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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 obsura 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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PLATES.

I. to III. Illustrative of Mr. J. H. Vincent's Paper on the Photography of Ripples.

ERRATA.

Page 50, Surface-tension Table, 1st column, *for* 0·8353 *read* 1·8353.
„ „ Refractive Index Table, 4th column, *for* $\pm 0{\cdot}0_2 00$, *read* $\pm 0{\cdot}0_3 00$.
„ 107, 9th line from foot of page, *after* "salts" *insert* ap and bp.
„ „ 4th line from foot of page, for l_p read l_b.
„ 207, lines 26 and 34, *for* 3/2 *read* 6.
„ 209, line 9, *for* double *read* 3/2.
„ „ „ 11, *for* $2^{-\frac{1}{6}}$ or ·9 *read* $(3/2)^{-\frac{1}{6}}$ or ·93.
„ „ „ 17, *for* $2^{-\frac{2}{3}}$ or ·63 *read* $(3/2)^{-\frac{2}{3}}$ or ·76.
„ „ „ 18, *for* 13 *read* 15
„ 298, in the second table, in the column headed L_i, *for* 80·01 *read* 88 01.
„ 316, line 4 from bottom, for $eB\omega$ read $eBr\omega$.

THE
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[FIFTH SERIES.]

JANUARY 1897.

I. *Electrical Notes.*

By Arthur Schuster, F.R.S.

III. *On the Magnetic Force acting on Moving Electrified Spheres*[*].

SCIENTIFIC opinion inclines to the view that electric conduction in gases and liquids is a process of molecular convection, the moving particles being charged with definite quantities of electricity. If we follow out that hypothesis we meet with a number of problems in which it becomes necessary to calculate the effects of magnetic forces on charged and moving electrified particles, and also the mutual magnetic effects of two or more such particles. To some extent the old question of the behaviour of current elements is thus renewed, although the recognition of "displacement currents" has diminished, if not entirely removed, the ambiguity of the problem.

The question of the magnetic field produced by a moving electrified sphere and the magnetic reactions on the sphere was first attacked in an important paper by J. J. Thomson[†]. Some time afterwards it was reopened by Heaviside[‡], who, "whilst agreeing with J. J. Thomson in the fundamentals," was "unable to corroborate some of his details." J. J. Thomson returns to the same problem in his 'Notes on Recent Researches in Electricity and Magnetism.' As regards the

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[*] Communicated by the Author.

[†] Phil. Mag. vol. xi. p. 227 (1881).

[‡] Ibid. vol. xxvii. p. 324 (1889).

magnetic effects of a moving charge all these investigations have led to the same result, but there is a remarkable discrepancy in the expression for the force which acts on the charge if it is moving in a magnetic field. In his first paper J. J. Thomson calculates that force to be $\frac{1}{2}\mu ep\mathrm{H}$, where μ is the magnetic permeability, e the charge, p the velocity, and H the field, the motion being supposed to take place at right angles to the lines of force. Heaviside omits the factor $\frac{1}{2}$. In his later researches J. J. Thomson calculates the force to be $\frac{1}{3}\mu ep\mathrm{H}$, and I believe he still takes this expression to be the correct one, applying it to some of his experiments on gas discharges. There are also some other discrepancies between J. J. Thomson's first paper and Heaviside's, which are exhibited in a tabular form as follows :—

	J. J. Thomson, 1881.	Heaviside, 1889.	J. J. Thomson, 1893.
T.	$2\mu e^2p^2/15a$	$\mu e^2p^2/3a$	$\mu e^2p^2/3a$
X.	$\frac{1}{2}\mu ep\mathrm{H}$	$\mu ep\mathrm{H}$	$\frac{1}{3}\mu ep\mathrm{H}$
M.	$\frac{1}{3}\mu e_1e_2p_1p_2\frac{\cos\epsilon}{r}$	$\frac{\mu e_1e_2p_1p_2}{r}(\cos\epsilon+\cos\alpha\cos\beta)$	

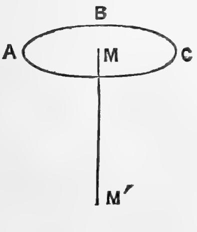
Here T represents the energy of the magnetic field produced by a moving sphere of radius a, and M the mutual energy of a pair of spheres carrying charges e_1, e_2, and moving with velocities p_1, p_2. X is the force already referred to which acts in a magnetic field H on the moving particle.

2. In the "Bakerian Lecture" of 1890, I calculated the effects of a magnetic field on kathode rays, and adopted Heaviside's expression for the force acting on a moving electrified particle. I may here give the simple reasoning which seemed to me to show its correctness.

Let A B C (Fig. 1) be a circular ring made up of a large number of rigidly connected but insulated electrified parts. Let e be the electric charge of each of these parts, and let their number per unit length be N. Let this ring be set into rotation about its axis MM′, the linear velocity being p, and let the two following assumptions be made :—

(1) The magnetic field produced by the convection of the electrified ring is the same as that of a current of strength Nep circulating round a conductor coincident with the ring.

Fig. 1.



(2) The magnetic action of the revolving ring on a magnet

MM′ is equal and opposite to the total reaction of the magnet on the electrified parts of the ring.

The first of these assumptions is supported by experiment, for since Rowland showed that electric convection did produce a magnetic field, he and others have proved that the field cannot differ much in intensity from that of an equivalent current, and is probably identical with it. Both J. J. Thomson's and Heaviside's investigations agree on this point. As regards the second of the above assumptions, it is tacitly made, I believe, by everyone, although the possibility has been pointed out that part of the reaction may take hold of the "medium;" but so far we have no ground to doubt the truth of Newton's third law as applied to *matter* alone.

The current $\mathrm{N}ep$ will produce a force on a magnetic pole of strength m, placed at M, which is equal to $\frac{2\pi m}{r}\mathrm{N}ep$. This will also be the force with which the pole will act on the ring tending to drive it in a direction parallel to its axis; as the magnetic force H at the ring due to this pole is $\frac{m}{\mu r^2}$, and the total number of particles is $2\pi\mathrm{N}r$, it is seen that each particle must be acted on by a force equal to $\mu\mathrm{H}ep$, which is Heaviside's result.

To trace the cause of the discrepancy it is necessary to enter somewhat fully into J. J. Thomson's first paper; but in criticising the correctness of some of its deductions I wish specially to guard myself against the supposition that I do not appreciate the high value of that paper. Criticism after a lapse of years is an easy task compared with the opening out of a new line of thought, when errors of detail are of comparatively little importance. I begin with an independent calculation of the magnetic forces acting on the sphere, assuming J. J. Thomson's values for the magnetic forces which are due to the sphere.

3. If the surface charge of the sphere is q, and the velocity is along the axis of X and equal to w, the components of magnetic force are

$$a=-qwy/r^3, \quad \beta=qwx/r^3, \quad \gamma=0, \quad . \quad . \quad . \quad . \quad (1)$$

where the velocity w is supposed to be small compared to the velocity of light.

To obtain the force which acts on the sphere in a magnetic field H, we may imagine that field to be homogeneous and produced by magnetic matter covering a concentric sphere S of radius R, with a surface-density $\sigma=3\mu\mathrm{H}\cos\gamma/4\pi$, where μ is the permeability and γ the angular distance between P and the pole of H. The force which acts on the moving sphere will be equal and opposite to that which acts on S, and therefore its components are

$$X=-\int \alpha\sigma dS, \quad Y=-\int \beta\sigma dS, \quad Z=0.$$

The values of α and β at the surface S are surface harmonics of the first degree, and the same holds for σ. Hence if C and A are the values of α and σ at the pole of H, the surface integrals may be written down at once, and we thus obtain

$$X=\frac{4\pi R^2}{3} C\, A.$$

If the axis of Y is chosen so that the axis of H lies in the plane of YZ, and θ is the angle between it and the direction of motion of the sphere,

$$X=\mu qv H \sin\theta,$$

$$Y=0, \quad Z=0.$$

This is Heaviside's result.

4. To bring out clearly the cause of the different result obtained by J. J. Thomson it is necessary to form expressions for the mutual energy of the sphere and an outside magnetic system.

The potential energy of a magnet placed in a magnetic field is

$$W=-\iiint(A\alpha+B\beta+C\gamma)\,dx\,dy\,dz, \quad . \quad . \quad . \quad (2)$$

where A, B, C represent the components of intensity of magnetization and α, β, γ, those of the magnetic forces acting on the magnet. If for α and β we substitute their values we find

$$-W=\iiint qv\left(A\frac{d}{dy}\frac{1}{r}-B\frac{d}{dx}\frac{1}{r}\right)dx\,dy\,dz=qvF_3, \quad . \quad (3)$$

where F_3 is the z-component of the vector-potential at the centre of the moving sphere.

We may also consider the energy as kinetic and use the equation

$$T=\frac{1}{8\pi}\iiint(\alpha a+\beta b+\gamma c)\,dx\,dy\,dz, \quad . \quad . \quad . \quad (4)$$

a, b, c being the components of magnetic induction. The expression is deduced under the supposition that the magnetic forces are all due to electric currents. In isotropic media we may therefore write

$$T=\frac{1}{8\pi\mu}\iiint(a^2+b^2+c^2)dx\,dy\,dz. \quad . \quad . \quad . \quad (5)$$

This integral may be transformed either in the manner indicated by Heaviside or in that of J. J. Thomson, and it is found that, retaining only that part of the energy which depends both on the outside magnetic force and the moving sphere, the result is

$$T=qwF_3.$$

5. J. J. Thomson's value differs from this because he wrongly applies equation (4) to the case in which the field is due partly to permanent magnets, substituting for a, b, c their values $\alpha+4\pi A$, &c.

Equation (4) thus becomes

$$T_1=\frac{1}{8\pi}\iiint[(a^2+b^2+c^2)-4\pi(aA+bB+cC)]dx\,dy\,dz. \quad . \quad (6)$$

In the subsequent investigation Thomson puts a, b, c equal to $\mu\alpha$, $\mu\beta$, $\mu\gamma$, where μ is apparently the magnetic permeability of the medium in which the charge moves. It is easily seen that if under these circumstances (6) is compared with (2) and (5), a relation is obtained between T_1, T, and W, viz.,

$$T_1=\mu(T+\tfrac{1}{2}W)=\tfrac{1}{2}\mu T=\tfrac{1}{2}\mu qwF_3.$$

This is J. J. Thomson's value, which differs therefore from that of Heaviside by the factor $\frac{1}{2}\mu$. It follows that J. J. Thomson's forces, which are calculated from this value of the energy, only give half the correct results in a medium of unit permeability; but the appearance of μ in the expression for the energy is alone sufficient to show that there is something wrong with it.

6. It has already been pointed out that in his 'Recent Researches in Electricity and Magnetism,' chapter i., J. J. Thomson obtains a value for the magnetic force which is only a third of that deduced by Heaviside and verified above. I confess I find it difficult to follow the method of "moving tubes" employed in that investigation. I speak with diffidence on the subject, but the investigation on page 22 of the work quoted seems to me to be obscure and incomplete. In the equations for U, V′, W the components f, g, h are taken to represent the polarization of the moving sphere only. Should there not be some additional polarization due to the outside magnetic forces? In the meantime all recognized methods give Heaviside's value for the force which acts on the sphere, viz.

ewH, and it is the only one really consistent with the third law of motion.

7. The energy of the magnetic field established by the moving sphere can be calculated either as Heaviside has done directly from the volume-integral of the square of the magnetic force, or as J. J. Thomson has done from the vector-potential according to Maxwell's equation

$$\tfrac{1}{2}\iiint(A_1C_1+A_2C_2+A_3C_3)\,dx\,dy\,dz. \quad . \quad . \quad . \quad (7)$$

A is the vector-potential, C the current-density, and the indices represent the components along the three axes. To calculate the vector-potential for the system of currents which are assumed to take place owing to the varying displacement in the space surrounding the moving sphere, I consider first the simple case of a sphere of radius a, the surface of which acts like a sink of an incompressible fluid, the total quantity abstracted in unit time being q. The current-density at a distance r from the origin would be $q/4\pi r^2$, and the current components would be

$$q\frac{d}{dx}\left(\frac{1}{4\pi r}\right),\quad q\frac{d}{dy}\left(\frac{1}{4\pi r}\right),\quad q\frac{d}{dz}\left(\frac{1}{4\pi r}\right).$$

Inside the sphere there is no current. Each component of the vector-potential, say A_1, has to satisfy the following conditions:—

$$\nabla^2 A_1 = -\mu q\frac{d}{dx}\frac{1}{r} \quad \text{if } r>a,$$

$$\nabla^2 A_1 = 0 \quad \text{if } r<a.$$

Further, A and its first derivatives are to be continuous at the surface.

Assuming the relation

$$\nabla^2 r^2\phi_n = (4n+6)\phi_n,$$

ϕ_n being a solid harmonic of degree n, the first condition is satisfied by

$$A_1 = \frac{q\mu}{2}r^2\frac{d}{dx}\frac{1}{r} = -\frac{\mu q x}{2r};$$

and to this we must add a solution of $\nabla^2 A=0$ which, together with the value just found, will satisfy the other conditions. I obtain in this way, outside the spherical surface

$$A_1 = \frac{q\mu x}{2r}\left(\frac{a^2}{3r^2}-1\right),$$

and inside the spherical surface

$$A_1 = -\frac{q\mu x}{3a}.$$

Take now two similar spheres at a distance w from each other along the axis of Z, the first acting as a sink, while the second acts like a similar source of electricity; and in order to do away with sinks and sources, unite each point of the first sphere to the corresponding point of the second by means of a straight-line current parallel to the axis of Z, and of such intensity that all currents now flow in closed paths. If w is small, the vector-potential due to the second sphere at any point P will be $-\left(A_1 - \frac{dA_1}{dz}w\right)$, and the complete vector-potential therefore, taking account of convection currents, will be

$$\left.\begin{aligned}
A_1 &= \mu q w \frac{d}{dz}\left[\frac{x}{2r}\left(\frac{a^2}{3r^2}-1\right)\right] = \mu q w \frac{r^2-a^2}{6}\,\frac{d^2}{dz\,dx}\left(\frac{1}{r}\right),\\
A_2 &= \mu q w \frac{d}{dz}\left[\frac{y}{2r}\left(\frac{a^2}{3r^2}-1\right)\right] = \mu q w \frac{r^2-a^2}{6}\,\frac{d^2}{dz\,dy}\left(\frac{1}{r}\right),\\
A_3 &= \mu q w \frac{d}{dz}\left[\frac{z}{2r}\left(\frac{a^2}{3r^2}-1\right)\right] + \frac{\mu q w}{r} = \mu q w \frac{r^2-a^2}{6}\,\frac{d^2}{dz^2}\left(\frac{1}{r}\right) + \frac{2}{3}\,\frac{\mu q w}{r}.
\end{aligned}\right\} \quad . \; (8a)$$

Inside the sphere,

$$\left.\begin{aligned}
A_1 &= A_2 = 0,\\
A_3 &= \frac{2}{3}\,\frac{\mu q w}{a}
\end{aligned}\right\} \quad . \; . \; . \; . \; . \; . \; . \; . \quad (8b)$$

The condition $\frac{dA_1}{dx} + \frac{dA_2}{dy} + \frac{dA_3}{dz} = 0$ is necessarily fulfilled as the currents have been made to flow in closed paths. The currents which are defined by equations (8*a*) and (8*b*) are in the outside space

$$C_1 = -\frac{1}{4\pi\mu}\nabla^2 A_1 = \frac{qw}{4\pi}\,\frac{d}{dz}\cdot\frac{d}{dx}\left(\frac{1}{r}\right),$$

$$C_2 = -\frac{1}{4\pi\mu}\nabla^2 A_2 = \frac{qw}{4\pi}\,\frac{d}{dz}\cdot\frac{d}{dy}\left(\frac{1}{r}\right),$$

$$C_3 = -\frac{1}{4\pi\mu}\nabla^2 A_3 = \frac{qw}{4\pi}\,\frac{d^2}{dz^2}\left(\frac{1}{r}\right).$$

But these are the current-densities which J. J. Thomson takes as the basis of his investigation. The vector-potential given by (8*a*) and (8*b*) therefore represents the solution of the problem if q measures the charge of the sphere and w the velocity. These equations differ from J. J. Thomson's by a quantity which does not affect the magnetic forces, but which does affect the energy if calculated according to equation (7). It can be shown that equations (8) are the only ones which satisfy the conditions of the problem, and give J. J. Thomson's values for the magnetic forces. Owing to our ignorance of what actually happens at the surface of the sphere when the medium passes suddenly from a condition in which there is no displacement to one in which there is a displacement, and *vice versâ*, the problem itself has a certain ambiguity; and we might possibly have to introduce some surface-currents at the sphere itself. But these surface-currents would affect the values of the magnetic forces as well as the vector-potential. What I contend for is that equations (8) are the only ones consistent with the values of the magnetic forces as defined by (1).

8. The energy of the field is now easily found with the help of the following theorem:—Let a system of electric currents be everywhere deducible from a current potential ϕ so that $C_1 = -\frac{d\phi}{dx}$ &c. The potential ϕ is supposed to be continuous throughout space and small, of the order $\frac{1}{R^2}$, at an infinite distance; but the differential coefficients of ϕ may be discontinuous at one or more surfaces. Let A_1, A_2, A_3 represent the components of a vector vanishing at an infinite distance and satisfying everywhere the condition of no divergence, but not necessarily continuous; then

$$\iiint (A_1C_1 + A_2C_2 + A_3C_3)\, dx\, dy\, dz = 0.$$

To prove this it is only necessary to change the form of the integral; calling l, m, n the direction-cosines of the normal at the limits of the space to which the integral is applied, a well-known transformation gives

$$-\iiint\left(A_1\frac{d\phi}{dx} + A_2\frac{d\phi}{dy} + A_3\frac{d\phi}{dy}\right)dx\, dy\, dz = \iint (lA_1 + mA_2 + nA_3)\phi dS$$
$$+\iiint\left(\frac{dA_1}{dx} + \frac{dA_2}{dy} + \frac{dA_3}{dy}\right)dx\, dy\, dz.$$

The second integral on the right-hand side vanishes everywhere, in consequence of the condition of no divergence, and the surface integral when taken over both sides of all surfaces at which $\frac{d\phi}{dx}$ is discontinuous will also vanish, provided ϕ itself is continuous.

Turning back to the system of currents considered in § 7, it is seen that, leaving out of account the straight-line currents, the above theorem becomes applicable, for the electric currents are now deducible from a potential ϕ. Hence that part of the energy which depends on displacement currents vanishes, and it is therefore sufficient to consider the convection currents alone. These vanish everywhere except at the surface of the sphere, where the vector-potential reduces to $A_1 = \frac{2qw}{3a}$. Hence

$$T = \frac{1}{2}\iint A_1 C_1 \, dS = \frac{1}{2}\iint \frac{2}{3}\,\frac{\mu qw}{a}\,\frac{qw}{4\pi a^2}\, dS,$$

$$= \frac{\mu q^2 w}{3a}.$$

This is a result identical with that deduced by Heaviside, as also with that obtained by J. J. Thomson in his 'Recent Researches.' The different value obtained by Thomson in his first paper is partly due to the wrong value of the vector-potential he adopted, and partly to his omission of the convection curents in calculating the energy. If the correct vector-potential had been taken, the neglect of the convection currents would have led to the result that the total energy is zero.

9. We may extend the investigation to the case where the charge of the sphere varies according to some spherical surface harmonic Y_n. Let the surface-density be given by

$$4\pi a^2 \sigma = (2n+1) Y_n.$$

The electric potential due to such a surface distribution will be $\frac{r^4}{a^{n+1}} Y_n$ inside the sphere, and $\frac{a^4}{r^{n+1}} Y_n$ outside the sphere. Let B be the vector-potential of electric currents which proceed along the lines of force with current intensities numerically equal and opposite everywhere to the displacements, that is to say, such currents as in unit time would destroy the charge of the sphere; then by the same reasoning as in § 7 we find that the vector-potential produced by the displacement and convection currents of a sphere charged as above, and moving with velocity w in the direction of the axis of Z, would be given by

$$A_1 = w\frac{dB_1}{dz}, \quad A_2 = w\frac{dB_2}{dz}, \quad A_3 = w\frac{dB_3}{dz} + w\int\frac{\sigma dS}{r}.$$

The term $\int\frac{\sigma dS}{r}$ represents the effect of the convection current, and becomes $\frac{r^n}{a^{n+1}}\mathrm{Y}_n$ and $\frac{a^n}{r^{n+1}}\mathrm{Y}_n$ inside the sphere and outside respectively. The vector B is determined by the condition that it must be continuous with its first derivatives at the surface of the sphere, and must satisfy the condition

$$\nabla^2\mathrm{B}_1=-\mu\frac{d}{dx}\frac{r^n}{a^{n+1}}\mathrm{Y}_n=-\mu\Psi'_n \text{ inside the sphere,}$$

and

$$\nabla^2\mathrm{B}_1=-\mu\frac{d}{dx}\frac{a^n}{r^{n+1}}\mathrm{Y}_n=-\mu\Psi'_{-n-1} \text{ outside the sphere.}$$

Ψ'_n and Ψ'_{-n-1} are solid harmonics of degree $n-1$ and $-n-2$ respectively. The conditions are satisfied by

$$\mathrm{B}_1=\frac{\mu}{2}\frac{d}{dx}\left(\frac{r^2}{2n-1}-\frac{a^2}{2n+3}\right)\Psi_{-n-1} \text{ outside the sphere,}$$

and

$$\mathrm{B}_1=\frac{\mu}{2}\frac{d}{dx}\left(\frac{a^2}{2n-1}-\frac{r^2}{2n+3}\right)\Psi_n \text{ inside the sphere.}$$

The complete vector-potential of the moving sphere is therefore in the outside space

$$\mathrm{A}_1=\frac{\mu w}{2}\frac{d^2}{dz\,dx}\left(\frac{r^2}{2n-1}-\frac{a^2}{2n+3}\right)\Psi_{-n-1}.$$

$$\mathrm{A}_2=\frac{\mu w}{2}\frac{d^2}{dz\,dy}\left(\frac{r^2}{2n-1}-\frac{a^2}{2n+3}\right)\Psi_{-n-1}.$$

$$\mathrm{A}_3=\frac{\mu w}{2}\frac{d^2}{dz\,dy}\left(\frac{r^2}{2n-1}-\frac{a^2}{2n+3}\right)\Psi_{-n-1}+\mu w\Psi_{-n-1}.$$

The expressions for the inside space are obtained by writing Ψ_n for Ψ_{-n-1}, and interchanging r and a.

In the calculations for the magnetic forces the last term of A_3 is the only one which produces an effect, and, consequently,

$$\alpha=w\frac{d}{dy}\frac{a^n}{r^{n+1}}\mathrm{Y}_n,\quad \beta=-w\frac{d}{dx}\frac{a^n}{r^{n+1}}\mathrm{Y}_n,\quad \gamma=0.$$

Let Ψ^σ_n represent the harmonic of degree n and type σ, *i.e.* with the usual notation

$$\frac{r^n}{a^{n+1}}\cos\sigma\phi\,\sin^\sigma\theta\,\frac{d^\sigma \mathrm{P}_n}{d\mu^\sigma};$$

and Ψ^s_n the corresponding harmonic

$$\frac{r^n}{a^{n+1}}\sin\sigma\phi\,\sin^\sigma\theta\,\frac{d^\sigma \mathrm{P}_n}{d\mu^\sigma}.$$

Then if $x=r\sin\theta\cos\phi$, $y=r\sin\theta\sin\phi$, $z=r\cos\theta$, it may be proved that

$$2\frac{d\Psi_n^\sigma}{dx}=(n+\sigma)(n+\sigma-1)\Psi_{n-1}^{\sigma-1}-\Psi_{n-1}^{\sigma+1},$$

$$2\frac{d\Psi_n^\sigma}{dy}=-(n+\sigma)(n+\sigma-1)\Psi_{n-1}^{s-1}-\Psi_{n-1}^{s+1}.$$

These equations hold both for positive and negative values of n, which seems a somewhat remarkable result[*]. Hence

$$2\frac{d\Psi_{-n-1}^\sigma}{dx}=(n+2-\sigma)(n+1-\sigma)\Psi_{-n-2}^{\sigma-1}-\Psi_{-n-2}^{\sigma+1},$$

$$2\frac{d\Psi_{-n-1}^\sigma}{dy}=-(n+2-\sigma)(n+1-\sigma)\Psi_{-n-1}^{s-1}-\Psi_{-n-2}^{s+1}.$$

From these equations the values of the magnetic forces are obtained in their normal form. The first terms vanish for the zonal harmonics for which $\sigma=0$; and the second terms vanish inside the sphere when $n<\sigma+2$. For the case of a uniform charge we must put $n=0$, $\sigma=0$, and obtain the values previously found. The magnetic forces on any sphere being of the nature of surface harmonics the expression for the electromagnetic energy of the field is also calculated without difficulty[†].

II. *Boyle's Law at Very Low Pressures.*
By William Sutherland[‡].

Great interest attaches to the question whether gases at lower and lower pressures show more and more rigorous conformity with Boyle's law, or, as some experimenters have maintained, after approximating to Boyle's law as a limit up to a certain degree of rarefaction, at still lower pressures depart from it. For the kinetic theory gives no hint that departure from Boyle's law is to be looked for at low pressures; so that if such a departure existed it would point to a new property of matter uncontemplated in the kinetic theory.

Usually it is supposed that surface-condensation of gases on

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[*] The equations are easily generalized to give very simple expressions for the pth differential coefficient of a tesseral harmonic in terms of tesseral harmonics.

[†] Reference should have been made to a second paper by J. J. Thomson (Phil. Mag. 1889, vol. xxviii. p. 1), in which possible effects are taken into account, for which there is at present no experimental evidence. The above investigation shows that the difference between the results of Heaviside and J. J. Thomson's original paper are not due to the effects discussed in his second paper.

[‡] Communicated by the Author.

the walls of containing vessels is likely to produce apparent departure from Boyle's law, becoming more conspicuous at low densities, because the mass condensed is supposed to become a larger fraction of the total mass the lower the density. It is proposed to show in this paper that it is not necessary that the effect of surface-condensation should be more appreciable at low densities than at high, and that the departures from Boyle's law in rare gases hitherto investigated are due to special circumstances and not to any general failure of the laws of gases at very low pressures.

In the paper on "Thermal Transpiration and Radiometer Motion"* it was incidentally shown that in a gas approximately obeying Boyle's law the pressure p at a distance z from a surface of a solid is connected with the pressure p_a at distance z_a by the relation

$$\log p/p_a = \frac{6A\pi\rho_2}{v_1^2}\log z_a/z, \quad . \quad . \quad . \quad . \quad (20)$$

where $3Am_1m_2/r^4$ is the attraction between a molecule of gas m_1 and of solid m_2 at distance r, ρ_2 being density of solid and v_1 velocity of m_1. This may be written

$$p/p_a = (z_a/z)^{\beta/k}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (29)$$

denoting $6Am_1\pi\rho_2$ by β, and $m_1v_1^2$ by k.

When we wish to carry this expression right up to the layer of molecules nearest to the solid wall, to find the the pressure there, we have to take z as having a value z_s such that the attraction on a continuous normal cylinder ending at z_s from the surface will equal the attraction on the discontinuous molecules in that cylinder; thus z_s will probably not be much different from half the mean distance of a molecule from its immediate neighbours near the solid surface. Now the domain of a molecule near the surface is (m_1/ρ_s); so that z_s is not much different from $(m_1/\rho_s)^{\frac{1}{3}}/2$. Then, if Boyle's law holds,

$$\rho_s/\rho = \left\{2z/(m_1/\rho_s)^{\frac{1}{3}}\right\}^{\beta/k} \quad . \quad . \quad . \quad . \quad . \quad (29a)$$

is an equation specifying the density at the surface when that at any distance z is known; but in most cases ρ becomes practically constant when z exceeds a certain small value, and when it is determined the whole distribution of density in the transition layer of variable density is specified.

To determine the mass of gas in a vessel, take an element of surface dS and erect a normal cylinder of height z starting at $z_s/2$ from the surface and reaching to $z+z_s/2$; with these

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* Phil. Mag. [5] xlii. p. 389.

as limits of integration, the mass of the cylinder is

$$d\mathrm{S}\int\rho dz = d\mathrm{S}\int\rho_a(z_a/z)^{\beta/k} = d\mathrm{S}\int\rho_a(z_a/z)^{\beta/k}dz$$

$$= \frac{d\mathrm{S}}{1-\beta/k}\{\rho z - z_s(\rho_s-\rho)/2\}. \quad . \quad . \quad . \quad . \quad . \qquad (30)$$

Now if z is large enough to end in the main body of the gas, where surface-action is negligible, $d\mathrm{S}\rho z$ is $d\mathrm{B}$, where B is an element of volume; so the mass of the cylinder becomes

$$\{\rho d\mathrm{B} - d\mathrm{S}z_s(\rho_s-\rho)/2\}/(1-\beta/k),$$

and total mass

$$\mathrm{M} = \{\rho\mathrm{B} - \mathrm{S}z_s(\rho_s-\rho)/2\}/(1-\beta/k). \quad . \quad . \qquad (31)$$

Here, then, we have the surface condensation effect expressed by a surface-term in the expression for the mass in addition to the $\rho\mathrm{B}$, which would suffice if the density were uniform; ρ_s is given by (29 a).

