, and formulæ similar to those in this paper. As I have not before this published any thing except a preliminary notice of the concave grating, I expected a little time to work up the subject, seeing that the practical work of photographing the spectrum has recently absorbed all my time. But probably I have waited too long.

dividing-engine may now and then make a grating which is good in one spectrum but not in all. And so we often find that one spectrum is better than another. Furthermore Prof. Young has observed that he could often improve the definition of a grating by slightly bending the plates on which it was ruled.

From the above theorem, we see that if a plate is ruled in circles whose radius is $r \sin \mu$ and whose distance apart is $\Delta r \sin \mu$, where Δr is constant, then the ruling will be appropriate to bring the spectrum to a focus at a distance r and angle of incidence μ. Thus we should need no telescope to view the spectrum in that particular position of the grating. Had the wave-surfaces been cylindrical instead of spherical, the lines would have been straight instead of circular, but at the above distances apart. In this case the spectrum would have been brought to a focus, but would have been diffused in the direction of the lines.

In the same way we can conclude that in flat gratings any departure from a straight line has the effect of causing the dust in the slit and the spectrum to have different foci—a fact sometimes observed.

We also see that if the departure from equal spaces is small, or, in other words, the distance r is great, the lines must be ruled at distances apart represented by

$$c\left(1-\frac{\cos^2 \mu}{r \sin \mu}x+\text{\&c.}\right)$$

in order to bring the light to a focus at the angle μ and distance r, c being a constant and x the distance from some point on the plate. If μ changes sign, the r must change in sign. Hence we see that the effect of a linear error in the spacing is to make the focus on one side shorter and the other side longer than the normal amount. Prof. Peirce has measured some of Mr. Rutherfurd's gratings and found that the spaces increased in passing along the grating; and he also found that the foci of symmetrical spectra were different. But this is the first attempt to connect the two. The definition of a grating may thus be very good even when the error of run of the screw is considerable, provided it is linear.

Concave Gratings.

Let us now take the special case of lines ruled on a spherical surface; and let us not confine ourselves to light coming back to the same point, but let the light return to another point. Let the coordinates of the radiant-point and focal point be $y=0$, $x=-a$, and $y=0$, $x=+a$, and let the centre of the sphere whose radius is ρ be at x', y'. Let r be the distance from the radiant-point to the point x, y, and let R be that from the focal point to x, y. Let us then write

$$2b = \mathrm{R} + rc,$$

where c is equal to ± 1 according as the reflected or transmitted ray is used. Should we increase b by equal quantities and draw the ellipsoids or hyperboloid so indicated, we could use the surfaces in the same way as the wave-surface above. The intersections of these surfaces with any other surface form what are known as Huyghens's zones. By actually drawing these zones on the surface we form a grating which will reflect or refract the light of a certain wave-length to the given focal point. For the particular problem in hand we need only work in the plane x, y for the present.

Let s be an element of the curve of intersection of the given surface with the plane x, y. Then our present problem is to find the width of Huyghens's zones on the surface—that is, ds in terms of db.

The equation of the circle is

$$(x-x')^2 + (y-'y)^2 = \rho^2;$$

and of the ellipse or hyperbola,

$$\mathrm{R} + rc = 2b,$$

or

$$(b^2-a^2)x^2 + b^2y^2 = b^2(b^2-a^2),$$

in which c has disappeared.

$$ds = \sqrt{dx^2+dy^2}, \quad \frac{dx}{dy} = -\frac{y-y'}{x-x'},$$

$$dx\left\{(b^2-a^2)x - b^2y\frac{x-x'}{y-y'}\right\} = b\{2b^2-(x^2+y^2+a^2)\}db,$$

$$dy\left\{-(b^2-a^2)x\frac{y-y'}{x-x'} + b^2y\right\} = b\{2b^2-(x^2+y^2+a^2)\}db;$$

$$\therefore \frac{ds}{db} = \rho b\frac{2b^2-(x^2+y^2+a^2)}{(b^2-a^2)(y-y')x - b^2(x-x')y}.$$

This equation gives us the proper distance of the rulings on the surface; and if we could get a dividing-engine to rule according to this formula, the problem of bringing the spectrum to a focus without telescopes would be solved. But an ordinary dividing-engine rules equal spaces; and so we shall further investigate the question whether there is any part of the circle where the spaces are equal. We can then write

$$\frac{ds}{db} = \mathrm{C};$$

and the differential of this with regard to an arc of the circle must be zero. Differentiating and reducing by the equations

$$\frac{dx}{dy}=-\frac{y-y'}{x-x'},\quad \frac{db}{dy}=-\frac{\rho}{\mathrm{C}(x-x')},$$

we have

$$\rho\left\{2xb(y-y')-2yb(x-x')-\frac{\rho}{\mathrm{C}}[6b^2-(x^2+y^2+a^2)]\right\}$$

$$+\mathrm{C}\left\{(y-y')[(b^2-a^2)(y-y')-b^2y]-(x-x')[(b^2-a^2)x-b^2(x-x')]\right.$$
$$\left.+\frac{2b\rho}{\mathrm{C}}[x(y-y')-y(x-x')]\right\}=0.$$

It is more simple to express this result in terms of R, r, ρ and the angles between them.

Let μ be the angle between ρ and r, and ν that between ρ and R. Let us also put

$$\alpha=\frac{\mu-\nu}{2},\quad \beta=\frac{\mu+\nu}{2}.$$

Let β, γ, δ also represent the angles made by r, R, and ρ respectively with the line joining the source of light and focus, and let

$$\eta=\frac{\beta+\gamma}{2}.$$

Then we have

$$x=\frac{\mathrm{R}\cos\gamma+\rho\cos\beta}{2},\quad y=\frac{\mathrm{R}\sin\gamma+r\sin\beta}{2},\quad a=\frac{r\cos\beta-\mathrm{R}\cos\gamma}{2},$$

$$(b^2-a^2)(y-y')^2+b^2(x-x')^2=\rho^2(b^2-a^2\sin^2\delta),$$

$$b^2-a^2=\mathrm{R}r\cos^2\alpha,$$

$$\sin\eta=\frac{\mathrm{R}+r}{2a}\sin\alpha,\qquad \cos\eta=\frac{r-\mathrm{R}}{2a}\cos\alpha,$$

$$\mathrm{R}=b-\frac{a}{b}x,\qquad \rho=b+\frac{a}{b}x,$$

$$x=b\frac{\cos y}{\cos\alpha},\quad y=a\frac{\sin\gamma\sin\beta}{\sin\alpha\cos\alpha}=\frac{\mathrm{R}r}{b}\sin\eta\cos\alpha,$$

$$b^2y(y-y')+x(b^2-a^2)(x-x')=\frac{b\mathrm{R}r\rho}{2}(\cos\mu+\cos\nu),$$

$$2b^2-(x^2+y^2+a^2)=\mathrm{R}r,$$

$$x(b^2-a^2)(y-y')-b^2y(x-x')=\frac{\mathrm{R}rb\rho}{2}(\sin\mu+\sin\nu),$$

$$\mathrm{C}=\frac{2}{\sin\mu+\sin\nu}=\frac{1}{\cos\alpha\sin\epsilon},$$

$$2a\cos\delta=r\cos\mu-\mathrm{R}\cos\nu,$$

$$2a\sin\delta=r\sin\mu-\mathrm{R}\sin\nu.$$

On substituting these values and reducing, we find

$$*\rho=2\mathrm{R}r\frac{\cos\alpha\cos\epsilon}{r\cos^2\nu+\mathrm{R}\cos^2\mu},$$

whence the focal length is

$$r=\rho\mathrm{R}\frac{\cos^2\mu}{2\mathrm{R}\cos\alpha\cos\epsilon-\rho\cos^2\nu}.$$

For the transmitted beam, change the sign of R.

Supposing ρ, R, and ν to remain constant and r and μ to vary, this equation will then give the line on which all the spectra and the central image are brought to a focus.

By far the most interesting case is obtained by making

$$r=\rho\cos\mu,\quad \mathrm{R}=\rho\cos\nu,$$

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* A more simple solution is the following. $\frac{ds}{db}$ must be constant in the direction in which the dividing-engine rules. If the dividing-engine rules in the direction of the axis, the differential of this with respect to y must be zero. But we can also take the reciprocal of this quantity; and so we can write for the equation of condition,

$$\frac{d}{dy}\frac{d(\mathrm{R}+r)}{ds}=0.$$

Taking a circle as our curve, we can write

$$(x-x')^2+(y-y')^2=\rho^2$$

and

$$(x-x'')^2+(y-y'')^2=\mathrm{R}^2,$$

$$(x-x''')^2+(y-y''')^2=r^2,$$

$$\frac{d(\mathrm{R}+r)}{ds}=\frac{1}{\rho}\left\{(y-y')\left(\frac{x-x''}{\mathrm{R}}+\frac{x-x'''}{r}\right)+(x-x')\left(\frac{y-y''}{\mathrm{R}}+\frac{y-y'''}{r}\right)\right\},$$

$$\frac{d}{dy}\frac{d(\mathrm{R}+r)}{ds}=\frac{1}{\rho}\left\{\frac{x-x''}{\mathrm{R}}+\frac{x-x'''}{r}-(y-y')\left[\frac{(x-x'')(y-y'')}{\mathrm{R}^3}+\frac{(x-x''')(y-y''')}{r^3}\right]\right.$$
$$\left.+(x-x')\left[\frac{(x-x'')^2}{\mathrm{R}^3}+\frac{(x-x''')^2}{r^3}\right]\right\}=0.$$

Making $x=0$, $y=0$, $y'=0$, $x'=\rho$, we have

$$\frac{x''}{\mathrm{R}}+\frac{x'''}{r}-\rho\left(\frac{x''^2}{\mathrm{R}^3}+\frac{x'''^2}{r^3}\right)=0,$$

or

$$\rho=\mathrm{R}r\frac{\cos\mu+\cos\nu}{r\cos^2\nu+\mathrm{R}\cos^2\mu}=2\mathrm{R}r\frac{\cos\alpha\cos\epsilon}{r\cos^2\nu+\mathrm{R}\cos^2\mu}.$$

since these values satisfy the equation. The line of foci is then a circle with a radius equal to one half ρ. Hence, if a source of light exist on this circle, the reflected image and all the spectra will be brought to a focus on the same circle. This is, if we attach the slit, the eyepiece, and the grating to the three radii of the circle, however we move them, we shall always have some spectrum in the focus of the eyepiece. But in some positions the line of foci is so oblique to the direction of the light, that only one line of the spectrum can be seen well at any one time. The best position of the eyepiece, as far as we consider this fact, is thus the one opposite to the grating and at its centre of curvature. In this position the line of foci is perpendicular to the direction of the light; and we shall show presently that the spectrum is normal at this point whatever the position of the slit, provided it is on the circle.

Fig. 1.

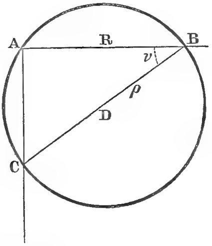


Fig. 1 represents this case. A is the slit, C is the eyepiece, and B is the grating with its centre of curvature at C. In this case all the conditions are satisfied by fixing the grating and eyepiece to the bar B C, whose ends rest on carriages moving on the rails A B and A C at right angles to each other. When desired, the radius A D may be put in to hold every thing steady; but this has been found practically unnecessary.

The proper formulæ for this case are as follows. If λ is the wave-length, and w the distance apart of the lines of the grating from centre to centre, then we have

$$\frac{1}{C}=\frac{\lambda N}{2w}=\sin\frac{\nu}{2},$$

where N is the order of the spectrum;

$$\therefore\ \lambda=\frac{w\sin\nu}{N}.$$

Now in the given case ρ is constant, and so $N\lambda$ is proportional to the line A C. Or, for any given spectrum, the wave-length is proportional to that line.

If a micrometer is fixed at C, we can consider the case as follows:

$$\frac{1}{C}=\frac{\lambda N}{2w}=\tfrac{1}{2}(\sin\mu+\sin\nu),$$

$$\frac{d\lambda}{du}=\frac{w}{N}\cos\mu.$$

If D is the distance the cross-hairs of the micrometer move forward for one division of the head, we can write for the point C,

$$d\mu=\frac{D}{\rho};$$

and for the same point μ is zero. Hence

$$\Delta\lambda=\frac{wD}{N\rho}.$$

But this is independent of ν; and we thus arrive at the important fact that the value of a division of the micrometer is always the same for the same spectrum and can always be determined with sufficient accuracy from the dimensions of the apparatus and number of lines on the grating, as well as by observations of the spectrum.

Furthermore, this proves that the spectrum is normal at this point and to the same scale in the same spectrum. Hence we have only to photograph the spectrum to obtain the normal spectrum, and a centimetre for any of the photographs always represents the same increase of wave-length.

It is to be specially noted that this theorem is *rigidly true* whether the adjustments are correct or not, provided only that the micrometer is on the line drawn perpendicularly from the centre of the grating, even if it is not at the centre of curvature.

As the radius of curvature of concave gratings is usually great, the distance through which the spectrum remains practically normal is very great. In the instrument which I principally use, the radius of curvature, ρ, is about 21 ft. 4 inches, the width of the ruling being about 5·5 inches. In such an instrument the spectrum thrown on a flat plate is normal within about 1 part in 1,000,000 for six inches, and less than 1 in 35,000 for eighteen inches. In photographing the spectrum on a flat plate, the definition is excellent for twelve inches; and by use of a plate bent to 11 ft. radius, a plate of twenty inches length is in perfect focus, and the spectrum still so nearly normal as to have its error neglected for most purposes.

It is also to be noted that this theorem of the normal spectrum applies also to the flat grating used with telescopes, and to either reflecting or transmitting gratings ; but in these cases only a small portion of the spectrum can be used, as no lens can be made perfectly achromatic. And so, as the distance of the micrometer has constantly to be changed when one passes along the spectrum, its constant does not remain constant but varies in an irregular manner. But it would be possible to fix the grating, one objective, and the camera rigidly on a bar, and then focus by moving the slit or then other objective. In this case the spectrum would be normal, but would probably be in focus for only a small length only, and the adjustment of the focus would not be automatic.

Another important property of the concave grating is that all the superimposed spectra are in focus at the same point, and so by micrometric measurements the relative wave-lengths are readily determined. Hence, knowing the absolute wave-length of one line, the whole spectrum can be measured. Prof. Peirce has determined the absolute wave-length of one line with great care; and I am now measuring the coincidences. This method is greatly more accurate than any hitherto known, as by mere eye-inspection the relative wave-length can often be judged to one part in twenty thousand, and with a micrometer to 1 in 1,000,000. Again, in dealing with the invisible portion of the spectrum, the focus can be obtained by examining the superimposed spectrum. Capt. Abney, by using a concave mirror in the place of telescopes, has been enabled to use this method for obtaining the focus in photographing the ultra-red rays of the spectrum.

But nothing can exceed the beauty and simplicity of the concave grating when mounted on a movable bar such as I have described and illustrated in fig. 1. Having selected the grating which we wish to use, we mount it in its plate-holder and put the proper collimating eyepiece in place. We then carefully adjust the focus by altering the length of ρ until the cross-hairs are at the exact centre of curvature of the grating. On moving the bar the whole series of spectra are then in exact focus, and the value of a division of the micrometer is a known quantity for that particular grating. The wooden way, A C, on which the carriage moves is graduated to equal divisions representing wave-lengths, since the wave-length is proportional to the distance A C. We can thus set the instrument to any particular wave-length we may wish to study, or even determine the wave-length to one part in at least five thousand by a simple reading. By having a variety of scales, one for each spectrum, we can immediately see what lines are superimposed on each other, and identify them accordingly when we are measuring their relative wave-length. On now replacing the eyepiece by a camera, we are in position to photograph the spectrum with the greatest ease. We put in the sensitive plate, either wet or dry, and move to the part we wish to photograph. Having exposed for that part, we move to another position and expose once more. We have no thought for the focus, for that remains perfect, but simply refer to the table giving the proper exposure for that portion of the spectrum, and so have a perfect plate. Thus we can photograph the whole spectrum on one plate in a few minutes from the F line to the extreme violet, in several strips each 20 inches long. And we may photograph to the red rays by prolonged exposure. Thus the work of days with any other apparatus becomes the work of hours with this. Furthermore each plate is to scale, an inch on any one of the strips representing *exactly* so much difference of wave-length. The scales of the different orders of spectra are *exactly* proportional to the order. Of course the superposition of the spectra gives the relative wave-lengths. To get the superposition, of course photography is the best.

Having so far obtained only the first approximation to the theory of the concave grating, let us now proceed to a second one. The dividing-engine rules equal spaces along the chord of the circular arc of the grating; the question is whether any other kind of ruling would be better. For the dividing-engine is so constructed that one might readily change it to rule slightly different from equal spaces.

The condition for theoretical perfection is that C shall remain constant for all portions of the mirror. I shall therefore investigate how nearly this is true. Let ρ be the radius of curvature, and let R and r be the true distances to any point of the grating, R_0 and r_0 being the distances to the centre. Let μ and ν be the general values of the angles, and μ_0 and ν_0 the angles referred to the centre of the mirror. The condition is that

$$\frac{2}{C} = \sin\mu + \sin\nu$$

shall be a constant for all parts of the surface of the grating. Let us then develop $\sin\mu$ and $\sin\nu$ in terms of μ_0, ν_0, and the angle δ between radii drawn to the centre of the grating and the point under consideration. Let δ' be the angle between R and R_0. Then we can write immediately

$$\rho \sin \mu = \rho \sin \mu_0 \cos \delta' + R_0 \sin \delta' - \rho \cos \mu_0 \sin \delta',$$

$$\sin \mu = \sin \mu_0 \cos \delta' \left\{ 1 + \frac{R_0}{\rho \sin \mu_0} A \tan \delta' \right\},$$

where

$$A = 1 - \frac{\rho \cos \mu_0}{R_0}.$$

Developing the value of $\cos \delta'$ in terms of δ, we have

$$\cos \delta' = \cos \delta \left\{ 1 + \frac{A}{2}\left[1 + \frac{\rho \cos \mu_0}{R_0}\right]\delta^2 \right.$$
$$\left. - \frac{\rho \sin \mu_0}{2R_0}\left[1 + A\left(1 + \frac{2\rho}{R_0}\right)\right]\delta^3 + \&c. \right\}.$$

As the cases we are to consider are those where A is small, it will be sufficient to write

$$\tan \delta' = \frac{\rho \cos \mu_0}{R^0}\delta,$$

whence we have

$$\sin \mu = \sin \mu_0 \cos \delta \left\{ 1 + \cot \mu_0 A\delta + \frac{A}{2}\left[1 + \frac{\rho \cos \mu_0}{R_0}\right]\delta^2 \right.$$
$$\left. + \left[A^2 \cot \mu_0\left(1 + \frac{\rho \cos \mu_0}{R_0}\right) - \frac{\rho \sin \mu_0}{2R_0}\left(1 + A\left(1 + \frac{2\rho}{R_0}\right)\right)\right]\delta^3 + \&c. \right\}$$

We can write the value of $\sin \nu$ from symmetry. But we have

$$2\frac{db}{ds} = \sin \mu + \sin \nu.$$

In this formula db can be considered as a constant depending on the wave-length of light &c., and ds as the width apart of the lines on the grating. The dividing-engine rules lines on the curved surface according to the formula

$$2\frac{db}{ds} = \cos \delta\,(\sin \mu_0 + \sin \nu_0).$$

But this is the second approximation to the true theoretical ruling. And this ruling will not only be approximately correct, but exact when all the terms of the series except the first vanish.

In the case where the slit and focus are on the circle of radius $\frac{1}{2}\rho$, as in the automatic arrangement described above, we have $A=0$, and the second and third terms of the series disappear, and we can write, since we have $\frac{R_0}{\rho} = \cos \mu_0$ and

$$\frac{r_0}{\rho} = \cos\nu_0,$$

$$2\frac{db}{ds}\cos\delta\,(\sin\mu_0 + \sin\nu_0)\left\{1 - \tfrac{1}{2}\frac{\sin\mu_0\tan\mu_0 + \sin\nu_0\tan\nu_0}{\sin\mu_0 + \sin\nu_0}\delta^3 + \&c.\right\}$$

But in the automatic arrangement we also have $\nu_0=0$; and so the formula becomes

$$2\frac{db}{ds} = \cos\delta\,(\sin\mu_0 + \sin\nu_0)\left\{1 - \tfrac{1}{2}\tan\mu_0\,\delta^3 + \&c.\right\}.$$

To find the greatest departure from theoretical perfection, δ must refer to the edge of the grating. In the gratings which I am now making, ρ is about 260 in., and the width of the grating about 5·4 in. Hence $\delta=\frac{1}{100}$ approximately, and the series becomes

$$1 - \frac{1}{2000000}\tan\mu_0.$$

Hence the *greatest* departure from the theoretical ruling, even when $\tan\mu_0 = 2$, is 1 in 1,000,000. Now the distance apart of the components of the 1474 line is somewhat nearly one forty-thousandth of the wave-length; and I scarcely suppose that any line has been divided by the best spectroscope in the world whose components are less than one third of this distance apart. Hence we see that the departure of the ruling from theoretical perfection is of little consequence until we are able to divide lines twenty times as fine as the 1474 line. Even in that case, since the error of ruling varies as δ^3, the greater portion of the grating would be ruled correctly.

The question now comes up as to whether there is any limit to the resolving power of a spectroscope. This evidently depends upon the magnifying power and the apparent width of the lines. The magnifying power can be varied at pleasure; and so we have only to consider the width of the lines of the spectrum. The width of the line evidently depends in a perfect grating upon three circumstances—the width of the slit, the number of lines in the grating, and the *true physical width of the line.* The width of the slit can be varied at pleasure; the number of lines on the grating can be made very great (160,000 in one of mine); and hence we are only limited by the true physical width of the lines. We have numerous cases of wide lines, such as the C line, the components of the D* and H lines, and numerous others which are perfectly fami-

* I have recently discovered that each component of the D line is double, probably from the partial reversal of the line as we nearly always see it in the flame-spectrum.

liar to every spectroscopist. Hence we are free to suppose that all lines have some physical width; and we are limited by that width in the resolving power of our spectroscopes. Indeed, from a theoretical standpoint we should suppose this to be true; for the molecules only vibrate freely while swinging through their free path; and in order to have the physical width $\frac{1}{100000}$ of the wave-length, the molecule must make somewhat nearly one hundred thousand vibrations in its free path; but this would require a free path of about two inches! Hence it would be only the outermost solar atmosphere that could produce such fine lines; and we can hardly expect to see much finer ones in the solar spectrum. Again*, it is found impossible to obtain interference between two rays whose paths differ by much more than 50,000 wave-lengths.

All the methods of determining the limit seem to point to about $\frac{1}{150000}$ of the wave-length as the smallest distance at which two lines can be separated in the solar spectrum by even a spectroscope of infinite power. As we can now nearly approach this limit, I am strongly of the opinion that we have nearly reached the limit of resolving-power, and that we can never hope to see very many more lines in the spectrum than can be seen at present, either by means of prisms or gratings; for the same limit holds in either case. It is not to be supposed, however, that the average wave-length of the line is not more definite than this; for we can easily point the cross-hairs to the centre of the line to perhaps 1 in 1,000,000 of the wave-length. The most exact method of detecting the coincidences of a line of a metal with one in the solar spectrum would thus be to take micrometric measurements first on one and then on the other; but I suppose it would take several readings to make the determination to 1 in 1,000,000.

Since writing the above I have greatly improved my apparatus, and can now photograph 150 lines between the H and K lines, including many whose wave-length does differ more than 1 in about 80,000. I have also photographed the 1474 and b_3 and b_4 widely double, and also one of the E lines just perceptibly double. With the eye much more can be seen; but I must say that I have not yet seen many signs of reaching a limit. The lines yet appear as fine and sharp as with a lower power. If my grating is assumed to be perfect, in the third spectrum I should be able to divide lines whose wave-length differed 1 in about 150,000, though not to photograph them.

* This method of determining the limit has been suggested to me by Prof. C. S. Hastings, of this University.

The E line has components about $\frac{1}{60000}$ of the wave-length apart. I believe I can resolve lines much closer than this, say 1 in 100,000 at least. Hence the idea of a limit has not yet been proved.

However, as some lines of the spectrum are wider than others, we should not expect any *definite* limit to resolving power, but a gradual falling-off as we increase our power. At first, in the short wave-lengths at least, the number of lines is nearly proportional to the resolving-power; but this law should fail as we approached the limit.

XXX. *On* Mr. Glazebrook's *Paper on the Aberration of Concave Gratings. By* Henry A. Rowland, *Professor of Physics, Johns Hopkins University, Baltimore**.

IN the June number of the Philosophical Magazine Mr. R. T. Glazebrook has considered the aberration of the concave grating, and arrives at the conclusion that the ones which I have hitherto made are too wide for their radius of curvature. As I had published nothing but a preliminary notice of the grating at that time, Mr. Glazebrook had not then seen my paper on the subject, of which I gave an abstract at the London Physical Society in November last. In this paper I arrive at the conclusion that there is practically no aberration, and that in this respect there is nothing further to be desired. The reason of this discrepancy is not far to seek. Mr. Glazebrook assumes that the spaces are equal on the arc of the circle. But I do not rule them in this manner, but the spaces are equal along the *chord* of the arc. Again, the surface is not cylindrical, but spherical.

These two errors entirely destroy the value of the paper as far as my gratings are concerned; for it only applies to a theoretical grating ruled in an entirely different manner and on a different form of surface from my own.

I am very much surprised to see the method given near the end of the paper for constructing aplanatic gratings on any surface; for this is the method by which I discovered the concave grating originally, and the figure is the same as that I put on the black board at the Meeting of the Physical Society in November last. I say I am surprised; for Mr. Glazebrook's paper was read at the Physical Society, where I had given the same method a few months before, and yet it passed without comment. Indeed I have given the same method at many of our own scientific societies. However, as Mr. Glazebrook was not present at the meeting referred to, he is entirely without blame in the matter.

* Communicated by the Author.

XXXI. *On the Production of Electricity by Evaporation, and on the electrical Neutrality of Vapour arising from electrified Still Surfaces of Liquids.* By L. J. Blake, *Ph.D.*[*]

Of the different theories as to the cause or causes of atmospheric electricity, two especially have been from time to time advocated. The basis of the first theory is the hypothesis that electricity is produced by the simple evaporation of a liquid. This hypothesis was first proposed by Volta[†], and afterwards found adherents in Pouillet[‡], Tait and Wanklyn[§]. Saussure[‖], Configliachi[¶], Erman[**], Reich[††], Gaugain[‡‡], and Riess[§§] attempted to overthrow it.

The second theory rests chiefly upon the hypothesis that there occurs a convection of the electricity which may be upon the surface of any evaporating liquid. This theory was maintained by Franklin[‖‖], E. Becquerel[¶¶], and De la Rive[***]; and the hypothesis itself has been experimentally tested by Buff[†††]. The following paper is a description of an exclusively experimental investigation of these two hypotheses. It was carried out by means of an electrometer herein described. With the very complicated phenomena of atmospheric electricity itself, this paper has nothing directly to do; hence a more detailed description of the theories just stated is unnecessary.

I. *Production of Electricity by Evaporation.*

(*a*) The investigation of the first hypothesis—*i. e.* that electricity is produced simply by the change of a liquid into vapour—was conducted in the following manner. The movable plate A of a Kohlrausch condenser was connected with

* Communicated by the Author.

† Phil. Trans. 1782, abridged, xv. p. 274.

‡ *Annales de Chimie et de Physique*, 1827, xxxv. p. 401, and xxxvi. p. 5.

§ Phil. Mag. [4] xxiii. p. 494; 'Nature,' vol. xxiii. p. 340.

‖ *Voyages dans les Alpes*, 1786, tome ii. pp. 227 & 249.

¶ Gilbert's *Ann.* 1811, xliii. p. 370.

** *Abh. der Berl. Akad.* 1818, xix. p. 25.

†† *Abh. bei Begründung der Sächs. Ges. d. Wissensch.* 1846, p. 199.

‡‡ *Comptes Rendus*, 1854, tome xxxviii. p. 1012.

§§ *Reibungselektricität*, 1853, ii. pp. 407 & 491.

‖‖ Compare Duprez, *Mém. cour. de l'Acad. d. Belg.* xvi. 1843.

¶¶ *Traité de l'Electricité*, 1836, tome iv. p. 116; *Comptes Rendus*, 1856, tome xlii. p. 661.

*** *Traité de l'Electricité*, 1858, tome iii. p. 190.

††† Liebig's *Annalen*, 1854, lxxxix. p. 203. Compare *Fortschr. der Phys.* x. p. 436.

a quadrant-electrometer constructed by R. Voss of Berlin, according to the suggestions of Von Helmholtz. The charge of the quadrant pairs of this electrometer was maintained by means of two Zamboni's piles of about 1500 pairs each. The aluminium needle of the electrometer, suspended by a fine silver thread, had the form of two nearly right-angled sectors of a circle whose apices were joined with each other. In order to make the instrument more sensitive, the distance between the two horizontal surfaces of the quadrants between which the needle swung was less than in former instruments of a similar kind. The mirror attached to the needle swung beneath the quadrants. The upper end of the silver thread was fastened in a torsion head, and served also for uniting any source of electricity with the needle. The metal frame of the instrument, which supported the Zamboni piles, was connected with the earth. One Daniell (Cu—$CuSO_4$—$ZnSO_4$—Zn), one pole of which was connected with the electrometer-needle, and the other with the earth, gave at the distance of 3 millim. between the millimetre-scale and the mirror a deflection from 67 to $71\frac{1}{2}$ millim. After six months, without disturbing in any way the charge of the quadrants maintained by the Zamboni piles, the deflection for 1 Daniell under similar arrangement of experiment was from $64\frac{1}{2}$ to 72 millim.

The condenser, the electrometer, and all other apparatus mentioned hereafter were enclosed in metal cases connected with the earth. All the insulated connecting wires were enclosed within brass tubes similarly connected with the earth to neutralize the effect of outside electricity. The brass axis, which supported at one end the collector plate B of the condenser, was prolonged at the other end, and about 25 centim. from B was bent into the form of a ring. In this ring rested a porcelain dish which contained in a sand-bath a second smaller dish of the same material. By this arrangement the insulation in the following experiments could be affected only through the support for the axis of B. The insulating power of this support, upon which evidently the reliability of the experiment depends, was measured both before and after each experiment. When B was charged by 4 Daniells, and the needle of the electrometer which was in connexion with A, distant $\frac{1}{2}$ millim. from B, gave a deflection of 140 millim. through influence from B, then the loss in electricity upon B, during 10 minutes, varied from $6\frac{1}{2}$ to 11 millim., *i. e.* from $4\frac{1}{2}$ to 7 per cent.

Further, there was a peculiar electrical action through this insulating support of the collector plate B. B, unelectrified and insulated, became very slowly charged in the presence of the tinfoil upon which the condenser rested. When B was left in the evening connected with the earth, and insulated the next morning, there was always found upon it a charge which produced a deflection from 10 to 17 millim., and under similar conditions always in the same direction. In 15 minutes B became charged to a potential corresponding to 0·5 and upwards to 2 millim. This peculiar electric action, which was always present, must evidently be taken into account, and is so in the following experiments. This action of the condenser having been determined, the inner porcelain dish was filled with the liquid to be experimented upon, and the sand-bath heated to 100° C. The flame employed for heating was then wholly removed*. A platinum wire inserted through a narrow glass tube fused around it formed metallic connexion between the plate B and the liquid. By the glass the steam was prevented from coming into contact with the wire. The condenser-plate A was separated $\frac{1}{2}$ millim. from B, and, together with the needle of the electrometer, connected with the earth. After 10 or 15 minutes of copious evaporation, A was insulated, and then separated from B. The electrical condition of A, and consequently of B, could be determined through the deflection of the electrometer-needle, and any difference of electrical condition at the begining and end of an experiment could be observed. The liquid used in the experiments was at first cold, and afterwards heated to rapid evaporation; but otherwise the conditions were unchanged, so as to be wholly independent of any disturbances possible from the nature of the apparatus itself. The following tables show the deflections at a distance of 3 millim. between the mirror and scale. 45 millim. scale-divisions denote a charge of 1 Daniell upon B. The deflection corresponding to 1 Daniell is here less than in the experiments mentioned above, because A is not charged directly, but indirectly from B.

* A line of experiment which I have not at the present time finished seems to indicate quite clearly the production of electricity in every flame. It was hence found necessary before each observation to extinguish the flame employed, because of its disturbing influence; owing to the presence of the sand-bath, however, the evaporation was not interrupted.

I. A. Sea-water (Baltic). Room temperature.

Experiment.	At the beginning.	After 5 minutes.	After 10 minutes.	After 15 minutes.
1.	0 scale-div.	−0·5 scale-div.	−1·0 scale-div.	−1·5 scale-div.
2.	0 "	......	−1·5 "	
3.	0 "	......	−0·5 "	
4.	0 "	−0·5 "	−0·5 "	−0·5 "
5.	0 "	−0·5 "	−1·0 "	−1·25 "
6.	0 "	−1·0 "	−1·0 "	−2·0 "
7.	0 "	−0·5 "	−1·0 "	
8.	0 "	−1·0 "	−1·5 "	
9.	0 "	−1·0 "	−1·25 "	−1·75 "
Average value...	0 "	−0·71 "	−1·02 "	−1·40 "

I. B. Sea-water (Baltic). 100° C.

Experiment.	At the beginning.	After 5 minutes.	After 10 minutes.	After 15 minutes.
1.	0 scale-div.	0 scale-div.	0 scale-div.	−0·5 scale-div.
2.	0 "	−0·5 "	......	−0·5 "
3.	0 "	0 "	0 "	−0·25 "
4.	0 "	0 "	0 "	−0·5 "
5.	0 "	0 "	−1·0 "	−1·0 "
6.	0 "	−0·5 "	−1·0 "	−1·5 "
7.	0 "	0 "	0 "	0 "
8.	0 "	−0·5 "	0 "	
9.	0 "	0 "	−1·0 "	−1·0 "
Average value...	0 "	−0·16 "	−0·37 "	−0·66 "

II. A. Sulphate-of-Copper Solution. Room temperature.

Experiment.	At the beginning.	After 5 minutes.	After 10 minutes.
1.	0 scale-div.	−1·5 scale-div.	−2·0 scale-div.
2.	0 "	−2·0 "	−2·0 "
3.	0 "	−1·0 "	−1·0 "
4.	0 "	0 "	−0·5 "
5.	0 "	−1·5 "	−0·5 "
Average value...	0 "	−1·2 "	−1·2 "

II. B. Sulphate-of-Copper Solution. 100° C.

Experiment.	At the beginning.	After 5 minutes.	After 10 minutes.
1.	0 scale-div.	−0·5 scale-div.	−1·0 scale-div.
2.	0 "	−1·25 "	−2·5 "
3.	0 "		−3·0 "
4.	0 "	−1·0 "	−2·0 "
5.	0 "	−1·0 "	−3·0 "
Average value...	0 "	−0·94 "	−2·3 "

III. Saturated Solution of Sodium Chloride.

	At the beginning.	After 5 minutes.	After 10 minutes.
(*a*) Room temperature......	0 scale-div.	−1·0 scale-div.	−1·5 scale-div.
(*b*) 100° C. ...	0 "	−1·5 "	−1·5 "

Nearly all the deflections are less than those from the electrical action already described as peculiar to B. The two exceptions exceed the limit of B's action by an amount so small that they need not be considered. The difference of average values in the Tables I. A and I. B for the experiments lasting 15 minutes equals −0·74 millim. scale-division. This corresponds to a negative charge upon B of 0·016 Daniell (0·0145 volt). This charge is too small in proportion to the sea-water evaporated, to be used as a basis for mathematical calculations concerning the electricity resident in the clouds*. Nor is it a sufficient ground for the assertion that the simple change of a liquid into a vapour produces electricity.

(*b*) A second method of experiment, in which the insulation does not come at all into consideration, was as follows:—A brass disk of 125 millim. diameter was fastened horizontally by its centre to the end of a stout wire which was connected, without the condenser, directly with the electrometer-needle. Both needle and disk C could be together either insulated or connected with the earth. The porcelain dish containing the liquid and resting in the insulated sand-bath was brought close under the disk C, and the liquid connected with the earth. The sand-bath having been heated and the flame employed afterwards extinguished, the vapour from the liquid was condensed very copiously upon the disk C. The reading of the electrometer-needle connected with the earth was first taken. Then the needle, and consequently the condensing-disk C, was insulated, and any deflection was observed. Afterwards the electrometer-needle was connected with the earth and its reading again taken. Thus a change in the needle's position of rest during the experiment, which always occurs with variations of room-temperature, was observed.

* Such calculations have been carried out by Freeman, Phil. Mag. [5] vol. xiii. p. 398 (June 1882).

70 Scale-divisions = 1 Daniell.

Experiment.	Needle's position of rest *before* the experiment.	Reading after 5 minutes' insulation.	Needle's position of rest *after* the experiment.	Change of position of rest during the experiment.	Charge of needle in 5 minutes.
Sea-water. Room temperature.					
1.	690	689·0	690	0 scale-div.	+1·0 scale-div.
2.	571	569·5	570	+1·0 „	+0·5 „
Sea-water. 100° C.					
1.	689	691	690	−1·0 scale-div.	−1·0 scale-div.
2.	568	567	567·5	+0·5 „	+0·5 „

The Tables indicate so slight a difference in the electrical action between cold and hot sea-water (and, what is more important, a difference not always of the same sign), that one is as little entitled here as in the first-described method of experiment to assert that a production of electricity by simple evaporation has been shown.

II. *Electrical Neutrality of Vapour arising from electrified Still Surfaces of Liquids*.*

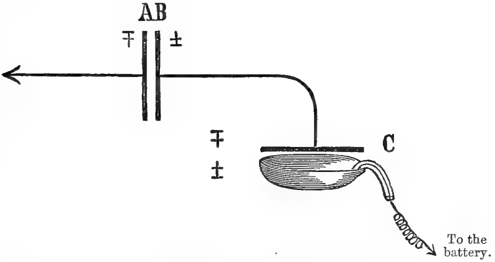
The second hypothesis to be tested is, that a convection of the electricity occurs by means of the vapour arising from the surface of an electrified liquid. An experimental research of the validity of this hypothesis was conducted in the following outlined methods.

(*a*) The movable plate A of the Kohlrausch condenser still remained in connexion with the needle of the electrometer. Upon the stationary collector plate B a water-stratum was deposited through condensation of warm-water vapour. B was then gently heated and connected with one pole of a galvanic battery, whose other pole was connected with the earth. The cooler plate A was then brought to a distance of 1 millim., and the vapours passing from B condensed copiously upon A. There occurred, however, no convection of the electricity, notwithstanding that the vapour originated in a water-surface containing one of the two electricities accumulated in the condenser. Experiments were begun with a weak potential-

* Compare L. J. Blake, "Ueber die elektrische Neutralität" etc., *Sitzungsberichte der Kgl. Akad. der Wissensch. zu Berlin*, 1882, Nr. xxix. 15 June, p. 635.

difference of 1 Daniell between the condenser-plates; and this difference increased up to 404 Daniells without affecting the result. Throughout the experiments the plate A was connected with the earth, until it was brought opposite the plate B, so as to protect the electrometer from too great deflections; then A was insulated. Any trace of electricity which might have been conveyed by means of the vapour would have manifested itself in the electrometer. Furthermore, there occurred no convection of the electricity either when B possessed the room-temperature and A was cooled below it, or when both plates were raised equally above the room-temperature, in which latter case evaporation without condensation occurs. If the vapour had conveyed with it the electricity from the water-surface, the presence of electricity upon A would have been detected in all the above cases.

(*b*) In order to produce evaporation from different liquids, an apparatus represented in the accompanying figure was



constructed. The experiments were conducted as follows. A horizontal brass disk, C, of 125 millim. diameter, surrounded by a metal box connected with the earth, was put in metallic connexion with the plate B of the condenser. The sealing-wax supports used for insulating the connecting wire between B and C, and at the same time for retaining firmly the disk C in its proper position, were so far separated from this disk (both being wholly outside the metal box) that vapour employed in the experiment could have no effect upon the insulation. An insulated porcelain dish, containing the liquid to be experimented upon, was placed on the arm of a retort-stand, so that it could be swung either under the disk C or wholly outside the metal box. The experiments were made in three series:—(1) Liquid as well as condensing-disk C of the temperature of the room, so that neither condensation nor marked evaporation took place. (2) Liquid 100° C., disk C cooled so that copious evaporation and condensation resulted. (3) Liquid and disk C at equal temperatures, but above the room-temperature, so that copious evaporation took place without condensation. The flame used in heating remained continually outside the box*.

It is necessary to free the surface of the liquid from dust-particles, and to avoid as much as possible air-currents within the metal box during the experiment. Moreover no minute drops must be tossed up during rapid evaporation. In these experiments the liquid experimented upon was in the first place electrified to a determined potential, and was swung under the condensing-disk C. B, and therefore C, had been previously insulated; and A, distant $\frac{1}{2}$ millim. from B, had been connected with the earth. Next, after 3 minutes the liquid was swung away from C. If a convection of electricity from the surface of the liquid to C had occurred, A would have fixed it upon B, and would have been itself charged with the opposite kind of electricity. After A was then insulated and separated, the presence of any fixed electricity upon A, and therefore also upon B and C, would have been shown by a deflection of the electrometer-needle connected with A. The liquid was electrified by a battery of 480 zinc, distilled water, copper elements (= 404 Daniells). The deflections, in millim. scale-divisions, observed are given in the following table.

	Liquid, room-temp. Plate C, room-temp.	Liquid, 100° C. Plate C, room-temp.	Liquid, 100° C. Plate C, 100° C.
DISTILLED WATER.	Charge +480 elements.		
	+4	+2	......
SALT-SOLUTION, (13 per cent. sodium chloride).	Charge +480 elements.		
	+4	+5	......
		+2·5	......
ALCOHOL, absolute.	Charge +480 elements.		
	0	+1	......
SULPHURIC ACID, concentrated.	Charge −480 elements.		
	0	−4	......
SEA-WATER (Baltic).	Charge +480 elements.		
	+2·5	+7·5	......
		+5	......
	Charge −480 elements.		
	−4	−2	−5
	−7	−4	−5
	−6	−6	−4

* See note page 213.

By a convection of electricity, the kind of electricity upon A, as mentioned above, must have been the opposite to that upon the surface of the electrified liquid. Now the tables not only do not show this, but in every case exactly the opposite. Upon A was always the same kind of electricity, and consequently upon B and C the opposite kind to that with which the liquid was charged. The following facts explain this result. During the experiment B and C were exposed to a strong induction. Of the two electricities induced upon B and C, one would be fixed by the surface of the liquid; the other would be free and would diffuse itself upon B. If the insulation of B and C was not perfect, then a loss of free induced electricity would occur. After the liquid was swung away from C, there would remain upon B and C electricity equal in quantity, but opposite in kind, to that dissipated from defective insulation during the experiment. This electricity would induce upon A the same kind of electricity as that of the electrified liquid. The correctness of this explanation of the charge upon A is confirmed by the following. Direct measurements of the insulating power of B and C were made previous to the experiments. B and C were charged either directly or indirectly by means of the 480 zinc, water, copper elements; and the loss during three minutes, which was the duration of the experiments mentioned above, was measured. This loss amounted to,

With positive charge,

7	scale-div.	out of	380	scale-div.,	or	1·8 per cent.
4·5	„	„	259	„	„	1·7 „
2·5	„	„	258	„	„	0·9 „

With negative charge,

8·5	scale-div.	out of	408	scale-div.,	or	2·0 per cent.
3·5	„	„	252	„	„	1·3 „

The induction upon B and C in these last experiments was maintained as nearly equal as possible to that in the first experiments. The close agreement of these figures with those of the deflections in the first tables, justify the conclusion that the electricity upon A in the first experiments was owing, not to a convection of the electricity present upon the surface of the liquid, but to a loss of electricity upon B and C, occurring in consequence of defective insulation during the experiment.

(*c*) The imperceptible variation in potential-difference between the poles of the zinc-water-copper battery rendered possible a method of experiment still more sensitive, in which insulation did not come at all into consideration. Without employing the Kohlrausch condenser, the condensing-disk C was directly connected with the electrometer-needle, and with this needle was put in metallic connexion with the earth. After the porcelain dish containing the electrified liquid was swung under C, the needle, and therefore C, were insulated. Only that electricity which was fixed by the closely lying electrified surface of the liquid could now be present upon C. *While* the liquid was under C, a deflection of the needle would show any convection of electricity from the surface of the liquid, and also any change of potential of that electrified surface. The following table gives the deflections, in scale-divisions, observed while the sea-water was 3 minutes under the disk C. As above, 70 scale-divisions correspond to a charge of 1 Daniell.

Positive charge, 480 elements.		Negative charge, 480 elements.	
Sea-water at room-temp.	Sea-water at 100° C.	Sea-water at room-temp.	Sea-water at 100° C.
+ 6	+12	+ 2	−9
+16	+25	−15	−5
		− 9	0

These numbers are opposed to convection of electricity by the vapour; otherwise no discrepancies in the signs of the deflections would occur. If convection by the vapour had occurred, a greater deflection would have appeared with a liquid rapidly evaporating than when cool. But in two cases the contrary takes place. When one considers that the rising vapour had condensed in large drops upon C, and that between C and the closely adjacent liquid a strong electrical attraction must have existed, the slight difference between the deflections for hot and cold sea-water loses all value as evidence of a convection of electricity by means of vapour.

The particles of dust which lie upon the surface of the liquid, or float in the surrounding air, are doubtless the bearers of the electricity upon C; for when the surface of the liquid was intentionally strewn with dust-particles, the charge upon the electrometer-needle was increased, while it was diminished when the surface was carefully cleaned. In another place the influence of dust-particles will be more clearly seen.

In regard to the experiments of Buff, already mentioned, I quote the following passage *:—“A glass flask, which con-

* Liebig's *Annalen*, lxxxix. p. 203, 1854.

tained the liquid to be evaporated, rested upon wire netting over the flame of a spirit-lamp. A platinum wire, connected with one pole of a galvanic battery, terminated in the liquid of the flask. The other battery-pole was connected with the earth. Close over the mouth of the flask was a strip of platinum, so placed as to be directly exposed to the action of the rising vapour. This strip could be connected either with the upper condenser-plate or with the earth, according to the arrangement of the experiment. The lower condenser-plate was fastened upon a very sensitive pile-electroscope. As soon as the water began to boil, or even approached boiling, and the under condenser-plate was connected with the earth, then the upper plate collected electricity of the same kind as that of the pole which stood in connexion with the water. A single pair of Bunsens was sufficient to noticeably separate the gold leaves of the Bennett electrometer. The charge increased proportionately with the number of pairs employed. The condenser remained closed one minute each time. If the liquid was in rapid ebullition, this length of time was not necessary for the charge to reach its maximum."

Against this method of arrangement of the experiment, which coincides in many of its principal points with the above described, arises the objection that convection of the electricity occurred, not by means of the vapour, but by the drops of water thrown up against the platinum strip during the ebullition.

Further, I have found during the experiment, that whenever a flame touched the porcelain dish (as it was allowed to do in Buff's experiments) a large deflection of the electrometer-needle occurred. This is to be explained only through a convection of electricity by means of the hot gases arising from the flame, because the deflections began immediately when the increase of temperature of the liquid was so slight that there was no practical increase of evaporation.

(*d*) By means of a modification of the apparatus described upon pages 217 & 218, proof of the accuracy of the results so far obtained by me was established by the most incontrovertible method of experiment possible to be devised. In place of the condensing-disk C, a brass ball of 20 millim. diameter was substituted. The arrangement of the experiment was not otherwise changed. This ball was placed over the still surface of the electrified liquid, as closely as possible without the passage of a spark between them ; consequently the distance between the ball and the surface of the liquid barely exceeded the striking-distance. The liquid was then electrified by means of a Töpler electrical machine, driven with great regularity by a small water-motor, so that thus the potential of the surface remained very constant. The striking-distance varied in different experiments from 3 to 8 millim. The ball and condenser-plate B in metallic connexion with it were insulated just previous to the electrification of the liquid; and the presence of any electricity conveyed to the ball was determined by the method described on page 218. It was necessary that the ball be highly polished and the liquid carefully cleaned in order to avoid dust-particles. Their influence is shown by the deflections in scale-divisions given in the following tables of experiments with sea-water.

45 scale-divisions = 1 Daniell.

Positive Charge.				
	Experiment.	Liquid at room-temp.	Liquid at 100° C.	Electrified brass disk replacing liquid surface under the ball.
Without special precaution against dust-particles	1.	−36·25	−14	+30·5
	2.	−37	−91·75	+21
	3.	+20·25	−43·75	
With precaution...	4.	+25·75	+16·25	+38·5
Negative Charge.				
Without special precaution against dust-particles	1.	+40·5	+98·75	+ 3·75
	2.	+25·5	+26·5	+81·25
With precaution	3.	−10	− 6	+12
	4.	+ 8·5	− 4·5	−25·25
	5.	−25·25	−15	
	6.	−19·5	− 8·5	−16·75

It was found by direct experiments that the loss of electricity upon B, and consequently upon the ball, during three minutes amounted to 39–44 scale-divisions. Between the action of cold and copiously evaporating sea-water was a difference of only 4 to 13 scale-divisions (0·088 to 0·288 Daniell) in those experiments made with precaution against dust-particles. The kind of electricity upon the ball was the opposite to that upon the sea-water. Had convection occurred, the reverse must have evidently taken place; and from the high potential of the sea-water, a strong convection of electricity was to be expected.

(*e*) To experiment with evaporating mercury, the following changes were made in the arrangement of the apparatus. In place of the condensing-disk C there was used a platinum disk of 12 centim. diameter, which could be connected at will by means of platinum wires, either directly with the needle of the electrometer or with the condenser-plate B. The porcelain dish in the sand-bath was replaced by an iron dish containing the mercury to be evaporated. Platinum or iron was necessary, because all metals which amalgamate with mercury show peculiar electric action, the causes of which I have not yet succeeded in detecting. At 200° C. the mercury vapour was visibly condensed upon the platinum disk C, and very copiously from 200° to 360° C. It is necessary in the experiments that the surface of the mercury be kept perfectly free from oxide.

First, the experiments were conducted according to the method on page 220; *i. e.* the platinum disk C was in direct connexion with the electrometer-needle, and after the mercury was swung under C was insulated with the needle. During the two minutes while the mercury was under C, and electrified by 480 zinc-water-copper elements, the electrometer-needle showed the following deflections:—

70 scale-divisions = 1 Daniell.

Positive charge of 404 Daniells.

Experiment.	Mercury at room-temp.	Mercury at 360° C.
1.	+ 8	+14
2.	+10	+15
3.	+ 6	+ 5
4.	+ 4	+ 5

Secondly, the mercury was charged by a Töpler electrical machine to a high potential, and observations were made according to the method described on page 218; *i. e.* the disk C was connected with the condenser-plate B of the Kohlrausch condenser, and with this was insulated before the mercury was swung under C. The plate A was ½ millim. distant from B, and during the two minutes of the experiment was connected with the earth. After the mercury was swung away from C, A was first insulated and then separated from B. By this method the insulating-power of B and C evidently comes into consideration. The results of this method are as follows:—

	Mercury at room-temp.	Mercury at 360° C.
Positive charge	−2 scale-div.	+3·75 scale-div.
	−4 ,,	
Negative charge	−4 ,,	+5 ,,
	−2 ,,	
	0 ,,	+3 ,,

In both methods the mercury, when rapidly evaporating, condensed in drops upon the platinum disk C. Since, according to the present view, a molecule of mercury contains but one atom of mercury, the result of this experiment is the most conclusive of the proofs against the hypothesis investigated by these experiments, and which has been universally held to the present time. This result with mercury, agreeing as it does with the results obtained with the other liquids used, is totally opposed to the hypothesis hitherto entertained, and justifies the assertion that the vapour arising from electrified still surfaces of liquids is electrically neutral.

Physical Institute, Berlin,
March 7, 1883.

XXXII. *An Investigation into the Relations between Radiation, Energy, and Temperature.* *By* Captain Abney, *R.E., F.R.S.*, and Lieut.-Col. Festing, *R.E.**

In the course of researches on the subject of atmospheric absorption of solar radiation, on which we have for some time past been engaged, it incidentally became desirable to ascertain the relation between the radiation from a black body and its temperature—a subject which, though well worn, is by no means exhausted. Sir William Siemens has made the most recent contribution to this subject in a paper lately communicated to the Royal Society, in which he describes his endeavour to solve the question by noting the increase of electrical resistance of platinum wire with increased temperature and by taking the relationship of energy to resistance—a method of much promise, but which appears to us to be defective for the following reasons. Platinum wire is not black at ordinary temperatures, and it is at least doubtful whether it is such a good radiator as carbon; and, secondly, much of the energy must have been dissipated by convection-currents.

It has, however, struck us that a continuation of the experi-

* Communicated by the Authors.

ments which we had some time ago initiated on the radiation from incandescence lamps might lead to results less liable to vitiation from external causes than those obtained from platinum wire.

Having in our possession incandescence lamps of many different patterns, we made careful simultaneous measurements of the radiation and of the energy in each when currents of varying strength were passed through the filaments.

A Grove's battery was employed in preference to a dynamo, because of the greater steadiness of the current obtained. The current was measured in some cases by a tangent galvanometer, and in others by one of Sir W. Thomson's current-meters, or by both in circuit, the difference of potential between the terminals of the lamp being measured by a Thomson's potential galvanometer, and the radiation from the lamp by a thermopile the receiving surface of which was coated with lampblack, and which was connected with a Thomson's reflecting galvanometer of ·5-ohm resistance. The readings were taken of the first deflection caused by the thermoelectric current and checked by the total deflection. The former is the more rapid mode of proceeding, and, we believe, the more accurate of the two, as there is no change of zero-point during the time of observation. A pair of cardboard screens with narrow slits in them were placed between the lamp and the thermopile in such a way as to cut off as much as possible of the radiation from the glass of the lamp, and yet to allow to pass the radiation from a certain length of the filament. Another cardboard screen was interposed in front of the lamp after each observation to cut off all radiation.

The lamps were also heated in an oven to different temperatures up to 350° C., and the change of resistance of the carbon filament measured directly by means of resistance-coils. We hope to be able in a subsequent paper to refer to the measurements of higher temperatures, our investigations in this respect being not quite complete.

The results so far obtained may be summed up as follows:—The current can be expressed as a function of the potential; the radiation, after a certain temperature of the filament has been reached, bears a simple proportion to the energy expended in the lamp; the resistance can be formulated as a function of the energy and therefore of the radiation; and the temperature appears to be nearly a simple function of the resistance.

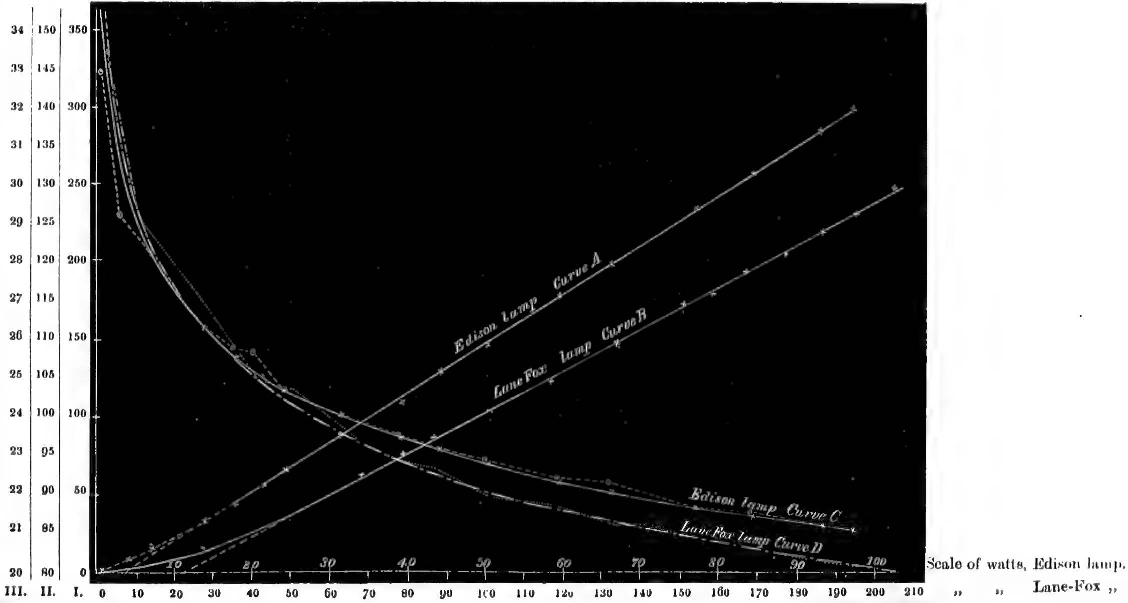
The curves described by using as ordinates and abscissæ the amounts of current and potential in the different experiments have the same general form, which appears from inspection to be that represented by the equation $c=ap+bp^{\frac{3}{2}}$; where c is the current and p the potential. The first term of the right-hand member seems to represent the amount of current which would pass if the filament could be kept at some particular temperature (probably the absolute zero), and the second the additional current due to increased conductivity caused by increase of temperature.

We append in a tabular form the results of typical experiments with four different patterns of lamps in which the dimensions of the carbon filaments differ considerably. These tables give the observed potentials and currents, together with the resistance and energy deduced from them, as well as the results obtained by the application of the above equation to the observed potentials; and it will be seen that these results correspond very closely with those deduced from the observed currents when the resistances have any considerable range. It should be remembered that for small currents, the deflections of the measuring instruments being very small, the percentage error of observation is liable to be large; these observations are therefore not so trustworthy as those of larger currents. It is probable that for these lower temperatures another term should be introduced into the equation, which may be disregarded at temperatures above, say, 400°.

From the above-stated equation another may be deduced for the value of the energy in watts: this would be $w=p^2\,(a+b\,p^{\frac{1}{2}})$; and the resistance in ohms would be $\dfrac{1}{a+bp^{\frac{1}{2}}}$. The form of equation between energy and resistance would be rather more complicated, $w=\left(\dfrac{1-ar}{br}\right)^4\times\dfrac{1}{r}$.

If the curve of current in relation to potential be plotted, it will be found to be fairly smooth. It is, however, evident that a result obtained by the combination of two observations each of which is liable to a small error will probably be further from the truth than that obtained by a direct observation. On this account, in the curve of potential and resistance, the points indicating resistance are more unevenly distributed than those indicating current in the former curve. Similar irregularity will be found in a curve of potential and energy, and still greater in the case of a curve of energy and resistance (see diagram, curves C and D). An inspection of curves A and B in the diagram will show that, as stated above, the radiation is directly proportional to the energy when the latter is above 30 watts; the deviation from the straight line below this point is doubtless due to the temperature of the surroundings being commensurate with that of the filament.

Vertical scale I. for radiation.	Lane-Fox lamp.	Curve A. Edison lamp.	Radiation and energy.
,, ,, ,, ,,	Edison ,,	Curve B. Lane-Fox ,,	,, ,, ,,
,, ,, II. Resistance.	Edison ,,	Curve C. Edison lamp.	Resistance and energy.
,, ,, III. ,,	Lane-Fox ,,	Curve D. Lane-Fox ,,	,, ,, ,,



Z 2

No. of cells.	Potential, volts.	Current, ampères. Obs.	Current, ampères. Calc.	Resistance, ohms. Obs.	Resistance, ohms. Calc.	Energy, watts. Obs.	Energy, watts. Calc.	Radiation.
One Edison 8-candle Lamp.								
5	9·68	·090	·0834	107·5	116·07	8·71	·807	1
6	11·44	·108	·103	106·0	111·07	1·24	1·18	2
10	19·14	·196	·195	98·0	98·15	3·75	3·73	5
15	28·16	·320	·320	88·1	88·00	9·01	9·01	20
20	37·18	·458	·460	81·5	80·47	16·93	17·10	43
23	42·24	·546	·545	77·5	77·50	23·90	23·02	61
25	46·20	·608	·613	76·4	75·37	28·09	28·32	74
27	49·70	·666	·674	74·8	73·74	33·10	33·50	89
30	54·56	·764	·764	71·6	71·57	41·68	41·68	117
32	58·30	·828	·835	70·8	69·82	48·28	48·68	138
34	60·72	·880	·882	69·0	68·62	53·44	53·56	154
35	63·36	·928	·934	68·3	67·84	58·80	59·17	174
36	64·68	·964	·960	67·5	67·37	62·35	62·09	185
37	66·44	1·004	·995	66·5	66·77	66·70	66·11	199
$a = \cdot00471$. $b = \cdot00126$.								
Edison 16-candle Lamp.								
5	10·12	·07	·065	144·7	155·7	·71	·68	1·75
10	19·80	·156	·147	126·9	134·7	3·089	2·911	6
15	29·04	·240	·240	121·0	121·0	6·97	6·97	15
20	38·06	·340	·340	111·8	111·8	14·22	14·22	33
23	44·00	·404	·408	108·9	107 84	17·77	17·95	45
25	47·52	·448	·451	108·3	105·36	21·29	21·43	56
27	51·04	·492	·495	103·65	103·11	24·11	24·26	68
30	56·32	·560	·562	100·57	100·21	31·53	31·54	88
33	61·64	·630	·633	97·84	97·44	38·83	39·01	109
35	65·12	·678	·678	96·04	96·04	44·15	44·15	127
37	68·86	·726	·732	94·85	94·07	50·00	50·40	146
40	73·92	·800	·805	92·40	91·82	59·13	59·50	175
42	77·44	·852	·856	90·89	90·47	66·02	66·31	197
45	82·72	·932	·934	88·72	88·57	77·09	77·26	234
47	86·24	·980	·987	88·00	87·37	84·16	84·56	256
49	89·76	1·040	1·041	86·25	86·22	93·35	93·44	285
50	91·30	1·064	1·064	85·81	85·81	97·17	97·13	299
$a = \cdot00387$. $b = \cdot000816$.								
British Electric-Light Co. Lamp.								
1	1·94	·013	·014	149·2	138·6	·025	·027	
2	3·87	·022	·020	175·9	193·5	·085	·077	
4	7·63	·050	·045	152·6	169·6	·382	·343	
6	11·61	·079	·076	147·0	152·8	·917	·883	
8	15·28	·109	·109	140·2	140·2	1·666	1·666	
10	19·14	·142	·144	134·8	132·9	2·718	2·756	
12	23·01	·184	·184	125·0	125·0	4·232	4·232	
13	24·89	·202	·203	123·2	122·6	5·028	5·053	
15	28·54	·242	·243	117·9	117·4	6·907	6·936	
20	37·84	·352	·352	107·5	107·5	13·32	13·32	
25	46·82	·475	·468	98·6	100·0	22·24	21·91	
30	55·72	·600	·596	92·9	93·5	33·43	33·21	
35	64·72	·730	·730	88·7	88·7	47·25	47·25	
40	73·26	·856	·856	85·6	85·6	62·71	62·71	
$a = \cdot00315$. $b = \cdot00101$.								

Maxim Lamp.

No. of cells.	Potential, volts.	Current, ampères. Obs.	Current, ampères. Calc.	Resistance, ohms. Obs.	Resistance, ohms. Calc.	Energy, watts. Obs.	Energy, watts. Calc.	Radiation.
2	3·96	·048	·054	82·5	73·3	·19	·21	
4	7·82	·102	·114	76·4	68·7	·80	·90	
6	11·61	·165	·180	77·0	64·5	1·92	2·01	
8	15·31	·234	·245	65·4	62·4	3·57	3·75	
10	18·92	·309	·312	61·3	60·7	5·80	5·9	
12	22·70	·380	·386	58·7	58·9	8·60	8·8	
15	28·13	·497	·495	56·6	56·5	14·0	14·0	
20	37·00	·695	·690	53·5	53·6	25·8	25·5	
25	45·87	·892	·891	51·4	51·4	40·9	40·7	
30	54·09	1·11	1·09	48·7	49·5	60·0	59·5	
35	62·40	1·295	1·30	48·0	48·0	81·0	81·2	
40	70·56	1·50	1·503	47·0	46·9	106·0	106·2	

$a = \cdot 0113.$ $b = \cdot 0012.$

Maxim Lamp.

No. of cells.	Potential, volts.	Current, ampères. Obs.	Current, ampères. Calc.	Resistance, ohms. Obs.	Resistance, ohms. Calc.	Energy, watts. Obs.	Energy, watts. Calc.	Radiation.
6	11·61	·185	·180	62·7	64·2	2·148	2·090	3
7	13·76	·211	·231	65·2	59·5	2·904	3·179	5
8	15·70	·246	·268	63·8	58·5	3·862	4 208	6
10	19·35	·317	·340	61·0	56·7	6·134	6·579	13
12	22·80	·387	·409	59·0	55·8	8·824	9·325	18
15	27·95	·510	·518	54·8	54·0	14·26	14·48	36
20	36·76	·713	·713	51·6	51·6	26·21	26·21	73
25	45·00	·915	·904	49·2	49·7	41·18	40·68	125
30	53·00	1·11	1·10	47·8	48·4	58·83	58·09	181
35	60·80	1·31	1·30	46·4	47·0	79·65	78·74	253
40	68·50	1·50	1·50	45·7	45·7	102·75	102·41	341
45	76·30	1·71	1·71	44·6	44·6	130·47	130·47	447
50	83·90	1·90	1·91	44·1	43·9	159·41	160·42	532

$a = \cdot 0127.$ $b = \cdot 0011.$

Lane-Fox Lamp.

No. of cells.	Potential, volts.	Current, ampères. Obs.	Current, ampères. Calc.	Resistance, ohms. Obs.	Resistance, ohms. Calc.	Energy, watts. Obs.	Energy, watts. Calc.	Radiation.
5	9·03	·28	·26	32·55	34·73	2·53	2·35	3
10	18·06	·62	·613	29·13	29·36	11·20	11·19	5
15	27·09	1·00	1·03	27·09	26·30	27·00	27·90	14
20	35·26	1·43	1·45	24·66	24·32	50·41	51·12	40
23	39·99	1·71	1·71	23·37	23·37	68·39	68·39	62
25	42·56	1·86	1·86	22·89	22·89	79·17	79·17	77·5
28	47·30	2·15	2·14	22·00	22·10	101·69	101·22	104·5
30	50·53	2·32	2·33	21·78	21·68	117·23	117·73	126
32	53·54	2·50	2·52	21·41	21·24	133·85	135·91	145
33	54·83	2·58	2·59	21·25	21·17	141·46	140·91	155
35	57·62	2·75	2·77	20·95	20·80	158·45	159·59	180
36	58·91	2·85	2·84	20·67	20·74	166·89	166·30	193
37	60·20	2·94	2·94	20·47	20·47	170·99	170·99	205
38	61·49	3·03	3·036	20·29	20·22	186·31	186·38	220
40	64·07	3·20	3·20	20·02	20·02	205·02	205·02	245

XXXIII. *Table of Totients, of Sum-totients, and of* $3\pi^2$ *into the Squares, of all the Numbers from* 501 *to* 1000 *inclusive.* *By* J. J. Sylvester*.

n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2}n^2$	n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2}n^2$	n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2}n^2$
501	332	76448	76295·15	542	270	89452	89293·54	583	520	103556	103313·87
502	250	76698	76600·03	543	360	89812	89623·34	584	288	103844	103668·60
503	502	77200	76905·52	544	256	90068	89953·75	585	288	104132	104023·93
504	144	77344	77211·61	545	432	90500	90284·77	586	292	104424	104379·87
505	400	77744	77518·31	546	144	90644	90616·39	587	586	105010	104736·42
506	220	77964	77825·62	547	546	91190	90948 62	588	168	105178	105093·58
507	312	78276	78133·54	548	272	91462	91281·46	589	540	105718	105451·35
508	252	78528	78442·06	549	360	91822	91614·91	590	232	105950	105809·72
509	508	79036	78751·19	550	200	92022	91948·97	591	392	106342	106168·70
510	128	79164	79060·93	551	504	92526	92283·64	592	288	106630	106528·29
511	432	79596	79371·28	552	176	92702	92618·91	593	592	107222	106888·49
512	256	79852	79682·23	553	468	93170	92954·79	594	180	107402	107249·29
513	324	80176	79993·79	554	276	93446	93291·28	595	384	107786	107610·70
514	256	80432	80305·96	555	288	93734	93628·38	596	296	108082	107972·72
515	408	80840	80618·74	556	276	94010	93966·08	597	396	108478	108335·35
516	168	81008	80932·13	557	556	94566	94304·39	598	264	108742	108698·59
517	460	81468	81246·12	558	180	94746	94643·31	599	598	109340	109062·43
518	216	81684	81560·72	559	504	95250	94982·84	600	160	109500	109426·88
519	344	82028	81875·93	560	192	95442	95322·98	601	600	110100	109791·94
520	192	82220	82191·75	561	320	95762	95663·72	602	252	110352	110157·61
521	520	82740	82508·18	562	280	96042	96005·07	603	396	110748	110523·89
522	168	82908	82825·21	563	562	96604	96347·03	604	300	111048	110890·77
523	522	83430	83142·85	564	184	96788	96689·60	605	440	111488	111258·26
524	260	83690	83461·10	565	448	97236	97032·77	606	200	111688	111626·36
525	240	83930	83779·95	566	282	97518	97376·55	607	606	112294	111995·07
526	262	84192	84099·42	567	324	97842	97720·94	608	288	112582	112364·39
527	480	84672	84419·49	568	280	98122	98065·94	609	336	112918	112734·31
528	160	84832	84740·17	569	568	98690	98411·55	610	240	113158	113104·84
529	506	85338	85061·46	570	144	98834	98757·76	611	552	113710	113475·98
530	208	85546	85383·36	571	570	99404	99104·58	612	192	113902	113847·73
531	348	85894	85705·87	572	240	99644	99452·01	613	612	114514	114220·09
532	216	86110	86028·98	573	380	100024	99800·05	614	306	114820	114593·05
533	480	86590	86352·70	574	240	100264	100148·70	615	320	115140	114966·62
534	176	86766	86677·03	575	440	100704	100497·95	616	240	115380	115340·80
535	424	87190	87001·97	576	192	100896	100847·81	617	616	115996	115715·59
536	264	87454	87327·51	577	576	101472	101198·28	618	204	116200	116090·99
537	356	87810	87653·66	578	272	101744	101549·36	619	618	116818	116466·99
538	268	88078	87980·42	579	384	102128	101901·05	620	240	117058	116843·60
539	420	88498	88307·79	580	224	102352	102253·34	621	396	117454	117220·82
540	144	88642	88635·77	581	492	102844	102606·24	622	310	117764	117598·65
541	540	89182	88964·35	582	192	103036	102959·75	623	528	118292	117977·08

* Continued from Phil. Mag. for April, p. 257, where the Table extends from 1 to 500 inclusive, and in which the following misprints occur:—

In the column of $\frac{3}{\pi^2}n^2$, opposite 33, *for* 333·01 *read* 331·01
" " " 303, *for* 28906·59 *read* 27906·59
" " " 401, *for* 48879·64 *read* 48877·64

In the extended as well as in the original Table it will be seen that the sum-totient is always intermediate between $3/\pi^2 . n^2$ and $3/\pi^2 . (n+1)^2$.

The formula of verification applied at every tenth step to the T column precludes the possibility of the existence of other than typographical errors or errors of transcription. Accumulative errors are rendered impossible.