Now, without going into details, we may be satisfied that β/k is small compared to unity; for if k is expressed in C.G.S. units, β is to be the force exerted by a long cylinder of square centimetre base on a molecule of the gas at a centimetre from the base along the axis, which all experience shows to be small compared to the kinetic energy of a molecule at ordinary temperatures. Of course, if we artificially increase the amount of surface enormously—as, for instance, by partially filling a vessel with a powder or a porous material like charcoal—we must remember that we cannot assume the pressure in the centre of each little interspace or pore to be the same as that in the free gas in communication with the manometer; but for each little interspace we can write an equation like (31), and for the gas in the whole of the interspaces we can use equation (31) if we change ρ to ρ_c, the average density at the centre of the interspaces. As β/k is small, it follows from (31) that the surface-term is always small compared to the volume-term, and therefore the equation for a mass of gas in a vessel which is partially occupied by a powder or porous material will be

$$\mathrm{M} = \rho_f\mathrm{B}_f + \rho_c\mathrm{B}_c, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (32)$$

where the suffix f connects with the free volume, and c with that in the interspaces. The relation between ρ_c and ρ_f depends on the average size of the interspaces and the attraction of the solid for gas, as we can see if we follow in imagination the course of a surface of constant pressure at the boundary between the free region and the powder: if the pores are large this surface may pass into the pore and clothe all the interspaces; whereas if the pores are fine it may show only a slight depression opposite a surface-pore and have no existence in the inner interspaces. Thus it would not be difficult to find the surface of constant pressure which is just prevented from entering an average pore, and so determining ρ_c as a function of ρ_f, size of pore, and molecular force, and so to find the theory of Kayser's experiments on the compression of gases in contact with powders (Wied. *Ann.* xiv.). For present purposes it is enough to say that by varying the size of the interspaces and the nature of the powder or fibre he was able to demonstrate distinct variation of the mass of gas adsorbed, namely $\rho_c B_c$, with NH_3, CO_2, and SO_2. With boxwood charcoal (Wied. *Ann.* xii.) Kayser, in confirmation of previous investigators, such as Chappuis and Joulin, found a measurable adsorption of both air and hydrogen; for instance, 1 c.c. of charcoal, of which about two-thirds is interspace, adsorbs about 3 c.c. of air at a pressure of one atmo and at 0° C. and about 1·5 c.c. of hydrogen at 14° C., but at 50° scarcely a trace of air is adsorbed—a fact which shows that the circumstances in the case of charcoal are quite special, and though interesting can throw no light on the present inquiry as to the general action of solid surfaces on gases.

If we return to equation (31) in the form

$$M = \rho B + M\beta/k - Sz_s(\rho_s - \rho)/2, \quad . \quad . \quad . \quad . \quad (33)$$

we see that in general the surface-term must be smaller than $M\beta/k$—that is to say, than a constant small fraction of the mass for a given gas—and therefore that however much we may rarefy a gas the relative importance of the surface-term can never exceed a certain small limiting amount. This is in direct contradiction to the prevailing impression that surface effect becomes of increasing importance with the rarefaction. We have therefore to consider the origin of the general impression, which is twofold, namely, first, the theoretical belief that the mass of a gas ought to be given by an equation of the form

$$M = \rho B + S(\rho_s - \rho),$$

where, in contrast to our equation (33), the surface-term has a positive sign, and there is no obvious reason why the surface-term ought not to become of increasing importance with rarefaction because of ρ_s diminishing more slowly than ρ; and secondly, the well-known experimental difficulties of getting rid of the last traces of volatile matter from the walls of glass vessels in preparing vacuum bulbs; but the experiments of

Warburg and Ihmori (Wied. *Ann.* xxvii., xxxi.) and others show that there is a chemical reason for part of the strong attraction of glass for water, since fresh glass has an alkaline film on its surface which can be dissolved off by water—a fact which explains the power that fresh glass surfaces have of condensing moisture from air which is far from being saturated. This water is easily got rid of, but there is water attached more firmly which can be driven off a glass surface only by heating at very low pressure. It seems to me that we have to do here with a sort of solution of water in the solid glass, and not with a genuine surface condensation; but the behaviour of water on a glass surface has been generally held to be only an extreme case of the behaviour of any gas; if, however, there is the great distinction which has just been suggested, there is no force in the inference from water. Carbon dioxide also comes freely from a glass surface sometimes, but Krause (Wied. *Ann.* xxxvi.) seems to have shown that the presence of water-vapour is necessary. Thus there seems to be no genuine evidence that gases condense in amounts hitherto measurable on ordinary solid surfaces. Of course, with an easily liquefiable gas below its critical temperature, we can imagine that if the attraction of the solid for the gas is much greater than the attraction of the gas for itself, a thin layer of gas liquefied on the surface would evaporate with greater difficulty than if it were on the surface of a large mass of itself; but from what we now know of molecular force we should expect such an effect merely to modify somewhat for such a layer the ordinary laws of evaporation, but not to alter them entirely, as the supposition of a retention of a layer of liquid at pressures far below that of saturation would necessitate; the retention of an appreciable amount of volatile substance, as of water, on glass brings us back to causes more of a chemical nature, as we have just seen.

Thus, with the law of molecular force as that of the inverse fourth power, we are led to the conclusion that condensation of gases on ordinary solid surfaces at pressures far removed from those of liquefaction does not occur to an extent measurable by methods hitherto applied, and that apparent departure from Boyle's law on account of surface condensation is too small to have been hitherto detected, and does not become relatively any larger in rare than in dense gases. The causes for any apparent breakdown of Boyle's law in rarefied gases are therefore to be sought for elsewhere than in surface condensation caused by molecular force if the law of that force is that of the inverse fourth power.

We have now to consider the experimental evidence, and will begin with the clearest case—that of oxygen, for which Bohr discovered (Wied. *Ann.* xxvii.) a decided departure from Boyle's law, and investigated it between pressures of 15 and ·1 mm. of mercury. As this departure in the case of oxygen is due to its spontaneous change into ozone at low pressures, it will be investigated in a separate paper. But since air consists of about one volume of oxygen to four of nitrogen, it is to be expected that the departure of the oxygen from Boyle's law will produce a departure of the air from that law, but at a region of pressures five times as large as those in which the deviations are perceptible for oxygen. The case of air will therefore better be discussed in connexion with that for oxygen. But when once the departure of air from Boyle's law is assigned to a special cause, the bulk of the experimental evidence in favour of the departure of rarefied gases in general from that law has disappeared, for naturally most of the experimental evidence has related to air. That which has attracted most attention is due to Mendeléeff, who seems to have devoted a large amount of work to the subject, but accessible to non-Russian readers only in two short accounts in the *Ann. de Chim. et de Phys.* (Mendeléeff and Kirpitscheff [5] ii., and Mendeléeff and Hemilian [5] ix.). The most important result obtained in the first of these papers is that between 600 and 20 mm. of mercury pB for air diminishes with diminishing p, and a series of numbers is given to illustrate the case; but in the next paper it is mentioned that while diminution of pB with p between these limits of pressure is always found, the determinations of its amount are not always accordant. The reason for this variation in the experimental numbers will be given when we are considering the theory of the departure in oxygen and air. Of course it is to be remembered that the departures of most gases from Boyle's law at a few atmos pressure are such as make pB increase with diminishing p, but towards the limit for a perfect gas; if, therefore, any tendency in the opposite direction is to set in it must first neutralise the small residual tendency of pB to increase with diminishing p before it can make itself seen as an actual diminution, and this is why Mendeléeff and his co-workers find a pressure of about 600 mm. as the one at which diminution of pB begins to be apparent with increasing rarefaction. The same investigators give data to show that with H_2, CO_2, and SO_2 a diminution of pB with p sets in at a certain pressure for each, but the amount of diminution obtained is so slight that it can hardly be relied on. Indeed Amagat, after carrying out a research on Boyle's law in rarefied gases (*Ann. de Chim. et de Phys.* [5] xxviii.), maintains that Mendeléeff and his co-workers claim for their measurements of low pressures a degree of accuracy that is beyond present experimental possibility. He claims that his data show that with present appliances one cannot read the position of the top of a mercury column to within less than ·01 cm., which implies that at 20 mm. pB cannot be free from liability to an error of 5 parts in 10,000, which is of the order of the departure given by Mendeléeff and Hemilian for SO_2; and while they obtain slight diminution of pB for H_2 down to 120 mm., Amagat gets an apparent slight increase at 6 and 3 mm., which, however, he speaks of as being of the order of the unavoidable experimental error. Similarly, in the case of CO_2 Amagat's results show that at 4 and 2 mm. a departure from Boyle's law greater than experimental error could not be found by his measurements. Thus it appears that when oxygen and air are set aside as exceptions traceable to a special cause, there is no satisfactory evidence of a failure of Boyle's law in rarefied gases.

The latest experimental paper on the subject is that of Baly and Ramsay (Phil. Mag. [5] xxxviii.), the chief interest of which lies in some startling experiences which they had with oxygen and air, wherein Boyle's law appears to be completely wrecked; for example, air which at 4·1 mm. of mercury gave $pB=100$, at 8 mm. gave $pB=9{\cdot}4$. This will be traced in the separate paper to a liberation of free ions of oxygen, whose electrical charges are responsible for the mischief. But with hydrogen Baly and Ramsay found in a somewhat indirect manner that Boyle's law holds down to 2·5 mm. of mercury, and probably to $760/10^7$, and the law of Charles down to ·4 mm. CO_2 behaved differently in two different gauges, showing probably a different state of the glass in them, and indicating that apparent departure of rare CO_2 in glass vessels from Boyle's law is due rather to the chemical causes already mentioned than to physical ones. Indeed, some other experiments carried out by Baly and Ramsay are suggestive of the existence of a dissociation pressure for the combination between CO_2 and glass; for they found that when the pressure of CO_2 was about $1/10^6$ atmo, 188 strokes with a mercury-pump, supposed to reduce the pressure to ·55 of its original value at each stroke, failed to lower the pressure at all, just as if the glass gave out and reabsorbed CO_2 in the same way as a vessel lined with silver chloride and filled with NH_3 at a suitable pressure would liberate and absorb NH_3 if an attempt were made to pump it out with apparatus all lined with AgCl.

Baly and Ramsay consider the coefficient of expansion of hydrogen is proved by their experiments to remain at 1/273 down to a pressure of ·4 mm., below which there are the three observations of 1/276 at ·25 mm., 1/297 at ·096 mm., and 1/300 at ·077 mm., which they consider to prove a diminution of the coefficient with diminution of pressure; but it seems to me that the sudden onset of the diminution is suggestive of some unrecognized source of experimental error, such, for instance, as electrification of the glass of one of the gauges with alteration of the capillary correction for the meniscus of mercury in the capillary volume-tube which amounts to 7 mm. of mercury. An error due to such a cause would, of course, produce more effect in altering the apparent coefficient of expansion the lower the pressure.

For nitrogen Baly and Ramsay found the following reciprocals of the coefficients of expansion:—

p	5·3 mm.	4·97	3	1·1	·8	·6
	304	309	302	304	331	from 301 to 377 Mean 343

Although these values all mean coefficients smaller than 1/273, it seems to me that the sudden change from 304 at 1·1 mm. to 331 at ·8 mm., so analogous to the sudden change with hydrogen near the same region of pressure, is suggestive of a hidden source of error such as that already indicated, and casting doubt on the measurements even at 5 mm. Baly and Ramsay mention that at pressures below ·6 mm. some unascertained cause of irregularity came in and caused their method to give worthless results—a fact which is again suggestive of the irregular operation of a source of error such as variable electrification. With oxygen, Baly and Ramsay found coefficients of expansion larger than 1/273 at pressures from 5 to ·07 mm., which in the special paper on oxygen will be accounted for by dissociation of ozone at the higher temperature. If the coefficient for nitrogen is really smaller than 1/273 at low pressures, this would be most readily accounted for by association of the nitrogen molecules at the higher temperature with possible production of an allotropic form of nitrogen; therefore, on all accounts, a continuation of Baly and Ramsay's research is desirable.

The contentions of the present paper may be summarized thus:—Departures from Boyle's law found experimentally in rarefied gases may be due either to a true failure of Boyle's law in the highest sense, that is to say, to a change in the kinetics of all molecules, or to only an apparent failure, which may be due to actions at the walls of the containing vessel or dissociation of the molecules. According to the law of the inverse fourth power, apparent departures from Boyle's law on account of pure molecular attraction of solid walls for the gas cannot be of amount at present detectable in ordinary vessels, but are possible in small interspaces between solids, as in powders or porous bodies, if the interspaces are small enough. As the kinetic theory gives no hint that the kinetics of molecules should alter with rarefaction of gases, except towards stricter and stricter rigorousness of Boyle's law, and as the theory of molecular force requires a very small effect for the walls of ordinary vessels, we may say that theoretically there is no cause to expect real or apparent departures from Boyle's law at low pressures on general grounds, and that when such are obtained experimentally they are due to special causes, such as a chemical or quasi-chemical attraction of glass for water or moist CO_2, or dissociation or combination, as in the case of oxygen (to be proved in a separate paper), and therefore in that of air. The case of hydrogen may be regarded as the typical one hitherto investigated that is free from special conditions, and the whole tenour of the experimental evidence relating to it is to justify the laws of Boyle and Charles for it when rarefied.

The direction which further experimental inquiry should take is obviously that of varying the conditions more widely than hitherto.

Although the amount of direct experimental evidence in favour of Boyle's law in rare gases has been shown to be small, it seems to be cogent; and in my next paper, on "Two New Pressure-Gauges," I shall show that it is well supported by indirect evidence obtainable from Crookes's experiments on High Vacua.

Melbourne, Aug. 1896.

III. *A Satisfactory Method of Measuring Electrolytic Conductivity by means of Continuous Currents. By* Prof. W. Stroud, *D.Sc., M.A., and* J. B. Henderson, *B.Sc.*[*]

THE devices for eliminating or reducing the disturbing effects of polarization in the measurement of the conductivity of electrolytes are very numerous. Wheatstone, Horsford, Wiedemann, Beetz, Paalzow, Ewing and Macgregor, Bouty, and others have all experimented on this subject. It will, however, be universally conceded that

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[*] Communicated by the Physical Society: read Oct. 30, 1896.

Kohlrausch's method of measuring the conductivity of electrolytes by the use of alternating currents and a telephone is superior to any method at present in use in which continuous currents are employed. That there are difficulties in the use of this method, arising from self-induction, capacity, &c., is admitted by Kohlrausch himself. Ever since the introduction of this method in 1875 physicists have not ceased to try to improve the continuous current methods, with the desire unquestionably of employing a galvanometer instead of a telephone as the indicating apparatus. In the method used by Fitzpatrick*, where the alternating currents are confined to the four arms of the bridge, we have an example of a successful attempt to utilize the advantages of alternating currents while dispensing with the telephone in favour of the more satisfactory galvanometer.

In the face of all the experimental work done in the past it may seem a bold thing to say that if the proper conditions are only satisfied, continuous currents are preferable in every way to alternating currents for the measurement of electrolytic conductivity, but such we believe to be the fact.

The idea underlying the method to be described struck one of us some six months ago. This idea was to obviate the detrimental effects of polarization in the electrolytic cell by inserting a second cell with the same size of plates &c., but with a very different length of electrolytic conductor in the corresponding arm of a Wheatstone bridge circuit.

The notion of employing such a balancing cell we subsequently found was suggested in 1877 by Kohlrausch and employed by Tollinger †.

The method used by Tollinger consisted in having an electrolytic cell in each arm of the bridge, but the method of finding the resistance was to obtain a balance for a particular position of the electrodes in one cell, then to diminish the distance between them by a measured amount and increase the resistance in that arm so as to restore the balance. A conceivable source of error arises in consequence of the motion of the electrode through the liquid producing a possible alteration in its polarization.

The results of Tollinger's work show that resistances measured by his continuous current method were 0·6 per cent. lower on the average than the same resistances measured by the alternating current method.

Elsas ‡ has also employed the balancing cell method. He

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* Brit. Assoc. Rep. 1886, p. 328.

† Wied. *Ann.* vol. i. 1877, p. 510.

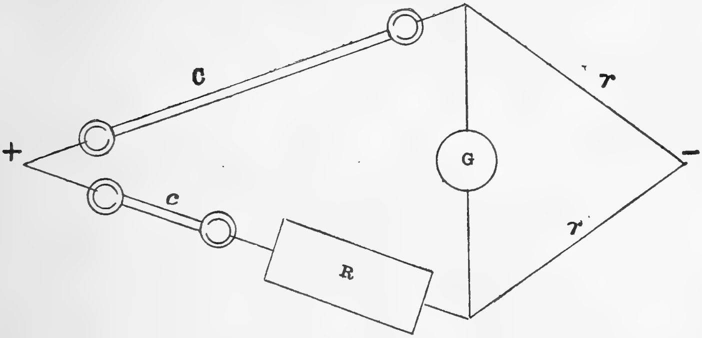
‡ Wied. *Ann.* xliv. p. 666.

used a long trough of known section with two fixed non-polarizable electrodes at the ends (Cu electrodes in $CuSO_4$) and one movable electrode between them. A resistance in series with the shorter column of liquid was adjusted to balance the bridge. The resistances so measured were of the order of 12 ohms or less. For many purposes the use of non-polarizable electrodes is impossible or inconvenient.

The method to be described is in many respects similar to those of Tollinger and Elsas, but it differs from them in the form of electrolytic cell employed, and especially in a very material point, viz.:—the employment of high voltages and high resistances so as to effectually drown any residual error arising from differential polarization.

Fig. 1 will make the arrangement clear, *r r* are equal resistances, C *c* are two similar electrolytic cells equal in all respects, except that in C the electrolytic conductor is very long, in *c* very short. The resistance-box R is adjusted till there is a balance, when, of course, the resistance in the box is equal to the difference in the resistances between the two cells C and *c*. Since the resistances *r r* are equal it will be clear that when approximate balance has been obtained equal currents will be traversing both electrolytic cells, and therefore there should be the same polarization in each cell, and these polarizations are clearly opposed and are in theory eliminated.

Fig. 1.



Instead of the arrangement shown in fig. 1, in which the cells are arranged in parallel, we have also tried the effect of interchanging the battery and galvanometer so as to arrange the cells in series. In the first case the polarization of one cell is opposed to that of the second, so that the polarization of each cell is much more persistent; in the other case the cells are joined in series, so that the polarization falls as soon

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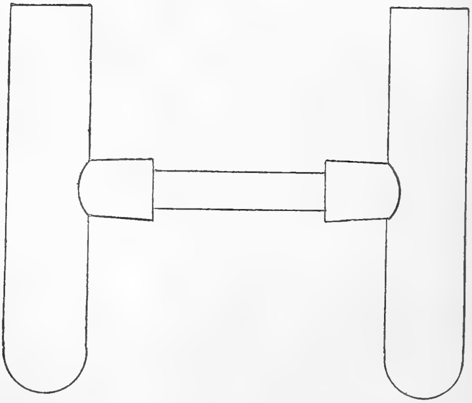
as the battery circuit is broken. Experiment shows that the arrangement shown in the figure is much the better of the two.

In practice, however, it is found that the polarization in the two cells is not always exactly the same; generally speaking at least 99 per cent. of the polarization may be balanced in this manner, but ordinarily the balance is much more perfect than this. The next point then is to drown the residual differential polarization by working with high resistances and high voltages, and in this way the detrimental effects of polarization are reduced to insignificance. With electrolytic cells having a difference in resistance of about 20,000 ohms, the remaining arms being 1000 ohms each, and with a D'Arsonval galvanometer of about 300 ohms, when the voltage on the bridge is 30 the measurements are very like measurements of metallic resistances.

To get the specific resistance immediately in ohms, without reference to any other electrolyte, the cells C and *c* are constructed as shown in the figure.

Each cell consists of three parts, two being small, thick-walled test-tubes with necks half-way up their sides. Into these necks fit the well-ground ends of a tube of nearly uniform bore. In the case of the cell C this tube is 30 centim. or more in length, while in the case of *c* it is only a few centim. long. In every other respect the two cells are as nearly as possible alike.

Fig. 2.



The vertical limbs of the H cells were about 1·2 centim. in diameter, and about 6 centim. high, and the horizontal tubes about 0·6 centim. external diameter, the diameter of the bore being chosen to give a convenient resistance with the electrolyte used. The lengths of the tubes were about 30 centim. in the one case and 5 centim. in the other. The tubes were calibrated by measuring their lengths and weighing in a watch-glass the mercury required to fill them. In this way a constant can be obtained for a particular pair of tubes such that on multiplying by the observed resistance of the electrolyte its specific resistance is at once obtained.

In determining the weight of mercury required to fill the tubes we found that the results were most concordant when the finely ground ends of the tube were stopped by means of a small piece of thin cover-glass backed with cork about $\frac{1}{10}$ inch thick, the diameter of the cover-glass and cork being about the same as the external diameter of the tube, so that the pressure of one finger might suffice to keep the cover-glass firmly against the end of the tube. In this way any tilting of the cover-glass relatively to the end of the tube was avoided.

The following weighings of mercury and watch-glass for one pair of tubes were made: 29·836, 29·837, and 29·835 grms. for the long tube, and 11·482, 11·481, and 11·481 for the short one. The lengths of the tubes were 29·70 and 4·89 centims. From these data, assuming the density of mercury to be 13·558 at the temperature of the experiment, the constant for this particular pair of tubes was $\frac{1}{454{\cdot}6}$.

If we were making fresh cells we should make them with the cross-tube near the foot instead of in the middle of the tubes. This would much facilitate mixing when a portion of the electrolyte is removed and replaced by water so as to alter the concentration. All that would then be necessary would be to tilt the cell two or three times to ensure effective mixing. The electrodes were pieces of platinum foil bent into cylindrical form to fit the vertical tubes of the cells. A platinum wire was welded on to each electrode and electrical connexion established with the arms of the bridge by the intervention of a mercury cup. By this means the electrodes were easily removable from the cells.

The cells and mercury cups were mounted on a wooden stand placed in an oil bath, oil being used as it was found that with a water bath the apparent resistance of the electrolyte depended on the direction of the current. The explanation of this was traced to leakage over the glass surface and through the water, and was entirely obviated by the substitution of an insulating liquid for the fluid in the bath.

The following tables give the results of some experiments made with a solution of potassium chloride, the strength being

$\frac{1}{5}$ molecular equivalent per litre. The arms $r\ r$ of the bridge were each 1000 ohms, 30 volts were used, and the balancing resistance R was of the order of 20,000 ohms. Between each two measurements of resistance the electrodes were washed, heated to redness, replaced, and the current reversed. The method of making an experiment was as follows:—The current was made for about half a minute to polarize the plates, and then broken; two minutes were allowed to elapse to enable the electrolyte to assume the temperature of the bath, which was constantly stirred, and then the balance was obtained by momentarily making the current and adjusting the resistance.

Solution I.—The solution was made from KCl twice recrystallized from water and dried in the air between filter-paper. With Kohlrausch's notation we found

$$\frac{k}{\mu} = 1005{\cdot}5 \times 10^{-8}.$$

We give no details of the experiment with this solution because the atomic weights used in making up the solution were only approximate, viz.:—K=39 and Cl=35·5. Correcting for this error the result would come about 0·1 per cent. higher. The salt, too, was dried in air, and it is probable that all the moisture was not removed, and any error resulting from this would cause the value obtained to be too low.

Solution II. (prepared for us with great care by Dr. Ewan).—The salt was prepared by fusing $KClO_3$ in a platinum crucible until no more gas was evolved. It was then recrystallized from dilute hydrochloric acid and dried by heating over a bunsen. The molecular weights used were K=39·13 and Cl=35·45. The strength was as before $\frac{1}{5}$ molecular equivalent per litre.

Table I.

Direction of current.	Temp.	Resistance.	Resistance reduced to 18° C.
+…………	18·26	21035	21149
−…………	18·19	21045	21129
+…………	18·17	21045	21119
−…………	18·11	21065	21113
+…………	18·08	21085	21120
−…………	18·06	21095	21121

The temperature coefficient used in the reduction was taken as 2·1 per cent. per 1° C.

The galvanometer gave a clear indication for a variation of 10 ohms.

Specific Resistance $= \dfrac{21125}{454{\cdot}6} = 46{\cdot}47$ ohm-centim. units.

Specific Conductivity in C.G.S. units $= 2151{\cdot}9 \times 10^{-14}$.
Conductivity compared with mercury $= 2024{\cdot}4 \times 10^{-9}$.

$$\frac{k}{\mu} = 1012{\cdot}2 \times 10^{-8}.$$

Solution III. (prepared by Dr. Ewan).—The details of preparation of this solution were similar to those employed in the previous case, except that the salt was not recrystallized from dilute hydrochloric acid, so that it is possible that a trace of chlorate or perchlorate might have been present, and this would tend to diminish the conductivity.

TABLE II.

Direction of current.	Temp.	Resistance.	Resistance reduced to 18° C.
+	18·11	21155	21204
−	17·98	21225	21214
+	17·84	21275	21204
−	17·70	21345	21211
			Mean 21208

whence as before $\dfrac{k}{\mu} = 1008{\cdot}3$.

Kohlrausch's latest result for the same strength of solution of the same salt at the same temperature is 1009, Bouty's value is 1035, Krannhal's 1003 (obtained by graphical interpolation from the data given in Ostwald's *Lehrbuch der allgemeinen Chemie*, p. 732).

Let us now investigate the different errors which are present in the determination, and assign to each its relative importance. Taking the numbers given in Table I., the mean error of observation comes to be $\pm 4{\cdot}8$, and the probable error $\pm 3{\cdot}2$. The mean error of a single observation is 12, and the probable error of a single observation is 8, so that taking only one determination, R is probably correct to 1 in 2000 or $\frac{1}{20}$ per cent. The probable error of the mean is about 1 in 7000.

Another source of error would arise if the heat caused by the passage of the current through the electrolyte had not sufficient time to escape into the liquid in the bath. The current in our experiments would produce a rise of temperature of $\frac{1}{2}$° C. per minute, but the surface exposed is so enormous compared with the mass of electrolyte in the cross-tube that calculation shows that the difference in temperature between bath and electrolyte after vigorous stirring for two minutes is infinitesimal.

The temperature of the bath was read by means of a thermometer graduated in tenths of a degree, and standardized by comparison with a thermometer certified by the Reichsanstalt. It was, however, difficult to read the thermometer with certainty to less than $\frac{1}{50}$° C. The error arising from this cause might amount to $\frac{1}{25}$ per cent.

We estimate the possible error in the calibration of the tubes to amount to not more than $\frac{1}{20}$ per cent., but this source of error arises largely from the fact that we had not in our possession apparatus for measuring the lengths of the tubes to less than 0·01 centim. With suitable apparatus there would be no difficulty in determining the constant for the tubes with much greater accuracy.

The greatest source of error appears to be a chemical one, viz.:—the preparation of the solutions, for while the one solution prepared in one way by Dr. Ewan gave 1008·3 for the conductivity, the second prepared by a different method gave 1012·2.

We have also made experiments to see whether the alternating current method of measuring electrolytic resistance would be improved by adopting the balancing-cell which we have described. To express the results very briefly, we find that for resistances not greater than 1000 ohms or thereabouts it is distinctly an improvement, enabling dead silence to be obtained in the telephone, while without the balancing-cell there was always a feeble buzz. For resistances much greater than 1000 ohms the introduction of the balancing-cell seemed of no avail whatever. We did not pursue these experiments with alternating currents very far, because it seemed clear that there was very little chance of getting as great accuracy as with the continuous current method described.

Conclusions.

1. The form of electrolytic cell described is very suitable for determining the specific conductivity of electrolytes directly without reference to the physical properties of any second electrolyte.

2. The method of measuring the conductivity of electrolytes described in this paper is, in our opinion, more convenient and more accurate than the method in which alternating currents are used.

The Yorkshire College, Leeds,
June 1896.

IV. *On the Heats of Vaporization of Liquids at their Boiling-Points.* *By* Miss Dorothy Marshall, *B.Sc., University College, London**.

§ 1. THE work which is the subject of this paper is a continuation of that described in a previous paper by Prof. Ramsay and the author (Phil. Mag. January 1896), and was carried out in a similar manner. A full account of the apparatus and method of experiment was given in the previous paper.

§ 2. Two modifications only have been introduced, viz.:—

(i.) The current which is to make the liquids boil is allowed to run through the other resistances of the circuit for at least ten minutes before the beginning of the experiment proper, in order that their temperatures may be already steady at the moment of starting. By adopting this simple precaution the current strength was constant from the first, and the points of balance on the bridge-wire were much steadier and more easily found than they had been in earlier experiments.

(ii.) Two new vessels were made in which spirals of fine platinum-silver wire took the place of the platinum spirals previously used: the terminals were as before of stout platinum wire, and the ends of the platinum-silver wire (which melts at a comparatively low temperature) were wound two or three times round the platinum and fused on. The object of this was to eliminate irregularities due to possible fluctuation in the temperature of the wire during ebullition: the temperature of the wire is certainly above

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* Communicated by the Author.

that of the surrounding liquid, and may change a little with the irregular escape of bubbles of vapour : such changes in temperature would at once affect the resistance of a platinum wire, but would be less operative in the case of platinum-silver, which has but a small temperature-coefficient. It was found, however, that the substitution of platinum-silver for platinum makes no difference whatever ; provided that the current is allowed to run for 10 or 15 minutes before the beginning of the experiment, the points of balance are found just as easily for one pair of vessels as for the other, and the numerical results previously obtained were confirmed.

§ 3. A number of experiments were made in order to see whether the same results are obtained when different strengths of current are used, *i. e.* when the D.P. across the terminals is altered. The limits of alteration are not wide, because if the current be made too weak or too strong mechanical difficulties arise : but within these practical limits the conclusion drawn is that alteration of current-strength produces no effect This is shown in the accompanying table, where R=approximate total resistance in circuit, E.M.F. being about 85 volts.

TABLE I.—Benzene and Alcohol.

B.	R.	Weights evaporated.		Ratio $\frac{V_1}{V_2}$.	Corrected Weights.		Ratio $\frac{L_1}{L_2}$.
		Alcohol.	Benzene.		Alcohol.	Benzene.	
772·5 mm.	95 ohms.	7·829	14·093	1·272	7·829	17·930	2·290
771·8 ,,	85 ,,	9·045	16·139	1·285	9·045	20·739	2·293
772·5 ,,	7 ,,	10·525	18·669	1·293	10·525	24·139	2·293

§ 4. Table II. records the values of L obtained for nine new liquids, each being compared with benzene. Data regarding the source and purity of these liquids are given in an Appendix.

TABLE II.

Name.	B.	Loss of weight (corrected).		Ratio $\frac{L_1}{L_2}$.	L_1.
		Liquid.	Benzene.		
Normal Hexane. .	747·3	12·086	9·993	0·827	
	762·4	8·669	7·369	0·839	79·2 cal.
Methyl Alcohol. .	761·9	6·379	17·585	2·757	
	730·2	6·112	17·018	2·784	261·6
Formic Acid * ...	755·8	12·784	16·309	1·276	
	753·0	11·770	14·998	1·274	120·4
Methyl Iodide ...	753·8	34·617	17·022	0·492	
	753·0	37·614	18·252	0·485	
	765·6	33·800	16·258	0·481	45·9
Ethyl Iodide......	767·6	32·355	16·572	0·512	
	763·4	35·988	18·127	0·504	
	762·8	34·881	17·306	0·496	47·6
Ethyl Bromide...	767·5	25·240	15·692	0·622	
	767·9	28·412	17·402	0·613	
	767·3	27·053	16·994	0·628	58·6
Chloroform	752·8	23·621	14·657	0·621	
	751·0	27·751	17·029	0·614	
	759·8	21·005	13·050	0·621	58·4
CCl_4	762·2	39·836	19·684	0·494	
	759·3	33·595	16·444	0·489	46·4
Aniline	765·6	15·493	18·920	1·221	
	766·9	13·388	15·609	1·166	
	764·5	15·308	18·072	1·181	
	757·4	14·269	17·882	1·253	
	749·0	13·671	16·630	1·216	113·9

§ 5. Other observers have suggested to the author from time to time that a loss of weight must be experienced from surface evaporation during the preliminary heating in the vapour jacket, especially in the case of those liquids which contain much dissolved air and lose it as their temperature is raised: the amount of loss was quite unknown, it must almost certainly be different for different liquids, and it might quite possibly introduce a considerable error into the results. Experiments have now been made in order to find out how great this loss of weight really is, and what the relation is between the losses experienced by different liquids in equal times. The apparatus was set up as for an experiment, the vessels being weighed; they were then heated by the vapour jackets for measured times, allowed to cool, and weighed again. Table III. shows the results for benzene and alcohol.

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* See also *Comptes Rendus*, cxxii. p. 1333 (1896).

TABLE III.

	8	10	15	25 min.
Benzene...........	0·416 gr.	0·543	0·698	0·974
Alcohol	0·180	0·227	0·267	0·318

The loss of weight is thus seen to be considerable. In order to find how this error affects the calculation of results, it is necessary in each case to estimate the surface loss during the average time of preliminary heating, and to subtract this from the total loss.

Table IV. shows this preliminary loss for four pairs of liquids; and Table V. gives the ratio $\frac{L_1}{L_2}$, got directly from the measured losses of weight (as given in Table II.), and the ratio obtained by applying the correction. It will be seen that the alteration is within the limits of experimental error.

TABLE IV.

	t.	*m.*
Benzene..................	5·5 min.	0·449 gr.
$CHCl_3$	5·5 „	0·580 „
Benzene.....................	5 „	0·405 „
CCl_4	5 „	0·523 „
C_2H_5Br.....................	5 „	0·350 „
C_2H_5I	5 „	0·535 „

§ 6. Two experiments were made to see whether an absolute determination of L might not be obtained by practically the same method. All that was needed was to measure the average strength of the current and the D. P. across the ends of the platinum spiral during boiling.

The D. P. was easily got on the bridge by comparison with that of a suitable number of Clark cells.

TABLE V.

	$\frac{L_1}{L_2}$.	$\frac{L_1}{L_2}$ corr.
$CHCl_3$ and C_6H_6	0·621 0·614 0·621	0·621 0·614 0·620
CCl_4 „ „	0·494 0·489	0·492 0·487
C_2H_5Br „ „	0·622 0·613 0·628	0·618 0·609 0·625
C_2H_5I „ „	0·512 0·504 0·496	0·511 0·502 0·494

The quantity of electricity that passes in the course of an experiment is summed in a voltameter introduced into the primary circuit.

By using two or more vessels simultaneously, several values can be got by one experiment.

The determinations were made with benzene.

The equation used in reducing the observations was

$$L=\frac{m}{M\,.\,e}\times\frac{V\times 10^8}{J},$$

where

M = mass of liquid vaporized,
m = mass of copper deposited,
e = electrochemical equivalent of copper,
V = D. P. in volts.
J = mechanical equivalent of heat.

In Exp. I.

$m=0{\cdot}337$ gramme,
$e=0{\cdot}00003282$,
$J=4{\cdot}199\times 10^7$.

(*a*)	(*b*)
$M_1=12{\cdot}179$ grs.	$M_2=15{\cdot}848$ grs.
$V_1=\ 4{\cdot}671$ volts.	$V_2=\ 5{\cdot}960$ volts.
$\therefore L=93{\cdot}78$ calories.	$\therefore L=91{\cdot}96$ calories.

In Exp. II.

$m = 0{\cdot}3659$ gramme.

(a)	(b)
$M_1 = 14{\cdot}698$ grs.	$M_2 = 18{\cdot}887$ grs.
$V_1 = 5{\cdot}359$ volts.	$V_2 = 6{\cdot}799$ volts.
$\therefore L = 96{\cdot}80$ calories.	$\therefore L = 95{\cdot}58$ calories.

Mean result of four experiments: $L = 94{\cdot}53$ cal.

The value of L for benzene is known to be 94·4 cal.* The agreement obtained here is not of course sufficient to make the results of any direct value: the difficulty lay in so adjusting the strength of the current that it should be strong enough to make the benzene boil tranquilly, and yet not be too strong for the voltameter. This might easily be arranged, and it is quite possible that fairly accurate absolute measurements might thus be made with comparative ease.

APPENDIX.

Regarding the purity of the liquids employed.

Hexane was lent by Prof. Sydney Young.

Methyl Alcohol was supplied by Baird and Tatlock, and guaranteed free from acetone. It was distilled with sodium until it boiled constantly at 64·95 at 761·9 mm.

Formic Acid (see also *Comptes Rendus*, cxxii. p. 1333, 1896) was supplied by Kahlbaum, and was subjected to fractional distillation until a portion was obtained boiling at 100° to 100°·5 at 751·2 mm.

Methyl Iodide was prepared by the author, dried with calcium chloride, and distilled, using a fractionator like that described by Young and Thomas (Chem. News, lxxi. p. 177): b.pt. 43°·4 at 769·8 mm.

Ethyl Iodide was similarly prepared, and distilled in the last instance from P_2O_5: b.pt. 72°·8–72°·9 at 765·5 mm.

Ethyl Bromide was dried with calcium chloride and phosphoric anhydride: b.pt. 39°·75–38·°95 at 765·5 mm.

Chloroform was obtained from Baird and Tatlock: it was washed with water, then with concentrated sulphuric acid, then with water again; left to stand with phosphoric anhydride for some hours, and distilled with the fractionator: b.pt. 61°·7–61°·8 at 760·0 mm.

Carbon Tetrachloride was fractionated from P_2O_5: b.pt. 76°·7–76°·8 at 759·5 mm.

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* Griffiths and Marshall, Phil. Mag., January 1896.

V. *Note on an Error in the Method of Determining the Mean Depth of the Ocean from the Velocity of Seismic Sea-waves.* By Charles Davison, *Sc.D., F.G.S., King Edward's High School, Birmingham*[*].

THE data we possess for determining the mean velocity V of sea-waves are the distance A and the time of transit T. From these we obtain

$$V = A/T.$$

We have, from hydrodynamics, the further equation

$$V = \surd(gH),$$

in the case when the sea is of considerable but uniform depth H along the path traversed by the wave.

If the depth of the sea is variable, it is generally assumed that the quantity H given by the equation

$$\surd(gH) = A/T$$

is the mean depth of the ocean along the path of the wave. But when the mean depth along the same path can also be obtained from soundings, it is found to be greater than that which is given by the above formula.

In the case of the Krakatoa sea-waves of August 26–30, 1883, a large number of very careful calculations were made by Capt. W. J. L. Wharton, F.R.S.[†]; and his results, so far as they concern the subject of the present paper, are given in the following Table. The stations marked with an asterisk are those at which the records were obtained by automatic gauges. In the second column the figures denote the mean depth of the sea calculated from the formula, and in the last column the mean depth obtained from known soundings.

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[*] Communicated by the Author.

[†] "On the Seismic Sea-waves caused by the Eruption of Krakatoa, August 26th and 27th, 1883." 'The Eruption of Krakatoa and Subsequent Phenomena,' pp. 89–150.

Station.	Mean Depth in Fathoms obtained from	
	Velocity of Sea-wave.	Soundings.
Galle	2120	2400
Colombo	1445	2380
*Negapatam	1880	2300
*Madras	1700	2300
*Vizagapatam	1700	2200
*False Point	1400	2150
*Dublat	1820	2100
*Beypore	1580	2300
Trincomalee	1770	2300
*Bombay	1650	1700
*Karachi	1710	2150
*Aden	1770	2150
Mauritius	2430	2600 ?
Seychelles	2120	2300 ?
*Port Alfred	2245	2300 ?
*Table Bay	2040	2300 ?
*Socoa	2660	2300
*Rochefort	2520	2300
*Devonport	2050	2300
*Cherbourg	2640	2300
*Portland	2420	2300
*Havre	2640	2300

Thus in every case, except along the paths to the six French and English stations, the mean depth obtained from the velocity of the sea-waves is less than that obtained from the soundings. Capt. Wharton does not, however, regard the evidence afforded by any of the six gauges as conclusive. The disturbance in all of them is very slight; though, on the other hand, "looking at them collectively, and seeing the fair accordance of the speed of the waves—which would travel on the same course—the evidence is strongly in favour of the disturbances on these gauges being the effect of one and the same cause, and one which originated at a great distance."

Similar calculations have been made from the waves which accompanied the Iquique earthquake of May 9, 1877 †. The disagreement between the mean depths calculated by Dr. Geinitz and Prof. Milne is chiefly due to the different positions of the origin assigned by them to the sea-waves. The values

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† Dr. E. Geinitz, "Das Erdbeben von Iquique am 9 Mai, 1877, und die durch dasselbe verursachte Erdbebenfluth in Grossen Ocean," Nova Acta der Ksl. Leop.-Carol.-Deutschen Akademie der Naturforscher, Bd. xl. 1878, pp. 385–444.

Prof. J. Milne, "The Peruvian Earthquake of May 9th, 1877." Japan Seismol. Soc. Trans. vol. ii. 1880, pp. 50–96. Also 'Earthquakes and other Earth Movements,' 1886, pp. 182–186.

of the mean depth obtained from soundings is calculated from data given by Prof. Milne.

Station.	Mean Depth in Fathoms obtained from		
	Velocity of Sea-waves.		Soundings.
	Geinitz.	Milne.	
Wellington	1430	1028	2473
Honolulu	2319	1635	2495
Samoa	1930	1662	2500
Kamaishi	2182	1563	2584

Both Captain Wharton and Prof. Milne notice the discrepancy between the calculated and observed mean depths, and both offer a partial explanation of it. According to the latter, "the common error in actual soundings is that they are usually too great, it being difficult in deep-sea sounding to determine when the lead actually reaches the bottom." Captain Wharton remarks that "any unknown ridges would diminish the speed; but these must be large, or the portion of the wave overlapping them would still travel at the speed due to deeper water, and over a very slightly longer course."

But even if the soundings were absolutely exact, and if there were no unknown ridges, the discrepancy would still exist; for, as will be shown below, the assumption that the mean depth of the sea is given by the equation $\sqrt{(g\mathrm{H})}=\mathrm{A}/\mathrm{T}$ is incorrect.

Taking the tide-gauge as the origin, the horizontal line joining the gauge and the epicentre as the axis of x, and the axis of y vertically downwards, then, neglecting the curvature of the earth,

$$\mathrm{T}=\frac{1}{\sqrt{g}}\int_0^{\mathrm{A}}\frac{dx}{\sqrt{y}},$$

and therefore

$$\mathrm{H}=\left(\frac{\mathrm{A}}{\int_0^{\mathrm{A}}\frac{dx}{\sqrt{y}}}\right)^2.$$

Also, D being the true mean depth,

$$\mathrm{D}=\frac{1}{\mathrm{A}}\int_0^{\mathrm{A}} y\,dx,$$

and

$$\frac{\mathrm{D}}{\mathrm{H}}=\int_0^{\mathrm{A}} y\,dx \times \left(\int_0^{\mathrm{A}} \frac{dx}{\sqrt{y}}\right)^2 \div \mathrm{A}^3.$$

Let

$$\int_0^{\mathrm{A}} y\,dx = h_1a_1+h_2a_2+\ldots+h_na_n,$$

where

$$a_1+a_2+\ldots+a_n=\mathrm{A}.$$

Then

$$\int_0^{\mathrm{A}} y\,dx \times \left(\int_0^{\mathrm{A}} \frac{dx}{\sqrt{y}}\right)^2 - \mathrm{A}^3$$

$$=(h_1a_1+h_2a_2+\ldots+h_na_n)\times\left(\frac{a_1}{\sqrt{h_1}}+\frac{a_2}{\sqrt{h_2}}+\ldots+\frac{a_n}{\sqrt{h_n}}\right)^2 - (a_1+a_2+\ldots+a_n)^3$$

$$=\Sigma\left\{a_1a_2^2\left(\frac{h_1}{h_2}+2\sqrt{\frac{h_2}{h_1}}-3\right)\right\}$$

$$+\Sigma\left\{2a_1a_2a_3\left(\frac{h_1}{\sqrt{h_2h_3}}+\frac{h_2}{\sqrt{h_3h_1}}+\frac{h_3}{\sqrt{h_1h_2}}-3\right)\right\}.$$

It may be easily shown that the coefficients of $a_1a_2^2$ and $a_1a_2a_3$ are positive. Hence D is greater than H, unless y is constant.

To obtain a numerical estimate of the inequality between D and H, I have taken the case of a cylindrical ocean-bed, whose right section is a parabola, the breadth of the ocean 1200 miles, its greatest depth 4 miles, and (for simplicity) I supposed the tide-gauge and the epicentre of the earthquake to be equidistant from the axis of the parabola, the depth of water at each place being one quarter of a mile. The true mean depth of the ocean along the path of the sea-wave is then 2420 fathoms, and the mean depth as calculated from the velocity of the sea-wave is 1900 fathoms, the latter number being about three quarters of the former.

VI. *On the Estimation of "Waste Space round the Needle of a Galvanometer."* *By* ANDREW GRAY, *LL.D., F.R.S., Professor of Physics in the University College of North Wales**.

MY attention has been recalled to Professor Holman's paper in the Philosophical Magazine for December 1895 by the reply from Professor Ayrton and Mr. Mather which appeared in the November number of 1896. In that

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* Communicated by the Author.

paper Professor Holman endeavoured to show that the statement put forward by Messrs. Ayrton, Mather, and Sumpner (Phil. Mag. July 1890) as to the extent of the waste space round the needle of a galvanometer, a statement which, as he said, I had adopted in my book on 'Absolute Measurements in Electricity and Magnetism,' was vitiated by neglect of the non-uniformity of the field produced at the needle by the current in a turn of wire of the coil; and that, when this was taken account of, the true extent of such space was found to be comparatively trifling.

At first sight Professor Holman's contention, based on a calculation of the turning moment exerted on each element of the magnet by the field at the element, seemed plausible; but on more closely examining the matter, I have found that no correction whatever of the statement is required, and that Professor Holman has, by omitting a term in the expression for the total turning motive due to the forces on an element of the needle, been led to an erroneous result.

In trying to show what the omission made by Professor Holman is, I shall not directly use the hypothesis of imaginary magnetic matter, but shall regard each element of length of the needle, supposed straight, infinitely thin, and uniformly magnetized in the direction of its length, as made up of a very large number of uniformly distributed molecular magnets, each practically infinitely small in every respect in comparison with the element of the magnet used in integrating up the action of the external field on its parts, and all perfectly aligned along the axis of the needle. This seems a preferable mode of regarding the magnet in the circumstances, as it has regard to the physical facts of the case and avoids the appearance of begging the question involved in arranging beforehand a series of equal and opposite surface distributions on those faces of adjacent slices which are in contact, and then showing, what is obvious, that no magnetic matter exists for force to act upon except on the end-faces of the needle. As a matter of fact, however, the results of the two processes are equivalent.

Let, then, the magnet be suspended horizontally so as to be free to turn round a vertical through its middle point; let F (Professor Holman's $f \cos \theta$) be the magnetic force perpendicular to the magnet's length at a point of the magnet distant s from the centre. Consider an element of the magnet of length ds having its centre at the point s; let the couple exerted on the element of the magnet when placed at right angles to the lines of force in a uniform field of intensity unity be mds, so that the magnetic moment of the aggregate of molecular magnets in unit length is m. *If we take* F *as* *uniform over the element of length ds*, the couple on the element will be $m\mathrm{F}ds$, which is Professor Holman's $mf\cos\theta\,.\,ds$. But it is to be carefully noticed that the *total couple on the magnet* is not obtained by integrating this expression from end to end of the magnet; for the value of F varies along the element, and this variation renders necessary the addition of a term to the quantity to be integrated.

The necessary correction for this variation is the moment round the axis of suspension of the force tending to produce a motion of translation of the element as a whole.

The potential energy of the element ds of the magnet in the field is

$$-m\mathrm{S}ds,$$

if S be the magnetic force in the direction of the length of the magnet at the point distant s from the centre. The force producing motion of translation of the element in the horizontal direction perpendicular to the magnet's length (that is, let us say, in the direction of x) is therefore

$$m\frac{d\mathrm{S}}{dx}ds.$$

But clearly $d\mathrm{S}/dx = d\mathrm{F}/ds$; and this force is

$$m\frac{d\mathrm{F}}{ds}ds;$$

while its moment round the axis of suspension is

$$ms\frac{d\mathrm{F}}{ds}ds.$$

The whole expression to be integrated along the magnet is thus

$$m\left(\mathrm{F}+s\frac{d\mathrm{F}}{ds}\right)ds = m\frac{d}{ds}(\mathrm{F}s)ds.$$

Hence integrating from $s=-l$ to $s=+l$ ($2l$ being the length of the magnet) we get for the total couple the equation

$$\mathrm{L}=ml(\mathrm{F}_{+l}+\mathrm{F}_{-l}),$$

where F_{+l}, F_{-l} denote the component forces perpendicular to the magnet at the ends. If these forces be equal we have

$$\mathrm{L}=2ml\mathrm{F}_l,$$

or the magnetic moment of the whole magnet multiplied by the component perpendicular to the magnet of horizontal magnetic force at either end.

This result is independent of the distribution of magnetism or of electric currents producing the magnetic field in which the needle is placed, since the equation $dS/dx = dF/dy$ holds in all cases.

I hope that in what precedes I have correctly understood Professor Holman's notation. His *m*, defined by him to be "the strength of pole of any thin transverse section or shell of the needle," I take to be that quantity which multiplied by the thickness *ds* of the shell gives *mds* as the magnetic moment of the shell; in other words it is, as stated above, the magnetic moment per unit length of the bar. It seems necessary to make this remark, as Messrs. Ayrton and Mather, in their reply, interpret Professor Holman's *m* differently, namely, as the amount of free magnetism per unit length at a given cross-section of the bar. In this sense *m* is zero everywhere except at the ends, where it is infinite, inasmuch as according to the theory of magnetic matter the finite, equal, and opposite quantities of magnetism are surface distributions on the end-faces.

Bangor, November 1896.

VII. *Absorption of Electric Waves along Wires by a Terminal Bridge. By* Edwin H. Barton, *D.Sc., F.R.S.E., Senior Lecturer in Physics at University College, Nottingham, and* Geo. B. Bryan, *B.Sc., "1851 Exhibition" Research Scholar*[*].

IN the discussion on Mr. Yule's electrical paper last year[†], one of us stated his intention of endeavouring to realise experimentally the extinction of electric waves along wires on their arrival at the end of the line, the urgent desirability of this having been felt by several investigators[‡]. The question is probably of interest also in connexion with Telephony. This task, small as it may seem, we had no opportunity to attack till this summer. The adoption of a resistance-bridge for the absorption of the electric waves was originally suggested by Mr. Oliver Heaviside's mathematical proof[§] that, given a bridge of suitable resistance at the end of a line, then the waves arriving there would be immediately absorbed;

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[*] Communicated by the Physical Society: read November 13, 1896.

[†] G. U. Yule, "On the Passage of an Oscillator Wave-Train through a Plate of Conducting Dielectric," Proc. Phys. Soc. vol. xiii. pp. 358–392 (1895); Phil. Mag. [5] xxxix. p. 309.

[‡] See *e.g.* J. von Geitler, pp. 7–8, 'Inaugural Dissertation,' Bonn, 1893; E. H. Barton, pp. 70–71, Proc. Roy. Soc. vol. lvii. 1894; G. U. Yule, Proc. Phys. Soc. *t. c.* pp. 384–387; Phil. Mag. *t. c.* pp. 334–337.

[§] 'Electrical Papers,' vol. ii. pp. 127 and 132–133.

"the electricity is all gobbled up at once, so to speak"*. And, a few preliminary difficulties being overcome, this important theoretical conclusion readily received experimental confirmation.

Experimental Arrangement.—The electric vibrations were generated by an oscillator of the type and size previously used by Mr. V. Bjerknes † and others. The waves, about 8½ metres long, were propagated along a "line" consisting of a pair of parallel copper wires 1·5 mm. in diameter and 8 cm. apart. The waves were detected and relatively measured by an attraction-electrometer made on the principle of that devised by Herr von Geitler ‡. The length of the line to the electrometer was 116 m. This instrument has a needle of aluminium foil suspended by a quartz fibre between disks which may be connected to the respective wires of the line. On the passage of a wave-train the needle, initially uncharged, is charged inductively, and its ends are accordingly attracted to the disks whatever the sign of their potential-difference.

The electrometer therefore responds by a deflexion of the needle towards its disks. Let us call this a positive deflexion. In addition to the needle just described, and fixed to it in the same vertical plane, is a second similar one, but with its pair of attracting disks so situated that when they alone are connected to the line a negative deflexion ensues. When both needles have their respective disks connected to the line the electrometer is said to be used differentially. When so used, and with the instrument in adjustment, no deflexion is produced by the simple passage of a wave-train. Suppose now, owing to anything on the line which causes reflexion, stationary waves are produced at the electrometer. Then, provided one needle be connected to a point on the line which is a *node* for the waves, and the other be at a *loop*, the electrometer must give a deflexion and thus prove that *reflexion* of the waves has occurred. It is easily seen that when a single needle is used the first throw is proportional to the time-integral of the square of the potential-difference of the two leads of the line at the place to which the disks are connected.

The electrometer-needle carried a light plane mirror, and the throws were read by a distant telescope and scale.

Theory of Absorption.—Mr. Heaviside's § theory shows that

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* *Loc. cit.* p. 127.

† V. Bjerknes, Wied. *Ann.* vol. xliv. pp. 519–520 (1891); E. H. Barton, Proc. Roy. Soc. vol. liv. p. 86 (1893); G. U. Yule, Proc. Roy. Soc. vol. liv. p. 97 (1893).

‡ Wied. *Ann.* vol. xlix. p. 188 (1893).

§ 'Electrical Papers,' vol. ii. pp. 132–133.

in the case of plane electric waves proceeding along parallel leads to a bridge at the end of the line, the ratio of the potential-differences of the two leads due respectively to the reflected and incident waves is given by

$$\rho=\frac{\mathrm{R}-\mathrm{L}v}{\mathrm{R}+\mathrm{L}v}, \quad \ldots \ldots \quad (1)$$

where ρ is the ratio in question, or *reflexion coefficient*, R is the resistance of the bridge, which must be of negligible inductance, L is the inductance per unit length of the line, and v is the velocity of light. Electromagnetic units are to be understood throughout.

Thus,

$$\rho=-1, \quad \text{for } \mathrm{R}=0, \quad \ldots \ldots \quad (2)$$

$$\rho=+1, \quad \text{for } \mathrm{R}=\infty, \quad \ldots \ldots \quad (3)$$

$$\text{and } \rho=0, \quad \text{for } \mathrm{R}=\mathrm{L}v. \quad \ldots \ldots \quad (4)$$

Now $v=3\times10^{10}$ cm. per sec. nearly, and in the present case $\mathrm{L}=4\log_\epsilon\left(\frac{8}{0\cdot075}\right)=18\cdot68$ nearly. Hence the critical value of R to give complete absorption is,

$$\mathrm{R}=3\times10^{10}\times18\cdot68 \text{ absolute units of resistance}=560 \text{ ohms.} \quad (5)$$

Details of Bridge.—At first an ordinary resistance-box was used as a bridge across the two wires of the line. But this was soon rejected lest the faces of the two bars at a plug opening should act as the plates of a condenser of appreciable capacity, which is inadmissible in the bridge desired. We next wound a small coil non-inductively on a bobbin in the usual manner and used it without any box or screw terminals. It was of well-covered wire and further insulated by thorough soaking in paraffin-wax both before and after winding. But this too proved unsatisfactory. For a time it stood, but then the insulation broke down and sparks were seen to pass between different turns of the wire. It was accordingly abandoned.

The simple expedient of making an electrical resistance by rubbing a common lead pencil on a disk of ground glass, small pieces of tin-foil gripped on by binding-screws forming the terminals, was then tried, and seemed in every way satisfactory. The electrostatic capacity of the terminals was proved to be negligible by testing such a disk and screws without any pencil. In this case no reflexion of the waves could be detected. Moreover, by making the track of the pencil-marks between the terminals very wide, the self-induction of the bridge was reduced to a minimum. It was also found easy to adjust the resistance to a few ohms and to restore it nearly to its original value after the change which spontaneously occurred in it from day to day. Three bridges of this type were used, of resistances 261, 549 to 560, and 1336 to 1355 ohms. Let these be denoted respectively by the letters F, G, and H. Finally, since the pencil-markings on the glass form an extremely thin sheet, the resistance may be assumed to be practically the same to high-frequency waves as to the steady current by which they were measured on the Post-office box.

Experiments.—The oscillator emits rapidly-damped electric vibrations, only the first dozen, say, being appreciable. We thus have, advancing along the line with the speed of light, a damped wave-train, its large end, or head, leading and its tail, after about twelve waves, being negligibly small. Suppose now one needle of the electrometer to be connected to the line and a bridge of no resistance to be placed at the end of the line a little beyond the electrometer. We shall then have, at the electrometer, stationary waves due to interference between the incident waves and those reflected at the bridge. Hence, if a series of readings be taken with the electrometer at different distances from the end of the line the throws will be found to periodically wax and wane. But when the bridge is distant a few wave-lengths from the electrometer we have the head of the wave-train interfering with the tail only. And with somewhat greater distances between electrometer and bridge the interferences of the waves and, consequently, the waxing and waning of the electrometer-throws cease to be appreciable. Thus, if the distances between electrometer and terminal bridge are plotted as abscissæ and electrometer throws as ordinates, we should expect the experiment to yield a damped wavy curve. And this is the case, as first shown by Mr. V. Bjerknes*, and utilized by him and his successors to determine the wave-length of the oscillations in use. The result of this experiment in our case is shown by the full-line curve E on the diagram.

In the case just considered the reflexion coefficient, ρ, of the bridge is -1, as shown by equation (2). The waves are therefore unchanged in magnitude by the act of reflexion. Turn now to the general case of a precisely similar experiment with a terminal bridge for which ρ is finite and less than unity. In this case, obviously, theory predicts a curve of similar form, wavy and damped, but lying between narrower limits, since the reflected wave is now always smaller than the

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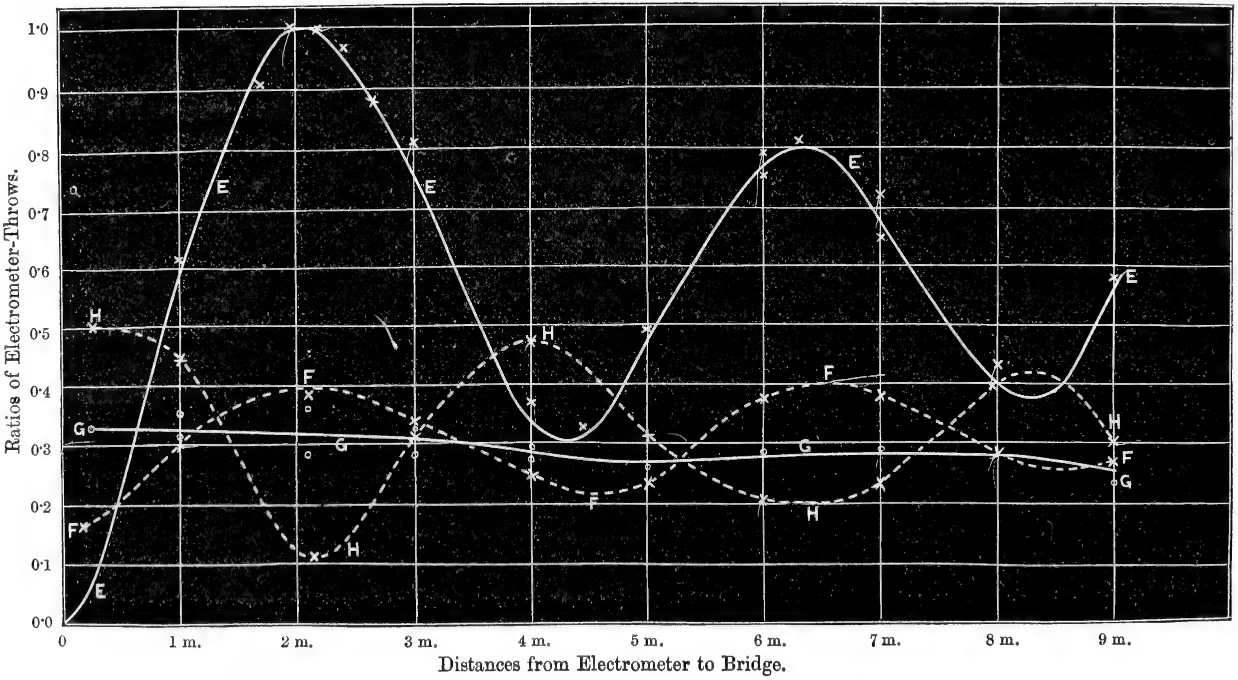
* Wied. *Ann.* vol. xliv. pp. 522–523 (1891).

Curves showing Interference due to Reflexion.

Line G G, being almost straight, indicates complete absorption by the corresponding bridge.



incident one. Such an experimental result may, therefore, be accepted as proof that the bridge in use diminishes the wave in the act of reflexion.

Further,

$$\text{for } R < Lv,\ \rho \text{ has a negative value,} \quad . \quad . \quad . \quad (6)$$

and consequently the maxima and minima of the curves occur for the same values of the abscissæ as when $R=0$. This is seen to be the case with the dotted curve, F, on the diagram, obtained with the bridge whose resistance was 261 ohms.

Again,

$$\text{for } R > Lv,\ \rho \text{ has a positive value;} \quad . \quad . \quad . \quad (7)$$

hence this curve is, broadly speaking, an inversion of that just considered, the maxima occurring for those values of the abscissæ at which the minima of the curve for $R=0$ occurred, and *vice versâ*. This is exemplified in the dotted curve, H, on the diagram, resulting from the experiment with the bridge whose resistance was of the order 1350 ohms.