Table (*continued*).

n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2}n^2$	n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2}n^2$	n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2}n^2$
624	192	118484	118356·12	679	576	140474	140139·66	734	366	164066	163762·18
625	500	118984	118735·77	680	256	140730	140552·75	735	336	164402	164208·70
626	312	119296	119116·03	681	452	141182	140966·44	736	352	164754	164655·83
627	360	119656	119496·90	682	300	141482	141380·74	737	660	165414	165103·57
628	312	119968	119878·37	683	682	142164	141795·65	738	240	165654	165551·92
629	576	120544	120260·45	684	216	142380	142211·17	739	738	166392	166000·87
630	144	120688	120643·14	685	544	142924	142627·30	740	288	166680	166450·43
631	630	121318	121026·44	686	294	143218	143044·03	741	432	167112	166900·60
632	312	121630	121410·35	687	456	143674	143461·37	742	312	167424	167351·38
633	420	122050	121794·86	688	336	144010	143879·32	743	742	168166	167802·77
634	316	122366	122179·98	689	624	144634	144297·88	744	240	168406	168254·76
635	504	122870	122565·71	690	176	144810	144717·05	745	592	168998	168707·36
636	208	123078	122952·05	691	690	145500	145136·82	746	372	169370	169160·57
637	504	123582	123338·00	692	344	145844	145557·20	747	492	169862	169614·39
638	280	123862	123726·55	693	360	146204	145978·19	748	320	170182	170068·82
639	420	124282	124114·71	694	346	146550	146399·79	749	636	170818	170523·85
640	256	124538	124503·48	695	552	147102	146821·99	750	200	171018	170979·50
641	640	125178	124892·86	696	224	147326	147244·80	751	750	171768	171435·75
642	212	125390	125282·85	697	640	147966	147668·22	752	368	172136	171892·61
643	642	126032	125673·44	698	348	148314	148092·25	753	500	172636	172350·07
644	264	126296	126064·64	699	464	148778	148516·89	754	336	172972	172808·14
645	336	126632	126456·45	700	240	149018	148942·14	755	600	173572	173266·82
646	288	126920	126848·87	701	700	149718	149367·99	756	216	173788	173726·11
647	646	127566	127241·89	702	216	149934	149794·45	757	756	174544	174186·01
648	216	127782	127635·52	703	648	150582	150221·52	758	378	174922	174646·52
649	580	128362	128029·76	704	320	150902	150649·20	759	440	175362	175107·63
650	240	128602	128424·60	705	368	151270	151077·48	760	288	175650	175569·35
651	360	128962	128820·06	706	352	151622	151506·37	761	760	176410	176031·68
652	324	129286	129216·12	707	600	152222	151935·87	762	252	176662	176494·62
653	652	129938	129612·79	708	232	152454	152365·98	763	648	177310	176958·16
654	216	130154	130010·07	709	708	153162	152796·70	764	380	177690	177422·31
655	520	130674	130407·96	710	280	153442	153228·02	765	384	178074	177887·07
656	320	130994	130806·46	711	468	153910	153659·95	766	382	178456	178352·44
657	432	131426	131205·56	712	352	154262	154092·49	767	696	179152	178818·42
658	276	131702	131605·27	713	660	154922	154525·64	768	256	179408	179285·00
659	658	132360	132005·59	714	192	155114	154959·40	769	768	180176	179752·19
660	160	132520	132406·52	715	480	155594	155393·76	770	240	180416	180219·99
661	660	133180	132808·06	716	356	155950	155828·73	771	512	180928	180688·40
662	330	133510	133210·20	717	476	156426	156264·31	772	384	181312	181157·42
663	384	133894	133612·95	718	358	156784	156700·50	773	772	182084	181627·04
664	328	134222	134016·31	719	718	157502	157137·30	774	252	182336	182097·27
665	432	134654	134420·28	720	192	157694	157574·70	775	600	182936	182568·11
666	216	134870	134824·86	721	612	158306	158012·71	776	384	183320	183039·56
667	616	135486	135230·04	722	342	158648	158451·33	777	432	183752	183511·61
668	332	135818	135635·83	723	480	159128	158890·56	778	388	184140	183984·28
669	444	136262	136042·23	724	360	159488	159330·40	779	720	184860	184457·55
670	264	136526	136449·24	725	560	160048	159770·84	780	192	185052	184931·43
671	600	137126	136856·86	726	220	160268	160211·89	781	700	185752	185405·92
672	192	137318	137265·08	727	726	160994	160653·55	782	352	186104	185881·01
673	672	137990	137673·91	728	288	161282	161095·82	783	504	186608	186356·71
674	336	138326	138083·35	729	486	161768	161538·69	784	336	186944	186833·02
675	360	138686	138493·40	730	288	162056	161982·17	785	624	187568	187309·94
676	312	138998	138904·05	731	672	162728	162426·26	786	260	187828	187787·47
677	676	139674	139315·31	732	240	162968	162870·96	787	786	188614	188265·60
678	224	139898	139727·18	733	732	163700	163316·27	788	392	189006	188744·34

Table (*continued*).

n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2}n^2$	n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2}n^2$	n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2}n^2$
789	524	189530	189223·69	844	420	216800	216524·18	899	840	246086	245663·65
790	312	189842	189703·65	845	624	217424	217037·57	900	240	246326	246210·48
791	672	190514	190184·22	846	276	217700	217551·58	901	832	247158	246757·91
792	240	190754	190665·39	847	660	218360	218066·19	902	400	247558	247305·96
793	720	191474	191147·17	848	416	218776	218581·40	903	504	248062	247854·61
794	396	191870	191629·56	849	564	219340	219097·23	904	448	248510	248403·88
795	416	192286	192112·56	850	320	219660	219613·66	905	720	249230	248953·75
796	396	192682	192596·17	851	792	220452	220130·71	906	300	249530	249504·22
797	796	193478	193080·39	852	280	220732	220648·36	907	906	250436	250055·31
798	216	193694	193565·21	853	852	221584	221166·62	908	452	250888	250607·00
799	736	194430	194050·64	854	360	221944	221685·48	909	600	251488	251159·31
800	320	194750	194536·67	855	432	222376	222204·96	910	288	251776	251712·22
801	528	195278	195023·32	856	424	222800	222725·04	911	910	252686	252265·73
802	400	195678	195510·57	857	856	223656	223245·73	912	288	252974	252819·86
803	720	196398	195998·43	858	240	223896	223767·03	913	820	253794	253374·59
804	264	196662	196486·90	859	858	224754	224288·93	914	456	254250	253929·93
805	528	197190	196975·98	860	336	225090	224811·44	915	480	254730	254485·88
806	360	197550	197465·66	861	480	225570	225334·56	916	456	255186	255042·44
807	536	198086	197955·96	862	430	226000	225858·29	917	780	255966	255599·61
808	400	198486	198446·86	863	862	226862	226382·62	918	288	256254	256157·38
809	808	199294	198938·37	864	288	227150	226907·57	919	918	257172	256715·76
810	216	199510	199430·48	865	688	227838	227433·12	920	352	257524	257274·75
811	810	200320	199923·21	866	432	228270	227959·28	921	612	258136	257834·34
812	336	200656	200416·54	867	544	228814	228486·05	922	460	258596	258394·55
813	540	201196	200910·48	868	360	229174	229013·43	923	840	259436	258955·36
814	360	201556	201405·03	869	780	229954	229541·41	924	240	259676	259516·78
815	648	202204	201900·19	870	224	230178	230070·01	925	720	260396	260078·81
816	256	202460	202395·95	871	792	230970	230599·21	926	462	260858	260641·45
817	756	203216	202892·32	872	432	231402	231129·02	927	612	261470	261204·69
818	408	203624	203389·30	873	576	231978	231659·43	928	448	261918	261768·55
819	432	204056	203886·89	874	396	232374	232190·46	929	928	262846	262333·01
820	320	204376	204385·09	875	600	232974	232722·09	930	240	263086	262898·07
821	820	205196	204883·89	876	288	233262	233254·33	931	756	263842	263463·75
822	272	205468	205383·30	877	876	234138	232787·18	932	464	264306	264030·03
823	822	206290	205883·32	878	438	234576	234320·64	933	620	264926	264596·93
824	408	206698	206383·95	879	584	235160	234854·70	934	466	265392	265164·43
825	400	207098	206885·19	880	320	235480	235389·37	935	640	266032	265732·53
826	348	207446	207387·03	881	880	236360	235924·65	936	288	266320	266301·25
827	826	208272	207889·48	882	252	236612	236460·54	937	936	267256	266870·57
828	264	208536	208392·54	883	882	237494	236997·04	938	396	267652	267440·51
829	828	209364	208896·21	884	384	237878	237534·14	939	624	268276	268011·05
830	328	209692	206400·49	885	464	238342	238071·85	940	368	268644	268582·19
831	552	210244	209905·37	886	442	238784	238610·17	941	940	269584	269153·95
832	384	210628	210410·86	887	886	239670	239149·10	942	312	269896	269726·31
833	672	211300	210916·96	888	288	239958	239688·64	943	880	270776	270299·28
834	276	211576	211423·67	889	756	240714	240228·78	944	464	271240	270872·86
835	664	212240	211930·98	890	352	241066	240769·53	945	432	271672	271447·05
836	360	212600	212438·91	891	540	241606	241310·89	946	420	272092	272021·84
837	540	213140	212947·44	892	444	242050	241852·86	947	946	273038	272597·25
838	418	213558	213456·58	893	828	242878	242395·43	948	312	273350	273173·26
839	838	214396	213966·32	894	296	243174	242938·62	949	864	274214	273749·88
840	192	214588	214476·68	895	712	243886	243482·41	950	360	274574	274327·10
841	812	215400	214987·64	896	384	244270	244026·81	951	632	275206	274905·94
842	420	215820	215499·21	897	528	244798	244571·81	952	384	275590	275483·38
843	560	216380	216011·39	898	448	245246	245117·43	953	952	276542	276062·43

Table (*continued*).

n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2}n^2$	n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2}n^2$	n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2}n^2$
954	312	276854	276642·09	970	384	286076	285999·30	986	448	295832	295512·15
955	760	277614	277222·36	971	970	287046	286589·30	987	552	296384	296111·87
956	476	278090	277803·23	972	324	287370	287179·90	988	432	296816	296712·20
957	560	278650	278384·71	973	828	288198	287771·11	989	924	297740	297313·14
958	478	279128	278966·80	974	486	288684	288362·92	990	240	297980	297914·68
959	816	279944	279549·50	975	480	289164	288955·35	991	990	298970	298516·83
960	256	280200	280132·81	976	480	289644	289548·39	992	480	299450	299119·59
961	930	281130	280716·72	977	976	290620	290142·03	993	660	300110	299722·96
962	432	281562	281301·24	978	324	290944	290736·28	994	420	300530	300326·94
963	636	282198	281886·37	979	880	291824	291331·13	995	792	301322	300931·52
964	480	282678	282472·11	980	336	292160	291926·60	996	328	301650	301536·71
965	768	283446	283058·46	981	648	292808	292522·67	997	996	302646	302142·51
966	264	283710	283645·41	982	490	293298	293119·35	998	498	303144	302748·92
967	966	284676	284232·97	983	982	294280	293716·64	999	648	303792	303355·93
968	440	285116	284821·14	984	320	294600	294314·54	1000	400	304192	303963·55
969	576	285692	285409·92	985	784	295384	294913·04				

XXXIV. *Notices respecting New Books.*

In Memoriam Dominici Chelini. (*Collectanea Mathematica, nunc primum edita cura et studio* L. Cremona *et* E. Beltrami.) Milan: Hoepli.

Of late years it has become a fashion, as it is in many cases a "necessity," to print in a collected form the scattered papers of our more eminent mathematicians. This is a good service both to the dead and to the living, but it is not the service rendered in the "memorial" volume before us. Rather is this a memorial cairn, the constituent stones of which are the twenty-nine varied but all highly polished stones, *i. e.* memoirs, contributed by the friendly hands of the foremost living* mathematicians of Europe. Chelini's life, "semplice e modesta quanto operosa," seems to have well merited such loving distinction.

Dominic Chelini was born, October 18, 1802, at Gragnano, and died November 16, 1878. He studied at the Collegio Nazareno from 1819 to 1826, and in 1827 was consecrated Priest. Subsequently, from 1831 to 1851, he filled the Mathematical chair at the above-named college. In 1843–4 he made the acquaintance of Jacobi, and subsequently of Lejeune-Dirichlet, Steiner, Schlæfli, and Borchardt, and always retained their regard and friendship. In 1851 he was nominated Professor of Mechanics and Hydraulics at the University of Bologna, a post he occupied until 1860. He

* Before the volume was issued, one of the most illustrious of these, Herr Borchardt, had passed away; and the further interest is attached to his paper of its being among the last he penned. More recently English mathematicians have mourned the loss of Prof. Henry Smith.

was allowed for a time to retain this post; but by a decree of the 18th December, 1864, he was deprived of his chair, as he refused to take the oath to the Government. Prof. Cremona writes;—"Il Chelini amava sinceramente la patria italiana ed era assolutamente alieno dall' associarsi a qualsiasi atto ostile al governo nazionale: dei quali suoi sentimenti gli amici intimi possono fare ampia testimonianza. . . . . . Il Chelini sopportò la sua disgrazia con ammirabile serenità d'animo." After a brief sojourn at Lucca, Chelini was called in 1867 to take charge of the chair of Rational Mechanics in the University of Rome. His occupancy of this post was short, as four years later Rome became the capital of Italy. In the spring of 1878 a small yearly pension was voted to him; but he did not long enjoy this, as he died in the same year, in the Collegio Nazareno, where he had resided since his return to Rome in 1865.

His was an uneventful life in the eyes of politicians and men of the world, but one spent in the production of several valuable mathematical memoirs and in the acquiring, to a remarkable degree, the affection of mathematicians of many nationalities. Hirst, amongst our own countrymen, was an especial friend, a friendship commenced at Bologna in 1864 having been subsequently renewed and increased at Rome.

Chelini's works range over a space of 44 years, and are 53 in number. His first paper was "Sulla teoria delle quantità proportionali" (read in July 1834); his last was a memoir "Sopra alcune questioni dinamiche" (presented April 1877). Our own knowledge of Chelini's works, prior to our study of the volume before us, was confined to the two or three passages in which memoirs of his are analyzed in Chasles's *Rapport sur les progrès de la Géométrie*. Ample justice is done to Chelini's labours, we believe, in the exhaustive Analytical Sketch prefixed to the Collectanea, which has been drawn up by Signor Beltrami: to the biography in this sketch, founded upon an address by Prof. Cremona, we are indebted for the few details we have recorded.

We now proceed to a brief examination of the memoirs in the order in which they are presented to us; and here we may state, once for all, that some of the "stones" are of such a kind (*i. e.* so mathematically technical) that we can only name them, and not examine them in any detail. The "Sur les Fonctions $\Theta(x)$ et $\mathrm{H}(x)$ de Jacobi," by M. Hermite, is of this character, though the process of solution is a very direct one. "L'Iperboloide centrale nella rotazione de' Corpi," F. Siacci. The property discovered may be thus stated:—"When a body, not acted on by any external forces, turns round a fixed point, a certain hyperboloid connected (*legato*) with the body and such that its axes coincide with the principal axes of inertia relatively to the fixed point, rolls without sliding on a right circular cylinder whose axis passes through the fixed point and is parallel to the axis of the impulsive couple (*coppia d' impulso*)." "On a Differential Equation," by A. Cayley: this is an equation considered by Kummer in his memoir on Hypergeometric Series.

"Sulle Cubiche ternarie sizigetiche," G. Battaglini: discusses several properties relating to a cubic and its Cayleyan, and generalizes some theorems of Clebsch on polar conics and poloconics. "On the Complexes generated by two Correlative Planes," T. A. Hirst: this is connected with Dr. Hirst's paper "On the Correlation of two Planes" (Proc. Lond. Math. Soc. vol. v.). "Nota sopra alcuni Iperboloidi annessi alla cubica gobba," E. D'Ovidio: commences with a *résumé* of results got in the author's "Studio sulle cubiche gobbe," and then, amongst other matters, specially treats of the properties of hyperboloids through three chords of a cubic. "Constructions planes des Eléments de Courbure de la surface de l'onde," A. Mannheim: starts from MacCullagh's generation of the wave-surface and construction for the normal at any point, and then determines the elements of curvature of the surface* (in this we find the author's definition of *normalie*, a term he employs elsewhere without defining it). "Sulla integrazione delle equazioni a derivate parziali del primo ordine," E. Padova: is an extension of a method of Ampère's to any number of Variables, and shows how it leads (avoiding a difficulty indicated by Bertrand) to the method given by Cauchy. "De fractionibus quibusdam continuis," H. J. S. Smith: discusses the equations $P_1P_2-2R^2=\pm 1$, $P_1P_2-3R^2=\pm 1$ (adopting a lemma due to Sylvester), and gives certain developments of square roots in the form of continued fractions. "Sopra i sistemi lineari triplamente infiniti di curve algebriche piane," E. Caporali: the object of this memoir is the solution of the principal problems of numerical (*enumerativa*) geometry which present themselves in the study of the triply infinite linear systems of plane curves. The investigation is founded on stereometric considerations; and the properties obtained are got from the study of a surface represented point by point on the plane of the linear systems, so that the plane sections have for images the curves of the linear system. "Intorno ad una generalizzazione di alcuni teoremi di Meccanica," V. Cerruti; "Sugli assi di Equilibrio," G. Bardelli; "Sur l'équation de Riccati," G. Darboux†: want of space forbids our noticing these three papers as we could wish; we must then pass on. "Sur deux Algorithmes analogues à celui de la moyenne Arithmético-géométrique de deux Eléments," C. W. Borchardt: this friendly letter, written on a sick-bed, applies an elegant analysis to what is sometimes called Schwab's series [discussed in Hansen's 'Theory of Perturbations,' by Glaisher ("Solutions of Cambridge Senate-House Problems, 1878"), M. J. Tannéry, and others]. "Sopra

* The author says his solutions "reposent sur la représentation géométrique d'un élément de surface réglée au moyen d'une droite auxiliaire et sur la représentation d'un pinceau de droites au moyen d'une circonférence et d'un point."

† The author applies a covariantive property to the solution of an equation studied by Prof. Cayley ('Messenger of Mathematics,' 1874, p. 69).

una forma binaria dell' ottavo ordine," and "Il risultante di due forme binarie l'una cubica e l'altra biquadratica:" two notes by F. Brioschi. "Ueber potentiale n-facher Mannigfaltigkeiten," L. Kronecker: gives a very simple expression for the potential of an ellipsoid, of n-plicity, which is not referred to its axes. "Sopra la propagazione del Calore," E. Betti: treats of the movement of heat in an indefinite isotropic medium, all whose points are of zero temperature with the exception of points contained in a certain determinate space. "Ueber quadratische Kugelcomplexe und confocale Cycliden," T. Reye: extends the notion of a normal to spherical linear complexes, and obtains a system of orthogonal and homofocal cyclides. Alcuni teoremi sulle funzioni di una variabile complessa," U. Dini: discusses theorems due to Weierstrass, Betti, and Mittag-Leffler. "Einige Bemerkungen über die Lamé'schen Funktionen," L. Schläfli; "Ueber die Abspiegelung der Sonnenfleckenperiode in den zu Rom beobachteten magnetischen Variationen," R. Wolf; "Ueber die dreifachen Secanten einer algebraischen Raumcurve," C. F. Geiser: this last discusses a problem in numerical geometry. "Una formola fondamentale concernente i discriminanti delle Equazioni differenziali e delle loro primitive complete," F. Casorati: finds a relation between the discriminants of a differential equation and of its primitive. "Sulle curve gobbe razionali del 5° ordine," E. Bertini: this is an investigation of some properties of *quintics* (skew, rational and without double points) which the author ranges under two species. "Sui momenti *obliqui* di un Sistema di Punti e sull' 'imaginäres Bild' di Hesse," G. Jung: we must refer our readers to the paper for the definition of *oblique* moments &c. "Sulla teoria degli assi di rotazione," E. Beltrami: an excellent paper, which fittingly finds a place here, as it follows on the lines of Chelini's "Elementi di meccanica razionale" (and of Prof. Turazzo's "Il moto dei Sistemi rigidi"). "Intorno ad un Testamento inedito di Nicolò Tartaglia," an autographic reproduction which is presented with all Signor Boncompagni's well-known wealth of bibliographical illustration. An interesting fact which comes out is the settlement of the date of Tartaglia's death ("a sette ore della notte dal lunedì 13 al martedì 14 dicembre del 1557") against the erroneous dates given by Libri, Poggendorff, Hankel, and others. "Sopra una certa superficie di quart' ordine," L. Cremona: treats of the surface generated by projective pencils (fasci) $[S_1+\lambda S_2=0,\ S_3+\lambda S_4=0]$ of surfaces of the second degree, on the supposition that the surfaces $(S=0)$ touch at the same point.

ROBERT TUCKER, *M.A.*

XXXV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 159.]

June 20, 1883.—J. W. Hulke, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "On the Discovery of *Ovibos moschatus* in the Forest-bed, and its Range in Space and Time." By Prof. W. Boyd Dawkins, M.A., F.R.S., F.G.S.

2. "On the Relative Age of some Valleys in Lincolnshire." By A. J. Jukes-Browne, Esq., B.A., F.G.S.

In a country which is traversed by a series of escarpments or hill-ranges, the valleys by which its drainage is effected are usually separable into two sets or systems—one parallel to the strike of the ridges, and the other more or less at right angles to the same. The origin of these longitudinal and transverse valleys has been explained by Mr. Jukes, who has shown that the course of a stream flowing in a transverse valley and crossing a longitudinal valley is not likely to be diverted, unless something happens to cut a deeper channel down the longitudinal valley to the sea. The possibility of this will depend upon the strike of the rocks and the trend of the sea-coast; where a longitudinal valley or the interspace between two escarpments abuts upon the coast, and is occupied by a stream running into the sea, the process of erosion may carry the sources of this stream back so far as to intercept the waters of a transverse river crossing a higher part of the same general depression or interspace. The author believes that this has happened in some instances, and notably in the case of two Lincolnshire valleys.

1. *Valleys of the Steeping and Calceby Becks.*—These two streams have their sources near one another in the vicinity of Telford. The Calceby beck occupies a transverse valley, and flows north-east to the Saltfleet marshes; the Steeping flows in a longitudinal valley south-east to the fens by Wainfleet.

The disposition of the Boulder-clays (Hessle and Purple) along the eastern border of the chalk wolds affords a criterion of the relative age of the valleys which open eastward, some being older and some newer than the formation of those clays. Glacial clays and gravels are found continuously along the Calceby valley, and occur also in the valleys of its tributaries; the same deposits sweep round the southern end of the Wold hills into the entrance of the Steeping valley, but do not run into it; hence it would appear that the Steeping valley is of later date than the Calceby valley.

Facts were given in support of the hypothesis that the Steeping valley has been rapidly developed and enlarged by the combined action of rain and springs, and that its backward extension has caused the interception of certain streams that originally flowed into the Calceby valley. As a matter of fact, the Steeping valley now extends behind the abrupt termination of a broad transverse valley, which is continuous with that of the Calceby beck, while the Telford beck, which appears to have originally been a tributary of the Calceby stream, now runs into the Steeping; and its peculiar course illustrates the manner in which this and other streams have been diverted from their original channels.

2. *Valleys of the Trent and Witham.*—The Trent flows in a transverse valley as far as Newark, and is then suddenly deflected northward into a longitudinal valley. Proofs are given that its ancient course was eastward, by Lincoln to the Fens; and a remarkable series of old river-gravels are described, which mark out the former courses of the rivers Trent, Witham, and Devon.

The longitudinal valley along which the Trent now flows, from Newark to Gainsborough, may have been excavated in the first instance by a tributary of the Idle; the recession of this valley towards that of the Trent (assisted by other causes) probably led to the diversion of the latter river from its original transverse valley into that of the Humber.

The study of these changes in the river-courses of Lincolnshire leads to the conclusion that whenever a succession of ridges and depressions has been developed out of the surface of a country, a river crossing any one of the longitudinal valleys which happens to stretch to the sea-coast is liable to diversion by the backward extension of a stream draining directly into the sea from the termination of the longitudinal valley.

3. "On the Section at Hordwell Cliffs, from the top of the Lower Headon to the base of the Upper Bagshot Sands." By the late E. B. Tawney, Esq., M.A., F.G.S., and H. Keeping, Esq., of the Woodwardian Museum. Communicated by the Rev. Osmond Fisher, M.A., F.G.S.

The authors, after a brief sketch of the literature of the subject and of the method which they have adopted in measuring the beds in the Hordwell section, passed on to describe these, viz. the freshwater Lower Headon series and the so-called Upper Bagshot Sands of the Geological Survey. They make the whole thickness of the former $83\frac{1}{2}$ feet. The bed numbered 32 in their section they identified with the Howledge Limestone on the other side of the Solent. It is almost the highest seen in the section, and underlies the true Middle Headon, which is now no longer exposed. The authors pointed out that in their opinion the late Marchioness of Hastings and Dr. Wright have somewhat misapprehended the position of these several beds. Details were then given of the remainder of the section, and comparisons made with the details published by former authors; after which the authors described the underlying estuarine series, or Upper Bagshot Sands, which has a thickness of $17\frac{1}{2}$ feet.

4. "On some new or imperfectly known Madreporaria from the Coral Rag and Portland Oolite of the Counties of Wilts, Oxford, Cambridge, and York." By R. F. Tomes, Esq., F.G.S.

5. "The Geology of Monte Somma and Vesuvius, being a Study in Vulcanology." By H. J. Johnston-Lavis, Esq., F.G.S.

The author, after referring to the vast amount of literature which has appeared dealing with the same subject, stated that his object was to lay before the Society the results of his personal observations.

The external form and general features of Monte Somma having been described, the origin of the present condition of the volcano was discussed in some detail, and the geological structure of the mountain and of the surrounding plain, as revealed by well-sections, was carefully considered.

As the result of his observations, the author believes that he is able to define eight successive phases in the history of the volcano; and the events which took place during these several periods, with the products of the eruption during each, were discussed in detail.

The earliest certainly recognized phase in the history of the mountain was distinguished by chronic activity, exhibited in outflows of lava and the ejection of scoria and ash. Possibly, however, a still earlier and paroxysmal stage is indicated by some of the phenomena described.

Phase II. was a period of inactivity and denudation, which was brought to a close by the violent paroxysms of Phase III., followed by the chronic activity of Phase IV. Phase V. marks the return of a period of inactivity and denudation, which was again followed by the paroxysms of Phase VI. and the less violent outbursts of Phase VII., the last subsiding into the chronic activity which is the characteristic of Phase VIII., the modern period of the history of the volcano.

The products of each of these periods of eruption were described in great detail.

The eruptive phenomena which are illustrated by these studies of Somma and Vesuvius were then considered, together with the nature and result of the denudation which alternated with eruptive action in originating the present form of the mountain.

The paper concluded with a statement of fifty propositions on the subject of vulcanology which appear to the author to be established by the studies detailed in the paper.

6. "Note on 'Cone-in-Cone' Structure." By John Young, Esq., F.G.S.

This note was written with the object of calling the attention of the Geological Society to some very fine and remarkably interesting examples of the "Cone-in-Cone" structure.

The author, after referring to the views of previous authors on the origin of this structure, proceeded to describe the interesting examples of it which occur in the coalfields of Ayrshire and Renfrewshire. He pointed out that the structure is generally exhibited in bands overlying beds of fossils.

7. "A Geological Sketch of Quidong, Manaro, Australia." By Alfred Morris, Esq., C.E., F.G.S.

This district is situated about 250 miles S.S.E. from Sydney. The cliffs about the Bombala river, are about 100 to 120 feet high, and formed of very dark limestone, crowded with fossils, chiefly *Pentamerus*. In the author's opinion there has been great disturbance in this region, resulting in a complete change in the course of the river Bombala and a displacement of the shale. A mass of ferruginous sandstone has also been upheaved. This, as well as the other rocks in the neighbourhood, contain Upper Silurian fossils. It appears to have been altered by heat. Pockets of galena and copper are occasionally found in the district, and there is a vein of hæmatite. Clay-slates occur as well as the above rocks, the cleavage being generally vertical or nearly so.

XXXVI. *Intelligence and Miscellaneous Articles.*

ON RADIOMETERS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I HAVE learned in a roundabout way that Prof. Osborne Reynolds is annoyed with me because I have not carried out an intention I expressed in a letter to him two years ago, which was, as well as I can recollect, to publish something more on the subject of Radiometers, and to state that I was on the whole satisfied with his position.

I did not do so because I got rambling on other matters, and thought that by my silence I practically accepted his answers to my questions as published in the Philosophical Magazine. And as concerns some points I wrote to him about privately, and which he very kindly answered at great length, I hardly thought them worth publishing alone (though I asked leave to incorporate them in what I then intended writing upon the subject, and was kindly authorized to do so); for I felt sure that as it was my stupidity that had mistaken his meaning, others were very unlikely to make such mistakes and so would not take any interest in them.

I regret taking up the pages of the Philosophical Magazine with such a purely personal matter; but as I should be very sorry indeed that any one, and least of all one who has treated me as Prof. Reynolds has done, should suppose that I shirked expressing myself as satisfied with answers to questions I raised, I hope you will publish this letter; and I further hope that Prof. Reynolds will forgive me for having forgotten my promise, which I would certainly never have done if I had thought that he cared one bit what I said of his work, which speaks for itself too well for any mere opinion of one such as I am to benefit or injure.

Yours obediently,

Clarisford, Killaloe,
August 6, 1883.

GEORGE FRANCIS FITZGERALD.

THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FIFTH SERIES.]

OCTOBER 1883.

XXXVII. *On some Controverted Points in Geological Climatology; a Reply to Professor Newcomb, Mr. Hill, and others. By* JAMES CROLL, *LL.D., F.R.S.*[*]

NINETEEN years ago the theory was advanced that the Glacial Epoch was the result of a combination of physical agents brought into operation by an increase in the eccentricity of the earth's orbit. Few or no objections have been urged against what may be called the astronomical part of the theory; but the portions relating to these physical agencies, which are by far the most important part, have from time to time met with considerable opposition. Considering the newness of the subject, and the complex nature of many of these combinations of physical agencies, it would not be surprising if some of the original deductions in regard to them proved erroneous; but after long and careful reconsideration of the whole matter, I have not found reason to abandon any of them or alter them to any material extent.

The only class of objections urged against the theory which I have as yet considered at length are those relating to the cause of ocean-currents, and their influence on the distribution of heat over the globe; and I think it will be admitted that the views which I have advocated on these points are now being pretty generally accepted.

* From advance sheets of the 'American Journal of Science' for October 1883.

But it is in reference to the influence of aqueous vapour, fogs, and clouds on the production and preservation of snow that the greatest diversity of opinion has prevailed. The object of the present article is to examine at some length the principal objections which have been advanced in regard to this part of the inquiry. I shall also take the present opportunity of discussing more fully some points on which I have been sometimes misunderstood, and which appear to have been treated rather too briefly on former occasions.

In the 'American Journal of Science' for April 1876, Professor Newcomb has done me the honour to review at some length my work, 'Climate and Time;' and as his article is mainly devoted to a criticism of my reasoning in regard to those very points to which I refer, I shall begin with an examination of his objections. One reason for entering at some length into an examination of Professor Newcomb's objections is the fact that they embrace to a large extent those which have been urged by reviewers in Great Britain. Some of his objections, however, as will be seen, are based upon a misapprehension of my reasoning.

Temperature of Space.—One of the most important factors in the theory of geological climate resulting from changes in the eccentricity of the earth's orbit is obviously the temperature of stellar space. Unless we have, at least, some rough idea of the proportion which the heat derived from the stars bears to that derived from the sun, we cannot form any estimate of how much the temperature of our earth would be lowered or raised by a given decrease or increase of the sun's distance.

The question of the temperature of space has been investigated in different ways by Pouillet and Herschel; and the result arrived at was that space has a temperature of $-239°$ F., or an absolute temperature of 222°. The mean absolute temperature of our earth is about 521°. Consequently, according to these results, the heat received from the stars is to that received from the sun as 222 to 299. All my determinations of the change of temperature due to changes in the sun's distance were computed on these data, although I believe, for reasons stated, that space must have a much lower temperature. Recent observations of Professor Langley made during the Mount-Whitney Expedition confirm the correctness of my belief.

Professor Newcomb, however, wholly ignores all that has been done on that subject, for he commences his review by the statement that "practically there is but one source from which the surface of the earth receives heat—the sun, since the quantity received from all other sources is quite insignificant in comparison."

Surely Professor Newcomb must have forgotten all about the researches of Pouillet and Herschel into what has been termed the "Temperature of Space," or he could not have affirmed so positively that "practically there is but one source from which the earth receives heat, and that all other sources are quite insignificant" without, at least, giving some reason for the assertion.

I am pleased to find that he agrees, in the main, with what has been advanced in 'Climate and Time' in reference to the heating-power of ocean-currents, and also as to their existence being due to the impulse of the winds. But he differs widely from me in regard to the heat conveyed by aerial currents.

On the Heat conveyed by Aerial Currents.—I stated that the quantity of heat conveyed from equatorial to high temperate and polar regions is trifling in comparison with that conveyed by ocean-currents; for the heated air rising off the hot ground of the equator, after ascending a few miles becomes exposed to the intense cold of the upper regions, and having to travel polewards for thousands of miles in those regions, it loses nearly all the heat which it brought from the equator before it can possibly reach high latitudes. To this Professor Newcomb objects as follows:—"He (Mr. Croll) speaks of the hot air rising from the earth and becoming exposed to the intense cold of the upper regions of the atmosphere. But what can this cold be *but the coldness of the very air itself which has been rising up?* If the warm air rises up into the cold air, and becomes *cooled by contact* with the latter, the latter must become warm by the very heat which the former loses; and if there is a continuous rising current the whole region must take the natural temperature of the rising air. This temperature is, indeed, much below that which maintains at the surface, *for the simple reason that air becomes cold by expansion* according to a definite and well-known law. Having thus got his rising current constantly cooled off *by contact with the cold air* of the upper regions, it has to pass on its journey towards the poles," etc. (p. 267)*.

Here the cooling of the ascending air is attributed to two causes—(1) the heat lost by expansion as the air rises; (2) the heat lost by contact with the colder air through which the ascending air passes and with which it mixes in the upper regions. But the two may be resolved into one, viz. the heat lost by expansion; for the cold air, to which the ascending air communicates its heat by contact, is assumed to have

* The italics are mine.

originally derived its cold, in like manner, from expansion. This is evident, for although he recognizes the effect of radiation into space, he assumes that this loss is compensated by counter-radiation. The upper regions are, he says, exposed to the radiation of the sun on the one side, and of the earth's lower atmosphere on the other, and there is no proof that these do not equal the surface-temperature. And again, when the air descends in high latitudes to the earth's surface, an amount of heat will be evolved by compression equal to that which it lost when it rose from the equator.

Professor Newcomb has misapprehended not only my meaning, but also the chief reason why the air in the upper region is so intensely cold. Any one who has read what I have stated in pp. 35–40, 'Climate and Time,' regarding the temperature of space will readily understand what I mean by the temperature of the upper regions. By the temperature of stellar space, it is not meant that space itself is a something possessed of a given temperature, say $-239°$ F. It simply means the temperature to which a body would fall were it exposed to no other source of heat than that of radiation from the stars. By the temperature of the upper regions I mean the temperature to which air in those regions sinks in consequence of loss from radiation into space. It is mainly to this cause, and not to the loss from expansion, as Professor Newcomb assumes, that the intense cold of the upper air is due. The air in that region has got beyond the screen which protected it when at the earth's surface, and it then throws off its heat into space during twelve hours of night, getting no return from without except from the radiation of the stars. And even at noonday, as I have endeavoured to show in Appendix, p. 551, the rays of a burning sun overhead would not be sufficient to raise the temperature of the air up to the freezing-point. But the recent observations of Professor Langley prove that the loss of heat from radiation is in reality far greater than I had anticipated. He says :—"The original observations, which will be given at length, lead to the conclusion that in the absence of an atmosphere the earth's temperature of insolation would at any rate fall below $-50°$ F.; by which it is meant that, for instance, mercury would remain a solid under the vertical rays of a tropical sun were radiation into space wholly unchecked, or even if, the atmosphere existing, it let radiations of all wave-lengths pass out as easily as they come in" ('Nature,' August 3rd, 1882).

The temperature of the upper atmosphere, even after making allowance for heat received from below, must in this case be at least nearly 80 degrees below the freezing-point. The quantity of heat lost by expansion must therefore be trifling compared with that lost by radiation; and although the heat lost by expansion is fully restored by compression, yet the air would reach the earth deprived almost entirely of the heat with which it left the equator. All that it could possibly give back would simply be the heat of compression; and this would hardly be sufficient to raise air at $-50°$ F. to the freezing-point. How then can the polar regions be greatly the better of air from the equatorial regions? Professor Newcomb says:—"If the upper current be as great as is commonly supposed, it must be as powerful as ocean-currents in tending to equalize the temperature of the globe." How can this be?

Why the Mean Temperature of the Ocean should be greater than that of the Land.—"Another proposition," he says, "which the author attempts to prove, reasoning which seems equally inconclusive, is that the mean temperature of the ocean is greater than that of the land over the entire globe." I certainly never attempted to prove that the mean temperature of the ocean is *greater* than that of the land over the entire globe. The very chapter to which he here refers, and which he is about to criticise, was written to explain why the mean temperature of the southern or water hemisphere is less than that of the northern or land hemisphere. What I attempted to prove was, not that the mean temperature of the ocean *is* greater than that of the land, but that, were it not for certain causes, the mean temperature of the ocean *ought* to be greater than that of the land in equatorial regions as well as in temperate and arctic regions. In other words, the object of the chapter is to prove that the mean temperature of the southern or water hemisphere is less than that of the northern or land hemisphere, not, as is generally supposed, because the former is mainly water and the latter land, but because of the enormous amount of heat transferred from the former to the latter hemisphere by means of ocean-currents; and that were it not for this transference the temperature of the water would exceed that of the land hemisphere. And it is in order to prove this that the "four *à priori* reasons" which Professor Newcomb criticises were adduced. The first of these is as follows:—

First.—'The ground stores up heat only by the slow process of conduction, whereas water, by the mobility of its particles and its transparency for heat-rays, especially those from the sun, becomes heated to a considerable depth rapidly. The quantity of heat stored up in the ground is thus comparatively small, while the quantity stored up in the ocean is great.'*

* 'Climate and Time,' p. 90.

These sentences are considered unworthy of criticism. Are they really so unworthy? Let us examine them a little more closely. It is in consequence of the sun's rays being able to penetrate to a great depth that the amount of heat stored up by the ocean is so great; and it is to this store that its warmth during winter is mainly due. The water is diathermanous for the rays of the sun, but it is not so, for reasons well known, for the rays of water itself. The upper layers of the ocean will allow a larger portion of the radiation from the sun to pass freely downward, but they will not allow radiation from the layers underneath to pass freely upwards. These upper layers, like the glass of a greenhouse, act as a trap to the sun's rays, and thus allow the water of the ocean to stand at a higher temperature than it would otherwise do. Again, the slowness with which the ocean thus parts with its heat enables it to maintain that comparatively high temperature during the long winter months. And again, it is to the mobility of the particles of water, the depth to which the heat penetrates, and the rapidity with which it is absorbed, that those great currents of warm water become possible. Were the waters of the ocean, like the land, not mobile, and were only a few inches at the surface reached by heat from the sun, there could be no Gulf-stream, or any great transference of heat from the Southern to the Northern hemisphere, or from equatorial to temperate and polar regions, by means of oceanic circulation.

Second.—'The air is probably heated more rapidly by contact with the ground than with the ocean; but, on the other hand, it is heated far more rapidly by radiation from the ocean than from the land. The aqueous vapour of the air is to a great extent diathermanous to radiation from the ground, while it absorbs the rays from water and thus becomes heated.'

To this Professor Newcomb objects as follows:—"If, then, the air is really heated by contact with the ground more rapidly than by contact with the ocean, it can only be because the ground is hotter than the ocean, which is directly contrary to the theory Mr. Croll is maintaining." What I maintained was that, were it not for certain causes, the *mean annual* temperature of the ocean would be higher than that of the land. During the day and also during the summer the surface of the ground is hotter than that of the ocean; and the air, of course, will be heated more rapidly by *contact* with the former than with the latter. But this does not prove that the air is not more rapidly heated by *radiation* from the ocean than from the land. Professor Newcomb says:—"The statement that the aqueous vapour of the air is diathermanous to radiation from land, but not to that from water, is quite new to us, and very surprising." I am surprised that he is not acquainted with the fact, and also with its physical explanation. This will help to account for his inability to perceive how radiation from the ocean may heat the air more rapidly than radiation from the land, even though the surface of the latter may be at a higher temperature than that of the former.

He says :—"The rapidity with which the heating process goes on depends on the difference of temperature, no matter whether the heat passes by conduction or by radiation." This statement will hardly harmonize with recent researches into radiant heat. It is found that the rapidity with which a body is heated by radiation depends upon the absorbing power of the body ; and the absorbing power again depends upon the quality of the heat-rays. Professor Tyndall, for example, found that in the case of vapours, as a rule, absorption *diminishes* as the temperature rises. With a platinum spiral heated till it was barely visible, the absorption of the vapour of bisulphide of carbon was 6·5, but when the spiral was raised to a white heat the absorption was reduced to 2·9. A similar result took place in the case of chloroform, formic ether, acetic ether, and other vapours. The physical cause of this is well known.

If the aqueous vapour of the air, he says, be more diathermanous to radiation from land than from water, as I have stated, then I assigned directly contrary effects to the same cause. For, "reasoning as in (1), he, Mr. Croll, would have said that the air over the land, owing to its transparency for the heat-rays from the land, becomes heated to a greater height rapidly, while the air over the ocean, not being transparent, can acquire heat from the ocean only by the slow process of convection." I would have said no such thing. Radiation from the surface of the land will, no doubt, penetrate more freely through the aqueous vapour than radiation from the ocean; but the aqueous vapour will not absorb the radiation of the land so rapidly as that of the ocean, for the ocean gives off that quality of rays which aqueous vapour absorbs most rapidly.

This is not in opposition to what I have stated in reason (1); for if the ground were transparent to the sun's rays like water, evidently the total quantity of heat absorbed by it would be greater than that by the ocean. But radiation from the sun heats only the surface of the ground, all below the surface depends for its supply on the slow process of conduction, whereas the ocean is heated by direct radiation to great depths. Consequently the total quantity of heat absorbed by the ocean, say per square mile, in a given time, is greater than that absorbed by the land.

Third.—'The air radiates back a considerable portion of its heat, and the ocean absorbs this radiation from the air more readily than the ground does. The ocean will not reflect the heat from the aqueous vapour of the air, but absorbs it, while the ground does the opposite. Radiation from the air, therefore, tends more readily to heat the ocean than it does the land.'

"Here we have," he says, "the air giving back to the ocean the same heat which it absorbs from it, and thus heating it." If Professor Newcomb means by this same heat the same amount of heat, then I believe in no such thing. But if his meaning be that here we have the air giving back to the ocean a quantity of the heat which it absorbed from it, then he is certainly correct in supposing that this is affirmed by me. But this is a conclusion which no physicist could for a moment doubt. To deny this would be to contradict Prevost's well-known theory of exchanges. Did the air throw back to the ocean none of the heat which it derives from it, the entire waters of the ocean would soon become solid ice. In fact, as we have seen, mercury would not remain fluid and every living thing on the face of the globe would perish.

He states that reason fourth seems to be little more than a repetition of reason second in a different form. It is, however, much more than that. It is a demonstration that were it not for the causes to which I have alluded, the mean temperature of the water hemisphere ought to be higher than that of the land hemisphere; and for this reason I shall here give the section in full.

Fourth.—'The aqueous vapour of the air acts as a screen to prevent the loss by radiation from water, while it allows radiation from the ground to pass more freely into space; the atmosphere over the ocean consequently throws back a greater amount of heat than is thrown back by the atmosphere over the land. The sea in this case has a much greater difficulty than the land has in getting quit of the heat received from the sun; in other words, the land tends to lose its heat more rapidly than the sea. The consequence of all these circumstances is that the ocean must stand at a higher mean temperature than the land. A state of equilibrium is never gained until the rate at which a body is receiving heat is equal to the rate at which it is losing it; but as equal surfaces of sea and land receive from the sun the same amount of heat, it therefore follows that in order that the sea may get quit of its heat as rapidly as the land, it *must stand at a higher temperature* than the land. The temperature of the sea must continue to rise till the amount of heat thrown off into space equals that received from the sun; when this point is reached, equilibrium is established and the temperature remains stationary. But, owing to the greater difficulty that the sea has in getting rid of its heat, the mean temperature of equilibrium of the ocean must be higher than that of the land; consequently the mean temperature of the ocean, and also of the air immediately over it, in tropical regions should be higher than the mean temperature of the land and the air over it.'

Since the publication of 'Climate and Time' the accuracy of this conclusion has been confirmed in a remarkable manner from more recent researches on the actual mean temperature of the two hemispheres, the details of which have been given by Mr. Ferrell in his 'Meteorological Researches' (Washington, 1877). It is found that them ean temperature of the northern or land hemisphere is higher than that of the southern or water hemisphere up only to about latitude 35°, and that beyond this latitude the mean temperature of the water hemisphere is the greater of the two. At latitude 40° the mean temperature of the southern hemisphere is 1°·4 higher than that of the same parallel on the northern hemisphere. At latitude 50° the difference amounts to 4°·4; while at latitude 60° the mean temperature of the southern hemisphere is actually 6° higher than that of the northern on the same parallel. The mean temperatures of the two hemispheres are as follows:—

Lat....	0°	10°	20°	30°	40°	50°	60°	70°	80°
Northern..	80°·1	81°·0	77°·6	67°·6	56°·5	43°·4	29°·3	14°·4	4°·5
Southern...	80·1	78·7	74·7	66·7	57·9	47·8	35 3		

From the above table we see that it is only in that area lying between the equator and latitude 35° that the southern hemisphere has a lower mean temperature than the northern. But it is from this area that the enormous amount of heat transferred to the northern hemisphere is mainly derived. Were the transference of heat to cease, the temperature of this area would be very considerably raised, and that of the corresponding area on the northern hemisphere lowered. The result would doubtless be that the southern hemisphere down to the equator would then be warmer than the northern. But, even as things are, as Mr. Ferrel remarks, "the mean temperature of the southern hemisphere is the greater of the two," the mean temperature of the southern being 60°·89 F. and that of the northern 59°·54 F.

Heat cut off by the Atmosphere.—Professor Newcomb says further, "Another idea of the author which calls for explanation is that solar heat absorbed by the atmosphere is entirely lost, so far as warming any region of the globe is concerned." This is no idea of mine. My idea is not that the heat cut off is entirely lost, but merely that the *greater part* is lost. A large portion of the heat is reflected, and of that absorbed one half, perhaps, is radiated back into space and lost, in so far as the earth is concerned.

Tables of Eccentricity.—Referring to my tables of eccentricity of the earth's orbit, he says:—"That there are from time to time such periods of great eccentricity is a well-established result of the mutual gravitation of the planets; but whether the particular epochs of great and small eccentricity computed by Mr. Croll are reliable is a different question." I may here mention that Professor McFarland, of the Ohio State University, Columbus, a few years ago, undertook the task of re-computing every one of the 150 periods given in my tables, and he states that, except in one instance, he did not find an error to the amount of ·001*.

"The data for this computation," continues Professor Newcomb, "are the formulæ of Le Verrier, worked out about 1845†, without any correction either for the later corrections to the masses of the planets or for the terms of the third order, subsequently discussed by Le Verrier himself. The probable magnitude of these corrections is such that reliance cannot be placed upon the values of eccentricity computed without reference to them for epochs distant by merely a million of years."

In regard to this objection I may mention that the whole subject of the secular variations of the elements of the planetary orbits has been re-investigated by Mr. Stockwell, taking into account the disturbing influence of the planet Neptune, the existence of which was not known at the time Le Verrier's investigations were made. Professor McFarland, with the aid of Mr. Stockwell's formulæ, has computed all the periods in the tables referred to above; and on comparing the results found by both formulæ, he states that "the two curves exhibit a general conformity throughout their whole extent." And his computations, I may state, extend from 3,260,000 years

* American Journal of Science, vol. xi. p. 456 (1876).

† Le Verrier's formulæ were worked out several years before 1845.

before 1850, and to 1,260,000 years after that date; or, in other words, over a period of no fewer than 4,520,000 years*, thus showing that Professor Newcomb's objection falls to the ground.

Influence of Winter in Aphelion.—I have maintained that at a time when the eccentricity is high and the winter occurs in aphelion, the great increase in the sun's distance and in the length of the winter would have the effect of causing a large increase in the quantity of snow falling during that season. This very obvious result follows as a necessary consequence from the fact that the moisture which now falls in the form of rain would then fall as snow. But Professor Newcomb actually states that he cannot accept the conclusion that this would lead to more snow.

Influence of a Snow-covered Surface.—I have argued that this accumulation of snow would lower the summer temperature, and tend to prevent the disappearance of the snow, and have assigned three reasons for this conclusion:—

First.—Direct radiation. The snow, for physical reasons well known, will cool the air more rapidly than the sun's rays will heat it. This is shown from the fact that in Greenland, a snow- and ice-covered country, a thermometer exposed to the direct radiation of the sun has been observed to stand above 100°, while the air surrounding the instrument was actually 12° below the freezing-point. Professor Newcomb and also Mr. Hill † regard the idea that this could in any way favour the accumulation of snow as absurd. They think that in fact it would have directly the opposite effect. They have perceived only one half of the result. It is quite true, as they affirm, that the cooling of the air by the snow will not prevent the melting of the snow, but the reverse. There is, however, another and far more important result overlooked in their objection. If the snow- and ice-covered surface keeps the temperature of the air, in summer, below the freezing-point, which it evidently does in Greenland and in the Antarctic continent, the moisture of the air will fall as snow and not as rain. No doubt this is the chief reason why in those regions, even in the middle of summer, rain seldom falls, the precipitation being almost always in the form of snow, although at that very season the direct heat of the sun is often as great as in India. Were the snow and icy mantle removed a snow-shower

* In this laborious undertaking Professor McFarland computed, by means of both formulæ, the eccentricity of the earth's orbit and the longitude of the perihelion for no fewer than 485 separate epochs. See American Journal of Science, vol. xx. p. 105 (1880).

† 'Geological Magazine' for January 1880, p. 12.

in summer would be as rare a phenomenon in those regions as it would be in the south of England.

Second.—'The rays which fall on snow and ice are to a great extent reflected back into space. But those that are not reflected, but absorbed, do not raise the temperature, for they disappear in the mechanical work of melting the ice.'

This reason is also regarded as absurd. The heat of the sun during the perihelion summer would, he says, suffice to melt the whole accumulation of winter snow in three or four days. "The reader," he continues, "can easily make a computation of the incredible reflecting power of the snow and of the unexampled transparency of the air required to keep the snow unmelted for three or four months." Incredible as it may appear to Professor Newcomb, I shall shortly show that a less amount of snow than the equivalent of the two feet of ice which he assumes does actually, in some places, defy the melting-power of a tropical sun. But he misapprehends my reasoning here also, by overlooking the more important factor in the affair, namely, the keeping of the air in the summer below the freezing-point. The direct effect that this has in preventing the sun from melting the snow and ice will be discussed shortly; but the point to which I wish at present to direct special attention is the fact that if the air is kept below, or even at the freezing-point, snow will fall and not rain. Snow is a good reflector of heat; consequently a large portion of the sun's rays falling on the snow and icy surface is reflected back into space. The aqueous vapour of the air, on the other hand, as the vibrations of its molecules agree in *period* with those of the snow and ice, cuts off a large portion of the heat *radiated* by the snow surface; but here in the case of *reflection* under consideration the rays are not cut off; for the reflected rays are of the same character as the incident rays which pass so freely through the aqueous vapour. And in respect to the remaining rays which are not reflected, but absorbed by the snow, they do not manage to raise the temperature of the snow above the freezing-point. Consequently the air is kept in the condition most favourable for the production of snow.

Third.—'Snow and ice lower the temperature by chilling the air and condensing the vapour into thick fogs. The great strength of the sun's rays during summer, due to his nearness at that season, would, in the first place, tend to produce an increased amount of evaporation. But the presence of snow-clad mountains and an icy sea would chill the atmosphere and condense the vapour into thick fogs. The thick fogs and cloudy sky would effectually prevent the sun's rays from reaching the earth, and the snow in consequence would remain unmelted during the entire summer.'

On this Professor Newcomb's criticism is as follows:—"Here he (Mr. Croll) says nothing about the latent heat set free by the condensation, nor does he say where the heat goes to which the air must lose in order to be chilled. The task of arguing with a disputant who in one breath maintains that the transparency of the air is such that the rays reflected from the snow pass freely into space, and in the next breath that thick fogs effectually prevent the rays ever reaching the snow at all, is not free from embarrassment."

If he really supposes my meaning to be that the air is so transparent as to allow the incident and reflected rays of the sun to pass freely without interruption, while at the same time and in the same place the air is not transparent but filled with dense fogs which effectually cut off the sun's rays and prevent them from reaching the earth, then I do not wonder that he should feel embarrassed in arguing with me. But if he supposes my meaning to be, as it of course is, that those two opposite conditions, existing at totally different times or in totally different places at the same time, should lead to similar results, namely the cooling of the air and consequent conservation of snow, then there is no ground whatever for any embarrassment about the matter.

"We might therefore show," he states, "that if the snow, air, fog, or whatever throws back the rays of the sun into space is so excellent a reflector of heat, it is a correspondingly poor radiator; and the same fog which will not be dissipated by the summer heat will not be affected by the winter's cold, and will therefore serve as a screen to prevent the radiation of heat from the earth during the winter."

There are few points in connection with terrestrial physics which appear to be so much misunderstood as that of the influence of fogs on climate. One chief cause of these misapprehensions is the somewhat complex nature of the subject arising from the fact that aqueous vapour acts so very differently under different conditions. When the vapour exists in the air as an invisible gas, we have often an intensely clear and transparent sky, allowing the sun's rays to pass to the ground with little or no interruption; and if the surface of the ground be covered with snow, a large portion of the incident rays are reflected back into space without heating either the snow or the air. The general effect of this loss of heat is, of course, to lower the general temperature. But when this vapour condenses into thick fogs it acts in a totally different manner. The transparency to a great extent disappears, and the fog then cuts off the sun's rays and prevents them from reaching the ground. This it does in two different ways. 1st. Its watery particles, like the crystals of the snow, are good reflectors, and the upper surface of the mass of fog on which the rays fall acts as a reflector, throwing back a large portion of the rays into stellar space. The rest of the rays which are not reflected enter the fog and the larger portion of them are absorbed by it. But it will be observed that by far the greater part of the absorption, if not nearly all of it, will take place in the upper half of the mass. This is a necessary result of a recognized principle in radiant heat known as the "sifting" of the rays. The deeper the rays penetrate into the fog, the less will be the amount of heat absorbed. If the depth of the mass be great, absorption will probably entirely disappear before the surface of the ground is reached. The fog will begin, of course, to radiate off the heat thus absorbed; but as it is the upper half of the mass which has received the principal part of the heat, the most of this heat will be radiated upward into stellar space, and, like the reflected heat, entirely lost in so far as heating the earth is concerned. A portion will also be radiated downward, some of which may reach the ground, but the greater portion will be reabsorbed in its passage through the mass. We have no means of estimating the amount of heat which would thus be thrown off into space by reflection and radiation; but it is certainly great. I think we may safely conclude that in places like South Georgia and Sandwich Land, where fogs prevail to such an extent during summer, one half at least of the heat from the sun never reaches the ground. A deprivation of sun-heat of a much less extent than this would certainly lower the summer temperature of these places far below the freezing-point, were it not for a compensating cause to which I shall now refer, viz. the heat "trapped" by the fog. The fog, although it prevents a large portion of the sun's heat from ever reaching a place, at the same time prevents to a great extent that place from losing the little heat which it does receive. In other words, it acts as a screen preventing the loss of heat by radiation into space. But the heat thus "trapped" never fully compensates for that not received, and a lowering of temperature is always the result.

Had all those considerations been taken into account by Professor Newcomb, Mr. Hill, Mr. Searles Wood, and others, they would have seen that I had by no means overestimated the powerful influence of fogs in lowering the summer temperature.

The influence of fogs on the summer temperature is a fact so well established by observation that it seems strange that any one should be found arguing against it.

Heat Evolved by Freezing.—There is one objection to which I may here refer, and which has been urged by nearly all my critics. It is said, correctly enough, that as water in freezing evolves just as much heat as is required to melt it, there is on the whole no actual loss of heat; that whatever heat may be absorbed in the mechanical work of melting the snow, just as much was evolved in the formation of the snow. Consequently it is inferred, in so far as climate is concerned, the one effect completely counterbalances the other. This inference, sound as it may at first sight appear, has been so well proved to be incorrect by Mr. Wallace that I cannot do better than quote his words :—

"In the act of freezing, no doubt water gives up some of its heat to the surrounding air, but *that air still remains below the freezing-point* or freezing would not take place. The heat liberated by freezing is therefore what may be termed low-grade heat—heat incapable of melting snow or ice; while the heat absorbed while ice or snow is melting is high-grade heat, such as is capable of melting snow and supporting vegetable growth. Moreover, the low-grade heat liberated in the formation of snow is usually liberated high up in the atmosphere, where it may be carried off by winds to more southern latitudes; while the heat absorbed in melting the surface of snow and ice is absorbed close to the earth, and is thus prevented from warming the lower atmosphere, which is in contact with vegetation. The two phenomena therefore by no means counterbalance or counteract each other, as it is so constantly and superficially asserted that they do" ('Island Life,' p. 140).

The Fundamental Misconception.—I come now to a misapprehension which more than any other has tended to prevent a proper understanding of the causes which lead to the conservation by snow. Whatever the eccentricity of the earth's orbit may be, the heat received from the sun during summer is more than sufficient to melt the snow of winter. Consequently it is assumed no permanent accumulation of snow can take place. This objection, as expressed by Mr. Hill, is as follows :—"We have no reason to suppose that at present, in the northern hemisphere, more snow or ice is anywhere formed in winter than is melted in summer. With greater eccentricity less heat than now would be received in winter, but exactly as much more in summer. More snow would therefore be formed in the one half of the year, but exactly as much more be melted in the other half. The colder winter and the warmer summer would exactly neutralize each other's effects, and on the average of years no accumulation could begin. *Primâ facie*, therefore, high eccentricity will not account for glacial periods "*. In the language of Prof. Newcomb, it is as follows:—"During this perihelion summer the amount of heat received from the sun by every part of the northern hemisphere would suffice to melt from four to six inches of ice per day over its entire surface; that is, it would suffice to melt the whole probable accumulation in three or four days. The reader can easily make a computation of the incredible reflecting power of the snow and of the unexampled transparency of the air required to keep the snow unmelted for three or four months."

It is assumed in this objection that because the heat received from the sun by an area is more than sufficient to melt all the snow that falls on it, no permanent accumulation of snow and ice can take place. It is assumed that the quantity of snow and ice melted must be proportional to the heat received. Suppose that on a certain area a given amount of snow falls annually. The amount of heat received from the sun per annum is computed; and after the usual deduction for that cut off by the atmosphere has been made, if it be found that the quantity remaining is far more than sufficient to melt the snow, it is then assumed that the snow must be melted, and that no accumulation of snow and ice year by year in this area is possible. To one approaching this perplexing subject for the first time such an assumption looks very plausible; but a little reflection will show that it is most superficial. The assumption is at the very outset totally opposed to known facts. Take the lofty peaks of the Himalayas and Andes as an example. Few, I suppose, would admit that at these great elevations as much as 50 per cent. of the sun's heat could be cut off. But if 50 per cent. reaches the snow, this would be sufficient to melt fifty feet of ice; and this, no doubt, is more than ten times the quantity which actually requires to be melted. Notwithstanding all this the snow is never melted, but remains permanent. Take, as another example, South Georgia, in the latitude of England. Suppose we assume that one half of the sun's heat is cut off by the clouds and fogs which prevail to such an extent in that place, still the remaining half would be sufficient to melt upwards of thirty feet of ice, which is certainly more than the equivalent of all the snow which falls; yet this island is covered with snow and ice down almost to the seashore during the whole year.

* Geological Magazine, January 1880, p. 12.

Take still another example, that of Greenland. The quantity of heat received between latitudes 60° and 80°, which is that of Greenland, is, according to Meech, one half that received at the equator; and were none cut off, it would be sufficient to melt fifty feet of ice. The annual precipitation on Greenland in the form of snow and rain, according to Dr. Rink, amounts to only twelve inches; and two inches of this he considers is never melted, but is carried away in the form of icebergs. Mr. Hill maintains * that, owing to the great thickness of the air traversed by the sun's rays, and the loss resulting from the great obliquity of reflection, the amount of heat reaching the ground would be insufficient to melt more than sixteen feet of ice. Supposing we admit this estimate to be correct, still this is nineteen times more than is actually melted. The sun melts only ten inches, notwithstanding the fact that it has the power to melt sixteen feet.

In short, there is not a place on the face of the globe where the amount of heat received from the sun is not far more than sufficient to melt all the snow which falls upon it. If it were true, as the objection assumes, that the amount of snow melted is proportional to the amount of heat received by the snow, then there could be no such thing as perpetual snow.

The reason why the amount of snow and ice melted is not necessarily proportional to the amount of heat received is not far to seek. Before snow or ice will melt, its temperature must be raised to the melting-point. No amount of heat, however great, will induce melting to begin unless the intensity of the heat be sufficient to raise the temperature to the melting-point. Keep the temperature of the snow below that point, and, though the sun may shine upon it for countless ages, it will still remain unmelted. It is easy to understand how the snow on the lofty summits of the Himalayas and the Andes never melts. According to the observations made at Mount Whitney, to which reference has already been made, the heat of even a vertical sun would not be sufficient at these altitudes to raise the temperature of the snow to near the melting-point; and thus melting, under these conditions, is impossible. The snow will evaporate, but it cannot melt. But, owing to the frozen condition of the snow, even evaporation will take place with extreme difficulty. If the sun could manage to soften the snow-crystals and bring them into a semifluid condition, evaporation would, no doubt, go on rapidly; but this the rays of the sun are unable to do;

* Geological Magazine, April 1880.

consequently, we have only the evaporation of a solid, which, of course, is necessarily small.

It may here be observed that at low elevations, where the snowfall is probably greater, and the amount of heat received even less, than at the summits, the snow melts and disappears. Here, again, the influence of that potent agent, aqueous vapour, comes into play. At high elevations the air is dry, and allows the heat radiated from the snow to pass into space; but at low elevations a very considerable amount of the heat radiated from the snow is absorbed by the aqueous vapour which it encounters in passing through the atmosphere. A considerable portion of the heat thus absorbed by the vapour is radiated back on the snow; but the heat thus radiated being of the same quality as that which the snow itself radiates, is on this account absorbed by the snow. Little or none of it is reflected, like that received from the sun. The consequence is, that the heat thus absorbed accumulates in the snow till melting takes place. Were the amount of aqueous vapour possessed by the atmosphere sufficiently diminished, perpetual snow would cover our globe down to the sea-shore. It is true that the air is warmer at the lower than at the higher levels, and, by contact with the snow, must tend to melt it more at the former than at the latter position. But we must remember that the air is warmer mainly in consequence of the influence of aqueous vapour, and that, were the quantity of vapour reduced to the amount in question, the difference of temperature at the two positions would not be great.

But it may be urged, as a further objection to the foregoing conclusion, that, as a matter of fact, on great mountain-chains the snow-line reaches to a lower level on the side where the air is moist than on the opposite side where it is dry and arid—as, for example, on the southern side of the Himalayas and on the eastern side of the Andes, where the snow-line descends 2000 or 3000 feet below that of the opposite or dry side.

But this is owing to the fact that it is on the moist side that by far the greatest amount of snow is precipitated. The moist winds of the south-west monsoon deposit their snow almost wholly on the southern side of the Himalayas, and the south-east trades on the east side of the Andes. Were the conditions in every respect the same on both sides of these mountain-ranges, with the exception only that the air on one side was perfectly dry, allowing radiation from the snow to pass without interruption into stellar space, while on the other side the air was moist and full of aqueous vapour absorbing the heat radiated from the snow, the snow-line would in this case undoubtedly descend to a lower level on the dry than on the moist side. Melting would certainly take place at a greater elevation on the moist than on the dry side; and this is what would mainly determine the position of the snow-line.

The annual precipitation on Greenland, as we have seen, is very small, scarcely one half that of the driest parts of Great Britain. This region is covered with snow and ice, not because the quantity of snow falling on it is great, but because the quantity melted is small; and the reason why the snow does not melt is not that the amount of heat received during the year is unequal to the work of melting the ice, but that, mainly through the dryness of the air, the snow is prevented from rising to the melting-point. The very cause which prevents a heavy snowfall protects the little which does fall from disappearing. The same remarks apply to the Antarctic regions.

In South Georgia and Fuego, where clouds and dense fogs prevail during nearly the whole year, the permanent snow and ice are due to a different cause. Here the snowfall is great, and the amount of heat cut off enormous; but this alone would not account for the non-disappearance of the snow and ice; for, notwithstanding this, the heat received is certainly more than sufficient to melt all the snow which falls, great as that amount may be. The real cause is that the heat received is not sufficiently intense to raise the temperature to the melting-point. More heat is actually received by the snow than is required to melt it; but it is dissipated and lost before it can manage to raise the temperature of the snow to the melting-point; consequently the snow is not melted. Here snow falls in the very middle of summer; but snow would not fall unless the temperature were near the freezing-point.

Foregoing principles applied to the case of the Glacial Epoch.—Let us now apply the foregoing principles to the case of the glacial epoch. As winter then occurred in aphelion during a high state of eccentricity, that season would be much longer and colder than at present. Snow in temperate regions would then fall in place of rain; and although the snowfall during the winter might not be great, yet, as the temperature would be far below the freezing-point, what fell would not melt. As heat, which produces *evaporation,* is just as essential to the accumulation of snow and ice as is cold, which produces *condensation,* after the sun had passed the vernal equinox and summer was approaching, the consequent rise of temperature would be accompanied by an increase in the snowfall. A melting of the snow would also begin; but it would be a very considerable time before the amount melted would equal the daily amount of snow falling. Rain, alternating with snow-showers, would probably result; and, for some time before midsummer, snow would cease and give place entirely to rain. Melting would then go on rapidly, and by the end of the summer the snow would all disappear except on high mountain-summits such as those of Scotland, Wales, and Scandinavia. Before the end of autumn, however, it would again begin to fall. Next year would bring a repetition of the same process, with this difference, however, that the snow-line would descend to a lower level than in the previous year. Year by year the snow-line would continue to descend till all the high grounds became covered with permanent snow.

It would not require a very great amount of change from the present condition of things to bring about such a result. A simple lowering of the temperature, which would secure that snow, instead of rain, should fall for six or eight months in the year, would suffice; and this would follow as a necessary result from an increase of eccentricity. Now, if all our mountain-summits were covered with permanent snow down to a considerable distance, the valleys would soon become filled with local glaciers. In such a case we should then have more than one half of Scotland, a large part of the north of England and Wales, with nearly the whole of Norway, covered with snow and ice. Here a new and powerful agent would come into operation which would greatly hasten on a glacial condition of things. This large snow- and ice-covered surface would tend to condense the vapour into snow. It would, during summer, chill the air and produce dense and continued fogs, cutting off the sun's rays, and leading to a state of things approaching to that of South Georgia, which would much retard the melting of the snow.

It is a great mistake, as I have repeatedly shown, to suppose that the perihelion summers of the glacial epoch could be hot. No snow- and ice-covered continent can enjoy a hot summer. This is clearly shown by the present condition of Greenland. Were it not for the ice, the summers of North Greenland, owing to the continuance of the sun above the horizon, would be as warm as those of England; but, instead of this, the Greenland summers are colder than our winters, and snow during that season falls more or less nine days out of ten. But were the ice-covering removed, a snow-shower during summer would be as great a rarity as it would be with us. On the other hand, cover India with an ice-sheet, and the summers of that place would be colder than those of England.

When the high grounds of Scotland and Scandinavia, with those of the northern parts of America, became covered with snow and ice, and the eccentricity went on increasing, a diminution of the Gulf-stream and a host of other physical agencies, all tending towards a glacial condition of things, would be brought into operation. This would ultimately and inevitably lead to a general state of glaciation, without the aid of any of those *additional* geographical changes of land and water which some have supposed.

The Mutual Reaction of the Physical Agents.—Those who think that the agencies to which I refer would not by themselves bring about a glacial condition appear to overlook a most important and remarkable circumstance regarding their mode of operation, to which I have frequently alluded in 'Climate and Time' (pp. 74–77) and other places. The circumstance is this:—The physical agencies in question not only all lead to one result, viz. an accumulation of snow and ice, but their efficiency in bringing about this result is actually strengthened by their mutual reaction on one another. In physics the effect reacts on the cause. In electricity and magnetism, for example, cause and effect in almost every case mutually act and react upon each other; but the reaction of the effect tends to weaken the cause. Those physical agents to which I have referred, no doubt, in their mutual actions and reactions obey the same law; but in reference to *one particular result*, viz. the accumulation and conservation of snow, those mutual reactions strengthen one another. This is not reasoning in a circle, as Mr. Searles Wood supposes; for the reaction of an effect may on the whole weaken the cause, and yet in regard to a particular result it may strengthen it. In the case under consideration the agents not only act in one direction, but their efficiency in acting in that one direction is strengthened by their mutual reactions. This curious circumstance throws a flood of light on the causes which tended to bring about the glacial epoch.

To begin with, we have a high state of eccentricity. This leads to long and cold winters. The cold leads to snow; and although heat is given out in the formation of the snow, yet the final result is that the snow intensifies the cold: it cools the air and leads to still more snow. The cold and snow bring a third agent into play—*fogs*, which act still in the same direction. The fogs intercept the sun's rays; this interception of the rays diminishes the melting-power of the sun, and so increases the accumulation. As the snow and ice continue to accumulate, more and more of the rays are cut off; and, on the other hand, as the rays continue to be cut off, the *rate* of accumulation increases, because the quantity of snow and ice melted becomes thus annually less and less. In addition, the loss of the rays cut off by the fogs lowers the temperature of the air and leads to more snow being formed, while, again, the snow thus formed chills the air still more and increases the fogs. Again, during the winters of a glacial epoch, the earth would be radiating its heat into space. Had this loss of heat simply lowered the temperature, the lowering of the temperature would have tended to diminish the rate of loss; but the result is the formation of snow rather than the lowering of the temperature.

Further, as snow and ice accumulate on the one hemisphere they diminish on the other. This increases the strength of the trade-winds on the cold hemisphere and weakens those on the warm. The effect of this is to impel the warm water of the tropics more to the warm hemisphere than to the cold. Suppose the northern hemisphere to be the cold one; then, as the snow and ice begin gradually to accumulate, the ocean-currents of that hemisphere, more particularly the Gulf-stream, begin to decrease in volume, while those on the southern or warm hemisphere begin *pari passu* to increase *. This withdrawal of heat from the northern hemisphere favours the accumulation of snow and ice; and as the snow and ice accumulate the ocean-currents decrease. On the other hand, as the ocean-currents diminish the snow and ice still more accumulate. Thus the two effects, in so far as the accumulation of snow and ice is concerned, mutually strengthen each other.

The same process of mutual action and reaction takes place among the agencies in operation on the warm hemisphere; only the result produced is diametrically opposite to that produced in the cold hemisphere. On this warm hemisphere action and reaction tend to raise the mean temperature and diminish the quantity of snow and ice existing in temperate and polar regions.

* Prof. Dana has shown that in North America those areas which at present have the greatest rainfall are, as a rule, the areas which were most glaciated during the glacial epoch. Mr. Searles V. Wood (Geol. Mag., July) maintains that this fact is inconsistent with the theory that the glacial period was due to the cause to which I attribute it. I am totally unable to comprehend how he arrives at this conclusion. Supposing the Gulf-stream, as I have maintained, were greatly diminished during the glacial period, still I think it would follow, other things being equal, that the areas which now have the greatest rainfall would during that period probably have the greatest snowfall, and consequently the greatest accumulation of ice. The amount of precipitation might be less than at present; but this would not prevent the areas which had the greatest snowfall from being most covered with ice.

The primary cause of all these physical agencies being set in operation is a high state of eccentricity of the earth's orbit; and with a continuance of that state a glacial epoch becomes inevitable.

The Explanation begins with Winter.—Mr. Hill asks why I always begin in my explanation with the aphelion winter rather than with the perihelion summer. The reason is that the character of the summer is determined by that of the winter, and not the winter by that of the summer. It is true that to a certain extent the influence is mutual; but the effect of the summer on the winter is trifling in comparison with that of the winter on the summer. To begin our explanation with the summer would be like beginning at the end of a story and telling it backward.

M. Woeikof on the Cause of Glaciation.—In an article by A. Woeikof on "Glaciers and Glacial Periods in their relations to Climate" ('Nature,' March 2nd, 1882), it is maintained that the chief cause which leads to the formation of snow, and consequently to a glacial condition, is a low surface-temperature of the sea surrounding or adjoining the land. When the surface-temperature of the water much exceeds the freezing-point, the vapour, he says, evaporated from the sea and condensed on the land will be rain and not snow; but when the temperature of the water is near the freezing-point, snow will be the result. A diminution, for example, in the heat brought by the Gulf-stream that would very greatly lower the surface-temperature of the sea surrounding Great Britain would, he says, bring about a heavy snowfall and lead to permanent snow and ice. Again, he maintains, "As there is no reason to suppose that the surface-temperature of the sea would be lower during winter in aphelion and high eccentricity, it follows that there will not be more snow than now in countries where rain is the rule, even in winter, all other things equal."

There is surely a fallacy lurking under this theory of M. Woeikof. Snow instead of rain is not, as he supposes, owing to the low temperature of the water from which the vapour is derived, but to the low temperature of the air where the vapour is precipitated. Of course, when the surface of the sea is near the freezing-point, the air over the sea and the adjoining land is usually also not far from the freezing-point, and consequently the precipitation is more likely to be snow than rain. If the air be cold, as it generally is over a snow- and ice-covered country, a high temperature of the adjoining seas, were this possible, would greatly increase the snowfall, because it would greatly augment the quantity of vapour which would be available for snow.

I shall next examine at considerable length Mr. Alfred R. Wallace's modification of the theory, as given in 'Island Life.'

XXXVIII. *On* Mr. Heath's *Criticism of* Ferrel's *Theory of Atmospheric Currents. By* Frank Waldo*.

It is not astonishing that Mr. Heath should not have a correct idea of the Theory of Atmospheric Currents accepted by the meteorologists of today; but he was rather hasty in writing the criticisms on Mr. Ferrel's paper which have just appeared (in the July number of this Journal).

I should do nothing more than refer the critic to papers from which he could obtain information, if it were not that an answer to his remarks is called for, not at all to defend Ferrel, because that is unnecessary, but to bring this matter a little more prominently before those who have read the criticism referred to, and who, if they accept it, will form an incorrect notion of the theory of atmospheric currents.

In the first place the critic seems to be almost totally unacquainted with the literature of the subject. It is true that he has apparently read Ferrel's first mathematical paper, and some minor papers in 'Nature;' but he makes no reference to the other papers (more than sixty) on this topic which have appeared within the last twenty-five years.

The whole subject is one that has been much neglected in England; and, apart from the papers in 'Nature,' an article published by Mr. Daniel Vaughan of Cincinnati (U.S.) in the British-Association Report for 1859, and an article by Professor Everett, I know of no purely English writings which can be called at all important on the new dynamical meteorology.

I will first take up some of the points touched upon by the critic, and make a few references to the literature.

Dr. Haughton, as quoted on page 13, renders only due credit to Ferrel when he says that Ferrel has given a solution, which is in general satisfactory, of the problem of air-currents in his important memoir. This memoir is justly considered the first of the series of papers which followed, in which the atmospheric movements are deduced dynamically; and it is recognized as the most important paper in the earlier history of the new meteorology—new as opposed to the Dovian.

Mr. Heath says (pp. 14–15):—"A mass situated anywhere near the surface, but unconnected with it and unresisted by the air, if put in motion, will begin to move in fixed

* Communicated by the Author.

space, in a straight line, in a direction and with a velocity compounded of the velocity it had in common with the surface at that place and that of the impulse given to it, in accordance with the principle of the Parallelogram of Velocities, and will be drawn out of that line only downwards, by the force of gravity at the centre of the earth; and therefore it will never leave the plane of the great circle, fixed in space, which comprises the original direction and this centre." This refers to absolute motion, while the question is one of relative; it will be spoken of further on. However, that this absolute motion is not in the least contradictory to the application of the principle of the preservation of areas as carried out by Ferrel and others and as questioned by Mr. Heath, is proven by Dr. A. Sprung in an article, "Zur Anwendung des Princips der Flächen in der Meteorologie," *Oesterr. Zeitschrift für Meteorologie*, Band xvi. page 62.