Finally, we have

$$\text{for } R=Lv, \quad \rho=0. \quad . \quad . \quad . \quad . \quad . \quad (8)$$

In this case, since no waves are reflected, no stationary waves can obtain, and consequently no waxing and waning of the electrometer-throws can occur. Thus theory predicts a straight line as the outcome of an experiment with a bridge of this critical resistance. This is approximately the case with the full-line curve, G, on the diagram, obtained with the bridge whose resistance was of the order 560 ohms, the theoretical value of the critical resistance for the line used.

Of course an absolutely straight line cannot be expected, since the sparks in the oscillator vary slightly. For this reason, throughout the experiments hitherto considered the throws with the bridge under examination at any desired distance beyond the electrometer were alternated with those obtained with a no-resistance bridge at the standard distance of a quarter-wave-length beyond. And the ordinates of the curves on the diagram represent, not the actual electrometer-throws with the bridge and distance in question, but the ratio of these to those obtained under the standard conditions just described. Thus the maximum ordinate in the diagram is unity, all the curves are reduced to the same scale, and the errors due to variations of the sparking at the oscillator are in great measure eliminated. In the above experiments the electrometer was used as a single-needle instrument, and the throws obtained varied up to 100 scale-divisions.

As a further test of the non-reflexion of waves by the bridge G, the electrometer was afterwards used differentially, the upper and lower needles having their disks attached to points on the line a quarter-wave-length apart. The bridges were placed at the end of the line 0·25 m. beyond the second needle. With a no-resistance bridge the throws were positive and of the order 50 scale-divisions; with a simple disconnexion ("infinity bridge") the throws were of the same order but negative. The bridges F and H gave smaller throws, positive and negative respectively. The bridge G also appeared to yield a small throw; but this was no larger than might be due to a creep of the zero or an imperfection in the adjustment of the electrometer-needles. The use of the differential electrometer is thus seen to confirm the results of the experiments conducted with a single needle.

It should be noted that this paper deals only with the absorption of waves by *terminal* bridges; no *intermediate* resistance-bridge will absorb all the waves arriving there. The reflexion and transmission coefficients ρ' and τ' for an intermediate bridge of resistance R are given by*

$$\rho'=\frac{-\mathrm{L}v}{2\mathrm{R}+\mathrm{L}v} \text{ and } \tau'=\frac{2\mathrm{R}}{2\mathrm{R}+\mathrm{L}v}, \quad . \quad . \quad . \quad (9)$$

the notation being the same as that previously used. Thus the condition for non-reflexion is $\mathrm{R}=\infty$, in which case all is transmitted. Hence complete absorption is impossible.

If, therefore, we wish to pass electric waves along wires through any layer of dielectric, or through any other arrangement which affects them, and are desirous of avoiding the annoying disturbance due to return of the transmitted portion after reflexion at the end of the line, the following method will suffice.

1. Place at the end of the line a bridge of the form herein described, whose resistance has the value given by the theory for plane waves.
2. Test experimentally the absorbing-power of the bridge, and, if necessary, adjust its resistance until no reflexion can be detected.

University College, Nottingham,
September 1896.

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* Oliver Heaviside's 'Electrical Papers,' vol. ii. pp. 141–142.

VIII. *On the Relation of the Physical Properties of Aqueous Solutions to their state of Ionization.* *By* Prof. J. G. MacGregor, *Dalhousie College, Halifax, N.S.**

It has often been pointed out that, according to the dissociation or ionization conception of the constitution of a solution of an electrolyte, the difference between the physical properties of one in which ionization is complete and those of the solvent must be compounded additively of the differences produced by the two ions. It would seem to be equally obvious that, in the case of solutions in which the ionization is not complete, the differences referred to must be similarly compounded of those produced by the undissociated molecules and by the free ions; and if so, it should be possible to express the numerical values of the various properties in terms of the state of ionization. Such an expression would take its simplest form in the case of solutions so dilute that the molecules, dissociated or undissociated, might be regarded as sufficiently far apart to render mutual action between them impossible, and in these circumstances the change produced in the properties of the solvent by the undissociated and the dissociated molecules respectively might be expected to be simply proportional to their respective numbers per unit of volume. It is the object of this paper to test the applicability to sufficiently dilute solutions of such an expression, viz.,

$$P=P_w+k(1-\alpha)n+l\alpha n, \quad . \quad . \quad . \quad . \quad (1)$$

where P is the numerical value of any property (density &c.), P_w that of the same property for water under the same physical conditions, n the molecular concentration of the solution, *i. e.*, the number of gramme-equivalents of the dissolved substance per unit volume of the solution, α the ionization-coefficient, αn and $(1-\alpha)n$ consequently the numbers of dissociated and undissociated gramme-equivalents per unit of volume respectively, and k and l constants, which may be spoken of as ionization-constants, which will vary with the solvent, the substance dissolved, the property to which they apply, the temperature, and the pressure, but not with the concentration of the solution.

The formula can obviously apply only to properties for which P_w has a finite value. Thus it is inapplicable to electrical resistance, for which P_w would have a practically infinite value.

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* Abstract of a paper read before the Nova Scotian Institute of Science. Communicated by the Author.

Simple Solutions.

In order to test the applicability of the above expression I have determined the ionization-constants for the density, thermal expansion, viscosity, surface-tension, and refractive index of solutions of Sodium and Potassium Chlorides, by the aid of observations made by Bender[*], Brückner[†], and Rother[‡]. I selected these observations as a first instalment not because of their precision (for in one or two cases more exact observations are available), but because these observers, in all cases but one, determined the values of the above properties for mixtures of solutions as well as for simple solutions. I selected the above chlorides partly because I thought it well to begin with salts of simple molecular structure, but largely also because, for the purpose of calculating the conductivity of mixtures of them (as described in my paper on this subject[§]), I had already obtained interpolation formulæ and curves which, judged by the results of that paper, gave with considerable accuracy the ionization-coefficients of the simple solutions of these salts in terms of their molecular concentration. To save space I may tabulate here the values of the ionization-coefficients used in the calculations for simple solutions. They are as follows:—

Sodium Chloride.		Potassium Chloride.	
Grm.-mols. per litre.	Ionization-coefficient at 18° C.	Grm.-mols. per litre.	Ionization-coefficient at 18° C.
·25	·792	·1875	·8267
·5	·736	·3402	·811
·8928	·6866	·375	·796
1·0	·676	·5	·788
1·5	·633	·6856	·769
1·8353	·601	·75	·768
2·0	·5866	1·0	·756
2·5	·5504	1·0467	·755
2·8373	·5255	1·4292	·731
3·0	·514	1·5	·731
3·9375	·4516	2·0	·712
		2·185	·7048
		2·5	·695
		2·986	·681
		3·0	·680

[*] Wied. *Ann.* vol. xxii. (1884) p. 184, and vol. xxxix. (1890) p. 89.

[†] *Ibid.* vol. xlii. (1891) p. 293.

[‡] *Ibid.* vol. xxi. (1884) p. 576.

[§] Phil. Mag. [5] xli. p. 276 (1896); and Trans. N.S. Inst. Sci. ix. (1896) p. 101.

These coefficients were obtained from Kohlrausch and Grotrian's and Kohlrausch's observations of conductivity at 18° C.* In obtaining them I took the specific molecular conductivity (referred to mercury) at infinite dilution to be 1216×10^{-8} for KCl, and 1028×10^{-8} for NaCl, not being aware at the time that Kohlrausch had given 1220 and 1030 respectively as more exact values. Nevertheless, to save labour I have used the above values of α in the calculations of this paper, having satisfied myself by a re-calculation in one case that no appreciable difference in the results would be produced by the employment of more exact values. It will be noticed that in one or two cases the above values of α are obviously a little out; but they would seem to be sufficiently accurate for my purpose. I did not foresee the extent of the calculations, or I should have determined all the values of α required at the outset, and checked them by comparison with one another.

I have determined the ionization-constants (k and l) in all cases in which more than two observations of a property on solutions of sufficient dilution were available by the method of least squares. The constants thus determined and used in the calculations are tabulated below. In all cases the available observations had been made on solutions of such great concentration that the values of the constants obtained cannot be regarded as exact; but the calculations may serve as a test of the general applicability of the expression referred to above. The only available observations, so far as I know, on solutions of sufficient dilution for the determination of the ionization-constants and the limits of concentration within which the above expression is applicable, are those by Kohlrausch and Hallwachs † on the specific gravity of dilute solutions, from which two of my students have undertaken to determine the density-constants for the salts and acids examined.

With regard to the observations which I used in determining the various ionization-constants, the following statements should be made:—

Bender's determinations of density (*i. e.* specific gravity referred to water at 4° C.) were made at 15° C., but were readily reduced to 18° by the aid of his observations on the thermal expansion between 15° and 20° of the same solutions. According to his statement, the fourth place of decimals in his values may be in error by ± 2 or ± 3. The density of water was taken to be 0·99863.

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* Wied. *Ann.* vi. (1879) p. 37, and xxvi. (1885) p. 195.

† Wied. *Ann.* liii. (1894) p. 14.

Bender's determinations of thermal expansion are for the interval between 15° and 20° C.; and will therefore be sufficiently nearly proportional to the coefficients of expansion at 18° for my purpose. He considers that they may be in error by ±2 in the sixth place of decimals. On plotting his observations, however, it becomes obvious that they do not all attain this degree of accuracy. The expansion of water was taken, according to his observations, to be $0{\cdot}0_3878$ for the same interval.

Brückner's observations of viscosity were made at 15° C.; but he gives an interpolation formula, applicable between 15° and 20°, by means of which at least approximate values for 18° were obtained. His values for water at 15° and 20° do not agree well with those given by Landolt and Börnstein. I have therefore taken 0·010613 as the viscosity at 18° of the water used by him, a value which has to his value at 15° the same ratio as Landolt and Börnstein's for the same temperatures. The actual concentrations of Brückner's solutions differed from those given in the tables below by about 0·1 per cent.; but so small a difference could produce no appreciable error in the result. He gives as his "mean probable error of observation," ±2·4 in the fifth place of decimals for sodium-chloride solutions, and ±1·8 for those of potassium chloride.

Rother's observations of surface-tension were made at 15°, and are therefore not precisely comparable with calculated values based on the values of ionization-coefficients for 18°. From Kohlrausch's data*, however, it would appear that between 15° and 18° in the case of potassium-chloride solutions containing 0·5 and 3 gramme-molecules per litre, the ionization-coefficient changes only by about 0·13 and 1·3 per cent. respectively; and in the case of sodium-chloride solutions of the same concentrations only by about 0·4 and 0·6 per cent. respectively. For the more dilute solutions, therefore, my calculations will be practically comparable with Rother's observations. He seems to regard his determinations as possibly in error by ± 5 to 8 in the third place of decimals. The surface-tension of the water he used he found to be 7·357.

Bender's observations of refractive index were made at 15° C., but were reduced to 18° by means of data provided in his paper, based on observations made by Fouqué†. The refractive index of the water he used he found to be 1·33310

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* Wied. *Ann.* xxvi. (1885) p. 223.

† *Compt. Rend.* lxiv. (1867) p. 121.

Sodium Chloride Solutions.				Potassium Chloride Solutions.			
Grm.-mols. per litre.	Observed value.	Calc. value.	Difference.	Grm.-mols. per litre.	Observed value.	Calc. value.	Difference.
Density (Bender's observations).							
0·25	1·00898	1·00916	$+0\cdot0_{3}18$	0·1875	1·00752	1·00731	$-0\cdot0_{3}21$
0·5	1·01930	1·01929	− 01	0·375	1·01567	1·01586	+ 19
1·0	1·03925	1·03910	− 15	0·75	1·03317	1·03278	− 39
1·5	1·05834	1·05842	+ 08	1·0	1·04362	1·04401	+ 39
2·0	1·07772	1·07701	− 71	1·5	1·06630	1·06621	− 09
2·5	1·09633	1·09532	$-0\cdot0_{2}101$	2·0	1·08767	1·08823	+ 56
				2·5	1·10755	1·11008	$+0\cdot0_{2}253$
				3·0	1·13057	1·13177	+ 120
Thermal Expansion (Bender's observations).							
0·25	·001013	·001022	$+0\cdot0_{5}9$	0·1875	·000963	·000966	$+0\cdot0_{4}03$
0·5	·001141	·001141	± 0	0·375	·001037	·001040	+ 03
1·0	·001357	·001349	− 8	0·75	·001183	·001173	− 10
1·5	·001522	·001526	+ 4	1·0	·001249	·001255	+ 06
2·0	·001663	·001657	− 6	1·5	·001395	·001395	± 00
2·5	·001776	·001769	− 7	2·0	·001500	·001517	+ 17
3	·001876	·001848	$-0\cdot0_{4}28$	2·5	·001580	·001621	+ 41
Viscosity (Brückner's observations).							
0·5	·010988	·010978	$-0\cdot0_{4}10$	0·5	·010457	·010451	$-0\cdot0_{4}06$
1·0	·011480	·011475	− 05	1·0	·010395	·010379	− 16
1·5	·012048	·012047	− 01	1·5	·010351	·010366	+ 15
2·0	·012707	·012730	+ 23	2·0	·010394	·010393	− 01
2·5	·013472	·013458	− 14	2·5	·010444	·010457	+ 13
3·0	·014373	·014267	$-0\cdot0_{3}106$	3·0	·010566	·010555	− 11
Surface-tension (Rother's observations).							
0·8928	7·482	7·482	±0·000	0·3402	7·411	7·408	−0·003
0·8353	7·629	7·629	± 0	0·6856	7·460	7·462	+ 2
2·8373	7·780	7·798	+ 18	1·0467	7·518	7·519	+ 1
3·9375	7·954	7·997	+ 43	1·4292	7·584	7·583	− 1
				2·1851	7·705	7·709	+ 4
				2·9859	7·844	7·846	+ 2
Refractive Index, D line (Bender's observations).							
0·5	1·33824	1·33824	$\pm0\cdot0_{2}00$	0·5	1·33803	1·33806	$+0\cdot0_{3}03$
1·0	1·34307	1·34306	− 01	1·0	1·34278	1·34274	− 04
1·5	1·34770	1·34770	± 00	1·5	1·34721	1·34722	+ 01
2·0	1·35213	1·35206	− 07	2·0	1·35179	1·35158	− 21
2·5	1·35673	1·35632	− 41	2·5	1·35623	1·35582	− 41

at 18° for the D line. He seems to regard his observations as possibly in error by ±1 in the fourth decimal place.

The Tables on page 50 contain the results of the calculations of the values of the physical properties mentioned, with both the observed values on which the determination of the constants was based, and a few additional observed values for stronger solutions.

The following comments may be made on the above tables:—

Density.—NaCl. The first four observations were used in determining the constants; and up to a concentration of 1·5 the differences are within the limits of experimental error, and show a satisfactory alternation of sign.—KCl. The first five observations were used. The differences are large, but the alternation of sign shows that the expression is applicable. On plotting Bender's values they are readily seen not to lie on a smooth curve.

Thermal Expansion.—NaCl. The first four observations were used. The differences up to a concentration of 2·5 are probably within the limits of experimental error, and their alternation of sign is satisfactory.—KCl. The first five observations were used. The differences are not so satisfactory as in the case of the sodium salt either as to magnitude or sign; but on plotting the observations the third is seen to be somewhat out; and it is obviously to this observation that the defective agreement is due.

Viscosity.—NaCl. The first five observations were used. The differences are within the limits of error, but the signs are not satisfactory. The fourth observation, however, appears to be defective. Mützel[*], in applying a formula for viscosity in terms of density and concentration to these observations, found also that this observation was out. It is worth noting, also, that Mützel found his formula, which expressed the increase of viscosity due to the salt in solution on the assumption that the only action occurring was between salt and water, was applicable to only the first five of the above observations. To represent the viscosity of stronger solutions he had to introduce a term expressing the effect due to the mutual action of the molecules of salt.—KCl. All six observations were used. The agreement in this case is quite satisfactory.

Surface-Tension.—NaCl. Only two observations on sufficiently dilute solutions were available, and the applicability of the formula cannot therefore be tested. The constants were found, for use in the calculation of the surface-tension of mixtures.—KCl. The first four observations were used,

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[*] Wied. *Ann.* xliii. (1891) p. 35.

and the agreement is quite satisfactory up to a concentration of 3 grm.-mols. per litre.

Refractive Index.—In the case of both salts the first three observations were used, and in both the agreement is quite satisfactory up to a concentration of about 2 grm.-mols. per litre.

The above tables seem to be at any rate quite consistent with the possibility of expressing the values of at least five of the physical properties of moderately dilute solutions in terms of their state of ionization. I hope to find leisure at an early date to extend the investigation to solutions of salts and acids of a more complex character and to other properties.

The following are the ionization-constants used in the above calculations :—

	Sodium Chloride.		Potassium Chloride.	
	k.	*l,*	*k.*	*l.*
Density	+·030841	+·045079	+·03543	+·048591
Thermal Expansion	−·0001445	+·0007658	−·000614	+·00069685
Viscosity	+·002347	+·0001504	+·001904	−·0009247
Surface-Tension ...	+·20574	+·11001	+·24249	+·126806
Refractive Index...	+·006318	+·011713	+·0027853	+·011853

These constants are obtained from solutions of too great concentration to be regarded as exact values. Nevertheless it may be admissible so far as these two salts are concerned to draw the following conclusions :—Undissociated and dissociated molecules are nearly equally effective in increasing the density, those dissociated, however, being somewhat the more effective of the two. (2) Undissociated molecules diminish the thermal expansion, those dissociated increasing it to a greater extent. (3) In the case of viscosity it is the undissociated molecules which have the preponderating influence, those dissociated having but a slight effect, which may be an increasing or a diminishing effect. Thus Arrhenius's expectation that all dissociated ions would be found to diminish viscosity seems to be only partially realized, though possibly from observations on more dilute solutions, both *l*'s might be found to be negative. (4) In the increase of surface-tension the undissociated molecules have about twice as great an influence as the dissociated. (5) In increasing the refractive index it is the dissociated molecules which have the preponderating influence; and their superiority is greater in increasing the refractive power than in increasing the density.

Mixtures of Solutions.

For a solution containing several salts, 1, 2, &c., the value of a property, according to the conception under consideration, will be :—

$$P=P_w+k_1(1-\alpha_1)n_1+l_1\alpha_1n_1+k_2(1-\alpha_2)n_2+l_2\alpha_2n_2+\text{ \&c.}, \quad . \ (2)$$

the n's being numbers of gramme-equivalents per unit volume of the solution. If the solution have been formed by the mixture of the volumes v_1 and v_2 of two simple solutions of salts, having one ion in common, for which, before the mixing, the property had the values :—

$$\left.\begin{array}{l} P_1=P_w+k_1(1-\alpha_1)n_1+l_1\alpha_1n_1, \\ P_2=P_w+k_2(1-\alpha_2)n_2+l_2\alpha_2n_2, \end{array}\right\} \quad . \ . \ . \ . \ . \ (3)$$

then, since, on mixing, the state of ionization will, in general, change, we shall have as the value of the property for the mixture, assuming no change of volume on mixing,

$$P=P_w+(k_1(1-\alpha_1')n_1+l_1\alpha_1'n_1)\frac{v_1}{v_1+v_2}+(k_2(1-\alpha_2')n_2+l_2\alpha_2'n_2)\frac{v_2}{v_1+v_2}, \ (4)$$

the n's being numbers of gramme-equivalents per unit volume of the original simple solutions, and α_1' and α_2' being the ionization-coefficients in the mixture. As the values of the k's and l's have been determined above for sodium and potassium chloride for a number of properties, and as I have shown in my paper on the "Conductivity of Mixtures," cited above, how the ionization-coefficients after mixing may be determined, it should be possible to predict the values of these properties for mixtures of solutions of these salts.

The following tables show that this can be done. The ionization-coefficients were determined in the way described in the paper referred to. The constants k and l employed in the calculations were those determined above. The observations were made by the authors whose determinations for simple solutions were used above; in fact in most cases it was the solutions of the tables given above which were mixed. The limits of experimental error are thus of about the magnitudes mentioned above in each case. All remarks made above with regard to the reduction of observations to 18° C., the values of the property for water, &c., apply also to the mixtures. In all cases, except that of surface-tension, the solutions mixed were mixed in equal volumes. Unfortunately, Bender made no observations on the refracting power of mixtures.

Constituent Solutions (grm.-mols. per litre).		Ionization Coefficients in Mixture.		Observed Value.	Calculated Value.	Difference.
NaCl.	KCl.	NaCl.	KCl.			
Density (Bender's observations).						
1·0	0·1875	·7268	·7720	1·02358	1·02350	$-0{\cdot}0_{3}08$
,,	0·375	·714	·7625	1·02785	1·02766	− 19
,,	0·75	·688	·7629	1·03641	1·03600	− 41
,,	1·0	·6728	·7632	1·04139	1·04158	+ 19
,,	1·5	·6494	·7478	1·05293	1·05263	$-0{\cdot}0_{3}30$
,,	3 0	·6143	·7283	1·08580	1·08595	+ 15
Thermal Expansion (Bender's observations).						
1·0	0·1875	·7268	·7720	·001174	·001174	$\pm0{\cdot}0_{4}00$
,,	0·375	·714	·7625	·001208	·001203	− 05
,,	0·75	·688	·7629	·001275	·001264	− 11
,,	1·0	·6728	·7632	·001297	·001305	+ 08
,,	1·5	·6494	·7478	·001376	·001376	± 00
,,	3	·6143	·7283	·001543	·001596	+ 53
Viscosity (Brückner's observations).						
1·0	0·5	·7059	·7635	·010940	·010947	$+0{\cdot}0_{4}07$
,,	1·0	·6728	·7632	·010918	·010920	+ 02
,,	1·5	·6494	·7478	·010876	·010915	+ 39
,,	2·0	·6143	·7283	·010890	·010956	+ 66

Surface Tension (Rother's observations).

Constituent Solutions.				Ionization Coefficients in Mixture.		Observed Value.	Calculated Value.	Difference.
Concentration (grm.-mols. per litre).		Volume (litres).						
NaCl.	KCl.	NaCl.	KCl.	NaCl.	KCl.			
0·8862	0·6836	0·14487	0·14545	0·6906	0·7632	7·477	7·472	−0·005
1·8109	1·411	0·13993	0·14096	0·6087	0·7279	7·607	7·602	− 5
0·8824	2·1822	0·14489	0·13659	0·6123	0·7311	7·600	7·591	− 9
2·8406	0·6862	0·13511	0·14544	0·6027	0·7298	7·622	7·616	− 6
1·8155	2·9887	0·13996	0·13241	0·5185	0·7125	7·734	7·810	+ 76

It will be noticed that in the case of the third mixture of the density and thermal expansion series (the same mixture) the differences are comparatively large, but it is obvious from the data of the fourth column in these series that the ionization-coefficients have not been accurately determined for mixtures of about the concentration of the one referred to. With these exceptions the agreement between observed and calculated values is satisfactory, the differences being either well within or at worst on the limit of observational error, up to mean concentrations of about 1·5. The determination of the ionization-coefficients was especially difficult in the case of the surface-tension observations, because Rother mixed equal weights of his simple solutions, not equal volumes. Nevertheless, in all except the strongest of these mixtures, the differences are probably not beyond the limits of experimental error. Obviously, alternation of sign is not to be expected in these calculations.

[To be continued.]

IX. *On a complete Apparatus for the Study of the Properties of Electric Waves. By* JAGADIS CHUNDER BOSE, *M.A. (Cantab.), D.Sc. (Lond.), Professor of Physical Science, Presidency College, Calcutta*[*].

THE work of Hertz and his eminent successors, both here[†] and on the Continent, has opened out for study a new region of æthereal vibration, bridging over the gap that hitherto existed between the comparatively slow æther vibrations and the quick oscillations which give rise to radiant heat. In the vast range of possible æther vibrations we recognize only a few octaves by our senses; the rest are beyond our perception. Many unexpected properties of

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[*] Read before the British Association at Liverpool, 21st Sept., 1896. Communicated by Lord Kelvin, F.R.S.

[The apparatus described in this communication is founded on Prof. Oliver Lodge's and M. Branly's discovery of the "coherer" for detecting electric waves. The general design of the apparatus, both in respect to generator and receiver, was given originally by Prof. Lodge, and described in his book 'The Work of Hertz and some of his Successors, published by the Electrician Co. in 1894.—EDITORS.]

[†] By "here" is meant not only England, but Professor Lodge's laboratory especially, where the paper was read, and where, as is well known, some of the most important investigations on electric radiation have been carried out. For my interest in the subject I owe greatly to Prof. Lodge.

these little-known æther waves are now being gradually discovered. Confining our attention to the electric waves, we find that there are many important problems which may perhaps be better attacked with these comparatively slow vibrations; among which may be mentioned the determination of the indices of refraction of various substances which are opaque to visible light, but are transparent to the electric ray; the relation between the dielectric constant and the refractive index when the rates of oscillation are made comparable in the two determinations; the variation of the index with the frequencies of vibration. Then there are the phenomena of double refraction, polarization, and the magnetic rotation of the electric ray; the determination of the wave-length, and other problems of a similar nature.

The fascination of the subject drew me to its study, though the investigations were rendered exceedingly difficult in India from want of facility for making the necessary apparatus. I ultimately succeeded in constructing a few instruments with which I was able to obtain the values of the indices of refraction of various substances for electric waves, the wave-length of electric radiation, to demonstrate the phenomena of double refraction and polarization of the electric rays, and to find out certain substances which act as electric tourmalines. The simplified apparatus with which many of the properties of electromagnetic radiation may be studied is here exhibited. This is a duplicate made by Messrs. Elliott, Brothers, of the apparatus which I brought from India. I also take this opportunity of thanking Mr. Bolton, F.R.A.S., of the Mathematical Instrument Department, Calcutta, for the divided circle in my apparatus.

The following are the experiments which may be carried out with this apparatus:—

A. Verification of the laws of reflexion.
   1. Plane mirrors.
   2. Curved mirrors.

B. Phenomena of refraction.
   1. Prisms.
   2. Total reflexion.
   3. Opacity caused by multiple refraction and reflexion.
   4. Determination of the indices of refraction.

C. Selective absorption.
   1. Electrically-coloured media.

D. Phenomena of interference.

1. Determination of the wave-length by curved gratings.
2. Bi-prism experiments.

E. Double refraction and polarization.

1. Polarizing gratings.
2. „ crystals.
3. Double refraction produced by crystals.
4. „ „ other substances.
5. „ „ by strain.
6. Circular polarization.
7. Magnetic rotation.
8. Electro-polariscope and polarimeter.

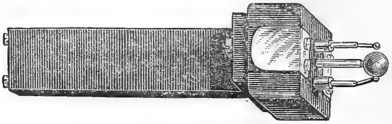
In the list of experiments above-mentioned, the determination of the wave-length by curved gratings has been carried out with a larger apparatus (*vide* Proc. Roy. Soc. vol. lx., "On the Determination of the Wave-length of Electric Radiation"). Experiments with circular polarization and magnetic rotation and with the bi-prism are still in progress. All the others have been repeated with the apparatus to be described below.

The complete apparatus consists of :—(1) A radiating apparatus emitting electric waves of short length ; (2) A receiver used as a detector of electric radiation; and (3) Various accessories for the study of the different phenomena.

I used various methods for the production of oscillatory discharge. One method was to imbed a row of metallic beads, with small spark spaces, in solid paraffin, the end beads being in connexion with the electric generator. Another method was to have the two sparking-balls immersed in kerosene ; this is effective, but troublesome. The simplest method, however, is Prof. Lodge's arrangement of two side balls and an interposed sphere.

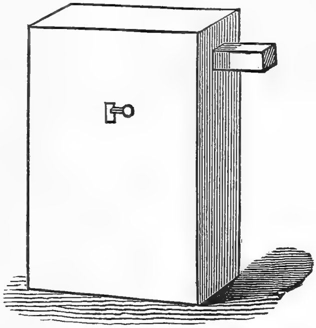
Electric oscillation is produced by sparking between two beads of platinum and an interposed sphere of the same metal. The discharge ceases to be oscillatory when the ball is roughened, and a platinum ball resists, to a great extent, the disintegrating action of the sparks. Two jointed electrodes carry the two beads at their ends. The distance between the beads and the interposed sphere can thus be adjusted. This is a matter of importance, as the receiver does not properly respond if the spark length is too large. It is more convenient to use short electric waves, and these are obtained by making the radiating spheres very small. The shortest wave-length produced is about 6 mm., and the corresponding number of oscillations is about 50,000 millions in a second. The frequency of vibration in this case will be seen to be about thirteen octaves lower than that which produces visible radiation. The intensity of radiation in the above case is rather feeble, and I use in general electric waves of about half an inch in length.

Fig. 1.—The Radiator.



The jointed electrodes carrying the beads are in connexion with a small modified Ruhmkorff's coil, actuated by a small storage cell. The usual vibrating interrupter is generally a source of trouble ; the contact points get worn out, and the break becomes irregular. The great objection (as Hertz found) to the continuous production of secondary sparks is the roughening of the surface of the radiating ball, by which the spark ceases to be oscillatory. It is very troublesome, in the middle of an experiment, to be obliged to take out the radiator for polishing. The flash of radiation produced by a single break is enough for an experiment, and it is a mere waste to have a series of useless oscillations. In my apparatus for quantitative measurements I have therefore discarded the vibrating interrupter in favour of a simple break-key. To economize space, I wind the condenser (a long strip of paraffined paper with tin foils on opposite sides) round the secondary of the coil, appropriate connexions being made with the interrupting key. The coil and a small storage cell are enclosed in a metal box, in accordance with the precautions which Prof. Lodge had found to be necessary. I used tinned iron in order to screen the space outside from magnetic disturbances due to the making or breaking of the primary circuit of the coil. A sudden magnetic variation disturbs the receiver. The iron box is placed inside a second box of thick brass or copper. These precautions are taken to prevent straying of electric radiation. Through a small opening in the back or side of the box the stud of the press-key projects. In front of the box is the radiator-tube, which may be square or cylindrical. Inside this tube is mounted the radiating originator. A flash of electric radiation is produced by a proper manipulation of the interrupting key. The radiating apparatus may thus be made very small and portable, and requires very little attention. After the storage cell is once charged, experiments may be carried on for days, a flash of radiation being produced at any time by merely manipulating the key.

Fig. 2.—The Radiating Box.



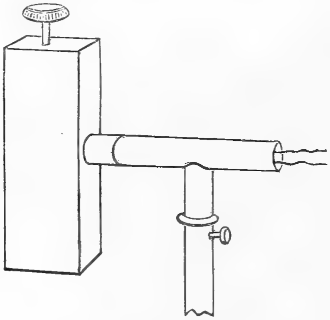
Spiral Spring Receiver.

For a detector of radiation I used a form of Prof. Lodge's coherer. At first I used one made of metallic filings, originally discovered by M. Branly; but great difficulty was experienced in making the receiver respond to different vibrations, and in the capriciousness of its response. The difficulty was still further enhanced when the radiator and the receiver had to be enclosed in narrow tubes to enable angular measurements to be made with any accuracy. It seemed to me that the frequent loss of sensibility might be due to the particles getting jammed together, and the fatigued condition of contact surfaces. In order to avoid this I used a layer of narrow spirals of steel, lying side by side, and rolling on a smooth surface. The points of contact are numerous, and fresh surfaces can be brought into action by a slight rolling of the spirals. By this spiral-spring arrangement the pressure exerted on contiguous spirals is also made fairly uniform.

From a series of experiments carried out to determine the other causes which may be instrumental in producing loss of sensibility, I found that the sensibility of the receiver to a given radiation depends (1) on the pressure to which the spirals are subjected, and (2) on the E.M.F. acting in the circuit. The pressure on the spirals may be adjusted, as will be described later on, by means of a fine screw. The E.M.F. is varied by a potentiometer-slide arrangement. This is a matter of great importance, as I often found a receiver, otherwise in good condition, failing to respond when the E.M.F. varied slightly from the proper value. The receiver, when subjected to radiation, undergoes exhaustion. The sensibility can, however, be maintained fairly uniform by slightly varying the E.M.F. to keep pace with the fatigue produced.

The receiving circuit thus consists of a spiral-spring coherer, in series with a voltaic cell and a dead-beat galvanometer. The receiver is made by cutting a narrow groove in a rectangular piece of ebonite, and filling the groove with bits of coiled steel springs arranged side by side in a single layer. The spirals are prevented from falling by a glass slide in front. The spirals are placed between two pieces of brass, of which the upper one is sliding and the lower one fixed.

Fig. 3.—The Spiral Spring Receiver.



These two pieces are in connexion with two projecting metallic rods, which serve as electrodes. An electric current enters along the breadth of the top spiral and leaves by the lowest spiral, having to traverse the intermediate spirals along the numerous points of contact. The resistance of the receiving circuit is thus almost entirely concentrated at the sensitive contact-surface, there being little useless short-circuiting by the mass of the conducting layer. When electric radiation is absorbed by the sensitive surface, there is a sudden diminution of the resistance and the galvanometer spot is violently deflected.

By means of a very fine screw the upper sliding-piece can be gently pushed in or out. In this way the spirals may be very gradually compressed, and the resistance of the receiver diminished. The galvanometer spot can thus easily be brought to any convenient position on the scale. When electric radiation falls on the sensitive surface the spot is deflected. By a slight unscrewing the resistance is increased, and the spot made to return to its old position. The receiver is thus re-sensitized for the next experiment.

The sensitiveness of the receiver may be increased by a proper adjustment of the E.M.F. acting on the receiving circuit. The receiver at each particular adjustment responds best to a definite range of vibration lying within about an octave. The same receiver could, however, be made to respond to a different range by an appropriate change of the E.M.F.; very careful adjustment of this is necessary to make the receiver respond at its best to a particular range of electric vibration. For simple experiments the adjustment of the receiver is not difficult; but for delicate experiments careful manipulation is necessary.

The proper adjustment of the E.M.F. is effected by taking a derived current from a circular potentiometer-slide, fixed at the base of the galvanometer. A simpler way is to take a U-tube, the two limbs being respectively filled with copper-sulphate solution and dilute sulphuric acid. Mixture of the two solutions is prevented by an interposed plug of asbestos. A rod of copper and a rod of zinc are plunged in the two electrolytes, the whole forming a modified Daniell cell. The cell is shunted by a suitable resistance, the receiving circuit being connected to the ends of the shunt. The current flowing through the shunt, and therefore the derived E.M.F. from its ends, is varied by plunging the rods more or less in the solutions.

The leading wires from the ends of the receiver are enclosed in layers of tin-foil; the galvanometer and cell have a metallic cover with a slit for the passage of the reflected spot of light. The receiving circuit is thus shielded from the disturbing action due to stray radiations.

The receiver is provided with a *collecting funnel.* This prevents lateral waves from acting on the receiver. The funnel has two hinged side-doors, by which its area—and, therefore, the amount of radiation collected—may be varied. When angular deviation is to be measured, the doors are made parallel and perpendicular to the layer of spirals. The aperture is reduced, and the receiver then only responds when the funnel points to the direction of the deviated ray.

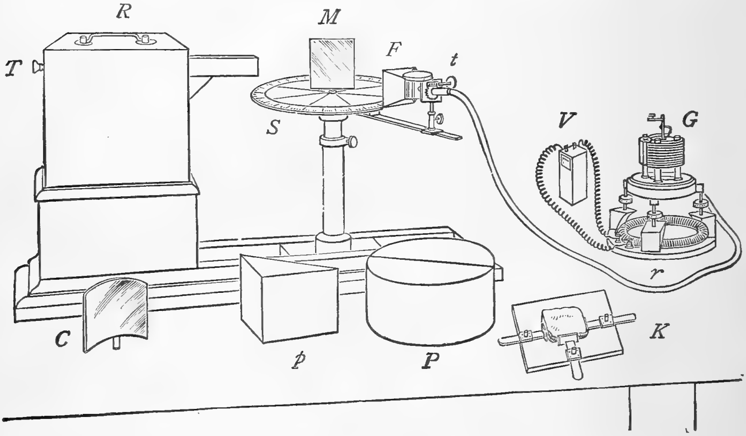
In polarization experiments it is necessary to adjust the receiver carrying the analyser in a crossed position. This is done by a tangent screw, the rotation of the analyser being measured by means of an index and a graduated vertical disk.

Arrangement of the Apparatus.

The radiating apparatus and the receiver are mounted on stands sliding in an optical bench. Experiments are carried out with divergent or parallel beams of electric radiation. To obtain a parallel beam, a cylindrical lens * of sulphur or ebonite is mounted in a square tube. This lens-tube fits on the radiator-tube, and is stopped by a guide when the oscillatory spark is at the principal focal line of the lens. The radiator-tube is further provided with a series of diaphragms by which the amount of radiation may be varied.

For experiments requiring angular measurement, a spectrometer-circle is mounted on one of the sliding stands. The spectrometer carries a circular platform, on which the various reflectors, refractors, &c. are placed. The platform carries an index, and can rotate independently of the circle on which it is mounted. The receiver is carried on a radial arm (provided with an index) and points to the centre of the circle. An observing telescope may also be used with an objective made of ebonite with a linear receiver at the focal plane. But an ordinary receiver provided with a funnel is all that is necessary for ordinary experiments.

Fig. 4.—Arrangement of the Apparatus. $\frac{1}{10}$ nat. size.



R, the Radiator. *T*, the Tapping Key. *S*, the Spectrometer-Circle. *M*, the Plane Mirror. *C*, the Cylindrical Mirror. *p*, Totally Reflecting Prism. *P*, the Semi-Cylinders. *K*, the Crystal-Holder. *F*, the Collecting Funnel attached to the Spiral Spring Receiver. *t*, the Tangent Screw, by which the Receiver is rotated. *V*, Voltaic Cell. *r*, the Circular Rheostat. *G*, the Galvanometer.

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* See Lodge and Howard, Phil. Mag. July 1889.

Laws of Reflexion.

Plane Mirror.—A parallel beam is used. The spectrometer-circle is adjusted with the zero division opposite to the radiator. The platform index is turned to zero, and a plane reflector placed on a previously marked diameter at right angles to the index. The receiver is placed, say, at 60°. The platform carrying the mirror is slowly rotated (electric radiation being at the same time produced by interrupting the key), till the receiver suddenly responds. It will now be found that the platform index points to 30°, midway between the radiator and the receiver.

Curved Mirror.—A cylindrical metallic mirror, with a radius of 25 cm., is placed on the platform, with its principal axis coinciding with the platform index. When the radiator is placed at a distance of 25 cm. from the mirror, the source of radiation would be at the axis of the cylinder. The reflected image will now be formed at an equal distance. The receiver mounted on the radial arm (at a distance of 25 cm. from the centre) is placed at a given angle ; the platform is rotated till the receiver responds. The index will now be found to bisect the angle included between the radiator and the receiver.

Refraction.

Deviation of the Electric Ray by a Prism.—An isosceles right-angled prism is made of sulphur or ebonite. Parallel beam is used. For showing deviation by refraction one of the acute angles is interposed on the path of the beam.

Total Reflexion.—An interesting experiment on total reflexion is shown in the following way :—The receiver is placed opposite to the radiator, and the prism interposed with one of its equal faces at right angles to the direction of the ray. The receiver will remain unaffected. The critical angle of ebonite being considerably less than 45°, the rays undergo total reflexion. On turning the receiver through 90° it responds to the totally reflected ray.

Opacity due to Multiple Refraction and Reflexion.—An experiment analogous to the opacity of powdered glass to light is shown by filling a long trough with irregular-shaped pieces of pitch, and interposing it between the radiator and the receiver. The electric ray is unable to pass through the heterogeneous media, owing to the multiplicity of refractions and reflexions, and the receiver remains unaffected. But on restoring partial homogeneity by pouring in kerosene, which has about the same refractive index as pitch, the radiation is easily transmitted.

Determination of the Indices of Refraction.—For the determination of the index the prism-method is not very suitable. I found the following to yield good results, the method depending on the determination of the critical angle. Two semicylinders of the given substance separated by an air-space are placed on the platform. When the radiator is placed at the principal focus of one of the semi-cylinders the rays emerge parallel into the air-film, and are then focussed on the receiver by the second semi-cylinder. A metallic plate with a narrow rectangular opening is interposed between the semi-cylinders to serve as a diaphragm, and cut off all but the central rays. As the platform is rotated, the incident angle on the plane surface separating the two media is gradually increased till the rays undergo total reflexion. When this is the case the receiver, which is placed opposite the radiator, suddenly ceases to respond. The trouble of following the deviated ray is thus obviated; the reading is also well defined as the transition from refraction to total reflexion is abrupt. The index-reading is now taken, and the cylinders rotated in an opposite direction till total reflexion takes place a second time. The difference of readings as given by the index in the two positions is evidently equal to twice the critical angle. Hence the value of the index can easily be deduced.

Fig. 5.

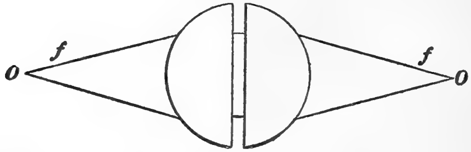
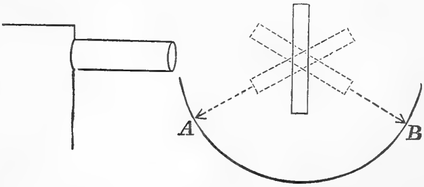


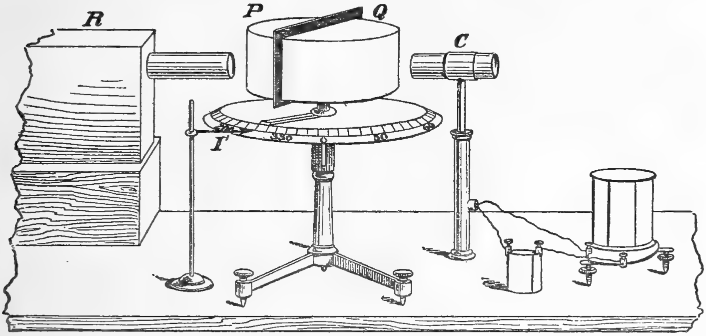
Fig. 6.



(The dotted lines show the two positions of the air-film for total reflexion.)

A preliminary experiment gives the approximate value of the index, from which the focal distance of the semi-cylinder is roughly calculated. The spark-gap of the radiator is placed at this focus, and the experiment repeated. In this way I have determined the indices of refraction of several solids for the electric ray (*vide* "On the Determination of the Indices of Refraction of various Substances for the Electric Ray," Proc. Roy. Soc. vol. lix.). The index of refraction of commercial sulphur is $=1{\cdot}73$; that of a specimen of pitch $=1{\cdot}48$.

Fig. 7.—Electric Refractometer.



R, the Radiator. *C*, the Receiver.

Indices for Liquids.—A cylindrical trough is filled with the given liquid ; two thin parallel glass plates enclosing an air-space are vertically placed so as to divide the liquid cylinder into two halves. The readings for total reflexion are taken as in the last case. The index for a specimen of coal-tar I found to be 1·32.

Selective Absorption.

A substance is said to be coloured when it allows light of one kind to pass through, but absorbs light of a different kind. If we take into account the entire range of radiation there is hardly a substance which is not, in this sense, coloured. In the spectrum of radiation transmitted through glass, for example, two broad absorption-bands would be observed, one in the ultra-violet, and the other in the infra-red, the electric and the visible rays not being absorbed to any great extent. A brick or a block of pitch would absorb light, but would transmit the electric ray. On the other hand, a stratum of water, though transparent to light, would absorb the electric ray. These substances exhibit selective absorption, and are therefore coloured.

If we take into account the electric radiation only, it would no doubt be found that radiations having different wave-lengths are unequally absorbed by different substances.

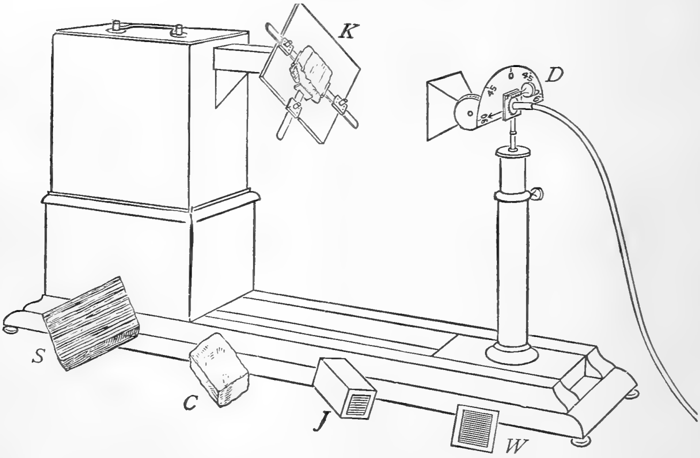
Phenomena of Interference.

Determination of the Wave-Lengths by Diffraction Gratings.—In a paper read before the Royal Society in June last (*vide* Proceedings of the Royal Society, vol. lx.) I have given an account of a method of obtaining pure spectra of electric radiation by means of curved gratings. The experiment was carried out with a large apparatus. The spectrum obtained was well defined, and appeared to be linear, and not continuous. I had not time to adapt the experiment to this small apparatus, but I think it would not be difficult to do so.

Double Refraction and Polarization.

The spectrometer circle is removed, and an ordinary stand for mounting the receiver substituted. By fitting the lens-tube the electric beam is made parallel. At the end of the lens-tube there is a slot in which is dropped the wire-grating polarizer. A crystal-holder provided with three sliding jaws is fitted on to the lens-tube, and is capable of rotation round an axis parallel to the direction of the electric ray.

Fig. 8.—Polarization Apparatus.



K, the Crystal-Holder. *S*, a Piece of Stratified Rock. *C*, a Crystal. *J*, the Jute Polarizer. *W*, the Wire-Grating Polarizer. *D*, the Vertical Graduated Disk, by which the Rotation is measured.

The receiver carrying the analyser is also capable of rotation round a horizontal axis by means of a tangent screw. The angular rotation is measured by means of an index fixed to the analyser, and a graduated vertical disk.

The gratings are made by winding fine copper wire, parallel, round square frames, as used by both Hertz and Lodge. A series of parallel slits cut in a metallic plate serves the purpose very well. Other forms used—the serpentine and the jute polarizers—will be described later on.

The spark-gap is placed vertical, and the polarizer is adjusted with wires horizontal. The emergent beam is now completely polarized, the vibration taking place in a vertical plane passing through the axis.

The analyser fitted on to the receiver may be placed in two positions :—

(1) Parallel position. When both the gratings are horizontal.

(2) Crossed position. When the polarizing grating is horizontal, and the analysing grating vertical.

In the first position the radiation, being transmitted through both the gratings, falls on the sensitive surface, and the galvanometer responds. The field is then said to be bright. In the second position the radiation is extinguished by the crossed gratings, the galvanometer remains unaffected, and the field is said to be dark. But on interposing certain crystals with their principal planes inclined at 45° to the horizon, the field is partially restored, and the galvanometer spot sweeps across the scale. This is the so-called depolarization action of double-refracting substances *.

Experiments with Wire Gratings.—A wire grating at 45° interposed between the crossed analyser and polarizer partially restores the field, but ordinary wire gauze does not transmit any radiation, the action of one set of wires being neutralized by that of the other set at right angles.

Double Refraction Produced by Crystals.—The crystals to be examined are mounted on the holder, and properly inclined. Double refraction is shown by all crystals belonging to the Rhombic, Rhombohedral, Triclinic, and Monoclinic systems. The effects exhibited by the following are very marked, small pieces even producing depolarization.

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* For a detailed account of experiments on the polarization of the electric ray, I would refer to my paper, "On the Polarization of the Electric Ray by Double-refracting Crystals," read before the Asiatic Society of Bengal, May 1895, and two subsequent papers ("On a new Electro-Polariscope" and "On Double-refraction of the Electric Ray by a Strained Dielectric") published in the 'Electrician,' 27th December, 1895.

(1) *Serpentine.*—This substance, which appears fibrous, transmits the ordinary and the extraordinary ray, with unequal intensity. A fairly thick piece completely absorbs vibrations parallel to the fibres, and transmits vibrations perpendicular to the fibres. Ordinary radiation, after transmission through a thick piece of serpentine, would be plane-polarized, the vibration taking place perpendicular to the fibres. A thick piece of serpentine thus acts as an efficient polarizer.

There are certain important points in connexion with selective conductivity and the phenomena of polarization by absorption exhibited by certain substances, which will be dealt with in a future paper.

(2) *Nemalite.*—This crystal exhibits this effect in a still more marked degree.

(3) *Tourmaline* also produces the depolarization effect. The difference in absorption of the ordinary and the extraordinary rays is, however, not so great as in the case of light.

(4) *Beryl*, *Apatite*, and *Barytes* are also very good crystals for exhibiting the depolarization effect.

Polarization Produced by other Substances.—I found many other natural substances producing polarization, the most interesting being vegetable fibres. Common jute (*Corchorus capsularis*) exhibits the property in a very marked degree. I cut fibres of this material about 3 cm. in length, and built with it a cell with all the fibres parallel. I subjected this cell to a strong pressure under a press. I thus obtained a compact cell 3 × 3 cm. in area, and about 5 cm. in thickness. This was mounted in a metallic case, with two openings about 2 × 2 cm. on opposite sides for the passage of the radiation.

This cell was found to quench vibrations parallel to the fibres, and transmit vibrations perpendicular to the fibres. Jute cells could thus be made to serve as polarizers or analysers.

Effect due to Strain.—Could be exhibited by stratified rocks, the plane of stratification being inclined at 45° to the horizon.

Effects similar to that produced by unannealed glass can be imitated by a block of unequally chilled paraffin.

The polarization-apparatus described above may also be used as a polarimeter, the rotation of the analyser being measured by the graduated disk.

X. *Notices respecting New Books.*

The Principles of the Transformer. By Frederick Bedell, *Ph.D., Assistant Professor of Physics in Cornell University.* New York. The Macmillan Co. 1896.

THE transformer has played so important a part in the development of alternate-current systems of distributing electricity that no excuse is needed for the publication of a treatise setting forth the principles of its action. Such a treatise, however, unless it takes into account the fact that nearly all transformers possess iron cores, degenerates into little more than a mathematical discussion of simple harmonic functions with constant amplitudes, and its practical value is thereby greatly diminished. The author of the present work has, in our opinion, devoted too much space to mathematics of this kind, as he only gives one short chapter on the effects of hysteresis and Foucault currents. It is somewhat disappointing to read at the end of the volume that the lag due to hysteresis and eddy currents may be "assumed to be 45° as an approximation in a closed magnetic-circuit transformer." Surely the designer of transformers will require something more definite than this! The chapters on construction and testing contain much useful information, and the interesting results are described of experiments upon transformers with open and closed magnetic circuits, made by the method of instantaneous contact.

The volume is well provided with figures, and especially with diagrams and curves; indeed the author has made use largely of the geometrical methods introduced by Blakesley and others in his treatment of periodic functions. J. L. H.

Geological Survey of Canada. Annual Report. New Series. Vol. VII. *Reports* A, B, C, F, J, M, R, S, 1894. 8vo; 1206 pages. With 11 maps, 15 plates, and numerous figures in the text. Ottawa: Dawson, 1896.

Report "A" (pp. 1–124) is a summary of the work of the Geological Survey for 1894, as to explorations and surveys, and the work in the Museum and Office.

The Kamloops map-sheet to which the Report "B" (pages 1–427), by the Director, Dr. G. M. Dawson, relates covers a portion of the Interior Plateau of British Columbia, with a comparatively small width of mountains of the adjacent Coast Ranges, on its western edge, which are quite distinct from the "Coast Ranges" of California and Oregon. It is a square of which the sides measure eighty miles, and consequently it includes an area of 6400 square miles, all parts of which are in the drainage-basin of the Fraser River and its tributaries. The physical features are first described, and, as in the other Reports, illustrated by some good plates from photographic views. The general geology follows.

The oldest rock-formations in the Eastern District are the

Nisconlith Series (argillites, chiefly), 15000 ft., and the Adams-Lake series (chiefly volcanic rocks), 17100 ft. These are Lower Palæozoic (chiefly Cambrian); and are succeeded by other palæozoics, the Campbell-Creek Beds, and other argillites, with limestones and volcanic rocks, 12500 ft. In the Western District there are Upper Palæozoic rocks (chiefly Carboniferous, with perhaps Devonian and Permian), consisting of the Câche Formation, of which the lower part (argillites, quartzites, volcanic rocks, and some limestone) is 6500 ft. thick; and the upper part has the Marble-Cañon limestone, some volcanic rocks, argillites, and quartzites, 3000 ft. Above these is the Nicola Formation; volcanic, with some limestones and argillites (Triassic and to some extent Lower Jurassic), from 7500 ft. to 13500 ft. Rocks of early Cretaceous age succeed, the Queen-Charlotte Islands formation (chiefly), consisting of sandstones, conglomerates, and argillites, 7000 ft. Next follow the Tertiaries. Of these the Coldwater group of conglomerates and sandstones (5000 ft.) is Oligocene. The Tranquille beds, volcanic (porphyrites, bedded tuffs, and basalt), 9400 ft., are Miocene. Some conglomerates referable to lake- and river-gravels, formed from the denudation of the Miocene basalts &c., belong probably to the Pliocene period; and others are possibly masked by the Drift deposits.

The relationship of the several formations to those known elsewhere is carefully pointed out. Detailed description of the more interesting rocks is given, including noticeable points in foliation, metamorphism, and other characters, metalliferous conditions, &c. The Plutonic rocks (granitic, sometimes auriferous) are separately noticed at pages 238–248.

Glaciation and Surface-deposits, or the Drifts, Boulder-clay, 3000 ft. to 5000 ft. thick, Moraines, &c. (Pleistocene), pages 248–302,—and the Glacial and Post-glacial history of the Fraser and Thompson Valleys (pages 302–310),—subjects of great interest to Glacialists at home and abroad, are well treated and illustrated.

The fossils observed in the several series of strata are mentioned as follow:—

Page 42 (Upper Palæozoic), *Fusulina* &c. in the lower part of the Câche-creek formation, and some other fossils at page 80. At pages 50–52 fossils referable to the Trias, and some to the Lower Jurassic, are quoted from the Nicola Series. Some Molluscan fossils at p. 64, and some fossil Plants at p. 148, are described as characterizing the Cretaceous series. The fossil Insects and Plants (chiefly the latter) coming from the Tertiary beds are enumerated at pages 75, 165, 231, and 234.

The minerals of economic worth constitute the next subject of this Report:—Gold, in the rock and in placers, is treated in detail at pages 310–340 as to the localities and conditions under which it is found and likely to be further discovered in the part of British Columbia here dealt with. Other minerals, ores, and useful stones of the region are noticed in detail, pages 340–348.

Four valuable Appendices follow:—1. The Petrology of the rocks of the district. 2. The Shuswap names of places in the district. 3. Upper and lower limits of the growth of trees and other plants. 4. Temperature at different heights.

The other Reports (C, F, J, M) on the physical features, geology, and economic products of parts of Canada are not so extensive as Report A; but are carried out with the same great care and full appreciation of the importance of exactness of both detailed and general information to the well-being of the bread-winning colonists, and knowledge-seeking geologists there and elsewhere.

Report "C" is On an Exploration of the Finlay and Omenica Rivers, by R. G. MacConnell. Report "F" On the Vicinity of Red Lake and Berens River, Keewatin, by D. R. Dowling. Report "J" is On part of the Province of Quebec, by R. Wells; and on the Laurentian, north of the St. Lawrence, by F. D. Adams. Report "M" treats of the Surface Geology of Eastern New-Brunswick; North-Western Nova-Scotia; and part of Prince-Edward Island, by R. Chalmers.

G. R. Hoffmann in Report "R" gives the results of the Laboratory work of the Survey during 1894. Nearly 700 specimens were received for examination; and the Report gives the results of such examinations, analyses, and assays as may be of general interest. Miscellaneous minerals, mineralogical notes, coals, iron ores, nickel and cobalt, marls, gold and silver, natural waters, and miscellaneous rocks, &c., occur in the order of subjects.

The very elaborate and useful Report "S" consists of the Mineral Statistics for 1893 and 1894, by E. D. Ingall and H. P. H. Brummell. The substances noticed are—grindstones, diatomaceous (here termed infusorial) earth, asbestus, chromite, coal, copper, graphite, gypsum, iron, lead, manganese, mica, nickel, petroleum, precious metals, and miscellaneous. The estimated value of the minerals and mineral products of Canada in 1893 was $21,100,100; in 1894 $20,950,000. The value of the *exports* of these materials from Canada in 1893 was $6,045,459; and the countries to which they were sent are also tabulated. The *imports* of similar materials into Canada in 1892–1893 were valued at $25,377,824.

This goodly volume of useful geological and economic information has a clear Table of Contents of eight pages, and a full Index.

The Scientific Papers of JOHN COUCH ADAMS, *M.A., Sc.D., D.C.L., LL.D., F.R.S., late Lowndean Professor of Astronomy and Geometry in the University of Cambridge.* Vol. I. Edited by WILLIAM GRYLLS ADAMS, Sc.D., F.R.S. With a Memoir by J. W. L. GLAISHER, Sc.D., F.R.S. (Cambridge: at the University Press.)

Dr. GLAISHER, who contributes the appreciative biographical notice prefixed to this volume (which occupies thirty-four pages), also wrote the obituary notice of Prof. Adams published in the Annual Report of the Royal Astronomical Society for February 1893. All the circumstances connected with the memorable discovery of Neptune are here given in great detail; and appended are Prof. Challis's

First Report to the University Syndicate after the discovery of the planet, and a facsimile of Adams's memorandum, dated 1841, July 3, of his design to take up the subject of the cause of the irregularities in the motion of Uranus, which was ably carried out in 1845, and would have led to the discovery of the planet, had it been searched for, a year before it was actually found. The scientific papers contained in the volume before us comprise all those which were published by the late Prof. Adams during his lifetime, extending from 1844 (when he was 23 years of age) to 1890 (two years before his death), and appear under the careful editorship of his brother, Dr. W. Grylls Adams. The earliest relate of course to the investigation of the place of Neptune before it was found, and the calculations of its orbit after its discovery in 1846. As is well known, Prof. Adams's most important subsequent work related to the secular acceleration of the moon's mean motion and other matters relating to the lunar theory. But there are also many miscellaneous papers—on the motions of certain comets, on the orbit of the November meteors, besides a considerable number on pure mathematical subjects—and no fewer than five addresses at presentations of the Gold Medal of the Royal Astronomical Society; the last of these (on February 11, 1876) being to the late M. LeVerrier, the co-discoverer with himself of Neptune. The volume is indispensable to all students of what was formerly called physical astronomy; but as it is called a first volume, the reader naturally asks what is to follow. We will answer in Dr. Grylls Adams's words in his Preface:—"Besides these there are many papers on various branches of Astronomy which were left in an incomplete state among Prof. Adams's manuscripts. These are being prepared for publication by Professor Sampson."

We are informed in Dr. Glaisher's biographical notice above referred to that the investigations contained in the manuscripts in question will suffice to form, when published, what will practically be a treatise on the lunar theory intermediate to the existing text-books and such complete theories as those of Plana and Delaunay. Adams received the Gold Medal of the Royal Astronomical Society in 1866 for his contributions to the development of the lunar theory, the address being given by De La Rue. When Challis resigned the directorship of the Cambridge observatory in 1861 Adams was appointed his successor (uniting the appointment with the Lowndean instead of the Plumian professorship), and remained there until his death on the 21st of January 1892, having declined the offer to succeed Sir George Airy as Astronomer Royal in 1881. He visited America in 1884 as one of the delegates for Great Britain to the International Prime Meridian Conference held at Washington.

XI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xlii. p. 450.]

November 4th, 1896.—Dr. Henry Hicks, F.R.S., President, in the Chair.

THE following communications were read:—

1. 'Additional Note on the Sections near the Summit of the Furka Pass (Switzerland).' By T. G. Bonney, D.Sc., LL.D., F.R.S., V.P.G.S., Professor of Geology in University College, London.

The author, during a visit to Switzerland in 1895, had taken the opportunity of completing the examination of the sections on the western side of the Furka Pass, and of glancing again at those previously studied. The white, sometimes slightly quartzose or micaceous, marble which, as already described, crosses the summit of the Pass, descends towards the west, but forms a cliff for some little distance by the roadside till it is crossed by the latter, and disappears under débris and turf. Above it is a greyish limestone, at most only subcrystalline in aspect, and retaining traces of organisms, as already noticed. Higher up is a small outcrop of a whitish rock, more like the marble in a very crushed condition than a Jurassic limestone; next comes indubitable Jurassic limestone, and lastly gneiss. In one place the top of the lower mass of marble could be seen within a few inches of dark shaly Jurassic rock. On the eastern side of the Pass two small pits had been opened since the author's last visit; they also showed the top of the marble underneath the Jurassic rock. Both rocks were rather shattered near the junction, but were as different as they well could be. The one resembles the marble, associated elsewhere in the Alps with crystalline schists; the other, a member of the Jurassic system. There is not the slightest sign of a passage between them, but much to suggest faulting. The field evidence is confirmed by study, macroscopic and microscopic, of the specimens. Accordingly the author adheres to the view already expressed, that the white marble is a rock much older than the Mesozoic era.