On page 15 he remarks that Sir John Herschel, in his 'Meteorology,' does not mislead his readers as Ferrel does. It may be here remarked that, in the book referred to, Herschel did not give the true theory of the winds as at present accepted. Ferrel was right in speaking of the wrong teaching of the text-books, because it is only a few years since the true explanation was first given in any of them, and even now those in which it is found can be counted on the fingers of one hand.

The "force" which is described on page 15 as incomprehensible is explained in Poisson's paper on projectiles, "Mémoire sur le mouvement des projectiles dans l'air, en ayant égard à la rotation de la terre," lu à l'Académie des Sciences, le 14 novembre 1837, *Journal de l'Ecole Polytechnique*, cahier xxvi. There is also a discussion on the subject in the *Comptes Rendus*, tome xlix. &c., &c.; and Poisson's paper is mentioned in Routh's 'Rigid Dynamics' (3rd edition), 1877, p. 218.

In regard to Ferrel's criticisms of Colding, he was right in saying that Colding did not take into account one component of the effect of the earth's rotation.

As to Ferrel and Everett having jointly discovered this component, there has never, to my knowledge, been such a claim made. It was discovered many years previously, but was rediscovered by Ferrel independently.

In regard to the two points brought out on page 18—first, the application of the principle of the conservation of areas, and, secondly, the path of a body on the earth's surface—Mr. Heath is in error in his criticism. As to the first point, this is not the first time that the application of this theorem has been questioned. Dr. Thiesen employs it in an article "Ueber

Bewegungen auf der Erdoberfläche," pages 203–206, Band xiv. of the *Oesterreichischen Zeitschrift für Meteorologie,* and, on being criticised for doing so, replies, "the area principle is always applicable in a determined plane, if the projection of the forces acting in this plane has no momentum" (see page 88, Band xv. of the same Journal). Also, page 89, "In the present case, in which the earth is regarded as a rotating body, the attracting force as well as the predominating pressure force on the earth's surface lies in the meridian, and consequently has in the equatorial plane no momentum of rotation in relation to the earth's axis."

On page 40 of Kirchhoff's *Mechanik* (2nd edition), as well as in Schell's *Theorie der Bewegung und der Kräfte* (2nd edition), pages 351–354, is found the theorem of the conservation of areas and the cases to which it can be applied; and here we find the theorem employed by Thiesen. This proves conclusively that Ferrel is correct in using it.

With respect to the second point, on page 209 of the third edition of Routh's 'Dynamics' we find the following theorem:—"In finding the motion of a particle of mass m with reference to any moving axes, we may treat the axes as if they were fixed in space, provided we regard the particle as acted on, in addition to the impressed forces, by two forces:—(1) a force equal and opposite to that which would constrain the particle to remain fixed to the moving axes, and which is measured by mf, where f is the reversed acceleration of the point of moving space occupied by the particle; (2) a force perpendicular to both the direction of relative motion of the particle and to the central axis or axis of rotation of the moving axes, and which is measured by $2mV\Omega \sin\theta$, where V is the relative velocity of the particle, Ω the resultant angular velocity of the moving axes, and θ the angle between the direction of the velocity and the axis of rotation."

It is a question of relative motion that is being considered; and it is evident, as Routh remarks on page 213, that "the motion of a body on the surface of the earth is not exactly the same as if the earth were at rest."

A casual reader might see part of Ferrel's paper in the same light as Mr. Heath; but we must remember that it is to be taken for the most part as a qualitative and not a quantitative investigation.

Although not in formula, yet in words the equation of continuity is taken into account.

On pages 18 to 23 we find a more detailed criticism of Ferrel's paper.

That Ferrel's results were somewhat peculiar is to be admitted; and it was perhaps this peculiarity that kept them so long unrecognized; but careful study has led to their acceptance by the foremost meteorologists. He could not hope to solve the complex problem as it really exists, and was obliged to make his solution very general.

On pp. 386–391, Band xiv. of the *Oesterr. Zeitsch. für Meteorologie* is given a review of the first part of Ferrel's elaboration, mentioned below, of his early paper; pp. 161–175 and 276–283 of the xvii. Band of the same Journal contain a review of the second part; and the whole was again reviewed in 'Nature' last year.

It appears as though Ferrel's critic had written his article from the point of view of a student of Laplace and Airy, and had not examined the more modern text-books on mechanics to see if Ferrel's reasoning was admissible.

It is hoped that instead of heeding the warning that has been sounded against Ferrel, the readers of this Journal will read the elaborations of his first paper, given as Appendices to the U. S. Coast-Survey Reports for 1875 and 1878.

Mr. Heath's paper will have one good effect, I hope; and that is, to interest some of the English mathematicians and physicists in this subject.

XXXIX. *On the Equation to the Secular Inequalities in the Planetary Theory.* *By* J. J. Sylvester, *F.R.S.**

A VERY long time ago I gave, in this Magazine, a proof of the reality of the roots in the above equation, in which I employed a certain property of the square of a symmetrical matrix which was left without demonstration. I will now state a more general theorem concerning the *product* of *any* two matrices of which that theorem is a particular case. In what follows it is of course to be understood that the product of two matrices means the matrix corresponding to the *combination* of two *substitutions* which those matrices represent.

It will be convenient to introduce here a notion (which plays a conspicuous part in my new theory of multiple algebra), viz. that of the *latent roots* of a matrix—latent in a somewhat similar sense as vapour may be said to be latent in water or smoke in a tobacco-leaf. If from each term in the diagonal of a given matrix, λ be subtracted, the determinant to the matrix so modified will be a rational integer function of λ; the roots of that function are the latent roots of the matrix; and there results the important theorem that the latent roots of

* Communicated by the Author.

any function of a matrix are respectively the same functions of the latent roots of the matrix itself: *ex. gr.* the latent roots of the square of a matrix are the squares of its latent roots.

The latent roots of the product of two matrices, it may be added, are the same in whichever order the factors be taken. If, now, m and n be any two matrices, and $M=mn$ or nm, I am able to show that the sum of the products of the latent roots of M taken i together in every possible way is equal to the sum of the products obtained by multiplying every minor determinant of the ith order in one of the two matrices m, n by its *altruistic opposite* in the other: the reflected image of any such determinant, in respect to the principal diagonal of the matrix to which it belongs, is its *proper* opposite, and the corresponding determinant to this in the other matrix is its *altruistic opposite.*

The proof of this theorem will be given in my large forthcoming memoir on Multiple Algebra designed for the 'American Journal of Mathematics.'

Suppose, now, that m and n are transverse to one another, *i. e.* that the lines in the one are identical with the columns in the other, and *vice versâ*, then any determinant in m becomes identical with its altruistic opposite in n; and furthermore, if m be a symmetrical matrix, it is its own transverse. Consequently we have the theorem (the one referred to at the outset of this paper) that the sum of the i-ary products of the latent roots of the square of a symmetrical matrix (*i. e.* of the squares of the roots of the matrix itself) is equal to the sum of the squares of all the minor determinants of the order i in the matrix; whence it follows, from Descartes's theorem, that when all the terms of a symmetrical matrix are real, none of its latent roots can be *pure* imaginaries, and, as an easy inference, cannot be *any kind* of imaginaries; or, in other words, all the latent roots of a symmetrical matrix are real, which is Laplace's theorem.

I may take this opportunity of stating the important theorem that if $\lambda_1, \lambda_2, \ldots \lambda_i$ are the latent roots of any matrix m, then

$$\phi m=\Sigma\frac{(m-\lambda_2)(m-\lambda_3)\ldots(m-\lambda_i)}{(\lambda_1-\lambda_2)(\lambda_1-\lambda_3)\ldots(\lambda_1-\lambda_i)}\phi\lambda.$$

This theorem of course presupposes the rule first stated by Prof. Cayley (Phil. Trans. 1857) for the addition of matrices.

When any of the latent roots are equal, the formula must be replaced by another obtained from it by the usual method of infinitesimal variation. If $\phi m=m^{\frac{1}{\omega}}$, it gives the expression for the ωth root of the matrix; and we see that the number of such roots is ω^i, where i is the order of the matrix. When, however, the matrix is *unitary*, *i. e.* all its terms except the diagonal ones are *zeros* or *zeroidal*, *i. e.* when all its terms are *zeros*, this conclusion is no longer applicable, and a certain definite number of arbitrary quantities enter into the general expressions for the roots.

The case of the extraction of any root of a unitary matrix of the second order was first considered and successfully treated by the late Mr. Babbage; it reappears in M. Serret's *Cours d'Algèbre supérieure*." This problem is of course the same as that of finding a function $\frac{ax+b}{cx+d}$ of any given order of periodicity. My memoir will give the solution of the corresponding problem for a matrix of any order. Of the many unexpected results which I have obtained by my new method, not the least striking is the *rapprochement* which it establishes between the theory of Matrices and that of Invariants. The theory of invariance relative to associated Matrices includes and transcends that relative to algebraical functions.

XL. *On the Influence of the Direction of the Lines of Force on the Distribution of Electricity on Metallic Bodies.* By Alfred Tribe, *F.Inst.C., Lecturer on Chemistry in Dulwich College**.

I WISH it to be understood at the outset that the results on electrical distribution, to which reference will be made in this paper, were obtained during the electrolysis of a solution of copper sulphate, by determining the amount, extent, position, or nature of the electrochemical action set up on a metallic plate, or other-shaped conductor, immersed in the electrolyte, but not in metallic connexion with the battery-electrodes. I would also point out that when I speak of the lines of force as having a certain direction with regard to a part or parts of a metallic plate, or other-shaped *analyzer*, or as having a certain direction with regard to the boundary of an electrochemical deposit, it is to be understood that such would be the direction supposing the analyzer itself exercised no disturbing influence on these imaginary lines in the field of action.

I. *When the direction of the lines of force is parallel to the sides and perpendicular to the ends of the metallic conductor.*

Some six years ago (Proc. Roy. Soc. 1877, no. 181), I

* Communicated by the Author.

ascertained, quantitatively, the distribution of negative electricity on a rectangular silver plate, by determining the amount on successive surfaces of equal area, and also on successive surfaces of equal length, measured from the apex of a diamond-shaped plate of the same metal. The plates were in the position in the electrolytic field indicated in the above subtitle, and were under similar conditions as to the strength of electrolyte, current, &c.

The bare experimental data given in my former paper become much more instructive when the figures express the quantity of electricity found on equal areas of the respective plates, as is done in the following table. For convenience of comparison, the quantity on the end surface of the rectangle is taken as 100, and all the other equal areas are calculated to this standard.

Distance in millimetres from the end facing + electrode.	On the rectangular plate.	On the diamond plate.
0 to 2	100·0	1966
2 „ 4	64·8	423
4 „ 6	52·4	220
6 „ 8	42·7	161
8 „ 10	36·5	147
10 „ 12	30·3	124
12 „ 14	25·5	88
14 „ 16	20·7	74
16 „ 18	15·8	51
18 „ 20	7·6	38
20 „ 22	trace	24
22 „ 24	...	6

The distribution of positive electricity was, at the same time, determined on a cylinder of pure copper; and taking the electricity found on the rounded end of the conductor as 100, the quantities on approximately equal areas of the other parts were as shown below:—

Distance in millimetres from the end facing — electrode.	
0 to 2	100·0
2 „ 4	45·7
4 „ 6	34·7
6 „ 8	29·5
8 „ 10	24·1
10 „ 12	19·6
12 „ 14	17·5
14 „ 16	14·7
16 „ 18	12·8
18 „ 20	10·6
20 „ 22	8·9
22 „ 24	6·6

These results establish, and, I think, conclusively, that the distribution of electricity on conductors in an electric field, when the direction of the lines of force is perpendicular to their ends, and when the material of the field consists of an electrolyte, is generally similar to what is known to be the distribution of electricity on conductors in an electric field when the material consists of a gaseous dielectric. And the relatively great electric density found on the end surface of the rhombus would also appear to establish a very close resemblance (of course under the given conditions) between the action of the points of conductors in electrolytic and dielectric fields respectively.

In the experiments now to be described, I employed a trough 305 millim. long, 120 broad, and 128 deep, copper electrodes of the same area as the ends of the trough, a 5-per-cent. solution of copper sulphate, and a current of one ampère flowing for six minutes.

II. *When the direction of the lines of force makes an oblique angle with the sides of the metallic conductor.*

In a former paper in the 'Philosophical Magazine,' June 1881, p. 447, it was shown that when the direction of the force is parallel to the plane of the analyzing plate, and perpendicular to its ends, the electrification of the same sign is identical in area on both sides of the plate; also that when the force makes an oblique angle with the plane of the metallic plate, the electrification of the same sign differs in area on the sides of the plate; and, furthermore, that this difference in area increases as the plane of the plate approaches a right angle to the direction of the lines of force.

In the annexed table are set out the areas in square millimetres of the electrifications and intermedial spaces, side by side with the angle of inclination of the plane of the plates. The analyzers employed were plates of fine silver measuring 67 × 7 millimetres. The expressions "+ side," "− side" in the table refer to the side of the analyzer opposite to the + or − electrode respectively.

Angle.	− electrification.		Intermedial space.		+ electrification.	
	+ side.	− side.	+ side.	− side.	+ side.	− side.
0°	112	112	91	91	266	266
$22\frac{1}{2}$	126	105	91	80	252	284
45	136	84	112	112	221	273
$67\frac{1}{2}$	152	52	202	217	115	200
79	121	trace	348	364	trace	105

It is evident from these results that as the plane of the plate became perpendicular to the direction of the lines of force—*first*, the area of − electrification increased on the + side (at least to $67\frac{1}{2}°$) and decreased on the − side of the same plate; *secondly*, the area of + electrification decreased on the + side, and varied irregularly on the − side; and, *thirdly*, the area of the intermedial space generally increased on both sides.

I have not explored the distribution of electricity on these plates by actual chemical analysis; but, judging from the colour and general appearance of the electro-deposits, it would appear that as the direction of the force approached a perpendicular to the plane of the analyzers the quantity of electricity on the plates diminished, and that the electric density also diminished at the respective ends of the plates.

In support of the first of these conclusions, I would point to some experiments bearing on the subject described in my paper in the 'Proceedings of the Royal Society,' 1876, p. 313.

III. *When the direction of the lines of force is perpendicular to the plane of the metallic conductor.*

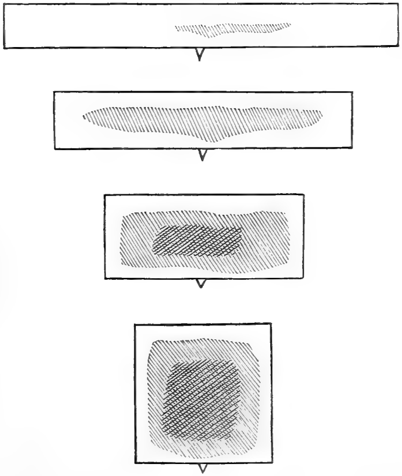
There evidently exists, as we have just seen, a relation between the quantity of electricity set up on an analyzing plate and the direction of the lines of force to its plane, from which it would follow that the minimum quantity would be found when the plane was perpendicular to the direction of the force. Such is the case. In fact, the only sign of electrification noticeable on a plate in this position, of the dimensions of those employed in the experiments described in II., was a mere trace of copper, longitudinally arranged along the central part of that side of the plate facing the + electrode. This latter fact was pointed out in my paper in the 'Philosophical Magazine' for June 1881, [5] xi. p. 448.

The further investigation of this subject has brought out many points of interest. It has shown that the distribution of electricity on a metallic plate is totally different when its plane is respectively parallel or perpendicular to the direction of the lines of force. It has already been pointed out that, when the direction is parallel, the maximum electric density is at the ends of a rectangular plate, and diminishes towards the centre; but, conversely, when the direction is perpendicular, the maximum electric density would appear to be on the central parts and evidently diminishes towards the edges, where, and for some distance from which, signs of electrification are entirely absent. Again, when the direction is parallel, the points or angular projections of the conductor, as has also been pointed out, accumulate a proportionally very large quantity of electricity; but experiment shows that the function of points and angular projections is the reverse of this when the plane of the conductor is perpendicular to the lines of force. It has shown also that the area of electrification, and, judging from the appearance of the electro-deposits, the quantity of electrification likewise, increases in an increasing proportion as the area of the plate increases, and furthermore that, with plates of a given area, the area of electrification (and probably the amount) is determined solely by the relation between the lengths and breadths of the plates.

In the annexed table is sett ou the areas of the — electrifications registered on plates differing in shape but of equal area, side by side with other particulars; and fig. 1 gives a rough idea of the appearance of the plates with the electro-deposit thereon*.

Area in square millimetres.	Dimensions in millimetres.	— electrifications in square millimetres.
500	100·0× 5	0
"	66·6× 7·5	40
"	50·0×10	180
"	33·3×15	270
"	22·4×22·4	370

Fig. 1.



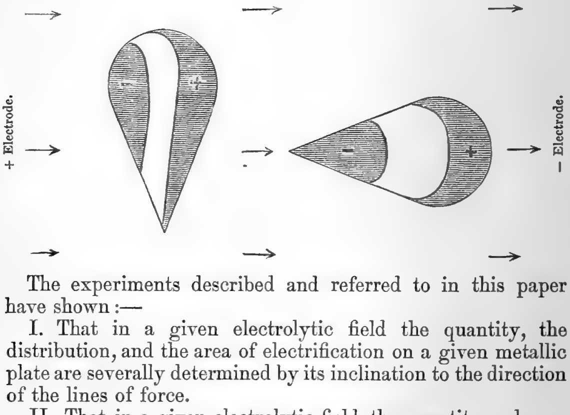
* Facsimiles of these plates, and of others described in my former papers, are given in the second edition of Gordon's 'Physical Treatise on Electricity,' vol. ii.

In the following table is set out in square millimetres the areas also of the — electrifications registered on a series of plates of the same shape but of different areas, side by side with other particulars.

Areas in square millimetres.	Dimensions in millimetres.	— electrifications in square millimetres.
100	10×10	0
225	15×15	64
400	20×20	225
1600	40×40	1225

In this paper I have, for the most part, spoken of the distribution of electricity on metallic plates; but I have satisfied myself that the distribution on other-shaped conductors is similarly modified by alterations in the direction of the lines of force. In illustration of this I subjoin a diagram (fig. 2) showing the distributions registered on a cone with a hemispherical base, placed so that its longer diameter was respectively parallel and perpendicular to the direction of the lines of force. The non-shaded parts of the figures represent the zones of no electrochemical action.

Fig. 2.



The experiments described and referred to in this paper have shown :—

I. That in a given electrolytic field the quantity, the distribution, and the area of electrification on a given metallic plate are severally determined by its inclination to the direction of the lines of force.

II. That in a given electrolytic field the quantity and area of electrification on a metallic plate of a given area, when its plane is perpendicular to the direction of the lines of force, are determined by the relation between its length and breadth.

III. That in a given electrolytic field the quantity and area of electrification on a plate of a given shape, when its plane is perpendicular to the direction of the lines of force, increase in an increasing proportion as the dimensions of the plate increase.

IV. That the distribution of electricity on a plate is substantially reversed when its plane is respectively parallel or perpendicular to the direction of the lines of force.

V. That when the direction of the lines of force, in an electrolytic field, is parallel to the sides and perpendicular to the ends of a metallic conductor, the distribution of electricity thereon is similar to the distribution of electricity set up on a similar-shaped conductor, in air or other gaseous dielectric, and subjected to ordinary inductive forces.

VI. That the power of points in accumulating electricity is, under the conditions set out in V., similar to the power shown in air when the conductor to which they are attached is subjected to ordinary inductive forces.

In conclusion, I would point out that our knowledge of the power of points to accumulate electric force, and, generally, of the distribution of electricity on conductors subjected to ordinary inductive forces in air, has been obtained under circumstances where it may be supposed that the direction of the lines of force was more or less parallel to the sides and perpendicular to the ends of such conductors. But I venture to think, seeing the close analogy which evidently exists under certain circumstances between the distribution of electricity on conductors in electrolytes and in dielectrics, that the further exploration of electrical distribution on conductors in air subjected to inductive forces differing in direction, will show that the laws governing the distribution of electricity on conductors in the respective electric fields are similar, if not identical. In other words, the distribution of electricity on metallic bodies in air may be found to be as dependent upon the direction of the lines of force as in electrolytes.

August 1883.

XLI. *The Dilatation of Crystals on Change of Temperature.*

By L. Fletcher, *M.A., of the Mineral Department, British Museum; late Fellow of University College, Oxford**.

[Second paper.]

[Plate III.]

IN a previous paper bearing the above title†, read before the Crystallological Society in November 1879, the following properties were shown to follow from the assumption that the

* Read before the Crystallological Society, July 3, 1883.

† Phil. Mag. 1880, vol. ix. p. 81.

geometrical and physical characters of a crystal are the same along all lines having the same direction :—

1. A plane of physical and geometrical symmetry is permanent for all changes of temperature so long as the crystalline arrangement is preserved. A crystal may thus, theoretically at least, pass on mere change of temperature from a system of lower to one of higher symmetry, but not *vice versâ*, without a preliminary destruction of the crystalline arrangement of its molecules.

2. A sphere at one temperature in general becomes an ellipsoid at a second.

3. Any three lines mutually perpendicular at the first temperature become three conjugate diameters of the ellipsoid at the second.

4. One particular triad of lines, namely that which at the second temperature coincides with the axes of the ellipsoid, is rectangular at both temperatures : the lines of this triad are the directions of greatest mean and least expansion, and have been called by Neumann *thermic axes*.

5. The pair of diameters at right angles to the circular sections of the ellipsoid are such that the expansion of all crystal-lines normal to them at the second temperature has been the same : they thus present a character analogous to that of the optic axes and of the magnetic axes of Plücker.

6. One triad of lines has absolutely the same position in space at the two temperatures, and at the suggestion of Prof. Maskelyne has been designated as a triad of *atropic lines* ; being in general not at right angles, they are distinct from the thermic axes ; and if *permanently* fixed in space for all variations of temperature they must be intimately related to the structure of the crystal, and would in such case have a claim to be regarded not as arbitrary but as veritable axes of the crystal.

7. An infinite number of triads of lines can be found which retain their mutual inclinations while changing their directions in space: the lines of such triads will in this paper be designated as *isotropic ;* the so-called thermic axes are merely the particular triad of isotropic lines for which the angles of mutual inclination are right angles.

8. If in a crystal belonging to the Oblique system the two lines in the symmetry-plane which are atropic for one pair of temperatures are atropic for all others, no lines isotropic for one pair of temperatures will retain their inclination permanently, and thus having no constancy of inclination can scarcely be important as structural lines of the crystal.

It was further shown that in the Cubic system all lines, and in the Tetragonal and Hexagonal systems all lines parallel or perpendicular to the axis of highest symmetry, are permanent in direction on change of temperature of the crystal; that in the Rhombic system both the thermic axes and the atropic lines are coincident with the crystallographic axes; that in the Oblique system one thermic axis and one atropic line coincide with the crystallographic axis perpendicular to the plane of symmetry, while the remaining thermic axes, atropic lines, and crystallographic axes lie without any evident connection in the plane of symmetry; and that in the Anorthic system the thermic axes, atropic lines, and crystallographic axes are apparently quite independent of each other.

With the view of experimentally testing the above theory of the possible want of permanency of the thermic axes, Mr. Beckenkamp*, at the instance of Professor Groth, has undertaken in the Strassburg laboratory the onerous task of making precise measurements of the angles of anorthite, axinite, orthoclase, and gypsum at various temperatures; and for this purpose a large goniometer reading to seconds has been at his disposal. Deducing from these measurements the positions of the thermic axes by the help of formulæ constructed by the elder and younger Neumann for Oblique and Anorthic crystals respectively†, Mr. Beckenkamp finds that, while the variation in the case of the other minerals examined is within the limits of the errors of experiment, in a crystal of anorthite the axis of minimum expansion for the pair of temperatures 20°—80° C. is as much as 26° 36′ distant from the corresponding axis for the pair of temperatures 20°—200° C.; and Mr. Beckenkamp further states that it is unnecessary to show that this is beyond any possible experimental error.

Any deviation, however small, from absolute rectangularity of the lines which are thermic axes for one pair of temperatures being clearly fatal to their permanency, the magnitude of this deviation was not touched upon in the first paper. As, however, the change in the mutual inclinations of these lines could scarcely be expected to exceed a few seconds, and the change in the position of the thermic axes perhaps a few minutes, it seemed necessary to carefully examine the original formulæ of Neumann and the observations and calculations of Beckenkamp before such an enormous variation as 26° 36′ in the position of a thermic axis could be considered absolutely

* *Zeitschrift für Krystallographie,* 1881, vol. v. p. 436; and 1882, vol. vi. p. 450.

† Pogg. *Ann.* 1833, vol. xxvii. p. 240; and 1861, vol. cxiv. p. 492.

proved. An examination undertaken with this object in view has led to the gradual development of the present paper.

It was soon found that the formulæ of Neumann are so complex, and are arrived at by so complicated a method, that it was far from easy to become quite confident that some small error might not have crept in to invalidate the result, more especially as Beckenkamp had himself had occasion to point out two printer's errors in the formulæ of the original paper of Neumann. This feeling will perhaps be more intelligible if we give Neumann's statement of the method by which the thermic axes are to be actually calculated. It is as follows:—

"Let OA, OB, OC be the crystallographic axes and have lengths A, B, C respectively: take three rectangular axes x_1, x_2, x_3, x_3 being coincident with OC, x_2 in the plane AOC and perpendicular to OC, and x_1 perpendicular to the plane AOC. Let $\alpha_1\,\alpha_2\,\alpha_3$, $\beta_1\,\beta_2\,\beta_3$, $\gamma_1\,\gamma_2\,\gamma_3$ be the direction-cosines of OA, OB, OC at the initial temperature θ with respect to the axes x_1, x_2, x_3; let the increments at the second temperature θ of the nine cosines $\alpha_1, \alpha_2, \alpha_3$, &c. &c. be denoted by $\Delta\alpha_1, \Delta\alpha_2, \ldots$, and of the lengths A, B, C by ΔA, ΔB, ΔC. For brevity write

$$\frac{\Delta A}{A}-\frac{\Delta C}{C}=\frac{\Delta a}{a} \text{ and } \frac{\Delta B}{B}-\frac{\Delta C}{C}=\frac{\Delta b}{b};$$

and let σ denote the determinant

$$\begin{vmatrix} \alpha_1 & \alpha_2 & \alpha_3 \\ \beta_1 & \beta_2 & \beta_3 \\ \gamma_1 & \gamma_2 & \gamma_3 \end{vmatrix}.$$

Find nine constants Λ from the following formula:—

$$\Lambda_k^{(i)}=\frac{\dfrac{d\sigma}{d\alpha_k}\Delta\alpha_i+\dfrac{d\sigma}{d\beta_k}\Delta\beta_i+\dfrac{d\sigma}{d\gamma_k}\Delta\gamma_i}{\sigma}+\frac{\dfrac{d\sigma}{d\alpha_k}\dfrac{\Delta a}{a}\alpha_i+\dfrac{d\sigma}{d\beta_k}\dfrac{\Delta b}{b}\beta_i}{\sigma},$$

and construct the equations

$$\begin{aligned}
2(\Lambda_1'-R)C_1 &+ (\Lambda_1''+\Lambda_2')C_2 + (\Lambda_1'''+\Lambda_3')C_3=0,\\
(\Lambda_2'+\Lambda_1'')C_1 &+ 2(\Lambda_2''-R)C_2 + (\Lambda_2'''+\Lambda_3'')C_3=0,\\
(\Lambda_3'+\Lambda_1''')C_1 &+ (\Lambda_3''+\Lambda_2''')C_2+2(\Lambda_3'''-R)C_3=0,\\
C_1^2 &+ C_2^2 + C_3^2=1.
\end{aligned}$$

Determine the roots R_1, R_2, R_3 of the cubic equation

$$\begin{vmatrix} 2(\Lambda_1'-R) & (\Lambda_1''+\Lambda_2') & (\Lambda_1'''+\Lambda_3') \\ (\Lambda_2'+\Lambda_1'') & 2(\Lambda_2''-R) & (\Lambda_2'''+\Lambda_3'') \\ (\Lambda_3'+\Lambda_1''') & (\Lambda_3''+\Lambda_2''') & 2(\Lambda_3'''-R) \end{vmatrix}=0.$$

Substituting in turn these roots in the preceding equations, calculate the values of C_1, C_2, C_3: each set of values of C_1, C_2, C_3 will give the direction-cosines relative to the rectangular axes x_1, x_2, x_3 of the directions at the second temperature θ' of thermic axes for the pair of temperatures θ and θ'."

It is not recorded that Neumann himself made any practical use of these formulæ; and it would appear that Pape, by his determination of the thermic axes of crystals of copper sulphate*, was the first to show that they come within the range of practical crystallography : the actual results, however, cannot be very precise, seeing that the instrument employed only afforded readings to a minute. Beckenkamp has now made extensive use of these formulæ to ascertain whether or not in an Anorthic crystal the same crystal-lines remain permanently at right angles, and decides the question in the negative.

It had occurred to the writer that results deduced from these formulæ would really be useless for the purpose of testing the permanency of the thermic axes, since the equations are obtained by neglecting small quantities of the second order upon which one would expect any change in the thermic axes to depend. In fact it will be shown later that on reversing the order of the temperatures, exactly the same equations, and consequently the same direction-cosines, as before will be obtained; but this time they must give the position of the thermic axes at the lower instead of at the higher temperature. In other words, the formulæ only give the position of the thermic axes within an angular distance equal to the actual displacement of the corresponding crystal-lines by the change of temperature.

As, however, the calculated deviation of $26° 36'$ in the position of one of the thermic axes of anorthite is evidently far beyond the displacement of any lines of the crystal, it would appear to result from some other cause than the neglect of the terms of the second order.

Since it is very difficult to imagine that two lines of a crystal can have the same mutual inclination at two different temperatures and yet deviate therefrom at intermediate ones, we propose to show in the first place by actual calculation that such a property is undoubtedly characteristic of crystalline bodies ; and incidentally we shall find the deviation in the case of the particular mineral used for illustration, thus obtaining a clue, however faint, to the order of magnitude of this deviation in the general case.

* Pogg. *Ann.* 1868, vol. cxxxv. p. 1.

The mineral we select for this illustration is rock-crystal; and for the following reasons :—

1st. Any section through the morphological axis contains two lines which are at once thermic axes and atropic lines for all temperatures;

2ndly. The expansions along these atropic lines have been determined by Fizeau with great accuracy; and

3rdly. These expansions are very different, one being almost double the other.

The precision attained by the method of Fizeau* will be better appreciated if we mention that two determinations made with especial care at times separated by an interval of about twelve months, during which the apparatus had been heated and cooled some hundreds of times, gave for the coefficient of expansion in the direction of the morphological axis 0·0000078118 and 0·0000078117 respectively.

We may here conveniently call attention to the fact that Fizeau's method when employed alone gives the variation in the thickness of the plate and not the variation in length of a crystal-line initially normal to its faces, the two quantities only being identical when the same line of the crystal is permanently normal to the plate. If h be the initial thickness of the plate, and h' the length at the second temperature of a line which, though at the first temperature normal to the plate, is at the second temperature inclined to the normal at an angle θ, the quantity measured by Fizeau is $h' \cos \theta - h$, and differs from the true expansion of the crystal-line, namely $h' - h$, by a quantity $h'(1 - \cos \theta)$, which, though always small, is in many cases quite appreciable ; if, for instance, for an interval of temperature of 100° C. the normal be rotated through an angle of 10′, as is sometimes the case, the correction of the coefficient of expansion for that interval is ·0000042 and for an interval of 1° C. is ·000000042, quantities scarcely to be neglected.

The present illustration is so chosen that this difficulty does not present itself, the expansions having been determined by Fizeau from plates normal to permanently atropic lines.

Let X O Z (Pl. III. fig. 1) be any section of a crystal of quartz of which O Z is the morphological axis and O X any line perpendicular to it, these lines being permanent in direction in space for all changes of temperature ; let the unit lengths along O X, O Z at the initial temperature t_1 become α, β at the temperature t_2 ; let L′ N′ be the position at the second temperature of the line which initially has the position L N, and let

* *Comptes Rendus*, 1868, vol. lxvi. p. 1006.

O P′, O P be the corresponding normals from O. Then

$$OL' = \alpha\, OL,$$

$$ON' = \beta\, ON;$$

whence

$$\tan P'X = \frac{OL'}{ON'} = \frac{\alpha}{\beta}\frac{OL}{ON} = \frac{\alpha}{\beta}\tan PX. \quad . \quad . \quad . \quad (1)$$

Similarly, if OQ′, OQ be the normals to a second plane at these two temperatures,

$$\tan Q'X = \frac{\alpha}{\beta}\tan QX\,; \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

whence, if the angle P′Q′ = PQ, or the planes P, Q be isotropic for the two temperatures t_1, t_2, we have

$$\tan(Q'X - P'X) = \tan(QX - PX),$$

or

$$\frac{\frac{\alpha}{\beta}(\tan QX - \tan PX)}{1 + \frac{\alpha^2}{\beta^2}\tan QX \tan PX} = \frac{\tan QX - \tan PX}{1 + \tan QX \tan PX};$$

whence

$$\tan QX = \frac{1}{\frac{\alpha}{\beta}\tan PX} = \tan P'Z \text{ from (1)}, \quad . \quad (3)$$

or

$$QX = P'Z = 90° - P'X, \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

a simple relation giving the position of a plane Q isotropic for this pair of temperatures to a given plane P.

It is now necessary to find the angle P″Q″ between these same planes P, Q at a third temperature t_3.

If the unit lengths along OX, OZ at t_1 become a and b respectively at t_3, we find, just as before, that

$$\tan P''X = \frac{a}{b}\tan PX,$$

$$\tan Q''X = \frac{a}{b}\tan QX,$$

$$= \frac{a}{b}\frac{\beta}{\alpha}\cot PX, \text{ from (3)};$$

whence

$$\frac{\tan Q''P''}{\tan QP}=\frac{\tan(Q''X-P''X)}{\tan(QX-PX)}$$

$$=\frac{\frac{a}{b}\frac{\beta}{\alpha}\cot PX-\frac{a}{b}\tan PX}{1+\frac{a^2}{b^2}\frac{\beta}{\alpha}}\times\frac{1+\frac{\beta}{\alpha}}{\frac{\beta}{\alpha}\cot PX-\tan PX}$$

$$=\frac{\alpha+\beta}{\alpha\frac{b}{a}+\beta\frac{a}{b}}\quad\text{or}\quad\frac{ab(\alpha+\beta)}{\alpha b^2+\beta a^2}, \quad . \quad . \quad . \quad . \quad . \quad . \qquad (5)$$

a relation which holds rigorously for any crystal-section containing two rectangular atropic lines.

The planes P, Q will thus not be permanently isotropic unless $ab(\alpha+\beta)$ equals $\alpha b^2+\beta a^2$, or $\frac{\alpha}{\beta}=\frac{a}{b}$ for all temperatures—a condition which is only satisfied for those crystals in which α is permanently equal to β.

In the case of rock-crystal, according to Fizeau's measurements*, if

$$t_1=10^\circ \text{ C.},\quad t_2=80^\circ \text{ C., and } t_3=50^\circ \text{ C.},$$

$$\alpha=1{\cdot}00100163,$$
$$\beta=1{\cdot}000552895,$$
$$a=1{\cdot}00055808,$$
$$b=1{\cdot}00030532;$$

whence, if P, Q be two planes isotropic for the pair of tempetures 10°—80° C., the angle between them at 50° C. will be given by the equation

$$\frac{\tan Q''P''}{\tan QP}=\frac{ab(\alpha+\beta)}{\alpha b^2+\beta a^2}=1{\cdot}0000002473,$$

or, to the second order of small quantities,

$$Q''P''-QP=0{\cdot}000000012365\sin 2QP. \quad . \quad . \qquad (6)$$

Hence the variation in the angle PQ will not exceed $\frac{1}{400}$th of a second.

If $\alpha=1+m$, $\beta=1+n$, $a=1+d$, $b=1+e$, and higher powers than the square of the small quantities m, n, d, e be neglected, it will be found that

$$\frac{ab(\alpha+\beta)}{\alpha b^2+\beta a^2}=1+\frac{(d-e)(m-n-d+e)}{2};$$

* *Comptes Rendus*, 1866, vol. lxii. p. 1145.

and thus to the same approximation,

$$Q''P''-QP=(d-e)(m-n-d+e)\frac{\sin 2QP}{4}. \quad . \quad . \quad (7)$$

Substituting the above values of m, n, d, e, it will be found in this manner that

$$Q''P''-QP=0{\cdot}000000012384 \sin 2QP, \quad . \quad . \quad (8)$$

which is virtually the same result as before.

Again, from equation (1) we have for the intermediate temperature t_3,

$$\tan P''X=\frac{a}{b}\tan PX,$$

whence

$$\tan P''P=\tan(P''X-PX)=\frac{\frac{a}{b}\tan PX-\tan PX}{1+\tan P''X\tan PX},$$

and, neglecting small quantities of the second order,

$$P''P=\frac{(a-b)}{2}\sin 2PX. \quad . \quad . \quad . \quad . \quad . \quad (9)$$

If for the sake of example we take $PX=30°$, we find in this way that the displacement of P for the change of temperature 10°—50° C. is about 23″: the change in the inclination of P to Q for the change of temperature 10°—50° is thus less than the $\frac{1}{9200}$ part of the absolute displacements of the poles P and Q.

From the above we think it will be clear that lines may be isotropic for one pair of temperatures without being so for others; that when there are two perpendicular atropic lines, this variation of angle will be a small quantity of the second order; and, further, that lines may be isotropic for one pair of temperatures and yet be absolutely without claim to importance as axes of the crystal.

In the previous paper it was mentioned that, whether the indices of the planes be rational or irrational, or, what comes to the same thing, whether the planes be natural or artificial, the indices will be unaltered by any change of temperature of the crystals; and, further, that the anharmonic ratios of any four natural or artificial planes in the same zone must be likewise constant on change of temperature, for they can be expressed in terms of the constant indices*. Considerable use of the latter property is made in the proof of the various propositions given in the present paper; as in the case of the anharmonic ratios themselves, the formulæ are true whatever

* 'A Tract on Crystallography,' by W. H. Miller, 1863, p. 10.

the relative positions of the poles if arcs measured in opposite directions are regarded as opposite in sign.

PROP. I. *Given the alterations of the angles between three planes of a zone, to determine the displacement of a fourth plane of the zone* [see also Props. V. and VI.] (fig. 2).

Let the poles a, c, d at the first temperature take up the positions a', c', d' at the second; and let it be required to find P′, the position at the second temperature of a pole having the position P at the first.

From the constancy of the anharmonic ratios we have

$$[c'd'\mathrm{P}'a'] = [cd\mathrm{P}a],$$

and

$$\frac{\sin \mathrm{P}'a'}{\sin \mathrm{P}'c'}\,\frac{\sin d'c'}{\sin d'a'} = \frac{\sin \mathrm{P}a}{\sin \mathrm{P}c}\,\frac{\sin dc}{\sin da},$$

whence

$$\frac{\sin \mathrm{P}'a'}{\sin (\mathrm{P}'a' - c'a')} = \frac{\sin \mathrm{P}a}{\sin \mathrm{P}c}\,\frac{\sin dc}{\sin d'c'}\,\frac{\sin d'a'}{\sin da}.$$

If

$$\tan \psi = \frac{\sin \mathrm{P}a}{\sin \mathrm{P}c}\,\frac{\sin dc}{\sin d'c'}\,\frac{\sin d'a'}{\sin da},$$

a known quantity, then

$$\tan\left(\mathrm{P}'a' - \frac{c'a'}{2}\right) = \tan\frac{c'a'}{2}\cot(\psi - 45^\circ),$$

a logarithmic formula by means of which the arc P′a' can readily be calculated.

PROP. II. *Given the alterations of the angles between three planes of a zone, to determine the position of a plane in the same zone isotropic to one of them.*

As before, let a, c, d be the given poles, and let it be required to find a pole α isotropic to a.

From the constancy of the anharmonic ratios,

$$[\alpha c d a] = [\alpha' c' d' a'],$$

whence

$$\frac{\sin \alpha c}{\sin \alpha a}\,\frac{\sin da}{\sin dc} = \frac{\sin \alpha' c'}{\sin \alpha' a'}\,\frac{\sin d'a'}{\sin d'c'}.$$

By hypothesis,

$$\alpha a = \alpha' a',$$

and thus

$$\frac{\sin \alpha c}{\sin \alpha' c'} = \frac{\sin dc}{\sin da}\,\frac{\sin d'a'}{\sin d'c'},$$

or

$$\frac{\sin (ac - a\alpha)}{\sin (a'c' - a\alpha)} = \frac{\sin dc}{\sin da}\,\frac{\sin d'a'}{\sin d'c'}.$$

If
$$\tan\phi = \frac{\sin dc}{\sin d'c'}\,\frac{\sin d'a'}{\sin da},$$
a known quantity, then
$$\tan\left(\frac{a'c'+ac}{2} - a\alpha\right) = \tan\frac{a'c'-ac}{2}\tan(45^\circ+\phi),$$
from which the arc $a\alpha$ can be readily found.

Prop. III. *To find a relation between any two pairs of isotropic planes in the same zone.*

If a, α, P, Q be two pairs of isotropic planes, we have, as before,
$$[aPQ\alpha] = [a'P'Q'\alpha'],$$
or
$$\frac{\sin aP \sin Q\alpha}{\sin a\alpha \sin QP} = \frac{\sin a'P' \sin Q'\alpha'}{\sin a'\alpha' \sin Q'P'}.$$
And since
$$a\alpha = a'\alpha' \text{ and } QP = Q'P',$$
we have
$$\frac{\sin a'P'}{\sin aP} = \frac{\sin Q\alpha}{\sin Q'\alpha'},$$
or
$$\frac{\sin(aP + PP' - aa')}{\sin aP} = \frac{\sin(Q'\alpha' + QQ' - \alpha\alpha')}{\sin Q'\alpha'}.$$
But
$$PP' = QQ' \text{ and } aa' = \alpha\alpha';$$
hence
$$aP = Q'\alpha'.$$

Or, if two pairs of planes be simultaneously isotropic, the angle between two of the planes, one from each pair, at the first temperature is equal to the angle between the other two planes at the second temperature.

Corollary.—In the first paper it was shown from mechanical considerations that, for any pair of temperatures, at least one pair of real planes is atropic.

If a, α be the poles of a pair of these planes, the relation just given becomes
$$aP = Q'\alpha.$$

Or if in any crystal two planes are isotropic for a given change of temperature, the angle which one of the isotropic planes makes at the first temperature with either of the atropic planes is equal to the angle which the second isotropic plane makes at the final temperature with the remaining atropic plane.

Prop. IV. *If two planes at right angles to the plane of symmetry of an Oblique crystal be permanently atropic, no other planes in this zone retain their mutual inclination at all temperatures* (fig. 3).

As before, let a, α be the planes which are permanently atropic, and let P, Q be planes isotropic for one pair of temperatures; whence $PQ=P'Q'$ and $PP'=QQ'$. From the last corollary $aP=Q'\alpha$: if p, q, S be the points of bisection of the arcs PP', QQ', $a\alpha$ respectively,

$$ap=q\alpha, \quad pq=PQ=P'Q', \text{ and } pS=Sq=\frac{PQ}{2}.$$

Whence, if the angle between the isotropic planes P, Q be given, the positions of p, q, the middle points of the arcs of displacement PP', QQ', are absolutely definite.

But if the temperature of the crystal be again changed and the poles P' Q' take up new positions P'' Q'', it is seen that the middle points of the arcs PP'' and QQ'' do not now coincide with $p\,q$; in other words, the planes P Q no longer retain their mutual inclination.

Prop. V. The following method of calculating the displacement of a given pole is sometimes more convenient than the method indicated in Prop. I.

Let a, α be the poles of any pair of isotropic planes, and d, d' the positions of a third pole at the two temperatures: if P, P' be the corresponding positions of a fourth pole, we have, as before,

$$[aPd\alpha]=[a'P'd'\alpha'],$$

$$\frac{\sin Pa}{\sin P\alpha}\,\frac{\sin d\alpha}{\sin da}=\frac{\sin P'a'\sin d'\alpha'}{\sin P'\alpha'\sin d'a'},$$

or

$$\frac{\sin Pa \sin P'\alpha'}{\sin P\alpha \sin P'a'}=\frac{\sin da \sin d'\alpha'}{\sin d\alpha \sin d'a'};$$

and is therefore, so long as we keep to the same pair of temperatures, constant for all positions of P.

Let

$$P'a'-Pa=P'\alpha'-P\alpha=\theta;$$

then

$$\frac{\sin Pa \sin P'\alpha'}{\sin P\alpha \sin P'a'}=\frac{1+\tan\theta\cot P\alpha}{1+\tan\theta\cot Pa};$$

whence

$$\frac{1+\tan\theta\cot P\alpha}{1+\tan\theta\cot Pa}=1+e,$$

where e is some small quantity dependent upon the temperatures, but independent of P.

Prop. VI. *To find an approximate formula for the displacement of a given pole* P.

Neglecting in the last formula higher powers of θ than the square, we find that

$$(1+\tan\theta\cot \mathrm{P}\alpha)(1-\tan\theta\cot \mathrm{P}a+\tan^2\theta\cot^2 \mathrm{P}a)=1+e,$$

whence

$$\tan\theta=\frac{e}{\sin\alpha a}\sin \mathrm{P}a\sin \mathrm{P}\alpha+\tan^2\theta\cot \mathrm{P}a.$$

If θ_1 be the first approximation to θ, obtained by neglecting the term of the second order $\tan^2\theta\cot \mathrm{P}a$, we have

$$\theta_1=k\sin \mathrm{P}a\sin \mathrm{P}\alpha,$$

where $k=\frac{e}{\sin\alpha a}$, and is thus a small quantity independent of the position of P.

If θ_2 be the second approximation,

$$\theta_2=\theta_1+\tan^2\theta_1\cot \mathrm{P}a.$$

Prop. VII. *To determine the value of the coefficient* k.

Let ϕ be the variation of any given angle $\mathrm{Q}a$; then (Prop. V.)

$$1+e=\frac{1+\tan\phi\cot \mathrm{Q}\alpha}{1+\tan\phi\cot \mathrm{Q}a},$$

whence

$$e=\frac{(\cot \mathrm{Q}\alpha-\cot \mathrm{Q}a)\tan\phi}{1+\tan\phi\cot \mathrm{Q}a},$$

and

$$k=\frac{e}{\sin\alpha a}=\frac{\sin\phi}{\sin \mathrm{Q}\alpha\sin(\mathrm{Q}a+\phi)},$$

a logarithmic formula by means of which k can be readily found.

Prop. VIII. *To find the maximum and minimum values of the term of the second order* $\tan^2\theta_1\cot \mathrm{P}a$.

This proposition is convenient for determining whether or not the term can be neglected in comparison with experimental errors.

Let $\qquad \alpha=180^\circ-\alpha a,$

then

$$\tan^2\theta_1\cot \mathrm{P}a=k^2\sin^2 \mathrm{P}a\sin^2(\mathrm{P}a+\alpha)\frac{\cos \mathrm{P}a}{\sin \mathrm{P}a}$$

$$=\frac{k^2}{2}\sin 2\mathrm{P}a\sin^2(\mathrm{P}a+\alpha).$$

The maximum value of the term can never exceed $\frac{k^2}{2}$.

Differentiating with respect to $\mathrm{P}a$, we have for a maximum or minimum,

$$2\cos 2\mathrm{P}a \sin^2(\mathrm{P}a+\alpha)+2\sin 2\mathrm{P}a \sin(\mathrm{P}a+\alpha)\cos(\mathrm{P}a+\alpha)=0,$$

or

$$\sin(\mathrm{P}a+\alpha)\sin(3\mathrm{P}a+\alpha)=0.$$

If $\sin(\mathrm{P}a+\alpha)=0$ the displacement of the pole is absolutely zero.

From $\sin(3\mathrm{P}a+\alpha)=0$ we deduce

$$\mathrm{P}a=\frac{\alpha a}{3},\quad \frac{\alpha a}{3}+60^\circ, \text{ or } \frac{\alpha a}{3}+120^\circ.$$

The corresponding values of the above term are

$$\frac{k^2}{2}\sin^3\frac{2}{3}\alpha a,\quad \frac{k^2}{2}\sin^3\left(\frac{2\alpha a}{3}+120^\circ\right),\quad \frac{k^2}{2}\sin^3\left(\frac{2\alpha a}{3}-120^\circ\right).$$

Prop. IX. *To find the lines of greatest and least expansion lying in a given zone of any crystal.*

If θ be the common displacement of any two isotropic poles P, Q lying in the zone, we find from Prop. V. that

$$\tan\theta=\frac{e}{\cot \mathrm{P}\alpha-(1+e)\cot \mathrm{P}a}=\frac{e}{\cot \mathrm{Q}\alpha-(1+e)\cot \mathrm{Q}a},$$

whence

$$\frac{\sin \mathrm{P}a \sin \mathrm{Q}a}{\sin \mathrm{P}\alpha \sin \mathrm{Q}\alpha}=1+e;$$

an equation which may readily be transformed into a logarithmic formula for determining Q the plane isotropic to P, if a, α, e, and P be given.

If the isotropic planes be rectangular, as is the case when their intersections with the zone-plane are the lines of greatest and least expansion,

$$\mathrm{Q}a=\mathrm{P}a+90^\circ,$$
$$\mathrm{Q}\alpha=\mathrm{P}\alpha+90^\circ;$$

and the above relation becomes

$$\frac{\sin 2\mathrm{P}a}{\sin 2\mathrm{P}\alpha}=1+e,$$

whence

$$\tan(2\mathrm{P}a-\alpha a)=\left(1+\frac{2}{e}\right)\tan\alpha a,$$

from which two positions of P may be deduced.

Prop. X. *If the atropic planes be permanent and two other planes be isotropic for a given pair of temperatures, to find the change in the mutual inclination of the latter at other temperatures.*

If θ, ϕ be the displacements of P and Q respectively for a change of temperature defined by the above quantity e, then, as before,

$$\tan\theta = \frac{e}{\cot P\alpha - (1+e)\cot Pa},$$

$$\tan\phi = \frac{e}{\cot Q\alpha - (1+e)\cot Qa},$$

whence

$$\tan(\theta-\phi) = e\frac{\cot Q\alpha - \cot P\alpha - (1+e)(\cot Qa - \cot Pa)}{e^2 + [\cot P\alpha - (1+e)\cot Pa][\cot Q\alpha - (1+e)\cot Qa]}.$$

If the planes P, Q are isotropic for a change of temperatures defined by the small quantity f corresponding to e, it will follow in exactly the same way that

$$\cot Q\alpha - \cot P\alpha - (1+f)(\cot Qa - \cot Pa) = 0.$$

Substituting in the above value of $\tan(\theta-\phi)$, we get

$$\tan(\theta-\phi) = \frac{e(f-e)(\cot Qa - \cot Pa)}{e^2 + [\cot P\alpha - (1+e)\cot Pa][\cot Q\alpha - (1+e)\cot Qa]};$$

and to the second order of small quantities,

$$\tan(\theta-\phi) = e(f-e)\,\frac{e(f-e)(\cot Qa - \cot Pa)}{[\cot P\alpha - \cot Pa][\cot Q\alpha - \cot Qa]}$$

$$= e(f-e)\frac{\sin PQ \sin P\alpha \sin Q\alpha}{\sin^2 \alpha a}.$$

Unless $\sin\alpha a$ is very small and comparable with e and f, the variation in the inclination of P to Q is a small quantity of the second order.

Prop. XI. We shall now consider the results which are obtained on neglecting the squares of small quantities.

(a) We have already seen in Prop. VI. that the displacement of any pole P is in this case given by the relation

$$\theta = k \sin Pa \sin P\alpha,$$

where k is some small quantity independent of the position of P.

(b) If γ, δ be the variations of the arcs ac, ad, then

$$\gamma = k \sin ca \sin(a\alpha - ac),$$

$$\delta = k \sin da \sin(a\alpha - ad),$$

whence both $a\alpha$ and k can be determined. Practically, however, it is quite as easy to use the rigorous formulæ of Propositions II. and VII.

(c) From the symmetry of the expression $k \sin Pa \sin P\alpha$

with respect to a and α, it is seen immediately that any two poles equidistant respectively from a and α and lying in the arc $a\alpha$ are isotropic.

(d) As a particular case, the directions of greatest and least expansion are inclined at an angle of 45° on opposite sides of the bisector of the arc $a\alpha$, and thus have fixed directions in space at all temperatures if the planes $a\,\alpha$ are themselves permanent in direction: as the crystal-lines momentarily coincident with them are in motion, the crystal-lines cannot retain this property of being directions of greatest and least expansion (see previous Paper).

(e) If θ, ϕ be the variations of the arcs Pa, Qa for the same change of temperature,

$$\frac{\theta}{\phi}=\frac{\sin \mathrm{P}a \sin \mathrm{P}\alpha}{\sin \mathrm{Q}a \sin \mathrm{Q}\alpha}.$$

Hence if the planes $a\,\alpha$ be permanently isotropic, the increment of any arc Pa bears a constant ratio to that of any other arc Qa for all temperatures.

(f) If P, p be any two rectangular planes and θ, θ' the respective variations of the arcs Pa, pa,

$$\theta+\theta'=k\,[\sin \mathrm{P}a \sin \mathrm{P}\alpha+\sin pa \sin p\alpha]=k \cos \alpha a,$$

or $\theta+\theta'$ is constant for all positions of P and p.

If ϕ, ϕ' be the corresponding variations for two other rectangular planes Q, q, then, as before, $\phi+\phi'=k \cos \alpha a$, whence $\theta-\phi=-(\theta'-\phi')$; or the increase of the angle between any two planes P, Q is equal to the diminution of the angle between the planes perpendicular to them.

Prop. XII. *The motion of the plane-normal* OP *is quite distinct from that of the crystal-line* OP (fig. 4).

It is easy to fall into the mistake of assuming that the formula $\theta=k \sin \mathrm{P}a \sin \mathrm{P}\alpha$ gives the displacement of the crystal-line OP. That this is not actually the case will be clear from the following :—

Let the *pole* P take at the second temperature the position P′ on the circle of projection, and the *point* P the position p' on the ellipse: the normal OP thus becomes the normal OP′, but the crystal-line OP becomes the crystal-line Op'; further, the plane tangent to the circle at P, and thus having OP for normal, will become the tangent plane to the ellipse at p', to which the ray Op' is only normal when p' is at the extremity of a principal axis.

The difference of motion of the plane-normal OP and the crystal-line OP may be found thus :—

Let Q be a pole distant 90° from P, so that Qa = Pa + 90° and Qα = Pα + 90° : if Q′ be the displaced position of the pole

Q, then (Prop. VI.)

$$Q'OQ = k \sin Qa \sin Q\alpha = k \cos Pa \cos P\alpha.$$

Now the crystal-line RQ tangent to the circle at Q, and thus parallel to the line OP, will become a tangent line $r'q'$ to the ellipse and parallel to the tangent to the circle at Q′; since parallel lines of the crystal are rotated through the same angle, the crystal-line OP will be rotated through the same angle as the tangent to the circle at Q, *i. e.* through the angle $k \cos Pa \cos P\alpha$. The difference of motion of OP the normal and OP the ray will therefore be

$$k \sin Pa \sin P\alpha - k \cos Pa \cos P\alpha,$$

or

$$-k \cos (Pa + P\alpha),$$

which will only vanish when

$$Pa + P\alpha = (2n+1)\frac{\pi}{2},$$

which is the case when OP is a line of greatest or least expansion (see Prop. XI. *d*).

Prop. XIII. *To find a physical expression for the quantity k* (fig. 5).

We have already seen that in the formula $\theta = k \sin Pa \sin P\alpha$, a, α are two isotropic poles; we shall now show that the coefficient k when expressed in circular measure is equal to the difference of the greatest and least coefficients of expansion of the crystal-lines lying in the plane of the zone.

Let Oa, $O\alpha$ be the normals to a pair of isotropic planes a, α; and let OL, OM, OL′, OM′ be the positions of the crystal-lines of greatest and least expansion at the two temperatures.

If OP, OP′ be the normals to LM, L′M′, and δ, δ_1 the coefficients of expansion of the lines OL, OM, we must have

$$\frac{OL'}{OL} = (1+\delta), \quad \frac{OM'}{OM} = (1+\delta_1),$$

whence

$$\frac{\tan P'OL'}{\tan POL} = \frac{1+\delta}{1+\delta_1}.$$

Since by Prop. IX. the position of L, and by Prop. I. the arc $L'a'$ can be calculated to any degree of accuracy, and also the arc $P'a'$ corresponding to any known arc Pa, the relation

$$\frac{1+\delta}{1+\delta_1} = \frac{\tan (P'a' - L'a')}{\tan (Pa - La)}$$

is one from which the ratio $\dfrac{1+\delta}{1+\delta_1}$ can likewise be calculated to any degree of precision.

Neglecting squares of small quantities,

$$\tan P'OL' - \tan POL = (\delta - \delta_1) \tan POL\ ;$$

or, to the same approximation,

$$P'OL' - POL = (\delta - \delta_1) \sin POL \cos POL.$$

Since

$$P'OL' - POL = P'OP - L'OL,$$

we may write the relation thus,

$$P'OP - L'OL = (\delta - \delta_1) \sin POL \cos POL \quad . \quad . \quad . \quad (A)$$

From the above formula (Prop. VI.),

$$P'OP = k \sin Pa \sin P\alpha = \frac{k}{2}[\cos \alpha a - \cos(Pa + P\alpha)].$$

By Proposition XII. the motion of the crystal-line OL is the same as that of the plane-normal OL, for OL is a line of greatest or least expansion.

Hence

$$L'OL = k \sin La \sin L\alpha = \frac{k}{2}[\cos \alpha a - \cos (La + L\alpha)],$$

and

$$P'OP - L'OL = \frac{k}{2}[\cos (La + L\alpha) - \cos (Pa + P\alpha)]$$

$$= k \sin \frac{Pa - La + P\alpha - L\alpha}{2} \sin \frac{Pa + La + P\alpha + L\alpha}{2}$$

$$= k \sin PL \sin (PL + La + L\alpha).$$

But, by Prop. XI. (d),

$$\sin (La + L\alpha) = \pm 1.$$

Hence

$$P'OP - L'OL = \pm k \sin PL \cos PL. \quad . \quad . \quad . \quad (B)$$

Equating the two values given by (A) and (B), we find k is equal to $\pm(\delta - \delta_1)$, and is independent of the particular pair of isotropic poles a, α employed in the formula $\theta = k \sin Pa \sin P\alpha$.

The positive or negative sign must be taken according as $\sin (La + L\alpha)$ is equal to $+1$ or -1, OL being that axis for which the coefficient of expansion is δ.

Prop. XIV. *To find the coefficient of expansion Δ of a line OP inclined to a direction of greatest or least expansion at an angle θ* (fig. 5).

Let δ, δ_1 be the coefficients of expansion of the lines OL, OM, and let it be required to find the coefficient of expansion of OP where $POL = \theta$.

If P'N', N'O be the rectangular coordinates of P', the second position of P, referred to the lines OL′, OM′, the corresponding positions of the crystal-lines OL, OM, then, as before,

$$ON' = (1+\delta)\ ON = (1+\delta)\ OP\cos\theta,$$

and
$$P'N' = (1+\delta_1)\ PN = (1+\delta_1)\ OP\sin\theta.$$

Whence, neglecting squares of small quantities,

$$OP'^2 = OP^2(1+2\delta\cos^2\theta + 2\delta_1\sin^2\theta);$$

and if
$$OP' = (1+\Delta)OP,$$

$$\Delta = \delta\cos^2\theta + \delta_1\sin^2\theta,$$

or
$$\Delta - \delta = (\delta_1 - \delta)\sin^2\theta.$$

PROP. XV. *To determine the expansion perpendicular to the symmetry-plane of an Oblique crystal* (fig. 6).

If, at the two temperatures, bm, bm' be the angular distances from b of a pole not lying in the plane of symmetry, it was shown (Prop. XIII.), from the fact of bh remaining permanently a quadrant, that, omitting small quantities of the second order,

$$bm' - bm = (\delta_2 - \Delta)\sin bm \sin mh,$$

Δ being the coefficient of expansion of Oh, the line of intersection of the zone-plane $[bm]$ with the plane of symmetry, and δ_2 the coefficient of expansion of Ob, a line perpendicular to the plane of symmetry.

If θ be the inclination of Oh to that thermic axis of which the coefficient of expansion is δ, then (Prop. XIV.)

$$\Delta - \delta = (\delta_1 - \delta)\sin^2\theta,$$

whence

$$\delta_2 - \delta = \frac{bm' - bm}{\sin bm \sin mh} + (\delta_1 - \delta)\sin^2\theta.$$

We shall now illustrate the above formulæ by a practical application of them.

The only published measurements of the angles of an Oblique crystal at different temperatures were, until the recent determinations of Beckenkamp, those made upon gypsum by Mitscherlich, the discoverer of the angular variations produced in crystals by change of temperature. From these measurements Neumann has calculated the positions and expansions of the thermic axes, and we shall thus be able to directly compare the results obtained by his method with those deduced from the preceding formulæ.

Fig 7 a represents a crystal of gypsum: b is the cleavage-plane and plane of symmetry (0 1 0), $m\ m_1$ are planes of the prism {1 1 0}, and $l\ l_1$ of the form {1 1 1}; the planes a (1 0 0),

$d\,(1\,0\,1)$, if they had presented themselves, would have truncated the edges $m\,m_1$, $l\,l_1$ respectively.

In the original paper* Mitscherlich states that, on a variation of temperature amounting to 100° C., the angle between the planes $m\,m_1$ becomes more obtuse by 10′ 50″, the angle between the planes $l\,l_1$ more obtuse by 8′ 25″, and the angle between the edges $d\,a$ more obtuse by 7′ 26″.

It is worthy of remark that, from the other results given in the same paper, we infer that these variations have not been directly observed, but have been calculated from the observed differences on the assumption that the changes of angle are proportional to the changes of temperature and are independent of the absolute temperatures at which the measurements are made. Perhaps this may partly account for the difference in the results obtained by Mitscherlich and Beckenkamp.

Mitscherlich gives no absolute measurements and no initial temperature. We shall adopt the absolute values as determined by Neumann† for an initial temperature of about $18\frac{3}{4}$° C., and used by him in his own calculations.

We then have

First temperature [Neumann].		Variations [Mitscherlich].	Second temperature.	
	° ′	′ ″		° ′ ″
$a\,m$	34 19	−5 25	$a'\,m'$	34 13 35
$d\,l$	18 9	−4 12·5	$d'\,l'$	18 4 47·5
$a\,d$	52 16	−7 26	$a'\,d'$	52 8 34

Let the zone $m\,l$ (fig. 7 *b*) meet the plane of symmetry in the pole $c\,(0\,0\,1)$; we require to calculate ac.

In the spherical triangle $m\,b\,l$, knowing the two sides $m\,b$, $b\,l$, and the included angle $m\,b\,l$, or the arc $a\,d$, we find that at the first temperature $m\,l = 49°\,0'\,35''{\cdot}73$, and at the second temperature $= 48°\,56'\,4''{\cdot}63$; also the angle $m\,l\,b$ at the first temperature $= 59°\,55'\,31''{\cdot}7$, and at the second $= 59°\,58'\,44''{\cdot}5$: hence from the right-angled triangle $c\,d\,l$, knowing $d\,l$ and the angle $d\,l\,c\;(=m\,l\,b)$, we find that $d\,c$ at the first temperature $= 28°\,16'\,37''{\cdot}2$, and at the second temperature $= 28°\,14'\,20''{\cdot}8$.

By addition we have

$$a\,c\;.\;.\;.\;80°\,32'\,37''{\cdot}2, \qquad a'\,c'\;.\;.\;.\;80°\,22'\,54''{\cdot}8.$$

(*a*) Find the pole α isotropic to a. From Prop. II. we have

$$\tan\phi = \frac{\sin\,dc\,\sin\,d'a'}{\sin\,d'c'\,\sin\,da} = \frac{\sin 28°\,16'\,37''{\cdot}2\,\sin 52°\,8'\,34''}{\sin 28°\,14'\,20''{\cdot}8\,\sin 52°\,16'\,0''};$$

* *Abh. d. k. P. Ak. zu Berlin*, 1825, p. 212.

† Pogg. *Ann.* 1833, vol. xxvii. p. 262.

whence

$$\phi = 44^\circ\ 59'\ 13''\cdot93.$$

Also

$$\tan\left(\frac{a'c'+ac}{2} - a\alpha\right) = \tan\frac{a'c'-ac}{2}\tan(45^\circ+\phi);$$

whence

$$\tan(80^\circ\ 27'\ 46'' - a\alpha) = -\tan 4'\ 51''\cdot2 \tan 89^\circ\ 59'\ 13''\cdot93,$$

$$= \frac{\tan 4'\ 51''\cdot2}{\tan\ 46''\cdot07}$$

$$= \tan 81^\circ\ 0'\ 35''\cdot7,$$

and

$$a\alpha = 161^\circ\ 28'\ 21''\cdot7,$$

the arc being measured in the positive direction, namely ac.

Also

$$\alpha = 180^\circ - a\alpha = 18^\circ\ 31'\ 38''\cdot3.$$

(*b*) Neglecting squares of small quantities, as is done by Neumann, the positions of the lines of greatest and least expansion can now be immediately found. From Prop. XI. d, if T be the axis nearest to a in the arc $a\alpha$,

$$a\mathrm{T} = \frac{a\alpha}{2} - 45^\circ$$

$$= 35^\circ\ 44'\ 10''\cdot85.$$

Neumann gives as result 35° 46′.

(*c*) If δ, δ_1 be the principal expansions, we have seen (Prop. XIII.), that

$$\delta_1 - \delta = k = \frac{\theta}{\sin \mathrm{P}a\ \sin \mathrm{P}\alpha},$$

for in this case $\sin(\mathrm{T}a + \mathrm{T}\alpha) = -1$.

If P coincide with c, we find

$$\delta_1 - \delta = \frac{582\cdot4 \times \text{circ. meas. of } 1''}{\sin 80^\circ\ 32'\ 37''\cdot2\ \ \sin 80^\circ\ 55'\ 44''\cdot5}$$

$$= \cdot0028987,$$

where δ refers to the axis nearest to the pole a in the arc $a\alpha$. Neumann gives as result ·002892.

Finally, by the method of Prop. XV., illustrated later by example (page 300), $\delta_2 - \delta$ may be calculated.

To ascertain to what extent we shall be justified in neglecting the small terms of the second order, we shall make some calculations founded on the more exact formulæ given above :—

(*a*) From Prop. VII. we have

$$k=\frac{\sin\phi}{\sin c\alpha\sin(ca+\phi)},$$

where we may write

$$ca=\ 80^\circ\ 32'\ 37''\cdot 2,$$
$$c\alpha=-80^\circ\ 55'\ 44''\cdot 5,$$
$$\phi=-582''\cdot 4;$$

whence

$$k=598''\cdot 18$$
$$=\ \cdot 0029001$$

in circular measure. Whence, as before only to the first approximation,

$$\delta_1-\delta=\cdot 0029001.$$

(*b*) If θ_1 be the displacement of a pole P when small quantities of the second order are neglected, we have

$$\theta_1=k\sin \mathrm{P}a\sin \mathrm{P}\alpha=k\sin \mathrm{P}a\sin(\mathrm{P}a-\alpha a),$$
$$=k\sin \mathrm{P}a\,(\sin \mathrm{P}a+\alpha-180^\circ),$$
$$=-k\sin \mathrm{P}a\sin(\mathrm{P}a+\alpha);$$

whence in the present instance

$$\theta_1=-598''\cdot 18\sin \mathrm{P}a\sin(\mathrm{P}a+18^\circ\ 31'\ 38''\cdot 3).$$

To show the displacements in various parts of the zone, the values of θ_1 for poles at every 10° from a are given below :—

P*a*.	θ_1''.	P*a*.	θ_1''.	P*a*.	θ_1''.
10°	− 49·61	70°	−561·92	130°	−239·24
20	−127·44	80	−582·58	140	−140·75
30	−224·10	90	−567·18	150	− 59·49
40	−327·94	100	−517·57	160	− 5·26
50	−426·43	110	−439·74	170	+ 15·40
60	−507·69	120	−343·08	180	0

Perhaps the alteration of the angles in various parts of the zone is more clearly shown by taking the differences of the above terms, and thus finding the variations of a series of arcs each of which is initially 10° in length.

We then have (still neglecting squares of small quantities):—

Arc.	Variation.	Arc.	Variation.
10°–20°	− 77″·83	100°–110°	+ 77″·83
20–30	− 96·66	110–120	+ 96·66
30–40	−103·84	120–130	+103·84
40–50	− 98·49	130–140	+ 98·49
50–60	− 81·26	140–150	+ 81·26
60–70	− 54·23	150–160	+ 54·23
70–80	− 20·66	160–170	+ 20·66
80–90	+ 15·40	170–180	− 15·40

This table also serves as a numerical example of Prop. XI. *f*, which states that to this approximation the quantities in the second and fourth columns are equal in magnitude but opposite in sign.

To proceed to the next degree of approximation we must calculate the value of the expression $\tan^2\theta_1 \cot Pa$, and substitute in the formula

$$\theta_2 = \theta_1 + \tan^2\theta_1 \cot Pa.$$

The smaller term calculated for every 10° is given in the following table in seconds :—

Pa.	$\tan^2\theta_1 \cot Pa$.	Pa.	$\tan^2\theta_1 \cot Pa$.	Pa.	$\tan^2\theta_1 \cot Pa$.
10°	+0″·07	70°	+0″·56	130°	−0″·23
20	+0·22	80	+0·29	140	−0·11
30	+0·42	90	0	150	−0·03
40	+0·62	100	−0·23	160	0
50	+0·74	110	−0·34	170	−0·01
60	+0·72	120	−0·33	180	0

The second term is thus a positive maximum for a pole between 50° and 60°; a negative maximum for a pole between 110° and 120°, and also for a pole between 160° and 180°; the largest maximum being the one first mentioned.

We find from Prop. VIII. that the exact distances of these poles from *a* are respectively

$$53° \, 49' \, 27''\cdot2, \; 113° \, 49' \, 27''\cdot2, \; 173° \, 49' \, 27''\cdot2;$$

and the largest maximum will be

$$\frac{k^2}{2} \sin^3 107° \, 38' \, 54''\cdot4$$

$$= 0''\cdot80.$$

The second term thus reaches a maximum value of only four-fifths of a second, and only exceeds half a second for poles in the small arc extending from 35° to 70° from a. The second term might thus, in the case of gypsum at least, be neglected in comparison with experimental errors.

To find the exact positions of the lines of greatest and least expansion, assuming the measurements of the angles to be perfect.

From page 296,

$$\frac{e}{\sin \alpha a} = k = \cdot 0029001,$$

whence

$$\frac{2}{e} = 2170\cdot 332.$$

If T be the principal axis nearest to a, then, from Prop. IX.,

$$\tan(2\mathrm{T}a - \alpha a) = \left(1 + \frac{2}{e}\right) \tan \alpha a = 2171\cdot 332 \tan \alpha a;$$

whence

$$2\mathrm{T}a - \alpha a + 90° = 283''\cdot 46,$$

and

$$\mathrm{T}a = 35°\ 46'\ 32''\cdot 58.$$

To test the accuracy of the formulæ we may calculate the motions of T and T_1 respectively relative to that of a.

(a) For the motion of T,

$$\theta_1 = -598''\cdot 18 \ \sin 35°\ 46'\ 32''\cdot 58 \ \sin(35°\ 46'\ 32''\cdot 58 + 18°\ 31'\ 38''\cdot 3)$$

$$= -284''\cdot 000,$$

$$\theta_2 - \theta_1 = \tan^2\theta_1 \cot \mathrm{T}a$$

$$= +0''\cdot 54266;$$

whence the motion of T to the *second* order of small quantities is $283''\cdot 457$.

(b) For the motion of T_1,

$$\theta_1 = -598''\cdot 18 \ \sin 125°\ 46'\ 32''\cdot 58 \ \sin(125°\ 46'\ 32''\cdot 58 + 18°\ 31'\ 38''\cdot 3)$$

$$= -283''\cdot 178,$$

$$\theta_2 - \theta_1 = \tan^2\theta_1 \cot \mathrm{T}_1 a = -0''\cdot 28014;$$

whence the motion of T_1 to the *second* order of small quantities is $283''\cdot 458$. T and T_1 are therefore isotropic, and distant 90° from each other at both temperatures.

We may remark that the values above deduced agree very satisfactorily with Prop. IV., which states that the middle point of the arc traced out by T must be distant exactly 45° from the point of bisection of the arc $a\alpha$.

To find the change at the second temperature in the mutual inclination of two planes which at the first temperature are perpendicular to each other but inclined to the thermic axes at an angle of 1° (Prop. VI.).

If P, Q be the two planes,

$$\mathrm{P}a = 36° \; 46' \; 32''\cdot 58, \quad \mathrm{Q}a = \mathrm{P}a + 90°.$$

(a) For the motion of P,

$$\theta_1 = -598''\cdot 18 \;\; \sin 36° \; 46' \; 32''\cdot 58 \;\; \sin 55° \; 18' \; 10''\cdot 88$$
$$= -294''\cdot 438,$$
$$\tan^2 \theta_1 \cot \mathrm{P}a = +0\cdot 56080;$$

whence the motion of P is $-293''\cdot 877$.

(b) For the motion of Q,

$$\theta_1 = -598''\cdot 18 \;\; \sin 126° \; 46' \; 32''\cdot 58 \;\; \sin 145° \; 18' \; 10''\cdot 88$$
$$= -272''\cdot 740,$$
$$\tan^2 \theta_1 \cot \mathrm{Q}a = -0''\cdot 26955;$$

whence the motion of Q is $273''\cdot 010$; and the change of inclination of P to Q is only $20''\cdot 867$.

If, then, we wish to find by trial a pair of planes which can be proved by direct measurement of the angle between them to retain their perpendicularity, a possible error of one second in the measurement of the angle between the planes will lead to a possible error of $\frac{3600}{20\cdot 867}$, or 172·52 seconds in the determination of the position of the pair of planes relative to the given plane a.

To find the motion relative to the plane a of a plane inclined to α at an initial angle of 1° (Prop. VI.).

Here $\theta_1 = -598''\cdot 18 \;\; \sin 1° \;\; \sin 19° \; 31' \; 38''\cdot 3$

$$= -3''\cdot 4895,$$

and

$$\tan^2 \theta_1 \cot \mathrm{P}a = 0;$$

whence the motion relative to a is only $-3''\cdot 4895$. Hence if we are given the plane a, and are required to find a plane α which can be proved by direct measurement to be equally inclined to a at the two temperatures, a possible error of one second in the measurement of the angle between the planes will lead to a possible error of $\frac{3600}{3\cdot4895}=1031\cdot7$ seconds in the determination of the isotropic plane α.

The last two numerical results serve to illustrate the difficulty which would be experienced in attempting to determine by direct measurement whether two planes are absolutely or only approximately coincident with isotropic planes; in other words, whether or not two planes are permanently isotropic.

The expansion δ_2 perpendicular to the plane of symmetry found (Prop. XV.).

Since aT is $35°\ 46'\ 32''\cdot58$, the expansion Δ in the direction Oa will be given by

$$\Delta-\delta=(\delta_1-\delta)\sin^2 35°\ 46'\ 32''\cdot58 \text{ [Prop. XIV.]},$$

where $\delta_1-\delta=\cdot0029001;$

thus $\Delta-\delta=+\cdot000991177.$

Also (Prop. XIII.)

$$\delta_2-\Delta=\frac{2(bm'-bm)}{\sin 2bm}.$$

From page 294, $bm'-bm=5'\ 25''$,

$bm=55°\ 41'$,

whence $\delta_2-\Delta=+\cdot0033839,$

and $\delta_2-\delta=0\cdot0043757.$

Neumann's result is 0·004371.

With the exception of Propositions IV. and XV., which require the zone-plane to be a plane of symmetry, all the above propositions are applicable not only to planes perpendicular to a plane of symmetry, but to any zone of a crystal whatever the system to which the crystal belongs; but it is clear that the directions of the maximum and minimum expansions of the crystal for lines lying in a given zone-plane are only those of the principal expansions of the whole crystal when the zone-plane is a plane of symmetry.

[To be continued.]

XLII. *The Law of Error.* *By* F. Y. Edgeworth*.

THE Law of Error is deducible from several hypotheses, of which the most important is that every measurable (physical observation, statistical number, &c.) may be regarded

* Communicated by the Author.

as a function of an indefinite number of elements, each element being subject to a determinate, although not in general the same, law of facility. Starting from this hypothesis, I attempt, *first*, to reach the usual conclusion by a path which, slightly diverging from the beaten road, may afford some interesting views ; *secondly*, to show that the exceptional cases in which that conclusion is not reached are more important than is commonly supposed.

The first step is one taken by Mr. Glaisher*, enabling us to regard the compound error as a *linear* function of the indefinitely numerous elements. A second step is, after Laplace, to express the sought function as a particular term of a certain multiple. Let us suppose at first that the elemental facility-functions are all identical and symmetrical, involving only even powers of one and the same variable; say $y=f(z^2)$, where

$$\int_{-\infty}^{\infty} f(z^2)dz=1.$$

Then the sought expression, the ordinate of the curve under which the values of the compound error are ranged, say u_{xs} (where x is the extent of error, and s the number of elements), is the $\frac{x}{\omega}$th term of the multiple

$$[f(z)t^{-\frac{z}{\omega}}+\&c.+f(2\omega)t^{-2}+f(\omega)t^{-1}+f(0)t^0+f(\omega)t^1+f(2\omega)t^{+2} \\ +\&c.+f(z)t^{+\frac{z}{\omega}}]^s.$$

Observing the formation of the coefficient of $t^{\frac{x}{\omega}}$ in the $(s+1)$th power of the expression within the brackets, we have

$$u_{s+1,x}=\int_{-\infty}^{\infty} f(z)u_{x+z,s}dz, \quad . \quad . \quad . \quad . \quad (1)$$

when only even powers of z are involved ; for otherwise the above integral will have to be separated into three parts. Assuming such a tendency towards a limiting form that the effect of proceeding from the sth to the $(s+1)$th power is indefinitely small (for that part of the result, those values of x with which we are concerned), we may write the left-hand member of (1), $u+\frac{du}{ds}$. The right-hand member may be ex-

* Monthly Notices Roy. Astron. Soc. vol. xl.

panded (the odd terms vanishing by hypothesis),

$$\int_{-\infty}^{\infty} f(z)dz\, u_{xs} + \int_{-\infty}^{\infty} f(z)\frac{z^2}{2}\, dz \frac{d_2 u_{xs}}{dx^2} + \&c.;$$

provided that we take for granted what DeMorgan has postulated (*Encycl. Metr.* § 88) and Mr. Crofton calls the "usual assumption"*, that the mean powers of the elemental errors are not infinite. In this case an approximate solution of (1) is afforded by a solution of the partial differential equation

$$\frac{du}{ds} = \frac{c^2}{4}\frac{d_2 u}{dx^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

(where $\frac{c^2}{2} = \int_{-\infty}^{\infty} z^2 f(z)dz$). For let w be a solution of (2); then the right-hand member of equation (1)

$$= \int_{-\infty}^{\infty} f(z)\, dru + \frac{c^2}{4}\frac{d_2 u}{dx^2} + \text{terms of the order of } \left(\frac{c^2}{4}\frac{d_2}{dx^2}\right)$$

raised to the second, third, &c. powers,

$$= u + \frac{c^2}{4}\frac{d_2 u}{dx^2} + \text{terms of the orders } \left(\frac{d}{ds}\right)^2, \left(\frac{d}{ds}\right)^3, \&c.,$$

$$= u + \frac{c^2}{4}\frac{d_2 u}{dx^2} + \text{terms of orders which may by hypothesis}$$

be neglected,

$$= u + \frac{du}{ds} = \text{the left-hand member of equation (1). Q. E. D.}$$

Equation (2) has two general symbolical solutions in terms respectively of $\frac{d}{ds}$ and $\frac{d}{dx}$. The former is resolvable into two series given by Poisson†, involving respectively even and odd powers of x. The even series is

$$\left[1 + \tfrac{1}{2}\frac{4}{c^2} x^2 \frac{d}{ds} + \frac{1}{\underline{|4}}\frac{16}{c^4} x^4 + \&c.\right]\phi(s).$$

Now the rough general experience, or intuition, which allows us to assume that $\frac{d_2 u}{ds^2}$ and higher differentials may be neglected for some values at least of x, allows us to assume that such values occur about the centre of the compound curve. The assumption holds accordingly for $x=0$. But when $x=0$, $u=\phi(s)$.

* Phil. Trans. 1870, p. 183.
† *Mécanique,* p. 358. *Cf.* Fourier, *Chaleur,* ch. 9.

Therefore $\frac{d^2\phi}{ds^2}$ and higher differentials may be neglected relatively to $\frac{d\phi}{ds}$. Therefore the third and following terms of the above series may be neglected relatively to the second, for values of x such that $\left(x^2 \frac{4}{c^2} \frac{d}{ds}\right)^2$ may be neglected. In that case the solution may be written

$$\left(1+\frac{2x^2}{c^2}\frac{d}{ds}\right)\phi(s)\,;$$

or, if we put $\frac{1}{\psi}$ for ϕ,

$$\frac{1}{\psi(s)}\left[1-\frac{2x^2}{c^2}\frac{\psi'(s)}{\psi(s)}\right].$$

This is approximately equal to

$$\frac{1}{\psi(s)}\,\epsilon^{-\frac{2x^2}{c^2}\frac{\psi'(s)}{\psi(s)}}.$$

And, *if* we may regard the last written expression as the appropriate form of the sought function in x, then the condition of a facility-curve (that the integral between the extreme limits equal unity) will afford a differential equation to determine $\psi(s)$. The solution is $\psi(s)=c\sqrt{\pi}\sqrt{s+\mathrm{A}}$; where A is a constant which will be found to be zero. But I submit that the condition on which the differential equation for ψ is based is not in general valid; that, for instance, in some cases it would be proper to equate the truncated series of Poisson, not, as above, to

$$\frac{1}{\psi(s)}\log_{-1}-\frac{2x^2}{c^2}\frac{\psi'(s)}{\psi(s)},$$

but to

$$\frac{1}{\psi(s)}\log_{-1}-\log_{-1}\frac{2x^2}{c^2}\frac{\psi'(s)}{\psi(s)}.$$

The inappropriateness of the former expression appears from the principle stated on page 304, which seems also to bar the parallel step* (from $1-\kappa^2x^2$ to $\epsilon^{-\kappa^2x^2}$) in the ordinary method.

The odd series is, I think, inappropriate to the case of even elements.

* Mr. Glaisher suspects the safety of this step (Monthly Notices Roy. Astron. Soc. 1873).

The other general symbolical solution of equation (2) is

$$u=\epsilon^{s\frac{c^2}{4}\frac{d_2u}{dx^2}}\phi(x).$$

Here, *if* we may choose $\phi(x)$ at pleasure, we may, by choosing it suitably, avail ourselves of the theorem given by Mr. Crofton (Phil. Trans. 1870, p. 187), in whose steps we are now almost treading. But, again, I submit that this procedure is in general illegitimate; that $\phi(x)$ is not to be chosen at pleasure, but so as to satisfy the condition

$$\epsilon^{\frac{c^2}{4}\frac{d_2u}{dx^2}}\phi(s)=f(x);$$

and that when f is hyper-exponential then the theorem of Mr. Crofton does not apply.

Subject to this limitation, the preceding proof of the law of error may be extended to the case in which the component element facility-curves are not identical (when c^2 is to be regarded as a function of s, and for sc^2 must be substituted Σc^2, approximately $\int c^2 ds$) and to unsymmetrical curves. By taking account of quantities of the order $\frac{d_2u}{ds^2}$ we may obtain a second approximation to the law of error.

There is another mode of dealing with equation (1) which has the advantage of being applicable to those cases in which the mean powers of error are not finite. It begins by expanding the right-hand member in terms of x and neglecting those above a certain order, and then proceeds somewhat as follows:—Construct the form of the sought compound, partly by observing the initial tendency exhibited by the superimposition of two or three elements, and partly upon the principle *that the compound has the same mean powers finite and infinite as the element, and a certain analogous theorem relating to mean exponentials,* such as $\int_{-\infty}^{\infty}\epsilon^{-z^2}f(z)dz$. Having assumed the form of the sought function, determine its constants by the differential equations presented by the conditions

$$\int_{-\infty}^{\infty}f(z)udz=\frac{du}{ds}+u(x=0),$$

$$\int_{-\infty}^{\infty}f(z)\frac{d_2u}{dz^2}dz=\frac{d_3u}{ds\,dz^2}+\frac{d_2u}{dz^2}(x=0),$$

$$\&c. \quad = \quad \&c.,$$

the powers of $\frac{d}{ds}$ above a certain degree being neglected.

Example 1. (Mean powers all finite.)

$$f(z)=\left(\alpha+\frac{2\beta}{c^2}z^2\right)\frac{1}{\sqrt{\pi c}}\;\epsilon^{-\frac{z^2}{c^2}}$$

(where $\alpha+\beta=1,\ \beta<\frac{1}{3}$).

Assume as the appropriate final form

$$\frac{1}{\sqrt{\pi}\sqrt{\mathrm{C}}}\rho^{-\frac{x^2}{\mathrm{C}}}.$$

Changing x to z in this expression, and prefixing $f(z)$ as a factor, and equating the integral between extreme limits to $\left(1+\frac{d}{ds}\right)\frac{1}{\sqrt{\pi}\sqrt{\mathrm{C}}}$ or $u_{s0}-\frac{1}{2}\frac{1}{\sqrt{\pi}}\frac{1}{\mathrm{C}^{\frac{3}{2}}}$, we find $\mathrm{C}=sc^2$. Which solution is such that the superimposition of the element $f(z)$ transforms u into u_{s+1}, not only as regards the absolute term of u, but also for the coefficient of $x^2\left\{\frac{d_2}{ds_2}\text{ \&c. being neglected}\right\}$.

Example 2. (Mean powers above second infinite.)

$$f(z)=\frac{2p^3}{\pi}\,\frac{1}{(z^2+p^2)^2}.$$

Assume, upon the principles above stated, as the final form,

$$\frac{2\mathrm{P}^3}{\pi}\,\frac{1}{(x^2+\mathrm{P}^2)^2},$$

and, proceeding as before, construct a differential equation for P; from which it will be found that $\mathrm{P}=2\sqrt{s+\mathrm{A}}p$—a solution true for the second power of x, the first power of $\frac{d}{ds}$. A similar result will be found when the denominator is not square; in which case pq takes the place of p^2 in the compound.

It may be observed that if the *average* curve be formed (which is done by changing x in the compound to $s\xi$ and multiplying the result by s), the absolute term of the average is approximately equal to the absolute term of the element multiplied by $\frac{1}{2}\sqrt{s}$, when the denominator is square, otherwise an additional factor appears. When the denominator is higher than a biquadratic, the elemental absolute term is still to be multiplied by $\sqrt{s}$; but for the factor $\frac{1}{2}$ must be substituted a larger fraction—on to the case when all the mean powers become finite and the fraction becomes unity.

Since now the fundamental equation (1)* is satisfied in the same sense in example 2 as in example 1, I submit, in the

* Above, page 301.

absence of evidence to the contrary, that *non-exponential* laws of error of the kind considered in example 2 do occur in *rerum naturâ*, that the "ancient solitary reign" of the exponential law of error should come to an end, and that the received formula for the probable error of the average requires modification.

This conclusion will be best confirmed by considering the objections which may be brought against it.

(1) There is, first, the direct appeal to experience—to registers of observations, such as Airy has appended to his treatise. I find that there is no difficulty in ranging under a curve of the form $\frac{\sqrt{2}}{\pi} \frac{\mathrm{P}^3}{\mathrm{P}^4+x^4}$ the observations which are registered by Airy, or those which are given under the article "Moyenne" in the 'Medical Cyclopædia.' Quetelet's statistics of conscripts may be less accommodating; but the fact that the exponential law of error is verified in one case does not prove that it is universally true.

In the sequel to this paper we shall be presented with an instance of a non-exponential form, if not in nature, at least in art—the art of measurement. The analogue in relation to the *law of error* of that incident of the *method of least squares* is the following:—Instead of the hypothesis with which we started*, of determinate elemental facility-curves, suppose a source of error consisting of a probability-curve with centre fixed but modulus varying at random between certain limits. Let there be taken an indefinite number of couples (or, *mutatis mutandis*, triplets &c.) of observations. Select all the couples which have the same difference, say b. Then the law of error for the mean of *those* couples is non-exponential. I do not know, however, that this method of selective generation has any existence outside the sphere of art; and I notice it here chiefly for the interest of the hypothesis of elements with varying modulus, which in general leads to an (elemental) facility-function, not indeed with finite mean powers, yet unfamiliar—for instance, not at first sight included in the type given by DeMorgan (*Encycl. Metrop.* § 88).

(2) An appeal may be made to a less specific experience, namely the fact† that groups of actual errors do possess finite mean powers. But here the premises relate to actual facility-curves, which have somehow, in coming into existence, lost the tails attached to them by theory. The conclusion relates to the tail of the typical ideal curve. It would be equally easy, in the case of the orthodox curve, to name caudal properties

* Above, page 301.

† *Cf.* the passage in DeMorgan's article just quoted.

(analogous to that of infinite mean powers) which, though postulated by theory, are absent in fact. For instance, the mean exponentials of the form $\int_{-\infty}^{\infty} \epsilon^{+x^4} \times \phi \, dx$, where ϕ is the law of error, ought to be infinite for the probability-curve. But in fact a finite cluster of points cannot present this infinity.

(3) The objection may be transferred from experience about (presumably) compound errors to assumption about elemental errors. It seems sufficient to reply that, as the probable error of a non-exponential facility-function may be made infinitesimal, so an element of this type may become identical with the received type to all intents and purposes, except indeed that of generation. The body of the two forms may be sensibly identical; what shape is to be (approximately) assigned to the tail is a nice question of relative infinities, concerning which, I suspect, not much is known.

(4) In fine it may be objected that the non-exponential curves are not reproductive in the same sense as the probability-curve; that the superposition of two curves of the former kind does not result in a curve of the same type. Whatever force there is in this objection is counteracted by the remark that the property in question is shared by multitudes of forms other than the simple exponential—namely, all of the type

$$f(x) = \frac{1}{\pi}\int_{-\infty}^{\infty} \epsilon^{-\alpha^t} \cos \alpha x \, . \, d\alpha \ (\alpha \text{ not changing sign}),$$

where t is any positive quantity, integer or fractional. For

$$\frac{1}{\pi}\int_{-\infty}^{\infty} f(x) \cos \alpha x \, dx = \frac{1}{\pi} \epsilon^{-\alpha^t}.$$

Whence it appears, by putting $\alpha = 0$, that $f(x)$ fulfils the condition that its integral between the extreme limits should be unity. And it also fulfils the condition that $\frac{df}{dx}$ should be always negative. Now, by the Laplace-Poisson analysis, the compound curve is found to be

$$\frac{1}{\pi}\int_{-\infty}^{\infty} \epsilon^{-s\alpha^t} \cos \alpha x \, d\alpha,$$

which, by putting $\alpha = s^{-\frac{1}{t}}\beta$, we may reduce to the parent type. The probable error of the average is that of the original curve divided by $s^{1-\frac{1}{t}}$; *not* in general $s^{\frac{1}{2}}$. Which was especially to be proved.

So far we have taken for granted that the compound curve does tend to a limiting form, that the average curve is less dispersed than the original, on the strength of an unwritten common sense, or unsymbolic reasoning, such as Mr. Venn, with his usual clearness, has expressed in the chapter on the Method of Least Squares in his 'Logic of Chance.' But in Chance, as in other provinces of speculation which have been invaded by mathematics, common sense must yield to symbol. The fact that in the case of one particular curve, $\frac{1}{\pi}\frac{c}{c^2+x^2}$, noticed by Poisson, the average is neither more nor less dispersed than, is the same as, the original, suggests that there may be forms on the other side, so to speak, of this particular form. And such, for instance, the form $\frac{1}{4(1+x)^{\frac{3}{2}}}$ (x positive both ways) is found to be; if the average of two curves be taken according to the principle of page 305 (modified for the case of odd powers of the variable). This property of divergence, as it may be termed, is even more clearly exhibited by the Laplace-Poisson analysis, if in our adumbration of that analysis (above, p. 307) we put t a positive fraction. In our second paper it will be shown that the *advantage* of taking an average depends upon the property that the integral $\int_0^x u\,dx$ is for every value of x greater in the case of the average than the original curve. This property is now seen not to be universal.

With some surprise I find that divergence is not confined to curves all of whose mean powers are infinite. Take, for example,

$$u=\frac{1}{(1+x)^n}\frac{(n-1)}{2}.$$

The absolute term of the compound of two is, by our method,

$$\int_{-\infty}^{\infty}\frac{1}{(1+z)^{2n}}\frac{(n-1)^2}{4}dz=\frac{2}{2n-1}\times\frac{(n-1)^2}{4}.$$

The absolute term of the average is double this; that is, less than $\frac{n-1}{2}$, the absolute term of the original.

The preceding propositions have corollaries relating to the hyperphysical applications of the Calculus of Probabilities.

(1) The conclusion that a considerable department of the Calculus of Probabilities, the calculation of probable errors, is less arithmetically precise than is commonly supposed, is, if correct, not unimportant; for the claims of the calculus to

be applied to the human sciences have been prejudiced by its pretensions to arithmetical precision. The mathematical forms of the science have been unnecessarily weighted with numerical content. The logic of Mr. Venn is supposed to fall with equal force on those who pretend to estimate numerically evidence and inverse probability, and those who hold that the entangled quantitative relations which are presented in this as in other departments of the human sciences are best unravelled by the aid of symbols representative, not of number indeed, yet of quantity. But if the extravagant pretensions to numerical accuracy be abandoned, the moderate claim put in for mathematical reasoning may be received with more favour. If our First Part has shown that the calculation of "probable error" is often not arithmetical, our Second Part may show that the calculation of *disadvantage* is often mathematical.

(2) The property of divergence in the average, in so far as it is connected with extent of error in the individual, may be looked for in moral measurements, which seem to admit of more nearly infinite error than physical observations. Accordingly the process of taking a mean between different judgments is not necessarily advantageous in the case of bad authorities. By multiplying authorities we may go further and fare worse; but in touching on the art of measurement we have already reached our Second Part.

King's College, London.

Postscript, *September* 27.—If the limits of the elemental errors are *finite*, the proof of the Law of Error by way of equation (2) becomes rigorous—analogous to Fourier's reasoning, *Théorie de Chaleur*, art. 377 *sqq*.

XLIII. *On* Laplace's *Theory of Capillarity. By* Lord Rayleigh, *D.C.L., F.R.S., Cavendish Professor of Physics in the University of Cambridge**.

FROM the hypothesis of forces sensible only at insensible distances Laplace†, it is well known, arrived at the conclusion that the pressure within a sphere of liquid of radius b may be expressed by

$$K+\frac{H}{b}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

H is the constant on which capillary phenomena depend, and

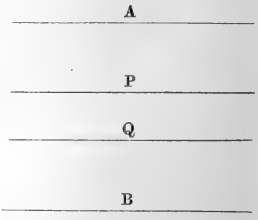
* Communicated by the Author.

† *Mécanique Céleste*, Supplement to Tenth Book.

the effect of the second term may be represented by the friction of a constant tension in the superficial layer. According to Laplace's theory, however, the first term K is enormously the greater; only, being the same at all points in the interior of the fluid, whatever may be the form of the boundary, it necessarily escapes direct observation.

When two liquids are in contact the difference of pressures within them will still be of the form (1), but the values of K and H will depend upon the properties of both kinds of matter.

The existence of an intense molecular pressure K is a necessary part of Laplace's, and probably of any similar, theory of these phenomena; but it has not met with universal acceptance*. The difficulty which has been felt appears to depend upon an omission in the theory as hitherto presented. Before we can speak of K as a molecular pressure proper to the liquid, it is necessary to show that the change, which we may denote by K_{13}, experienced in passing the surface dividing liquid I. from liquid III. is identical with the sum of the changes denoted by K_{12} and K_{23}; so that it makes no difference whether we pass from I. to III. directly or by way of II. That this should be the case upon Laplace's principles will be shown further on. The point, however, is so important that I propose to give in addition a proof of much wider generality, by which the relation is placed upon a sound basis. The existence of an intense internal pressure is probable for many reasons; and it is hoped that no further difficulty need be felt in admitting it as a legitimate hypothesis.



Let us imagine different kinds of liquids, varying continuously or discontinuously, to be arranged in plane strata, and let us examine the difference of pressure, due to the attracting forces, at two points A and B, round each of which the fluid is uniform to a distance exceeding the range of the forces. The difference of pressure in crossing any infinitely thin stratum at P is due to the forces operative between P and all the other strata. The force between one of the interior strata Q and P will depend upon the thicknesses of the strata, upon the nature and condition of the fluids composing them, and upon the distance PQ. But what-

* Quincke, Pogg. *Ann.* 1870. Also Riley, Phil. Mag. March 1883.

ever may be the law of the action in these respects, the force exerted by Q upon P must be absolutely the same as the force exerted by P upon Q. Now, as we pass downwards from A to B, every pair of elements between A and B comes into consideration twice. In passing through P we find an increase of pressure due to the action of Q upon P, but in passing through Q we have an equal diminution of pressure due to the action of P upon Q. Along the whole path from A to B the only elements which can contribute to a final difference of pressure are those which lie outside, *i. e.* in the fluid above A and below B. By hypothesis the action of the fluid above A on the strata traversed in going towards B ceases within the limits of the uniform fluid about A; and consequently the whole difference of pressure due, according to this way of treating the matter, to the fluid above A depends only upon the properties of A. In like manner the difference due to the fluid below B depends only upon the properties of B; and we conclude that the whole difference of pressure due to the action of the forces along the path AB depends upon the properties of the fluids at A and B, and not upon the manner in which the transition between the two is made. In particular the difference is the same whether we pass direct from one to the other, or through an intermediate fluid of any properties whatsoever.

It is evident that the enormous pressure which Laplace's theory indicates as prevalent in the interior of liquids cannot be submitted to any direct test. Capillary observations can neither prove nor disprove it. But it seems to have been thought that the relation

$$K_{13} = K_{12} + K_{23} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

implies a corresponding relation between the capillary constants

$$H_{13} = H_{12} + H_{23}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and the fact that (3) is inconsistent with observation is supposed to throw doubts upon (2). Indeed Mr. Riley*, in his interesting remarks upon Capillary Phenomena, goes the length of asserting that, according to Laplace, K is a function of H. It is thus important to show that Laplace's principles, even in their most restricted form, are consistent with the violation of (3).

In attempting calculations of this kind we must make some assumption as to the forces in operation when more than one kind of fluid is concerned. The simplest supposition is that

* *Loc. cit.* p. 193.

the law of force between any two elements is always the same, $\phi(r)$, as a function of the distance, and that the difference between one fluid and another shows itself only in the intensity of the action. The coefficient proper to each fluid may be called the "density," without meaning to imply that it has any relation to inertia or weight. The force between two elements (of unit volume) of fluids I. and II. may thus be denoted by $\rho_1\rho_2\,\phi(r)$; that between two elements of the same fluid by $\rho_1^2\,\phi(r)$, or $\rho_2^2\,\phi(r)$, as the case may be.

We will first examine the forces operative in a fluid whose density varies slowly, that is to say undergoes only a small change in distances of the order of the range of the forces, supposing, for simplicity, that the strata are surfaces of revolution round the axis of z. The first step will be to form an expression for the force at any point O on the axis.

The direction of this force is evidently along z, and its magnitude depends upon the variation of density in the neighbourhood of O. If the density were constant, there would be no force. We may write

$$\delta\rho=\frac{d\rho}{dz}z+\frac{d^2\rho}{dz^2}\frac{z^2}{2}+\frac{d^2\rho}{dx^2}\frac{x^2}{2}+\dots,$$

or in polar coordinates,

$$\delta\rho=\frac{d\rho}{dz}r\cos\theta+\frac{d^2\rho}{dz^2}\frac{r^2\cos^2\theta}{2}+\frac{d^2\rho}{dx^2}\frac{r^2\sin^2\theta}{2}+\text{terms in } r^3. \quad (4)$$

For the attraction of the shell of radius r and thickness dr we have

$$2\pi r^2\,dr\,\phi(r)\int_0^\pi \delta\rho\cos\theta\sin\theta\,d\theta=\frac{4\pi}{3}r^3\,\phi(r)\,dr\frac{d\rho}{dz}+\dots;$$

and for the complete attraction,

$$\frac{4\pi}{3}\frac{d\rho}{dz}\int_0^\infty r^3\,\phi(r)\,dr+\text{terms in}\int_0^\infty r^5\,\phi(r)\,dr.$$

The difference of pressure corresponding to a displacement dz is found by multiplying this by $\rho\,dz$. Thus

$$dp=\frac{2\pi}{3}\frac{d\rho^2}{dz}\int_0^\infty r^3\,\phi(r)\,dr+\dots,$$

and

$$p_1-p_2=\frac{2\pi}{3}(\rho_1^2-\rho_2^2)\int_0^\infty r^3\,\phi(r)\,dr\ +\text{terms in}\int_0^\infty r^5\phi(r)dr. \quad (5)$$

Laplace employs a function ψ, such that

$$\tfrac{1}{3}\int_0^\infty r^3\,\phi(r)\,dr=\int_0^\infty \psi(r)\,dr; \quad . \quad . \quad . \quad . \quad (6)$$

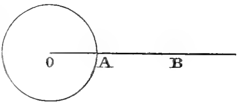
and he finds that in the case of a uniform fluid in contact with air the principal term, K, depends upon $\int \psi(r)\,dr$, and the second, H, upon $\int r\,\psi(r)\,dr$. For the continuously varying fluid here considered, we see from (5) that

$$p_1-p_2=2\pi(\rho_1^2-\rho_2^2)\int_0^\infty \psi(r)\,dr, \quad . \quad . \quad . \quad (7)$$

and that there is no term of the order of the capillary force. Equation (7) agrees with our general result that the difference of pressures required to equilibrate the forces operating between two points depends only upon the nature of the fluid at the final points; and it shows further that, under the more special suppositions upon which the present calculation proceeds, the molecular pressure at any point is to be regarded as proportional to the square of the density.

But what is more particularly to be noticed is that, in spite of the curvature of the strata, there is no variation of pressure of the nature of the capillary force; from which we may infer that the existence of a capillary force is connected with suddenness of transition from one medium to another, and that it may disappear altogether when the transition is sufficiently gradual.

For the further elucidation of this question we will now consider the problem of an abrupt transition. It does not appear that Laplace has anywhere investigated the forces operative at the common surface of two fluids of finite density, but the results given by him for a single fluid are easily extended.



Let O A (equal to a) be the radius of a spherical mass of liquid of "density" ρ_2, surrounded by an indefinite quantity of other fluid of density ρ_1, and let us consider the variation of pressure along a line from a point (say O) removed from the surface on one side to a point B also removed from the surface on the other side. The difference of pressure corresponding to each element of the path O B is found by multiplying the length of the element by the local density of the fluid and by the resultant attraction at the point.

The attraction of the whole mass of fluid may be regarded as due to an uninterrupted mass of infinite extent of density ρ_1, and to a spherical mass O A of density $(\rho_2-\rho_1)$. Since the first part can produce no effect at any part of O B, we have to deal merely with the attraction of the sphere O A.

Laplace has shown that if O A were of unit density, its action along the line O A would be

$$\mathrm{K}+\frac{\mathrm{H}}{a},$$

where

$$\mathrm{K}=2\pi\int_0^\infty \psi(r)\,dr,\quad \mathrm{H}=2\pi\int_0^\infty r\,\psi(r)\,dr; \quad . \quad . \quad (8)$$

while along A B its action would be

$$\mathrm{K}-\frac{\mathrm{H}}{a}.$$

The loss of pressure in going outwards from O to A is thus

$$(\rho_2-\rho_1)\rho_1\left(\mathrm{K}+\frac{\mathrm{H}}{a}\right);$$

and from A to B,

$$(\rho_2-\rho_1)\rho_2\left(\mathrm{K}-\frac{\mathrm{H}}{a}\right).$$

Accordingly the whole difference of pressure between O and B is

$$\mathrm{K}(\rho_2^2-\rho_1^2)+\frac{\mathrm{H}}{a}(\rho_2-\rho_1)^2. \quad . \quad . \quad . \quad . \quad (9)$$

Thus, in addition to the former result that the difference of pressure independent of curvature varies as $(\rho_2^2-\rho_1^2)$, we see that the capillary pressure, proportional to the curvature, varies as $(\rho_2-\rho_1)^2$.

The reasoning just given is in fact little more than an expansion of that of Young*. If the effect depends only upon the difference of densities, it cannot fail to be proportional to $(\rho_2-\rho_1)^2$.

Writing $\mathrm{H}_{12}=\mathrm{H}(\rho_1-\rho_2)^2$, we see that there is no reason whatever for supposing that the capillary constants of three liquids should be subject to the relation

$$\mathrm{H}_{13}=\mathrm{H}_{12}+\mathrm{H}_{23}.$$

On the contrary, the relation to be expected, if the suppositions at the basis of the present calculations agree with reality, is

$$\sqrt{\mathrm{H}_{13}}=\sqrt{\mathrm{H}_{12}}+\sqrt{\mathrm{H}_{23}}. \quad . \quad . \quad . \quad . \quad . \quad (10)$$

* *Encyc. Brit.* 1816. Young's works, vol. i. p. 463.

In (10) the three radicals are supposed to be positive, and H_{13} is the greatest.

If we suppose that the third fluid is air, and put $\rho_3=0$, we have

$$\sqrt{H_{12}}=\sqrt{H_1}-\sqrt{H_2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

in which $H_1>H_2$. From (11)

$$H_{12}=H_1-H_2-2\sqrt{H_{12}}\sqrt{H_2},$$

so that

$$H_{12}<(H_1-H_2). \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

The reason why the capillary force should disappear when the transition between two liquids is sufficiently gradual will now be evident. Suppose that the transition from 0 to ρ is made in two equal steps, the thickness of the intermediate layer of density $\frac{1}{2}\rho$ being large compared to the range of the molecular forces, but small in comparison with the radius of curvature. At each step the difference of capillary pressure is only $\frac{1}{4}$ of that due to the sudden transition from 0 to ρ, and thus altogether half the effect is lost by the interposition of the layer. If there were three equal steps, the effect would be reduced to one third, and so on. When the number of steps is infinite, the capillary pressure disappears altogether.

Although the relation (12) is given by Quincke* as the result of experiment, the numerical values found by him do not agree with (11). In most cases the tension at the common surface of two liquids exceeds that calculated from the separate tensions in contact with air. This result, which must be considered to disprove the applicability of our special hypotheses, need not much surprise us. There was really no ground for the assumption that the law of force is always the same with the exception of a constant multiplier. The action of one fluid upon another might follow an altogether different law from its action upon itself. Besides this we are not entitled to assume that a fluid retains its properties close to the surface of contact with another fluid. Even if the hypothesis, which would refer every thing to a difference of "densities," were correct, its application would be rendered uncertain by any modifications which the contiguous layers of different liquids might impose upon one another. As we have seen, if this modification were of the nature of making the transition less abrupt, the capillary forces would be thereby diminished.

September 18.

* *Loc. cit.* pp. 27, 87.

XLIV. *Notices respecting New Books.*

Formulaire pratique de l'Électricien. Par E. Hospitalier, *Ingénieur des arts et manufactures, Professeur à l'école de physique et de chimie industrielles de la ville de Paris.*

THE aim of this book, as given in the preface, is to assist amateurs and those who are engaged in electrical engineering by throwing together in a convenient form for reference the formulæ connected with its various branches, and the guiding principles underlying the various operations that an electrician may find himself called on to conduct. It is designed, in short, as a practical handbook on electrical subjects.

The first part treats of the definitions and general laws of electricity and magnetism. It may be noted that the custom of English writers is followed in naming the poles of a magnet, the north pole being the north-seeking pole of the magnet. The second part treats of units, and contains several useful tables, such as are to be found in the "Report of the Committee on Electrical Standards appointed by the British Association," and in 'Illustrations of the C.G.S. System of Units' by Professor Everett. Among the units is included the "watt," which, however, is taken to be an amount of work and not a rate of working. This is not the meaning attached to the watt by its originator. It is to be hoped no confusion will arise as to the meaning of this convenient term, for it would be a pity if, after its use had become general, it had to be abandoned to avoid ambiguity, as in the case of the "weber."

Brief descriptions of various forms of apparatus and methods employed in making electrical measurements form the third part of the book. This contains diagrams of the various circuits employed in the ordinary tests; and it may be noted that the conventional sign here used for a battery is just the reverse of that ordinarily employed in English text-books. The fourth part, comprising nearly half the book, treats of applications of electricity to useful purposes, and gives some accounts of the results already achieved. Details such as algebraical and trigonometrical formulæ, specific gravities, resistances of metals and alloys, &c. &c., which are frequently required in practical work, are presented in a tabular form, and the values of various quantities (such as the E.M.F. of cells, accumulators, and thermopiles) are duly recorded. Probably the sections dealing with generators and motors will be found of considerable interest, giving as they do in a tabular form the results claimed for various machines. The concluding pages of this part treat very briefly of telegraphy and telephony: this portion of the work might well be amplified, and illustrated with diagrams of the circuits in common use, in a future edition.

G. A. Carr, *Lieut. R.E.*

XLV. *Intelligence and Miscellaneous Articles.*

ON PROFESSOR LANGLEY'S "SELECTIVE ABSORPTION." BY C. H. KOYL*.

DURING a series of years, beginning with 1859, Prof. Tyndall carried on experiments which demonstrated the great absorptive power of water, carbonic dioxide, and the vapour of water in the infra-red region of the spectrum, and which also demonstrated the non-absorptive character of these substances in the visible parts. Other investigators have, almost without exception, arrived at the same conclusions; and it now appears beyond dispute that pure water with its vapour and pure carbonic dioxide deeply absorb the long-wave rays, but exercise little influence upon the shorter. The same series of experiments proves also that dry oxygen and dry nitrogen, either singly or mixed, are almost without effect upon any part of the spectrum.

Our atmosphere is composed principally of oxygen, nitrogen, watery vapour, and carbonic dioxide. It follows, then, that when there are not clouds or suspended haze the sky may be, as far as pure absorption is concerned, almost perfectly transparent to the visible rays of solar energy, but to the longer waves opaque, to a degree dependent upon the amount of dissolved aqueous vapour.

Until within a short time actual experiments upon the absorption of the earth's atmosphere as a whole have been of little value because of deficient apparatus; but since the invention and application to this work of the bolometer by Prof. Langley, we are able to arrive at approximate results. In his late paper he shows that the ratios of energy in different wave-lengths stopped by our atmosphere are not by any means such as we should expect from the laboratory experiments above mentioned, but that if ϵ', ϵ'', ϵ''' represent the amounts of energy transmitted, (ϵ') to our atmosphere, (ϵ'') through it on a clear day with a noon sun, and (ϵ''') with a low sun, the ratios will be about as follows for three representative wave-lengths, in the ultra-violet, in the green, and in the infra-red:—

	V.	G.	R.
λ ...	·375	·500	1·000
′ ...	353	1203	309
ϵ'' ...	112	570	235
ϵ''' ...	27	225	167

demonstrating immediately the fact that though some 54 per cent. of long-wave energy is transmitted at low sun, only about 8 per

* Abstract of remarks at a meeting of the University Scientific Association, May 2, 1883.

cent. of short-wave radiation reaches us under similar circumstances. The intermediate rays are transmitted in amounts proportional to some direct function of the wave-length. It seems almost certain, from Tyndall's experiments above quoted, that these short waves are not *absorbed* in the ordinary acceptation of the term. What then becomes of them?

In 1869 Tyndall demonstrated that if ordinary light fall upon a cloud of suspended matter in sufficiently great subdivision, the long waves pass through unhindered, while the blue are reflected; and from this he deduced an explanation of the colour of the sky. In 1880 I published a brief paper experimentally extending these results to ordinary coatings of oxides of the metals projected upon charcoal by a blowpipe-flame. From these it was shown that an oxide in thin layers might reflect only a beautiful blue; that if the coating naturally absorbed blue, it reflected in preponderance in thin layers the next longer wave-length; and that, in fact, the amount and character of reflection depended upon the size of particles and thickness of layer. No quantitative measurements were made; but from the magnificent character of the blues obtained, I concluded that nearly all the incident light of that wave-length was reflected. Applying this theory to the sky, we have an explanation not only of its blue colour and of its sunset tints, but also of Prof. Langley's results that through the lower air, full of these floating particles, the rays of shorter wave-length do not penetrate. It is not selective absorption; it is selective reflection.—*Johns Hopkins University Circular*, August 1883.

ON THE RECIPROCAL EXCITATION OF ELASTIC BODIES TUNED TO NEARLY THE SAME PITCH. BY DR. G. KREBS.

When two strings tuned perfectly alike are stretched upon the same monochord, it is well known that each is capable of causing the other to vibrate with it. The same is true of two tuning-forks of exactly the same pitch placed opposite each other on two resonators.

But if the pitch of two strings is not exactly the same, the deeper-toned one can excite the higher, but not the higher-toned one the lower, provided that the difference between the numbers of vibrations amounts to at least 2 or 3, at most 3 or 4.

Conviction of this is obtained when the strings are first brought to exactly the same pitch, and then the tension of one of them somewhat relaxed: if now one of the strings be struck or pulled in the middle with the finger, after a paper rider has been placed on the other, it can be distinctly seen that the rider leaps from the higher-toned string if the difference of the numbers of vibrations amounts to 3 or 4, while, conversely, the rider upon the deeper-toned string remains almost unmoved when the higher-toned is struck.

If the difference of the numbers of vibrations amounts not to 2, each string is in a condition to excite the other; but the deeper-toned is observed to act more powerfully on the higher than *vice versâ*.

For a counterproof, the string which just now was the lower can be stretched more strongly, so that it becomes the higher, &c.

The limits of the excitability moreover follow the length, thickness, and tension. With short, thick, strongly stretched strings the limit of excitability of the higher by the deeper-toned lies at a difference of 1 or 2 vibrations in a second.

With tuning-forks, particularly those with short thick prongs, the difference of the numbers of vibrations must not amount to 1, if the deeper-toned is to be able to excite the higher. It was, however, on tuning-forks that I first observed the phenomenon. I had two forks upon equal resonators, which were said to have exactly the same pitch. I placed them over against one another, and struck one of them, when the other did not sound with it; but when I struck the other, the first replied very audibly. Having found out which of the two forks gave the higher tone, I loaded one of its prongs with a little wax, and, by repeated alteration of the loading, easily arranged so that each fork was in a condition to excite the other with equal force. I now augmented the load of the originally higher-toned fork, and found that now this could excite the other, but not *vice versâ*.—Wiedemann's *Annalen*, xix. pp. 935, 936.

ELEVATED CORAL REEFS OF CUBA. BY W. O. CROSBY*.

Mr. Crosby describes in this paper the elevated coral reefs of Cuba, and draws from them the apparently well-sustained conclusion that they indicate a slow subsidence during their formation, and hence, further, that Darwin's theory of the origin of coral islands is the true theory. The *lowest* reef-terrace of the northern side of the island has a height of 30 feet, and varies in width from a few rods to a mile; it was once plainly the fringing reef of the shore. The *second* reef-terrace rises abruptly from the level of the lower to a height of 200 to 250 feet, and bears evidence of having been of like origin with the lower. The altitude of the *third* reef is about 500 feet; and the *fourth* has a height east of Baracoa, near the Yumuri River, "of probably not less than 800 feet." These old reef-terraces extend, "with slight interruptions, around the entire coast of Cuba; and in the western part of the island, where the erosion is less rapid than further east, they are the predominant formation, and they are well preserved on the summits of the highest hills. Mr. Alexander Agassiz states that the hills about Havanna and Matanzas, which reach a height of over 200 feet, are entirely composed of reef-limestone."

In the precipitous mountain called El Yunque (the Anvil), five miles west of Baracoa, reef-limestone, 1000 feet thick, constitutes the upper half of the mountain, the lower part, on which the reef rests, consisting of eruptive rocks and slates: and originally the upper limit of this modern limestone formation must have been

* Proc. Boston Soc. Nat. Hist.

2000 feet above the sea-level. Mr. Sawkins gives 2000 feet as the maximum thickness of the Jamaica elevated coral reefs above the sea.

Evidence that the reefs were not formed during a progressive rising of the land is drawn from the thickness of the reefs. Mr. Crosby observes that the reefs reaching to a height of 500 and 1000 feet, if not also to that of 2000 feet, show, by the remains within them, that they were made chiefly of reef-building corals, and hence that they were not begun in deep water, as is assumed in the theory of Mr. Agassiz, but that they were made in shallow water during a progressive subsidence. Mr. Crosby concludes as follows:—

"We have then apparently no recourse but to accept Darwin's theory as an adequate explanation of the elevated reefs of the Greater Antilles, and therefore to admit that the upheaval of this portion of the earth's crust has been interrupted by periods of profound subsidence during which the reefs were formed. The subsidence of 2000 feet, of which El Yunque is a monument, must have reduced the Greater Antilles to a few lines of small but high and rugged islands; and, as Mr. Bland has shown, this fully accounts for the absence in these immense tracts of all large animals, although they were abundant here in Pliocene and earlier times."

The writer adds here the following objections to the theory of the formation of coral atolls in deep waters out of the calcareous secretions of deep-water life:—(1) It is very improbable that submarine eruptions ever make the large and well-defined craters, like those of subaerial action, which are appealed to in order to explain the lagoon feature of atolls; (2) Many coral atolls are twenty miles or more in diameter, which is vastly larger than the largest of craters; (3) The atolls are never circular, and the larger have the irregularities of outline or diversities of form characterizing other large islands of the ocean; (4) In the actual reefs and islands of the Feejee group (see the map of the islands in the writer's 'Corals and Coral Islands') all the conditions, from the first stage to that of the almost completed atoll, are well illustrated, one island having only a single peak of rock within the lagoon, not $\frac{1}{100}$ of the whole area, which a little more of subsidence would put beneath the waters and leave the lagoon wholly free.—J. D. Dana in Silliman's *American Journal* for August 1883.

POSTSCRIPT TO DR. CROLL'S PAPER ON GEOLOGICAL CLIMATOLOGY.

September 22.—Since this paper was written, Baron Nordenskjöld has returned, and, as might have been expected, found Greenland to be a desert of ice, with the surface of the icy plain sloping upwards towards the interior. The results of this Expedition, however, are of the most important character, affording, as they do, a striking confirmation of the true theory of continental ice.

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XLVI. *On certain Molecular Constants.* *By* FREDERICK GUTHRIE.*

[Plates IV. & V.]

Path-density. Path-mass. Liquid Slabs. Metallic Diffusion.

§ 1. *PATH-DENSITY.*—For some years past I have been trying to make clear to myself and others the idea of path-density as distinguished from ordinary density. One is reluctant to introduce new conceptions into science unless they are called for by existing known facts, or unless they suggest the existence of unknown ones. The idea of path-density arose from the attempt to get an insight into the condition of the common surface of two media, and especially of that imperfectly understood condition of the surface of liquid masses, known as Surface-tension or Skin. The connexion between path-density and surface-tension will be shown in §§ 13, 14.

§ 2. If a given mass of matter alters its volume, it thereby alters its density. If a point or minute piece of matter which weighs a gram were to swell to the size of a cube centimeter, or if a cube yard of matter weighing a gram† were to shrink to the size of a cube centimeter, matter of unit density would

* The experimental parts of this paper were communicated to the Physical Society during the Session 1882–83.

† I use the terms "gram as weight" and "gram as mass" indiscriminately.

be obtained (water at 4° C.). Expressed generally,

$$D=\frac{M}{V}.$$

§ 3. (α) *A cube centimeter of water at 4° C. moves in a straight line at right angles to one of its faces at a uniform rate of* 1 *centimeter in* 1 *second. It traces out a path of unit density.* This conception is perhaps put into a more useful form: (β) *A square centimeter of surface having the mass of* 1 *gram moves at the rate of* 1 *centimeter a second; its path is of unit density.* The thickness of the surface is here nothing, and its density is infinite. More generally: (γ) *Unit path-density is made when a plane surface of area* $\frac{1}{n}$ *centimeter, weighing* $\frac{1}{n}$ *gram, moves with uniform velocity.*

Perhaps this is clearer if we imagine an infinite series of square centimeter-gram-surfaces following one another at equal intervals of 1 centimeter apart, and moving at any velocity along a straight path at right angles to them. Then any cubic centimeter of such an infinite path will weigh 1 gram.

Returning to concrete examples:—

If Pd denote path-density,
 δ denote density (specific gravity),
 l denote the thickness in direction of motion (centimeter),
 r denote rate (centimeter-second);

then

$$Pd=\frac{l\,.\,\delta}{r}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

§ 4. The few following obvious deductions may serve as illustrations. $Pd=\delta$ when $\frac{l}{r}=1$; that is, when the space passed over by any point of the moving mass in a unit of time is the same as the length of the matter which passes through that point when the point is at rest. In other words, there must be no gaps. Moving continuous matter has a path of the same density as the matter itself.

§ 5. If the mass be at rest, the expression (1) becomes $Pd=\frac{0\times\delta}{0}$, which, though of ambiguous form, here means that $Pd=\delta$. The two 0's are equal, having been derived from rational diminution of l and r. The same is true if r and l are both infinite.

It is clear, but must be especially noted, *That the* ratio *between the path-densities* Pd_{a_1} *and* Pd_{a_2} *is the same whether those paths are generated by the single transit (in an eternity) of the masses* A_1 *and* A_2 *moving at the rates* r_1 *and* r_2 *respectively:*

or by an endless succession of masses A_1 *moving at the rate* r_1 *at a distance* d *apart, and an endless succession of masses* A_2 *moving at the rate* r_2 *at the same distance* d *apart: or by the same masses* A_1 *and* A_2 *returning at equal intervals with the velocities* r_1 *and* r_2.

These statements may be illustrated as follows:—Let there be (Plate IV. fig. 1) an annular tube b of 12 cub. centim. capacity, and let there be 1 cub. centim. of water at a. At whatever rate a may pass round b, the density of the path is the same, namely $\frac{1}{12}$. If it move round at the rate of once in a second, let its path-density be Pd. If, now, it moves round n times in a second, then in expression (1) r becomes nr, and l becomes nl; so that Pd remains unchanged.

Again, if the tube b be doubled in length, the path-density will be halved at whatever rate a may move.

§ 6. When an irregularly* shaped mass of uniform density moves with uniform velocity in a straight line, the path is of uniform density longitudinally but varies in density transversely (like a sword-stick).

Taking the case of a triangle moving on its base (fig. 2, Pl. IV.), we have at once

$$\frac{\mathrm{P}d_a}{\mathrm{P}d_b}=\frac{a}{b}.$$

The distances a and b are here indeed nothing more than the expression l in § 3, equation (1).

§ 7. If l be constant, that is if the moving matter have equal thickness all over in the direction of its motion, variation in density at different parts will of course produce corresponding and proportional variation in the path-density.

It follows that a heavy sector of a circle of unit density revolving about the centre of the circle will give rise to a path which is of uniform density, and whose density is $\frac{\theta}{360^\circ}$, if θ be the angle of the sector. The shadow of such a disk is uniformly dense or the moving disk is equally transparent to light.

If a heavy line revolve about one of its ends, the density of the path, which is a circular surface, varies inversely with the distance from the centre. The shadow of such a revolving line (or narrow strip) varies in density according to the same rule.

§ 8. The path-density of a heavy plane moving parallel to itself, as in § 3 (β), but inclined to its path at an angle θ, is $\frac{\mathrm{P}d}{\sin\theta}$, if P$d$ be, as before, the path-density, when the plane is at

* Any shape excepting a prism having parallel back and front faces.

right angles to the path. The sectional area of the path is of course proportionally less, being sin θ.

§ 9. *Path-mass.*—For unit of path-mass it will be convenient to take the same conditions of motion as before (§ 3), namely, a square gram-centimeter moving at the rate of 1 centimeter in 1 second at right angles to itself.

If two such plates follow one another in a second, the path-density and the path-mass are both doubled. If they are placed edge to edge in one plane and move with the unit velocity, their path-mass will be doubled. For the path has the unit density but double the volume of that generated by the single square. Just as in ordinary mass we have

$$M=\delta V$$

(where M = mass, δ = density, and V = volume), so here we have

$$P_m = VP_d.$$

Of course, if the path be maintained at uniform density by the passage of successive surfaces, the total mass will be infinite. But the mass per unit length is proportional to the sectional area of the path at right angles to it. And this is the case when a finite path is maintained at constant density by the to-and-fro motion of the surface or by its orbital return, as in § 5.

§ 10. However a mass of matter may rotate, however irregular it may be in shape or density, however it may change its shape or density, and whatever may be its velocity or change of velocity, its total path-mass is constant for the same time-interval provided only its mass remains the same.

§ 11. If the earth's orbit be 300,000,000 kilometers in diameter (a little more than 186,000,000 miles) and its diameter be 8000 miles, the height of a cylinder having the same volume as the earth and the same diameter is 5333 miles, or 8580·797 kilometers. It therefore appears that the mean orbit-density of the earth is 7·761 times the specific gravity of the earth, or, say, about 39 or 40.

§ 12. If a single moving atom were enclosed in a box of fixed internal dimensions, the mean density of the gas constructed by it would be constant, however the atom might move. But if the atom move to and fro between opposite walls with acceleration, say harmonically, its path will be denser at its extremities and densest at the walls according to the law of sines, or as though the material area between a semicircle and its tangent had been compressed upon the tangent (fig. 3).

§ 13. The increased path-density caused by the retardation of solid matter towards the end of its harmonic excursions is visible with a vibrating rod whose excursions are many times longer than its thickness. Better with a long monochord wire. The shadow of such a wire shows most distinctly the increased path-density at its edges when it vibrates in a plane at right angles to the light. If such a cord be made to vibrate in a circular path, or if an upright rod be fastened excentrically to the top of a humming-top, the shadow is also graduated, being densest at the edges. Here we have really of course the projection of a circular path of uniform density on a tangent to the circle, precisely as in § 12. The vibrating solids have skins.

§ 14. The hypothesis which I submit, and which is quite independent of the facts of the preceding paragraphs, is briefly that, when a mass of liquid has a free surface, while the internal particles have paths more or less free, those that strike the bounding surface never have free paths unless they strike it at such angles as enable them to escape as vapour. They are momentarily at rest; and however small and elastic (in regard to the surface) they may be, they form a dynamic (ever renewed) skin, which in its turn acts as a check upon the passing particles, delays them, and so thickens their crowd.

There is accordingly at all such surfaces an increased density due to diminished mean velocity; and it is this increased density which forms the so-called surface-tension or skin. Gases and vapours should have such skins at the bounding surface between themselves and liquids and solids; and perhaps it is for this reason that a solid has the power of what is virtually condensing a gas, even sometimes to liquefaction, upon its surface.

§ 15. *Liquid Slabs.*—Whether or not the hypothesis in § 14 as to the cause of surface-tension be correct, and whether or not the terms surface-tension or skin be either of them satisfactory, it is convenient to adopt some such expression for the apparent toughness of the surfaces of liquids. When a little liquid is poured upon a flat horizontal surface which is not attacked by the liquid, a circular disk of liquid is formed, the shape of the edge of which has been very fully examined by Quincke and others. In most such cases, one of the most important factors is the specific relationship in the sense of adhesion between the solid and the liquid. In fact the question, like all questions of capillarity, involves density (and gravitation), cohesion, adhesion, and surface-tension. Such experiments show the relationship between two bodies as well as the physical attributes of one. About twenty years ago I made an attempt to get rid of the factor adhesion, with partial success, by examining the size of a liquid drop. But I soon found that other factors, notably the shape of the solid bodies from which the dropping occurred, and the rate of dropping, introduced arbitrary conditions which removed the measurements from the class of simple physical constants.

§ 16. The plan adopted in the following experiments is the endeavour to support a mass of liquid above a plane surface in such a way that no actual contact ensues, not even such as takes place between clean glass and mercury. If such can be done, it is clear that we shall have a circular flat slab with rounded edges, and into the shape of that slab the influence of adhesion by no means enters. If the thickness of the slab be found to be a constant, we shall have a constant as characteristic as density, and, like density, varying for the same mass only according to volume, such volume-change in our case being brought about by heat alone. Such slab-thickness has for its negative influence the action of gravity (density), for its positive the cohesion and surface-tension.

§ 17. The actual measurements of the slab-thickness I have performed in two ways:—(1) by a spherimeter which, when used as such, gives results trustworthy to the $\frac{2}{10000}$ of an inch. But the upper of the two surfaces whose distance has to be measured being liquid, and the lower one not very hard, the spherimeter cannot be used by the method of touch. Accordingly I have measured the slab-thickness indirectly. A known volume of the liquid is poured on the surface, and teased into the circular form if it shows any noticeable departures from it. Four or five diameters are measured by means of a small horizontal cathetometer. The mean being taken, an allowance has to be made for the meniscus. This reduces the shape to the cylindrical, from which the thickness h is deduced by means of the equation

$$h = \frac{V}{\pi r^2}.$$

§ 18. In regard to the actual apparatus:—Upon a thick round slab of paraffin, a foot in diameter and 4 inches thick, a massive foot of plaster is cast. The whole is placed on a three-screw levelling support. The surface of the paraffin is scraped into a true plane. When water was being examined, the surface of the paraffin was lightly powdered with lycopodium and the water poured on vertically from a fine opening. With some care a perfectly round slab of water 6 inches in diameter can be formed, which is so free to move that the greatest nicety of adjustment in the levelling-screws is necessary. Precisely the same arrangement can be adopted for mercury. But it was found that for the latter liquid a sheet of blotting-paper wetted and allowed to dry on a sheet of plate-glass gave results identical with those of the paraffin surface. The paper surface was used in some of the experiments. As to the allowance for the meniscus, it is clear that this is of less consequence with large slabs than with small ones. Indeed, with slabs a few inches in diameter the meniscus might be neglected. This was imperfectly shown in the case of mercury by adding exactly equal volumes to a small slab. After the slab had passed 2 inches in diameter, each additional volume produced a "parabolic" increase in the diameter. Data derived from this and from the measurement of an enlarged photograph of the edge gave me as a mean 2 millim. to be deducted from the diameter in the case of mercury. Assuming it to be the same for water, the error incurred, after making this reduction, could not in a 6-inch slab be more than $\frac{1}{500}$ of the diameter. This would be negligible in the deduced thickness.

§ 19. I give the following datum for mercury on account of the accidental coincidence of the experimental numbers with numbers easy of remembrance, excepting as to the temperature, which is, however, not far from the conventional temperature of 60° F.

100 cub. centim. of mercury at 14° C. has an extreme radius of 100 millim.

$$h=\frac{100{,}000}{3{\cdot}1416\times 99^2};$$

thickness of mercury slab $=3{\cdot}248$ millim.

In the case of water it was found so difficult to get a nearly circular slab with 100 cub. centim., that only 50 were employed. The slab may then be teased into a circular form by means of a stick of paraffin covered with lycopodium.

50 cub. centim. of water at 14° C. has an extreme radius of 54·8 millim.

$$h=\frac{50{,}000}{3{\cdot}1416\times (53{\cdot}8)^2};$$

thickness $=5{\cdot}50$ millim.

Glycerine* is a beautiful liquid in this respect. It is kept off from the paraffin surface by a very faint blush of lycopodium, and it travels very slowly. It can be got into a circular slab more easily than water; but, perhaps on account of its capillary action towards its lycopodium props, it is more persistent in its motion. In fact, unless there be hills of that sub-

* Commercial, "Price's."

stance to confront it, it rolls along (for that is the motion of a slab however large) and forms a "level," which requires a very steady support to avoid the notion that its motion is affected by the gravity of the observer.

50 cub. centim. of glycerine at 14° C. has an extreme radius of 59 millim.

$$h=\frac{50,000}{3\cdot1416\times(58)^2};$$

thickness $=4\cdot731$ millim.

§ 20. Accordingly, taking the slab thickness of water as unity, we may begin a table which will at some future time assuredly be extended.

Specific Slab-thickness (at 14° C.).

Water .	=1·0000,
Glycerine	=0·8602,
Mercury	=0·5906.

These numbers may be, with instruction, considered in reference to the numbers in table vii. which concern the drop sizes of the same three liquids in the 'Proceedings of the Royal Society,' 1864, p. 17 [" Recess "]. It will, I have no doubt, appear that in all cases the greater the drop-size the greater the slab-thickness. Water will, no doubt, again assert its singularity and exhibit the greatest slab-thickness.

§ 21. Restrained as slabs are in their form by skin-tension as well as cohesion, it is found that the addition of a liquid which diminishes the former diminishes also the slab-thickness. Taking 25 cub. centim. of water at 14° C., a slab was formed having 38 millim. corrected radius. This gives a thickness of 5·51 millim. Such a slab is unchanged if touched in the middle by a drop of glycerine. But on touching it with " glacial " acetic acid, it instantly acquires a corrected radius of 44 millim., or thickness of 4·16 millim. This means a diminution in thickness of very nearly 25 per cent., or one quarter. The question therefore presented itself, What is the slab-thickness of " glacial " acetic acid ?

I reserve the results of my experiments in the direction of the relationship between the liquids and the alteration of skin-tension.

§ 22. The mercury slab, like the water slab, has what virtually amounts to a skin; and it became interesting to examine the conditions of this skin or region of surface-tension. If lycopodium be strewn upon the surface of a mercury slab, and a little tin, zinc, or lead, or amalgam of these metals, be made to touch the slab in the middle, no noticeable disturbance takes place. But if such a slab be touched by an amalgam of K or Na, the slab instantly expands, and the film of lycopodium-powder on its surface cracks radially, exposing the brilliant metallic surface, which is seen to be agitated over its whole extent. In a few seconds the slab contracts to its original size and the lycopodium cracks heal.

Does this extension of the slab depend upon the diminution of the cohesion of the mass of the mercury, or upon a surface effect?

§ 23. I frequently in my researches have had recourse to the fact, which I first described in the year 1863, that a little sodium added to mercury enables that metal to touch with positive capillarity metals which in its and their ordinary state are not wetted by the liquid metal. I here make use of the same fact. A platinum tube, 6 millim. in internal diameter and 2 centim. in height, is rubbed and soaked in some weak sodium amalgam, and then washed in several quantities of pure mercury. Placing such a tube vertically in the middle of a slab of mercury so that its lower edge is clear of the surface upon which the mercury slab rests, we have the condition shown in fig. 4. A little grain of sodium amalgam dropped into the platinum tube causes no immediate change; but in a time measurable by seconds, say 20 to 30 seconds, the slab starts on its expansion and reaches its maximum size, apparently immediately. It seems, then, that since the effect is not instantaneous, it is a surface effect. The effect when produced is due to an alteration of the surface between the tube and the outer portion of the slab. By dipping the platinum tube further down into the slab so as to be within $\frac{1}{50}$ of an inch of the bottom, I have found the effect to be distinctly delayed.

§ 24. The fact mentioned in § 23, that the release of the mercury skin-tension by sodium is brought about after a time, short indeed, but appreciable when introduced into the central part of a liquid slab inside the platinum tube, points to the existence of a true diffusion between the metals; and this leads to the third part of this communication. For I have examined already a few such cases, which I will now describe, because I believe the subject of elementary diffusion has been neglected excepting in the case of gases, and even here but little is really known.

§ 25. *Metallic Diffusion.*—The metals potassium and sodium suggested themselves of course at once. They offer exceptional facilities for the determination of the composition of the mixture, when they have diffused through mercury, because the mere addition of water translates the alkaline metal into hydrogen. The neutralization of the alkalized water, say, by hydrochloric acid, and subsequent evaporization and weighing, give a control upon the hydrogen translation of the alkaline metal. The mercury is thereupon left nearly ready for weighing.

On the other hand, I have not yet been able to establish a column of mercury having an unlimited stock of pure cold alkaline metal above pure mercury at the same temperature below. I do not see the possibility of it. Granted that when such metals as tin, or lead, or gold, or silver dissolve in mercury heat may move, such movement of heat is, I should think, swamped in its power of causing convection-currents by the conductivity of the mass. But in the case of the alkaline metals the first contact of the two metals is accompanied by so much heat that the conditions obtainable with other metals are here far more difficult. Perhaps mercury and sodium brought into contact at a temperature far below the freezing-point of mercury might give the required starting-point. If their contact were real and the elevation of temperature very gradual and well controlled, we might have a trustworthy condition; but scarcely at a single temperature.

Such a condition would represent a certain fixed sodium potential (not infinite, because the sodium has to be disintegrated), on the one hand, and a lower, but not zero, on the other; and between the two the integral of the resistances of the various amalgams after the first contact.

§ 26. This being so, I elected to employ sodium amalgam and potassium amalgam rather than the free metals.

On mixing sodium with mercury, the two combine with great energy and liberate so much heat as to point to a loss of volume. Is this loss of volume, if it take place under any circumstance, so great as to give rise to an amalgam having a greater density than mercury itself?

If $\frac{m_1}{v_1}$ be the density of mercury and $\frac{m_2}{v_2}$ that of sodium, and if v_3 be the volume of the amalgam, then the density of the amalgam would be equal to that of the mercury, if

$$\frac{m_1+m_2}{v_3}=\frac{m_1}{v_1},$$

or

$$v_3=v_1\left(1+\frac{m_2}{m_1}\right).$$

If v_3 should be less than this for any ratio between the constituents, the convection-currents of sodium would at all events begin to flow down if such an amalgam were at the top of the mercurial column.

On this point, without making a study of the specific gravity of alloys of sodium of different strengths, I have satisfied myself that, as long as the amalgam is liquid, it is lighter than mercury. This is easily shown by introducing mercury into one limb and the various liquid amalgams of sodium into the other limb of a long U-tube: whereupon the pure mercury always prevails in weight. Now when a solid amalgam of sodium is brought into contact with mercury, heat may be either set free or absorbed. Chemists will understand me if I remind them that a pounds of water mixed with b pounds of chloride of calcium will give a body which will set free or absorb heat according as a is greater or less than x.

§ 27. Accordingly I made a pound or two of a sodium amalgam of such a strength as to be solid at the atmospheric temperature. This was beaten up in an iron mortar as it cooled. Putting some of this into a porcelain crucible, plunging it into water containing a few drops of hydrochloric acid, and collecting the hydrogen, it was found that after a day or two, if the amalgam was occasionally stirred, all evolution of hydrogen ceased; the volumes, reduced to dry hydrogen at 0° C. and 760 millim., were $\begin{Bmatrix} 156 \\ 128 \end{Bmatrix}$ cub. centim. The mercury, after drying, was found to weigh $\begin{Bmatrix} 15·1096 \\ 13·7841 \end{Bmatrix}$. This gives the percentage of the amalgam which I shall call Am amalgam:—

Hg	98·2	97·97	98·08	
Na	1·8	2·03	1·92	
	100·0	100·00	100·00	(mean)

The ideal amalgam would perhaps be one of such a composition that heat would neither be set free nor absorbed on further mixing with mercury. But such an ideal condition could only be ideal in its beginning, and, I think, disturbances due to this cause are insensible in comparison with other sources of error. The above amalgam when stirred with mercury may reduce its temperature as much as 5° C.

I am informed that sodium may contain a large quantity of hydrogen. I am not called on to discuss the experiments (not my own) upon which this rests; but I think that any considerable quantity would be expelled on amalgamation. Perhaps the glow or blush to be described immediately and in § 28 is due to the escape of residual hydrogen at the released tension-surface of the mercury.

The first experiment in regard to the diffusion of sodium out of this amalgam into mercury was of course a qualitative one. A U-tube (fig. 5, Pl. V.) was made of glass tube of $\frac{1}{2}$ inch internal diameter—the one limb, A, being about 3 inches and the other, B, about $2\frac{1}{2}$ inches long, reckoned from the inner bend a. This was fastened into a mass of fusible metal foot to give stability. The U-tube was dried perfectly under the ordinary air-condition, and received pure dry mercury, which stood in both limbs at a height of about $2\frac{2}{5}$ inches (reckoned from a). The whole was placed in a flat-bottomed vessel g containing a little melted paraffin, and then upon an immovable slab, to which it was stuck by a few drops of paraffin. The vessel g then received water slightly acidulated with HCl so as to cover the mercury in the shorter limb, and reach about $\frac{1}{4}$ inch above the edge of the glass tube on that side. A test-tube filled with similarly acidulated water was inverted over the shorter limb. Upon the surface of the mercury in A about 15 grams of the amalgam Am was placed; this was covered with petroleum, and the tube was plugged with cotton-wool.

Immediately after introducing the sodium amalgam a kind of frosted appearance is seen on the immediately lower parts of the mercury and glass surface in A. This appearance, which is a blush of bubbles, creeps downwards with strange rapidity, reaching the bend, say $2\frac{4}{10}$ inches, in a quarter of an hour.

In about 30 hours, bubbles of hydrogen appear at the surface of the mercury in B and collect in the pneumatic tube. Such evolution continues sensible for about a month. After two months such evolution ceased, the contents were emptied out, thereby being of course mixed, and no further evolution of hydrogen could be detected.

Such a method of experimentation is, however, far from quantitative, because, when the sodium has diffused down through A as far as a, it will, being lighter than mercury, rise through B and cause whirls.

The ideal condition of such diffusion would be of course similar to the ideal condition of heat- or electrical transference, where one may have a given potential at one end of the column, and a given lower one, fancifully called zero, at the other.

Perhaps this condition is to be attained with the greatest practical completeness by the simple *long* vertical column.

§ 28. Three glass burettes were made, about a foot in length and an inch in internal diameter. They were drawn out sharply at the bottom into capillary tubes, upon which pressure-taps were fixed in the ordinary way. These were nearly filled with pure mercury. A little of the mercury was allowed to run through so as to fill the capillary and caoutchouc tube.

Upon a tube so prepared and filled, about 15 grams of the amalgam Am were placed. The amalgam was thereupon covered liberally with petroleum; and the top of the tube was slightly corked. Instantly clouds of minute bubbles began to make their appearance between the mercury and the glass. In half an hour the whole column appeared frosted (see § 27). On drawing off a measure, say $\frac{1}{13}$ of the whole, from the bottom after two or three hours, no appreciable amount of hydrogen was to be got from it.

Accordingly the tube was reemptied, cleaned, dried, and refilled. The amalgam (A m) was then allowed to rest upon the top for 14 days and nights in an undisturbed and steady place, where the temperature ranged from 13° to 18° C. At the end of this time the amalgam was drawn off. The drawing off was effected as follows:—A little block of paraffin was hollowed so as to have a smooth cavity of the capacity of about $\frac{1}{13}$ of the tube in fig. 6. The edge was ground flat, and a flat slab of paraffin served as a cover. The amalgam was drawn into this very slowly so as to stand above the edge; the slab being then pressed down, a unit volume was entrapped. This being transferred to a porcelain capsule, the few drops of overflow were returned to the unit measure, which was again filled up, and so on. The six lowest measures (each about $\frac{1}{13}$) did not show a trace of hydrogen. The seven higher ones evolved hydrogen in the quantities shown in the following table, in which the actual weights of the mercury are reduced to 100, the cub. centim. of hydrogen being recalculated and reduced to dry hydrogen at 0° C. and 760 millim. It appears that in 14 days the sodium had penetrated down a little more than halfway, say 7 inches, in quantity appreciable.

I put now these results in such a form that they may be as far as possible immediately comparable with the results obtained by other metals. They come out as follows:—

Per cent. Na.	Hg.		Na.
·0035...........	100	and	·0035
·0178...........	,,	,,	·0178
·0665...........	,,	,,	·0666
·1769...........	,,	,,	·1772
·2034...........	,,	,,	·2038
·2295...........	,,	,,	·230
·2414...........	,,	,,	·242

§ 29. A potassium amalgam prepared in a similar manner was found, when analyzed as in § 27, to have the composition 1·34 per cent. of K. About the same quantity of this was put into the same tube as had been used for the Na, under, as far as possible, the same conditions.

Reducing the evolved H to 0° and 760, as before, it was found that the 13 volumes of the column (all of which were nearly equal except the last, which, instead of about 84–82 grams of mercury, only held about 52, for this was the drainage from the amalgam), had the composition:—

Per cent. of K.
0·00082
·0038
·0146
·0331
·1185
·2061
·2811
·3490

As to the comparison between Na and K, we need only contemplate the potential difference between 1·92 and 1·34 respectively.

With regard to the frosted appearance mentioned in §§ 27, 28, it can scarcely be doubted that the minute bubbles which compose it are hydrogen, due to the film of water or vapour on the glass. But while this appearance travels at the rate of at least one foot an hour, there is no sensible quantity of Na to be found at even a lesser depth after fourteen days. The effect must therefore be a surface-effect, and be of the same order as the effect described in § 22, where the mercury-slab expands when touched by sodium amalgam, on account of the metals spreading almost instantaneously over its surface and enfeebling its skin. The condition actually set up in the mercury column is probably this:—A minute film of sodium spreads downwards between the mercury and the glass: this decomposes the water on the glass, and so clothes the glass with a film of minute hydrogen bubbles, and the mercury surface with a film of caustic soda, which latter is in absolute contact with the mercury surface. It is a question whether the sodium film is less than, equal to, or more than sufficient to decompose the water—probably more. At all events it is so minute as not to exhibit itself in any chemical reaction. The spectroscopic reaction here has no significance.

The curves Na and K, Plate IV., which represent these experiments graphically, are not directly comparable with the curves Sn, Pb, and Zn (§ 31) in the same plate, because in the case of Na and K, for reasons given in § 25, it was found necessary to start with an amalgam, and indeed with one containing only about 2 per cent. of sodium. The time in the case of the Na and K amalgams was also only a little less than half that occupied in the diffusion of Zn, Pb, and Sn.

§ 30. The rapid penetration of zinc by mercury suggested the question whether, when an amalgam of an alkaline metal was presented to zinc, the mercury would penetrate the zinc and carry the alkaline metal with it. Accordingly the above potassium amalgam was introduced into a hollow cylinder of cast zinc, 17 millim. internal, and 21 millim. external diameter (thickness 2 millim.), 45 millim. external height, 35 millim. internal height (10 millim. thickness of bottom). The amalgam was scraped upon the zinc so as to ensure contact, and then covered with petroleum. The zinc cylinder was thereupon corked up and covered with paraffin. It was placed in a beaker of distilled water and covered with a tube of water according to fig. 7. After two months' standing at a uniform temperature of about 15°, scarcely a pin's-head volume of gas had collected in the top of the tube. Abundance of the semiflocculent fine oxyhydro-carbonate had collected on the zinc and at the bottom of the beaker. That part on the zinc was rubbed off the zinc with an ivory blade, and, together with the sediment in the beaker, dissolved in hydrochloric acid overneutralized with ammonia and sulphide of ammonium. After separation of the Zn, no trace of K could be found. No potassium had found its way through the zinc. Perhaps a more remarkable fact still is this, that on scraping about a gram of the solid metal from the outside of the zinc cylinder, not a trace of mercury could be found in it. Not only, therefore, did the alkaline metal fail to follow the mercury into the zinc, but it prevented the mercury from entering the zinc. Compare this with § 32, where the cylinder of zinc is literally "slaked" by the mercury.

§ 31. Cylinders of zinc, lead, and tin were cast, an inch and a quarter long and $\frac{7}{8}$ inch in diameter. These were floated on the mercury contained in the tubes described in § 28. The quantity of the mercury in each tube was such that it stood at the same height, reckoning from the bottom of each cylinder. The burettes had been previously lashed to massive stands, cork buffers being interposed between the tubes and the stands. The three were placed side by side on a slab let into the wall, and were protected as much as possible by cloths from sudden changes of temperature. The mean temperature was 15° C. The experiments lasted a month, and the extreme range of temperature was from 13° C. to 17°·5 C.