2. 'Geological and Petrographical Studies of the Sudbury Nickel District (Canada).' By T. L. Walker, Ph.D., M.A.

Sudbury is a small town situated in Northern Ontario, in the centre of the nickel-mining district. North of the Great Lakes granite and gneiss form almost boundless terranes, interrupted only by belts of Huronian rocks, which are in turn associated with post-Huronian eruptives, the most important of which are the large nickel-bearing massives.

The Huronian rocks in the vicinity of Sudbury were examined by Prof. Bonney, who published his results in the Quarterly Journal of this Society (vol. xliv., 1888). These rocks form a large belt extending from the northern shore of Lake Huron north-eastward for several hundred miles. In the immediate vicinity of Sudbury they are composed of quartzite, mica-schist, phyllite, slate, volcanic breccia, and greywacke.

Far more interesting are the nickel-bearing rocks, which are eruptive and form long elliptical stocks which conform to the strike of the Huronian rocks containing them. Contact-action indicates that they are younger than the rocks previously referred to. The smaller eruptives are composed of greenstone, which appears to have been formed from norite or gabbro. Some of the larger eruptives, however, have been highly differentiated on cooling, as they are now composed of granite and greenstone with gradual transitions from the one to the other. The greenstone generally forms one side of the eruptive, and on the outer border is often characterized by large masses of nickeliferous pyrrhotite, chalcopyrite, and nickeliferous pyrite, with frequent smaller masses of magnetic iron-ore rich in titanic acid. The writer regards these mineral masses as genetically related to the greenstone and granite, in that they appear to be the extreme products of differentiation. About half the world's nickel supply is drawn from these deposits.

The greenstone is generally somewhat altered, but at times it is only slightly changed, when it is seen to be typical norite.

The alteration of augite and hypersthene is described in detail, and the term 'migration' is suggested for the process in which secondary hornblende is formed in plagioclase, as if bastite-substance had been carried in solution along the cleavage-lines of the felspar and by reacting upon its host had produced secondary hornblende. Hornblende so formed is referred to as 'emigrated hornblende.' Areas of uralitic hornblende generally extinguish under crossed nicols in two portions. On a very favourable section it was possible to determine that the two portions of hornblende are in definite crystallographic orientation—namely, that of twinning on the orthopinacoid, as is common in hornblende-crystals.

A mineral resembling wöhlerite was found to be relatively abundant in some of the more acid rocks of the Windy Lake eruptive.

The nickeliferous rocks are cut by younger eruptives—stocks of granite and dykes of olivine-diabase.

A few pages are devoted to the subject of differentiation, in which it is pointed out that no one of the generally accepted theories is able to account for all the phenomena observed in the differentiation of the nickel-bearing rocks of the Sudbury district. Stress has been laid upon the part which gravitation undoubtedly plays in producing heterogeneity in eruptive rocks. All the old theories of differentiation are directed to explain the presence of basic borders on more acid central portions, while they do not account for those cases where the central portions of stocks are basic and the margin acid. They fail also to give any explanation of the commonest case, namely, the eruptive showing little or no differentiation. These different cases are not only explained, but predicted, by the application of the principle of gravitation to slowly-cooling eruptive magmas.

3. 'On the Distribution in Space of the Accessory Shocks of the Great Japanese Earthquake of 1891.' By Charles Davison, Sc.D., F.G.S.

The object of the author in this paper is to consider the geographical distribution of the numerous shocks which preceded and followed the great earthquake of 1891. A brief summary of Prof. Ōmori's work on the distribution of the after-shocks of this earthquake is given, and the difference between his method of treatment and that adopted in the present paper pointed out; the author furthermore indicates possible sources of error in his maps, and explains how these may be practically neglected.

In a map of the coast within the Mino-Owari district, the boundary of the area over which the principal shock was felt is enclosed by a line which bifurcates towards the south. The longer axis of this area coincides generally, as shown by Prof. Koto, with the direction of a fault-scarp which, however, is only prolonged into the south-eastern fork of the disturbed area. This the author speaks of as the main fault, and he infers the evidence of a secondary fault running along the southern fork.

In discussing the preparation for the great earthquake, reasons are given for believing that the distribution of earthquakes in 1890–91 was little, if at all, due to the marked shock of May 12th, 1889, but that the earthquakes of these years were preparatory to the great earthquake, the consequent relief at numerous and widely distributed points equalizing the effective strain along the whole fault-system, and so clearing the way for one or more almost instantaneous slips along its entire length. This outlining of the fault-system points to the previous existence of the faults, and implies that the great earthquake was due not to the rupturing of the strata, but probably to the intense friction called into action by the sudden displacement.

The distribution of the after-shocks is then discussed, and it is maintained that the after-shocks of the Mino-Owari earthquake for the first fourteen months were subject to the following conditions:—decline of frequency, decrease in the area of seismic action, and a gradual but oscillating withdrawal of that action to a more or less central district.

XII. *Intelligence and Miscellaneous Articles.*

ON THE ACTION OF RÖNTGEN RAYS ON A JET OF STEAM.

BY FRANZ RICHARZ.

PROF. RÖNTGEN has already found that air traversed by his rays becomes an electrical conductor, and this property lasts for a short time even when the Röntgen rays no longer pass. According to the theory first put forward by Hr. W. Giese and Prof. Arthur Schuster, electricity is conducted in gases by dissociated atoms, by "ions,"—a theory which has been powerfully confirmed by many experiments[*]. Robert von Helmholtz, in a research published

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[*] See Wiedemann's *Annalen*, vol. lii. p. 389 (1894).

by himself[*], and in a second one in conjunction with myself published after his death[†], showed that not only does fine dust produce condensation in a jet of steam, as already well known, but that a series of chemical and electrical processes, in which isolated atoms are formed in the air, or in other gases, and have access to the jet of steam, also favour an increased production of fog. It appeared therefore probable from this that the Röntgen rays would increase the condensation in a jet of steam.

It was easy to show that the rays issuing from what is called a "focus-tube," as described by Hr. Walter König[‡] and Prof. Röntgen, act on the jet. This experiment is not, however, quite conclusive, for, according to our previous observations, the sudden variations of the electrical force in the discharge through the tube might directly affect the jet of steam. To exclude this disturbing influence it would be necessary to be in a closed metal box together with the steam-jet as described by Prof. Röntgen in his second communication.

I have been satisfied with using a paper screen 160 cm. long and 105 cm. high, lined on both sides with thick lead foil. The discharge-tube was just in front of the middle of one side of this screen, which had a small window closed by aluminium foil for allowing the Röntgen rays to pass. On the other side of this window was the place where the jet issued into the air, the place which acts most sensitively. Sparking from the conducting wire to the screen was prevented by an interposed glass disk. The metal coating and the aluminium foil of the screen, which are put to earth, do not allow variations of the electrical force to pass, while Röntgen rays pass the aluminium window with but little enfeeblement, as shown by the fluorescence of a barium platinocyanide screen. If the tube shows strong radiation the phenomenon of the steam-jet can be seen with certainty though not very distinctly, especially if the hammer of the induction-coil is alternately held fixed, and then its play allowed again.

This is again a fresh case in which on the one hand the occurrence of isolated atoms—of ions—in the air is probable, and on the other the occurrence of the phenomenon of the steam-jet is demonstrated. Since the last publication by R. v. Helmholtz and myself, J. J. Thomson has given a perfectly satisfactory explanation of the manner in which ions promote the formation of fog[§]. This depends on the fact that the formation of drops of water in the strong electrical field of the charged atoms is accompanied by a decrease of the potential energy of the field of the charged atoms, and that this decrease exceeds the increase of potential energy which is always connected with the formation of the smallest drops on account of the surface tension.—Wiedemann's *Annalen*, no. 11, 1896.

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[*] Wiedemann's *Annalen*, vol. xxxii. p. 1 (1887).

[†] *Ibid.* vol. xl. p. 161 (1890).

[‡] *Verh. der physik. Gesellschaft zu Berlin*, xv. p. 74 1896).

[§] J. J. Thomson, Phil. Mag. (5) vol. xxxvi. p. 313.

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

[FIFTH SERIES.]

FEBRUARY 1897.

XIII. *The Spectra of Argon.*

By JOHN TROWBRIDGE *and* THEODORE WILLIAM RICHARDS*.

IT is well known that argon possesses at least two marked spectra—one, termed the red, which is chiefly characterized by red lines; and another called the blue, which, as its name signifies, is strongly marked by blue lines.

In studying these spectra by means of a high-tension accumulator, we have been led to observe carefully the electrical conditions for producing them. It is obvious that a battery of a large number of cells is the most suitable source for the study of the discharges of electricity through gases. Especially is this true of a storage battery; for the readiness with which it can be charged by a dynamo, the constancy of the electromotive force (about 2·1 volts per cell), the ease with which it can be coupled for quantity or tension, and the steadiness of the discharge afforded by it make such a battery far superior to an induction-coil or to an electrical machine.

With an induction-coil the discharge is not unidirectional, and is affected by the necessary irregularities of the break. These irregularities make themselves felt in a marked degree when a condenser is used in the secondary circuit. The electrical machine gives an intermittent current, and has a varying capacity. The advantages of a battery for the study of the discharge of electricity through gases have been pointed out by De La Rue and Müller and by Hittorf†. These investi-

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* Communicated by the Authors.

† *Ann. der Phys. und Chem.* (N. F.) vol. vii. 1879, p. 553.

gators worked with voltaic cells which were not constant and which required great watchfulness and continual renewals. In our investigations we are using a lead accumulator of the Planté type, and find it highly advantageous for spectroscopic work; for by means of the steady current afforded by it one can study the spectra of gases under especially good conditions.

Our battery consists of five thousand cells, so arranged that they may be disconnected and wholly reconnected in any desired manner in less than a minute. The electromotive force of the complete series is somewhat over ten thousand volts, but when the cells are connected for quantity they may be readily charged by means of a dynamo giving a tension of only sixty volts. The insulation of the terminals of this battery was a matter of some difficulty, for even dry wood allows considerable leakage from one case of cells to another; but by the plentiful use of paraffin, mica, and vulcanite the problem was solved with reasonable success. The discharge from only a very small fraction of the battery produced a most uncomfortable shock, and it is probable that the discharge of the whole battery would be instantly fatal. The great heat of this full discharge immediately shatters a Geissler tube, the glass being splintered throughout the whole length of the capillary. Hence a resistance of several million ohms was usually interposed between the battery and the rest of the apparatus. This resistance was also of service in protecting the experimenters from serious accidental shocks. Ordinary distilled water contained in long tubes with movable electrodes was the most convenient resistance for our purpose; dilute solutions of cadmic iodide in amyl alcohol and of cadmic sulphate in water between cadmium electrodes were also sometimes used. Unless these liquids are contained in tubes of rather large diameter they are likely to cause irregularities by boiling under the influence of the heat of the current. Graphite resistances are too combustible to answer the purpose.

The argon used in our experiments was very kindly given to one of us by Lord Rayleigh. It was a portion of the purest preparation which had been used in his final determinations of the density of the gas; and our tubes were very carefully filled with it by the kindness of F. O. R. Götze, of Leipzig. The preliminary work described in this paper was chiefly done with a single tube containing gas at a pressure of about one millimetre. The tube had a wide capillary and was about 15 centim. in total length. In such a tube the red glow of argon is readily obtained with a voltage of about two thousand, but not with much less. A higher tension of gas demands a higher tension of electricity in order to start the discharge, no matter how much or how little other resistance is interposed ; but when the glow has once started it is continued by means of a much smaller electromotive force. This is shown by the fact that a Thomson electrostatic voltmeter connected with the terminals of the Geissler tube indicated differences of potential between the ends of the tube of amounts varying from six hundred and thirty volts upward. De La Rue and Müller, who found no potential-difference between the ends of Geissler tubes, must have been working with discontinuous discharges.

Crookes's estimate that 27,600 volts is necessary to produce the red spectrum is evidently excessive.

The introduction of a capacity between the terminals of the Geissler tube, for example two plates of metal sixteen hundred square centimetres in area separated by plate glass one centimetre thick, made no difference in the red glow so long as the connexions were good and the condenser quiet*. As soon as a spark-gap was introduced, or the condenser began to emit the humming sound peculiar to it, the beautiful blue glow so characteristic of argon immediately appeared.

If this light is examined by a revolving mirror it is seen to consist of intermittent discharges. The battery charges the condenser to the potential necessary to produce a spark between the terminals of the spark-gap. The discharge of this accumulated electricity is produced in the tube and then the operation is repeated. The time-interval between the discharges is evidently a function of the capacity of the condenser, as well as of the electromotive force of the battery and the resistance between it and the condenser.

The accurate determinations of the potential and current-strength of the intermittent blue discharge is a matter of some difficulty ; and at present we feel hardly in position to make a definite statement regarding these measurements. However, the potential required certainly cannot be greater than two thousand volts—the electromotive force of the battery which will easily produce the blue glow. Here, again, Crookes's estimate of far above 27,000 volts was very much too large.

Since it is necessary to employ a condenser to produce the blue spectrum of argon, we were led to examine the electrical conditions which are necessary for the discharge. In the circuit with the tube containing argon, between the tube and one of the plates of the condenser, we first interposed a small coil of about eight ohms resistance, having a self-induction of

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* Sir W. Thomson (Lord Kelvin), 'Papers on Electrostatics and Magnetism' (Macmillan, 1872, p. 236).

·015 of a henry. The blue glow changed to the red glow. We then modified the self-induction, and discovered that even the self-induction of the leads to the tube, which consisted of a few feet of uncoiled wire, undoubtedly modified the blue discharge, for an amount of induction equivalent to + ·000051 henry had a marked effect in diminishing the brilliancy of the blue glow.

A comparatively small ohmic resistance substituted for the impedance of the self-induction between the tube and one plate of the condenser produced precisely the same effect as this coil, causing a complete transformation from blue to red. The change from blue to red is so marked that a tube of argon may well serve as an inductometer of some sensitiveness, as well as a means of comparing the influence of self-induction with ohmic resistance. The effect of impedance or resistance must be to prolong or damp the oscillations. Indeed the resistance of the tube itself may be so great as to damp the oscillations without the need of the introduction of outside resistance or self-induction; therefore argon at high tension gives the red glow with a condenser and rate of oscillation which are quite capable of producing the blue glow in a tube of lower tension.

Kayser* criticises Crookes's statement that a condenser and a spark-gap are necessary for the production of the blue spectrum. He finds that with a lower pressure in the tube than 2 millim. the other spectrum can be readily obtained without condensers and spark-gaps. He also states that it is much easier to produce the pure blue spectrum than the pure red. In order to obtain the red spectrum the strength of the current must be adapted to the gas pressure. Kayser employed an induction-coil. The condenser, however, in the primary of an induction-coil sends an oscillatory discharge through the secondary. Although Kayser did not employ a condenser in the manner recommended by Crookes, he still had a condenser in his electrical system, and the resistance of his Geissler tube was probably so proportioned that the secondary circuit was in resonance with the primary circuit. To prove this we placed a tube containing argon across the terminals of the secondary of an induction-coil, and having removed the condenser attached to the primary we sent a discharge through the tube by means of the break in the induction-coil. The light of the discharge was red, and when it was examined by a revolving mirror no trace of blue was seen in the capillary portion of the tube. An adjustable

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* *Sitzungsberichte der königlich preussischen Akad. der Wissenschaften zu Berlin,* May 7, 1896.

condenser and a variable induction were then placed in the primary circuit. By varying the amount of the capacity together with the self-induction in the primary system, the discharge in the secondary, when examined by a revolving mirror, was seen to consist of both red and blue discharges. The red glow was evidently caused by a unidirectional discharge, and the blue by an oscillatory discharge. The unidirectional discharge was due to the failure of the breaks to charge the condenser of the primary, or to increased resistance in the tube. When, however, the condenser was charged it immediately discharged in an oscillatory manner; and the secondary coil responded by resonance. The rarified argon thus shows in an interesting manner what is the function of the condenser in the primary of an induction-coil. It serves to send oscillatory discharges through the primary circuit; and the greatest effect is obtained in the secondary circuit when it is in resonance, or in tune so to speak, with the primary.

The presence of a condenser was necessary to form the blue glow in Kayser's work, only when the resistance of his tube and the self-induction of his coil together were enough to damp the discharge of the small capacity of the coil. He could have obtained the pure red in any case by interposing, as we have done, a resistance or self-induction between the condenser and the tube, although our other resource for obtaining the red glow in any tube from the continuous discharge of a constant battery was apparently not open to him.

By taking out all resistance except the spectrum-tube, and sending an exceedingly strong current through the tube for very brief intervals of time, we have been able to cause the blue glow; but it seems probable that under these conditions the capacity of the battery itself engenders oscillations which are no longer damped by interposed resistance. Whether the blue glow with its accompanying change of spectrum is due merely to the great quantity of electricity discharged in a very short space of time, or to some property intrinsic in the to-and-fro motion of the oscillatory discharge, is a question which we hope soon to answer. The red glow, if caused by oscillations at all, must be caused by oscillations within the Geissler tube itself*; for all outside oscillations are cut off by the large resistance between the battery and the tube.

The effect of the oscillatory discharge in producing the blue spectrum of argon can also be shown by the use of an electrical machine. If the terminals of the tube containing argon

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* *Ann. der Phys. und Chem.* 1893, vol. xlviii. p. 549. Ebert and E. Wiedemann.

are connected with the terminals of an electrical machine the pure red spectrum is obtained. If a spark-gap is interposed in such a manner that a condenser charged by the machine can discharge through the tube, the blue discharge immediately results. The condenser discharge oscillates through the gas.

The oscillatory discharge of the condenser is evidently an important factor in producing the blue spectrum of argon. According to Lord Kelvin's law, if R denotes the resistance of the circuit, L the self-induction, and C the capacity of the circuit, the discharge of the condenser becomes non-oscillatory when $R > \sqrt{\frac{4L}{C}}$. It may be, therefore, that an estimate of the resistance in the tube can be obtained by measuring the self-induction which is required to change from the blue discharge to the red.

When the tube containing argon at a suitable pressure is brought near a Hertz oscillator giving a rate of about 115,000,000 oscillations per second, it immediately shows the blue colour. In this case the oscillator consisted of two zinc plates about 40 centim. square with a spark-gap between them. The capacity and impedance of the circuit were extremely small.

The extreme sensitiveness of an argon tube to oscillatory discharges leads us to believe that it will be of great use in the study of wave-motions of electricity. As we have seen, it is competent to show when the Hertz oscillator is working properly, that is, sending forth electrical oscillations and not unidirectional discharges. The change of colour in the tube from red to blue is so marked that an argon tube reveals what is not shown in a conspicuous manner by other gases. We have thought this remarkable property of an argon tube worthy of being distinguished by a name which might describe it, and we have therefore called an argon tube fitted for the study of electrical waves a Talantoscope (ταλάντωσις).

In an oscillatory discharge the molecules receive powerful electrical impulses of opposite sign. These impulses are separated, it may be, by one millionth of a second. It is significant that the shorter wave-lengths of light accompany these electrical oscillations. It is our purpose to extend our study of the effect of electrical oscillations through more highly rarefied media in which arise the Röntgen rays. These rays are probably highly modified by the oscillatory discharge. A battery of a large number of cells now at our command will afford the best means of studying this subject ;

for its discharges, as we have pointed out, are free from the fluctuating effects produced by induction-coils, transformers, and electrical machines. Our present paper is therefore only preliminary to a more exhaustive study of the discharges of electricity through rarefied gases by means of a storage battery of ten thousand cells, which will give an electromotive force of about twenty thousand volts.

Harvard University, Cambridge, Mass., U.S.A.,
December 1st, 1896.

XIV. *Two New Pressure-Gauges for the Highest Vacua.*

By WILLIAM SUTHERLAND *.

HITHERTO the M'Leod gauge has been accepted as the most accurate instrument for the measurement of pressures in vacua, although there has been a good deal of misgiving as to the limit up to which its indications are fairly reliable, with an impression that near a millionth of an atmo its measurements become illusory. Baly and Ramsay, in their experiments on rare gases (Phil. Mag. [5] xxxviii.), after working with two M'Leod gauges of very high sensitiveness, pronounced the type of gauge worthless for air and CO_2 though reliable for hydrogen; but the failure of their gauges with air was due to a most interesting and very special phenomenon, to be considered in my next paper, liable to occur only under special circumstances and not affecting the principle of the gauge or its application to air with avoidance of the perturbing conditions. In the case of CO_2 the M'Leod gauge is less reliable than with other gases on account of some action between glass and CO_2, especially if moisture be present; but this does not necessitate that the gauge should be worthless for CO_2, but that there should be a limit to its application, this limit probably depending on the nature of the glass and its treatment. Towards the end of this paper it will be shown that Crookes's M'Leod gauge begins to fail decidedly at $5/10^6$ atmo, with increasing unreliability at lower pressures, the cause of which will be proved to be probably a small residue of water-vapour dissolved, so to speak, in the glass of the gauge. Thus, if the theoretical reasoning of "Boyle's Law at very Low Pressures" † is sound, the principle of the M'Leod gauge ought to be applicable up to any degree of rarefaction, if we can only make it of such a material or so manipulate it as to free it from this trouble with moisture.

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* Communicated by the Author.

† *Suprà*, p. 11.

But it is very desirable to have an independent method and instrument for controlling the indications of the M'Leod gauge, reaching to higher vacua and simpler in its practical working; and one outcome of the paper on "Thermal Transpiration and Radiometer Motion"* is the furnishing of two instruments capable of independently measuring the pressure in high vacua. These two instruments were in a manner united into a single one in Crookes's torsion radiometer briefly described but rather fully discussed in that paper; and though for some purposes there might be advantage in uniting the two forms, each would be capable of its best performance when separate. The two instruments are a disk viscosity-meter and a torsional radiometer of special design to be discussed in this paper. Crookes used his torsion radiometer as a viscosity-meter by forcing it to vibrate and determining the decrement of the logarithm of the angular amplitude of successive vibrations. At pressures near one atmo the vertical plate of the radiometer in oscillating round a vertical axis in the bulb sweeps air bodily in front of it and draws air behind it in a very complex manner, so that its motion is conditioned partly by the mass motion of the gas, as well as by viscosity of gas and viscosity of suspending fibre; but as Crookes's experiments showed, in a manner brought out by Stokes's theoretical discussion (Phil. Trans. clxxii.), the effect of the mass motion of the gas on the log. dec. disappears rapidly, and the log. dec. is practically constant for a considerable range of pressure, proving that viscosity alone is effective in retarding the motion of the vibrating vertical plate. The following values of 10^4 times the log. dec. in air are taken from Crookes's results (Phil. Trans. clxxii.):—

p (mm.) ...	760	550	301	155	47	12	4	3	1
	1124	1073	1022	1006	1001	1000	1000	994	988

These show that from 47 mm. to 4 mm., the latter being only 1/12 of the former, the log. dec. is constant; so that at this range of pressure the mass motion of the gas is negligible, and the viscosity is independent of pressure, as it ought to be according to Maxwell's discovery, so long as slipping of the gas on the solid surfaces of the edge of the plate and the walls of the bulb is negligible; but then at 3 mm. and 1 mm. the log. dec. diminishes decidedly in a manner more fully illustrated by the following further data from Crookes, the unit of pressure being now $1/10^6$ atmo:—

p	1000	495	300	100	53	24	13	8
	988	966	952	876	774	620	500	390

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* Phil. Mag. for November and December 1896.

Now, as was first shown by Kundt and Warburg, and as has been shown in "Thermal Transpiration and Radiometer Motion," the effect of slipping of gas on the influence of viscosity is to replace the viscosity η by $\eta/(1+2\zeta/D)$, where ζ is the coefficient of slipping and equal to $a\lambda_s$, where a is a fraction which is probably nearly the same for most gases, and λ_s is the mean free path of a molecule of the gas near the solid surface, D being distance between moving and resting solid surfaces; for a given gas, λ_s is nearly proportional to λ, the mean free path of a molecule far from the surface, and as λ is inversely proportional to the density, we have the result that ζ varies inversely as the density of the gas. Thus a measurement of slipping furnishes a means of measuring density, and therefore, if the law connecting density and pressure is known, of measuring pressure.

If Boyle's law holds, then $\lambda=\lambda_0 p_0/p$, where λ_0 is the mean free path at some standard pressure p_0; thus η is replaced by $\eta/(1+2a\lambda_0 p_0/Dp)$. When p is large enough, this is indistinguishable from η, as we saw to be the case in Crookes's experiments with air from 47 mm. to 4 mm., through which range the log. dec. retained a constant value which we may denote by L. Then, if l is the log. dec. at lower pressures, L/l gives $1+2a\lambda_0 p_0/Dp$, whence we can get values of $2a\lambda_0 p_0/D$ which ought to be all the same. But there is first one little correction to make, namely that for the viscosity of the torsion fibre, because a small constant portion of the log. dec. is due to its small viscosity; it is the value of the log. dec. for the apparatus if an absolute vacuum were attained in it. Call this part of the log. dec. μ, then

$$\left(\frac{L-\mu}{l-\mu}-1\right)p = 2a\lambda_0 p_0/D. \quad . \quad . \quad . \quad (34)$$

At the higher values of p we can neglect μ and obtain at once a mean value of the constant $2a\lambda_0 p_0/D$, and then with this as a known quantity solve for μ at the lower pressures. In this way, from the data already given for air, $2a\lambda_0 p_0/D$ appears to be 15 and μ to be ·004. With these values and that for L, namely ·1000, the last equation becomes one for obtaining p by a measurement of l; and to show how it works, we give the values of p obtained by it from Crookes's values of l already given:—

p by gauge	1000	495	300	100	53	24	13	8
p from log. dec.......	1153	406	283	101	49	23	13·8	8·6

Now it is obvious that the new method of finding the pressure in a vacuum becomes less delicate when l becomes nearly equal to L, for then the value of p is liable to be greatly affected by only a small error in l. Thus, for example, a diminution of l in the first case from ·0988 to ·0986 would reduce the calculated pressure to 1000 ; thus the first two or three values of the calculated pressures belong to a range where this particular torsion radiometer is not sensitive as a pressure-gauge. Of course we are at present assuming that the pressures as given by Crookes's M'Leod gauge are correct ; it must be remembered that what the indications of the M'Leod gauge really amount to is a sort of a measure of the density of the gas, with an inference as to its pressure by means of Boyle's law, and I have already pointed out that the measurement deduced from slipping is strictly only a measurement of density with a pressure inferred by means of Boyle's law ; the fact, therefore, that the pressures as found by the log. dec. and by the M'Leod gauge in the last table are on the whole so consistent furnishes no proof of the validity of Boyle's law at low pressures, but this, however, has been fairly established on other grounds in "Boyle's Law at very Low Pressures," and therefore the consistency of the two sets of pressures is a proof of the relative correctness of the pressures obtained by Crookes with his M'Leod gauge.

The great advantage about the new viscosity-meter gauge is that its sensitiveness tends to increase with diminution of the pressure which it has to measure. Let us therefore proceed to compare the pressures of air as found by Crookes's M'Leod gauge at still lower pressures, and as calculated by (34) from his measurements of the log. dec.

p by gauge	7·2	5·9	4·1	3·4	2·6	1·9	1·3	1·0
p from log. dec. ...	7·9	6·7	5·0	4·4	3·6	3·0	2·5	2·2
10^4 log. dec.	372	337	281	256	225	198	175	161

p by gauge	·55	·46	·22	·14	·06	·02
p from log. dec. ...	1·82	1·65	1·33	1·25	·95	·53
10^4 log. dec..........	144	135	118	114	97	72

Here we have increasing divergence between the two values of the pressure, till at a pressure estimated as $\cdot 02/10^6$ atmo by the M'Leod gauge and Boyle's law the log. dec. in the viscosity-meter gives a pressure 26 times as great. Now it is rather important that we should have some test as to which of the above series of pressures is the more reliable, because although from what we know of the tenacious cling of H_2O to glass there is reason to expect the M'Leod gauge to fail at some point, still we cannot without further support attribute the discrepancy in the last comparison entirely to the inadequacy of the indications of the gauge at these very low pressures.

In Crookes's measurements of the apparent repulsive force of a candle on his torsion radiometer we have another means of determining the pressure independently, and the torsion radiometer thus used becomes our second form of pressure-gauge. In "Thermal Transpiration and Radiometer Motion" it was shown that the deflecting force exercised by a source of constant radiation on the vertical plate of the torsion radiometer is connected with the pressure of the gas by the formula

$$\text{deflecting force} = c'/(\mathrm{A}''p+\mathrm{B}'''+1/p). \quad . \quad . \quad . \quad (26)$$

But we must remember that this is derived from the equation (12),

$$\frac{dp}{dx}\cdot\frac{3\mathrm{R}^2}{4\eta}(1+4\zeta/\mathrm{R}) = -v\lambda\left(\frac{1}{n}\frac{dp}{dx}+\frac{1}{v}\frac{dv}{dx}\right),$$

which, when n is large enough, reduces to

$$\frac{dp}{dx}\frac{3\mathrm{R}^2}{4\eta} = -\lambda\frac{dv}{dx},$$

whence the deflecting force becomes proportional to λ—that is, inversely proportional to the density. Hence, at a large enough value of n, the deflecting force gives a measure only of the density, just as the M'Leod gauge and the viscosity-meter gauge do; and it is only by using Boyle's law that we get the form of equation (26) given above, which, when p is large enough, becomes

$$\text{deflecting force} = c'/\mathrm{A}''p.$$

Similarly it can be shown that when p becomes small enough the equation really only gives a connexion between the mean density and the difference of density at the front and back of the plate, which passes into a connexion between deflecting force and pressure only on the assumption of a law connecting density and pressure. When this is Boyle's law, the complete equation is (26) above. For Crookes's instrument with air $c'=3{\cdot}0$, $\mathrm{A}''={\cdot}001$, and $\mathrm{B}'''={\cdot}01$ when the unit of pressure is $1/10^6$ atmo and the unit of deflecting force arbitrary. Accordingly, with the values of the deflecting force at low pressure, we can calculate the pressures; thus in the following table we give the pressure as given by the M'Leod gauge with Boyle's law, by the deflecting force with equation (26), and by the log. dec. as given in the last table, and we add a row for the ratio of pressure from log. dec. to pressure from deflecting force.