At the end of the month (31 days) the mercury was run off from the bottom very slowly and discontinuously into the paraffin vessel described in § 28; so that, with the exception of the top quantities, the volumes of the successive portions were very nearly the same. With regard to the top quantities, it is clear that, since the metals float at different depths in the mercury, the surfaces of contact are not the same in the several cases; and therefore these top, or richest, amalgams can scarcely be compared. Again, the shape of the bottom of the tube with its capillary &c. puts the lowest or poorest out of court. But as the contents of the lower, irregular part of the tube is not more than a third of the volume of the unit measure, it is only the very lowest amalgam that need be rejected.

In each case there were twelve full unit vessels drawn off, and in each case a fraction of a thirteenth, which last contained the drainage from the metal.

Through the kindness of Dr. Hodgkinson a number of these amalgams were analyzed in the chemical laboratory by Messrs. Adie, Gahan, and Grange, to whom I am therefore indebted. These three gentlemen analyzed the zinc, lead, and tin amalgams respectively. The metals were determined in the following manners:—

Lead.—The amalgam dissolved in nitric acid and evaporated with sulphuric acid, and the residue either ignited directly or after washing with dilute alcohol (as sulphate of lead).

Tin.—The amalgam dissolved in nitric acid, evaporated to dryness, and ignited (as metastannic acid).

Zinc.—(α) By dissolving in nitric acid, evaporating to dryness, and igniting; or (β) by separating the mercury as sulphide and the zinc as sulphide, and igniting (both as oxide of zinc).

§ 32. In Table I. the results of such determinations are given, so that the proportion of the errors of analysis may be compared with the true diffusion in each, and the difference of diffusion in the three cases.

At the end of the experiment the cylinders of tin and lead presented nothing remarkable in appearance. On standing a couple of months the upper part of the lead cylinder has become as hard as zinc, though there is no sensible deformation. The zinc cylinder swelled considerably in the tube; and when left to itself afterwards, though drained from the mercury, it continued to swell and crack, and ultimately fell to pieces like a "lime-light" lime cylinder when slaked. Two cones with their apices towards the centre of the cylinder were formed at top and bottom; the cracking otherwise was for the most part in radial planes.

In Plate V. the percentages of metal in the several amalgams of the three metals are given graphically, and without rounding off or other interpolation. The abscissæ reckoned from the left are distances from the bottom; the ordinates are the corresponding percentages of the respective metals.

TABLE I.

	Zinc. Per cent.		Lead. Per cent.		Tin. Per cent.	
1 (bottom).......	0·064, 0·062	0·063	0·188, 0·204	0·196	0·13, 0·21, 0·174	0·171
2.	0·076, 0·080	0·078				
3.	....		0·243, 0·225	0·234	0·28, 0·30	0·29
4.	0·126, 0·117	0·122				
5.	....		0·303, 0·274	0·289	0·38, 0·41	0·40
6.	0·214, 0·183	0·199				
7.	....		0·402, 0·403	0·403	0·63, 0·62	0·63
8.	0·254, 0·266	0·260				
9.	0·337, 0·338, 0·325	0·333	0·609, 0·646	0·628	0·99, 1·01	1·00
10.	0·408, 0·365	0·387				
11.	0·468, 0·454	0·461	1·03, 0·94	0·99	1·68, 1·41, 1·25	1·45
12.	0·573, 0·569	0·571				
13 (top).	0·618, 0·735	0·677	1·38, 1·47	1·43	1·86, 1·87, 1·76	1·83

§ 33. It appears accordingly that the three metals lead, tin, and zinc, all of which and all of whose amalgams are lighter than mercury, diffuse downwards through this latter metal in such a fashion that they appear, after a month's interval, in appreciable quantity at a depth of a foot beneath the surface when the temperature is about 16°–17° C. With regard to this latter point, as to temperature, I suppose that mercury is so good a conductor of heat that the influence of convection-currents is at least as inconsiderable as in the experiments which have been performed for determining the diffusion of soluble salts in water. It is scarcely worth while amusing oneself by dividing these diffusion *percentages* by the so-called "atomic weights" of the metals. A more serious consideration might be the result of the division of the diffused weight by the specific gravity of the metal. Comparing the numbers of the group Sn, Pb, and Zn with one another, we may remember that the metals are all cast, and therefore so far indefinite in structure. This may be especially the case with zinc, which cracks and thereby allows the mercury to rise by capillarity and so enrich itself, and generally set up conditions of amalgamation which I do not care to trace, for I do not see my way through.

As to the comparison of the alkaline group with the Sn, Pb, Zn groups, such comparison must be vague, for the reason that the K and Na are employed as amalgams (as though one would study the diffusion of nitre into water by employing a solution of nitre containing only 2 per cent. of the anhydrous salt); whereas the Sn, Pb, and Zn are use with what was supposed to be a sufficient supply of pure 100-per-cent. metal. But this imperfection of these conditions is manifest if we remember that while the solid metal melts and dissolves downwards, the liquid mercury rises. Accordingly there is, after the first instant of contact, supposing the metals diffuse, no constant metallo-motive force in the same place.

§ 34. I conclude therefore that the general curve of amalgamation, and therefore of alloyage, and therefore perhaps of elementary atomic and molecular diffusion generally, is of the kind shown in fig. 8. In the case of Na almost the complete curve was obtained; whereas in the case of Zn the point of contriflexure had not been reached. The very fact that the K and Na curves are more complete, in this fashion, than the Pb, Zn, and Sn, prove to my mind that K and Na have a far greater diffusive energy than the heavier metals examined. And although in this case the percentage of metal actually found at a given depth was in all cases much less than the percentages of the heavier metals, it will be borne in mind that, while the latter were fewer and had acted for thirty-one days, the former were amalgams containing less than 2 per cent. of the metal. Comparing K with Na, I do not think we can draw any conclusion beyond the rather negative one, that the superior diffusive faculty which seems to be the property of K salts in regard to water does not evidence itself, if it exist, when that metal and sodium are compared in respect to their diffusion in mercury. I am far from asserting that such preeminence may not exist; but I do not think that it is here made conspicuous.

My friend Prof. Chandler Roberts has for a long time been engaged in studying the diffusion of melted metals, and the matter has been a subject of frequent conversation between us. I await with great interest the details of his experiments. The relative dates of our publication have no relation to the dates of our experiments.

XLVII. *On* Laplace's *Theory of Capillarity.* *By* A. M. Worthington, *M.A.*

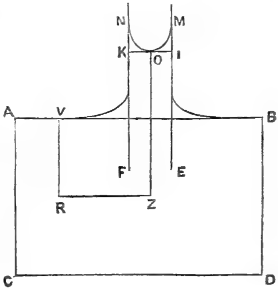
To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

The appearance of Lord Rayleigh's paper on Laplace's Theory of Capillarity encourages me to send you some remarks on the same subject which I had prepared some months ago in connexion with work on capillarity on which I have been engaged. I should be glad, however, that it should appear on the present occasion, as I think it may be of some use in explaining, rather more explicitly than Lord Rayleigh attempts to do, how the misconception of Laplace's quantity K has arisen—a misconception which has become of almost historical importance in connexion with this subject.

The object with which Laplace sets out is to explain the fact that the hydrostatic pressure in a liquid just below a curved surface differs from the hydrostatic pressure just below a plane surface by an amount depending on the curvature of the surface.

He gives the accompanying figure, representing a vertical section of a capillary tube N E plunged in a vessel A C containing a liquid which wets the tube.



V R Z O represents an infinitely thin canal or filament of the liquid whose cross section is taken as the unit of area, and which meets the plane surface at V and the curved surface at its lowest point O. On the assumption that the fluid is of uniform density throughout, and that between any pair of elementary volumes of it there is an attraction which is a function only of their masses and the distance between them, and that this function vanishes when the distance becomes sensible, he points out that any elementary length dz of the filament at a sensible distance below the surface is attracted by the surrounding liquid equally in all directions, and that to establish hydrostatic equilibrium in the canal it is only necessary to show that the pressure on the liquid of the canal at V exceeds the pressure at O by the weight of the column reaching from O to the level of V. He therefore examines the action of the surrounding liquid on the topmost element of the canal at V, and finds that it can be expressed by a certain integral, which he calls K, and is of the nature of a pull downwards, producing a pressure K on the base of the element dz which is transmitted along the canal. At the other extremity O of the canal there is the same action K downwards due to the liquid below the tangent-plane K O I, and at the same time an action upwards due to the meniscus N K I M, which latter action he finds can be expressed by $\frac{H}{b}$, where H is a second integral and b the radius of the curved surface (supposed in the first instance spherical). And he points out that this second quantity $\frac{H}{b}$ is very much smaller than the quantity K; and he argues that the elevation of the liquid in the canal Z O above the level V is due to the attraction of the meniscus.

This proceeding is perfectly legitimate. It explains the equilibrium of one portion of the fluid, namely the infinitely thin filament, considered on the supposition that the surrounding liquid is in equilibrium both with itself and with all portions of the filament at a depth greater than dz below the surface. But it does not account for this latter equilibrium. Nevertheless Laplace goes on as if he had now explained the elevation of the whole of the liquid in the tube. In doing so he tacitly assumes that the argument is equally applicable (or at least applicable in kind) to all the filaments of which the liquid in the tube may be regarded as composed. It is here that his theory is incomplete.

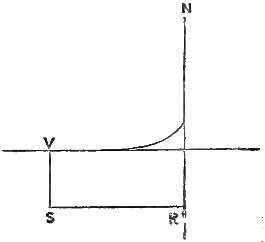
In considering that the equilibrium of the filament is determined by the pressures parallel to its length produced by the action of the surrounding liquid on the topmost element at each end, Laplace assumes that all elements at a greater depth below the surface are in equilibrium with the liquid around them. If this assumption is made for one filament, it must be made for all. In other words, he really starts with the assumption that all the liquid at a depth greater than dz below the surface is in equilibrium with itself, and then considers the action between this mass and the surface layers.

Now to every action there is an equal and opposite reaction, and consequently the attraction K downwards of any topmost element dz at the plane surface by the surrounding liquid is accompanied by an equal attraction of the surrounding filaments upwards exercised on portions whose depth is greater than dz, *i. e.* on portions in other respects in equilibrium; and since the sum of the actions K downwards in all the filaments is equal to the sum of the reactions upwards, it is evident that no pressure will be transmitted to the liquid in the canal at a sensible depth below the surface, but that the only effect of these equal and opposite forces is to create an elastic reaction within the liquid.

This is equivalent to saying that each layer of the liquid of less than sensible thickness throughout the liquid clings to the next with a cohesive force producing what may be called a molecular pressure of the nature of an elastic reaction within the liquid. This molecular pressure is not transmitted, like an hydrostatic pressure, through the liquid to any sensible distance, but at any point has its origin in the molecular actions in the immediate neighbourhood of that point.

Although Laplace consistently adopts the fiction that this molecular pressure K is transmitted along any infinitely thin canal from the surface and balanced by an equal pressure transmitted from the other end, yet he seems to have had a perfectly correct view of the physical meaning of the quantity.

Thus, in his introduction on p. 351, *Mécanique Céleste*, Supplément au Livre X., he says:—"Je pense que de ce terme (K) dépendent la suspension du mercure dans un tube de baromètre à une hauteur deux ou trois fois plus grande que celle qui est due à la pression de l'atmosphère, le pouvoir réfringent de corps diaphanes, la cohésion, et généralement les affinités chimiques."



Again, in No. 11, p. 391, in considering the pressure transmitted along an elementary canal V S R to the plane surface N R of an immersed solid, after pointing out that the pressure transmitted along S R is equal to the external pressure P, +K, $+g$. VS, he goes on to say:—"L'action dont le fluide du canal R S est animé est égale: 1° à l'action du fluide sur ce canal, et cette action est égale à K; 2° à l'action du plan sur le même canal. Mais cette action est détruite par l'attraction du fluide sur le plan, et il ne peut en résulter dans le plan aucune tendance à se mouvoir; car, en ne considérant que ces attractions réciproques, le fluide et le plan seraient en repos, l'action étant égale et contraire à la réaction; ces attractions ne peuvent produire qu'une adhérence du plan au fluide, et l'on peut ici en faire abstraction. Il suit de là que le fluide presse le point R avec une force égale à $P+K+g.VS-K$, ou simplement $P+g.VS$." Thus showing quite clearly that he did not regard the pressure K as capable of being transmitted to a solid surface.

It is important, however, to observe that in the next page, when comparing the pressures at the two sides of the plate at another level, he does not take the trouble to cancel K in this way, but speaks of it as transmitted to the plate, but balanced by an equal pressure transmitted from the other side. He writes carelessly in fact. And though in the very next paragraph, in treating in precisely the same manner the pressure at a third point, he again accurately cancels the K before it reaches the plate, it is probable that he does so because of the necessity of getting rid of it (there being at this point nothing but atmospheric pressure at the other side of the plate), rather than from a sense of the importance of using language that should be strictly correct.

It will be observed, however, that he consistently speaks of the elevation of the liquid in a capillary tube as due to the attraction of the meniscus, and not to the excess of pressure at the plane surface.

I have quoted these passages because they show clearly that Laplace did not himself fall into the error of regarding the pressure K as transmitted to immersed solids, or of considering the elevation of the liquid as due to a *vis a tergo* having its origin in the free surface. It is not surprising, however, that others should have fallen into this mistake when we find that he occasionally used language which could only encourage such a view.

It may be noticed that, through taking for granted the equilibrium of all the liquid except the single canal which he considers, Laplace does not at first discover the real seat of the external force by which the liquid is elevated. In the Second Supplement, however, proceeding by a different method, he traces it to the action of the solid matter of the tube just beyond the edge of the liquid where it meets the inner wall of the tube.

It is well known that Young objected to Laplace's theory that he had not considered the "repulsive force of heat within the liquid"—a phrase of which the nearest modern equivalent would be "the repulsive or elastic reaction between the molecules considered with reference to temperature;" and indeed the passage which I have quoted is, I believe, the only one in which Laplace takes any elastic reaction into consideration.

In a short paper published in the *Bulletin de la Société Philomathique de Paris*, 1819–1822 (referred to by Poisson), after pointing out that the chemical attraction is very great while the capillary effect is very small, he says:—"La théorie que j'ai donnée de ces phénomènes embrasse l'action des deux forces dont je viens de parler en prenant pour l'expression intégrale de l'effet capillaire, la différence des deux intégrales relatives à l'attraction moléculaire et à la force répulsive de la chaleur ce qui répond à l'objection du savant physicien M. Young." Poisson, however, points out that this answer is quite insufficient; and in his own theory, by examining carefully into the nature of the internal equilibrium in which the "repulsive force of heat" is involved, concludes (1) that there must be a very rapid increase of density close to the surface as we descend into the liquid, and (2) that but for this great variation of density none of the phenomena of capillarity would occur. Clerk Maxwell, in his article in the *Encyclopædia Britannica* on this subject, speaks of the latter conclusion as mathematically wrong, by which I think he means that in liquids, for example, much less compressible than any we know of, there would still be a surface-tension, though the variation of density near the surface was vanishingly small.

Laplace does indeed point out, on p. 494 in the General Considerations appended to the Second Supplement, that the compression of the liquid is zero at the surface, and increases with extreme rapidity, reaching a constant value before a sensible depth below the surface is attained. But he is content with this cursory observation. Had he attended sufficiently to this point, he would probably not have spoken as he does in the last paragraph of his treatise of the idea of a surface-tension, where he endorses the view of Segner:—"Qu'elle n'était qu'une fiction propre à représenter les phénomènes, mais que l'on ne devait pas admettre qu'autant qu'elle se rattachait à la loi d'une attraction insensible à des distances sensibles."

These words of Laplace have, I believe, contributed greatly to hinder the acceptance of the idea of a surface-tension. That he should have held such a view is, however, not surprising; for it can be shown that it is in virtue of this very *extension* of the liquid near the surface, which he ignores, that the surface-tension exists, and that the surface layers are a seat of energy, and physically different from the liquid in the interior.

I am, Gentlemen,

Your obedient Servant,

Clifton, October 1, 1883. A. M. WORTHINGTON.

XLVIII. *The Dilatation of Crystals on Change of Temperature.*

By L. FLETCHER, *M.A., of the Mineral Department, British Museum; late Fellow of University College, Oxford.*

[Continued from p. 300.]

Second Part.

ALTHOUGH we can thus find to any degree of accuracy the isotropic planes and the maximum and minimum expansions for lines in any zone-plane of an Anorthic crystal, and therefore the positions and relative magnitudes of the principal axes of the section of the ellipsoid made by the given zone-plane, the above method cannot be satisfactorily extended to the calculation of the directions and relative magnitudes of the three principal expansions of the crystal.

We shall accordingly solve the problem in an entirely different way, and, to render the solution for three dimensions more easy to follow, shall take first the case of an Oblique crystal and obtain remarkably simple formulæ for the calculation of the thermic axes directly from the parameters themselves.

At the first temperature let OA, OB, OC be the crystallographic axes, and let their lengths be denoted by A, B, C; as usual let OB be the axis perpendicular to the plane of symmetry containing the two axes OA, OC. At the second temperature the axis OB′ will coincide in direction with OB, but will have a length B′, and the axes OA, OC will have taken up new directions OA′, OC′ and new lengths A′, C′.

The measurement of the angles of a crystal at two temperatures can give only relative, not absolute, displacements of the lines; and without affecting calculations of these relative positions, the crystal may be imagined to be rotated at the second temperature round OB until OC′ coincides in direction with OC. If it should be hereafter necessary to determine the absolute displacement of any line, this imaginary rotation of the line with the whole crystal will have to be compounded with the relative displacement now about to be calculated; till then this rotation of the whole crystal may be disregarded.

It will be convenient first to calculate from the alterations of the lines OA and OC the corresponding alterations of a line OX in the plane AOC and initially at right angles to OC (fig. 8).

Denote as usual the angle AOC by β.

(1) Let x, z be the coordinates of a point P referred to the rectangular axes OX, OC, and x_1, z_1 the coordinates of the same point referred to the oblique axes OA, OC; draw PN parallel to CO and meeting OX, OA in N and M respectively.

Then
$$x = \mathrm{ON}, \qquad z = \mathrm{PN},$$
$$x_1 = \mathrm{OM}, \qquad z_1 = \mathrm{PM};$$
whence
$$\left.\begin{aligned} x &= x_1 \sin\beta, & z &= z_1 + x_1 \cos\beta, \\ x_1 &= \frac{x}{\sin\beta}, & z_1 &= z - x\cot\beta. \end{aligned}\right\} \quad . \quad . \quad . \quad \text{(I.)}$$
and

(2) At the second temperature OA, OC become OA′, OC′, whilst β becomes β' and OP, OP′; if, as before, a line P′M′ be drawn parallel to OC′ or OC, then, since parallel lines or parts of the same line retain their ratios on change of temperature of the crystal,
$$\frac{\mathrm{OM'}}{\mathrm{OM}} = \frac{\mathrm{OA'}}{\mathrm{OA}} = \frac{\mathrm{A'}}{\mathrm{A}},$$
$$\frac{\mathrm{P'M'}}{\mathrm{PM}} = \frac{\mathrm{OC'}}{\mathrm{OC}} = \frac{\mathrm{C'}}{\mathrm{C}};$$
whence, if ξ_1, ζ_1 be the coordinates OM′, P′M′, of P′ referred to the axes OA′, OC′,
$$\xi_1 = \frac{\mathrm{A'}}{\mathrm{A}}\,\mathrm{OM} = \frac{\mathrm{A'}}{\mathrm{A}}\frac{x}{\sin\beta},$$
$$\zeta_1 = \frac{\mathrm{C'}}{\mathrm{C}}\,\mathrm{PM} = \frac{\mathrm{C'}}{\mathrm{C}}(z - x\cot\beta).$$

(3) If, further, ξ, ζ be the coordinates of P′ referred to the old rectangular axes OX, OC, then, just as in equations (I.),
$$\xi = \xi_1 \sin\beta',$$
$$\zeta = \zeta_1 + \xi_1 \cos\beta';$$
whence the rectangular coordinates of P′, namely ξ, ζ, are related to the rectangular coordinates of P, namely x, z, by the equations
$$\left.\begin{aligned} \xi &= x\frac{\mathrm{A'}}{\mathrm{A}}\,\frac{\sin\beta'}{\sin\beta}. \\ \zeta &= x\frac{\mathrm{A'}}{\mathrm{A}}\,\frac{\cos\beta'}{\sin\beta} + z\frac{\mathrm{C'}}{\mathrm{C}} - x\frac{\mathrm{C'}}{\mathrm{C}}\cot\beta. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad \text{(II.)}$$

(4) Let P, R be the expansions of the unit lines in the directions OX, OC, and let E be the angle of rotation of the crystal-line OX for the given change of temperature; then

$$\left.\begin{array}{l} \mathrm{R}=\dfrac{\mathrm{C}'}{\mathrm{C}}-1. \\[2ex] \text{For the point X we have, by writing } x=\mathrm{OX}, \text{ and } z=0 \\ \text{in equations (II.),} \\ \qquad \mathrm{P}=\dfrac{\mathrm{OX}'}{\mathrm{OX}}-1=\dfrac{\mathrm{A}'}{\mathrm{A}}\dfrac{\sin\beta'}{\sin\beta}-1; \\[2ex] \text{and, neglecting small quantities of the second order,} \\ \qquad \mathrm{E}=\dfrac{\mathrm{XX}'}{\mathrm{OX}}=\dfrac{\mathrm{A}'}{\mathrm{A}}\dfrac{\cos\beta'}{\sin\beta}-\dfrac{\mathrm{C}'}{\mathrm{C}}\dfrac{\cos\beta}{\sin\beta}. \end{array}\right\} \text{(III.)}$$

(5) In exactly the same way in which the relations of the rectangular coordinates ξ, ζ of any point at the second temperature to those of the same point at the first were found in terms of the alterations of the crystal-lines OA, OC, and of the angle AOC, we can find these coordinates in terms of the alterations of the crystal-lines OX, OC, and of the angle XOC; in fact we need only to write in equations (II.) $\frac{\mathrm{OX}'}{\mathrm{OX}}$ for $\frac{\mathrm{OA}'}{\mathrm{OA}}$, XOC for AOC, and X′OC for A′OC. The relations may be determined also by substituting in equations (II.) the values of the coefficients given by equations (III.).

We thus get, neglecting squares of small quantities,

$$\left.\begin{array}{l} \xi=(1+\mathrm{P})\,x, \\ \zeta=(1+\mathrm{R})\,z+\mathrm{E}x; \end{array}\right\} \quad . \quad . \quad . \quad . \quad \text{(IV.)}$$

or, reversing,

$$\left.\begin{array}{l} x=(1-\mathrm{P})\,\xi, \\ z=(1-\mathrm{R})\,\zeta-\mathrm{E}\xi. \end{array}\right\} \quad . \quad . \quad . \quad . \quad \text{(V.)}$$

(6) We can now find a formula for the determination of the lines of greatest and least expansion in either of the following ways:—

First Method.—Let ψ, ψ' be the inclinations of the same crystal-line OP, OP′ to the line OX at the two temperatures; then, from equations (IV.),

$$\tan\psi'=\frac{\zeta}{\xi}=\frac{(1+\mathrm{R})\,z+\mathrm{E}x}{(1+\mathrm{P})\,x}=(1+\mathrm{R}-\mathrm{P})\tan\psi+\mathrm{E},$$

whence $\tan\psi'-\tan\psi=(\mathrm{R}-\mathrm{P})\tan\psi+\mathrm{E}$;
and, still neglecting squares of small quantities,

$$\psi'-\psi=(\mathrm{R}-\mathrm{P})\sin\psi\cos\psi+\mathrm{E}\cos^2\psi.$$

For the line perpendicular to OP, where $\psi_1=\psi+\frac{\pi}{2}$, we have

$$\psi_1'-\psi_1 = -(\mathrm{R}-\mathrm{P})\sin\psi\cos\psi+\mathrm{E}\sin^2\psi,$$

whence, if the lines be isotropic, we have, by equating the above values,

$$\tan 2\psi = \frac{\mathrm{E}}{\mathrm{P}-\mathrm{R}}. \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{VI.})$$

Second Method.—From equations (V.) it is seen that the circle $x^2+z^2=1$ will become the ellipse

$$(1-2\mathrm{P})\xi^2-2\mathrm{E}\xi\zeta+(1-2\mathrm{R})\zeta^2=1,$$

whence, by the usual formula, if ψ, $\psi+\frac{\pi}{2}$ be the inclinations of the axes of the ellipse to the axis OX,

$$\tan 2\psi=\frac{\mathrm{E}}{\mathrm{P}-\mathrm{R}},$$

as before.

(7) By measurement of the angles of a crystal, not the absolute lengths A, B, C, but the ratios A : B : C are determined, and are generally expressed by the symbol $a:1:c$. If the parametral ratios for the two temperatures be $a:1:c$ and $a':1:c'$, we shall thus have $\mathrm{A}=\mathrm{B}a$, $\mathrm{C}=\mathrm{B}c$; similarly for the second temperature, $\mathrm{A}'=\mathrm{B}'a'$, $\mathrm{C}'=\mathrm{B}'c'$; whence, if λ be the coefficient of expansion in the direction OB and therefore $\frac{\mathrm{B}'}{\mathrm{B}}=(1+\lambda)$, we may write, still neglecting squares of small quantities,

$$\frac{\mathrm{A}'}{\mathrm{A}}=(1+\lambda)\frac{a'}{a}=\frac{a'}{a}+\lambda,$$

$$\frac{\mathrm{C}'}{\mathrm{C}}=(1+\lambda)\frac{c'}{c}=\frac{c'}{c}+\lambda.$$

Substituting these values of $\frac{\mathrm{A}'}{\mathrm{A}}, \frac{\mathrm{C}'}{\mathrm{C}}$ in equations (III.), we find that we may write

$$\left.\begin{aligned} &\mathrm{P}=p+\lambda, \quad \mathrm{R}=r+\lambda, \quad \mathrm{E}=e, \\ \text{where}\quad & \\ &p=\frac{a'}{a}\,\frac{\sin\beta'}{\sin\beta}-1, \\ &r=\frac{c'}{c}-1, \\ &e=\frac{a'}{a}\,\frac{\cos\beta'}{\sin\beta}-\frac{c'}{c}\,\frac{\cos\beta}{\sin\beta}. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (\mathrm{VII.})$$

(8) Substituting these values of P, R, E in the expression for the inclinations of the thermic axes, we get

$$\tan 2\psi = \frac{\mathrm{E}}{\mathrm{P}-\mathrm{R}} = \frac{e}{p-r} = \frac{\frac{a'}{a}\frac{\cos\beta'}{\sin} - \frac{c'}{c}\frac{\cos\beta}{\sin\beta}}{\frac{a'}{a}\frac{\sin\beta'}{\sin\beta} - \frac{c'}{c}},$$

from which, it may be remarked, λ has disappeared. Simplifying, we get

$$\tan 2\psi = \frac{\frac{\frac{a'}{a}-\frac{c'}{c}}{\beta'-\beta} - \tan\beta}{\frac{\frac{a'}{a}-\frac{c'}{c}}{\beta'-\beta}\tan\beta + 1}.$$

If $\frac{\frac{a'}{a}-\frac{c'}{c}}{\beta'-\beta} = \tan\chi$, we have $\psi = \frac{\chi-\beta}{2}$. . . . (VIII.)

These simple formulæ (VIII.) thus give the directions of the thermic axes in terms of the parameters at the two temperatures.

(9) To determine the principal expansions directly from the parameters at the two temperatures.

(*a*) From Prop. XIV., p. 292, we know that if Δ and Δ_1 be the expansions in directions inclined to a thermic axis at angles θ and ϕ respectively,

$$\Delta - \delta = (\delta_1 - \delta)\sin^2\theta,$$
$$\Delta_1 - \delta = (\delta_1 - \delta)\sin^2\phi;$$

whence

$$\Delta_1 - \Delta = (\delta_1 - \delta)(\sin^2\phi - \sin^2\theta)$$
$$= (\delta_1 - \delta)\sin(\phi+\theta)\sin(\phi-\theta),$$

and

$$\delta_1 - \delta = \frac{\Delta_1 - \Delta}{\sin(\phi+\theta)\sin(\phi-\theta)}.$$

If the directions θ, ϕ coincide with the lines OA, OC respectively,

$$\theta = \frac{\pi}{2} - (\psi+\beta), \quad \phi = \frac{\pi}{2} - \psi,$$

and

$$\Delta_1 - \Delta = \frac{\mathrm{C}'}{\mathrm{C}} - \frac{\mathrm{A}'}{\mathrm{A}}$$
$$= \frac{c'}{c} - \frac{a'}{a}.$$

Substituting these values in the above formula we get

$$\delta-\delta_1=\frac{\frac{a'}{a}-\frac{c'}{c}}{\sin\beta\sin(2\psi+\beta)}. \quad . \quad . \quad . \quad \text{(IX.)}$$

Since

$$\psi=\frac{\chi-\beta}{2} \quad \text{and } \tan\chi=\frac{\frac{a'}{a}-\frac{c'}{c}}{\beta'-\beta},$$

the equation (IX.) may be expressed also in the following ways

$$\delta-\delta_1=\frac{\frac{a'}{a}-\frac{c'}{c}}{\sin\beta\sin\chi}=\frac{\beta'-\beta}{\sin\beta\cos\chi}. \quad . \quad . \quad \text{(X.)}$$

(*b*) From the above we also have

$$\Delta-\delta=(\delta_1-\delta)\cos^2(\psi+\beta),$$

and

$$\Delta_1-\delta=(\delta_1-\delta)\cos^2\psi,$$

where Δ and Δ_1 are the expansions along OA, OC.

If δ_2 be the expansion perpendicular to the symmetry-plane,

$$\delta_2=\frac{\text{B}'}{\text{B}}-1=\lambda=\frac{\text{A}'}{\text{A}}-\frac{a'}{a}=1+\Delta-\frac{a'}{a}=\Delta-\frac{a'-a}{a};$$

similarly

$$\delta_2=\Delta_1-\frac{c'-c}{c}.$$

Hence

$$\delta_2-\delta=-\frac{a'-a}{a}+(\delta_1-\delta)\cos^2(\psi+\beta)$$

$$=-\frac{c'-c}{c}+(\delta_1-\delta)\cos^2\psi;$$

from either of which equations $\delta_2-\delta$ can be found.

(10) We shall illustrate the above formulæ by application to the same case as before.

From the angles given on page 294, it follows from the usual formulæ, namely,

$$a=\frac{\tan ma}{\sin ac},$$

$$c=\frac{\cot lb}{\sin da},$$

$$\beta=180°-ac,$$

that the parameters are as follows :—

First temperature.	Second temperature.
$a=0{\cdot}6919835$,	$a'=0{\cdot}6899715$,
$c=0{\cdot}4145025$,	$c'=0{\cdot}4134817$,
$\beta=99^\circ\ 27'\ 22''{\cdot}8$,	$\beta'=99^\circ\ 37'\ 5''{\cdot}2$;

whence

$$(a)\quad \tan\chi=\frac{\frac{a'-a}{a}-\frac{c'-c}{c}}{\beta'-\beta}=-\frac{{\cdot}002907584-{\cdot}002462712}{582{\cdot}4\times \text{c.m. of } 1''},$$

$$\chi=180^\circ-8^\circ\ 57'\ 13''{\cdot}6,$$

and

$$\psi=\frac{\chi-\beta}{2}=35^\circ\ 47'\ 41''{\cdot}8.$$

The previous method gave as a first approximation 35° 44′ 10″·85 (page 295), and as a second approximation 35° 46′ 32″·58 (page 298).

$$(b)\quad \delta-\delta_1=\frac{\frac{a'-a}{a}-\frac{c'-c}{c}}{\sin\beta\sin\chi}$$

$$=-\frac{{\cdot}00444872}{\sin 99^\circ\ 27'\ 22''{\cdot}8\ \sin 8^\circ\ 57'\ 13''{\cdot}6};$$

whence $\delta_1-\delta=0{\cdot}00289776.$

The previous method (page 295) gave 0·0028987, and Neumann's result was 0·002892.

$$(c)\qquad \delta_2-\delta=-\frac{a'-a}{a}+(\delta_1-\delta)\cos^2(\psi+\beta)$$

$$=+{\cdot}002907584+0{\cdot}00289776\cos^2 135^\circ\ 15'\ 4''{\cdot}6$$
$$={\cdot}004369172.$$

Again,

$$\delta_2-\delta=-\frac{c'-c}{c}+(\delta_1-\delta)\cos^2\psi$$

$$={\cdot}002462712+{\cdot}00289776\cos^2 35^\circ\ 47'\ 41''{\cdot}8$$
$$={\cdot}004369172.$$

The previous method gave 0·0043757 (page 300); Neumann's result was 0·004371.

[To be continued.]

XLIX. *The Ice of Greenland and the Antarctic Continent not due to Elevation of the Land.* By JAMES CROLL, *LL.D., F.R.S.**

GREENLAND.—The only two continents on the globe covered by permanent ice and snow are Greenland and the Antarctic. But are these continents to be regarded as Highlands or as Lowlands? It is an opinion held by many that these regions are greatly elevated, and that it is mainly owing to this elevation that they are so completely buried under ice. I have been wholly unable to find evidence for any such conclusion. It is of course true that, in regard to Greenland at least, the observations of Rink, Heyes, Nordenskjöld, Jensen, Brown, and others show that the upper surface of the inland ice is greatly elevated above the sea-level. Dr. Rink, for example, states that the elevation of this icy plain, at its junction with the outskirts of the country where it begins to lower itself through the valleys, in the ramifications of the Bay of Omenak is about 2000 feet, from which it gradually *rises towards the interior*. Nordenskjöld, 30 miles from the coast, reached an elevation of 2200 feet, and found the ice continued to *rise inwards*. Heyes, who penetrated 50 miles into the interior, found the elevation about 5000 feet, and still *continuing to slope upwards towards the interior* of the continent. This upward slope is a necessary condition of continental ice, and must continue till the centre of dispersion is reached. As the larger portion of the Greenland ice is discharged at the west coast, it is probable that this centre of dispersion, or rather ice-shade, will lie nearer to the east coast than to the west. There is little doubt that the greater part of the surface of the inland ice is far above the snow-line; but this does not prove that Greenland is an elevated country, for this elevation of the upper surface of the ice may be due *entirely* to the thickness of the sheet. If the sheet is at least 1000 or 2000 feet thick at its edge, it is not surprising that it should be 5000 feet thick 50 miles in the interior, seeing that it is a physical and mechanical necessity that continental ice should gradually thicken towards the centre of dispersion. It has been shown from physical considerations ('Climate and Time,' pp. 374–386) that the thickness of the ice in the centre of Greenland is probably upwards of 2 miles, and that the Antarctic ice-cap at the South Pole, which is most likely not far from the centre of dispersion, must be over 6 miles in thickness.

* Communicated by the Author.

Certainly no one has ever seen, and probably no one ever will see, elevated land under the ice either of Greenland or the Antarctic continent; and to assume its existence because those regions are so completely glaciated would simply be to beg the very question at issue.

It will doubtless be urged that, although the ground under the ice may not be elevated, yet there may be lofty mountain-chains in the interior which might account for the origin of the ice. We have, I think, good grounds for concluding that if there are mountain-ranges in the interior of Greenland (of which there is absolutely no proof, although one or two isolated peaks have been seen), they must be wholly buried under the ice. For if mountain-masses rise above the icy mantle, there ought to be evidence of this in the form of broken rock, stones, earth, and other moraine matter lying on the inland ice. "But as soon as we leave the immediate vicinity of the coast," says Dr. Brown, "no moraine is seen coming over the inland ice. No living creature, animal or plant, except a minute alga." This could not possibly be the case if ranges of mountains rose above the general ice-covering. These mountain-ranges, if they exist, are doubtless covered with snow and their sides with glaciers; but this would not prevent pieces of broken rock and stones from rolling down upon the inland ice. In fact it would have the very opposite result; for glaciers would be one of the most effective agents possible in bringing down such material, and it is certain that no avalanche of snow could take place without carrying along with it masses of stones and rubbish. All these materials brought down from the sides of the projecting peaks would be deposited on the surface of the inland ice and carried along with it in its outward motion from the centre of dispersion, and could not fail to be observed did they exist. The fact that no such thing is ever seen is conclusive proof that these supposed projecting mountain-ranges do not exist.

But it may still be urged that the absence of moraine matter on the surface of the inland ice is not sufficient evidence that they do not exist; for as this material from the interior would have to travel hundreds of miles before reaching the outskirts, a journey occupying a period of many years, the stones would become buried under the successive layers of ice formed on the surface during their passage outwards. But supposing this were the case, these buried moraines, if they existed, ought to be seen projecting from the edge of the sheet at places where icebergs break off, and also on the edges of the icebergs themselves near to their tops; but such, I presume, is never the case. Further, as the inland ice has to force its way through the comparatively narrow fjords before reaching the sea, the moraines could not fail to be occasionally observed did they exist.

But supposing there were mountains in the interior, this would not account for the general ice-covering. It would not account for the intervening spaces between the mountains being filled up with ice. To account for the whole country being covered with ice through the influence of mountains, we should have to assume that it was studded over with them at no great distance from one another; otherwise all that we should have would simply be local glaciers.

Dr. Robert Brown, one of the highest authorities in matters relating to Greenland, who does not believe in the existence of mountain-masses in the interior, says:—"I do not think a range of mountains at all necessary for the formation of this huge *mer de glace*, for this idea is derived from the Alpine and other mountain-ranges, where the glacial system is a petty affair compared with that of Greenland. I look upon Greenland," he continues, "and its interior ice-field in the light of a broad-lipped shallow vessel, but with breaks in the lip here and there, and the glacier like some viscous matter in it. As more is poured in, the viscous matter will run over the edges, naturally taking the line of the chinks as its line of outflow. The broad lips of the vessel, in my homely *simile*, are the outlying islands or 'outskirts;' the viscous matter in the vessel, the inland ice; the additional matter continually being poured in, the enormous snow-covering, which, winter after winter, for seven or eight months in the year, falls almost continuously on it; and the chinks or breaks in the vessel are the fjords or valleys down which the glaciers, representing the outflowing viscous matter, empty the surplus of the vessel" *.

In North Greenland and along Smith Sound a warm south-east wind, somewhat similar to the *Föhn* of Switzerland, has been reported in the middle of winter. From this it has been inferred by some that there must be high ranges of mountains in the interior from which this wind descends. There are, however, certainly no good grounds for such a conclusion; for we know that the upper surface of the inland ice of North Greenland, 50 or 100 miles from the outskirts, has an elevation of at least 4000 or 5000 feet. Now a wind crossing this icy plateau and descending to the sea-level would have its temperature raised by upwards of 20°, and also its capacity for moisture at the same time greatly increased. The consequence would therefore be that, in the midst of a Greenland winter, such a wind would be felt to be hot and dry.

* 'Arctic Papers for the Expedition of 1875,' p. 24.

The opinion was expressed by Giesecke, who long resided in Greenland, that that country is merely a collection of islands fused together by ice. This opinion is concurred in by Dr. Brown, who says that "most likely it will be found that Greenland will end in a broken series of islands forming a Polar archipelago. That the continent (?) is itself a series of such islands and islets—consolidated by means of the inland ice—I have already shown to be highly probable, if not absolutely certain, as Giesecke and Scoresby affirmed." It has long been a belief that several of the west-coast fjords cut through Greenland from sea to sea—in short, that they are simply straits filled up with ice. The important bearing that this island-condition of Greenland has on the explanation of the warm interglacial periods of that country will be shown in a future article.

Antarctic Regions.—It needly hardly be remarked, that what has been stated as to the total absence of proof that Greenland possesses elevated plateaus and ranges of lofty mountains holds in a still more marked degree in reference to the Antarctic continent. Here is a region nearly 3000 miles across, buried under ice, on which the foot of man never trod. There is not the shadow of a ground for concluding that the interior of this immense region is, under the ice, greatly elevated, or that it possesses lofty mountain-ranges. The probability seems rather to be that, like Greenland, the area, as Sir Wyville Thomson supposes, consists of comparatively low dismembered land or groups of islands bound together by a continuous sheet of ice. "We have no evidence," says Sir Wyville, "that this space, which includes an area of about 4,500,000 square miles, nearly double that of Australia, is continuous land. The presumption would seem rather to be that it is at all events greatly broken up; a large portion of it probably consisting of groups of low islands united and combined by an extension of the ice-sheet."

"Various patches of Antarctic land," he continues, "are now known with certainty, most of them between the parallels of 65° and 70° S.; most of these are comparatively low, their height, including the thickness of their ice-covering, rarely exceeding 2000 to 3000 feet. The exceptions to this rule are the volcanic chain, stretching from Balleny Island to latitude 78° S.; and a group of land between 55° and 95° west longitude, including Peter the Great Island, Alexander Land, Graham Land, Adelaide Island, and Louis Philippe Land. The remaining Antarctic Land, including Adelie Land, Clairie Land, Sabrina Land, Kemp Land, and Enderby Land, nowhere rises to any great height"*.

* 'Lecture on Antarctic Regions' (Collins, Glasgow, 1877); 'Nature,' vol. xv.

There is another class of facts which shows still more conclusively the probably low flat nature of most of the Antarctic regions. I refer to the character of the great ice-barrier, and the bergs which break off from it. The icebergs of the southern ocean are almost all of the tabular form, and their surface is perfectly level, and parallel with the surface of the sea. The icebergs are all stratified; the stratifications run parallel with the surface of the berg. The stratified beds, as we may call them, are separated from each other by a well-marked blue band. These blue lines or bands, as Sir Wyville Thomson remarks, are the sections of sheets of clear ice; while the white intervening spaces between them are the sections of layers of ice where the particles are not in such close contact and probably contain some air. The blue bands, as Sir Wyville suggests, probably represent portions of the snow surface which during the heat of summer becomes partially melted and refrozen into compact ice; while the intervening white portions represent the snow of the greater part of the year, which of course would become converted into ice without ever being actually melted. It is therefore more than probable that each bed with its corresponding blue band may represent the formation of one year. Judging from the number of these layers in an iceberg, some of these layers must be of immense age, occupying a period probably of several thousand years in their formation. And as the ice is in a constant state of motion outwards from the centre of dispersion—probably the South Pole—the bergs before becoming detached from their parent mass must have traversed a distance of hundreds of miles.

The fact that these bergs must have travelled from great distances in the interior is further evident from the following consideration. The distance between the well-marked blue lines is greatest near the top of the berg, where it may be a foot or more, and becomes less and less as we descend, until, near the surface of the water, it is not more than two or three inches. This diminution in the thickness of the ice-strata from the top downwards has been considered by Sir Wyville to be mainly due to two causes—compression, and melting of the ice, particularly the latter. But in my paper on the Antarctic Ice (Quart. Journ. of Science, Jan. 1879) I have shown that, although compression and melting may have had something to do in the matter, this thinning of the strata from the top downwards is a necessary physical consequence of continental ice radiating from a centre of dispersion. Assuming the South Pole to be this centre, a layer which in, say, latitude 85° covers 1 square foot of surface will, on reaching latitude 80°, cover 2 square feet; at latitude 70° it will occupy 4 square feet, and at latitude 60° the space covered will be 6 square feet. Then if the layer was 1 foot thick at latitude 85°, it would be only 6 inches thick at latitude 80°, 3 inches thick at latitude 70°, and 2 inches at latitude 60°. Had the square foot of ice come from latitude 89° it would occupy 30 square feet by the time it reached latitude 60°, and its thickness would be reduced to $\frac{1}{30}$ of a foot, or $\frac{2}{5}$ of an inch.

Now the lower the layer the older it is, and the greater the distance which it has travelled. A layer near the bottom may have been travelling from the Pole for the past 10,000 or 15,000 years, whereas a layer near the top may perhaps not be 20 years old, and may not have travelled the distance of a mile. The ice at the bottom of a berg may have come from near the Pole, whereas the ice at the top may not have travelled 100 yards.

There is still another consideration which must be taken into account. It is this: the icebergs all seem to bear the mark of their original structure, and the horizontal stratifications appear also never to have been materially altered in their passage from the interior. This fact seems to have struck Sir Wyville forcibly. "I never saw," he says, "a single instance of deviation from the horizontal and symmetrical stratification which could in any way be referred to original structure; which could not, in fact, be at once accounted for by changes which we had an opportunity of observing taking place in the icebergs. There was not, so far as we could see, in any iceberg the slightest trace of structure stamped upon the ice in passing down a valley, or during its passage over *roches moutonnées* or any other form of uneven land; the only structure except the parallel stratifications which we ever observed which could be regarded as bearing upon the mode of original formation of the ice-mass, was an occasional local thinning-out of some of the layers and thickening of others, just such an appearance as might be expected to result from the occasional drifting of large beds of snow before they have time to become consolidated."

There cannot, I think, be the shadow of a doubt that these thin horizontal bands of clear blue ice, with their less dense and white intervening beds, are the original structure of the bergs. And it is evident that if the ice had crossed mountain-ridges, valleys, or other obstructions in the course of its journey from the interior, these beds could not have avoided being crushed, fractured, broken up, and mixed together. Had this happened, it would have been physically impossible that they could ever have been restored to their old positions. Ice is, no doubt, plastic, and pressure, along with motion, might perhaps induce fresh lines of stratification; but neither motion nor pressure could have selected broken blue bands from among the white and placed them in their old positions.

Why the icebergs from Greenland are not of the tabular form and stratified like those of the Antarctic regions, is doubtless owing to the fact that the Greenland ice is discharged through narrow fjords, which completely destroy the original horizontal stratifications.

Let us now see the consequence to which the foregoing considerations all lead. The tabular form and flat-topped character of the icebergs, with their perfectly horizontal bedding, show that they have been formed on a flat and even surface. They show also that this flat surface is not a mere local affair, but that it must be the general character of the Antarctic land ; for all, or nearly all, of the bergs are of this tabular form. Again, the unaltered character of the stratifications of the bergs shows that there can be no great mountain-ranges, or even much rough and uneven ground in the interior; for if there were, the bergs in their passage outwards would have had to pass over it; and this they could not have done and still have retained, as they actually have, their horizontal stratifications undisturbed. These icebergs, as we have seen, must have traversed in their outward motion, before being disconnected with the ice-sheet, a distance of hundreds of miles ; yet none of them bear the marks of having passed down or across a valley or even over *roches moutonnées*.

That the Antarctic continent has a flat and even surface, the character of the icebergs shows beyond dispute. But this, it will be urged, does not *prove* that this surface may not be greatly elevated ; in other words, that it may not be a flat elevated plateau. This, of course, is true ; but it is evidently far more likely that this region, nearly 3000 miles across, should consist of flat dismembered land, or groups of low islands separated and surrounded by shallow seas, than that it should consist of a lofty plateau without either hills, valleys, or mountain-ridges. In this case it may be that the greater part of the Antarctic ice-cap rests on land actually below sea-level—viz. on the floor of the shallow seas surrounding those island-groups. We know that such a condition of things was actually the case in regard to the great ice-sheet of North-western Europe during the glacial epoch. A glance at the Chart of the path of the ice given in 'Climate and Time,' p. 448 (which since its publication has been proved to be correct in almost every particular), will show that the larger portion of the sheet rested on the bed of the Baltic, German Ocean, and the seas around Great Britain and Ireland and the Orkney and Shetland islands. That the Antarctic ice was formed on low and flat land bordered for considerable distances by shoal water was the opinion also of Sir Wyville Thomson.

Assuming then, what seems thus probable, that the Antarctic regions consist of low discontinuous land, it will help to explain, as will be shown in a future paper, the disappearance of the ice during the warm interglacial periods of the southern hemisphere.

On the Argument against the Existence of a South-Polar Ice-cap.—We have certainly no evidence that during even the severest part of the glacial epoch an ice-cap, like that advocated by Agassiz and other extreme glacialists, ever existed at the North Pole; I am, however, unable to admit with Mr. Alfred R. Wallace that some such cap, though of smaller dimensions, does not at present exist at the South Pole. Speaking of the Antarctic ice-cap, Mr. Wallace says:—"A similar ice-cap is, however, believed to exist on the Antarctic Pole at the present day. We have, however, shown that the production of any such ice-cap is improbable, if not impossible; because snow and ice can only accumulate where precipitation is greater than melting and evaporation, and this is never the case except in areas exposed to the full influence of the vapour-bearing winds. The outer rim of the ice-sheet would inevitably exhaust the air of so much of its moisture, that what reached the inner parts would produce far less snow than would be melted by the long hot days of summer"*.

This opinion, that the mass of ice is probably greatest at the outer rim, which of course is most exposed to moist winds, and that it gradually becomes less and less as we proceed inwards till at last it disappears altogether, is by no means an uncommon one. At the present moment while I write (July 9th), Professor Nordenskjöld is probably attempting to cross the inland ice of Greenland with the hope of finding in the interior, hills, valleys, and green fields completely free from ice.

It by no means follows, as some might be apt to suppose, that the ice must be thickest where the snowfall is greatest. In case of continental ice the greatest thickness must always be at the centre of dispersion; but it is here that, owing to distance from the ocean, the snowfall is likely to be least.

We have no reason to believe that the quantity of snow falling, at least at the South Pole, is not considerable. Lieut.

* 'Island Life,' p. 156. I am unable to reconcile the above altogether with what Mr. Wallace says at page 132, where he refers approvingly to my statement that the Antarctic ice-sheet has been proved to be in some places at least over a mile in thickness at the edge, and that it must consequently be far thicker inland.

Wilkes estimated the snowfall of the Antarctic regions to be about 30 feet per annum; and Sir John Ross says that during a whole month they had only three days free from snow. But there is one circumstance which must tend to make the snowfall near the South Pole considerable, and that is the inflow of moist winds in all directions towards it; and as the area on which these currents deposit their snow becomes less and less as the Pole is reached, this must, to a corresponding extent, increase the quantity of snow falling on a given area. Let us assume, for example, that the clouds in passing from lat. 60° to lat. 80° deposit moisture sufficient to produce, say, 30 feet of snow per annum, and supposing that by the time they reach lat. 80° they are in possession of only one tenth part of their original store of moisture, still, as the area between lat. 80° and the Pole is but one eighth of that between lat. 60° and 80°, this would notwithstanding give 24 feet as the annual amount of snowfall between lat. 80° and the Pole.

However small may be the snowfall, and consequent amount of ice formed annually around the South Pole, unless it all melted it must of necessity accumulate year by year till the sheet becomes thickest there; for the ice could not move out of its position till this were the case. But supposing there were no snow whatever falling at the Pole and no ice being formed there, still this would not alter this state of matters; for in this case the ice forming at some distance from the Pole all around would flow back towards the centre, and continue to accumulate there till the resistance to the inward flow became greater than the resistance to the outward; but this state would not be reached till the ice became at least as thick on the poleward as on the outward side. There is no evading of this conclusion unless we assume, what is certainly very improbable, if not impossible, viz. that the ice flowing polewards should melt as rapidly as it advances. We know, however, that in respect to the ice which flows outwards towards the sea little, if any, of it is melted; and it is only after it breaks off in the form of bergs and floats to warmer latitudes that it disappears, and that even with difficulty. It is therefore not likely that the ice flowing inwards towards the Pole, and without the advantage of escape in the form of bergs, should all happen to melt. If *little or none* of the ice flowing toward the Equator melts, it is physically impossible that *all* the ice flowing polewards should manage to do so; and if it did not all melt, it would accumulate year by year around the Pole till it acquired a thickness sufficient to prevent any further flow in that direction, or, in other words, till its thickness at the Pole became as great as it is all around.

The opinion that the great mass of the ice on the Antarctic continent and also on Greenland lies near to the outer edge, and that it gradually diminishes inwards till at last it disappears, is evidently one based on a misapprehension as to the physical conditions of continental ice. I cannot help believing that had Professor Nordenskjöld duly reflected on the necessary physical and mechanical conditions of the problem which he is endeavouring to solve, he would not have undertaken the journey across the Greenland ice.

NOTE, *September 22nd.*—Baron Nordenskjöld has just returned, and, as might have been expected, he found the interior of Greenland a desert of ice, with the icy plain gradually sloping upwards to the interior. The ice rose at the furthest spot reached (280 miles from the coast) to 7000 feet above the sea, and was *still seen to rise to the east.* The results of the expedition are, however, of the most important character, confirming, as they do, the true theory of continental ice.

L. *The Method of Least Squares. By* F. Y. EDGEWORTH, *M.A., Lecturer on Logic at King's College, London*.*

I.

THE Law of Error and the Method of Least Squares do not traverse the same ground ; and the direction of the one course of reasoning is inverse to that of the other. In the former we derive the formation of a source† of error from the confluence of an indefinite number of small tributaries. In the latter we start from a position lower down on the stream of causation, from observations resulting from a source of error, and reason up from given observations (accompanied with some knowledge of the source of error from which they result) to (a more complete knowledge of) the source of error, the facility-curve under which the observations range themselves. For example:—Given x_1, x_2, &c. observations diverging according to a probability-curve of known modulus, but unknown centre, to determine the centre. These distinctions are likely to recommend themselves to those who have studied Mr. Glaisher's article and other first-rate authorities. But there is another distinction, more interesting to the philo-

* Communicated by the Author.

† According to the received view, one particular source—the probability-curve; according to our views, a great variety of facility-curves (see previous paper).

sopher and less familiar to the mathematician, namely, that in the Law of Errors we are concerned only with the objective quantities about which mathematical reasoning is ordinarily exercised; whereas in the Method of Least Squares, as in the moral sciences, we are concerned with a psychical quantity—the *greatest possible quantity of advantage.* To illustrate this application of mathematics to psychical quantity is the primary object of the following paper; a secondary purpose is to classify the problems falling under our title (taken in a wide sense), and to offer some contributions towards their solution.

In order to attain the first object it is not necessary to go much beyond Laplace's Method of Least Squares. In the problem of Book II. art. 20 Laplace in effect, if not very explicitly, assumes that the sought result may be regarded as a linear function of the observations. He posits this form of the result, not assuming that the *most probable* value expressed in terms of the observations will be a linear function of the observations, which is in fact not generally true, but selecting the linear form as most *advantageous,* advantageous in respect of convenience to the calculator and avoidance of trouble. The linear form being assumed, Laplace goes on to determine the values of the constants. He decides in favour of the system of values which are inversely proportional to the respective mean squares of error upon two grounds, of which the *second* is here regarded as the more fundamental—namely, that system of values is to be preferred which minimizes the disadvantage incurred in the long run by employing any particular system of values. Laplace takes as the measure of this integrated disadvantage the *mean error.* Gauss (dissenting from Laplace on what may seem almost trivial grounds) takes as the "moment" of error the measure of detriment incurred in the long run, the *mean square.* And it is conceivable that another criterion, which, in comparison with that of Laplace and Gauss, may be described as the *mean zero power* (corresponding to that system which, as compared with other linear systems, affords the *most probable* value), may have been assumed by some, not as a first principle (Mr. Glaisher's view, to be presently considered), but as a derivative principle, as the measure of disadvantage.

It is here submitted that these three criteria are equally right and equally wrong. The probable error, the mean error, the mean square of error, are forms divined to resemble in an essential feature the real object of which they are the imperfect symbols—the quantity of evil, the diminution of pleasure, incurred by error. The proper symbol, it is submitted, for the quantity of evil incurred by a simple error is not any power of the error, nor any definite function at all, but an almost arbitrary function, restricted only by the conditions that it should vanish when the independent variable, the error, vanishes, and continually increase with the increase of the error. The proper symbol for the disadvantage incurred in the long run is an integral whose elements involve as factors the said arbitrary function. Advancing, then, in the direction indicated by Laplace and Gauss, let us designate the disadvantage of a single error by the symbol $F(x^2)$, where x is the amount of error and F continually increases with x. Or, if the detriment is not a symmetrical function of error, is not equal for the same extent of error, whether it be in excess or defect, put $F(x)$ for the right-hand value of x, and $f(x)$ for the left-hand value of x; x being taken as positive in both cases. Now suppose we have adopted some particular system of values for the constants γ_1, γ_2, &c. Then, by the *law of errors*, if we make several sets of observations, say

$$\begin{array}{cccc} x_1 & x_2 & x_3 & \&c., \\ x_1' & x_2' & x_3' & \&c., \\ x_1'' & x_2'' & x_3'' & \&c., \\ \&c. & & & \&c. \end{array}$$

the quantities

$$\begin{array}{c} \gamma_1 x_1 + \gamma_2 x_2 + \gamma_3 x_3 + \&c. \div S\gamma, \\ \gamma_1 x_1' + \gamma_2 x_2' + \gamma_3 x_3' + \&c. \div S\gamma, \\ \&c. \qquad\qquad \&c. \end{array}$$

will be ranged under a probability-curve of the form* $\frac{1}{\sqrt{\pi}\alpha}\epsilon^{-\frac{x^2}{\alpha^2}}$, where α is a known function of the sought quantities γ_1, γ_2, &c. We have now to take α so that the total disadvantage in the long run of an indefinite number of sets of observations may be a minimum. This total disadvantage is

$$\int_0^\infty \frac{1}{\sqrt{\pi}\alpha}\epsilon^{-\frac{x^2}{\alpha^2}}[F(x)+f(x)]dx.$$

Put $x=\alpha\xi$. The quantity which it is proposed to minimize becomes

$$\int_0^\infty \frac{1}{\sqrt{\pi}}\epsilon^{-\xi^2}[F(\alpha\xi)+f(\alpha\xi)]d\xi\,;$$

α being regarded as variable, the first term of variation

* Glaisher, *op. cit.*

becomes

$$d\alpha \int_0^\infty \frac{1}{\sqrt{\pi}} \epsilon^{-\xi^2} [F'(\alpha\xi) + f'(\alpha\xi)] \xi \, d\xi.$$

Every element of this integral is positive. Therefore the integral is positive. Therefore the propositum, the disadvantage, continually increases as α increases. Therefore it is the least possible when α is the least possible. Which was to be demonstrated.

By an extension of the preceding reasoning we obtain the following fundamental theorem. One instrument (or one method of using the same instrument, one method of treating given observations) is to be preferred to another, when, $\phi_1(x)$, $\phi_2(x)$ being the facility-functions expressing the divergence in the first and second cases respectively from the real point, $\int_0^x \phi_1(x)dx$ is greater than $\int_0^x \phi_2(x)dx$ for every value of x.

It may be objected that these results might better have been grounded on the more solid and objective foundation of greatest probability rather than greatest advantage; agreeably to Laplace's first view as formulated by Mr. Glaisher*—namely, it being assumed that the quantity to be measured is accurately determined, if its error lies between zero and infinitesimal k, that system of factors which renders the probability that the result obtained by means of them is accurately determined greater than the probability of a result obtained by means of any other system of factors is to be preferred. It may be replied that the principles of greatest advantage and greatest probability do *not* coincide *in general*; that here, as in other departments of action, when there is a discrepancy between the principle of utility and any other rule, the former should have precedence.

To exhibit this discrepancy it suffices to observe that the disadvantage which it is proposed to minimize is the loss of utility, the quantity of pain due to an erroneous measure being employed in practice, in the arts. Why is this evil a minimum when the probability of our measurement being within the distance k of the real quantity is a maximum? Let us take a simple, although grotesque, example. Here are two shoemakers competing for the contract to supply an army with boots. Other things being equal, we have to select him who makes the best fits, who minimizes the disadvantage expressed by Horace:—

> ". . . . ut calceus olim,
> Si pede major erit, subvertet; si minor, uret."

* Memoirs of the Royal Astronomical Society, xl. p. 101.

Is the shoemaker who makes a rather greater proportion of exact fits (who has a smaller probable error), but when he does make a misfit makes a terribly painful one, necessarily the most advantageous? To quote Horace* again, the slave who, in spite of his many accomplishments, "semel latuit" ("once and but once I caught him in a lie," as Pope turns it), may have had, so to speak, a less "probable error," and yet may have been less "advantageous" to his master than one who had a greater number of less serious faults. The man who seldom tells a lie, but when he does "lies like a man," may do more harm than the habitual dealer in white lies. I am aware that there is something paradoxical in the preceding illustrations; but I submit that it would be affectation in a mathematical writer not occasionally to glance at the real objects to which his ideal conceptions are applied, and that the disadvantages just instanced are quite homogeneous with, only more familiar than, the disadvantage due to the employment in the arts of erroneous measurement (*e. g.* the disadvantage of astronomical mismeasurement), the disadvantage which, according to Laplace and Gauss, it is the object of the calculus to minimize.

The following is a more dignified example. Here are two instruments of observation, the errors incurred by which are ranged respectively under a probability-curve with modulus unity, and under the facility-curve $y=\frac{1}{\pi}\frac{c}{c^2+x^2}$, where c is small. Which of these instruments is to be preferred? According to Mr. Glaisher's test, unquestionably the latter. But, according to our view, it may well happen that the disadvantage in the long run is in the former case finite, in the latter case infinite: for example, if the disadvantage dependent upon a particular error may be expressed as any power (not less than the first), or sum of powers, of the extent of error.

Nor, again, does the principle of greatest probability as compared with the principle of greatest utility give a consonant answer in the following case:—Given the law of error of a certain instrument, is it better to make a practice of confining ourselves to a single observation, or of proceeding to an average? Upon Mr. Glaisher's view, if the average facility-curve is such as to have its maximum value (its head, so to speak) above the primary curve, the average must be preferable to the simple observation. This also follows from our first principle (by the theory of p. 363) *when there is only one intersection between the primary and average semi-curves*; but not quite generally. Suppose that there are three inter-

* Ep. II. 2.

sections on each side of the origin, and that the space on each side of the origin is divided by ordinates (one at the origin, one at infinity, and two intermediate) into three compartments such that for each compartment the area bounded by the abscissa, ordinates, and primary curve is equal to the area bounded by the same right lines and the average curve. Suppose, further, that throughout the first and third compartments the disutility-function is almost level, the disadvantage of a particular error only just increases with the extent of error, while in the second compartment the same function rises steeply. Then the appropriate mathematico-psychical reasoning will show that it is better to abide by a single observation than to take an average.

The two preceding examples, to which it would be easy to add others similar, are not put forward as practically important, but rather (like the imaginary cases put by Hume in his inquiry concerning moral sentiments) as assisting us to distinguish the general principle from the particular rule, and in the act of doing so to discern the supremacy of the principle of utility.

We have next to review some typical instances of the problems solved by the method of least squares. Let us take as the principle of a rough classification, complexity. As a first division we may demarcate those cases in which there is but one measurable; a single quantity x, whereof x_1, x_2, &c. are values, or more generally functions, given. This class may be subdivided according as the facility-curves which generate x_1, x_2, &c. are (I.) or are not (II.) symmetrical. In the simpler cases we need not take the trouble of distinguishing between the most probable and most advantageous values, since they are coincident. But the distinction soon emerges.

Subclass I. may be subdivided according as the symmetrical facility-curves under which the observations are ranged are (A) or are not (B) *Probability*-curves.

I. A (1) The following is one of the simplest problems which our subject presents :—Given a set of observations x_1, x_2, &c., and given that they have been generated by divergence from an unknown point according to one given law of error, a probability-curve of given modulus, to find the most probable (and advantageous) value for the unknown point. By a familiar application of the differential calculus the sought value is the mean of the observations.

A variant of this problem is when the observations are distributed into groups, each of which diverges from one and the same unknown point according to different given probability-curves. The solution is of course the *weighted mean*.

Another variant in this and, be it said once for all, all the subsequent typical problems, is when the observations stand for, not the simple value of, but definite functions of, the real quantity.

I. A (2) Given a set of observations x_1, x_2, &c., and given that they were generated by divergence according to a given curve of probability of which the modulus is not given but sought. Let c be sought modulus; then it must be taken so that $\left(\frac{1}{c}\right)^n \epsilon^{-\frac{x_1^2+x_2^2+\&c.}{c^2}}$ should be a maximum; whence

$$\frac{c^2}{2}=\frac{\mathrm{S}x_r^2}{n}*.$$

A variant would be the case when the observations are distributed into groups, each with a different modulus, and the ratio between the moduli is given.

I. A (3) By the degradation of the data in either of the preceding problems we reach the complex problem:—Given a set of observations x_1, x_2, &c., and given that they have been generated by divergence according to one and the same probability-curve from a single point, but given neither that point nor the modulus, to find both. Put as the probability of the concurrence of a particular central point and a particular modulus

$$\frac{\mathrm{P}\,d\xi\,dc}{\int_0^\infty\int_{-\infty}^\infty \mathrm{P}\,d\xi\,dc},$$

where

$$\pi^{\frac{2}{n}}\times\mathrm{P}=\left(\frac{1}{c}\right)^n\times\epsilon^{-\left[\frac{(x_1-\xi)^2+(x_2-\xi)^2+\&c.}{c^2}\right]}.$$

Equating to zero the first term of variation (that is, equating to zero the coefficient of $d\xi$ and also the coefficient of dc in the development of the above *propositum*), we have two equations to determine ξ and c. Whence the most probable value of ξ is the mean of x_1, x_2, &c., and the most probable value of c is the square root of twice the mean square of apparent errors, viz.

$$\sqrt{2\left[\frac{(\xi-x_1)^2+(\xi-x_2)^2+\&c.}{n}\right]};$$

a solution of the compound problem which follows the analogy of the simple problems (1) and (2), and *which holds whether the number of observations be finite or infinite.*

I believe that this conclusion is usually restricted to the

* *Cf.* Merriman, 'On Least Squares,' p. 143.

case of an infinite number of errors; that, for the finite case, there would usually be substituted $(n-1)$ for n in the above value of c. It is with the greatest diffidence that I submit a statement which seems to be contradicted by the most distinguished authorities, including Gauss and Airy*; and only not contradicted, but not, as far as I remember, asserted, by Laplace and Poisson. Perhaps I have misunderstood the quæsitum proposed by Airy, Merriman†, and other writers. For I assert with some confidence that (in spite of the rather puzzling expressions of some authors) the procedure above employed to determine the maximum of an expression involving two independent variables is correct. I submit also that the only significant, or at least the most important, quæsita afforded by the case in hand are *either* what was enounced above, *or* what is the "probable error" ‡ incurred by taking the mean of the observations as the real value. To investigate this probable error, it is proper to consider the given observations as generated by one of an indefinite number of constitutions corresponding to the different values of ξ and c's, each of which operates in the long run a number of times proportionate to $\frac{\mathrm{P}\,dc\,d\xi}{\mathrm{SP}}$; that is, if we take the origin at the mean point of the observations, and put $c=\frac{1}{h}$ proportionate to

$$\frac{\dfrac{h^n}{\pi^{\frac{n}{2}}}\epsilon^{-[n\xi^2+\mathrm{S}x^2{}_r]h^2}dh\,d\xi}{\displaystyle\int_{-\infty}^{\infty}\int_0^{\infty}\mathrm{P}\,dh\,d\xi}.$$

It may be observed that when h is constant this expression reduces to

$$\frac{1}{\sqrt{\pi}}\sqrt{n}\,h\,\epsilon^{-nh^2\xi^2};$$

from which we see that in case (1) the probable error incurred by taking the average of operations is the unit probable error, as it may be called, ·476 multiplied by $\frac{h}{\sqrt{n}}$; as was to be expected. But in case (3) this analogy is deserted. It appears from the preceding expression, that the number of times we

* §§ 58–60.
† 'Least Squares.'
‡ *Cf.* Merriman, 'Least Squares, pp. 27 & 146

shall make an error of ξ in taking the mean as the true value is

$$\frac{\int_0^\infty \mathrm{P}\,dh}{\int_{-\infty}^\infty\int_0^\infty \mathrm{P}\,dh\,d\xi}\,.\,d\xi,$$

of the form

$$\frac{\mathrm{J}}{(\mathrm{S}a_r^2+n\xi^2)^{\frac{n+1}{2}}} \text{ (say } n \text{ even).}$$

The errors committed by taking the mean as the real value are ranged under the above* *sub-exponential* expression. Whence it appears that what may be called the measure of the probable error is the sum of the squares of the apparent errors divided by n, not, as some might seem to imply, by $(n-1)$; although this *modulus*, as I think it may be called with propriety, is not to be multiplied by the usual factor ·476, but by the length of the abscissa which halves the half-area of the curve just indicated.

What is frequently said in favour of the expression $2\,\frac{\mathrm{S}(\xi-x_r)^2}{n-1}$ (where ξ is the mean of the observations) instead of $2\,\frac{\mathrm{S}(\xi-x_r)^2}{n}$ in the present and similar problems, namely, that the latter expression, the sum of squares of apparent errors, is certainly less than the sum of squares of real errors, appears to be true but not pertinent. For what have we to do with the sum of squares of real errors, except as a mark of one or other of the quæsita above proposed?—(1) The law according to which the observations diverged, the modulus of the probability-curve, from the groups constituted by which the observations are regarded as random selections; (2) the errors incurred by taking the mean as the real value It may be observed, too, that the most probable value (as deduced from the observations) of the sum of squares of real errors is the sum of squares of apparent errors; a statement which is quite consistent with the admission that this latter value is certainly (infinitely probably) less than the real value. The case might be compared to the following:—A random selection of an abscissa being made from the group indicated by the curve $y=\frac{1}{\sqrt{\pi}\,k}\epsilon^{-\frac{x^2}{k^2}}$, required the most probable value

* See Phil. Mag. Oct. 1883, p. 306.

of the square of the selected abscissa. It is zero. Yet it is infinitely probable that the real value is greater than zero.

It appears from the subexponential form* incidental to this problem, that if, after taking the mean of a group of observations of the type I. A. 3, we take the mean of a second group of the same type, the mean of those two means (or of any number of such means) is not likely to be as much nearer the true value as might have been expected.

A variant of Prob. (3) is obtained by the distribution of the observations into groups each of the type (3). This variant might be obtained from the variant of Prob. I. Thus, supposing the limits of the groups still given, let our knowledge of the weight corresponding to each group be degraded through all degrees of conjectural knowledge to absolute uncertainty. Now let the barriers which separate the groups be unfixed, and finally become absolutely uncertain, and we shall have reached a fourth case,

I. A 4, in which we do not know the weight of any of the observations; and they may all, for all we know, have different weights. The solution may be reserved till we reach the more general case of I. B. 4.

I. B. Facility-curves which are symmetrical†, but not probability-curves, present similar cases for the exact solution of which it would be necessary to assume as given some particular form, *e. g.* $\frac{1}{\sqrt{\pi}c}\left(\alpha+\frac{2\beta}{c^2}\right)\epsilon^{-\frac{x^2}{c^2}}$, where $\alpha+\beta=1$; unless indeed, as Donkin has observed‡, it were possible by an unimaginable perfection of the calculus of functions to take, as it were, a mean of all admissible functions.

Approximate solutions of cases under this heading are given by the Method of Least Squares, as discovered by Laplace. Concerning these it may be observed that, if the *subexponential* form is an appreciable ingredient of the elemental facility-curves, then the factors assigned by the method of least squares are *not* in general the most advantageous. It is obvious to object that the number of the observations in the case to which the Method of Least Squares is applicable is supposed infinite; and therefore that the principle mentioned in the Postscript of the previous paper will apply. But then infinite is to be interpreted here as very large. Or, rather, not so very large; as we are told in recommendation of this very theorem whose

* See previous paper.

† Some of the following inquiries can only by courtesy be included under the term *Least Squares.*

‡ Phil. Mag. [4] vol. ii. p. 56.

accuracy I impugn. How, then, can we be certain that the effect of a subexponential ingredient is extinguished?

Agreeably to the distinction clearly exhibited by Mr. Glaisher (*op. cit.* p. 102 foot, and p. 103), the method of least squares belongs to B (3) or B (4) according as it is, or is not, given that all the observations have the same weight. The latter case presents the much vexed question, What shall be done with considerable outlying errors? Shall the exceptional observation be omitted from the average, as Peirce* says? Or shall it count for one, as Airy says†? Or shall it count indeed, but not count for one, as says DeMorgan? Upon the hypothesis here entertained DeMorgan's view is undoubtedly correct in theory, though in practice it may not differ from the practice of Peirce. The approximative method is justified in following the analogy of the exact method; which would, according to the inverse method here all along contemplated, include all the data among the premises, though it may be that the conclusion, which is a function of them all, is less affected by (the variation or omission of) some than others. Thus, to take an example transferred here from its proper place (A 4): suppose we have two observations, x_1 and x_2, close together, and a third outlying, given that the generating facility-curves are probability-curves, but nothing further. Then we have to determine x, h_1, h_2, h_3, so that

$$\pi^{-\frac{3}{2}} h_1 h_2 h_3 \, \epsilon^{-[h_1^2(x-x_1)^2+h_2^2(x-x_2)^2+h_3^2(x-x_3)^2]}$$

is a maximum. Whence

$$(1) \quad 1-2h_1^2(x-x_1)^2=0,$$

$$(2) \quad 1-2h_2^2(x-x_2)^2=0,$$

$$(3) \quad 1-2h_3^2(x-x_3)^2=0,$$

$$(4) \quad h_1^2(x-x_1)+h_2^2(x-x_2)+h_3^2(x-x_3)=0.$$

Whence

$$\frac{1}{(x-x_1)}+\frac{1}{(x-x_2)}+\frac{1}{(x-x_3)}=0.$$

Of the roots, that one is to be selected which makes a maximum

$$h_1 h_2 h_3 \times \epsilon^{-h_1^2(x-x_1)^2-h_2^2(x-x_2)-h_3^2(x-x_3)^2};$$

or, as

$$h_1^2=\frac{1}{2(x-x_1)^2},$$

* Astron. Journ. vol. ii. p. 161 (Cambridge, America).

† 'Astronomical Journal,' vol. iv. p. 137.

that which makes a maximum,

$$\frac{1}{(x-x_1)(x-x_2)(x-x_3)}.$$

When two observations are close together, the root of x which lies close to the two observations is evidently the appropriate value. The weight of those two observations is then high; the weight of the third observation, if it lies at a distance from the other two, is small. These considerations may easily be extended to the case of any number of observations given by *probability*-curves. The equation

$$\frac{1}{x-x_1}+\frac{1}{x-x_2}+\&c.+\frac{1}{x-x_n}$$

is easily formed, and admits of approximative treatment for one or two observations remote from the majority; and doubtless in other cases.

In case of laws of error other than probability-curves, the method of course does not afford the *most probable* value. But I submit that the method is still a very *advantageous* method; more advantageous both in respect of accuracy and convenience than the method proposed by DeMorgan and Mr. Glaisher. Our method is at least accurate in one case, and that the very important and typical case of the law of error being a probability-curve; comparable in that respect to Laplace's treatment of the sought value as a *linear function* of the observations; a procedure which leads to the *most probable* value in the case of probability-curves, but is in general only a convenient, an *advantageous*, procedure*. But their method is never (except by chance) accurate, and always inconvenient. As I understand Mr. Glaisher, if three observations of unknown weight are given, say x_1, x_2, x_3, his procedure is first to take the mean $\frac{x_1+x_2+x_3}{3}$; then to form the apparent errors $\frac{x_2+x_3-2x_1}{3}$ &c.; then to form what may be called the provisional weight (for all the observations),

$$h^2=\frac{1}{2\left[\left(\frac{x_2+x_3-2x_1}{3}\right)^2+\&c.\right]}.$$

Then put

$$p_1=\frac{1}{\sqrt{\pi}\sqrt{2}\left[\frac{(x_2+x_3-2x_1)^2}{3}+\&c.\right]}\epsilon^{-h^2\left(\frac{x_2+x_3-2x_1}{3}\right)^2}.$$

* See *ante*, p. 361.

Let p_1^2 be the new provisional weight of the first observation. Then make

$$p_1^2(x-x_1)^2+p_2^2(x-x_2)^2+p_3^2(x-x_3)^2$$

a minimum. Let x be the new (weighted) mean. Calculate a new p_1 &c., and continue the process until you come to a standstill.

It may be questioned whether this is exactly the procedure which DeMorgan had in view. Would not his language apply to the following process? Find, as before, the *primâ facie* value $\frac{x_1+x_2+x_3}{3}$; then calculate the weights, upon the principles herein adopted, on the hypothesis that the putative is the real point; and that the apparent errors are the result of divergence therefrom, according to probability-curves of different moduli. The weights* are

$$h_1^2=\frac{1}{2\left(\frac{x_2+x_3-2x_1}{3}\right)^2}$$

$$\&c. = \qquad \&c.$$

Then make $h_1^2(x-x_1)^2+h_2^2(x-x_2)^2+h_3^2(x-x_3)^2$ a minimum,

$$x=\frac{\frac{x_1}{(x_2+x_3-2x_1)^2}+\frac{x_2}{(x_3+x_1-2x_2)^2}+\&c.}{\frac{1}{(x_2+x_3-2x_2)^2}+\qquad\&c.}$$

Find a new set of weights, and so on.

Neither process leads directly, in the case of probability-curves, to the *most probable* value, which is

$$\textit{The Mean} \pm\sqrt{(x_1^2+x_2^2+x_3^2-(x_1x_2+x_2x_3+x_3x_1)}.$$

Query whether the DeMorgan-Glaisher process would ultimately reach the most probable value. Their method, then, appears to be less advantageous in respect of accuracy than the method here suggested. Their method would perhaps be more advantageous in respect of convenience in some cases where the real weights are nearly equal, and not many approximative steps are required. It must be remembered, however, that in these cases, as our analysis shows, the observations must be close together; and therefore that our method also becomes facilitated in this case. For instance, taking the mean of the observations as origin, neglect powers higher than the second of x_1, x_2, &c. measured from this origin.

* See I. A. (2).

Then the general equation for x becomes

$$nx^{n-1}-(n-1)\Sigma x_r \,.\, x^{n-2}+(n-2)\Sigma x_r x_s x^{n-3},$$

which reduces to a quadratic. And in the general case the coefficients of the equation of the $(n-1)$th degree, consisting of combinations of the observations, might possibly be evaluated by a calculating machine. Whereas the alternative methods do not seem equally to lend themselves to mechanical treatment. It may be questioned, therefore, whether even in respect of *convenience* the DeMorgan-Glaisher method has the advantage.

II. The difficulties thicken as the hypotheses become more rarefied. For unsymmetrical facility-curves the most probable value is not in general, even presumably, coincident with the most advantageous value. The most probable value may, of course, be determined if we assume a particular form of facility-curve, and determine by inverse calculations analogous to those already given the value of the constants. It should be observed that we must here expressly assume, what before was taken for granted, that is, not only what the form of each generating facility-curve is, but also how it is disposed with regard to the real point. Whereas in unsymmetrical curves the longest ordinate, the centre of gravity, the bisection of the area, may all correspond to different abscissæ, which of them, if any of them, corresponds to the real point?

The scruple just raised affects also that approximative method of determining the most advantageous value, which is afforded by Poisson's extension of what may be called Laplace's law of errors; I mean Poisson's proof that, even in the case of unsymmetrical curves, the several values of a linear function of observation, $\dfrac{\gamma_1 x_1+\gamma_2 x_2+\&c.}{\gamma_1+\gamma_2+\&c.}$ (x_1, x_2, &c. being numerous), may be regarded as ranged under a probability-curve, whose centre is the mean of the mean errors of the elemental curves. If now each facility-curve be so disposed about the "real point" (as, waving metaphysical difficulties, I have called it), then indeed the weighted mean* will be the most advantageous value; but not in general. Poisson himself, as I understand him, points out this cause of failure (*Connaissance des Temps*, 1832 "Suite," § 9 beginning).

A fresh difficulty, connected with a fresh quæsitum, now also makes its appearance. We have all along described the method of least squares (taken in a large sense) as an inverse

* Assuming of course that the utility-functions are symmetrical, and that the influence of subexponential elements may be neglected.

process by which we remount from measurements to the measurable, from the plural manifestations of a thing to the thing itself. But now there becomes differentiated another quæsitum which has hitherto been coincident with the above; viz. given n manifestations (observations), what is the (most probable or most advantageous) value of the $(n+1)$th manifestation? For example, if the positions of n shot-marks on a target are reported to me, and it is given that they are all results of firing at a wafer, I can in my study calculate (1) the most probable position at which the shots were fired; (2) (supposing that the firing is renewed after the n observations have been noted) the point most likely to be hit by the $(n+1)$th shot. When the law according to which the shots diverge right or left of the bulls-eye is symmetrical, these quæsita are identical, but not otherwise.

Continually degrading our data concerning the genesis of the observations supplied to us, as hypothesis after hypothesis unwinds, we should come to the very zero of assumption—absolute ignorance as to the genesis of x_1, x_2, &c. In this case many of the preceding problems become unmeaning. But not the least important—what is the most probable (or advantageous) value of the $(n+1)$th observation—may still be asked. An answer, which is probably not altogether valueless, but serviceable as a starting-point for hypotheses, is afforded by the remarkable method explained by Boole in the 'Proceedings of the Royal Society of Edinburgh;' especially if we admit, what Boole does not admit, that the value of a quite unknown constant expressing probability is to be treated as $\frac{1}{2}$; as Donkin* contends. Both Donkin's assumption and those upon which the whole of Boole's new method of probability is grounded are not to be regarded as arbitrary, but as the solid result of experience—that constants do in general *in rerum naturâ* as often present one value as another. This principle is illustrated by the occurrence of one digit as often as another in natural constants (a fact actually verified by Mr. Proctor in the case of logarithms). The principle is verified and shown to be at least a good working hypothesis by the fact that it underlies all the methods of least squares. For they all presuppose that the *à priori* probability of the real quantity, the quæsitum, being, say, between a and $a+\Delta a$, is the same as the *à priori* probability of its being between b and $b+\Delta b$—between formally infinite and practically considerable limits. And this supposition of equal *à priori* probability can have no significance, as Mr. Venn† well

* Phil. Mag. [4] vol. i.

† 'Logic of Chance,' chap. vi.

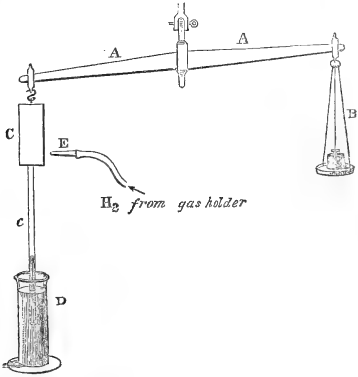
reasons, unless it mean that measurables do, as a matter of fact, as often have one value as another. But, in touching these considerations, we have already passed the frontier which separates mathematical reasoning from general philosophy.

It remains only to add that the preceding problems are immensely complicated when, instead of our single variable x, there are several variables; but that these additional difficulties have been triumphed over by a long line of distinguished mathematicians, from Laplace to Glaisher.

LI. *Description of an Apparatus to illustrate the Production of Work by Diffusion.* By C. J. Woodward, *B.Sc.**

DIFFUSION, as a source of energy, is usually shown in the lecture-room by bringing a jar of hydrogen over a porous vessel fitted up with a glass tube dipping into water. Hydrogen, by inward diffusion, enters the jar; the internal pressure thus produced forces the water down, and a stream of bubbles escapes from the tube. On removing now the jar of hydrogen, outward diffusion of the hydrogen takes place, a minus pressure is produced in the porous vessel, and the water is lifted.

The apparatus I am about to describe is an adaptation of this experiment to the production of an oscillatory movement of a beam from alternate inward and outward diffusion of hydrogen.



The apparatus is represented in the annexed figure. A A is a scale-beam about 3 feet long, carrying at one end a scale-

* Communicated by the Physical Society, having been read at the Meeting on May 12th, 1883.

pan and counterpoise, B, and at the other the porous jar, C, fitted with a cork, and a glass tube *c* which dips into a gas-jar D containing water or methylated spirit. Three or four glass jets, of which one is shown at E, are supported in a horizontal position, and the opening of each jet is placed as near as possible to the porous vessel without touching it during the oscillations of the beam. These jets are connected by means of a flexible tube with a gas-holder containing hydrogen; the bell of the holder being loaded so as to force the hydrogen in a gentle stream against the sides of the porous vessel. The best position for the jets is found by trial; but usually I place them a little below the middle of the porous jar when the beam is horizontal. The action of the apparatus is simple. On turning on the hydrogen, inward diffusion takes place, producing plus pressure within the jar; this pressure is resisted equally in all directions but the vertical, and in this direction, owing to the little friction of the water, movement takes place, and the jar rises. When the jar has risen above the jets, inward diffusion diminishes, or perhaps ceases, while outward diffusion of the hydrogen commences; a minus pressure is thus produced in the porous vessel, and the external pressure of the air causes it to descend. This descent brings the jar again opposite the jets, when the series of movements again begins.

The work done with this arrangement is very small, and falls far short of the theoretical value*. For the best effect the jar should be surrounded by hydrogen for inward diffusion to take place, and subsequently the connexion with the hydrogen should be completely cut off and air take its place. I have tried to devise some water-seal arrangement by which the flow of hydrogen could be turned off and on at the right moments by the movement of the beam; but have not succeeded, as the friction thus introduced would be more than the movement of the beam could overcome. In the arrangement exhibited there is a considerable waste of energy due to the imperfect cut-off of the hydrogen, even when the flow of gas has been regulated so as to obtain the maximum effect.

* "The work that may be done during the mixing of the volumes v_1 and v_2 of two different gases is the same as that which would be gained during the expansion of the first gas from volume v_1 to volume v_1+v_2, together with the work gained during the expansion of the second gas from v_2 to v_1+v_2, the expansion being supposed to be made into vacuum." See a paper by Lord Rayleigh in the Phil. Mag. [4] xlix. p. 311.

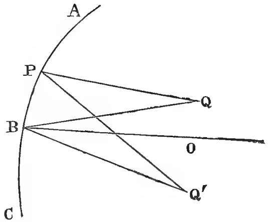
LII. *On Curved Diffraction-gratings.*—II.

By R. T. Glazebrook, *M.A.*, *F.R.S.**

In the September number of the Philosophical Magazine there is a paper by Prof. Rowland on my paper read before the Physical Society bearing the above title. In that paper (Phil. Mag. June 1883) I investigated the aberration of a curved diffraction-grating ruled so that the spaces between the lines are equal when measured along the arc of the curve, and applied my results to Professor Rowland's gratings. Until Professor Rowland's paper (Phil. Mag. Sept. 1883), this assumption had been made explicitly or tacitly in all I have seen written on the subject. In that paper Prof. Rowland mentions (I think for the first time in print) that the spaces are equal, not along the arc, but along the chord of the arc, and therefore that my theory fails to apply to the gratings as he rules them. Owing to absence from home I have not until the present date (October 3) had an opportunity of reading Professor Rowland's paper carefully. I wish now to express my agreement with him so far as this criticism is concerned, and to show the concurrence of results between the two methods of attacking the problem followed by Professor Rowland and myself respectively when the gratings are ruled in the manner he has adopted. Another criticism is, that I have supposed the gratings to be ruled on a cylindrical instead of a spherical surface. To this I would reply (1) that Prof. Rowland does the same himself. "For the particular problem in hand," he says, "we need only work in the plane xy at present." And throughout the paper he works in the plane xy. This is clearly equivalent to treating the surface as cylindrical. And (2) we are both quite right in so doing.

For let Q (fig. 1) be the source of light, A B C one of the lines of the grating, and Q′ a point at which the illumination is required. Let O be the centre of the grating. In the problem we consider Q, O, and Q′ lie in a plane which is at right angles to the lines of the grating, the plane $x\,y$ with Prof. Rowland's notation; let this plane cut

Fig. 1.



* Communicated by the Author.

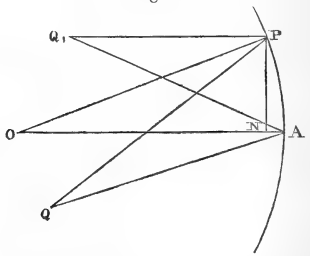
A B C in B. Then, if P be any point in A B C, $QP + PQ'$ is a minimum when P coincides with B; and the portion of the line A B C which is effective in illuminating Q′ is an element near B*. Similar results follow for each of the other reflecting-lines of the grating; and these small elements all lie in the plane Q O Q′. Thus, in considering the diffraction effects, we need only consider a section of the grating by this plane; and this is what has been done both by Professor Rowland and myself.

With regard to the last paragraph of Prof. Rowland's note, I would avail myself of this opportunity of stating that I had no idea that the method referred to was that by which he had arrived at the theory of the concave grating, or that any account of such method had been previously given. It seemed to me instructive, and so I mentioned it. Prof. Rowland's paper is a conclusive proof of its power. I can only express the regret that my absence from the meeting of the Physical Society in November, at which Prof. Rowland's paper was read, has led me to appear to claim a priority which really belongs to him.

In Prof. Rowland's paper he treats of the aberration by considering how much the spaces between two consecutive lines ruled near the edge of the grating differ from theoretical perfection. In mine I determine the difference in phase between the light to the focus coming from the two extreme lines of the grating.

With the notation and figure of my previous paper, Q (fig. 2)

Fig. 2.



is the source of light, Q_1 the focus, O the centre of the sphere,

* Verdet, *Optique physique*, tome i. § 58.

A the centre of the grating; and we have

$$\left.\begin{array}{lll} QAO=\phi, & Q_1AO=\psi, & AOP=\omega; \\ QA=u, & Q_1A=u', & OA=a; \\ \multicolumn{3}{l}{QP^2=u^2+4a^2\sin^2\frac{\omega}{2}-4au\sin\left(\frac{\omega}{2}-\phi\right).} \end{array}\right\} \quad . \quad (1)$$

We will expand in terms of $\sin\omega$ instead of in terms of ω. We find thus, going as far as $\sin^4\omega$,

$$\begin{aligned} QP=&u+au\sin\omega\sin\phi \\ &-\tfrac{1}{2}a\sin^2\omega\cos\phi\left(1-\frac{a}{u}\cos\phi\right) \\ &+\tfrac{1}{2}\frac{a^2}{u}\sin^3\omega\sin\phi\cos\phi\left(1-\frac{a}{u}\cos\phi\right) \\ &+\tfrac{1}{8}a\sin^4\omega\left[\frac{a-u\cos\phi}{u}\left\{1-\frac{a(a-u\cos\phi)}{u^2}\right\}\right. \\ &\left.+\frac{a^2}{u^3}\{6(a-u\cos\phi)\sin^2\phi-5a\sin^4\phi\}\right]. \quad . \quad . \quad (2) \end{aligned}$$

A similar expression can be found for Q_1P.

Thus $QP+Q_1P$

$$\begin{aligned} =&u+u'+a\sin\omega(\sin\phi-\sin\psi) \\ &-\tfrac{1}{2}a\sin^2\omega\left\{\cos\phi\left(1-\frac{a}{u}\cos\phi\right)+\cos\psi\left(1-\frac{a}{u'}\cos\psi\right)\right\} \\ &+\tfrac{1}{2}a\sin^3\omega\left\{\frac{a}{u}\sin\phi\cos\phi\left(1-\frac{a}{u}\cos\phi\right)\right. \\ &\left.-\frac{a}{u'}\sin\psi\cos\psi\left(1-\frac{a}{u'}\cos\psi\right)\right\} \\ &+\tfrac{1}{8}a\sin^4\omega\left[\frac{a-u\cos\phi}{u}\left\{1-\frac{a(a-u\cos\phi)}{u^2}\right\}\right. \\ &+\frac{a^2}{u^3}\{6(a-u\cos\phi)-5a\sin^2\phi\}\sin^2\phi \\ &+\frac{a-u'\cos\psi}{u'}\left\{1-\frac{a(a-u'\cos\psi)}{u'^2}\right\} \\ &\left.+\frac{a^2}{u'^3}\{6(a-u'\cos\psi)-5a\sin^2\psi\}\sin^2\psi\right]. \quad . \quad . \quad . \quad . \quad (3) \end{aligned}$$

But if Q_1 be the focus,

$$QP+Q_1P=u+u'\pm n\lambda.$$

And to the first approximation, u and ϕ being fixed, u' and ψ

are determined by

$$a \sin\omega (\sin\phi - \sin\psi) = \pm n\lambda, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

and

$$\cos\phi\left(1 - \frac{a}{u}\cos\phi\right) + \cos\psi\left(1 - \frac{a}{u'}\cos\psi\right) = 0; \quad . \quad (5)$$

while the aberration of the true focus from that so determined is given by

$$\delta u' = \tfrac{1}{2} a \sin^3\omega \left\{ \frac{a}{u}\sin\phi\cos\phi\left(1 - \frac{a}{u}\cos\phi\right) - \frac{a}{u'}\sin\psi\cos\psi\left(1 - \frac{a}{u'}\cos\psi\right)\right\}, \quad . \quad . \quad (6)$$

neglecting the terms in $\sin^4\omega$. But if we place the source of light on the circle through O which touches the grating in A, then $u = a\cos\phi$. Hence $u' = a\cos\psi$, and the coefficient of $\sin^3\omega$ vanishes; so that we require to consider the term $\sin^4\omega$. Also, if PN be perpendicular to OA, and σ be the distance between two lines measured along NP (this in Prof. Rowland's gratings is constant), since P is on the nth line,

$$n\sigma = \mathrm{PN} = a\sin\omega; \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

and equation (4) becomes

$$\sigma(\sin\phi - \sin\psi) = \pm\lambda, \quad . \quad . \quad . \quad . \quad . \quad (8)$$

which is independent of n; so that the equations (4) can be rigorously satisfied for all values of n, and the aberration involve only terms in $\sin^4\omega$.

If, as I assumed in my former paper, the spaces were equal along the arc, we should have instead of (7) the equation

$$n\sigma = a\omega,$$

which, on substitution in (4) and expansion of $\sin\omega$, would give us a term in ω^3 on which the aberration would depend. Thus in Professor Rowland's gratings the aberration (between the centre and extreme ray) is given by

$$\tfrac{1}{8} a \sin^4\omega (\sin\phi\tan\phi + \sin\psi\tan\psi).$$

We may express this in terms of λ by means of the equation

$$a\sin\omega(\sin\phi - \sin\psi) = \pm n\lambda;$$

and we get for the aberration,

$$\pm\tfrac{1}{8}\frac{n\lambda\sin^3\omega(\sin\phi\tan\phi + \sin\psi\tan\psi)}{\sin\phi - \sin\psi}.$$

If the source of light be the centre so that $\phi = 0$, this difference of phase is

$$\mp\tfrac{1}{8} n\lambda \sin^3\omega \tan\psi.$$

In the second spectrum the difference will be doubled, and so on. The corresponding departure from theoretical perfection in ruling is, according to Prof. Rowland,

$$\tfrac{1}{2}\omega^3 \tan\psi.$$

Thus in the two gratings discussed in my former paper, the extreme difference of phase in the first spectrum will be about $01 \times \lambda \tan\psi$ instead of $\cdot 7\lambda$, and $\cdot 04 \times \lambda \tan\psi$ instead of $4\cdot 8 \times \lambda$. Quantities like this are quite inappreciable; and Professor Rowland may indeed claim that his gratings are perfect so far as this qualification is concerned. It will be interesting to calculate the difference in the position of the extreme line in the grating, supposing that the lines were ruled with equal spaces along the arc instead of along the chord. The difference clearly is that between $a\omega$ and $a \sin\omega$. For if ω' be the angle to the extreme line in this case we have

$$a\omega' = n\sigma = a \sin\omega = a(\omega - \tfrac{1}{6}\omega^3)\,;$$

and difference required is $a(\omega - \omega')$, or

$$\tfrac{1}{6}\, a\omega^3.$$

Taking the grating mentioned by Prof. Rowland in his paper, the value of this is about $\frac{1}{10000}$ of a centimetre, or little more than the distance between two consecutive lines on the grating. The extreme minuteness of the change required to produce so great an alteration in the theoretical definition is very remarkable, and may help to give us some idea of the difficulties Professor Rowland has had to surmount to produce his gratings.

LIII. *On the Magnetic Susceptibility and Retentiveness of Iron and Steel.* *By* J. A. Ewing, *B.Sc., F.R.S.E., Professor of Engineering in University College, Dundee, formerly Professor of Mechanical Engineering and Physics in the University of Tokio**.

DURING three years the writer has been engaged, while in Japan, in prosecuting researches on the magnetization of iron and steel, and on the effects of stress on magnetic susceptibility and thermoelectric quality. Preliminary notices of some of his earlier results have appeared in the 'Proceedings of the Royal Society' (Nos. 214 and 216, 1881, and No. 220, 1882); but a detailed account of the work has still to be given. Meanwhile, the following points, not previously noticed, are perhaps of sufficient interest to justify their separate publication.

* Communicated by the Author, having been read before Section A of the British Association at Southport.

In the experiments on magnetization, iron and steel wires were used, either welded into rings or in the form of straight pieces of such great length that the influence of the ends was negligible. Curves were obtained, in some cases by the ballistic method, and in others by the direct magnetometric method, showing the changes of magnetization which occurred when magnetizing force was gradually applied, withdrawn, reapplied, reversed, and so on.

The results of many experiments with several specimens of carefully annealed soft-iron wires have shown that they possess in very high degree a property not generally credited to soft iron,—the property of remaining strongly magnetic when the magnetizing force is removed.

As an example, the case may be cited of an annealed iron wire which was subjected to a magnetizing force of 22·4 C.G.S. units. This gave it a magnetic Induction amounting to 16,000 C.G.S. units. When the magnetizing force was gradually and completely removed, the Induction fell only to 15,000 units. In other words, the Intensity of residual magnetization was equal to nearly 1200 C.G.S. units.

Here more than 93 per cent. of the whole induced magnetization survived the removal of the magnetizing force; while in many other cases the residual magnetism amounted to nearly 90 per cent. The somewhat extraordinary spectacle was thus presented of a piece of soft iron, entirely free from magnetic influence, and nevertheless holding (per unit of its volume) an amount of magnetism far in excess of what is ever held by permanent magnets of the best tempered steel.

In this condition, however, the magnetic character of the iron is highly unstable. The application of a reverse magnetizing force quickly causes demagnetization; and the slightest mechanical disturbance has a similar effect. Gentle tapping removes the residual magnetism almost completely. Variations of temperature reduce it greatly, and so does any application of stress. On the other hand, if the iron be carefully protected from disturbance, it seems that the residual magnetism disappears only very slowly, if at all, with the mere lapse of time.

If, after magnetization, the magnetizing force be removed suddenly, the residual magnetism is, as might be expected, less than if the force be removed gradually.

The ratio of residual to total magnetization is always small when the intensity of magnetization is small, and passes a maximum when the intensity is increased. This maximum is particularly distinct in wires which have been hardened by stretching; but it also occurs in soft annealed wires. In one instance, where the wire had been hardened by stretching, the maximum ratio of residual to total magnetism was 0·60, which was given by the application of a magnetizing force of about 10 C.G.S. units; but after the application of a force of 90 units the ratio fell to 0·33. In steel the maximum in this ratio is less sharp, but still distinct. Neither in hard iron nor in steel is the ratio, even at its maximum, so great as it is in soft iron, where (as has been said) it frequently reaches 0·9.

During the magnetization of soft-iron wires the greatest ratio (κ) of Intensity of magnetization (I) to magnetizing force was generally about 200, sometimes nearly 300. And by gently tapping the wire during the application of magnetizing force, this coefficient was on one occasion raised to the enormous value of 1590. In the case alluded to the magnetization went on so rapidly as the magnetizing force was increased, that a force of 1 C.G.S. unit gave an Induction of 10,000.

In this and other particulars the experiments have been strongly confirmatory of the idea that there is in soft iron a static frictional resistance to the rotation of the magnetic molecules, which is the principal cause of the remarkable retentiveness described above, and which is overcome by gentle mechanical agitation.

Numerous measurements have been made of the energy expended in taking iron and steel through cyclic changes of magnetization. For example, in changing the magnetism of one specimen of annealed iron wire from $I = 1250$ to $I = -1240$, and back, the amount of work done against magnetic friction (apart from any induction of currents) was 1670 centimetre-dynes per cubic centimetre of the metal. In hardened iron, and especially in steel, the work done is much greater.

The effects of stress on existing magnetism and on magnetic susceptibility have been investigated at great length. The most remarkable effects occur in wires which have been hardened by stretching. In them the presence of a moderate longitudinal tensile stress increases the magnetic susceptibility immensely at low values of the magnetizing force, but diminishes it at high values. It also increases very greatly the ratio of residual to temporary magnetization. Each of these effects passes a maximum when the stress is sufficiently increased.

The whole subject is much complicated by the presence of the peculiar action which in previous papers the writer has named *Hysteresis*, the study of which, in reference both to magnetism and to thermoelectric quality, has formed a large part of his work.

LIV. *On the Distribution of Electricity on Hollow Conductors in Electrolytes.* By ALFRED TRIBE, *F.Inst.C., Lecturer on Chemistry in Dulwich College**.

THE point of view set out in my paper in the 'Proceedings of the Royal Society,' 1876, p. 310, would doubtless enable the general results of an examination into the distribution of electricity on hollow conductors in electrolytes to be foreseen. An experimental treatment of the subject, however, was desirable in order to establish definitely any *à priori* conclusion deduced either from analogy or general theoretical considerations. Experience in this case has verified the conclusion that the closest analogy would be found to exist between the distribution of electricity on hollow conductors in electrolytic and in dielectric fields respectively. Other experiments to be described in this paper show that the analogy extends to cases where a conductor is enclosed in metal tubes and even in tubes of wire gauze. A few associated experiments are also described in this paper, the results of which may assist, when the subject is more developed, in determining between the different views which present themselves, as to the mode by which a metallic conductor becomes electrified when placed in a binary compound undergoing electrolysis, but not in metallic connexion with the electrodes.

In the experiments to be described, an electrolytic cell was employed, 120 millim. broad, 128 deep, and 305 long, filled to within 8 millimetres with a 5-per-cent. solution of copper sulphate, copper electrodes of the same area as the ends of the cell, and a current of one ampere flowing for six miuutes. The tubes or hollow conductors were in all cases placed lengthwise, midway between the electrodes and perpendicular to them, the bottom of the cell, and the surface of the electrolyte.

Experiment.—A silver tube 100 millim. long, having an internal diameter of 10 millim., was placed within the field of action. The electrifications, in millimetres, counting from the opposite ends of the tube, on the outside and inside respectively, and also the respective zones of non-evident electrification (intermedial space), were as below:—

	− electrification.	Intermedial space.	+ electrification.
Outside of tube...	29	13	58
Inside of tube ...	6	74	20

* Communicated by the Author.

In a second experiment a silver plate, 60 × 7 millim., was placed within the limits of no electrochemical action, along the axis of a similar silver tube, but not in metallic connexion with it, and then placed in the electrolyte as before. The silver plate was completely protected from electrification, at least signs of both copper and peroxide were entirely absent from the surface of the plate. The electrifications on the tube itself were practically the same in area as in the first experiment.

In a third experiment the plate of silver was equal in length to the tube, *i. e.* 100 millim. The electrifications on the outside of the tube were again identical in area with those on the corresponding parts of the tube in the first experiment. The − on the inside was also the same, but the + measured 4 millim. less in length. The − electrification on the plate measured 13 millim., and the + 15 millim.

In a fourth experiment the silver plate projected 10 millim. beyond the tube at both ends, *i. e.* the plate was 120 millim. long. The electrifications on the tube itself, inside and outside, were practically equal in area with those in the experiment last described. The areas on the plate, however, were notably different. The limit of the − electrification was very sharply defined, and measured only 11 millim., *i. e.* it penetrated 1 millim. only within the tube. On the other hand the + measured 25 millim., and therefore penetrated 15 millim. within the tube, the same as in the last experiment. The 10 millim. of the plate projecting beyond the ends of the tube appeared then to have exerted no appreciable influence on the capacity of the + electrification to diffuse itself over the surface of the plate within the tube. It was very different, however, with the − electrification; so much so in fact as to almost prevent its diffusion on that part of the plate coming immediately within the shadow, so to speak, of the hollow conductor.

Faraday's well-known experiments on the distribution of an electric charge on nets &c. naturally suggested the idea of substituting tubes of wire gauze for those of metallic silver. Gauze of copper wire was employed having 72 meshes to the square centimetre. The area of the electrifications set up on tubes of this material could not be ascertained with exactitude for the reason that the limits of the opposite electrifications were not so distinctly marked as in the case of the silver tubes; but the distribution of electrochemical action was sufficiently evident to leave no doubt as to the electrifications being mainly confined to the external surfaces of wire gauze, when in the form of tubes, and placed in the field of electrolytic action.

In the annexed table is set out the length, in millimetres, of the electrifications &c. registered on silver plates placed along the axes of tubes of copper gauze of different diameters. The tubes were each 100 millim. long, and the silver plates 120×7 millim. wide, projecting, as in the last experiment with the silver tube, 10 millim. beyond their respective ends. The electrifications &c. on a silver plate of the dimensions given, but without the metallic surroundings, were found to measure 38 millim. —, 15 intermedial space, and 67 +. The numbers in the fifth column express the power exercised by the several tubes in protecting the partially enclosed plates from electrification, and were found by subtracting the intermedial space left on an unprotected plate (15 millim. as given above) from the intermedial space obtained in the several experiments, as given in the third column of the Table.

Diameter of tube.	− electrification.	Intermedial space.	+ electrification.	Protecting-power.
10 millim. ...	7	97	16	82
20 " ...	9	91	20	76
40 " ...	12	83	25	68

It was of course to be foreseen that, as the diameter of the tubes increased the power to protect from electrification the part of the conductor within them would decrease. These results show, moreover, that in the experiment with the smallest tube its negatively-charged end cut off, so to speak, the electrification of the same sign on the plate 3 millim. from the mouth of the tube; and that in the third experiment the — electrification on the plate passed only 2 millim. within a tube of 40 millim. diameter. In connection with this, which may prove to be decisive evidence of a repulsive action, I would point to the different magnitudes of the electrifications (especially the —) on the plate in the fourth experiment with the silver tube (page 385) and on the plate in the experiment with the smallest tube of gauze respectively. In the former of these experiments the — electrification on the plate measured 11 millim., while in the latter the same electrification measured only 7 millim., the only difference in the two experiments being in the material and structure of the tubes.

October 1883.

LV. *On the Reality of Force.* *By* Walter R. Browne, *M.A.*, *M. Inst. C.E., late Fellow of Trinity College, Cambridge**.

THE Royal Society of Edinburgh have lately published the first part of an Essay on the Laws of Motion by Professor Tait, F.R.S.E. This essay is a further development of the views upon Force and upon the proper mode of presenting the principles of Mechanics, which are set forth in the article on Mechanics by the same author in the new edition of the *Encyclopædia Britannica.* Their appearance in such a publication, together with the weight attaching to the name of their author, is sure to give to these views great currency and authority; and I trust therefore it will be considered only just that they should be submitted to careful but fearless criticism.

The main point of difference between Prof. Tait and previous writers on Mechanics is the view which he takes of Force. Force he takes to be a mere expression, an abridged notation for some such words as "the time-rate of change of momentum," having no real or objective existence whatever. Accordingly it should be possible, and is even desirable, to expound the whole of Mechanics without introducing this word at all, and so without giving the student a chance of mistaking it (as he is certainly prone to do) for the symbol of a real existence. In preparing his article for the *Encyclopædia*, however, Prof. Tait found it difficult to make this desirable change; and accordingly that article proceeds on the old lines until it arrives at the last chapter, where the new discovery is set forth and expanded. In his recent paper Prof. Tait proposes to supply this defect, and to give a sort of outline of a new *Principia*, in which the term Force is absent, and replaced by the purely abstract conception which is its only proper signification.

I shall not criticise this first instalment of the work, only remarking that, before studying the laws of motion, the student will apparently have to master such conceptions as those of Potential Energy, Conservation of Energy, Quaternions, Vectors and Scalars, the Principle of Least Action, &c. I am thankful, at least, that I was myself taught Mechanics before its text-books were constructed on the new principle. But the new treatise will not need much discussion if its *raison d'être* (the non-objectivity of Force) is shown to be erroneous; and it is this point to which I wish to address myself.

Turning to the article "Mechanics" in the new *Encyclo-*

* Communicated by the Author.

pædia, which alone supplies the evidence on which this rests, we find, as already mentioned, that the new conception of Force, as something without objective existence, is only hinted at the beginning, and then relegated to an appendix at the end, the whole of the results being developed in the ordinary manner. This appendix of "General Considerations" is therefore the place where we are at last to find the evidence we seek; and here, in fact, we do find it, put in the simplest and clearest form; so that we are at once able to examine and estimate its value. It is all confined to a very few paragraphs (291–295), and may be expressed in the following propositions:—

(1) We believe Matter, whatever it may be, to have an objective existence, chiefly because it is "conserved," *i. e.* because experiment teaches us that its quantity cannot be altered.

(2) The only thing in nature which is also conserved in this sense is Energy.

(3) Therefore Energy is the other objective reality in the physical universe; and we must look to it for information as to the true nature of what we call Force.

(4) Taking as the simplest case the fall of a stone towards the earth, we find the equation

$$\tfrac{1}{2}\mathrm{M}v^2=\mathrm{W}h,$$

which may be interpreted as stating that the kinetic energy acquired is equal to the force acting multiplied by the distance fallen through. But if we introduce the element of time, by means of the relation $h=\frac{vt}{2}$, this equation at once becomes

$$\frac{\mathrm{M}v}{t}=\mathrm{W}.$$

(5) Hence Force appears in a new light. *It is* now the time-rate at which momentum is generated in the falling stone.

(6) But a mere rate, be it a space-rate or a time-rate, is not a thing which has objective existence. No one would confound the bank rate of interest with a sum of money, nor the birth- or death-rate of a country with a group of individual human beings.

(7) Therefore Force, being a rate of generation of momentum, is not an objective reality.

I do not think Prof. Tait can quarrel with this mode of stating the argument, which is mainly in his own words. It is a clear and connected chain of reasoning; and therefore the conclusion may be overthrown by overthrowing any one of the premises. It may be thus overthrown, unless I am mistaken, in more ways than one.

First, we may proceed by attacking Prop. 1. That conservation cannot be the ordinary ground for believing in the objectivity of matter is simply proved by the fact that the mass of mankind have always believed (and, according to Prof. Tait, believed rightly) that matter exists, without having any idea what conservation of matter means: nay, more, while believing that matter is not conserved. And if it be said that it is not a question of what is, but what ought to be, the evidence for our belief, this does not affect my denial. We believe matter has an objective existence, not because it is conserved, but because *it persists*: in other words, because it has effects upon us which are regular, constant, can be re-experienced at will, and have all the other characteristics of an independent object. This proves to us that matter exists now; but it does not even begin to prove that it has always existed, and will always exist. I have not the slightest difficulty in conceiving that the universe may be annihilated to-morrow, though I am sure it exists to-day; even as Prospero did not mean to deny the reality of cloud-capped towers and gorgeous palaces, while asserting that they would one day become as the baseless fabric of a vision.

Secondly, we may challenge Prop. 3. We have only to put it in a general form to see its weakness. It would then run thus:—"We believe a subject of thought X to have a characteristic A chiefly because it has another characteristic B. There is another subject Y which also has the characteristic B: therefore it also, and independently, has the characteristic A." It is clear that this does not hold unless we assume the characteristic B to be always and necessarily implied by A. But obviously this need not be the case. Thus, I may believe Camoens to be a great poet, chiefly because a great many people have considered him as such; but a great many people have considered Mr. Tupper a great poet, and yet I am not logically bound to accept their verdict. But apart from this, there is another flaw in the proposition; for the subject Y, though really having the characteristic A, may be simply another form, or a function of subject X; or, again, both may be functions of a third subject Z, which has the same characteristic. In either case Y is not a separate independent possessor of B. The latter supposition really holds in the case of Energy, as will be seen hereafter.

Thirdly, we have a still more important and obvious fallacy in Proposition 5, which is really the key of the whole. It needs only to be stated in order to become evident. It is the fallacy that because one thing, A, is proportional to and measured by another, B, therefore A is the same as B, and nothing else.

To show that we have here a real instance of this general fallacy, we have only to put side by side Newton's Second Law, as quoted by Prof. Tait himself (' Elements of Nat. Phil.' 1873, p. 66), and the words stated in Proposition 5.

Newton, Law 2. Change of Motion *is proportional to* the impressed force.

Tait, Prop. 5. Force *is* the time-rate at which momentum is generated.

The fundamental difference between Newton and Tait, and the fallacy mentioned above, could not be more clearly illustrated. Nobody, I presume, will assert that Newton meant to identify the two things spoken of. We do not say that the Queen is proportional to the Empress of India, or a triangle proportional to a three-sided rectilineal figure.

The absurdities into which we should fall if we adopted this view generally will be patent to everybody. For instance, we must say that the heating-power of a fuel *is* a certain number of pounds of water evaporated per hour; that the quantity of heat in a body *is* the product of a certain number of pounds of water and a degree on the mercury-scale; and so forth. But we need go no further than the equation given by Prof. Tait himself in Proposition 4. The expression on the left-hand side is the energy acquired, and that on the right-hand side the force multiplied by the distance. According to Prof. Tait, the force is not objective, because the symbol representing it expresses the time-rate at which momentum is generated; while the energy is one of the two objective existences which are beyond the reach of cavil. But if we interpret the expression for the energy in the same fashion, we find that it is a mass (*i. e.* a weight W divided by a velocity g) multiplied by the square of a velocity (or rate of motion), and divided by 2. We are therefore bound to regard as non-objective something which may be expressed as a time-rate of momentum; but we are bound to regard as objective something which may be expressed as a weight multiplied by the square of a rate, and divided by twice another rate. It is difficult to see how this can be supported; or, again, why an argument which is true for one side of an equation may not be applied to the other.

The only other mode in which Prof. Tait proceeds to prove the non-objectivity of Force is a curious one; it proceeds by anecdote rather than argument. After observing that the third Law of Motion tells us that force is always dual, and that to every action there is always an equal and contrary reaction, he goes on thus (art. 289):—" 'Do you mean to tell me,' said a medical man of the old school, 'that if I pull a 'subject' by the hand, it will pull me with an equal and opposite force?' When he was convinced of the truth of this statement, he gave up the objectivity of force at once."

I cannot help thinking that this gentleman was not only a doctor of a very old school, but a very old doctor of any school; for I have in vain endeavoured to discover in this rebellious behaviour on the part of subjects anything which could constitute any reason—physical, metaphysical, logical, or otherwise—for believing or not believing in the objectivity of matter. Why a thing should be real if left to itself, but become unreal and fictitious if it is opposed to something equal to itself, is a puzzle. We may conceive our doctor arriving at very singular conclusions if he carried out the same principle consistently. Thus the celebrated Irishman, who complained that it was not his fall that hurt him, but the stopping so suddenly, might have been told that he was in error: his fall was an objective action, but when it was stopped by the equal and opposite action of the earth it became a mere rhetorical figment. Again, if a gentleman squeezes a lady's hand, that is an objective fact; but if she squeezes his in return, then it becomes merely a subjective impression. This would not interest the doctor, but may be a useful hint to younger practitioners. If it be objected that in these cases the opposition is temporary, while in the case of force it is permanent, I would reply that permanence, *teste* Prof. Tait himself, is a proof of reality rather than the reverse. And I still inquire in what way the existence of two equal and opposite causes proves the unreality of either or both of them.

There is one deduction from the new view, which Prof. Tait makes himself (art. 297), and which deserves notice. He observes that "equivalent quantities must always be expressed by equal numbers when both are measured in terms of the same system of units. It appears therefore, from the conservation of energy directly, that potential energy must, like kinetic energy, be of dimensions $[ML^2T^{-2}]$. Now it is impossible to conceive of a truly dormant form of energy whose magnitude should depend in any way on the unit of time; and we are therefore forced to the conclusion that potential energy, like kinetic energy, depends (in some as yet unexplained, or rather unimagined, way) upon motion. . . The conclusion appears inevitable that, whatever matter may be, the other reality in the physical universe—energy, which is never found unassociated with matter, depends in all its widely varied forms upon motion of matter."

Now I should have thought it an accepted principle in science that if a train of reasoning is found to lead to a conclusion which not only has not been explained, but of which no explanation has been imagined—in other words, which is not only a mere unsupported hypothesis, but as to which no hypothesis can by the wit of man be framed—then that is sufficient reason for concluding, not that the unimaginable is inevitably true, but that the reasoning is inevitably false. It will be a bad day for science when its leaders forget the principle of which Newton was so brilliant an exponent—the principle, namely, of distrusting your conclusions the moment they are shown to be incompatible with ordinary matters of fact. But in the present case the particular flaw in the argument (apart from the general question at issue) is easily seen by an instance. Let us suppose a current of water (it is an ordinary case) running through a fan water-meter, the disk of which it keeps in rotation, and then passing by a pipe into a tank. When this is over, the quantity of water which has come to rest in the tank should be the same as that which has passed through the meter; but this will be indicated by the counter, *i. e.* by the number of revolutions which the disk of the meter has made in the time. Then, following Prof. Tait's reasoning, we should say:—"It is impossible to conceive of a truly stationary mass of water whose magnitude should depend in any way on the number of revolutions of a meter; and therefore we are forced to the conclusion that the water in the tank must really be continually causing the revolution of a meter, though we cannot explain, or even imagine, where the meter can be." To this it would be sufficient to reply that the water was measured, not when it was at rest, but when it was moving; and so we reply that what we have measured is not potential energy directly, but kinetic energy which was being transformed into or from potential energy, as the case may be. It may be well to add that what we have measured does not of course give us the total potential energy existing in the body, any more than the meter would give the total quantity of water in the tank, supposing, for instance, that this happened to be the sea.

That the criticism of this paper may not be negative only, I will indicate another line of attack on Prof. Tait's position, which is of a positive character. We have seen that he recognises two distinct and independent realities as revealed to us in Nature, namely Matter and Energy; and his argument is based on the fact that both of these are subject to the Law of Conservation. But I have elsewhere shown at length* that the whole of the recognised laws of Mechanics, including the Conservation of Energy and Matter, flow directly from the three Laws of Motion (if not from more general principles still), if we take as our definition of Matter that it is a "collection of centres of force distributed in space, and acting upon each other according to laws which do not vary with time, but do vary with distance." I have also shown† that the second principle—the Conservation of Energy—does not hold in any cases where the forces are not of the above character. Hence instead of the four fundamental realities Space, Time, Matter, and Energy, we need only three—Space, Time, and Force; and from these the mechanical universe, as we know it, can be constructed. But it will not be contended that we know anything of Energy as an independent objective reality, except what is revealed to us in the study of Mechanics; in fact, its existence was never even suspected until the modern development of that study had begun. Hence it appears that all the facts forthcoming to prove its independent existence can be perfectly accounted for apart from that hypothesis; and that being so, the evidence in favour of the hypothesis sinks absolutely to zero. But that for which there is no evidence is not to be believed.

I will here conclude this paper, perhaps already too long. If any illustrations used are not of a kind ordinarily adduced in such discussions, it is Prof. Tait's old-school doctor and his "subjects" who must be my excuse. For the paper itself I do not make any excuse, because I am convinced that the new views promulgated by Prof. Tait, and by others, on the foundations of Mechanics are doing very serious harm, especially among those who approach the subject from the practical side. It is not that they are led to inquire more closely into these fundamental principles, and the evidence for them—that would be a useful result; but they are led to think that there is no real ground of truth in any of them—that they are mere convenient working hypotheses, which may be left to contradict and stultify each other just as may happen. When this belief is fully accepted, the era of fruitful progress in Physical Science will be at an end.

* 'The Student's Mechanics (Charles Griffin & Co., 1883).

† Phil. Mag. 1883, p. 35.

LVI. *On the Involution and Evolution of Quaternions.* *By* J. J. Sylvester, *F.R.S.**

THE subject-matter of quaternions is really nothing more nor less than that of substitutions of the second order, such as occur in the familiar theory of quadratic forms. A linear substitution of the second order is in essence identical with a square matrix of the second order, the law of multiplication between one such matrix and another being understood to be the same as that of the composition of one substitution with another, and therefore depending on the order of the factors; but as regards the multiplication of three or more matrices, subject to the same associative law as in ordinary algebraical multiplication.

Every matrix of the second order may be regarded as representing a quaternion, and *vice versâ*: in fact if, using i to denote $\sqrt{-1}$, we write a matrix m of the second order under the form

$$\begin{matrix} a+bi, & c+di, \\ -c+di, & a-bi, \end{matrix}$$

we have by definition,

$$m = a\alpha + b\beta + c\gamma + d\delta,$$

where

$$\alpha = \begin{matrix} 1 & 0 \\ 0 & 1 \end{matrix}, \quad \beta = \begin{matrix} i & 0 \\ 0 & -i \end{matrix}, \quad \gamma = \begin{matrix} 0 & 1 \\ -1 & 0 \end{matrix}, \quad \delta = \begin{matrix} 0 & i \\ i & 0 \end{matrix}.$$

Now

$$\alpha^2 = \alpha, \quad \beta^2 = \gamma^2 = \delta^2 = -\alpha,$$

$$\alpha\beta = \beta\alpha = \beta, \quad \alpha\gamma = \gamma\alpha = \gamma, \quad \alpha\delta = \delta\alpha = \delta,$$

$$\beta\gamma = -\gamma\beta = \alpha, \quad \gamma\delta = -\delta\gamma = \beta, \quad \delta\beta = -\beta\delta = \gamma;$$

so that we may for $\alpha, \beta, \gamma, \delta$, substitute $1, h, k, l$, four symbols subject to the same laws of self-operation and mutual interaction as unity and the three Hamiltonian symbols. Now I have given the universal formula for expressing any given function of a matrix of *any* order as a rational function of that matrix and its latent roots; and consequently the qth power or root of any quadratic matrix, and therefore of any quaternion, is known. As far as I am informed, only the square root of a quaternion has been given in the text-books

* Communicated by the Author.

on quaternions, notably by Hamilton in his Lectures on Quaternions.

The latent roots of m are the roots of the quadratic equation

$$\lambda^2-2a\lambda+a^2+b^2+c^2+d^2=0.$$

The general formula

$$\phi m=\Sigma\phi\lambda_1\frac{(m-\lambda_2)(m-\lambda_3)\ldots(m-\lambda_i)}{(\lambda_1-\lambda_2)(\lambda_1-\lambda_3)\ldots(\lambda_1-\lambda_i)};$$

where i is the order of the matrix m, when $i=2$ and $\phi m=m^{\frac{1}{2}}$, becomes

$$m^{\frac{1}{q}}=\frac{\lambda^{\frac{1}{q}}{}_1-\lambda^{\frac{1}{q}}{}_2}{\lambda_1-\lambda_2}\,m-\frac{\lambda_3\lambda^{\frac{1}{q}}{}_1-\lambda_1\lambda^{\frac{1}{q}}{}_2}{\lambda_1-\lambda_2},$$

where λ_1, λ_2 are the roots of the above equation. If, then, μ is the modulus of the quaternion, viz. is $\sqrt{a^2+b^2+c^2+d^2}$, and $\mu\cos\theta=a$, the latent roots λ_1, λ_2 assume the form

$$\mu(\cos\theta\pm i\sin\theta).$$

When the modulus is zero the two latent roots are equal to one another, and to a the scalar of the quaternion; so that in this case the ordinary theory of vanishing fractions shows that

$$m^{\frac{1}{q}}=a^{\frac{1}{q}}\left(\frac{m}{a}+\frac{q-1}{q}\right).$$

In the general case there are q^2 roots of the qth order to a quaternion. Calling $\frac{\pi}{q}=\omega$, and writing $m^{\frac{1}{q}}=\mathrm{A}m+\mathrm{B}$,

$$\mathrm{A}=\frac{\mu^{\frac{1}{q}}}{\mu}\,\frac{\cos\left(\frac{\theta}{q}+2k\omega\right)+i\sin\left(\frac{\theta}{q}+2k\omega\right)-\cos\left(\frac{\theta}{q}+2k'\omega\right)+i\sin\left(\frac{\theta}{q}+2k'\omega\right)}{2i\sin\theta},$$

$$-\cos\left(\frac{q-1}{q}\theta+2k'\omega\right)+i\sin\left(\frac{q-1}{q}\theta+2k'\omega\right).$$

$$\mathrm{B}=\mu^{\frac{1}{q}}\,\frac{-\cos\left(\frac{q-1}{q}\theta+2k'\omega\right)+i\sin\left(\frac{q-1}{q}\theta+2k'\omega\right)}{2i\sin\theta}$$

For the q system of values $k=k'=1, 2, 3 \ldots q$, the coefficients A and B will be real, for the other q^2-q systems of values imaginary; so that there are q quaternion-proper qth roots of a quaternion-proper in Hamilton's sense, and q^2-q of the sort which, by a most regrettable piece of nomenclature, he terms bi-quaternions. The real or proper-quaternion values of $m^{\frac{1}{q}}$ are

$$\frac{\mu^{\frac{1}{q}}}{\sin\theta}\left\{\sin\frac{(\theta+2k\omega)}{q}\frac{m}{\mu}+\sin\frac{(q-1)(\theta+2k\omega)}{q}\right\},$$

$\mu^{\frac{1}{q}}$ meaning *the* or (when there is an alternative) *either* real value of the qth root of the modulus.

In the qth root (or power) of a quaternion m, the form $\mathrm{A}m+\mathrm{B}$ shows that the vector-part remains constant to an ordinary algebraical factor *près*; and we know *à priori* from the geometrical point of view that this ought to be the case. When the vector disappears a porism starts into being; and besides the values of the roots given by the general formula, there are others involving arbitrary parameters. Babbage's famous investigation of the form of the homographic function of $\frac{px+q}{rx+s}$ of x, which has a periodicity of any given degree q, is in fact (surprising as such a statement would have appeared to Babbage and Hamilton) one and the same thing as to find the qth root of unity under the form of a quaternion!

It is but justice to the eminent President of the British Association to draw attention to the fact that the substance of the results here set forth (although arrived at from an independent and more elevated order of ideas) may be regarded as a statement (reduced to the explicit and most simple form) of results capable of being extracted from his memoir on the Theory of Matrices, Phil. Trans. vol. cxlviii. (1858) (*vide* pp. 32–34, arts. 44–49).

LVII. *Intelligence and Miscellaneous Articles.*

INFLUENCE OF MAGNETISM UPON THERMAL CONDUCTIVITY. BY JOHN TROWBRIDGE AND CHARLES BINGHAM PENROSE.

THE following experiments were made in order to test Maggi's results * in regard to the effect of magnetism upon the thermal conductivity of iron. Maggi's conclusions have never been confirmed, and have been much doubted by other observers. The experiments of Sir W. Thomson †, in which he found that longitudinal magnetization diminished, while transverse magnetization increased, the electrical conductivity of iron, afford—from the fact that electrical and thermal conductivities are in general proportional —the chief confirmation of Maggi's results. The experiments of Thomson have, however, been questioned.

In the method employed by Maggi, a circular plate of soft iron was placed horizontally upon the poles of a vertical horseshoe magnet. Through the centre of the plate passed a lead tube conveying steam. The surface of the plate was covered with a mixture of oil and wax. When the magnet was made, the melted wax was bounded by an ellipse. If the conductivity had been equal in all directions, it would have been bounded by a circle. The long axis of the ellipse was perpendicular to the line joining the two poles; the short axis was parallel to the line. The ratio of the axes was 6 : 5. The two poles were separated from the iron plate by paper. In order to compensate for the direct effect of the poles upon the flow of heat, two bars of soft iron were placed symmetrically beneath the plate, at the extremities of the diameter, perpendicular to the line joining the poles. No effort seems to have been made to avoid the effect due to the frictional generation of heat in the magnetic coil. Several other complicating causes are apparent in the arrangement which Maggi used.

A year ago we made some experiments by a rather rough method, to find the effect of magnetism upon thermal conductivity, and obtained decidedly negative results. The same results have been obtained in the present experiments, though a much more sensitive arrangement was employed. The following method was used:—A bar of soft Norway iron, 95 cm. long, 1·3 cm. wide, and 0·2 cm. in thickness, was placed horizontally through the sides of a wooden box 6 cm. wide and 25 cm. high. The top and one side of the box were removed. At 17 cm. from each end of the bar was soldered a thick German-silver wire. Each projecting arm of the bar was enclosed in a glass tube 1·4 cm. in diameter. The ends of the tubes were closed with cotton. The ends of the iron bar projected slightly beyond the ends of the tubes, and were exposed to the air of the room. One arm of the bar was placed between the poles of a large electro-magnet, with its flat surface perpendicular to the axis of the magnet. The axes of the poles were in the same

* *Bibl. Univ. Archiv.* 1850.
† Phil. Trans. vol. cxlvi.

horizontal line, perpendicular to the bar. The tube between the magnetic poles was wrapped in a piece of thick asbestos cloth, in order to avoid complications arising from the generation of heat in the magnetic coils. The distance between the poles was about 2·5 cm. A Bunsen lamp was placed in the wooden box, and was so regulated as; o maintain the iron above it at a very dull cherry-red heat. The German silver wires were connected with the wires of a reflecting-galvanometer of six ohms-resistance; the connections were separated by paper bound together and covered with cloth. The lamp was always lighted from four to five hours before any observations were made. It was found that by this time the apparatus had pratically reached a condition of thermal equilibrium. At first the current from a battery of ten Grove cells was used; afterwards a battery of twenty-six very large Bunsen cells was used.

After the lamp had been burning for four hours, there was always a permanent deflection of the galvanometer of about 12 cm. When the current was now passed through the magnets, this deflection was immediately changed permanently. The change was found to be due to the direct action of the magnet upon the galvanometer-needle, though the distance between the two pieces of apparatus was about 10 metres. This deflection amounted to 2·8 cm. when the stronger current was used. Thirty minutes after the magnet was made, the galvanometer spot was always found to have changed by about 3 cm. The direction of the change was such as to show that the junction on the magnetized arm was becoming warmer. It was at first thought that this confirmed Maggi's results.

The apparatus was next arranged with one arm of the iron parallel to the axis of the magnet. The arm was passed through the hollow core of an electro-magnet somewhat stronger than the preceding. The same battery was used. The details of the experiment were exactly the same as when the bar was perpendicular to the lines of force. The results of severval observations here also showed that the junction on the magnetized arm became hotter under magnetization.

We had previously assumed that the heat developed in the magnetic coil would be too slight to affect the iron bar. This assumption was now proved to be incorrect, by placing the unheated bar, arranged exactly as in the preceding experiments, in the magnetic field. When the magnet was made, the galvanometer-needle began slowly to move, always in the direction showing a heating of the junction on the magnetized arm. This deflection was slightly larger, after the same space of time, than the deflections observed in the previous experiments, and consequently rendered the results of these experiments useless.

In order to avoid complications arising from the heat generated by the electric current, the following arrangement was adopted:—About 17 cm. from one end, the iron bar was bent upon itself. At the end of the bent part, at the point of the bar opposite this end, were soldered two German-silver wires. These two thermo-electric junctions were about 0·4 cm. apart, and were separated from each other by densely packed asbestos. The arm was placed in a glass tube arranged as before. The bar was heated about 19 cm. from the thermo-electric junctions. By this arrangement the heating of the magnetic coils had an equal effect upon the two junctions; while any change, due to altered conductivity, of the flow of heat along the bar would affect the relative temperatures of the junctions. When the German-silver wires were connected as before with the galvanometer, and the unheated bar was placed either parallel or perpendicular to the axis of the magnet, there was, after forty-five minutes' observation, absolutely no deflection in the galvanometer. This showed that the arrangement obviated all difficulties arising from heating the coil; and moreover that the magnetic field did not perceptibly alter the thermo-electric relation of iron and German-silver. When the bar was placed perpendicular to the axis of the magnet, thin plates of soft iron, running the length of the glass tube, were placed upon the magnetic poles, thus lengthening the field through which the bar passed. The bar was next heated as before. After several hours the galvanometer generally showed a permanent deflection of 35–40 cm., in a direction indicating that the junction at the end of the bar was the cooler. Several observations were made, both when the bar was parallel and perpendicular to the axis of the magnet. The current was passed for about one hour; and in every case there was absolutely no change of the deflection beyond the immediate change due to the direct action of the magnet.

The result of these experiments seems conclusively to show that longitudinal and transverse magnetism, at least of the strength used, have no influence upon thermal conductivity of soft iron. It was, however, decided to try a thinner piece of iron than the preceding. A strip of ordinary tinned iron was therefore cut about 1·3 cm. wide, and was bent over and arranged exactly as before. The whole tube was packed with asbestos and cotton to avoid any motion of the strip when the magnet was made. The distance of the flame from the two thermo-electric junctions was 10 cm. A heating of ninety minutes was found to be sufficient in this case for the strip to reach a permanent condition of temperature. The deflection showing the difference of temperature between the junctions was about 13 cm. When the magnet was made, there was no change of the deflection after thirty-five minutes' observation.

The strength of the magnetic field when the bar was placed perpendicular to the lines of force was measured after the preceding experiments were made, and was found to be 10,420 times the horizontal intensity of the earth's magnetism at Cambridge. In the C.G.S. system this would be about 1760.

Aside from the experiments of Maggi, those of Thomson upon electrical conductivity are the only experiments that seem to be directly opposed to the conclusion that must be drawn from our observations. Magnetism undoubtedly changes several physical properties of iron, but though this renders it probable, *a priori*, that the thermal conductivity might be changed, yet it does not necessitate such a change. The thermo-electric relation of iron is changed by magnetization; but the thermo-electric relation appears to be unconnected with thermal conductivity.—*Proceedings of the American Academy of Arts and Sciences*, May 29th, 1883.

ON A NEW METHOD OF INSULATING METAL WIRES EMPLOYED IN TELEGRAPHY AND TELEPHONY. BY C. WIEDEMANN.

Having had occasion during the past year to employ, for the decoration of jewellery and other objects of art, the processes described by Nobili and Becquerel for producing various colorations by means of baths of alkaline plumbates and ferrates, I observed that the objects thus coloured had become absolutely resistant of all galvanic action, that their surfaces once coated with the peroxide of lead or iron were no longer capable of conducting the electric current. A copper or brass wire, or even one of iron, became thus covered with an insulating layer analogous to that of a coating of resin or of gutta percha.

It appears to me that this process may prove of considerable use in the manufacture of cables or wires employed in telegraphy and telephony. It is easily effected and its cost is small. The coating, which resists in a very high degree all atmospheric influences, is a guarantee of its duration. The insulation is absolute. The mode of preparation is extremely simple: it suffices to prepare a bath of plumbate of potash by dissolving 10 grms. of litharge in a litre of water to which has been added 200 grams of caustic potash, and to boil for about half an hour; it is allowed to subside and is then decanted, when the bath is ready for use. The metal wire to be coated with the peroxide of lead is attached to the *positive* wire, and a small platinum anode at the negative pole immersed in the bath; finely divided metallic lead is precipitated at the negative pole, and the peroxide of lead is deposited on the metallic wire, which passes successively through all the colours of the spectrum. The insulation is only perfect when the wire has attained the final tint, which is a blackish brown.

Thus coated the wire is perfectly insensible to all electric action; perfectly cleansed objects may be attached to it and connected with the negative pole in a gold, silver, or nickel bath without the current, however powerful, having the slightest action on the objects to be coated with metal. A wire thus treated, placed in a current and brought in contact with another wire connected with a galvanometer, has not the slightest effect upon the latter; there is no diminution in the first current, which passes through the wire coated with the peroxide of lead.

This perfect insulation may possibly be turned to account by electricians in the construction of various apparatus.—*Comptes Rendus*, Oct. 15, 1883.

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LVIII. *On Sun-spots and Terrestrial Elements in the Sun.*

By G. D. LIVEING, *M.A., F.R.S., Professor of Chemistry, and* JAMES DEWAR, *M.A., F.R.S., Jacksonian Professor, in the University of Cambridge**.

[Plate VI.]

THE publication of spectroscopic observations upon sun-spots made at Greenwich places within our reach a gradually increasing body of facts bearing on solar chemistry; and though we have not made a special study of sun-spots, yet the points of contact between the appearances presented in sun-spots and those observed by us in the spectra of terrestrial substances are so many, that we think some discussion of them will be of interest.

The spectroscopic appearances of sun-spots in general point, as all, we believe, are agreed, to the conclusion that in a spot we are looking through an unusual depth of the solar atmosphere charged with metallic and other vapours: that for some reason there is at a spot a depression in the general contour of the photospheric clouds, so that the light which reaches us from the spot has come to us from a greater depth in the sun and been filtered through a greater thickness of absorbent gaseous matter. The depression in the photospheric cloud may be the result of some of the violent atmospheric motions which are frequent in the sun, moving the clouds about and causing a downrush at the spot; or it may be the result of an ascending current of vapour at a temperature higher than the

* Communicated by the Authors.

mean temperature of the photosphere, which converts the clouds into the gaseous condition, as sunshine dissipates the aqueous clouds in our atmosphere. If the spot is due only to a downrush of the upper atmosphere of the sun, we should be merely looking through a greater quantity of the outer atmosphere, increased in density and in temperature but consisting mainly of the same materials as before, and driving the photosphere, the chief source of light, downwards. We should expect to see the ordinary absorption-lines strengthened, and perhaps some new lines, due to a higher temperature, developed. For we must remember that a vapour is capable of absorbing the same kind of radiation as it emits; and that as its emission-spectrum varies with change of circumstances, of which temperature is one, its absorption will vary too. If the clouds of the photosphere were not merely mechanically depressed, but partly vaporized at the spot, we should expect to see new absorption-lines; not only on account of the higher temperature of the vapours previously existing as such, but because matter which had before been cloud, and emitted a continuous spectrum, had now become gaseous, with a discontinuous spectrum.

And here we would say a word about an *à priori* objection to the supposition that a spot may be a region of a temperature generally equal to, or even higher than, that of the photosphere at the same level. Messrs. De La Rue, Stewart, and Loewy, in their 'Researches on Solar Physics' (1st ser. p. 31), have argued, very cautiously and in a questioning manner, from the known laws of radiation and absorption, that we must be looking through a stratum of atmosphere at the spot cooler than the photosphere, because from such a thickness of matter as the sun we must at all points get the total radiation due to the temperature. We do not at all question the general principle of this argument, but we question its application in this case, because the total radiation is not the same thing as the luminosity. There may be more radiation on the whole from the darker spot than from the brighter photosphere, if the excess be in the ultra-violet or the infra-red. It is a general rule with solids that the radiations of short wave-length increase with the temperature more rapidly than those of longer wave-length; and some of the terrestrial elements which are abundant in the sun, such as iron and magnesium, have an emission-spectrum which appears to be much stronger in the ultra-violet than in the visible region. The substance we call iron, whether it be one element or many, emits in the electric arc ultra-violet rays which are extraordinary both in number and intensity. We are not in a position to test whether the total radiation from a spot is as great as from an equal portion of the photosphere, because all the radiation more refrangible than the solar line U is cut off before it reaches us by some absorbent medium. There is, however, one argument which makes strongly for the supposition that the spot is a region of a temperature generally not lower than that of the rest of the photosphere, which is this: If we suppose the photosphere to be of the nature of clouds formed by condensation of metallic and other vapours in the sun's atmosphere, then the gaseous part of that atmosphere will be saturated with the vapour of the matters forming the cloud, and any depression of its temperature must cause a fresh formation of cloud, which would tend to destroy the character of a spot. Indeed an apparent dissolution of photospheric matter carried into spots has frequently been observed, according to Secchi, and seems to suggest an evaporation of the cloudy matter in the spot.

Dismissing, then, the *à priori* argument for the coolness of the spot in comparison with the photosphere as insufficient, and likely to encumber us in the interpretation of the observed facts, let us come to the recorded phenomena.

There is, first, the widening of the Fraunhofer lines. This is observed in all spots, but the lines are by no means all widened or all equally widened. The diagram (Pl. VI.) represents the lines in two spots (taken as samples) observed at Greenwich (Obs. 1881) to be widened. The range observed is from F to *b*, and the length of the lines drawn under the corresponding lines in Ångstrom's map indicates the amount of widening, except where the widening is excessive, when the actual breadth is given as it appeared. As to the precise cause of the widening of spectral lines, physicists are not agreed; but as a fact we know that increased density of the emitting vapour is directly or indirectly a cause of the widening of the spectral lines of terrestrial elements; and we may reasonably attribute the widening in the spots to the increased thickness of the solar atmosphere, denser of course in the lower part, through which the light comes to us. But, then, why are not all the Fraunhofer lines widened? and why are those that are widened so unequally widened? Whatever the cause be, similar phenomena are observed in the emission-spectra of terrestrial elements. In the first place, the lines of some metals are much more readily expanded than those of others. The ready expansibility of the lines of hydrogen and of the D lines of sodium is well known; while the lines of iron and titanium are much less easily expanded, but are nevertheless very sensibly enlarged when the electric arc is freely supplied with the metal. But even so some lines expand more than others. This is well seen in the case of magnesium, which has one ultra-violet line (w.-l. 2852) which is far more easily expanded than any other of its lines. And for a large number of metals, when heated in the arc, we have observed that lines, or groups of lines, repeat themselves at intervals, and that of such series the alternate members are far more readily expanded than the others. This is the case with the doublets of the visible spectrum of sodium—which are alternately diffuse and sharp, the diffuse lines being more easily expansible—and with the triplets of the magnesium, calcium, and zinc spectra. In more complicated spectra it is very probable that the same thing occurs, but, from overlapping of the groups, the alternations are not so easily detected. At any rate, they exhibit some lines which are more easily expanded than others.

It is only necessary to watch with a spectroscope of sufficient dispersive power the spectrum of the arc while iron and other metals are fed into it, in order to see the great expansion of some lines and the comparative inexpansibility of others. Any theory which is to account satisfactorily for the expansion of lines must account for the difference in the amount of expansion of the different lines of the same substance, as well as for the fact, noticed in several cases, that lines do not all expand symmetrically (some spread out more on one side than on the other), and also for absorption-lines expanding as well as the emission-lines, and for the fact that the tension of the vapour has more to do with the expansion of the lines than its temperature has. We do not know enough about the mechanical nature of the collisions, as they are called, between the particles of a gas, whereby the motion of translation due to temperature is supposed to be converted into vibratory motions producing radiation, to say whether in a gas of high density the frequency of the collisions between similar vibrating particles may not affect the period of some of their vibrations much more than that of others.

When we come to examine what lines are usually seen expanded in sun-spots, we see that a large number are those of iron; and besides these the lines of magnesium, calcium, barium, sodium, titanium, and nickel are frequently enlarged. In fact the greater part of the lines of all these elements are more or less enlarged in most of the spots observed at Greenwich in 1881. The hydrogen lines are sometimes enlarged, sometimes not. There may be, probably are, great variations of pressure in the region of a spot; and as it is not very likely that the material of the photospheric cloud contains hydrogen, the tension of the hydrogen in the solar atmosphere will not be increased by the evaporation of the cloud. Hence the variations in the density of the hydrogen will be chiefly due to the currents; and the same remark will apply to all those substances which do not ordinarily exist in the condition of saturated vapour in the sun's atmosphere.

But not only are lines sometimes widened and sometimes not widened in spots, but sometimes lines usually seen as dark lines disappear, or appear as bright lines. This has been satisfactorily explained as the effect of ascending currents bearing vapours into the upper regions at such a high temperature that their emission is equal to or exceeds their absorption. In fact it is just those lines which have been observed as bright lines above the sun's limb in solar storms which are absent or are reversed in spots. There is still the question why some lines of such elements as iron and calcium should belong to the category of lines seen extending to considerable heights in the solar atmosphere while others do not so extend. We are inclined to the opinion that this appearance of certain lines at high elevations to the exclusion of others is dependent more on the tension of the vapour than on its temperature. Of course there is a relation between the tension of a vapour and its temperature, and this relation is by no means the same for saturated as it is for unsaturated vapour; and there may be unsaturated low-tension vapour at a very high temperature when but little of the material of the vapour is present, as well as saturated low-tension vapour at a low temperature. Now the distinction of long and short lines in the electric discharge, first introduced by Thalén, to which Mr. Lockyer has since drawn more particular attention, corresponds, as he has noted, with difference in the density of the vapour; the short lines being seen only near the poles or in the central portion of the discharge, while the long lines extend further. We know but little about the temperatures of different parts of an electric discharge; but in this case temperature and density of vapour may very well go together. It is otherwise when one element is present in very small quantity, as in the alloys on which Mr. Lockyer has experimented. He found that in alloys containing only a very small percentage of one metal, it was the long lines of that metal which were persistent in the spectrum. This seems conclusive that long lines are the lines of vapour of low tension rather than of low temperature; the lines corresponding to the vibrations which the particles take up most readily when they are in the most unfettered state, in their least complicated aggregations. The short lines will on this supposition correspond to vibrations either of more condensed particles, or to vibrations induced by the constrained condition of a dense gas. This view is in harmony with the fact that only the long lines usually appear in the higher regions of the sun's atmosphere where the temperature is high enough to make the hydrogen luminous, but where the pressure, if we may judge by the width of the lines, is certainly very low.

But besides the disappearance of some lines in some spots, new lines and bands frequently make their appearance. Now of the Fraunhofer lines only a fraction have as yet been identified as corresponding to lines of terrestrial elements; but we are inclined to the opinion that this is mainly due to the very imperfect examination which has as yet been made of the spectra of terrestrial substances. At any rate, we have found that it is only necessary to examine with high dispersive power any small section of the spectrum of the arc while different chemicals are dropped into it, in order to see a vast number of new lines develop themselves which have been hitherto unrecorded. The lowest horizon of the diagram shows the results of a somewhat hasty examination of the lines developed in this way by titanium and cerium, and in a few places by other substances. The titanium lines are permanent, but many of the cerium lines are evanescent: they come out strongly when fresh cerium is introduced, sometimes as broad bands, and quickly vanish as the cerium is dissipated, a few lines only remaining. The lines which remain are the lines of vapour of low tension, or long lines, while the others we suppose to be lines of vapour of high tension. It seems very probable, then, that if a careful examination were made of all the lines developed in the arc by all the known substances, most, if not all, of the Fraunhofer lines now ascribed to unknown substances would be accounted for. It is not a little remarkable that several of the most striking developments of lines observed at Greenwich in sun-spots closely correspond to new lines which we have observed to be given in the arc by cerium or titanium. In the case of cerium, the correspondence is so marked that it can hardly be accidental. Of course it is always easy to account for lines in the sun by the supposition of unknown elements brought up from the interior, or of unknown combinations or decompositions occurring under circumstances which we have not learned to imitate; but that is to cut the knot, not to untie it. When we consider the influence which circumstances besides temperature have on the vibrations which a given substance can assume, the comparatively partial way in which the spectra of our chemicals have been examined, the tendency of recent observations such as the artificial production of auroras with the characteristic auroral line at no very great elevation in our atmosphere, and the identification by Egeroff of the groups A and B with the absorption of oxygen, we shall hesitate to say that there is ground for believing that the sun contains any thing which is not to be found on earth.

After all, there are very few of the lines developed in spots which do not appear at least as faint Fraunhofer lines in the ordinary solar spectrum. The maps of Fievez and Vogel give many such lines which are not in Ångström's map. Whenever lines of two metals are coincident, or nearly so, there is of course a double chance of increased width; and this corresponds with the observations. Further, it is noticed that the expansion of some lines is unsymmetrical—they expand more on one side than on the other. This may be an effect of the upward or downward motion of that portion of the vapour which is under observation; but it may also be due to the development of a second line close to that ordinarily seen. Such an effect we have observed in the case of the blue line of lithium, which appears to expand with increased tension of its vapour more on one side than on the other; but the effect is really due to the development of a second line close to the first and the expansion of both lines. This second line is seen as a narrow line as the metal is dissipated, and then disappears entirely long before the usual blue line shows any sign of failing. This will, we believe, satisfactorily account for some of the apparent motions of lines. Where a displacement, by reason of rapid motion of the sun's atmosphere, of some of the lines occurs without any displacement of others, we must suppose that the displaced lines are produced by one layer of the sun's atmosphere, while the undisplaced are due to another. This will account for the long lines, or lines of vapour of low tension, being displaced, while those due to vapour of high tension are unmoved, and *vice versâ*. There is, however, one line which appears to be very anomalous in its behaviour, and requires further notice, because Mr. Lockyer has more than once called special attention to it. This is the line with wave-length 4923. There is an iron line with this wave-length; but in iron it is certainly not a long line: it is not seen in the arc when there is only a very little iron present; and it comes out with increasing density of vapour. Corresponding with this character, it is frequently seen strengthened in spots like the other iron lines. On the other hand, a line of this wave-length is one of those most frequently (40 per cent.) observed by Young as a bright line high up in the solar atmosphere; and it has as frequently been observed by Tacchini in the spectrum of prominences. It should therefore be a long line, or one of vapour of low tension, if judged by this character; and this is confirmed by the fact recorded by Mr. Lockyer that it is frequently absent from spots, which indicates that the emission in the upper regions balances the absorption. Moreover this line has (Proc. R. S. xxxi. p. 349) been observed to be undisplaced in spots when the iron lines near it are displaced, showing that the substance producing it is at a different level from that producing the displaced lines. This is consistent with its existing in the upper regions to which only low-tension vapour of iron can reach. But as the line is certainly not a line of low-tension vapour of iron on earth, we are not justified in assuming it to be such in the sun; and we are driven to suppose it due to some unknown substance X other than iron which at low tension has a line coincident, or nearly so, with a line of high-tension vapour of iron. Mr. Lockyer thinks that the substance X is a constituent of terrestrial iron; but as the behaviour of the line in the sun does not correspond to the behaviour of the iron line on earth, the connexion between the two is reduced to the probable equality of wave-length. We suppose the tension of vapours in the upper part of the sun's atmosphere to be low as a general rule; but it may at times be locally higher, for solid matters falling into the sun and vaporized in its atmosphere may produce vapour of considerable tension until it has had time to diffuse. The descent of dust from the region of the corona may thus produce lines at elevations where we should not otherwise expect to find them.