Def. for............	30·9	22·3	20·2	17	13·1	11·5	8·5
p by gauge.........	13	8	7·2	5·9	4·1	3·4	2·6
p from def. for....	13·5	8·6	7·6	6·2	4·7	4·0	2·9
p from log. dec....	13·8	8·6	7·9	6·7	5·0	4·4	3·6
	1·0	1·0	1·0	1·1	1·1	1·1	1·2

Def. for.	7·1	4·2	2·1	2·0	1 7	1·4	1·0	·7	·5
p by gauge	1·9	1·3	1·0	·55	·46	·22	·14	·06	·02
p from def. for. ...	2·4	1·4	·70	·67	·57	·47	·33	·23	·17
p from log. dec. ...	3·0	2·5	2·2	1·8	1·65	1·33	1·25	·95	·53
	1·2	1·8	3·1	2·7	2·9	2·8	3·8	4·1	3 1

At first sight these numbers seem to show that the pressures given by the deflecting force are in much better harmony with those given by the gauge than are those given by log. dec., but we shall show that this is due to a defect in Crookes's torsion radiometer when made to do duty as a pressure-gauge, a defect which could be avoided in a suitable design; for a study of the row of ratios shows that while the value increases slowly from 1·0 to 1·2 there is a sudden transition to a value about 3·0, and the theoretical reason for expecting such a change of ratio was pointed out in "Thermal Transpiration and Radiometer Motion," as it was stated that in Crookes's instrument the formula would represent the relation between deflecting force and pressure as long as the vertical plate was able to control the fall of temperature in the gas near its edge, but that it would cease to do this as soon as the mean path of the molecules became nearly equal to the radius of the bulb, for then the great surface of the bulb compared to the thickness of the plate would enable it to swamp the influence of the plate, even in the plate's own neighbourhood; thus there should be a change from the limiting relation def. force $=c'p$ for the plate, to def. force $=c''p$ for the bulb. Now the above change of ratio is established at a pressure about $2·5/10^6$ atmo as given by log. dec., when the mean free path of a molecule of air ought to be about $10^6 \times ·00001 \div 2·5$ centim. or 4 centim., which is about the radius of the bulb. The approximate constancy of the new ratio at about 3 should increase our confidence in the pressures derived from the log. dec. as continuously fairly reliable down to the lowest pressure of half-a-millionth of an atmo, whereas the M'Leod gauge gives pressures only half as large as they ought to be at about $2/10^6$ atmo and about 1/26 as large as they ought to be at $·5/10^6$ atmo.

Thus Crookes's data for air show the principles of both the new gauges to be sound, but to get the best results from each it ought to be developed on its own lines. To avoid prolixity we will call the viscosity-meter when used as a pressure-gauge the Viscometer Gauge; its design on the large scale has been worked out by Maxwell (Phil. Trans. 1866), who also furnished the theory of it, and on a smaller scale by Kundt and Warburg (Pogg. *Ann.* clvi.). A still smaller scale would, in most cases, be advisable for the viscometer gauge, as its volume ought to be kept small. In Maxwell's apparatus three glass plates 28 centim. in diameter and 2 millim. thick were fixed at equal distances along a solid axis, the continuation of which was a fine vertical torsion-wire of steel; fixed disks were mounted so that each of the movable disks was half-way between two fixed ones at distances varying from 5 millim. to 17; the whole was enclosed in a glass vessel with a vertical continuation for the 125 centim. of torsion-wire. The inconvenience about this form is that special arrangements have to be made for determining the viscosity of the rather strong wire that is required to support so heavy a system. Kundt and Warburg simplified the apparatus by reducing the weight and adopting a bifilar suspension whose viscosity effect was practically negligible. They used a single movable plate of glass of 16 centim. diameter and thickness about 1 millim. vibrating between two other fixed ones, the distance between fixed and moving surfaces varying from 1·1 millim. to 2·8. It is to be remembered that both these instruments were designed to obtain accurate absolute values of the coefficients of viscosity of gases, and for that purpose Maxwell worked out a pretty complete theoretical expression connecting decrement of the logarithm of consecutive angular amplitudes of the vibrating system with the viscosity of the gas between the plates, the viscosity of the wire, and the constants of the apparatus.

Now we have just seen that Crookes succeeded in getting remarkably good readings of log. dec. with his apparatus, where the surface at which the viscosity produces its effect is only the minute thickness of a small straight edge of a mica plate moving not particularly close to a spherical surface, that is to say, under very unfavourable circumstances; it is therefore evident that a thin glass, mica, or untarnishable metal disk about 5 centim. in diameter and as light as possible, vibrating between two fixed plates with a distance not greater than about 1 millim. between fixed and moving surfaces, the suspension being by means of a quartz thread, would give values of log. dec. of far higher accuracy than

Crookes's, and would therefore enable our formula to give the coefficient of slipping ζ with great accuracy, and conversely the pressure.

The simplest form which the apparatus could take would be that in which the two fixed plates form the top and bottom of a shallow cylindrical drum strong enough to stand a pressure of an atmo on its outer wall without appreciable deformation; within the drum the movable disk is suspended equidistant from top and bottom by a quartz fibre passing down the axis of a fine vertical tube rising from the centre of the top of the drum; a small tube in the centre of the bottom would furnish connexion with the gas supply whose pressure is to be measured, a side tube from the upper vertical tube facilitating the operation of filling the drum with the pure gas. A small mirror attached to the quartz fibre at a suitable point near its entrance to the drum would give the means of measuring the angular amplitude of successive vibrations of the disk, which could be set in motion by means of a small magnet attached to the back of the mirror.

The whole apparatus ought to be made so that it could be raised to as high a temperature as possible to secure thorough drying and exhaustion. The special advantage of this form of viscometer gauge is that its volume is small, only the unoccupied space of the drum, say $2\pi \times 2{\cdot}5^2 \times {\cdot}1$ cub. centim., together with the internal volume of the tube, which is, say, 50 centim. long and 1 millim. radius, namely $\pi \times {\cdot}1^2 \times 50$ cub. centim., that is a total volume of about 6 cub. centim.

A more accurately constructable form of the viscometer gauge would be one in which the two fixed disks and the movable one with its suspension fibre passing through a hole in the upper are all enclosed as in Maxwell's and in Kundt and Warburg's apparatus in a suitable vessel for connexion with the gas supply whose pressure is to be measured. Now in these forms of viscometer gauge the expression given by Maxwell for the relation between log. dec. and viscosity simplifies at, say, 1/20 atmo to one which makes the log. dec. minus the unknown log. dec. of an absolute vacuum due to the viscosity of the fibre, proportional to the viscosity of the gas. The part of the log. dec. due to the fibre could be found by loading the fibre with a sphere of lead equal in weight to the disk of the viscometer gauge and observing the log. dec. in air at any ordinary pressure, the small effect of the viscosity of the air being allowed for by calculation, or the sphere might be torsionally vibrated in a bulb exhausted till the highest obtainable vacuum was reached, when the log. dec. ought to become constant or tend to a constant limit which could be definitely inferred, and would give the part to be subtracted from all readings of the log. dec. in the viscometer gauge as due to the fibre. Or μ in our formula (34) could be found directly from the readings of the values of the log. dec. as we found it with Crookes's observations, that is to say, by making observations of the log. dec. at different pressures measured by a reliable form of M'Leod gauge. One measurement of L and two measurements of l at suitable pressures would suffice for the determination of μ and $2a\lambda_0 p_0/\mathrm{D}$, but mean values of these obtained from a number of observations would of course be preferable. The gauge is then ready for the measurement of any pressure in the particular gas under observation; for although μ is the same whatever the gas may be, $2a\lambda_0 p_0/\mathrm{D}$ varies from one gas to another; but when once μ has been obtained it is only necessary to take a reading of L and one reading of l at any measured suitable pressure to give $2a\lambda_0 p_0/\mathrm{D}$ for the gas under study, and then all other pressures are obtainable by measurements of the log. dec. and a simple calculation.

One advantage of the viscometer gauge is that when once its constants have been determined for any particular gas, the only operation necessary for obtaining any pressure is that of observing the amplitude of the mth, nth, and pth vibrations, an operation so simple that it need only delay the experimenter a minute or two, and by a short calculation he can satisfy himself in a moment as to the magnitude of the pressure in his gas supply.

The other form of pressure-gauge, the torsion radiometer, or, as we may call it, the Transpiration Gauge, has already had its essential features indicated in "Thermal Transpiration and Radiometer Motion" as a piston enclosed in a cylinder with a uniform distance between the curved surfaces of both, the ends of the cylinder to be of glass or other transparent material to facilitate irradiation of one end of the piston by means of a constant source of heat such as a platinum spiral carrying a constant electric current or a steam bath; or the ends might be metal with a steam bath at one end and ice bath at the other. It has been shown in "Thermal Transpiration and Radiometer Motion" that when the temperatures of the two ends of the piston become constant there is a pressure driving the piston from hot to cold with a total force $c'/(\mathrm{A}''p+\mathrm{B}'''+1/p)$, where the coefficients are all constant for a given apparatus and a given gas, but varying with the gas. It is necessary to attach to the piston some means of enabling it to retain its position against this force, and of measuring the amount of the force; this may be done by placing the axis of cylinder and piston horizontal and in one of the lines where the horizontal plane through the axis cuts the cylinder, piercing it with a small hole at its middle point; this hole establishes communication with a small chamber, and through it passes an arm for supporting the piston in its right position, the chamber being large enough to hold a counterpoise to the piston attached to the other end of the arm; from the top of the chamber rises the fine vertical tube which is to contain the quartz torsion fibre rigidly attached to the arm and connected at the top of the tube with a torsion head, whereby a sufficient couple can be applied to balance the twisting effect of the pressure on the piston. In order not to interfere with the vacuum in the apparatus the torsion head should consist of a small magnet so pivoted that it can be turned through any angle by a large magnet outside, and so twist the quartz fibre through an angle measured by the usual mirror arrangement. To bring the piston always to the same position a mark on the counterpoise as seen through a window in the little chamber is brought into alignment with a mark on the window and a fixed point of reference such as cross fibres at a convenient position outside. The angle of torsion can be written equal to $c'/(A''p+B'''+1/p)$, and three or more measurements of its value at pressures determined by a M'Leod gauge of assured reliability furnish the values of the constants or rather parameters c', A'', B''' for the instrument and the gas, and then any other pressure can be calculated from an observation of the angle of torsion.

As to the material for the piston, Crookes's experience seems to show that a disk of mica blackened on one side would be eminently suitable; a circular disk, out of which a smaller one was cut, leaving an empty ring between the two, would form both piston and cylinder, and there would only remain to attach the solid ring inside the drum or vessel of suitable shape which is to hold the gas under study; for example, the solid ring could be cut of such a size that it just fits into a shallow cylinder to which it can be cemented, so that when the top and bottom of the cylinder are closed by thin glass or metal plates, and when the piston is in position, there are two shallow cylindrical chambers communicating through the annular space between piston and cylinder. The junctions would have to be such that the whole could be raised to 200° or 300° C. for thorough drying and evacuation. The front of both the disk and ring of mica being black and the back clear, there would be established a fall of temperature through the annular interspace such as is contemplated in "Thermal Transpiration and Radiometer Motion." For the highest accuracy it would be necessary to take steps to keep the whole drum at a constant mean temperature.

A special advantage of this form of gauge is that after a certain degree of rarefaction is reached $A''p+B'''$ may be neglected, and the deflecting force and angle of torsion become proportional to the pressure, so that the experimenter can ascertain at a glance what pressure he is dealing with.

To show a defect to which both new forms of gauge are subject, and yet how they can support one another and bring to light an interesting phenomenon, we will discuss Crookes's observations on hydrogen just as we have done those on air. In "Thermal Transpiration and Radiometer Motion" it was shown that the deflecting force for hydrogen is not expressed by the ordinary formula until changed to

$$c'/\{A''p/(1-\alpha p)+B'''+1/p\}.$$

Now the term $A''p$ in the ordinary formula originates from a term involving the product of the mean free path near the solid and the mean free path far from the solid: and the mean free path near the solid was shown to be altered by condensation of the gas, but in such a manner that it is always proportional to the free path in the gas far from the solid; thus molecular attraction of the solid for the gas alters slightly the numerical value of A'', but does not introduce any factor $1/(1-\alpha p)$; therefore the introduction of this factor is probably due to some other surface action; if it is really connected with some other alteration of the free path of a molecule of the gas near the solid surface we ought to find the slipping of the gas affected; for theoretically the amount of slipping is proportional to the mean free path near the solid, and at the same time, as the free path far from the surface is unaffected, the viscosity of the gas should be practically unaltered by the surface action. Accordingly some interest attaches to the study of slipping in Crookes's experiments with hydrogen. The constant upper limit for the log. dec. in H_2 is ·0499, and in the case of air we took μ, the part of the log. dec. due to viscosity of fibre, as ·0040; thus in (34) we have L and μ, the values of l being given in the next table at the pressures given as found by the M'Leod gauge and Boyle's law in terms of $1/10^6$ atmo as unit; in the third row will be found $(L-\mu)/(l-\mu)-1$, and in the fourth

$$\{(L-\mu)/(l-\mu)-1\}p,$$

which, under ordinary circumstances, ought to have the constant value $2a\lambda_0p_0/\mathrm{D}$.

p by gauge...	330	234	155	110	59	37	26·5	20	14·5	8
10^4 log. dec..	495	488	484	472	441	408	373	351	324	270
	·009	·024	·034	·063	·145	·247	·378	·476	·616	·995
	3·0	5·6	5·3	6·9	8·6	9·0	10·0	9·5	8·9	8·0
	12·9	12·6	9·9	10·2	10·3	10·2	10·8	10·1	9·3	8·2

The numbers in the fourth row instead of being constant rise to a maximum and decline; but if to the numbers in the third row, which represent $2\zeta/\mathrm{D}$, we add ·03 and then multiply by p, we get the products given in the last row, which are practically constant down to $20/10^6$ atmo. Thus, then, the free path near the solid λ_s, which is proportional to the coefficient of slipping, instead of being proportional to λ is proportional to $\lambda - h$, where h is a constant.

Now in the formula for deflecting force $\mathrm{A}''p$ is the form given to a term varying inversely as λ_s, so that this term cannot be written as inversely proportional to λ, and therefore directly proportional to p, but it must be taken as inversely proportional to $\lambda(1-h/\lambda)$, that is to $(1-ph/\lambda_0p_0)/p$, and therefore the term from which $\mathrm{A}''p$ is derived in the general theory gives $\mathrm{A}''p/(1-ph/\lambda_0p_0)$ in the case of hydrogen, which, in "Thermal Transpiration and Radiometer Motion," we actually found to be the case, if we identify h/λ_0p_0 with the constant α of that paper. And there we also found the value of A'' to take its theoretical place along with those for other gases, which shows that in connexion with the deflecting force λ_s in hydrogen is altered in its relation to λ simply by a factor such as $(1-ph/\lambda_0p_0)$ with a limit 1 and not any other number; but if from the last table we take 10 as the value of $(2\zeta/\mathrm{D}+{\cdot}03)p$, and compare it with the value 15 for $(2\zeta/\mathrm{D})p$ in air, which stands for $2a\lambda_0p_0/\mathrm{D}$, and remember that at any standard pressure p_0 the values of λ_0 for hydrogen and air are as those of $\eta/m^{\frac{1}{2}}$, that is nearly as 2 to 1, we find that $(2\zeta/\mathrm{D}+{\cdot}03)p$ for hydrogen would have to become 30 to give this theoretical relation of 2 to 1 if the alteration of ·03 were all that is required; this then indicates that with hydrogen, in addition to a factor $(1-ph/\lambda_0p_0)$ in the relation of λ_s to λ, there is a factor 1/3 whose occurrence here, and not in deflecting force, has to be explained.

The explanation probably lies in the presence of a trace of water-vapour, for notwithstanding the very thorough measures which Crookes took to get rid of the water-vapour after it had given him considerable trouble, I am still inclined to think that a trace had been left in the hydrogen which both the Viscometer Gauge and the Transpiration Gauge have succeeded in detecting. The first question that naturally arises is, why should we not have similar evidence of the presence of water-vapour in the case of other gases? the answer to which is that, just as Graham showed experimentally in his study of the viscosity of mixed gases that the viscosity of hydrogen is specially sensitive to the presence of other gases, a partial theory of the phenomenon being given in "The Viscosity of Mixed Gases" (Phil. Mag. [5] xl.), so it is probable that a small amount of impurity largely affects the slipping of hydrogen while only slightly affecting that of the other gases, the difference between the case of hydrogen and those of the other gases being due to two causes, first, the small attraction of the solid for the hydrogen molecules, whereby they are less condensed at the surface than molecules of other gases, so that a given amount of vapour of water in the gas makes a larger impurity in the surface hydrogen than in the surface layers of other gases, and, secondly, the small mass of the hydrogen molecule. Thus we can give the following general sketch of the effect of the presence of water-vapour in a gas on the surface phenomena which we are treating of at present. Water-vapour appears almost to dissolve in solid glass, forming a union which also retains some free water-vapour at the surface; thus a vibrating solid surface would practically carry such a layer of water-vapour as part of itself, and thus the layer would introduce error into the slipping of the gas, the amount of which for the ordinary gases is small, but becomes important with hydrogen for the reasons already stated. Now if the water-vapour is mostly gathered at the surface it cannot affect the general viscosity of the gas, but only the slipping and the deflecting force. In the case of hydrogen the effect of the water-vapour seems to be to shorten the mean free path by an amount which is nearly constant, and, moreover, in the case of slipping alters the constant a in the relation $\zeta = a\lambda$ to 1/3 of its usual value, so efficient is the layer of massive water molecules in sweeping the light hydrogen molecules with it. The curious result that the shortening of the free path of hydrogen near a solid surface by the water-vapour should be independent or nearly independent of the pressure invites inquiry, but we can hardly delay any longer here on this hydrogen episode.

Resuming the study of the pressure-gauges, we will now calculate the pressures in Crookes's highest hydrogen vacua according to the equations

$$p\left(\frac{\mathrm{L}-\mu}{l-\mu}-1+\cdot 03\right)=2a\lambda_0 p_0/\mathrm{D}, \quad . \quad . \quad . \quad (35)$$

with $\mathrm{L}=\cdot 0499$, $\mu=\cdot 004$, and $2a\lambda_0 p_0/\mathrm{D}=10\cdot 5$,

and deflecting force $=c'/\{\mathrm{A}''p/(1-\alpha p)+\mathrm{B}'''+1/p\}$, . (36)

with $c'=4\cdot 16$, $\mathrm{A}''=\cdot 0006$, $\alpha=\cdot 0016$, $\mathrm{B}'''=\cdot 01$.

10^4 log. dec. ...	324	304	270	253	232	214	191	172	169	157	130	118
def. for.	49	45	37	31	29	26	15	10	9	7	3	2
p by gauge ...	14·5	12	8	6·5	5	4	2·6	1·8	1·5	1·0	·37	·16
p from log. dec.	16·3	13·6	10·2	8·9	7·4	6·3	5·1	4·2	4·1	2·6	2·5	2·1
p from def. for.	15·2	13·3	10·3	8·3	7·7	6·8	3·8	2·5	2·2	1·7	·72	·48
	1·1	1·0	1·0	1·1	1·0	·9	1·3	1·7	1·9	1·5	3·5	4·4

The numbers in the last row are the ratios of the pressures given by Crookes's radiometer when used as a viscometer gauge and as a transpiration gauge, and they show that they agree with one another down to a pressure of about $5/10^6$ atmo as given by the viscometer gauge, although at that pressure they have become 50 per cent. larger than the pressure given by M'Leod's gauge. Then, just as in the case of air, we find the value of the ratio rise suddenly to be nearly 2 instead of 1, and the same explanation holds for the fact here as for air, namely that the mean free path is getting to be about as large as the radius of the bulb. Now as the mean free path in hydrogen is about double that in air, the pressure at which the change in the ratio occurs with hydrogen ought to be about double that with air, and as the change occurs at pressures about $5/10^6$ atmo and $2\cdot 5/10^6$ as measured by the viscometer gauge, the theoretical condition is realized in the experimental results.

According to the viscometer gauge the lowest pressure is 13 times as great as that given by the M'Leod gauge, a result which shows how useful the control of the different gauges by one another will be in measurements of the highest vacua. The change in ratio which we have found both with hydrogen and air seems at first sight to disqualify the transpiration gauge, but it is to be remembered that the magnitude of the change is the result of the unsuitableness of Crookes's torsion radiometer to serve as a type of the ideal transpiration gauge, which, if designed on the lines explained a few pages back, would be liable to only a trifling change of ratio, which could be reduced to zero if steps were taken to bring the whole inner surface of each of the two chambers connected by the annular space to a uniform temperature.

We will now briefly discuss the performances of the two gauges with CO_2, CO, and N_2 by means of Crookes's measurements of log. dec. and deflecting force, for using the latter of which the appropriate values of c', A'', and B''' are given in "Thermal Transpiration and Radiometer Motion;" for using Crookes's values of the log. dec. we have the following:—

	CO_2.	CO.	N_2.
10^4 L	822	968	970
$2a\lambda_0 p_0/D$ ...	15	17	16·6

	CO_2.				CO.		
10^4 log. dec.........	424	347	325	298	474	448	305
def. for.	16	11	10	8	14	13	7
p by gauge	15	10	9	7·6	13	12	6·5
p from log. dec. ...	14·4	9·7	8·6	7·4	14·9	13·4	6·8
p from def. for. ...	14·5	9·3	8·3	6·6	13·1	11·9	5·7

	N_2.					
10^4 log. dec.........	420	351	318	257	207	178
def. for.	17	14	13	9	3	1
p by gauge	13	9·6	8·3	5·8	3·3	2·8
p from log. dec. ...	11·5	8·3	7·1	5·1	3·6	2·9
p from def. for. ...	10·5	8·1	7·3	4·9	1·5	·5

In the case of N_2 the pressure from the log. dec. and that from deflecting force agree down to about $5/10^6$ atmo, although systematically less than those by the M'Leod gauge and Boyle's law; but at the two lowest pressures a great difference appears, which is too large for the same explanation as applied to the change of ratio with air and H_2; in comparison with the deflecting force in air that in N_2 dies away in a very sudden manner—for example, while at $3/10^6$ atmo the deflecting force in air is about 10, in N_2 it is only 1, a very remarkable difference, calling for further experimental inquiry.

Crookes's measurements for oxygen, being of special importance, will be discussed separately in my next paper. The fairly good agreement which we have found between the indications of the M'Leod gauge at low densities and those of the viscometer gauge and the transpiration gauge furnish an indirect proof that there can be no surface condensation which suffices with the gases discussed to produce any appreciable departure from Boyle's law; for if there were, there could not be a constant relation between density as found by the M'Leod gauge and by the other two; but as to departure from Boyle's law due to other causes than surface condensation the agreement tells nothing, as it simply means an agreement in three measurements of density. But it is of some importance to learn that surface condensation produces no appreciable effect, even by way of an apparent departure from Boyle's law in rare gases (see "Boyle's Law at very Low Pressures"); of course it is not implied that there is no surface condensation, for, on the contrary, its nature and amount have been calculated in "Boyle's Law at very Low Pressures," but in ordinary vessels the mass condensed is a small constant fraction (for a given gas) of the total mass in the vessel, and produces no effect on Boyle's law to the degree of accuracy of measurement hitherto attained.

The trace of water-vapour which was the probable cause of the abnormalities discussed in connexion with hydrogen is also the probable cause of the failure of Crookes's M'Leod gauge at pressures near and below $1/10^6$ atmo both with hydrogen and air. As at the lowest pressure in air the M'Leod gauge gave a measurement of the pressure only 1/26 of that of the viscometer gauge, it follows that 25/26 of the gas in the highest vacuum would appear to have been H_2O, which, when the gas is compressed into the volume-tube of the M'Leod gauge, simply recombines with the glass again; if this is so, then the pressures calculated from the log. dec. and from the deflecting force both for air and hydrogen are inaccurate, as they ought to be calculated with the values of the parameters for H_2O instead of those for air and H_2. In fact, if at the highest vacua in both air and H_2 water-vapour is the main constituent, then l, the log. dec., ought to have the same value both for air and H_2 at the same erroneous values of the pressure given by the M'Leod gauge. The following little table furnishes a comparison of the log. decs.:—

apparent p...	1·3	1·0	·37	·16
$10^4 l$ for air...	175	161	126	115
$10^4 l$ for H_2...	164	157	130	118

This shows that our supposition as to water's forming the chief ingredient of Crookes's highest vacua is probably true, and therefore the pressures which we previously obtained from the viscometer and transpiration gauges at the highest vacua are erroneous, being calculated for air and H_2 instead of for H_2O, but obviously the amount of their error cannot attain to that of the indications of the M'Leod gauge. Thus there is strong evidence that in the investigations of highly rarefied gases in glass vessels there is always an unremoved trace of water which exercises a perturbing influence increasing with the rarefaction, and becoming serious in Crookes's experiments at about a millionth of an atmo. In investigating the highest vacua it would seem to be desirable to dispense with glass or to ascertain if glass can be obtained which does not exercise a special "dissolving" action on water-vapour.

Melbourne, August 1896.

XV. *On the Relation of the Physical Properties of Aqueous Solutions to their state of Ionization. By* Prof. J. G. MacGregor, *Dalhousie College, Halifax, N.S.**

[Concluded from p. 55.]

Relative Values of a Property for a Mixture and for its Constituents. "Corresponding" Solutions.

As change of ionization in general occurs on mixing two solutions, it follows from (3) and (4) that the value of a property for a mixture of two solutions having one common ion will differ from the volume-mean, $(v_1 P_1 + v_2 P_2)/(v_1 + v_2)$, of its values for the constituents by the amount

$$(l_1 - k_1) \frac{n_1 v_1}{v_1 + v_2} (\alpha_1' - \alpha_1) + (l_2 - k_2) \frac{n_2 v_2}{v_1 + v_2} (\alpha_2' - \alpha_2). \quad . \quad (5)$$

The name of "corresponding" solutions has been given to solutions for which this quantity vanishes. In general it will obviously have a value, though that value may be small.

In most cases this conclusion is borne out by experience. But Rother has concluded from his observations that, in the case of surface-tension, throughout a wide range of concentration, solutions of all concentrations are "corresponding." Were this the case it would throw serious doubt on the possibility of expressing surface-tension in terms of state of ionization. If, however, with the aid of the constants for surface-tension determined above, we compute, in the case of Sodium and Potassium Chlorides, the difference between the value for a mixture and the volume-mean of the values for its constituents, we find it to be beyond the limit of Rother's power of observation. Thus, in the case of his first mixture calculated above, the difference amounts to only $0{\cdot}0_3 15$. His

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* Communicated by the Author.

conclusion should thus have been that the difference, if any, between the surface-tension of a mixture and the volume-mean of those of its constituents was within the limits of his experimental error. He might even have concluded, however, that there was probably such a difference in the case of Sodium and Potassium Chlorides; for in all the mixtures of solutions of these salts which he examined, the volume-means of the values for the constituent solutions were found to be less than the values for the mixtures.

The above expression (5) will vanish if the constituents of the mixture are isohydric, *i. e.*, have states of ionization which do not change in the mixing; and it will vanish in that case, whatever the values of the other quantities involved in the expression may be. When the constituents are not isohydric the condition of its vanishing will be

$$\frac{n_1}{n_2}=\frac{(l_2-k_2)(\alpha_2'-\alpha_2)v_2}{(l_1-k_1)(\alpha_1-\alpha_1')v_1}. \quad . \quad . \quad . \quad . \quad . \quad (6)$$

It is obviously improbable that in any case in which this condition may be fulfilled the numbers of gramme-equivalents per litre in the constituent solutions will have a simple relation, such as 1 : 2, 4 : 3, &c.

The conclusions drawn by Bender and Brückner from their observations on density, thermal expansion, electrical conductivity, and viscosity, viz. that there is such a simple relation in the case of all "corresponding" solutions, so far as the properties mentioned are concerned, is thus inconsistent with the possibility of expressing the values of these properties in terms of the state of ionization.

Both Bender* and Brückner† obtained their results from numerous series of observations, in each of which a solution of given concentration of one salt was mixed in succession, in equal volumes, with a number of solutions of different concentrations of a second salt having one ion in common with the first. The values of the property under consideration were determined both for the simple solutions and for the mixtures and the arithmetic means of the values for the constituents of the several mixtures were found. Curves were then plotted with molecular concentrations of the simple solutions of the second salt as abscissæ, and the observed values for the mixtures and the arithmetic means of the values for the constituents, respectively, as ordinates. The "corresponding" solutions were indicated by the points of intersection or contact of these curves. In all cases the curves for

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* Wied. *Ann.* xxii. (1884) p. 184, and xxxix. (1890) p. 89.

† *Ibid.* xlii. (1891) p. 293.

each series are found to run very close together, so close that it is impossible to determine exactly at what points they touch or cross; and when the observational errors admitted by the authors are taken into account, they must be considered to be within touching or crossing distance at considerable distances on each side of the points at which Bender and Brückner assumed them to be in contact or to intersect. It is probably needless to give details; but I may say that I have plotted a number of these curves so as to indicate accurately all significant figures, and have found, on taking possible errors of observation into account, that in no case can a more definite conclusion be drawn than that "corresponding" solutions have pretty nearly the simple relations as to concentration claimed by the authors. I have found also that in most cases the limits of the concentration of the second salt within which the curves must be considered to be possibly in contact, include the isohydric concentration. It would thus appear that both Bender and Brückner drew too definite conclusions from their observations, and that the observations themselves are not inconsistent with the applicability of expression (1) to the physical property of solutions.

Applications of the Assumed Law of Ionization-Constants.—Variation of Temperature and other Coefficients with Concentration.

If the expression under consideration is applicable to solutions of moderate dilution it should give by deduction the laws which have been found to hold for particular properties of such solutions, and might be expected to be of use in showing their relation to one another. I need not refer here to the more obvious of such deductions, as, for example, the properties of non-electrolytes, or of electrolytes at extreme dilution, but may restrict myself to cases in which both constants k and l play a part.