LIX. *On some Improved Laboratory Appliances for conducting many Chemical Operations at the same time, and hastening the completion of several of them. By* Robert Galloway, *M.R.I.A., F.C.S., and* Francis J. O'Farrell, *M.R.I.A., F.C.S.**

[Plate VII.]

It is very generally conceded that it requires a greater expenditure of time and labour to arrive at results in Chemistry than it requires to attain like ends in any other of the inductive sciences. Any improvement therefore in expediting and rendering less laborious analytical and other chemical operations, which are only a means to an end, assists materially in aiding the progress of the science. The improvement the late Baron Liebig effected in quantitative organic analysis is a remarkable instance of this; for if the improvement had been made before Chevreul commenced his examination into the constitution of fats, he would have been able to have shortened the time he gave (fourteen years) to this investigation by one half, viz. seven years. Beautiful as an

* Communicated by the Authors.

analytical process as Bunsen's method is for the determination of nitrogen in organic substances, no chemist would adopt it in cases where Will and Varentrapp's less beautiful, but easier and speedier method, could be employed; for, as already stated, analytical operations are only a means to an end, and the speedier, with accuracy, chemists can arrive at results, the more conducive it must be to the progress of the science.

At the present time a great deal of labour is involved, and time wasted, by having to set up in a laboratory, where a variety of work is going on, so many distinct pieces of apparatus, each requiring to be started separately. For obtaining the indispensable distilled water, a special still and heating-apparatus is generally set apart for the purpose. Distillation under diminished pressure is another distinct, and frequently troublesome, operation. If a filter-pump is employed to hasten filtration, the pump is solely devoted to that purpose. Then there are the open and closed water-baths, each distinct and requiring a separate heating-apparatus; and for heating and drying substances above 100° C. the methods are most inconvenient. But of all the slow methods, that of evaporating by means of the ordinary air-pump and absorbing the water as it evaporates by means of sulphuric acid is, we think, the slowest. The appliances that are in use generally for forcing steam, air, or gases over substances are, to say the least, inconvenient.

By the aid of a small general air-pump connected to a little stationary engine, and ordinary or superheated steam, all these operations can be carried on at the same time with these appliances excepting the one last described; for this operation there is required, in addition to the air- or suction-pump, a compression-pump in connexion with the engine.

In carrying out some researches recently we found great convenience and saving of labour and time in employing a general air-pump. This has led us to devise appliances for the extension of our plan to a larger number of chemical operations, which we will now describe, as we think the arrangement will be found very useful not only in educational laboratories, but also in pharmaceutical laboratories, especially in the preparation of delicate organic compounds.

The boiler A (Plate VII. fig. 1) we recommend is Cochran's patent multitubular vertical boiler with internal fire-box and horizontal tubes, because it is the boiler, in our opinion, which gives the maximum heating-surface for the space occupied: a boiler 2 feet in diameter and 2 feet 6 inches high will afford, for example, sufficient steam to work a one-horse-power engine. No brick setting or brick chimney is required; and the top can be removed, so that the interior of the boiler can be cleaned when required. It has the usual fittings, such as the water-gauge, the pressure-gauge, and the safety-valve. By the arrangement shown in the Plate, distilled water can be obtained from it; and a steam-pipe (not shown in the Plate) could also be connected with it for the purpose of supplying steam to an open air-bath, this bath not being connected, as those shown in the Plate are, with the air-pump.

The engine, B, is of the donkey-pump type, with one inverted cylinder whose rod acts directly on the air-pump C. D is the ordinary vessel for the exhaust steam to escape into after having performed work in the engine; it is from there conveyed, if required, directly, or after it has been superheated, or a mixture of the two (normal and superheated steam), into the steam-jackets of the vessels E and F. If it is not required to heat these vessels, it is carried away through the pipe *a* on opening the stopcock *b* into the escape-pipe or chimney *c*.

G represents a filtering-apparatus in position; and H a retort and receiver for carrying on distillations under diminished pressure.

The downward pipes marked *d* from the vessels E and F, and the longer pipe *e*, in which they terminate, form together the suction-pipe in connexion with the different pieces of apparatus and with the air-pump; it terminates, as shown in the Plate, in the air-tight box I, in which terminates also the pipe *f* from the air-pump. This box is provided with an overflow-pipe for carrying off the excess of water which accumulates from the aqueous vapour conveyed from the vessels E, F, G, and H by the suction-pipe. A vacuum-gauge *g* may be attached to the box I; it would of course indicate the vacuum throughout the entire system; but if it was considered desirable to ascertain more accurately the vacuum in any one or all of the different pieces of apparatus, a separate mercurial gauge would be required in connexion with the particular apparatus, as will be presently noticed.

Little further description is required as regards the apparatus G and H; we have not considered it necessary to illustrate a condenser in connexion with the retort and receiver. The filtering-apparatus shown is one for large non-quantitative operations: if the filtrate is required, it will be seen from the drawing that portions of it can be drawn off from time to time by means of the tap for evaporation or for other purposes, whilst the filtration or washing is taking place, without interfering with the vacuum. It need scarcely be observed that quantitative filtrations can be as conveniently carried on; and that many filtrations and distillations are capable, by this system, of being carried on at the same time.

E consists of a vessel considerably deeper than an ordinary evaporating-dish; the one we employed was made of copper plated with silver; but the metal dish might be enamelled; or the dish might be made of the same material as ordinary evaporating dishes are made of; it is fitted steam-tight into a metal steam-jacket, and around it is a rim of sufficient breadth for a bell-jar to rest upon. On this rim and also on that of the vessel F an india-rubber washer or collar is placed, on which the bell-jar is fixed. The end of the suction-pipe which terminates a little above the vessel and the end of the one in connexion with the bath F we prefer to have closed, and to have the tube pierced all round just below the end with small holes whose united area is equal to the area of the tube.

On adjusting the bell-jar and setting the air-pump to work a vacuum is speedily produced; and by conveying either the exhaust steam, or the steam superheated, or a mixture of the exhaust and superheated, into the steam-jacket, we are able to obtain different degrees of temperature. In this vessel therefore liquids can be speedily concentrated or evaporated to dryness, and organic liquids of higher boiling-points than water can be speedily and safely dehydrated. We have also found it extremely convenient and useful for bringing about chemical combinations which are not easily brought about under ordinary conditions.

This evaporating-dish, whatever may be its form, not being movable, as it is fixed to the steam-jacket, cannot be used for the evaporation of liquids for quantitative purposes; the vessel F is therefore designed to carry on these latter operations, as well as for drying and dehydrating solid substances.

F is a steam-tight jacket, the upper surface having dish-shaped indentations of different sizes for seating evaporating-dishes in. As this vessel is intended to evaporate liquids in movable dishes *in vacuo* with or without steam heat, and also for dehydrating solid substances intended for analysis &c., it cannot of course be employed for evaporating liquids and drying solids at the same time; and therefore where much work has to be done, a similar vessel, but with a flat upper surface, would have to be employed for drying the solid bodies. It will be seen from the Plate that, by a simple piece of machinery, a thermometer is so held that it can be introduced into any of the dishes in which evaporation is to take place. F is provided, like E, with a rim on which the bell-jar is fixed.

At the bottom of the steam-jackets E and F a stopcock is attached for carrying off the water which is formed by condensation when the steam is first turned on; the steam, as shown by the Plate, is carried away from each of the vessels by the pipes *h* into the escape-pipe or chimney *c*.

The cocks figured in the Plate show how the exhaust-steam, or the steam after it has been superheated by passing through the superheater K, or a mixture of the two, can be conveyed into, or shut off from, the steam-jackets of the vessels E and F.

In order not to have to stop the engine and air-pump when it is desired to disconnect any one vessel or apparatus with the air-pump, a stopcock is attached to each branch of the general suction-pipe *e*, and above these stopcocks are attached the special mercurial gauges if desired. These stopcocks also enable one to moderate the suction-action on any of the vessels.

Fig. 2 (Plate VII.) represents the combination of the air-pump and compression-pump; they can be worked together, or either of them can be thrown out of action by removing the pin which connects it with the rod of the steam-engine.

We think that in the future the chemical action of bodies on one another under increased and diminished pressures will be more studied than has yet been done; and we hope and expect the appliances we have described, as they will be always ready for use, will greatly conduce to the study of chemical action under either or both of these conditions; and therefore it will be seen that we expect that these mechanical arrangements will have a much wider application than we have outlined.

Our friend Mr. J. G. Douglas, B.A., who has kindly made the drawings for us, has also most kindly given us an approximate estimate of the cost of the apparatus; he thinks the whole of the apparatus, excluding the table, would not exceed £50. It will be admitted, we think, that this is by no means costly when all the different pieces of apparatus, such as stills, air-pumps, &c., that will be dispensed with are taken into account; even leaving out of consideration altogether the saving of time and labour which will be effected by the adoption of these new laboratory appliances.

LX. *The Dilatation of Crystals on Change of Temperature.*

By L. Fletcher, *M.A., of the Mineral Department, British Museum; late Fellow of University College, Oxford.*

[Concluded from p. 350.]

Third Part.

We now proceed to the consideration of the case of an Anorthic crystal; and the method we shall adopt is one identical in character with that indicated above for an Oblique crystal.

At the first temperature let the crystallographic axes have directions OA, OB, OC, and lengths A, B, C respectively; at the second temperature these axes will have taken up new positions OA′, OB′, OC′, and new lengths A′, B′, C′. As at the second temperature the positions of the crystal-lines relative to each other are independent of their absolute position in space, let the crystal be rotated from its position at the second temperature until the axis OC′ coincides in direction with OC and the axial plane OA′C′ with the axial plane OAC: OA′ will not coincide with OA nor OB′ with OB.

It will be convenient first to calculate in terms of the above quantities the alterations in magnitude and direction of a certain triad of perpendicular lines: one, OZ, coincident with OC; a second, OX, lying in the axial plane OAC; and the third, OY, perpendicular to this plane.

Denote the axial angles BOC, COA, AOB respectively by α, β, γ, and the angle ACB by C.

I. Let $\lambda_1\mu_1\nu_1$, $\lambda_2\mu_2\nu_2$, $\lambda_3\mu_3\nu_3$ be the direction-cosines of OA, OB, OC relative to the rectangular axes OX, OY, OZ (fig. 9). Then

$\lambda_1 = \cos AX = \sin\beta$	$\lambda_2 = \cos BX = \sin BC \cos ACB = \sin\alpha\cos C$	$\lambda_3 = \cos CX = 0$
$\mu_1 = \cos AY = 0$	$\mu_2 = \cos BY = \sin BC \sin ACB = \sin\alpha\sin C$	$\mu_3 = \cos CY = 0$
$\nu_1 = \cos AZ = \cos\beta$	$\nu_2 = \cos BZ = \cos\alpha$	$\nu_3 = \cos CZ = 1$

II. Let x, y, z be the coordinates of a point P referred to the rectangular axes OX, OY, OZ, and x_1, y_1, z_1 the coordinates of the same point referred to the oblique axes OA, OB, OC. Then

$$x = \lambda_1 x_1 + \lambda_2 y_1 + \lambda_3 z_1,$$

$$y = \mu_1 x_1 + \mu_2 y_1 + \mu_3 z_1,$$

$$z = \nu_1 x_1 + \nu_2 y_1 + \nu_3 z_1.$$

Substituting the above values of λ_1, μ_1, ν_1, &c., we find

$$x = x_1 \sin\beta + y_1 \sin\alpha\cos C,$$

$$y = y_1 \sin\alpha\sin C,$$

$$z = x_1\cos\beta + y_1\cos\alpha + z_1.$$

III. Find as a particular case of the above the coordinates $(x_1\, y_1\, z_1)$ of each of the points X, Y referred to the oblique axes OA, OB, OC.

For X, if $OX = s$,

$$x = s, \quad y = 0, \quad z = 0;$$

whence

$$x_1 = \frac{s}{\sin\beta},$$

$$y_1 = 0,$$

$$z_1 = -s\cot\beta.$$

For Y, if $OY = t$,

$$x=0, \quad y=t, \quad z=0\,;$$

whence

$$x_1 = -\frac{t\cot C}{\sin\beta},$$

$$y_1 = \frac{t}{\sin\alpha\sin C},$$

$$z_1 = t\left(\cot\beta\cot C - \frac{\cot\alpha}{\sin C}\right).$$

IV. Find the coordinates $(x_1'\, y_1'\, z_1')$ of each of the points X′, Y′ at the second temperature relative to the oblique axes OA′, OB′, OC′.

For X′,

$$x_1' = \frac{A'}{A}x_1 = \frac{A'}{A}\frac{s}{\sin\beta},$$

$$y_1' = 0,$$

$$z_1' = \frac{C'}{C}z_1 = -\frac{C'}{C}s\cot\beta.$$

For Y′,

$$x_1' = \frac{A'}{A}x_1 = -\frac{A'}{A}\frac{t\cot C}{\sin\beta},$$

$$y_1' = \frac{B'}{B}y_1 = \frac{B'}{B}\frac{t}{\sin\alpha\sin C},$$

$$z_1' = \frac{C'}{C}z_1 = \frac{C'}{C}t\left(\cot\beta\cot C - \frac{\cot\alpha}{\sin C}\right).$$

V. Find the coordinates x', y', z' of each of the same points X′, Y′ relative to the rectangular axes OX, OY, OZ.

In just the same way as in § **II.** we find that

$$x' = x_1'\sin\beta' + y_1'\sin\alpha'\cos C',$$

$$y' = y_1'\sin\alpha'\sin C',$$

$$z' = x_1'\cos\beta' + y_1'\cos\alpha' + z_1'.$$

Substituting in these equations the values of x_1', y_1', z_1' deduced in § **IV.**, we find

For X′,

$$\frac{x'}{s}=\frac{\mathrm{A}'}{\mathrm{A}}\frac{\sin\beta'}{\sin\beta},$$

$$y'=0,$$

$$\frac{z'}{s}=\frac{\mathrm{A}'}{\mathrm{A}}\frac{\cos\beta'}{\sin\beta}-\frac{\mathrm{C}'}{\mathrm{C}}\frac{\cos\beta}{\sin\beta};$$

and for Y′,

$$\frac{x'}{t}=-\frac{\mathrm{A}'}{\mathrm{A}}\frac{\sin\beta'}{\sin\beta}\frac{\cos\mathrm{C}}{\sin\mathrm{C}}+\frac{\mathrm{B}'}{\mathrm{B}}\frac{\sin\alpha'}{\sin\alpha}\frac{\cos\mathrm{C}'}{\sin\mathrm{C}},$$

$$\frac{y'}{t}=\frac{\mathrm{B}'}{\mathrm{B}}\frac{\sin\alpha'\sin\mathrm{C}'}{\sin\alpha\sin\mathrm{C}},$$

$$\frac{z'}{t}=-\frac{\mathrm{A}'}{\mathrm{A}}\frac{\cos\beta'\cos\mathrm{C}}{\sin\beta\sin\mathrm{C}}+\frac{\mathrm{C}'}{\mathrm{C}}\frac{\cos\beta\cos\mathrm{C}}{\sin\beta\sin\mathrm{C}}$$

$$+\frac{\mathrm{B}'\cos\alpha'}{\mathrm{B}\sin\alpha\sin\mathrm{C}}-\frac{\mathrm{C}'}{\mathrm{C}}\frac{\cos\alpha}{\sin\alpha\sin\mathrm{C}}.$$

VI. Hence P, the coefficient of expansion of the crystal-line OX, is

$$\frac{x'}{s}-1=\frac{\mathrm{A}'}{\mathrm{A}}\frac{\sin\beta'}{\sin\beta}-1;$$

Q, the coefficient of expansion of the crystal-line OY, is

$$\frac{y'}{t}-1=\frac{\mathrm{B}'}{\mathrm{B}}\frac{\sin\alpha'\sin\mathrm{C}'}{\sin\alpha\sin\mathrm{C}}-1;$$

and R, the coefficient of expansion of the crystal-line OZ, is

$$\frac{\mathrm{C}'}{\mathrm{C}}-1.$$

Also (fig. 10), if

$$\mathrm{D}=\frac{\pi}{2}-\mathrm{Y'OZ},\quad \mathrm{E}=\mathrm{X'OX},\quad \mathrm{F}=\frac{\pi}{2}-\mathrm{Y'OX},$$

$$\mathrm{D}=\cos\mathrm{ZOY'}=\frac{z'}{t}=-\frac{\mathrm{A}'}{\mathrm{A}}\frac{\cos\beta'\cos\mathrm{C}}{\sin\beta\sin\mathrm{C}}+\frac{\mathrm{C}'}{\mathrm{C}}\frac{\cos\beta\cos\mathrm{C}}{\sin\beta\sin\mathrm{C}}$$

$$+\frac{\mathrm{B}'}{\mathrm{B}}\frac{\cos\alpha'}{\sin\alpha\sin\mathrm{C}}-\frac{\mathrm{C}'}{\mathrm{C}}\frac{\cos\alpha}{\sin\alpha\sin\mathrm{C}},$$

$$\mathrm{E}=\mathrm{XOX'}\quad=\frac{z'}{s}=\quad\frac{\mathrm{A}'}{\mathrm{A}}\frac{\cos\beta'}{\sin\beta}-\frac{\mathrm{C}'}{\mathrm{C}}\frac{\cos\beta}{\sin\beta},$$

$$\mathrm{F}=\cos\mathrm{XOY'}=\frac{x'}{t}=-\frac{\mathrm{A}'}{\mathrm{A}}\frac{\sin\beta'\cos\mathrm{C}}{\sin\beta\sin\mathrm{C}}+\frac{\mathrm{B}'}{\mathrm{B}}\frac{\sin\alpha'\cos\mathrm{C}'}{\sin\alpha\sin\mathrm{C}}.$$

VII. As was pointed out above (p. 347), angular measurement of a crystal can result in the determination of only the ratios A : B : C and A′ : B′ : C′, and not of the absolute lengths of the axes. If the parametral ratios are, as usual, given in the form $a : 1 : c$ and $a' : 1' : c'$, and $\frac{B'}{B}=1+\lambda$, we shall have, as before,

$$\frac{A'}{A}=(1+\lambda)\frac{a'}{a}=\frac{a'}{a}+\lambda,$$

$$\frac{C'}{C}=\frac{c'}{c}+\lambda.$$

Substituting these values of $\frac{A'}{A}, \frac{B'}{B}, \frac{C'}{C}$ in the above expressions for P, Q, R, D, E, F, and still neglecting small quantities of the second order, it will be found that

$$\begin{aligned} P&=p+\lambda, \qquad & D&=d, \\ Q&=q+\lambda, & E&=e, \\ R&=r+\lambda, & F&=f, \end{aligned}$$

where

$$\left\{\begin{aligned} p&=\frac{a'}{a}\frac{\sin\beta'}{\sin\beta}-1, \quad q=\frac{\sin\alpha'\sin C'}{\sin\alpha\sin C}-1, \quad r=\frac{c'}{c}-1, \\ d&=-\frac{a'}{a}\frac{\cos\beta'\cos C}{\sin\beta\sin C}+\frac{c'}{c}\frac{\cos\beta\cos C}{\sin\beta\sin C}+\frac{\cos\alpha'}{\sin\alpha\sin C}-\frac{c'}{c}\frac{\cos\alpha}{\sin\alpha\sin C}, \\ e&=\frac{a'}{a}\frac{\cos\beta'}{\sin\beta}-\frac{c'}{c}\frac{\cos\beta}{\sin\beta'}, \\ f&=-\frac{a'}{a}\frac{\sin\beta'\cos C}{\sin\beta\sin C}+\frac{\sin\alpha'\cos C'}{\sin\alpha\sin C}. \end{aligned}\right.$$

We have now determined the expansions and rotations of three crystal-lines which are initially perpendicular in terms of the coefficient of expansion λ of a line in the direction OB and of the parameters of the crystal at the two temperatures.

VIII. Let $\lambda_1\,\mu_1\,\nu_1$, $\lambda_2\,\mu_2\,\nu_2$, $\lambda_3\,\mu_3\,\nu_3$ be the direction-cosines of the lines OX′, OY′, OZ′, referred to the three rectangular axes OX, OY, OZ; OX′, OY′, OZ′ being the positions at the second temperature of the crystal-lines which coincide with OX, OY, OZ at the first. Then, neglecting squares of small quantities,

$$\begin{aligned} &\text{for OX}', \qquad \lambda_1=1, \qquad && \mu_1=0, \quad \nu_1=E=e; \\ &\text{for OY}', \qquad \lambda_2=F=f, && \mu_2=1, \quad \nu_2=D=d; \\ &\text{for OZ}', \qquad \lambda_3=0, && \mu_3=0, \quad \nu_3=1. \end{aligned}$$

IX. Let the coordinates of any point P at the first temperature referred to OX, OY, OZ be x, y, z; the coordinates x', y', z' of P′, the position of P at the second temperature, referred to the three oblique axes OX′, OY′, OZ′, will be

$$(1+p+\lambda)x, \quad (1+q+\lambda)y, \quad (1+r+\lambda)z.$$

But, from the formulæ stated in § **II.**, if ξ, η, ζ be the coordinates of P′ referred to the rectangular axes OX, OY, OZ, we have

$$\begin{aligned}\xi&=\lambda_1 x'+\lambda_2 y'+\lambda_3 z',\\ \eta&=\mu_1 x'+\mu_2 y'+\mu_3 z',\\ \zeta&=\nu_1 x'+\nu_2 y'+\nu_3 z'.\end{aligned}$$

Substituting in these formulæ the above values of x', y', z', λ_1, μ_1, ν_1, &c., we get (neglecting squares of small quantities)

$$\left\{\begin{aligned}\xi&=(1+p+\lambda)x+f(1+q+\lambda)y=x+(p+\lambda)x+fy,\\ \eta&=(1+q+\lambda)y \qquad\qquad\qquad\quad =y+(q+\lambda)y,\\ \zeta&=e(1+p+\lambda)x+d(1+q+\lambda)y+(1+r+\lambda)z=z+ex+dy\\ &\qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad +(r+\lambda)z;\end{aligned}\right.$$

and, reversing the formulæ,

$$\begin{aligned}x&=\xi-(p+\lambda)\xi-f\eta,\\ y&=\eta-(q+\lambda)\eta,\\ z&=\zeta-e\xi-d\eta-(r+\lambda)\zeta.\end{aligned}$$

X. If all the points P lie on a sphere of unit radius having the equation $x^2+y^2+z^2=1$, all the points P′ will lie on an ellipsoid having the following equation:—

$$\begin{aligned}[\xi^2-2(p+\lambda)\xi^2-2f\eta\xi]+[\eta^2-2(q+\lambda)\eta^2]&\\ +[\zeta^2-2e\xi\zeta-2d\eta\zeta-2(r+\lambda)\zeta^2]&=1,\end{aligned}$$

or

$$(1-2\lambda)[\xi^2+\eta^2+\zeta^2]-2[p\xi^2+q\eta^2+r\zeta^2+d\eta\zeta+e\zeta\xi+f\xi\eta]=1.$$

The thermic axes, being the axes of this ellipsoid, will have the same direction as the axes of the quadric

$$px^2+qy^2+rz^2+dyz+ezx+fxy=1,$$

all the coefficients of which are known in terms of the parameters, and have, as shown above, extremely simple physical representations; p, q, r being respectively differences of the expansions of the lines OX, OY, OZ from the expansion along OB, d the angle of displacement of OY′ from its initial plane XOY, f the angle of displacement of the same line from its initial plane YOZ, and e the angle of displacement of the line OX′ from its initial position OX.

We may here conveniently point out that the above proves the truth of the remark made on page 279—that if squares of small quantities be neglected, the equations for the determination of the thermic axes will be identical whether the lower or the higher temperature be the starting-point. In fact, if we proceed from the higher to the lower temperature, each one of the coefficients p, q, r, d, e, f retains its numerical value but changes its sign, and the same quadric can be used for the determination of the axes. But as this quadric always gives the positions of the axes for the final temperature, it will give in this case the position, not at the higher, but at the lower temperature.

XI. Whatever be the value of k, the principal axes of the series of quadrics

$$k(x^2+y^2+z^2)+px^2+qy^2+rz^2+dyz+ezx+fxy=1$$

have the same directions; we may thus diminish the labour of numerical calculation by making $k+r$ vanish, and finding the axes of the coaxal quadric

$$(p-r)x^2+(q-r)y^2+dyz+ezx+fxy=1.$$

By comparison of numerical results the equations given by Neumann for the determination of the thermic axes are found to refer to this particular quadric.

XII. Without λ being known, the relative angular displacement of a given line OP can be determined.

From the above equations (§ **IX.**) we can write

$$\frac{\xi}{\eta}=\frac{x(1+p+\lambda)+fy}{y(1+q+\lambda)}=\frac{x}{y}(1+p-q)+f,$$

and

$$\frac{\zeta}{\eta}=\frac{z(1+r+\lambda)+ex+dy}{y(1+q+\lambda)}=\frac{z}{y}(1+r-q)+e\frac{x}{y}+d,$$

from which $\cos P'X=\dfrac{\xi}{\sqrt{\xi^2+\eta^2+\zeta^2}}$ &c. can be calculated.

When y is zero or very small, we must use the corresponding equations wherein y does not appear as a denominator; namely,

$$\frac{\zeta}{\xi}=\frac{z(1+r+\lambda)+ex+dy}{x(1+p+\lambda)+fy}=\frac{\frac{z}{x}(1+r-p)+d\frac{y}{x}+e}{1+f\frac{y}{x}}$$

$$=\frac{z}{x}(1+r-p)-f\frac{y}{x}\frac{z}{x}+d\frac{y}{x}+e,$$

and

$$\frac{\eta}{\xi}=\frac{y(1+q+\lambda)}{x(1+p+\lambda)+fy}=\frac{\frac{y}{x}(1+q-p)}{1+f\frac{y}{x}}=\frac{y}{x}(1+q-p)-f\frac{y^2}{x^2}.$$

XIII. To find the position at the first temperature of a line for which the position at the second temperature is given, the above formulæ must be reversed. We then get, still neglecting small quantities of the second order,

$$\frac{x}{y}=(1-p+q)\frac{\xi}{\eta}-f,$$

$$\frac{z}{y}=(1-r+q)\frac{\zeta}{\eta}-e\frac{\xi}{\eta}-d;$$

and (for use when η is small),

$$\frac{z}{x}=(1-r+p)\frac{\zeta}{\xi}+f\frac{\zeta}{\xi}\frac{\eta}{\xi}-d\frac{\eta}{\xi}-e,$$

$$\frac{y}{x}=(1-q+p)\frac{\eta}{\xi}+\frac{f\eta^2}{\xi^2}.$$

XIV. The equation to the ellipsoid at the second temperature being

$$(1-2\lambda)(\xi^2+\eta^2+\zeta^2)-2(p\xi^2+q\eta^2+r\zeta^2+d\eta\zeta+e\zeta\xi+f\xi\eta)=1,$$

it follows that the radius vector OP′($=\rho$) having direction-cosines l, m, n is given by the equation

$$\frac{1}{\rho^2}=1-2\lambda-2(pl^2+qm^2+rn^2+dm+enl+flm),$$

whence

$$\rho-1=\lambda+pl^2+qm^2+rn^2+dmn+enl+flm.$$

The absolute coefficient of expansion for the crystal-line having the direction $l\,m\,n$ is therefore given by the right-hand side of this equation, in which λ represents the absolute coefficient of expansion of the crystal-line OB.

If the absolute coefficient of expansion of a given line of the crystal be determined by any method, the unknown quantity λ in the above formula can be eliminated.

XV. We now proceed to apply this method to the calculation of the thermic axes of anorthite.

The following Table gives the parameters, as calculated by Beckenkamp from the observed angles, and also the logarithms of the various terms required in the calculation of p, q, r, d, e, f.

$t=20°$ C.		$t-80°$ C.		$t=140°$ C.		$t=200°$ C.	
$a=$	0·635319	a'	0·635499	a''	0·635689	a'''	0·635949
$c=$	0·550427	c'	0·550380	c''	0·550425	c'''	0·550445
$\alpha=$	93° 8′ 6″·6	α'	93° 7′ 58″·6	α''	93° 7′ 53″·2	α'''	93° 7′ 51″·6
$\beta-$	115° 53′ 8″·4	β'	115° 52′ 0″·1	β''	115° 50′ 32″·9	β'''	115 48′ 6″·4
$\gamma=$	91° 15′ 17″·1	γ'	91° 15′ 36″·6	γ''	91° 15′ 56″·1	γ'''	91° 17′ 25″·1
C=	92° 55′ 15″·0	C′	92° 55′ 27″·4	C″	92° 55′ 39″·5	C‴	92° 57′ 6″·2
Logarithms.							
a	$\bar{1}$·8029918·2	a'	$\bar{1}$·8031149·1	a''	$\bar{1}$·8032447·1	a'''	$\bar{1}$·8034223·1
c	$\bar{1}$·7406997·3	c'	$\bar{1}$·7406626·0	c''	$\bar{1}$·7406981·5	c'''	$\bar{1}$·7407139·5
$\sin\alpha$	9·9993495·1	$\sin\alpha'$	9·9993504·5	$\sin\alpha''$	9·9993510·5	$\sin\alpha'''$	9·9993512·2
$\cos(\pi-\alpha)$	8·7379212·0	$\cos(\pi-\alpha')$	8·7376136·3	$\cos(\pi-\alpha'')$	8·7374058·4	$\cos(\pi-\alpha''')$	8·7373442·7
$\sin\beta$	9·9540818·3	$\sin\beta'$	9·9541516·0	$\sin\beta''$	9·9542405·4	$\sin\beta'''$	9·9543897·7
$\cos(\pi-\beta)$	9·6400605·6	$\cos(\pi-\beta')$	9·6397641·3	$\cos(\pi-\beta'')$	9·6393852·1	$\cos(\pi-\beta''')$	9·6387478·0
$\sin C$	9·9994354·5	$\sin C'$	9·9994340·9	$\sin C''$	9·9994328·5	$\sin C'''$	9·9994234·2
$\cos(\pi-C)$	8·7071960·2	$\cos(\pi-C')$	8·7077074·2	$\cos(\pi-C'')$	8·7082058·8	$\cos(\pi-C''')$	8·7117607·4

The values of p, q, r, d, e, f deduced from these by the formulæ of § **VII.** are as follows:—

20°—80° C.	20°—140° C.	20°—200° C.
$p=+0{\cdot}0004442$	$+0{\cdot}0009482$	$+0{\cdot}0017017$
$q=-0{\cdot}0000010$	$-0{\cdot}0000025$	$-0{\cdot}0000238$
$r=-0{\cdot}0000855$	$-0{\cdot}0000037$	$+0{\cdot}0000327$
$d=+0{\cdot}0000419$	$+0{\cdot}0000888$	$+0{\cdot}0001257$
$e=+0{\cdot}0001522$	$+0{\cdot}0004701$	$+0{\cdot}0010007$
$f=-0{\cdot}0000376$	$-0{\cdot}0000706$	$-0{\cdot}0004525$

XVI. We may in the first place remark that the expansions p, q, r and the angles of rotation d, e, f are far from proportional to the changes of temperature; f in fact is greater at 140° C. than at 80° C. and 200° C.

The thermic axes for the different pairs of temperatures are the axes of the following quadrics (§ **XI.**):—

$$\begin{array}{ll} 20°\text{—}\ 80° & 530x^2+84y^2+\ 42yz+\ 152zx-\ 38xy=1, \\ 20°\text{—}140° & 952x^2+y^2\quad+\ 89yz+\ 470zx-\ 71xy=1, \\ 20°\text{—}200° & 1669x^2-57y^2+126yz+1001zx-452xy=1. \end{array}$$

It will be found that the values of Λ_1' &c. used by Beckenkamp (p. 278) correspond to the following quadrics:—

$$\begin{array}{ll} 20°\text{—}\ 80° & 529x^2+84y^2+\ 42yz+\ 152zx-\ 37xy=1, \\ 20°\text{—}140° & 952x^2+y^2\quad+\ 88yz+\ 467zx-\ 70xy=1, \\ 20°\text{—}200° & 1668x^2-57y^2+126yz+1000zx-451xy=1; \end{array}$$

and these are virtually the same as the equations just given.

XVII. Owing to this identity of the equations as calculated by these different methods, it was at first intended to assume the accuracy of the remainder of Beckenkamp's calculations of the axes of the 20°—200° C. quadric, and to calculate therefrom the variation in the angles between these lines at 80° C. and 140° C. It was found, however, that from some cause or other the angles between the lines given by him are not quite right angles; and in fact it will be found that if L‴, M‴, N‴ be the positions at 200° C. of the thermic axes for the pair of temperatures 20°—200° C., the logarithms of cos L‴X, cos L‴Y, &c. given by Beckenkamp lead to the angles $90°-16''{\cdot}87$,

$90-78''\cdot01$, $90^\circ+73''\cdot86$ for L‴M‴, M‴N‴, N‴L‴ respectively instead of 90°.

As the variations of the angles between these same lines at 80° C. and 140° C. was expected to be only a few seconds, it was necessary to find out to what the above differences are due, and for this it was practically the more convenient course to recalculate the axes of the quadric; the conclusion being that the lines given by Beckenkamp, though not quite at right angles, are close approximations to the thermic axes, differing only therefrom by small angles, probably due to the fact that, as the roots of the cubic equation are only approximate, the axial equations are, on the substitution of each of the values of r, only approximately consistent with each other. Indeed, there is considerable difficulty in obtaining the final angles accurate to seconds with the aid of seven-figure logarithms. As, however, the quadric itself is only obtained by neglecting squares of small quantities, such precision is really unnecessary, and it will be quite sufficient for our purpose to calculate the positions of three lines exactly at right angles to each other, and as near as possible to the lines just determined. For the convenience of future verification, we give the more important numbers obtained in this calculation.

XVIII. The equation to the 20°—200° C. quadric being

$$1669x^2-57y^2+126yz+1001zx-452xy=1,$$

the directions of the principal axes are to be determined in the usual way from the following equations:—

$$\begin{aligned}
&(1)\quad 3338x-452y+1001z=rx,\\
&(2)\quad -452x-114y+126z=ry,\\
&(3)\quad 1001x+126y=rz;
\end{aligned}$$

in which, since the equations are simultaneously consistent, r must be a root of the following cubic

$$\begin{vmatrix} 3338-r & -452 & 1001 \\ -452 & -114-r & 126 \\ 1001 & 126 & -r \end{vmatrix}=0,$$

or

$$r^3-3224r^2-1602713r+52783878=0.$$

The roots of this equation, to the first place of decimals, are

$$+3658\cdot2 \qquad +31\cdot0 \qquad -465\cdot2\,;$$

each of these roots corresponds to one of the thermic axes which are denoted respectively as L‴, M‴, N‴. As it was found that these roots are not sufficiently approximate to give the final angles accurately to seconds, the axes were determined by the substitution of the values of r, not in a single pair but in each of the three pairs of equations, the mean result being taken as the true value.

For brevity, the angle $(\pi - PX)$ is denoted by $P\overline{X}$, and thus $-\cos PX$ by $\cos P\overline{X}$.

(*a*) To find L''', the axis corresponding to the root $+3658\cdot 2$. Substituting for r the value $+3658\cdot 2$, and solving for $x : y : z$ or $\cos L'''X : \cos L'''Y : \cos L'''Z$, we get from the respective pairs of equations (1, 2), (2, 3), (3, 1):—

$$(1,2)\ x : y : z :: 1859510\cdot 1 : -206053\cdot 4 : 501777\cdot 22,$$
$$(2,3)\ x : y : z :: 984541\cdot 9 : -109098\cdot 6 : 265644\cdot 3,$$
$$(3,1)\ x : y : z :: 1527380\cdot 4 : -169354\cdot 64 : 412106\cdot 8\ ;$$

whence

	log cos $L'''X$.	log cos $L'''\overline{Y}$.	log cos $L'''Z$.
(1, 2)	9·9822662·1,	9·0268474·4,	9·4133786·1,
(2, 3)	9·9822692·7,	9·0268542·4,	9·4133355·4,
(3, 1)	9·9822665·0,	9·0271163·7,	9·4133290·3;

whence, taking the mean, we have from the equations (1, 2, 3):—

log cos $L'''X$, 9·9822673·3; log cos $L'''\overline{Y}$, 9·0269393·5;
log cos $L'''Z$, 9·4133477·3.

(*b*) To find M''', the axis corresponding to the root $+31$. Proceeding as before, we find

$$(1,2)\ x : y : z :: -88193 : 869134 : 683819,$$
$$(2,3)\ x : y : z :: -11381 : 112114 : 88193,$$
$$(3,1)\ x : y : z :: -112114 : 1104518 : 869134\ ;$$

whence

	log cos $M'''\overline{X}$.	log cos $M'''Y$.	log cos $M'''Z$.
(1, 2)	8·9003438·5	9·8939964·5	9·7898509·1
(2, 3)	8·9005463·7	9·8940258·5	9·7898000·7
(3, 1)	8·9004600·8	9·8939729·7	9·7898869·0

And for mean,

(1, 2, 3)	8·9004501·0	9·8939984·2	9·7898459·6;

(*c*) To find N''', the axis corresponding to the root $-465\cdot 2$. Proceeding as before, we find

$$(1,2)\ x : y : z :: 204251\cdot 6 : 465827\cdot 60 : -565689\cdot 92,$$
$$(2,3)\ x : y : z :: 73751\cdot 12 : 168198\cdot 20 : -204251\cdot 60,$$
$$(3,1)\ x : y : z :: 336396\cdot 4 : 767247\cdot 64 : -931655\cdot 20,$$

whence

	$\log\cos N'''\overline{X}$.	$\log\cos N'''\overline{Y}$.	$\log\cos N'''Z$.
(1, 2)	9·4289315·4	9·7869912·9	9·8713444·9
(2, 3)	9·4289430·7	9·7869958·2	9·8713399·5
(3, 1)	9·4289274·6	9·7870116·5	9·8713313·4

And for mean,

(1, 2, 3)	9·4289340·2	9·7869995·9	9·8713385·9.

It will be found by actual calculation that, in each of the three cases, the sum of the squares of the cosines corresponding to the above mean logarithms is unity.

The angles $L'''M'''$, $M'''N'''$, $N'''L'''$ calculated from these mean logarithms will be found to be respectively $90°+6''$, $90°+9''$, $90°-3''$, and thus still differ from 90°.

From the fact that the values determined from the different pairs of equations present in each case only small differences, we conclude that the above mean direction-cosines cannot be very far from precise; and, further, that it will be as laborious as useless to attempt to obtain a nearer approximation by the help of seven-figure logarithm-tables. We shall therefore now content ourselves, after thus proving that the differences are accounted for by the neglect of small quantities of the second order, with finding a set of lines exactly perpendicular to each other, and as nearly as possible coincident with the above positions of L''', M''', N'''.

It will be found that such a triad of lines is defined by the numbers given below:—

$\log\cos L'''X$ 9·9822675·4	$\log\cos L'''\overline{Y}$ 9·0269392·4	$\log\cos L'''Z$ 9·4133462·5
$\log\cos M'''\overline{X}$ 8·9002688·0	$\log\cos M'''Y$ 9·8940013·0	$\log\cos M'''Z$ 9·7898459·4
$\log\cos N'''\overline{X}$ 9·4289635·6	$\log\cos N'''\overline{Y}$ 9·7869661·7	$\log\cos N'''Z$ 9·8713562·8

And we shall henceforth adopt these lines as the positions at 200° C. of the lines which are at right angles both at 20° C. and 200° C. As has been pointed out above, they may, to the same degree of approximation, be regarded as the positions at 20° C. of the lines which are at right angles at the same pair of temperatures.

XIX. Before we proceed to determine the variation of the angles between these lines at the several temperatures, we shall put on their trial the formulæ to be used. For this purpose we shall calculate, with the help of the values of p, q, r, d, e, f given in § **XV.**, the angles between the crystallographic axes at 20° C. from the angles at 200° C., and compare them with the values given by Beckenkamp.

The direction-cosines of A‴, B‴, C‴ are defined by the following numbers (§§ **I.** & **XV.**):—

log cos A‴X = 9·9543897·7, cos A‴Y = 0, log cos A‴$\overline{\text{Z}}$ = 9·6387478·0;
log cos B‴$\overline{\text{X}}$ = 8·7111119·6, log cos B‴Y = 9·9987746·4, log cos B‴$\overline{\text{Z}}$ = 8·7373442·7;
cos C‴X = 0, cos C‴Y = 0, cos C‴Z = 1;

for from these we find

		Beckenkamp.
B‴C‴ . . .	93° 7′ 51″·6	93° 7′ 51″·6
C‴A‴ . . .	115 48 6·4	115 48 6·4
A‴B‴ . . .	91 17 25·1	91 17 25·1

(*a*) To find A, the position of A‴ at 20° C.

Let A be $x\,y\,z$, and A‴ be $\xi\,\eta\,\zeta$, where $y=\eta=0$. In this case the formula of § **XIII.** becomes

$$\frac{z}{x}=(1-r+p)\frac{\zeta}{\xi}-e.$$

We have seen (§ **XV.**) that for the temperatures 20°—200° C.,

$$p=+\cdot0017017,\quad d=+\cdot0001257,$$
$$q=-\cdot0000238,\quad e=+\cdot0010007,$$
$$r=+\cdot0000327,\quad f=-\cdot0004525;$$

whence

$$\frac{z}{x}=1\cdot001669\frac{\zeta}{\xi}-\cdot0010007;$$

and substituting the values of $\frac{\zeta}{\xi}$ or $\frac{\cos \text{A}'''\text{Z}}{\cos \text{A}'''\text{X}}$ given above, we find

log cos AX, 9·9540818·3; cos AY = 0; log cos A$\overline{\text{Z}}$, 9·6400605·8.

(*b*) To find B, the position of B‴ at 20° C.

Since y is not small, we may here conveniently use the formulæ (§ **XIII.**)

$$\left.\begin{aligned}\frac{x}{y}&=(1-p+q)\frac{\xi}{\eta}-f,\\ \frac{z}{y}&=(1-r+q)\frac{\zeta}{\eta}-e\frac{\xi}{\eta}-d;\end{aligned}\right\}$$

which on substitution become

$$\left.\begin{aligned}\frac{x}{y}&=\cdot9982745\frac{\xi}{\eta}+\cdot0004525,\\ \frac{z}{y}&=\cdot9999435\frac{\zeta}{\eta}-\cdot0010007\frac{\xi}{\eta}-\cdot0001257;\end{aligned}\right\}$$

whence

$$\log\cos B\overline{X},\ 8{\cdot}7065376{\cdot}2;\quad \log\cos BY,\ 9{\cdot}9987849{\cdot}7;\quad \log\cos B\overline{Z},\ 8{\cdot}7379172{\cdot}1.$$

From the logarithms given in (*a*) and (*b*) we find

		Beckenkamp.
	° ′ ″	° ′ ″
BC . . .	93 8 6·5	93 8 6·6
CA . . .	115 53 8·4	115 53 8·4
AB . . .	91 15 17·0	91 15 17·1

a result which shows that the formulæ of § **XIII.** are so far perfectly satisfactory.

XX. We now proceed to determine the relative positions L M N, L′ M′ N′, L″ M″ N″ of the lines L‴ M‴ N‴ at 20° C., 80° C., and 140° C. respectively.

(*a*) To find the positions L M N at 20° C.

If OP‴, $\xi\,\eta\,\zeta$, and OP, $x\,y\,z$ be positions of the same crystal-line at 20° C. and 200° C. respectively, then, from § **XIX.** (*b*),

$$\left.\begin{aligned}\frac{x}{y}&=0{\cdot}9982745\frac{\xi}{\eta}+{\cdot}0004525,\\ \frac{z}{y}&=0{\cdot}9999435\frac{\zeta}{\eta}-{\cdot}0010007\frac{\xi}{\eta}-{\cdot}0001257.\end{aligned}\right\}$$

Substituting in these formulæ the values of the direction-cosines given in § **XVIII.**, we deduce

$\log\cos LX$, 9·9823151·8	$\log\cos L\overline{Y}$, 9·0277586·8	$\log\cos LZ$, 9·4125499·3
$\log\cos M\overline{X}$, 8·8976047·8	$\log\cos MY$, 9·8940320·2	$\log\cos MZ$, 9·7898387·6
$\log\cos N\overline{X}$, 9·4285864·8	$\log\cos N\overline{Y}$, 9·7868904·4	$\log\cos NZ$, 9·8714577·9

whence

$$LM=90°+0''{\cdot}08,\quad MN=90°+0''{\cdot}02,\quad NL=90°+0''{\cdot}18:$$

thus confirming the statement that the above lines are virtually at right angles both at 20° C. and 200° C.

(*b*) To find the positions L′ M′ N′ at 80° C.

For 20°—80° C., according to § **XV.**,

$$p-q=+{\cdot}0004452,\quad f=-{\cdot}0000376,$$
$$r-q=-{\cdot}0000845,\quad e=+{\cdot}0001522,\quad d={\cdot}0000419;$$

and we have from § **XII.**, if P′ be $\xi\eta\zeta$, and P be xyz, as before,

$$\frac{\xi}{\eta}=(1+p-q)\frac{x}{y}+f,$$
$$\frac{\zeta}{\eta}=(1+r-q)\frac{z}{y}+e\frac{x}{y}+d,$$

or
$$\frac{\xi}{\eta}=1{\cdot}0004452\frac{x}{y}-{\cdot}0000376,$$
$$\frac{\zeta}{\eta}=0{\cdot}9999155\frac{z}{y}+{\cdot}0001522\frac{x}{y}+{\cdot}0000419.$$

Substituting for $\frac{x}{y}\frac{z}{y}$ the values given in § **XX.** (*a*) for the lines L M N respectively, we find:—

log cos $L'X$, 9·9823170·0	log cos $L'\overline{Y}$, 9·0275653·8	log cos $L'Z$, 9·4125578·3
log cos $M'\overline{X}$, 8·8979660·7	log cos $M'Y$, 9·8940381·5	log cos $M'Z$, 9·7898228·7
log cos $N'\overline{X}$, 9·4287729·9	log cos $N'\overline{Y}$, 9·7869209·6	log cos $N'Z$, 9·8714128·4

whence
$$L'M'=90°+6''{\cdot}35,\quad M'N'=90°+16''{\cdot}08,\quad N'L'=90°+31''{\cdot}65.$$

(*c*) To find the positions L'' M'' N'' at 140° C.

For 20°—140° C.,
$$p-q=+{\cdot}0009507,\quad f=-{\cdot}0000706,$$
$$r-q=-{\cdot}0000012,\quad e=+{\cdot}0004701,\quad d=+{\cdot}0000888;$$
whence
$$\frac{\xi}{\eta}=1{\cdot}0009507\frac{x}{y}-{\cdot}0000706,$$
$$\frac{\zeta}{\eta}=0{\cdot}9999988\frac{z}{y}+{\cdot}0004701\frac{x}{y}+{\cdot}0000888,$$
and

log cos $L''X$, 9·9822981·8	log cos $L''\overline{Y}$, 9·0273255·7	log cos $L''Z$, 9·4128578·9
log cos $M''\overline{X}$, 8·8983082·1	log cos $M''Y$, 9·8940190·7	log cos $M''Z$, 9·7898481·1
log cos $N''\overline{X}$, 9·4289631·7	log cos $N''\overline{Y}$, 9·7869243·4	log cos $N''Z$, 9·8713857·5

from which we find
$$L''M''=90°-16''{\cdot}89,\qquad M''N''=90°+7''{\cdot}53,$$
$$N''L''=90°+35''{\cdot}01.$$

(*d*) To find the positions L''' M''' N''' at 200° C., given the positions L M N at 20° C.

This calculation will serve merely to test the influence of small quantities of the second order on the reversal of the formulæ.

For 20°—200° C.,
$$p-q=+{\cdot}0017255,\quad f=-{\cdot}0004525,$$
$$r-q=+{\cdot}0000565,\ e=+{\cdot}0010007,\quad d=+{\cdot}0001257;$$
whence
$$\frac{\xi}{\eta}=1{\cdot}0017255\frac{x}{y}-{\cdot}0004525,$$
$$\frac{\zeta}{\eta}=1{\cdot}0000565\frac{z}{y}+{\cdot}0010007\frac{x}{y}+{\cdot}0001257,$$

and

log cos L‴X, 9·9822675·1	log cos L‴$\bar{Y}$, 9·0269404·6	log cos L‴Z, 9·4133445·3
log cos M‴$\bar{X}$, 8·9002634·3	log cos M‴Y, 9·8940005·5	log cos M‴Z, 9·7898455·7
log cos N‴$\bar{X}$, 9·4289636·6	log cos N‴$\bar{Y}$, 9·7869667·7	log cos N‴Z, 9·8713569·4

which are almost identical with the original values given in § **XVIII.**

XXI. For convenience we here tabulate the above variations from 90° of the angles between the crystal-lines L, M, N at the different temperatures :—

Variation of	LM.	MN.	LN.
At 20° C. ...	0″	0″	0″
„ 80 C. ...	+ 6	+16	+32
„ 140 C. ...	−17	+ 8	+35
„ 200 C. ...	0	0	0

The variations at 80° C. and 140° C. of the angles between the three crystal-lines which are at right angles both at 20° C. and 200° C. are thus very small ; even if parameters determined by experiment are accepted as perfectly accurate.

XXII. Proceeding in exactly the same way we have calculated the positions of the lines which are at right angles both at 20° C. and 80° C., and find that they are virtually coincident with those calculated by Beckenkamp ; hence we infer that he is correct in stating that the directions of the thermic axes as calculated from his measurements widely differ for the two pairs of temperatures.

XXIII. The variations tabulated in § **XXI.** are, however, sufficient to prove that even this wide difference of about 26½° results from changes in the relative inclinations of LMN so small that they might be accounted for by the minute errors incidental to the measurement of the angles. In the first place, although single seconds were read off by the microscope, an angle, when determined with a rising temperature, varied in some cases from that determined with a falling temperature by as much as 10″, the average variation being 6″·1 ; secondly, the corrections derived according to the method of least squares and applied by Beckenkamp to the measured angles have an average value of 3″·2 and reach a positive maximum of 9″·7 and a negative maximum of −8″·7; thirdly, although it is not impossible, still it is curious that the pair of lines LM should be at right angles not only at 20° C. and 200° C. but also at a third temperature between 80° C. and 140° C.; and lastly, as Beckenkamp points out in the later paper, there may be a minute distortion of the crystal owing to the heating-effect of the rays concentrated on one side during the measurement of an angle.

In bringing to a close this paper, of which the latter part has been given up to a critical examination of the important results obtained by Beckenkamp, we beg to express on behalf of the students of crystals our hearty thanks for the series of careful observations made by him with the elaborate instrument belonging to the University of Strasburg; and we may add that the labour involved in the interpretation of his measurements will only be fully appreciated by those who may be attracted by the subject to repeat some of the calculations.

LXI. *On Lines of no Chemical Change.* *By* Edmund J. Mills, *D.Sc., F.R.S., and* William M'D. Mackey*.

[Plate VIII.]

MOST of the chemical changes we ordinarily observe are examined either in their course or at their termination; but although it is clear that they must have a beginning, the beginning is extremely difficult as a mental conception, and quite inaccessible to direct experiment.

We have considered it of great interest to determine in certain cases the "origin" of chemical change, such origin being obviously identifiable in many instances with a line of no chemical change.

The substances selected for reaction were aqueous hydric sulphate and zinc, brought in contact under definite conditions. Supposing the temperature to remain constant, and hydrogen to be evolved from the unrenewed reagents, the gas will, as is well known, cease to appear when the hydric sulphate has been partially exhausted. If y be the percentage strength of the sulphate (H_2SO_4) and x the amount of gas evolved, we can express y in terms of x (provided the experimental numbers lie sufficiently near to the origin) by the equation

$$y=a+bx+cx^2,$$

where a, b, c are constants to be calculated from three experiments at least. Then putting $x=0$, we obtain the value a for y: this value is a point on the line of no chemical change.

Other points can be similarly obtained at other constant temperatures; and when these are all continuously joined, the result is the line of no chemical change for the hydrogen reaction as a function of strength and temperature. We have always used three experiments and three terms of Taylor's

* Communicated by the Authors.

series; having found these sufficient to enable us to draw, with fair approximate accuracy, the line of which we were in quest*.

The zinc we employed was prepared electrolytically from a solution of highly purified acetate, pressed together, and then rapidly melted in a porcelain crucible by the heat of an alcohol flame. It was then flattened out between recently polished steel surfaces until almost exactly 0·5 millim. thick; ribbon-shaped pieces cut from it, 1 millim. wide and weighing 0·5 grm., were coiled in the form of a short helix. Each zinc, before immersion in the experimental liquid, was dipped in alcoholic potash for three minutes, rapidly washed with water, and drained on clear filter-paper; the metal was thus perfectly cleaned without material loss. A fresh zinc was used for each experiment.

The hydric sulphate was prepared by fractional distillation from an extremely pure purchased sample. We failed to detect in it any foreign body excepting water. As the basis of our apparatus, we employed a constant-temperature bath, designed by one of us some time since (Edinb. Phil. Trans. 1881, p. 567, to which place we refer for a detailed drawing and description of the instrument). Into the central or "constant" compartment of this bath we introduced, as will be seen in the Plate, a glass tube (15 × 155 millim.) closed at one end, which always held 5 cub. cent. of the sulphate under examination. Thermometer 40803, on which 0·01 C. could be read by estimation, was next inserted, and when this showed a sensibly constant temperature the zinc helix was dropped in. The delivery-cap (see Plate VIII. fig. 1) was now slid down over the reaction-tube, its point inserted under the collecting-cylinder, and time noted when the first bubble of gas came off. The bubbles were in all cases very small. In some cases, especially in the first three or four sets of experiments, the cessation of the reaction was difficult to observe. In these the gas came off sluggishly during the first minute, rose to a maximum of delivery-rate during the third or fourth minute, and then gradually became slower. It is among these groups that the longest times were recorded; and chemical change was considered to have ceased when the rate fell below one bubble a minute. In the other sets, on the contrary, there was no difficulty in determining the point of termination; action commenced briskly at once, and concluded with considerable abruptness. It will be observed that, as the temperature rises, the time (as might have been expected) diminishes. All our experiments were performed as

* Compare Set IV., exp. 1 in Table I., with the subsequent values of *a*.

above described, except those corresponding to $t^\circ = \cdot 84$: in these (as shown in fig. 2) the reaction-tube was inserted in a test-tube containing water and surrounded by melting ice.

Our temperatures are corrected for zero error and exposure, and refer to the mercurial thermometer.

In the following Table (I.) we have collected the results of our experiments, recording under each group the corresponding equation.

TABLE I.

	Initial temperature.	Average temperature.	y. Percentage strength of sulphate.	x. Cub. cent. gas evolved.	Time, in minutes.
I.	°·68	°·82	58·42	21·68	19
	·78	·91	58·22	34·34	25
	·66	·80	57·28	79·55	100
	$y = 58{\cdot}706 - {\cdot}010964\,x - {\cdot}000089320\,x^2$.				
II.	7·59	8·57	58·22	17·49	12
	8·68	8·74	56·65	54·06	50
	8·80	8·69	56·08	120·61	125
	$y = 59{\cdot}286 - {\cdot}066776\,x + {\cdot}00033327\,x^2$.				
III.	15·78	15·80	56·65	16·11	13
	15·70	15·59	56·08	75·88	50
	15·70	15·67	54·61	155·08	90
	$y = 56{\cdot}724 - 0{\cdot}0035633\,x - {\cdot}000064935\,x^2$.				
IV.	21·08	21·08	56·65	1·80	
	21·11	21·09	56·08	22·42	14
	21·05	21·05	55·98	28·55	14
	21·00	20·96	54·61	75·91	45
	$y = 56{\cdot}718 - {\cdot}037049\,x + {\cdot}00038836\,x^2$.				
V.	26·80	26·99	54·61	37·02	16
	27·00	27·01	54·36	53·45	19
	27·18	27·12	53·86	119·45	60
	$y = 55{\cdot}357 - {\cdot}023603\,x + {\cdot}000092698\,x^2$.				
VI.	35·24	35·21	54·36	44·21	10
	35·33	35·25	53·86	46·52	13
	35·32	35·29	53·34	69·64	20
	$y = 79{\cdot}621 - {\cdot}90869\,x + {\cdot}0076292\,x^2$.				
VII.	43·15	43·09	54·36	14·29	5
	43·13	43·16	53·86	43·59	7
	43·03	43·17	53·34	62·64	8
	$y = 54{\cdot}472 - {\cdot}0048164\,x - {\cdot}00021162\,x^2$.				
VIII.	51·57	51·62	53·86	22·03	4
	51·93	51·90	53·34	28·98	4
	51·85	51·99	52·60	50·71	5
	$y = 56{\cdot}416 - {\cdot}14733\,x + {\cdot}0014733\,x^2$.				

Table II. contains the "average temperatures" t, and the corresponding zero-points s of percentage strength.

TABLE II.

t.	s. Zero strength.	Zero strength calculated.
$^{\circ}$·84	58·71	58·15
8·67	59·29	57·95
15·69	56·72	57·95
21·07	56·72	57·19
27·04	55·36	55·88
35·25	79·62	79·62
35·25	79·62	79·62
43·14	54·47	54·60
51·84	56·42	56·30

Probable error of a single comparison with theory of the first six strengths, ·56; or ·92 per cent. on the average strength.

As regards the first portion of the curve (*i. e.* up to and including $t=35·25$), the relation between zero-strength s and temperature is indicated sufficiently well by an equation based on a commonly occurring type of chemical effect*,

$$s_1=54·47+\frac{25·15-4·3(35·25-t)}{1-(35·25-t)};$$

$s_1=58·77$ being an asymptote.

For the second or right-hand portion of the curve we have three experiments only. These are accurately represented by the equation

$$s_2=54·47+\frac{25·15-3·3(t-35·25)}{1-(t-35·25)},$$

which shows that the second hyperbola tends strongly towards symmetry with the first: $s_2=57·77$ is an asymptote.

The point of contact of the two curves is necessarily when $s=79·62$. A drawing of them (fig. 3) is appended.

The percentage strength of sulphate which, as computed from our data, just prevents action at $35^{\circ}·25$ is 79·62—a number decidedly remote from the experimental values from which it was calculated. We therefore thought it worth while to make trials with 76·55 per cent. sulphate at mean temperatures of $35^{\circ}·35$ and $35^{\circ}·20$; in both cases we obtained a very distinct, though slight evolution of gas. These results add considerable weight to the particular value under discussion.

* Guldberg and Waage, *Études*, p. 63.

The nature of the reaction between pure aqueous hydric sulphate and zinc is shown by our experiments to be of a very complicated character. Thus, from 58·77 to 79·62 there are two temperatures corresponding to every strength; and between strength 54·47 and 57·77 at least, there are four temperatures corresponding to every strength.

The law of relation of temperature to chemical change has hitherto been very little investigated. Hood and Warder, after some trial experiments, have regarded chemical effect as proportional to the square of the temperature. Our own representation places temperature on the footing of a chemical reagent.

Our work has had exclusive reference to the beginning of a chemical reaction; its object has been to find the initial line of no chemical change. There is, however, obviously a terminal zero-line, similarly obtainable, and related to the conditions existing at the close of a reaction. Between these two lines lies the surface, on which would occur all possible events in the actual process of change.

It is our intention to resume this investigation.

LXII. *The Physical Basis of Probability.* *By* F. Y. Edgeworth, *M.A., Lecturer on Logic at King's College, London**.

A REMARKABLE analogy has been drawn by Donkin† between the behaviour of a material particle tending to equilibrium under the influence of attractive centres of various force, and the determination of the judgment to the "weighted mean" of several observations. The essential feature of the analogy is, according to the view of the present writer, the circumstance that mental as well as mechanical equilibrium is represented by a sum of squares. The expression

$$g_1(x_1-x)^2+g_2(x_2-x)^2+g_3(x_3-x)^2+\&c.$$

represents (twice) the potential energy of a dynamical system consisting of centres, at the points x_1, x_2, &c. along a line, of attractive force proportioned to the simple distance multiplied by g_1, g_2, &c. respectively. The same expression represents the disadvantage incurred by taking x as the real point from which the observations x_1, x_2, &c. of weights respectively g_1, g_2, &c. have diverged. But the representation in the latter case is not so faithful as in the former. The simple sum of squares is not the measure, but only the criterion, of the psychical quantity; decreasing as it decreases, and becoming

* Communicated by the Author.

† Ashmol. Soc. Trans. 1844; Liouville, *Journ. Math.* xv. (1850).

the least possible when it becomes the least possible, but not proportional to it. This sort of relation between psychical quantity and mathematical expression may be illustrated by the mathematical theory of exchange. There the forces at work, the tastes of the buyers and sellers, are of inconceivable complexity. Yet the position of equilibrium is characterized by a feature of geometrical simplicity, uniformity of rate-of-exchange. This possibility of mathematically representing maximum advantage is due to the same cause in the market as in the observatory: what may be called the law of great numbers. The sum of squares above written makes its appearance in virtue of the exponential law of error or probability-curve incidental to the Method of Least Squares; and this simple form arises when the observations are independent of each other and indefinitely * numerous. Similarly the law of unity of price holds good where the competitors are independent and indefinitely numerous. In both cases uniformity is due to plurality; definite order to infinite numbers.

It follows from this view that Donkin's representation of the *forces* of the intellectual machine is (except at the vanishing-point of equilibrium) nugatory. There is no correspondence between a force proportional to the simple distance and the mysterious pleasure-force which urges us to choose the most advantageous value. So, too, it would be easy to enhance the geometrical† representation of the field of competition by introducing the assumption that each economic atom is urged to objects of gratification according to some simple law of force. But the assumption would be destitute of scientific value.

The real point of union between the things compared by Donkin is the correspondence between the tendency of a mechanical system to maximum (kinetic, minimum potential) energy and the tendency of volition to maximum pleasure. This seems to be the physical basis of volition, and if so, of belief, with which, upon a plausible‡ theory, volition may almost be identified. No doubt it is difficult to refer all acts of will to one and the same law of maximum pleasure; it is not easy to refer some actions to any such law. It is difficult also to identify all the energy-principles of mathematical physics; and one of the most important (the Principle of Least Action) may seem to hold good only in cases of a certain simplicity—

* Infinite *relatively* to the limits of a single observation; as indicated in the postscript of the article on the Law of Error, Phil. Mag. Oct. 1883.

† The reader is referred here and throughout this note to the writer's essay on 'Mathematical Psychics' (Kegan Paul).

‡ Mr. Bain's.

between successive kinetic foci, in the absence of reflecting or refracting surfaces, and so forth. Nevertheless he who has realized the omnipresence and all-powerfulness of a maximum-principle in one form or another, both in the physical and the moral sciences, and who is persuaded of the harmony of those sciences, will not hastily abandon the conjecture that in the correspondence between maximum energy and greatest possible happiness is to be sought the first principle of Psycho-Physics.

LXIII. *On the Electromotive Force of Alloys.* *By* JOHN TROWBRIDGE *and* E. K. STEVENS.*

THE best study of alloys and the most thorough work on them has been done by Matthiessen, who proved conclusively that alloys were neither mechanical mixtures nor chemical compounds, but what he terms, in a general way, "a solidified solution of one metal in another." He also showed that, with reference to the formation of alloys, metals were divided into two classes—the first class being those which, when alloyed with each other, give a conductivity in proportion to the respective volumes of the two metals; and the second those which, when alloyed with each other, give a conductivity which is less than that of the respective volumes of the two metals.

The aim of this investigation has been to note the variation of electromotive force in different alloys of the same metals and to deduce, if possible, some general law which governs the variation.

Two sets of alloys were used—one set of lead and tin, and the other of copper and zinc. The first set was made by taking the proportional weights of lead and tin, and melting them together in a crucible, and then pouring them out on a flat surface and allowing them to cool. The second set was made by melting a weighed amount of copper in a Fletcher gas-furnace, and, when in a molten state, adding more than the required amount of zinc, in order to make allowance for volatilization. Pure metals were obtained, in order that the results might be as accurate as possible.

It was deemed sufficient, as far as the lead and tin alloys were concerned, to weigh out carefully the required amounts of each metal, and to take those weights as showing the composition. This could not be done with the copper and zinc alloys, as it is impossible to determine how much of the zinc

* Proceedings of the American Academy of Arts and Sciences, vol. xviii. (May 29, 1883).

volatilizes; so with these it is necessary to resort to analytical methods of determining the percentage of each. The copper was determined by electrolysis, by precipitating the copper, from a sulphuric-acid solution of the alloy, upon a platinum disk connected with the negative pole of a battery, and the positive pole dipping in the solution. The zinc was determined by subtracting the per cent. of copper from a hundred per cent.

The composition of the alloys are given in the tables below, and will be referred to by number hereafter.

Number.	Alloys of Sn and Pb.		Number.	Alloys of Cu and Zn.	
	Parts by weight of Sn.	Parts by weight of Pb.		Per cent. of Cu.	Per cent. of Zn.
I.	1	9	I.	91·92	8·08
II.	2	8	II.	85·75	14·25
III.	3	7	III.	72·99	27·01
IV.	4	6			
V.	5	5	IV.	66·70	33·30
VI.	6	4	V.	49·32	50·68
			VI.	27·99	72·01
VII.	7	3			
VIII.	8	2			
IX.	9	1	VII.	7·53	92·47

Four determinations were made with these alloys, the first two being the observation of the electromotive force of each alloy, with platinum for the positive pole and the alloy as the negative pole, with fresh pond-water as the liquid; the second two being the determination of the electromotive force with the same positive pole, but with distilled water acidulated with a small quantity of sulphuric acid for a liquid. A mirror-galvanometer and ground-glass scale were used, and a large resistance placed in the circuit, and the galvanometer shunted so as to reduce the deflection.

The first two tables do not give any general law for the electromotive force of alloys, the force being especially irregular, which is perhaps due to the fact that the electromotive forces of the two metals are very nearly alike.

The explanation of the third table is rather unsatisfactory, since the sulphate of lead is insoluble, while the sulphate of tin is not known; and this last may account for the change from the first tables. In the fourth table, the increase in electromotive force of the alloys containing the more copper may be accounted for by the fact that the sulphate of copper is more readily soluble than the sulphate of zinc.

I. The electromotive force of the alloys of tin and lead, and of the metals themselves, when the resistance is 7180 ohms, and the constant of the galvanometer ·000003435.

The first column gives the deflection in millimetres; the second the tangent of one half the angle of deflection; the third the product of the constant by the total resistance; and the fourth the electromotive force in volts. The liquid in this case is water.

Number.	Deflection.	Tan $\frac{1}{2}$ angle of deflection.	E.M.F. in volts.
Pb	207	·0953	0·238
I.	205	·0942	0·249
II.	182	·0837	0·222
III.	173	·0795	0·201
IV.	212	·0975	0·258
V.	184	·0846	0·224
VI.	216	·0993	0·263
VII.	175	·0805	0·213
VIII.	208	·0956	0·253
IX.	195	·0896	0·237
Sn	164	·0754	0·199

II. The electromotive force of the alloys of copper and zinc, with a total resistance of 19,708 ohms, the constant being the same as before, and the liquid fresh pond-water.

Number.	Deflection.	Tan $\frac{1}{2}$ angle of deflection.	E.M.F. in volts.
Cu	10	·0046	0·031
I.	12	·0055	0·037
II.	17	·0078	0·053
III.	52	·0239	0·162
IV.	64	·0294	0·199
V.	106	·0487	0·330
VI.	131	·0602	0·408
VII.	218	·1002	0·678
Zn	228	·1048	0·709

The resistance of distilled water is so great, that it was impossible to get any good or satisfactory results. The addition of about one tenth of a cubic centimetre of strong sulphuric acid to about one hundred and fifty cubic centimetres of distilled water gave the liquid used in the last two observations.

III. The electromotive force of the alloys of lead and tin, with total resistance of 22,608 ohms, the constant being the same, and the liquid as stated above.

Number.	Deflection.	Tan ½ angle of deflection.	E.M.F. in volts.
Pb	185	·0881	0·661
I.	193	·0887	0·689
II.	195	·0896	0·696
III.	198	·0911	0·708
IV.	197	·0906	0·704
V.	196	·0902	0·701
VI.	194	·0892	0·693
VII.	198	·0911	0·708
VIII.	189	·0874	0·679
IX.	104	·0892	0·693
Sn	202	·0929	0·722

IV. The electromotive force of the alloys of copper and zinc, with a total resistance of 2380 ohms, and the same constant of galvanometer and the same liquid as in the preceding determination.

Number.	Deflection.	Tan ½ angle of deflection.	E.M.F. in volts.
Cu	43	·0188	0·153
I.	34	·0156	0·130
II.	40	·0184	0·150
III.	43	·0188	0·153
IV.	47	·0216	0·176
V.	55	·0253	0·206
VI.	115	·0529	0·432
VII.	203	·0939	0·768
Zn	234	·1763	0·442

It would seem to follow, from the last table at least, that in acid solutions the electromotive force of alloys is determined by the proportional part of that metal which is most readily attacked by the acid.

The general differences in the behaviour of the two sets of alloys may perhaps be accounted for by the distinction which Matthiessen* made between the two kinds of alloys. He classes an alloy of lead and tin among those which are "solidified solutions of one metal in another," while he calls alloys like copper and zinc "solidified solutions of one metal in an allotropic modification of another."

* British-Association Report, 1863. p. 47.

LXIV. *On the Laws of Motion.* *By* Professor TAIT.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I DO not think that any useful purpose could be served by my entering upon a discussion with Mr. Browne, as his views are so entirely different from mine.

But I must request you kindly to afford me an opportunity of letting your readers know precisely what are those views of mine which Mr. Browne regards as dangerous.

They will see from the enclosed abstract of my paper that all I propose is to dispense with the sense-suggested idea of *Force*, and to introduce in its place the objective reality *Energy*.

I think your readers will probably consider Mr. Browne to be a much more sweeping and dangerous innovator than myself; for while we both acknowledge Time and Space, I maintain the objectivity alike of *Matter* and of *Energy*, and base my system on them; but Mr. Browne practically throws them both overboard, and constructs his physical universe by means of *Force* alone.

Yours truly,

P. G. TAIT.

College, Edinburgh,
November 10th, 1883.

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*On the Laws of Motion**.

THE one objection to which, in modern times, Newton's wonderfully complete and compact system (the *Axiomata sive Leges Motûs*) is liable, is that it is expressly founded on the conception of what is now called "force" as an agent which "compels" a change of the state of rest or motion of a body. This is part of the first law; and the second law is merely a definite statement of the amount of change produced by a given force.

There can be no doubt that the proper use of the term *force* in modern science is that which is implied in the statement—Force is whatever changes a body's state of rest or motion. This is part of the first law of motion. Thus we see that force is the English equivalent of Newton's term *vis impressa*. But it is also manifest that, on many occasions, *but only where his meaning admitted of no doubt*, Newton omitted the word *impressa* and used *vis* alone, in the proper sense of force. In other cases he omitted the word *impressa*, as being implied in some other adjective such as *centripeta*, *gravitans*, &c.,

* Communicated by the Author, as an Abstract of a Paper read before the Royal Society of Edinburgh, December 18, 1882.

which he employed to qualify the word *vis*. That this is the true state of the case is made absolutely certain by the following:—

Definitio IV. *Vis impressa est actio in corpus exercita, ad mutandum ejus statum vel quiescendi vel movendi uniformiter in directum.*

Contrast this with the various senses in which the word *vis* is used in the comment which immediately follows, viz.:—

Constitit hæc vis in actione solâ, neque post actionem permanet in corpore. Perseverat enim corpus in statu omni novo per solam vim inertiæ. Est autem vis impressa diversarum originum, ut ex ictu, ex pressione, ex vi centripetâ.

These passages are translated by Motte as below:—

"Definition IV. *An impressed force is an action exerted upon a body, in order to change its state, either of rest, or of moving uniformly forward in a right line.*"

"This force consists in the action only, and remains no longer in the body when the action is over. For a body maintains every new state it acquires, by its *vis inertiæ* only. Impressed forces are of different origins; as from percussion, from pressure, from centripetal force."

The difficulty which Motte here makes for himself, and which he escapes from only by leaving part of the passage in the original Latin, is introduced solely by his use of the word *force* as the equivalent of the Latin *vis*.

We may quote two other passages of Newton bearing definitely on this point.

Definitio III. *Materiæ vis insita est potentia resistendi, quâ corpus unumquodque, quantum in se est, perseverat in statu suo vel quiescendi vel movendi uniformiter in directum.*

It is perfectly clear that, in this passage, the phrase *vis insita* is one idea, not two, and that *vis* cannot here be translated by *force*. Yet Motte has

"The *vis insita*, or innate force of matter, is" &c.

Definitio V. *Vis centripeta est, quâ corpora versus punctum aliquod, tanquam ad centrum, undique trahuntur, impelluntur, vel utcumque tendunt.*

It is obvious that the qualifying term *centripeta* here includes the idea suggested by *impressa*, defining in fact the direction of the *vis*, and therefore implying that its origin is outside the body.

After what has just been said, no further comment need be added to show the absurdity of the terms *accelerating force*, *innate force*, *impressed force*, &c. All of these have arisen simply from mistranslation. *Vis*, by itself, is often used for *force*; but *vis acceleratrix*, *vis impressa*, *vis insita*, and other phrases of the kind must be taken as wholes; and, in them, *vis* does not mean *force.*

The absurdity of translating the word *vis* by *force* comes out still more clearly when we think of the term *vis viva*, or *living force* as it is sometimes called; a name for kinetic energy, which depends on the unit of length in a different way from force. It must be looked upon as one of the most extraordinary instances of Newton's clearness of insight that, at a time when the very terminology of science was only as it were shaping itself, he laid down with such wonderful precision a system absolutely self-consistent.

From the passages just quoted, taken in conjunction with the second law of motion, we see that (as above stated) in Newton's view—

Force is whatever causes (but not, *or tends to cause*) *a change in a body's state of rest or motion.*

Newton gives no sanction to the so-called *statical* ideas of force. Every force, in his view, produces its effect. The effects may be such as to balance or compensate one another; but there is no balancing of forces.

(Next comes a discussion as to the objectivity or subjectivity of force. An abstract of this is given in §§ 288–296 of the article Mechanics in the new edition of the *Encyc. Brit.*, and need not be reproduced here.)

But, just as there can be no doubt that force has no objective existence, so there can be no doubt that the introduction of this conception enabled Newton to put his *Axiomata* in their exceedingly simple form. And there would be, even now, no really valid objection to Newton's system (with all its exquisite simplicity and convenience) could we only substitute for the words "force" and "action" &c., in the statement of his laws, words which (like rate or gradient &c.) do not imply objectivity or causation in the idea expressed. It is not easy to see how such words could be introduced; but assuredly they will be required if Newton's system is to be maintained. The word "stress" might, even yet, be introduced for this purpose; though, like force, it has come to be regarded as something objective. Were this possible, we might avoid the necessity for any very serious change in the *form* of Newton's system. I intend, on another occasion, to consider this question. How complete Newton's statement is, is most easily seen by considering the so-called "additions" which have been made to it.

The second and third laws, together with the scholium to the latter, expressly include the whole system of "effective forces" &c., for which D'Alembert even now receives in many quarters such extraordinarily exaggerated credit. The "reversed effective force" on a particle revolving uniformly in a circle is nothing but an old friend—"centrifugal force." And even this phantom is still of use, *in skilled hands*, in forming the equations for certain cases of motion.

The chief arguments for and against a modern modification of the laws of motion are therefore as follows (where we must remember that they refer exclusively to the elementary teaching of the subject, and have no application to the case of those who have sufficient knowledge to enable them to avoid the possible dangers of Newton's method):—

I. For. Is it wise to teach a student by means of the conception of force, and then, as it were, to kick down the scaffolding by telling him there is no such thing?

II. Against. Is it wise to give up the use of a system, due to such an altogether exceptional genius as that of Newton, and which amply suffices for all practical purposes, merely because it owes part of its simplicity and compactness to the introduction of a conception which, though strongly impressed on us by our muscular sense, corresponds to nothing objective?

Every one must answer these questions for himself; and his answer will probably be determined quite as much by his notions of the usefulness of the study of natural philosophy as by his own idiosyncrasies of thought.

Those who desire that their scientific code should be, as far as possible, representative of our real knowledge of objective things, would undoubtedly prefer to that of Newton a system in which there is not an attempt, however successful, to gain simplicity by the introduction of subjective impressions and the corresponding conceptions.

In the present paper simplicity of *principle*, only, is sought for; and the mathematical methods employed are those which appeared (independent altogether of the question of their fitness for a beginner) the shortest and most direct. A second part will be devoted to simplicity of *method* for elementary teaching.

(1) So far as our modern knowledge goes, there are but two objective things in the physical world—matter and energy. Energy cannot exist except as associated with matter; and it can be perceived and measured by us only when it is being transferred, by a "dynamical transaction," from one portion of matter to another. In such transferences it is often "transformed;" but no process has ever been devised or observed by which the quantity, either of matter or energy, has been altered.

(2) Hence the true bases of our subject, so far as we yet know, are:—

1. Conservation of matter.

2. Conservation of energy.

3. That property (those properties?) of matter, in virtue of which it is the necessary vehicle, or, as the case may be, the storehouse, of energy.

(3) The third of these alone presents any difficulty. So long as energy is obviously kinetic, this property is merely our old friend *inertia.* But the mutual potential energy of two gravitating masses, two electrified bodies, two currents, or two magnets, is certainly associated (at least in part, and in some as yet unknown way) with matter, of a kind not yet subjected to chemical scrutiny, which occupies the region in which these masses &c. are situated. And even when the potential energy obviously depends on the strain of a portion of ordinary matter, as in compressed air, a bent spring, a deformed elastic solid, &c., we can, even now, only describe it as due to "molecular action," depending on mechanism of a kind as yet unknown to us:—though in some cases, such as the kinetic gas theory, at least partially guessed at.

(4) The necessity for the explicit assumption of the third principle, and a hint at least of the limits within which it must be extended, appear when we consider the very simplest case of motion, viz. that of a lone particle moving in a region in which its potential energy is the same at every point. For the conservation of energy tells us merely that its *speed* is unaltered. We know, however, that this is only part of the truth: the *velocity* is constant. It will be seen later that this has most important dynamical consequences in various directions.

(The remarkable discussion of this point by Clerk-Maxwell is then referred to, in which it is virtually shown that, were things otherwise, it would be possible for a human mind to have knowledge of *absolute* position and of *absolute* velocity.)

(5) But Maxwell's reasoning is easily seen to apply equally to any component of the velocity. Hence, when we come to the case in which the potential energy depends on the position, the only change in the particle's motion at any instant is a change of the speed in the normal to the equipotential surface on which the particle is at that instant situated. The conservation of energy assigns the amount of this change, and thus the motion is completely determined. If V be the potential energy of unit mass at the point ρ, we have at once

$$\ddot{\rho} = -\nabla V,$$

from which the circumstances of the motion can be deduced. In fact, this problem is precisely the same as was that of the motion of a luminous corpuscle in a non-homogeneous medium, the speed of passing through any point of the medium being assigned.

(6) It is next shown that the above inertia-condition (that the velocity parallel to the equipotential surface is the same for two successive elements of the path) at once leads to a "stationary" value of the sum of the quantities vds for each two successive elements, and therefore for any finite arc, of the path. This is, for a single particle, the *Principle of Least Action*, which is thus seen to be a direct consequence of inertia.

(It is then shown that the results above can be easily extended to a particle which has two degrees of freedom only.)

But it is necessary to remember that, in these cases, we take a partial view of the circumstances; for a lone particle cannot strictly be said to have potential energy, nor can we conceive of a constraint which does not depend upon matter other than that which is constrained. Hence the true statement of such cases requires further investigation.

(7) To pass to the case of a system of free particles we require some quasi kinematical preliminaries. These are summed up in the following self-evident proposition:—If with each particle of a system we associate two vectors, *e. g.* Θ_1, Φ_1, with the mass m_1, &c., we have

$$\Sigma m\Theta\Phi=\Sigma(m)\,.\,\Theta_0\Phi_0+\Sigma m\theta\phi,$$

where

$$\Theta=\Theta_0+\theta,$$
$$\Phi=\Phi_0+\phi,$$

and

$$\Sigma m\Theta=\Sigma(m)\,.\,\Theta_0,$$
$$\Sigma m\Phi=\Sigma(m)\,.\,\Phi_0;$$

so that Θ_0 and Φ_0 are the values of Θ and Φ for the whole mass collected at its centre of inertia, and θ, ϕ those of the separate particles relative to that centre.

(8) Thus, if $\Theta=\mathrm{P}=\mathrm{P}_0+\rho$ be the vector of m,

$$\Phi=\dot{\Theta}=\dot{\mathrm{P}}=\dot{\mathrm{P}}_0+\dot{\rho}$$

its velocity, we have

$$\Sigma m\mathrm{P}\dot{\mathrm{P}}=\Sigma(m)\,.\,\mathrm{P}_0\dot{\mathrm{P}}_0+\Sigma m\rho\dot{\rho},$$

the scalar of which is, in a differentiated form, a well-known property of the centre of inertia. The vector part shows that the sum of the moments of momentum about any axis is equal to that of the whole mass collected at its centre of inertia, together with those of the several particles about a parallel axis through the centre of inertia.

If

$$\Theta=\Phi=\dot{\mathrm{P}},$$

we have

$$\Sigma m\dot{\mathrm{P}}^2=\Sigma(m)\,.\,\dot{\mathrm{P}}_0^2+\Sigma m\dot{\rho}^2;$$

i. e. the kinetic energy, referred to any point, is equal to that of the mass collected at its centre of inertia, together with that of the separate particles relative to the centre of inertia.

If we integrate this expression, multiplied by dt, between any limits, we easily obtain a similar theorem with regard to the Action of the system.

Such theorems may be multiplied indefinitely.

(9) From those just given, however, if we take them along with 3 above, we see at once that, provided the particles of the system be all free, while the energy of each is purely kinetic and independent alike of the configuration of the system and of its position in space:—

1. The centre of inertia has constant velocity;
2. The vector-moment of momentum about it is constant;
3. So is that of the system relative to any uniformly moving point;
4. $\Sigma\int mvds$ is obviously a minimum.

(10) The result of (7) points to an independence between two parts of the motion of a system, *i. e.* that relative to the centre of inertia and that of the whole mass supposed concentrated at the centre of inertia. Maxwell's reasoning is applicable directly to the latter if the system be self-contained, *i. e.* if it do not receive energy from, or part with it to, external bodies. Hence we may extend the axiom 3 to the centre of inertia of any such self-contained system, and, as will presently be shown, also to the motion of the system relative to its centre of inertia. This, though not *formally* identical with Newton's Lex III., leads, as we shall see, to exactly the same consequences.

(11) If, for a moment, we confine our attention to a free system consisting of two particles only, we have

$$m_1\dot{\rho}_1+m_2\dot{\rho}_2=(m_1+m_2)\alpha,$$

or

$$m_1\ddot{\rho}_1+m_2\ddot{\rho}_2=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\alpha)$$

This must be consistent with the conservation of energy, which gives

$$\tfrac{1}{2}(m_1\dot{\rho}_1^2+m_2\dot{\rho}_2^2)=f\,[\mathrm{T}(\rho_1-\rho_2)], \quad . \quad . \quad . \quad (\beta)$$

since the potential energy must depend (so far as *position* goes) on the distance between the particles only. Comparing (α) and (β), we see that we may treat (β) by partial differentiation, so far as the coordinates of m_1 and m_2 are separately concerned. For we thus obtain

$$m_1\ddot{\rho}_1=\nabla_{\rho_1}.f=f'.\,\mathrm{U}(\rho_1-\rho_2),$$
$$m_2\ddot{\rho}_2=\nabla_{\rho_2}.f=-f'.\,\mathrm{U}(\rho_1-\rho_2).$$

Each of these, again, is separately consistent with the equation in § 5 for a lone particle. Hence, again, the integral

$$\int(m_1v_1ds_1+m_2v_2ds_2)$$

has a stationary value.

Hence also, whatever be the origin, provided its velocity be constant,

$$\Sigma m \mathrm{V} \rho\ddot{\rho}=0.$$

Thus, even when there is a transformation of the energy of the system, the results of § 9 still hold good.

And it is to be observed that if one of the masses, say m_2, is enormously greater than the other, the equation

$$m_1\ddot{\rho}_1+m_2\ddot{\rho}_2=0$$

shows that $\ddot{\rho}_2$ is excessively small, and the visible change of motion is confined to the smaller mass. Carrying this to the limit, we have the case of motion about a (so-called) "fixed centre." In such a case it is clear that, though the *momenta* of the two masses relative to their centre of inertia are equal and opposite, the kinetic energy of the greater mass vanishes in comparison with that of the smaller.

These results are then extended to any self-contained system of free particles, and the principle of *Varying Action* follows at once. It is thus seen to be a general expression of the three propositions of § 2 above.

(12) So far as we have gone, nothing has been said as to *how* the mutual potential energy of two particles depends on their distance apart. If we suppose it to be enormously increased by a very small increase of distance, we have practically the case of two particles connected by an inextensible string—as a chain-shot. But from this point of view such cases, like those of connexion by an extensible string, fall under the previous categories.

The case of impact of two particles falls under the same rules, so far as motion of the centre of inertia and moment of momentum about that centre are concerned. The conservation of energy requires in such cases the consideration of the energy spent in permanently disfiguring the impinging bodies, setting them into internal vibration, or heating them. But the first and third of these, at least, are beyond the scope of abstract dynamics.

(13) The same may be said of constraint by a curve or surface, and of loss of energy by friction or resistance of a medium. Thus a constraining curve or surface must be looked upon (like all physical bodies) as deformable; but, if neces-

sary, such that a very small deformation corresponds to a very great expenditure of energy.

(14) To deal with communications of energy from bodies outside the system, all we need do is to *include them in the system*. Treat as before the whole system thus increased, and then consider only the motion of the original parts of the system. This method applies with perfect generality whether the external masses be themselves free, constrained, or resisted.

(15) Another method of applying the same principles is then given. Starting from the *definition* $dA = \Sigma m S\dot{\rho} d\rho$, the kinematical properties of A are developed. Then, by the help of § 2, these are exhibited in their physical translations.

(16) The paper concludes with a brief comparison of the fundamental principles of the science as they have been introduced by Newton, Lagrange, Hamilton, Peirce, Kirchhoff, and Clerk-Maxwell respectively, and also as they appear in the unique Vortex-system of Thomson.

LXV. *Experiments on the Velocity of Sound in Air.* *By* D. J. Blaikley*.

THE method of experiment which I venture to bring before you this afternoon is the outcome of various attempts made by me to determine with greater accuracy than had hitherto been done the velocity of sound in small tubes, such as are used for musical instruments; in addition to which practical purpose the idea presented itself to my mind that, if a series of tubes were taken, having their diameters in a definite ratio, the observed results might by calculation be extended so as to include a value for a tube of infinite diameter, that is for free air. Some of the results were brought before the Musical Association in June last; but the experiments have since been repeated with greater accuracy.

It will not be necessary to take up your time by referring to more than a few of the many determinations that have been made. A useful summary was given by Mr. Bosanquet in the 'Philosophical Magazine' for April 1877; but we may note that only those observations in which corrections both for temperature and for moisture have been made can be considered at all accurate.

Such corrections being made, there remained at the time of Regnault's great series of experiments† considerable differences in the results arrived at by different observers, partly due,

* Communicated by the Physical Society. Read November 10, 1883.

† *Mémoires de l'Académie des Sciences*, tome xxxvii.

doubtless, to errors of observation and partly to an assumption of the absolute correctness of Laplace's formula, the theory in the application of which is that the excess of pressure in the wave above the barometric pressure of the air is infinitely small.

Experiments on the velocity of sound may be classed as open-air experiments and laboratory experiments; and I venture to think that the latter offer advantages which cannot be enjoyed in open-air work. The usual method in the open air has been for an observer at a distance from a gun to note the time which elapses between the flash and the hearing of the report; but even when the actual record of the time is aided by electrical or other apparatus, some difficulties and sources of error remain. For instance, the accurate registration of temperature and moisture is difficult, especially when the sound-wave passes at various heights above the earth's surface, as is the case when the experiment is carried out on two hills separated by a valley.

Many of these gun-fire determinations were critically examined by Le Roux*, and an estimated correction made for errors in temperature, the readings having been in all probability too high for air some metres above the ground.

Midway in character between open-air and laboratory experiments stand those of Regnault, carried out in gas- and water-mains; one of his reasons for choosing this method being the facility afforded by these tubes for the accurate observation of temperature and moisture. Passing by his work for the moment, we may note the laboratory method employed by Kundt, and also Le Roux's method, the latter giving 330·7 metres at 0° C. Kundt's method consists in its best form in the use of two glass tubes connected by a smaller tube or rod of glass, wood, or metal, this connecting-rod being clamped in certain positions to establish nodes, and its free vibrating ends being fitted with pistons working in the large tubes. The waves are excited by friction in the vibrating rod and transmitted therefrom to the air or other gas in the tubes, and the successive half-wave lengths are registered by the positions assumed by lycopodium dust during the vibration. By using tubes of different diameters he obtained the results shown in Table I., and came to the conclusion that the velocity observed in his largest tube was not appreciably different from the velocity in free air. He appears, however, to have experienced some difficulty in the determination of pitch, owing to the evanescent character of the sound. The intensity

* *Ann. de Chimie*, ser. 4, tom. xii. Nov. 1867.

also was not registered. The method is beautifully adapted for comparative rather than for absolute results.

TABLE I.

Velocity of Sound in Tubes, in metres per second, at 0° C. (Kundt.)

Diameter of tube. millim.	Velocity. metre.	Difference. metre.
55	332·80	·07
26	332·73	2·26
13	329·47	6·47
6·5	323·00	7·58
3·5	305·42	

Le Roux's method consisted in employing a U-shaped tube, 0·07 metre (70 millim.) diameter, closed by a membrane at each end. One membrane was tapped with a small beater, and the time occupied by the resulting wave in travelling between the two membranes, as indicated by the disturbance of the second one, was registered. It appears to me, however, that the employment of membranes may introduce a source of error in this way:—Let A B be a tube closed by a rigid material at one end, and of a length to give the maximum resonance to a quarter-wave. Now, instead of the rigid end, let the tube be closed by a membrane: this will require to be in the position C, *i. e.* nearer to A than B is, the exact position depending upon the tension of the membrane. In Le Roux's experiments, unless the two membranes were of exactly the same tension, a source of error would be introduced.

We may now turn to Regnault's experiments, a summary of which is here given.

TABLE II.

Velocity of Sound in Tubes, in metres per second, at 0° C. (Regnault.)

Diameter of tube. millim.	Velocity (mean). metres.	Velocity (limiting). metres.
1100	330·5	330·3
300	330·3	329·25
108	327·52	324·25
Free air.	330·71	330·60

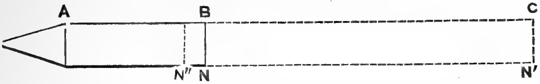
The one point which appears to me to be open to question is whether the rate of diminution of velocity is so great as his work appears to prove; for if this rate of diminution is extended until we reach tubes of the size used in musical instruments, we should have a velocity much less than experiments show to exist in such tubes (compare with Kundt). Probably the want of perfect smoothness in Regnault's tubes may account for some of the difference; but I think that it is doubtful whether the influence of the membranes closing his tubes was thoroughly allowed for, and feel that the question is still an open one.

It has long appeared to me that useful results might be obtained by making use of tubes giving musical notes, as the pitch of a steadily sounding note can be readily determined within a remarkable degree of accuracy, and there should be no difficulty in determining the temperature within half a degree Fahr., which is equivalent to 6 inches in the velocity. (Experiment—Resonance of closed tube to fork of 512 vibrations, the length of the tube being modified during the experiment to show both the maximum and constrained or imperfect resonance.) (See paper in Phil. Mag. for May 1879.)

Modifying this experiment by sounding the tube with an organ-pipe mouth, the disturbing influence of the contraction of area at the lip comes into play in addition to the correction for the open end; so that, although the value of the latter is pretty accurately known, no measurement of velocity based merely upon the length of a pipe is at all reliable. By adding, however, a half-wave or wave-length to a stopped pipe, maintaining the original pitch, and measuring this added length, I hoped to be able to get reliable results. The observations, though agreeing very well indeed so long as the pressure remained constant, did not agree in their different series when taken under very slight differences of pressure, much less than would cause sensible variation through change of intensity, according to Regnault's determinations.

To properly understand the causes of these variations, it will be necessary to examine some of the results of imperfect or constrained resonance, and consider the separate and conjoined influences of (1) the blast, air-reed, or inducing current; (2) the prime tone of the resonating air-column; and (3) the higher or partial tones sounding with the prime; the pitch of the resultant note, or alternating induced current, depending upon the values of these three forces, which have a power of mutual influence within certain limits. The subject presents itself to my mind in the following way:—

In the figure, let the blast from A cause the quarter-wave length of pipe AB to speak a note of a certain pitch, say 105 vibrations, and let BC be the half wave-length of pipe added to AB; the three-quarter wave-length AC being tuned to



speak the same note as AB. Assuming values in four typical cases, we obtain the following results (p. 452), in working out which arithmetical means instead of mean proportionals have been taken for the sake of numerical simplicity, although perhaps the latter would be more correct.

In Cases 1 and 2 the length NN′ is the true length of a half-wave of 105 vibrations. In (1) the position of N (the node) would remain unchanged on the addition of the half-wave; but in (2) the three-quarter wave would be of $106\frac{2}{3}$, and N would change its position to N″: this, however, would introduce no error in the result, for $3(106\frac{2}{3})-110=2\times105$, the length for half-wave as determined by the positions of N and N′.

In these two cases it is assumed that the first quarter-wave has equal constraining-power with the added quarters; but this is not strictly correct for cylindrical organ-pipes, in which the first quarter-wave is shortened and the mass in vibration reduced, through the contraction of the opening at the speaking end. The tendency of this would be to influence the results as shown by Case 3, where the first quarter-wave is assumed to have a mass of $\frac{2}{3}$ instead of unity, and in which the measured length would be represented by $3(107\frac{1}{2})-115=207\frac{1}{2}$, whereas it should be $2\times105=210$.

In Case 4 (*a*), suppose the point N to have been determined for the pitch as heard, 105, with a quarter wave-length of tube. A half-wave is added, and tuned to the original pitch, $=105$, and NN′ is taken as its measure of length. This is correct if the relative strength of the partials remains the same; but if a modification such as (*b*) takes place, the third partial falling to half-strength, the position of N as observed would no longer be correct for a pitch of 105 but only for a pitch of $103\frac{1}{3}$. A further complication would arise in this case, through the introduction of the point considered in Case 3.

To eliminate these sources of error, one of two things must be attained: we must either deal with pure tones or be careful to have resonating pipes of such form as to have their various proper tones strictly in accord with the harmonic series, and not merely approximately as is the case with organ-pipes;

	Uncon-strained Pitch.	Difference.	Constrain-ing power or stability.	Difference × Power.	Values of Compo-nents.	No. of Com-ponents.	Pitch of note heard.
CASE 1.—Blast and air-column of same pitch.							
Blast from A	105	....	1	....	105		
Quarter-wave, AB	105	....	1	....	105	÷ 2	= 105
Add half-wave, BC	105	....	1	....	210 210	÷ 4	= 105
CASE 2.—Blast and air-column of different pitches.							
Blast	100	....	1	....	100		
Quarter-wave	110	10	× 1	= 10	110	÷ 2	= 105
(*a*) Add half-wave, BC	105	5	× 1	= 5	210 210	÷ 4	= 105
Or, Blast	100	....	1	....	100		
(*b*) Three-quarter-wave	$106\frac{2}{3}$	$6\frac{2}{3}$	× 1	= $6\frac{2}{3}$	320	÷ 4	= 105
CASE 3.—Blast and air-column of different pitches.							
Blast	100	....	1	....	100		
Quarter-wave	115	15	× $\frac{2}{3}$	= 10	110	÷ 2	= 105
With added half-wave.							
Blast	100	....	1	....	100		
Quarter-wave	$107\frac{1}{2}$	$7\frac{1}{2}$	× $\frac{2}{3}$	= 5	105	÷ 4	= 105
Half-wave	$107\frac{1}{2}$	$\frac{1}{2}$	× 1	= $7\frac{1}{2}$	215		
CASE 4.—(Case 2 modified by presence of 12th of prime or 3rd partial.)							
(*a*) Blast	100	....	1	....	100		
Quarter-wave (prime)	105	....	1	....	105	÷ 3	= 105
„ „ (3rd partial)	$\frac{330}{3}$	....	1	....	110		
(*b*) Blast	100	....	1	....	100		
Quarter-wave (prime)	105	....	1	...	105	÷ 3	= $103\frac{1}{3}$
„ „ (3rd partial)	$\frac{330}{3}$	10	× $\frac{1}{2}$	5	105		

and in either case be careful that the blast is of exactly the power to give the desired note without constraint.

The tubes with which I experimented were of four diameters—·45, ·75, 1·25, and 2·08 inches respectively (11·7, 19·5, 32·5, and 54·1 millim.). After detecting the possible sources of error which have just been described, and trying a few modifications of the ordinary organ-pipe, the speaking ends or first quarter-wave lengths of the tubes were modified in form from cylindrical to pear-shaped, approximating to the shape of Helmholtz's resonators, and by this means a pure tone was obtained. The blast was obtained from a fan, the wind from which passed through a regulating bellows with automatic-valve action, and it was found that great care was necessary on this point. The pressure in the bellows was 2·5 inches of water, and in the speaking mouth in every case very small.

The temperature was observed by means of a thermometer entering the tube, so that the actual temperature during vibration might be recorded. The wet-bulb temperature and barometric pressure were also taken for moisture correction. The pitch was taken from a carefully tested Koenig fork of 256 vibrations, and the tubes were set to give a beating rate of about 4 per second, the lengths being read by a micrometer and standard rods. All the notes were exceedingly feeble, the pressure in the mouth being less than $\frac{1}{10}$ inch of water, much under the lowest which Regnault found to influence the velocity.

The results are given in the following Table:—

TABLE III.

Velocity of Sound in dry air, at 0° C., in tubes.

Diameter of tube	11·7 millim.	19·5 millim.	32·5 millim.	54·1 millim.
		327·09	328·72	329·90
		327·14	328·74	329·82
		326·98	328·78	329·84
		326·70	328·72	329·70
		327·09	328·72	329·95
		326·69	328·89	329·80
		326·99	328·76	329·53
		326·79	328·84	329·56
		326·70	328·84	329·65
		326·85	328·83	329·48
Means	324·56	326·90	328·78	329·72
Differences ...	2·34	1·88	0·94	

I found it very difficult to get observations with the tube of

11·7 millim. diameter. Very many trials were made; and although the best single observation gave 324·78, I have reason to think that the mean value 324·56 is too high. I hope yet to verify it, and also to continue experiments with a tube 90·2 millim. diameter.

There is one point connected with the velocity of sound which appears to require elucidation, and that is, the modification it may undergo near its point of origin; for the waves which affect us as sound are usually not plane-waves, but emanate from an origin which more or less nearly approaches a point—this point being the centre of a system of spherical waves.

We may refer to the vibration in conical tubes as bearing upon this point. A complete cone, speaking its lowest note, with its apex or closed end for the position of the node, is of twice the length of a closed cylindrical tube of the same pitch, and has the same succession of harmonics as the open cylindrical tube (see Phil. Mag. August 1878). This property is independent of the proportion of base to height in the cone; and if we assume the base of the cone to be a portion of a spherical surface described from the apex, we may continually increase the conical angle until the cone becomes a sphere. Now, if we consider the effect of this principle and apply it to divergent waves, it will be found that such waves cannot have exactly the same velocity for all pitches, but that the lower the pitch the greater will be the velocity, owing to the difference of velocity between the first and succeeding quarter-wave lengths. To take a numerical example, we may choose two wave-lengths of 32 feet and 8 feet respectively:—

	Quarter-wave lengths. Cylinder.	Quarter-wave lengths. Cone.	Excess in Cone.
32-ft. wave . . .	8 ft.	16 ft.	8 ft.
8-ft. wave . . .	2 ft.	4 ft.	2 ft.
	Difference		6 ft.

Or, if we assume a velocity in free air (away from origin of vibrations) of 1120 feet per second, we shall have:—

Velocity of sound-wave of any pitch, not including first quarter-wave	1120 ft.
Velocity of 8-ft. wave, or wave of 140 vibrations measured from apex of cone, 1120+2 . . .	1122 ft.
Velocity of 32-ft. wave, or wave of 35 vibrations measured from apex of cone, 1120+8 . . .	1128 ft.

We thus find that waves near their origin are not of normal length.

In gun-fire experiments the pitch of the explosion is not known, and there may therefore be a variation in the recorded velocities which is due simply to an effect of conical or spherical divergence. If, in addition to the corrections made by Le Roux for temperature, some of the gun-fire experiments were submitted to a correction of this nature, the discrepancies would probably be much reduced.

LXVI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 240.]

November 7, 1883.—J. W. Hulke, Esq., F.R.S., President, in the Chair.

THE following communications were read :—

1. "On the Geology of the South Devon Coast from Tor Cross to Hope Cove." By Prof. T. G. Bonney, M.A., F.R.S., Sec. G.S.

The author, after a brief reference to the literature of the subject, stated that the chief petrographical problem presented by this district was whether it afforded an example of a gradual transition from slaty to foliated rocks, or whether the two groups were perfectly distinct. He described the coast from Tor Cross round by the Start Point to Prawle Point, and thence for some distance up the estuary leading to Kingsbridge. Commencing again to the north of Salcombe, on the other shore of this inlet, he described the coast round by the Bolt Head and Bolt Tail to Hope Cove. These rocks, admittedly metamorphic, consist of a rather thick mass of a dark mica-schist and of a somewhat variable chloritic schist, which also contains a good deal of epidote. In the lower part of this are some bands of a mica-schist not materially different from the upper mass. It is possible that there are two thick masses of mica-schist, one above and one below the chloritic schist; but, for reasons given, he inclined to the view that there was only one important mass, repeated by very sharp foldings.

The junction between the admittedly metamorphic group and the slaty series at Hope Cove, as well as that north of Salcombe, is clearly a fault, and the rocks on either side of it differ materially. Between the Start and Tor Cross the author believes there is also a fault, running down a valley, and so concealed. On the north side of this the rocks, though greatly contorted and exhibiting such alterations as are usual in greatly compressed rocks, cannot properly be called foliated, while on the south side all are foliated. This division he places near Hallsands, about $\frac{1}{2}$ mile to the south of where it is laid down on the geological map.

As a further proof of the distinctness of the two series, the author pointed out that there were clear indications that the foliated series had undergone great crumpling and folding after the process of foliation had been completed; hence that it was long anterior to the great earth-movements which had affected the Palæozoic rocks of South Devon. He stated that the nature of these disturbances suggested that this district of South Devon had formed the flank of a mountain-range of some elevation, which had lain to the south. Of the foundations of this we may see traces in the crystalline gneisses of the Eddystone and of the Channel Islands, besides possibly the older rocks of South Cornwall and of Brittany. He also called attention to some very remarkable structures in the slaty series near Tor Cross, which appeared to him to throw light upon some of the structures observed at times in gneisses and other foliated rocks.

2. "Notes on Brocchi's Collection of Subapennine Shells." By J. Gwyn Jeffreys, Esq., LL.D., F.R.S., F.G.S.

3. "British Cretaceous Nuculidæ." By John Starkie Gardner, Esq., F.G.S.

LXVII. *Intelligence and Miscellaneous Articles.*

ON THE INDUCTION PRODUCED BY VARIATION OF THE INTENSITY OF THE ELECTRIC CURRENT IN A SPHERICAL SOLENOID. BY M. QUET.

THE problem which I propose to solve is not without some interest, for, supposing the solenoid of the same volume as the sun and at the same distance from the earth, it will be possible to examine whether (in spite of that enormous distance, and without the necessity of attributing to its electric currents momentary variations of intensity which would be excessive in comparison with those of our voltaic currents) its induction is capable of producing appreciable effects. It will be seen further on what equivalence there exists between the real sun and the fictitious solenoidal sun in the point of view of terrestrial induction.

Imagine upon the surface of a sphere a series of circular coils, parallel with each other, following at a constant distance, and traversed by the same electric current, the intensity of which undergoes at every point the same momentary variation. Barlow constructed a solenoid of this description, the several circuits of which were excited by a single and constant current. The direction of the radius of the sphere which is perpendicular to the planes of the circles, and from which the currents are seen to move from right to left, is the axis of the solenoid. From the point O, the centre of the induced elementary mass m, I draw Ox perpendicular to the axis of the system, Oz parallel to this line, and Oy perpendicular to the plane zOx, to the left of Oz as personified and looking towards Ox. The inductive action of each circular current, and consequently that of the solenoid, is a force applied to the mass m and in a direction either similar or the reverse to Oy, according as the momentary variation is positive or negative. When the distance of the induced mass is sufficiently great, the action due to asin gle circular current is calculated by the formula

$$B=\frac{K}{2}m\frac{di}{dt}\frac{\pi\rho'^2}{R'^2}\sin\epsilon';$$

in which R′ is the distance from the point O to the centre c of the current under consideration, ϵ' the angle which R′ makes with the axis of the solenoid, ρ' the radius of the circular current of which c is the centre. I designate by u the latitude of the circuit C in regard to the plane drawn from the centre G of the sphere perpendicularly to the axis of the system; by δ the distance Gc; by ρ the radius of the sphere; by R the distance OG; and by ϵ the angle which R makes with the axis of the solenoid. Whence follow the formulæ:—

$$\delta=\rho\sin u;\quad R'\sin\epsilon'=R\sin\epsilon;$$

$$R'^2=R^2+2\delta R\cos\epsilon+\delta^2;$$

$$B=\frac{K}{2}m\frac{di}{dt}\frac{\pi\rho^2}{R^2}\sin\epsilon\frac{\cos^2 u}{\left(1+2\frac{\rho}{R}\cos\epsilon\sin u+\frac{\rho^3}{R^2}\sin^2 u\right)^{\frac{2}{3}}}.$$

u varies only when the transition is made from one circuit to the other, since the momentary variation di has been supposed the same throughout the current. Designating by u' the measure of the angle whose vertex is at G and whose sides intercept the constant distance l, which separates two consecutive circuits upon the sphere, we have $\rho u'=l$, and we see that u' will be very small in conformity with the ratio of l to ρ. If we represent by $f(u)$ the general value of B, and by Y the resultant of the forces analogous to this last, which forces proceed from the currents comprised between the values u_1 and u_2 of u, we have

$$Y=f(u_1)+f(u_1+u')+f(u_1+2u')+\ldots+f(u_2).$$

When u' is very small the second member differs little from a definite integral; and designating this correction by Δ, we have

$$Y=\frac{1}{u'}\int_{u_1}^{u_2}f(u)du+\Delta.$$

An approximate value of Δ is given by the well-known formula, to be found at the commencement of Poisson's *Traité de Mécanique*:—

$$\Delta=\tfrac{1}{2}[f(u_1)+f(u_2)]+\frac{u'}{12}[f'(u_2)-f'(u_1)].$$

The limits which we shall take for the integral will be $-\frac{\pi}{2},\frac{\pi}{2}$; and at these limits $\cos u$ is null, as also are the four terms of Δ. We have then, with a sufficiently near approximation,

$$Y=\frac{K}{2}m\frac{di}{dt}\sin\epsilon\frac{\pi\rho^2}{R^2}\cdot\frac{l}{\rho}\int_{-\frac{\pi}{2}}^{\frac{\pi}{2}}\frac{\cos^2 u\,du}{\left(1+\frac{2\rho}{R}\cos\epsilon\sin u+\frac{\rho^2}{R^2}\sin^2 u\right)^{\frac{3}{2}}}.$$

In order to calculate approximately the value of the integral, we shall in the result neglect the terms of the second order in respect of the relation $\frac{\rho}{R}$, which is supposed very small. We have then

$$Y=\frac{K}{2}m\frac{di}{dt}\sin\epsilon\frac{\pi\rho^2}{R^2}\frac{\rho}{l}\int_{-\frac{\pi}{2}}^{\frac{\pi}{2}}\cos^2 u\left(1-\frac{3\rho}{R}\cos\epsilon\sin u\right)du.$$

The value of this integral is $\frac{\pi}{2}$; we have therefore finally

$$Y=\frac{K}{2}m\frac{di}{dt}\sin\epsilon\frac{\pi\rho^3}{R^2}\frac{\pi\rho}{2l}.$$

Let us now suppose that the solenoid has the same radius as the sun, and that the distance R at which the induced mass m is placed is that which separates the earth from its luminary. We have then

$$R=220{\cdot}95\,.\,\rho;\quad \frac{\rho^2}{R^2}=0{\cdot}000020484.$$

The fourth of the circumference of the earth is 100,000 hectometres; the corresponding term for the sun is therefore 10,855,000 hectometres. If we assume that the electric currents succeed each other upon the sun at a distance l, expressed by a fraction $\frac{1}{n}$th of a hectometre, we shall have

$$l=\frac{1}{n};\quad \frac{\rho^2}{R^2}\frac{\pi\rho}{2l}=n\,.\,222{\cdot}35;$$

and consequently

$$Y=\frac{\pi}{2}m\frac{di}{dt}\pi\sin\epsilon\, n\,.\,222{\cdot}35.$$

To know whether this inductive force is efficacious at the distance at which the earth is placed, and without giving excessive values to $\frac{di}{dt}$, we have only to compare it with an analogous force produced by a laboratory experiment and giving sensible effects. This I intend doing.

For a system of spherical solenoids which should be concentric, similar to each other, and similarly placed, and of which all the currents experienced the same momentary variation of intensity, the inductive force is

$$Y_1 = \frac{K}{2} m \frac{di}{dt} \pi \sin \epsilon \frac{\rho^2}{R^2} \frac{\pi\rho}{2l} \frac{\rho}{3\lambda}\left(1 - \frac{\rho'^2}{\rho^2}\right).$$

Here ρ, ρ' are the exterior and interior radii of the system; λ is the constant distance which separates two consecutive surfaces.—*Comptes Rendus de l'Académie des Sciences*, Oct. 8, 1883, pp. 800-802.

ON THE RELATION BETWEEN THE INTERNAL FRICTION AND RESISTANCE OF SOLUTIONS OF SALTS IN VARIOUS SOLVENTS. BY EILHARD WIEDEMANN.

In connexion with a remark of G. Wiedmann* attempts have in recent times been frequently made to establish strict numerical relations between the coefficients of friction and the conductivity for voltaic electricity, against the universality of which, however, he raises theoretical objections. Experimenters on this subject have either followed the change in the above magnitudes with the temperature in the same solution, or with the concentration in various solutions; and a certain analogy has been ascertained to exist between the two processes. For a strict investigation, solutions of the same salt should be investigated which have as different coefficients of friction as possible. I have therefore made some experiments with solution of zinc sulphate in water and in aqueous glycerine. A special investigation showed that the glycerine was quite free from acid, and conducted the current very badly.

The resistances to friction were determined with the apparatus described by Sprung†; the electrical resistances were determined by Wheatstone's bridge. Solutions of zinc sulphate in water and in glycerine of the same strengths were placed in two troughs of equal dimensions, and two equal amalgamated zinc plates were dipped in them. Both troughs were simultaneously introduced into the two arms of the bridge.

In the first series of experiments the solutions were prepared by dissolving 10 parts of $ZnSO_4 + 7H_2O$ in 100 parts of water, and then making this solution up to 500 c. c. by the addition of 50 c. c., 100 c. c., or 250 c. c. of water or of glycerine. Hence the solutions always contained an equal number of molecules of the salt in equal volumes.

For the solutions in water and glycerine the following ratios were obtained:—

	Internal Friction.	Resistance.
50 $ZnSO_4$	1 : 68·7	1 : 12·1
100 $ZnSO_4$	1 : 29·8	1 : 9·52
250 $ZnSO_4$	1 : 6·15	1 : 3·68

* G. Wiedmann, *Galvan.* 2 ed. i. p. 631 *et seqq.*; 3 ed. i. p. 952.

† Pogg. *Ann.* clix. p. 1, 1876.

In all cases the solution of glycerine showed the greater frictional and electrical resistances.

The second experiment was made by dissolving 50 gr. $ZnSO_4 + 7H_2O$ in water to a volume of 500 c. c.; and on the other hand 50 gr. of $ZnSO_4$ was dissolved in a little water, and made up to 500 c. c. by glycerine.

It was not possible to dissolve zinc vitriol directly in glycerine, because glycerine first of all withdraws water from this salt, and the salt with a smaller quantity of water of crystallization dissolves with difficulty in glycerine.

In this case the ratios were:—

Frictional resistance 1 : 86·2; Electrical resistance 1 : 109.

The above numbers show throughout that *no* proportionality exists between frictional and electrical resistance; but that the solvent has a predominant influence in the processes in question. It follows moreover that the more concentrated are the solutions the less predominant is the influence of the solvent.

Leipzig, August 1883.

SPECTROSCOPIC NOTES.

BY PROFESSOR C. A. YOUNG, PRINCETON, N. J.

For the past few months I have been examining the spectra of sun-spots with great care, and with an instrument of high dispersion. The spectroscope employed consists of a comet-seeker of five inches aperture and about forty-eight inches focal length, used both as collimator and view-telescope after Littrow's method, the slit and diagonal eye-piece being as close together as it is convenient to place them. A small spot of black paper, about three tenths of an inch in diameter, is cemented to the centre of the object-glass (as suggested by my colleague, Professor Brackett, in a note published in the American Journal of Science, July 1882), and entirely destroys the internal reflections, which would otherwise most seriously interfere with vision.

The dispersion is obtained by one of Professor Rowland's magnificent gratings on a speculum-metal plane, with a ruled surface three and one-half inches by five, 14,000 lines to the inch. The slit and eye-piece of the telescope are so placed that the line joining them is parallel to the lines of the ruling. An instrument of this sort is incomparably more convenient than one in which the collimator and view-telescope are separate, though of course, on account of the inclination of the visual rays to the axis of the object-glass, there is a little aberration, and the *maximissimum* of definition is not quite reached. There is no difficulty, however, in seeing easily the duplicity of the *b*'s, E_1, and other similar tests with the instrument thus arranged. The spectroscope is mounted upon a strong plank, stiffly braced, and this is attached by powerful ring-clamps to the tail-piece of the 23-inch equatoreal of the Halsted observatory, so that the image of the sun falls directly upon the slit.

The detailed examination of the spot-spectra has been thus far confined mainly to a few limited regions in the neighbourhood of C, D, and *b*.

With the high dispersive power employed, the widening and "winging" of the heavier lines of the spectrum is not well seen, not nearly so well as with a single-prism spectroscope. All diffuse shadings disappear much in the same way as the naked-eye markings on the moon's face vanish in a powerful telescope—to be replaced by others more minute but not less interesting. In a few spots, however, the broadening of the D's and the reversal and occasional "lumping" of C has been noticeable even with this high dispersion. But the most striking result is that, in certain regions the spectrum of the spot-nucleus, instead of appearing as a mere continuous shade, crossed here and there by markings dark and light, is resolved into a countless number of lines, exceedingly fine and closely packed, interrupted frequently between E and F (and occasionally below E) by lines as bright as the spectrum outside the spot. These bright lines, so far as the eye can determine, may be either real lines *superposed*, or merely vacancies left in the shading of fine dark lines, since they are not sensibly brighter than the ordinary background of the surrounding spectrum.

The darker and more intense the spot, the more distinctly the fine lines come out, both the bright and the dark; and so far as I have been able to make out yet, there is no difference as regards these fine lines between one spot and another. I have never yet seen any evidence of displacement in them due to motion, no "lumpiness" nor want of smoothness in them.

When seeing is at the best, and everything favourable, close attention enables one to trace nearly all these lines out beyond the spot and its penumbra. But they are so exceedingly faint on the sun's general surface that usually they cannot be detected outside the spot-spectrum. This resolution of the spot-spectrum into a congeries of fine lines is most easily made out in the green and blue. Near D, and below it, it is much more difficult to see; and I am not even quite sure that this structure still exists in the regions around C and below it. Here, in the red, even with the highest dispersion and under the most favourable circumstances of vision, the spot-spectrum appears simply as a continuous shade, crossed here and there by widened and darkened lines, which, however, are very few and far between as compared with the number of such lines in the higher regions. Of course the resolution of the spot-spectrum into lines tends to indicate that the absorption which darkens the centre of a sun-spot is produced, not by granules of solid or liquid matter, but by matter in the gaseous form; and it becomes interesting to inquire what substances are capable of producing such a spectrum, and under what conditions. As to the fineness and number of the lines, it may be noted that in the region included between b_1 and b_4

the single lines appear to be each about half as wide as the components of b_3, and are separated by an interval about one third as great. The whole number between b_1 and b_4 must be over a hundred, though they are of course very difficult to count with accuracy. They are a little wider in the middle of the spot-spectrum, in fact spindle-shaped, running out into extremely fine threads where they pass into the penumbra, and in my instrument they seem to be a little more hardly and sharply defined on the upper (more-refrangible) edge than on the lower.

The bright lines, of which there are six between b_1 and b_4, are generally about as wide as the interspace between the components of b_3. They are sharply defined at both edges, and no brighter at the centre than at the edge—a fact which rather bears in favour of the idea that they are merely interruptions in the dark-line series, and not really superposed bright lines. Just above b_4 (at λ 5162·3) there is a very conspicuous one, which is also noticeable enough in the ordinary solar spectrum. Attention has indeed been frequently called to it long since by other observers. Below E these bright lines are rare. Higher up in the spectrum, between F and G, they become very numerous.

I have also made a considerable number of observations upon prominences with the nine and one-half inch equatoreal and its own spectroscope. There have been lately numerous very fine exhibitions, especially in connection with the spots. The number of lines reversed in the spectrum of the chromosphere has at times been very great, far exceeding the number observed and catalogued in 1872; but I have not been able to detect a single *new* one below C, though the two mentioned in my catalogue have been seen almost continuously. On two occasions (July 31st and August 1st) a new line above H (λ 3884$\pm$2) was conspicuously visible for an hour or two each time, during a specially vigorous eruption of the prominences associated with the great spot, which was then just passing off the limb. This line was seen easily without the aid of any fluorescent eye-piece; and I am satisfied that on a photographic plate it would have been more brilliant than either H or K. I could not determine its position within one or two units on account of the difficulty of identifying the numberless fine lines around it.

With the widened slit it showed clearly the form of the lower part of the prominence, but not the upper. It was almost precisely imitated by two new lines at λ 4092 and 4026; and the catalogue-lines 4077 and 3990 resembled it also. On the other hand, *h*, H, and K showed the higher parts of the prominence as well as the lower, while the lines at 4045 and 3970 were exceedingly fine and smooth, without knottiness or structure.

On August 1st, at 2^h 58^m local time ($=7^h$ 57^m Greenwich time), the intensity of the chromosphere-spectrum was very remarkable, the bright lines more vivid and numerous than I remember ever to have seen them before. Between this time and 3.12 a prominence was shot up in fragments of flame to an elevation of over 120,000 miles. It will be interesting to learn whether any corresponding magnetic twitch appears on the magnetometer records.—Silliman's *American Journal*, November 1883.

THE FLUORESCENCE OF IODINE VAPOUR. BY E. LOMMEL.

Fluorescence has hitherto only been observed in solids and liquids. It is an interesting question whether *gaseous* bodies are not also susceptible of fluorescence.

In order to approach the question, I have made a few preliminary experiments in this direction with some of the more powerfully absorbing gases and vapours, especially hyponitric acid, chlorine, and the vapours of bromine, iodine, and sulphur.

It was found that *iodine vapour has a pronounced orange-yellow fluorescence.* This is easily observed when sunlight concentrated by a lens is allowed to fall on a flask containing vapour of iodine which is not too dense. The orange-yellow pencil of light is particularly marked when the exciting light has passed through a *green* glass. If a blue glass is used as filter for the rays, the fluorescence is only feeble; while with red glass it disappears altogether.

Of the pure spectral colours, the most active were the green rays on each side of the line E; the yellow and bluish green were less so; and the red, blue, violet, and ultra-violet were quite inactive—a result which was to be expected, from the well-known absorptive properties of iodine vapour.

In order to ascertain whether the ultra-violet rays were active, the spectrum was produced by means of apparatus of quartz; and after it had been ascertained that this part of the spectrum could not excite fluorescence of iodine vapour, it was also confirmed that iodine vapour does not absorb either the ultra-violet, the blue, or the violet to any appreciable extent; for the very highly luminous ultra-violet part of the spectrum received on solution of esculine, remained unchanged when dense violet vapours of iodine were liberated in a test-tube held in front of the slit.

Hence iodine vapour is the only fluorescent substance yet known in which *the violet and ultra-violet rays are quite inactive.*

The feebly luminous spectrum of the orange-yellow fluorescent light is seen to be composed of red, orange, yellow, and green, extends from the division 35 of Bunsen's scale to about 60, and is brightest in orange. It appeared continuous, without lighter and darker lines.

Solutions of iodine in bisulphide of carbon and alcohol exhibited none of the fluorescence characteristic of iodine. It has already been stated that solid iodine does not fluoresce.

I was unable to recognize any fluorescence in the gases and vapours mentioned above.—Wiedemann's *Annalen*, 1883, No. 6, p. 356.

CLASSIFICATION OF METEORITES.

Professor Tschermak, who has made many important contributions to the study of meteorites, has been led by the results of recent microscopic study to propose some changes in the system of classification given by Rose in 1864. The meteorites are classified after the principles of lithology according to the kind and relative amounts of their mineralogical constituents. The classification as proposed by Tschermak, with some typical examples under each head, is as follows:—

I. Meteorites consisting essentially of iron: *Meteoric iron.*

II. Meteorites having an iron ground-mass with enclosed silicates. (*a*) *Pallasite*: iron and olivine the chief constituents (Pallas-iron, Atacama, Bitburg); (*b*) *Mesosiderite*: iron with olivine and bronzite (Hainholz, Estherville); (*c*) *Siderophyr*: iron and bronzite (Rittersgrün, Breitenbach, Steinbach); *Grahamite*: iron with plagioclase, olivine, bronzite (Serra da Chaco).

III. Meteorites consisting chiefly of olivine and bronzite with iron as a subordinate constituent; the texture mostly chondritic. *Chondrite* (Aigle, Knyahinya, New Concord, Pultusk).

IV. Meteorites consisting essentially of olivine, bronzite, pyroxene. (*a*) *Chassignite*: olivine (Chassigny); (*b*) *Amphoterite*: olivine and bronzite (Manbhoom); (*c*) *Diogenite*: bronzite or hypersthene (Ibbenbühren, Shalka); (*d*) *Chladnite*, enstatite (Bishopville); (*e*) *Bustite*: diopside and enstatite (Busti).

V. Meteorites consisting essentially of augite, bronzite, lime felspar; with a shining crust. (*a*) *Howardite*: augite, bronzite, plagioclase (Frankfort, Loutolaks); (*b*) *Eukrite*: augite, anorthite or maskelynite (Juvinas, Jonzac, Stannern, Peterborough).—Silliman's *American Journal*, Nov. 1883 (*Ber. Ak. Wien*, July 7, 1883).

PROFESSOR FERREL'S THEORY OF ATMOSPHERIC CURRENTS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Understanding that you consider that this question has been sufficiently discussed in your pages, I nevertheless hope you will allow me to set myself right with Prof. Everett.

First, by acknowledging a blunder which he has pointed out. A mass moving in a fixed great circle over the earth's surface does gain and lose, not only relative (as I said), but also absolute velocity parallel to the equator, because it changes its direction relative to that plane.

Secondly, by stating that this is not the doctrine which I still understand Prof. Ferrel to maintain, and which I still dispute.

Thirdly, by pointing out that this, after all, is not "the main point of my criticism." That main point is the utter baselessness of the mathematical theory propounded in the paper of 1860.

I remain,
Your obedient servant,
D. D. HEATH.

Kitlands, Dorking,
Oct. 27th, 1883.

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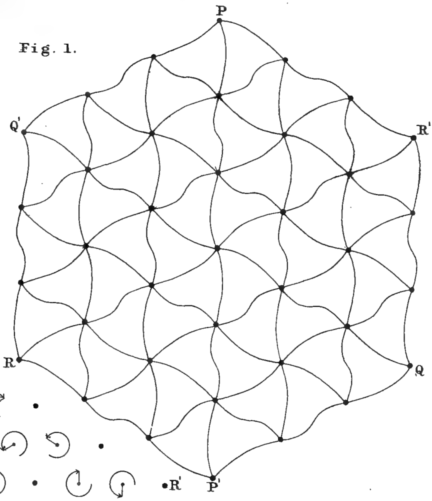

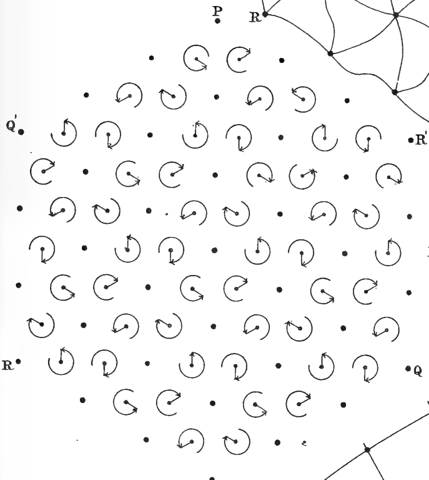

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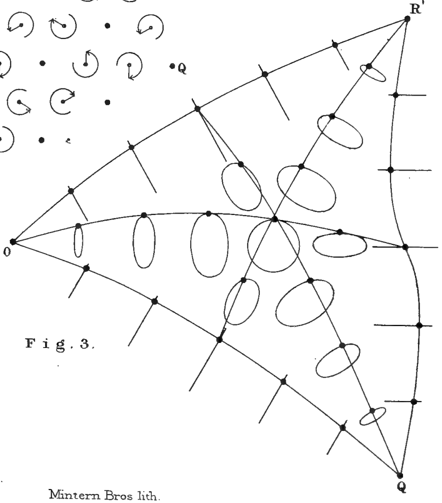

Fig. 3.

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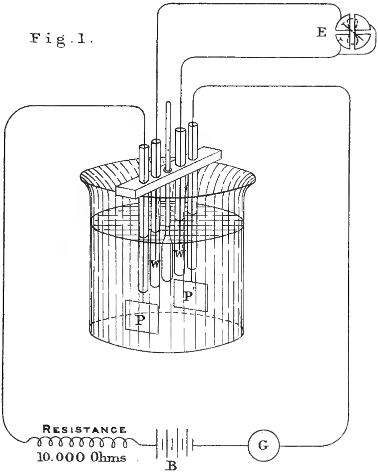

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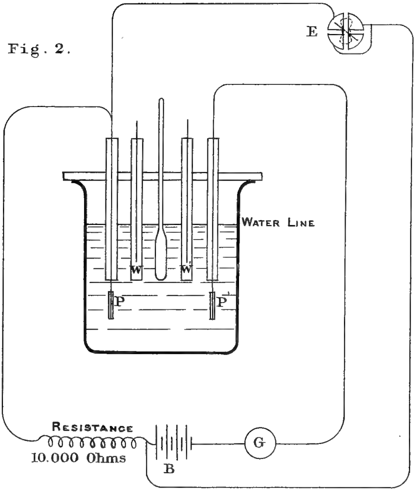

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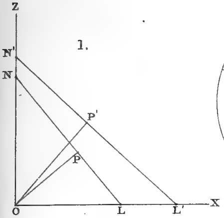

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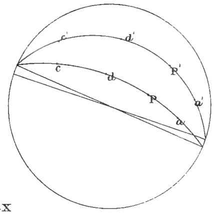

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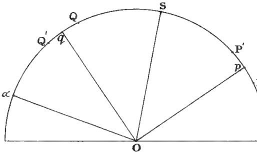

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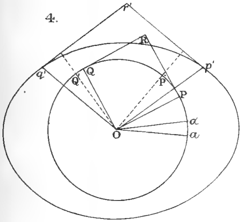

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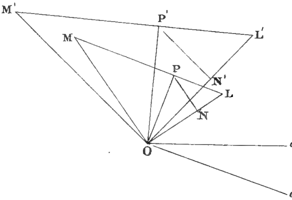

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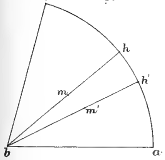

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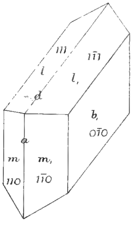

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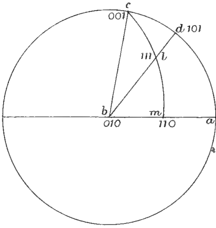

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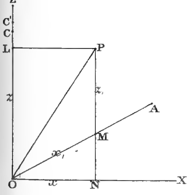

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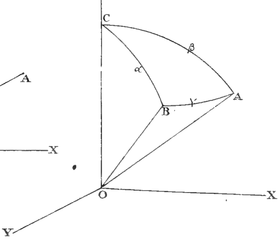

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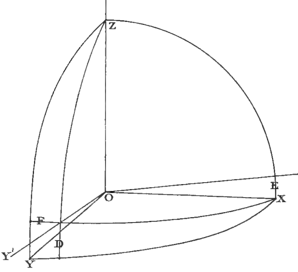

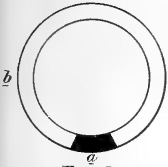

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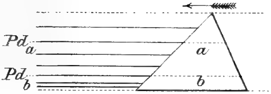

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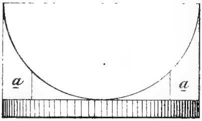

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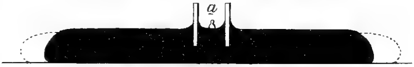

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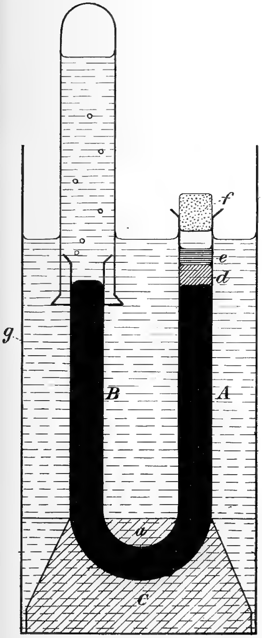

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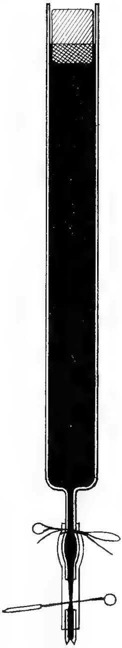
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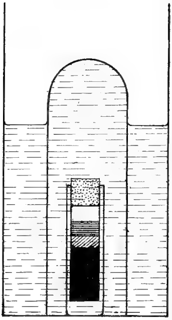
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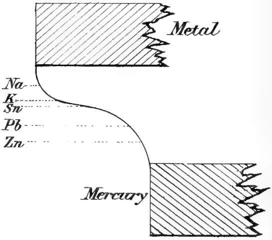

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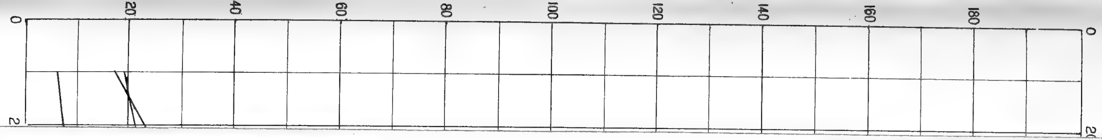

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Nº 4 gives the lines of Iron & Nº 5 those of some other metals as observed in the Arc.

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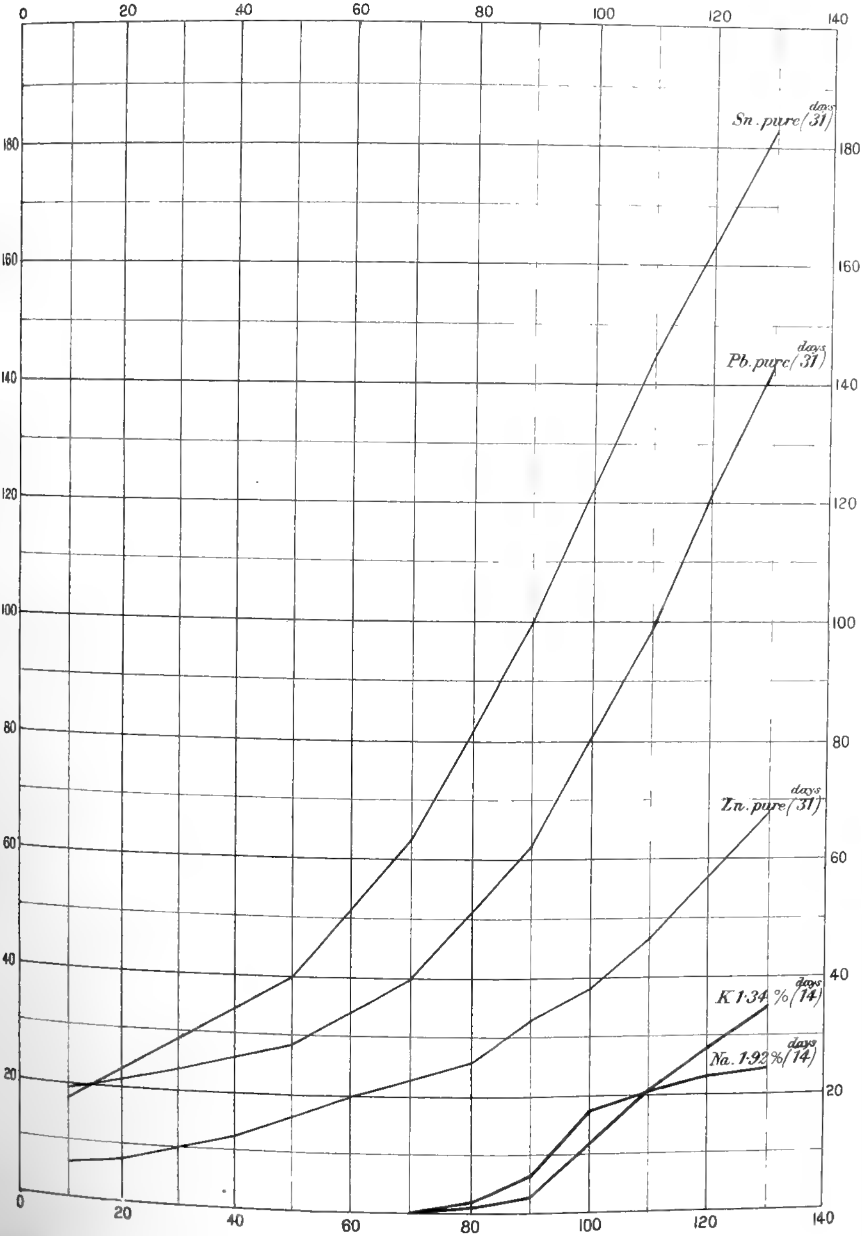

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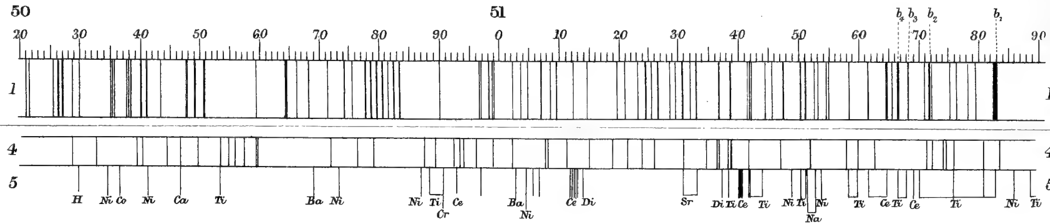

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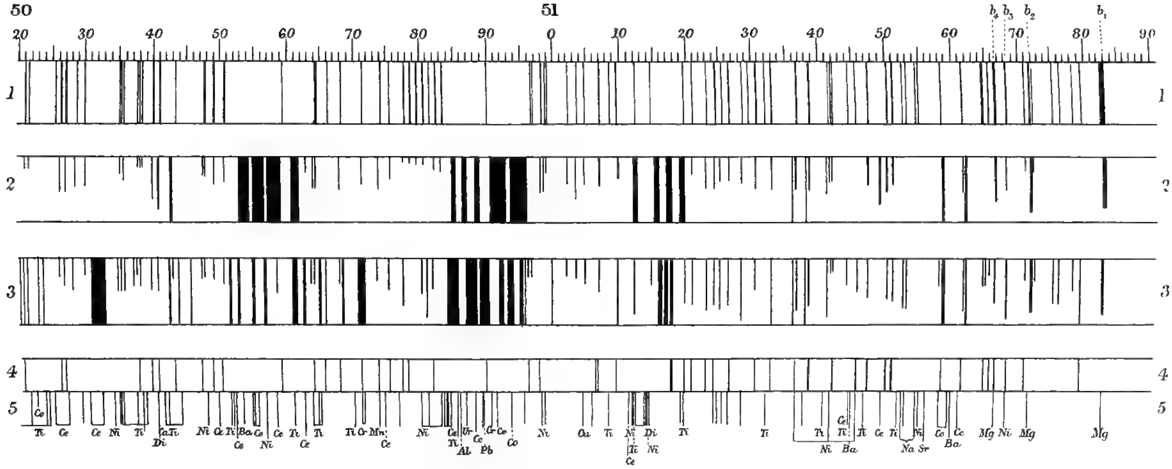

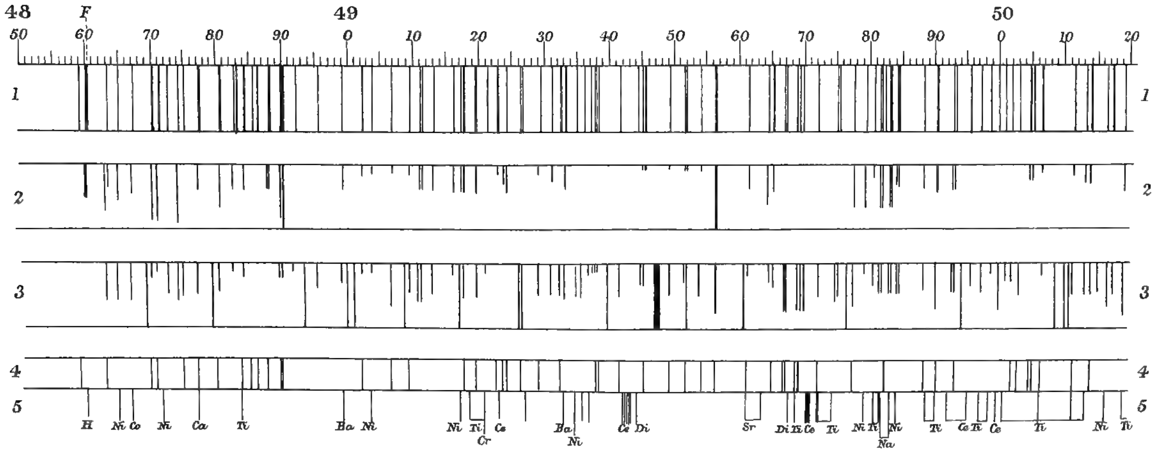

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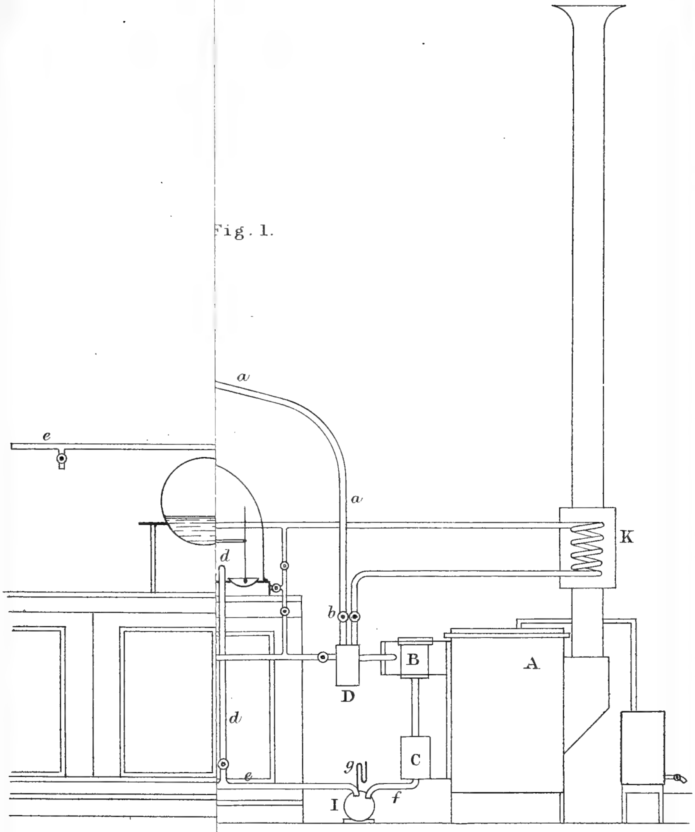

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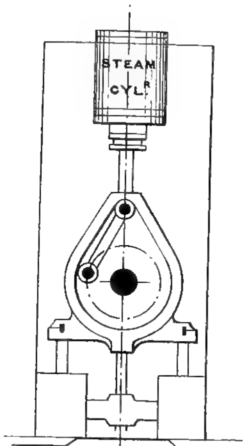

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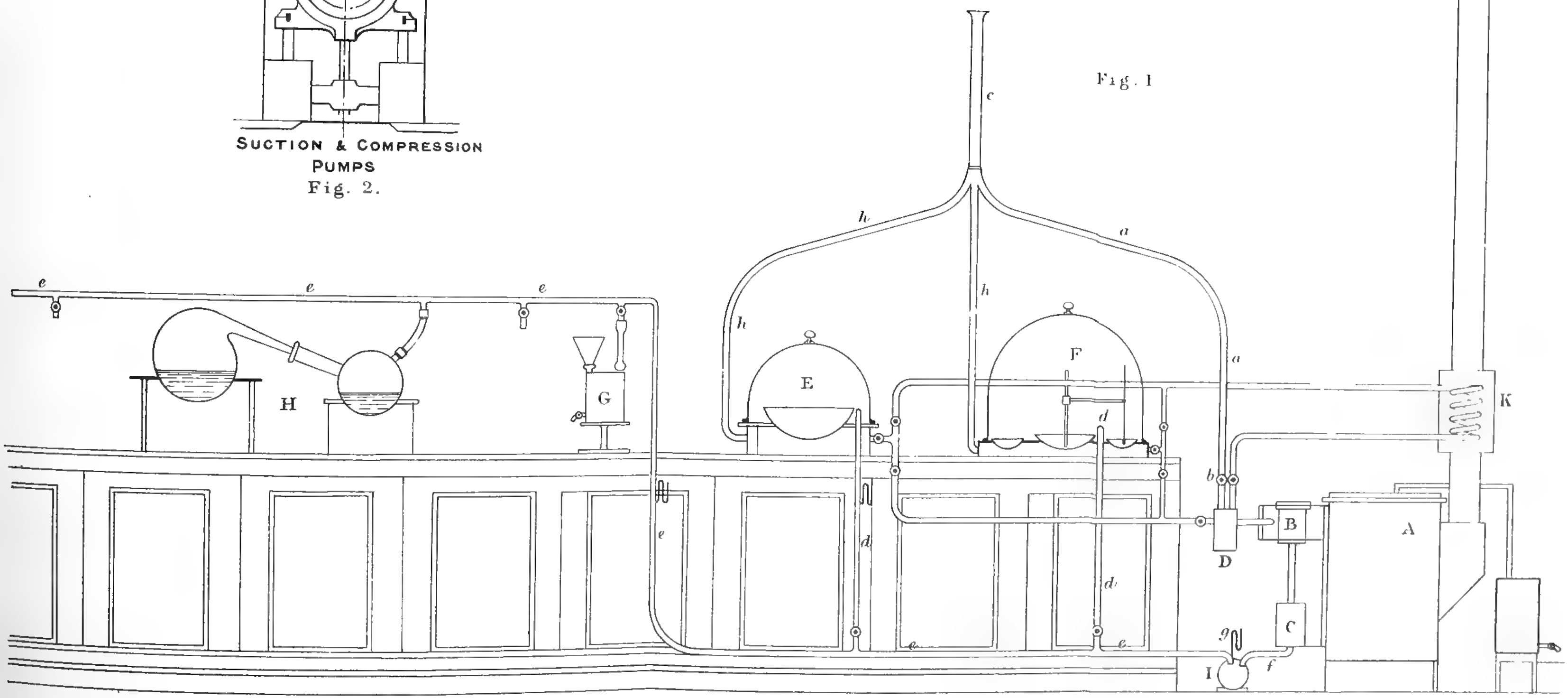

Fig. 1

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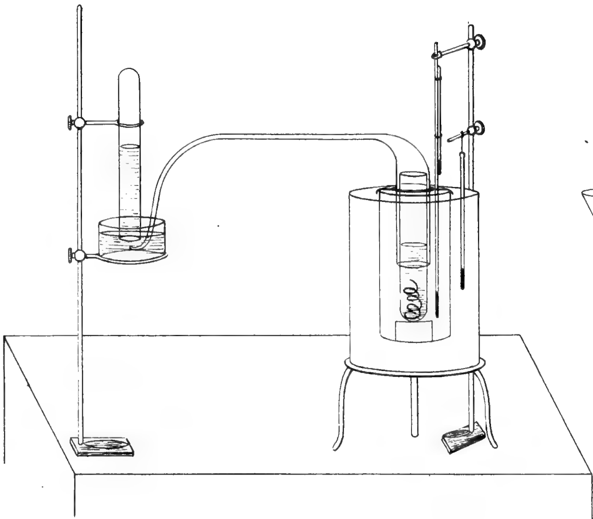

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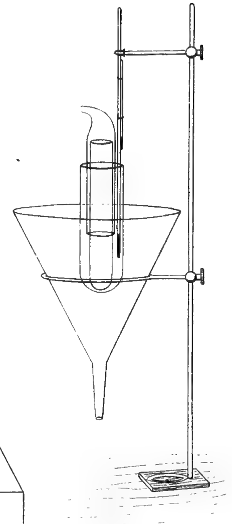

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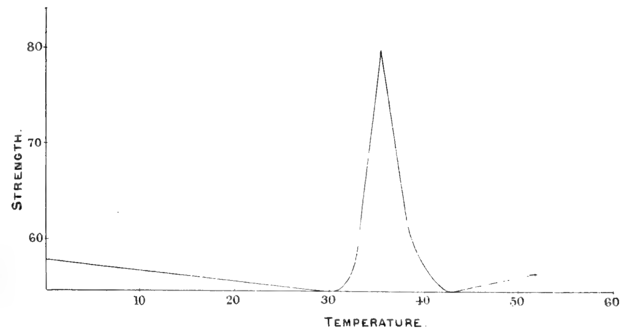

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Errata in No. 98 (August 1883).

Page 147, lines 28, 29, *for* and, on account of disturbances, neglected, *read* and on account of disturbances neglected,

— 152, at foot, *for* $I=\frac{Wr}{L\gamma}g$ read $I=\frac{Wr}{Lb\gamma}g$.

— 156, *instead of the equations*

$$q = AI = \&c.$$
$$q' = AI' = \&c.$$

read

$$q = \frac{AI}{R} = \&c.$$
$$q' = \frac{AI'}{R} = \&c.,$$

where R is the total resistance in circuit;

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Erratum in No. 100 (October 1883).

Page 310, line 1, *for* friction *read* fiction.

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