The temperature-coefficient for any property of a solution of given concentration will be

$$\frac{1}{\mathrm{P}}\frac{\partial \mathrm{P}}{\partial t}=\frac{\dfrac{\partial \mathrm{P}_w}{\partial t}+\dfrac{\partial k}{\partial t}n+\left(\dfrac{\partial l}{\partial t}-\dfrac{\partial k}{\partial t}\right)\alpha n+(l-k)\dfrac{\partial \alpha}{\partial t}n}{\mathrm{P}_w+kn+(l-k)\alpha n}. \quad . \quad (7)$$

The pressure-coefficient will have the same form, p being written for t. The concentration-coefficient will be

$$\frac{1}{\mathrm{P}}\frac{\partial \mathrm{P}}{\partial n}=\frac{k+(l-k)\left(\alpha+\dfrac{\partial \alpha}{\partial n}n\right)}{\mathrm{P}_w+kn+(l-k)\alpha n}. \quad . \quad . \quad . \quad . \quad (8)$$

In the case of a solution of a given salt of given concentration, temperature, and pressure, α, n, and α's rates of change have definite values the same for all properties. For moderately dilute solutions, $\partial\alpha/\partial t$, $\partial\alpha/\partial p$*, and $\partial\alpha/\partial n$ are all small, and $\partial\alpha/\partial t$ and $\partial\alpha/\partial n$ at least have the same sign. Also the k's and l's for the different properties all depend upon the mutual action between molecules and solvent, and may thus be expected to have more or less closely related values. We may therefore expect not only that the coefficients of one kind for the various properties of solutions of a given salt will vary with concentration in a somewhat similar manner, but also that the variation with concentration of all the coefficients, but especially the temperature and pressure-coefficients, will exhibit a certain family likeness. It is obviously not to be expected that the variation will be exactly similar in any case.

This family likeness has been observed in the case of the temperature-coefficients for electrical conductivity and fluidity by Grotrian †, who found that, in general, with increasing concentration both of these temperature-coefficients undergo changes in the same sense. Grossmann ‡ claimed to have proved these coefficients to be equal; but afterwards withdrew the claim as based on an error §. Kohlrausch and Hallwachs also have noticed for very dilute solutions a close similarity between the curves representing the density and the conductivity respectively of the same salt as functions of the concentration.

The following tables show that this family likeness extends, to a greater or less extent, to all the coefficients for at any rate a considerable number of the properties of solutions. The tables include some of Grotrian's coefficients with others calculated from the observations of Kohlrausch, Bender, Brückner, Rother, Röntgen and Schneider ‖, Fink ¶, and Timberg**. The coefficients are in almost all cases mean values, the ranges of temperature &c. to which they apply, though the smallest for which data are available, being not in

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* I have not seen Röntgen's paper, on which the statement that $\partial\alpha/\partial p$ is small is based. The *Fortschritte der Physik* reports Tammann as quoting him to that effect.

† Wied. *Ann.* viii. (1879) p. 552.

‡ *Ibid.* xviii. (1883) p. 119.

§ See Kohlrausch, Wied. *Ann.* xxvi. (1885) p. 224.

‖ Wied. *Ann.* xxix. (1886) p. 194.

¶ *Ibid.* xxvi. (1885) p. 505.

** *Ibid.* xxx. (1887) p. 545.

Sodium Chloride Solutions.

Temperature-coefficients for						Pressure-coefficients for				Concentration-coefficients for							
Conductivity.		Fluidity.		Density.		Conductivity.		Density.		Conductivity.		Fluidity.		Density.		Surface-tension.	
n.	Coeff.	*n.*	Coeff.	*n.*	Coeff.	*n.*	Coeff.	*n.*	Coeff.	*n.*	Coeff.	*n.*	Coeff.	*n.*	Coeff.	*n.*	Coeff.
0·883	·0218			1·0	$-\cdot0_{3}271$	0·170	$\cdot0_{4}801$	0·712	$\cdot0_{4}429$	0·5	1·67	0·5	−·0895	0·5	·0394	0·895	·0209
1·828	·0215	1·441	·0245	1·5	$-\cdot0_{3}304$	0·882	$\cdot0_{4}600$	1·496	$\cdot0_{4}394$	1·0	0·639	1·5	−·1081	1·5	·0368	1·835	·0198
2·839	·0213	2·694	·0243	2·0	$-\cdot0_{3}333$	1·828	$\cdot0_{4}491$	2·651	$\cdot0_{4}352$	3·0	0·122	3·0	−·1532	3·0	·0314	2·837	·0203
3·918	·0217	4·655	·0252	2·5	$-\cdot0_{3}355$	2·806	$\cdot0_{4}381$	3·941	$\cdot0_{4}313$							3·937	·0243
5·078	·0228			3·0	$-\cdot0_{3}375$	3·835	$\cdot0_{4}083$	5·420	$\cdot0_{4}277$								
						5·059	$-\cdot0_{4}110$										
						5·421	$-\cdot0_{4}142$										

Barium Chloride Solutions.

Temperature-coefficients for						Concentration-coefficients for	
Conductivity.		Fluidity.		Density.		Conductivity.	
n.	Coeff.	*n.*	Coeff.	*n.*	Coeff.	*n.*	Coeff.
0·501	·0215	0·781	·0240	1·0	$-\cdot0_{3}258$	0·5	1·629
1·051	·0207	1·704	·0226	1·5	$-\cdot0_{3}290$	1·0	0·610
1·652	·0201	2·945	·0222	2·0	$-\cdot0_{3}318$		
2·314	·0196			2·5	$-\cdot0_{3}341$		
2·895	·0193			3·0	$-\cdot0_{3}361$		

Calcium Chloride Solutions.

Temperature-coefficients for Surface-tension.	
n.	Coeff.
About 6·4	$-\cdot0_{2}134$
,, 10·0	$-\cdot0_{2}179$

all cases the same. As I wish to show only a general similarity, it is not necessary to specify the ranges. The temperatures &c. of the lower limits of the ranges are also not in general exactly the same. The data of the tables are thus not exactly comparable; but they are sufficiently so for my purpose. The heading n stands for gramme-equivalents per litre.

A glance at these tables shows that, if regard be had to sign, Grotrian's conclusion as to the temperature-coefficients for conductivity and fluidity applies to all the coefficients for all the properties tested. A given change of concentration produces a change in the coefficients in the same sense. Too much importance, however, must not be attached to this; for it is obvious that if we had tabulated, say, the coefficients for conductivity, surface-tension, viscosity, and specific volume, it would have been found that the changes produced in the first two would have been in the opposite sense to those produced in the last two. But it is interesting to note that the expectation suggested by the above formulæ is distinctly realized.

At very great dilution of electrolytes the temperature-coefficient becomes, approximately,

$$\frac{1}{\mathrm{P}}\frac{\partial \mathrm{P}}{\partial t}=\left(\frac{\partial \mathrm{P}_w}{\partial t}+n\frac{\partial l}{\partial t}\right)\Big/(\mathrm{P}_w+nl), \quad . \quad . \quad . \quad (9)$$

the pressure-coefficient having the same form. The concentration-coefficient becomes

$$\frac{1}{\mathrm{P}}\frac{\partial \mathrm{P}}{\partial n}=l/(\mathrm{P}^w+nl). \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

If we compare (9) and (10) with (7) and (8), it becomes obvious that the variation with concentration of the temperature and pressure-coefficients will probably be more closely related at low than at high concentrations; but that the opposite will be true of the concentration-coefficients. Accordingly, having plotted Grotrian's coefficients and those of the above tables as functions of the concentration, I find that the temperature-coefficient curves are in general more closely similar at low than at high concentrations; but that this is not the case for the concentration-coefficient curves. In the case of the pressure-coefficients the data are insufficient.

A corresponding similarity holds for the absorption-spectra of solutions, though it cannot be expressed in coefficients. In a former paper * I have shown that for all solutions for which

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* Trans. Roy. Soc. Can. ix. (1891), sec. 3, p. 27.

data were available the absorption-spectra were similarly affected by elevation of temperature and increase of concentration.

The occasional constancy in the Difference between the Molecular Values of Properties of Solutions having the same Molecular Concentration.

The difference between the values per gramme-equivalent of any property for two simple solutions, 1 and 2, of the same concentration will be

$$(P_1-P_2)/n=k_1-k_2+(l_1-k_1)\alpha_1-(l_2-k_2)\alpha_2. \quad . \quad (11)$$

Now α in all cases diminishes as n increases. Provided, therefore, the values of the $(l-k)$'s have the same sign, and the rates of change of the α's with concentration are inversely proportional, or approximately so, to the $(l-k)$'s of their respective solutions, we shall have $(P_1-P_2)/n$ exactly or approximately constant, *i. e.* independent of the concentration. This approximate constancy will of course hold in all cases at extreme dilution.

In the case of solutions of moderate strength this phenomenon has been observed by Valson and Bender* for the density and refractive index, by Wagner† for viscosity constants, and by Jahn‡ for the electromagnetic rotation of the plane of polarization; and a very close approximation to constancy in the case of the densities of very dilute solutions is clearly shown in the results of Kohlrausch and Hallwachs's § observations.

It is obvious, from the values of l and k determined above for the density of sodium and potassium chlorides, that, as Bender found, this approximate constancy must hold for the densities of these salts. For $l-k$ for NaCl has the value $+0{\cdot}01424$, and for KCl $+0{\cdot}01316$; while a glance at the first table shows that the ionization-coefficient of solutions of the former salt falls off somewhat more rapidly with increasing concentration than that of the latter. It is equally obvious that as the value of $l-k$ for the thermal expansion of NaCl is $\cdot 0_3 91$, and for KCl $\cdot 0_2 13$, the thermal expansions of these salts will not exhibit this constancy. We accordingly find from Bender's observations

For $n=$	1.	1·5.	2.	2·5.
$(P_1-P_2)/n=$	$\cdot 0_3 108$	$\cdot 0_4 85$	$\cdot 0_4 815$	$\cdot 0_4 78$

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* Wied. *Ann.* xxxix. (1890) p. 89.

† *Zeitschr. für phys. Chemie*, v. (1890) p. 31.

‡ Wied. *Ann.* xliii. (1891) p. 280.

§ *Ibid.* liii. (1894) p. 14.

For viscosity $l-k$ for NaCl is $-0{\cdot}0022$, and for KCl $-0{\cdot}0028$. We may thus expect a closer approximation to constancy than in the last case; and from Brückner's observations we find

For n =	0·5.	1·0.	1·5.	2·0.	2·5.
$(P_1-P_2)/n$ =	·00116	·00122	·00126	·00128	·00135

For surface-tension $l-k$ for NaCl is $-0{\cdot}096$, and for KCl $-0{\cdot}116$. The approximation will thus not be so close as in the last case. Rother's observations give, by graphical interpolation,

For n =	1.	1·5.	2.
$(P_1-P_2)/n$ =	·016	·0113	·0105

For refractive index $l-k$ for NaCl is ·0054, and for KCl ·0091. The approximation will thus be still less close in this case. Bender's observations give for the D-line,

For n =	1.	2.	3.
$(P_1-P_2)/n$ =	$\cdot 0_3 29$	$\cdot 0_3 17$	$\cdot 0_3 24$

The above account of this phenomenon may be further tested by the aid of Kohlrausch's observations of conductivity; for in this case $l-k$ is the molecular conductivity at infinite dilution (μ_∞). The following values of differences of molecular conductivity will be sufficient:—

n.	$(P_1-P_2)/n$ for Conductivity.			
	HCl and NaCl.	HCl and $\frac{1}{2}K_2CO_3$.	$\frac{1}{2}H_2SO_4$ and $AgNO_3$.	$AgNO_3$ and NaCl.
0·01	2454	2333	1838	+55
0·1	2379	2365	1198	+21
0·5	2260	2289	1171	−29
1·0	2085	2120	1185	−60

Compare with this the following table of values of μ_∞ and α:—

n.	Ionization-Coefficients (α).				
	HCl $\mu_\infty=3500.$	NaCl $\mu_\infty=1030.$	$\frac{1}{2}K_2CO_3$ $\mu_\infty=1400.$	$\frac{1}{2}H_2SO_4.$ $\mu_\infty=3700.$	$AgNO_3.$ $\mu_\infty=1090.$
0·01	·976	·934	·773	·772	·933
0·1	·927	·840	·628	·563	·794
0·5	·862	·735	·520	·513	·668
1·0	·794	·675	·471	·492	·582

The approximate constancy holds in the case of HCl and $\frac{1}{2}K_2CO_3$, because μ_∞ for HCl being more than twice as great as for $\frac{1}{2}K_2CO_3$, α for the latter falls off nearly twice as rapidly as for the former. In the case of $AgNO_3$ and NaCl there is no approximation to constancy, because the values of μ_∞ being nearly equal, the rates at which α varies with n are very unequal.

The Independence of the Contributions made to the Value of a Property by the Free Ions.

The constant l for a salt *ap* will, according to the dissociation conception, be composed additively of two parts, l_a and l_p, pertaining to the ions a and p respectively, and these constants l_a and l_p will be characteristic of the ions and will not depend upon the salt from which they have been dissociated. A certain amount of evidence has been accumulated which may be said to point in this direction. In the case of several properties it has been shown that for solutions of considerable dilution, the difference between the values of the property for solutions of two salts having a common ion and the same molecular concentration, is independent of what the common ion may be; and the value of the difference divided by the numbers of gramme-equivalents per litre of the salts in solution has been taken to be approximately the difference between the constants l_a and l_p. Results of this kind have been obtained by Valson and Bender for density and refracting power, by Kohlrausch for electrical conductivity, by Raoult for the depression of the freezing-point, by Traube* for the change

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* *Ztschr. anorgan. Chemie*, iii. (1892), p. 1.

of volume on solution, by Röntgen and Schneider for compressibility, and by Jahn for the electromagnetic rotation of the plane of polarization.

Applying the above expression, we have for the difference in the values of a property per unit of molecular concentration,

$$(\mathrm{P}_{ap}-\mathrm{P}_{bp})/n=k_{ap}(1-\alpha_{ap})-k_{bp}(1-\alpha_{bp})+l_p(\alpha_{ap}-\alpha_{bp})+l_a\alpha_{ap}-l_b\alpha_{bp}, \quad (12)$$

and at infinite dilution

$$(\mathrm{P}_{ap}-\mathrm{P}_{bp})/n=l_a-l_b. \quad . \quad . \quad . \quad . \quad . \quad (13)$$

Had the experiments referred to been all carried out at extreme dilution, as were those of Kohlrausch, afterwards extended by Loeb and Nernst*, the evidence would be quite satisfactory. But in general they have been made at only moderate dilution, and it is obvious from (12) that the approximate independence of the common ion on the part of $(\mathrm{P}_{ap}-\mathrm{P}_{bp})/n$, may be quite consistent with considerable variation in l_a-l_b. It is clear that the first three terms of (12) may readily mask any variation in the last two, and that, if the last two did not vary, l_a-l_b could not in all cases be the same.

That no satisfactory conclusion can be drawn from experiments of this kind, unless conducted at extreme dilution, may be shown roughly in the case of density by the aid of the results obtained above. For we may assume that the ionization-constants for density obtained above will not be very different from those which would be derived from observations made at greater dilution†. We know from Kohlrausch and Hallwachs's observations that if ap and bp represent NaCl and $\frac{1}{2}\mathrm{Na_2CO_3}$ respectively $(\mathrm{P}_{ap}-\mathrm{P}_{bp})/n$ will have the value 0·0139 for solutions containing ·005 grm.-equivalents per litre, and that for NaCl and HCl it will have the value 0·0235. We may assume that for NaCl and KCl it will be about ·02. From the values of k for these salts we find the first two terms of (12) to be $\cdot 0_3 64$. If we assume l_p to have half the mean value of l for NaCl and KCl, the third term will amount to $-\cdot 0_4 98$. The first three terms thus amount to about $\cdot 0_3 54$, or say 4 per cent. of the value of $(\mathrm{P}_{ap}-\mathrm{P}_{bp})/n$. Thus, observations of the kind referred to, for density, could give no satisfactory result, even if conducted at this very great dilution. At a dilution of ·001 grm.-molecules per litre, the first three terms of (12), calculated

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* *Ztschr. für phys. Chemie*, ii. (1888), p. 948.

† Mr. E. H. Archibald, one of my students, tells me that for magnesium sulphate, Kohlrausch and Hallwachs's data give $k=\cdot 05663$ and $l=\cdot 066887$.

in the same way, amount to $\cdot 0_3 1$, or about 0·5 per cent. of $(P_{ap}-P_{bp})/n$. A proved independence of p at this dilution would be more satisfactory.

Observations at such extreme dilutions, in the case of most properties of solutions, are probably impracticable. But they are fortunately unnecessary for the settling of the question under consideration. For if the values of the ionization-constants for any property have been obtained as above from observations over a range extending to great, though not necessarily extreme, dilution, the values so obtained may fairly be assumed to apply very approximately to much greater dilutions; and from the values of l_a+l_p, l_b+l_p, l_a+l_q, l_b+l_q, &c., thus obtained, it may readily be determined whether or not l_a-l_b is independent of the ions p, q, &c. Unfortunately, Kohlrausch and Hallwachs's observations on density are not sufficiently numerous for this purpose.

The Determination of the Ionization-Constants for the Free Ions.

The values of the constants l_a, l_b, l_p, &c. may probably, in some cases at least, be determinable in the following way. The experiments just referred to would give l_a+l_p, l_b+l_p, &c., as well as k_{ap} and k_{bp}, &c. If now, guessing at the value of l_p, we find the first three terms of (12) to be negligible at dilutions at which P_{ab} and P_{bp} can be determined with sufficient accuracy, a determination of these quantities will give the value of $l_a\alpha_{ap}-l_b\alpha_{bp}$; and if α_{ap} and α_{bp} be known with sufficient accuracy, l_a, l_b, and l_p may then be found. It would of course be necessary to check our guess at the value of l_p by substituting the value found in expression (12) and seeing whether or not with this value the first three terms would be negligible.

The accurate determination of the ionization-constants for the various properties of a variety of solutions would form valuable material to serve as a basis for theory. I may therefore express the hope that observers who are so fortunate as to have the necessary instruments of precision at their disposal may be led by this paper to make the requisite observations.

XVI. *The Relation of Circular Polarization, as occurring both in the Amorphous and Crystalline States, to the Symmetry and Partitioning of Homogeneous Structures,* i. e. *of Crystals.*

By WILLIAM BARLOW[*].

THE general prevalence of the belief that circular polarization is a property connected with the symmetrical arrangement of small parts of the bodies displaying it, dates from Reusch's famous device for obtaining it with a stack of mica plates arranged as a staircase spiral. As we are now acquainted with the nature of the repetition in space which constitutes homogeneity of structure, and know how structures thus formed may be partitioned symmetrically[†], the time seems to have arrived for comparing the various distinct classes of cases in which circular polarization occurs with the geometrical possibilities for homogeneous structures, unbroken and broken, *i. e.* for substances both in the crystalline and the fluid conditions.

Adopting substantially the classification given by Pope, in a paper which he has recently published[‡], we may place the substances which possess the power of converting plane-polarized light into circularly-polarized light under five heads.

1. Those which exhibit circular polarization only in the amorphous state (that is, when dissolved, melted, or converted into gas).

2. Those among the substances showing circular polarization in the crystalline state only which owe the property to complex grouping or intercalation of crystal individuals, such as occurs in many cases of pseudo-symmetry.

3. Those other substances showing circular polarization in the crystalline state only in which it is an inherent property of the homogeneous structure itself.

4. Those among the substances which rotate the plane of polarization both when amorphous and when crystalline which owe the property, as displayed in the solid state, to complex intercalation of individual crystals.

5. Those other substances which possess the property both in the amorphous and in the crystalline state in which the circular polarization, when the latter state prevails, is a specific property of the homogeneous structure, and is not due to intercalation.

On the threshold of an inquiry as to the relation between

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[*] Communicated by the Author.

[†] 'Mineralogical Magazine,' 1896, xi. p. 119.

[‡] Trans. Chem. Soc. 1896, p. 971.

circular polarization and structure the very obvious conclusion suggests itself that since circular polarization is essentially an enantiomorphous property, if it is traceable to arrangement of parts, the structures in which it occurs must be enantiomorphous, *i. e.* not identical with their own mirror-images.

Now if we adopt the conclusion, reached in my paper just referred to, that the partitioning which achieves the separation of a crystal into individual molecules must be one which is compatible with the coincidence-movements (Deckbewegungen) of the structure *, it is evident that, when a structure identical with its own mirror-image undergoes such a partitioning, the fragments produced will either themselves be identical with their own mirror-images, or will occur in equal numbers in two kinds which are enantiomorphs. And that in the former case there will be no enantiomorphism, and that in the latter the effect of such a property of one kind of fragment will, so far as the general mass is concerned, be just cancelled by the similar property, of opposite hand, of the other kind of fragment.

We see therefore that we may confine our attention to those types of homogeneous structure which are enantiomorphs, *not only when dealing with crystals, but also when considering those cases where the substances displaying the property in question are in one of the amorphous states.*

If circular polarization is in some way connected with structure, the following general principles may be laid down :—

I. Circular polarization is due to the existence of certain arrangements of parts or particles of a structure, and these arrangements, which will be designated *effective configurations*, are of enantiomorphous form.

II. Absence of the property in a substance may be due either to the absence of effective configurations, or to the presence in equal numbers of two opposite kinds, whose effects just cancel one another, the arrangement of parts displayed by one kind being generally enantiomorphous, or nearly so, to that presented by the other kind.

Let us now endeavour to arrive geometrically at a classification of homogeneous structures according to the effective configurations which they contain, both when unbroken and when dislocated, which shall be parallel to the classification given above of substances possessing the property of circular polarization.

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* Min. Mag. 1896, xi. p. 130.

Class 1.

As a parallel to those substances which exhibit circular polarization in the amorphous state only, we have

Enantiomorphous homogeneous structures which while unbroken contain effective configurations of two opposite hands whose effects just cancel one another, but which, after being symmetrically partitioned and dislocated, contain but one kind, the other being destroyed in the process of dislocation.

An example from the cubic system will make this clear.

A homogeneous structure with gyrohedral symmetry of type 8[*] is, as we know, not identical with its own mirror-image; and if such a structure contains effective configurations, we may suppose that they consist of particles so placed as to form a single kind of Sohncke's 24-point-set, the centre A of the configurations occupying, therefore, the centres of half the cubes of a close-packed system of cubes filling space, symmetrically chosen, *i. e.* so chosen that they are in contact at their edges only[†].

But it can easily be shown that particles thus placed may equally well be regarded as forming other 24-point-sets whose centres lie at the centres B of the cubes of the other half of the system of cubes.

And *if the distance of a particle from the point* A *nearest to it is the same, or practically the same, as its distance from the nearest point* B, the only material difference between the forms of the 24-point-sets thus related will be that *one will be right-handed the other left-handed.*

If therefore the parts or particles of the effective configurations occupy positions thus about midway between the two kinds of singular points[‡] A, B, the rotation produced by their arrangement about centres A may be neutralized by the effect of their arrangement, of the contrary hand, about centres B.

The homogeneous structure, when in the solid or unbroken state, will then, as a whole, produce practically no rotation.

Not so, however, if it is symmetrically partitioned into fragments having one or other of the two kinds of singular points A, B for their centres, and then dislocated.

For it is evident that this will destroy one of the two kinds of effective configurations, and leave the other to pro-

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[*] *Zeitschr. f. Kryst.* 1894, xxiii. p. 18.

[†] The centres A form therefore a "cubisches flächencentrirtes Raumgitter," or, as Sohncke calls it, a "regulär oktaëdrisches Raumgitter."

[‡] *Zeitschr. f. Kryst.* xxiii. p. 60.

duce a rotation which is not counterbalanced by one of the opposite hand.

Points thus capable of being regarded as simultaneously forming two sets of distinct configurations which are enantiomorphous to one another or nearly so, are easily found in either of the enantiomorphous types *in which all the coincidence-movements taken together are identical with their own mirror-images*, *i. e.* in all of these types except Nos. 3, 4, 14, 15, 16, 17, 18, 19, 21, 22, 26, 27, 30, 31, 32, 33, 42, 43, 44, 45, 46, and 47.

In the last-named types the presence of helical structures of one hand without the helical structures enantiomorphous to them seems to prohibit any arrangement leading to *certain* compensation.

Class 2.

Parallel to those among the substances showing circular polarization in the crystalline state only which owe the property to complex grouping or intercalation of crystal individuals, such as occurs in many cases of pseudo-symmetry, we have

Homogeneous structures, single individuals of which contain no effective configurations, but which, when differently-orientated twin individuals of them are intercalated, form such configurations where the twin individuals meet, but not in two kinds which are enantiomorphs.

An example illustrating this may be presented by a twin combination of type 48[*].

For suppose that two identical homogeneous structures of this enantiomorphous type, which contain no effective configurations, are so intercalated that while their orientations about an axis differ by 60°, one particular set of axes is common to both[†], and further that at least one set of the singular points on these axes in one individual form, with the corresponding set in the other individual, a single continuous space-lattice. There are two ways in which this may happen, either the axes of one individual may have the same, or they may have the opposite orientation to that of the axes of the other individual.

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[*] *Zeitschr. f. Kryst.* xxiii. p. 31.

[†] A case in which the system of axes taken alone always possesses higher symmetry than the structure to which it belongs has been selected. Where this is not the case, for twinning of the nature described to occur, higher symmetry of the system of axes will have to subsist as a special condition, *e. g.* for individuals possessing rhombic symmetry when twinning thus to have common axes, the axes, *taken alone*, must be so situated as to form a system in trigonal or hexagonal symmetry.

For convenience of description let us suppose the axes to be vertical.

Then wherever one of the specified singular points of the one individual is found next to and vertically over a corresponding singular point of the other individual, the parts of one individual will be found turned through 60° as compared with the corresponding parts of the other, and thus they will be related like two succeeding steps of a spiral staircase. And we may suppose that such a disposition, in the case in question, is an effective configuration.

And since the individual structures thus related are identical, the effective configurations produced in this way will be all identical, and no configuration enantiomorphous to them will be present to set up circular polarization of the opposite hand to neutralize that which they set up.

As the formation of the effective configurations is an incident of the crystal grouping, they will not be found when the structure is partitioned and dislocated, but only where the twinning competent to produce them exists.

Class 3.

The parallel to the case of those of the substances showing circular polarization in the crystalline state only in which the property is inherent, is found in

Homogeneous structures which contain effective configurations that are not counterbalanced by configurations enantiomorphous to them, and in which, when partitioned and dislocated, these configurations are wanting, their destruction having been brought about by the dislocation.

An example to illustrate this case can be found in any enantiomorphous type which contains helical structure, the more prominent examples being furnished by those types in which the coincidence-movements due to the presence of helical structure are all of one hand.

For the helical structures may be supposed to be effective configurations, and when symmetrical partitioning and dislocation of the similar fragments take place, they will necessarily disappear*.

If, with the helical structure, all enantiomorphism disappears, so that the fragments of the enantiomorphous structure are, when taken alone, identical with their own mirror-images, it is evident that when a homogeneous structure is reformed from the fragments, it may be either a right-handed or a left-handed enantiomorph.

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* *See* Min. Mag. 1896, xi. p. 133.

With this may be compared such a fact as that sodium chlorate, which is optically inactive in solution, can be obtained in a dextro- or lævo-rotatory form at will.

Class 4.

To those among the substances that rotate the plane of polarization both when amorphous and when crystalline which owe the property, as displayed in the solid state, to complex intercalation of individual crystals, we find a parallel in

Homogeneous structures which, like those of Class 1, *while unbroken contain effective configurations of two opposite kinds whose effects just cancel one another, but which, after being symmetrically partitioned and dislocated, contain but one kind, the other being destroyed in the process of dislocation; and which also like those of Class* 2, *when differently orientated twin individuals of them are intercalcated, form additional effective configurations where the twinned individuals meet, but not in two kinds which are enantiomorphs.*

Type 49[*] will furnish an example of this if the following conditions obtain:—

a. The particles which furnish the effective configurations in the amorphous state, *i. e.* after partitioning and dislocation have taken place, form enantiomorphous 6-point-sets whose centres in the unbroken structure are a set of the principal singular points, *i. e.* of those lying at the intersections of trigonal and digonal axes.

b. These particles are so placed that the 6-point-sets of opposite hand which, in the unbroken structure, they also form[†], and whose centres constitute another set of principal singular points, are practically enantiomorphous to the first-named 6-point-sets, and the circular polarization produced by one set of 6-point-sets neutralizes, on the whole, in the unbroken structure, that of opposite rotation produced by the other set.

c. Intercalation of twinned individuals, similar to that described as occurring in Class 2, produces, as in that case, effective configurations where the individuals meet.

The origin of the circular polarization displayed in the amorphous state is in this class different from that to which it is traceable in the twinned condition.

A new and striking instance of this class of circular polarization is given by Pope in the paper already referred to[‡], viz., *trans*-π-camphotricarboxylic acid.

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[*] *Zeitschr. f. Kryst.* xxiii. p. 31.

[†] Comp. p. 112.

[‡] Trans. Chem. Soc. 1896, pp. 972 and 978.

Class 5.

Finally, a parallel to those among the substances which rotate the plane of polarization both when amorphous and when crystalline, in which the circular-polarization, when the latter state prevails, is a specific property of the homogeneous structure, is afforded by

Homogeneous structures which contain effective configurations that do not, on the whole, neutralize one another, and some of which are not destroyed when the structure is partitioned symmetrically and dislocated.

A careful examination of the various types of enantiomorphous homogeneous structure reveals the fact that in most of them the presence of effective configurations of any one kind, which are not helical structures, involves the presence in the same structure of other configurations formed of the same particles as those which compose the first-named configurations but differently allotted, which configurations, whether very similar or not, *are of the opposite hand**.

In such cases the two kinds of configurations need not, of course, neutralize one another, although probably in many cases they would practically do so.

However, there are many other types whose parts are not thus balanced, and which certainly will not by any such property be made inactive.

It is one of these latter which is selected as an illustration.

In a structure of type 12 † we may suppose that the effective configurations consist of particles so placed as to form Sohncke 24-point-sets whose centres A are the cube-centres of a close-packed system of cubes.

These same particles may then equally well be regarded as forming 24-point-sets whose centres B occupy all the cube angles, but the two kinds of configuration cannot possibly in this case be supposed to neutralize one another because they are both of the same hand. They must, on the contrary, be expected to reinforce one another.

Symmetrical partitioning into similar fragments, followed by dislocation, if it leaves one of the two sets of configurations intact, will destroy the other, and thus we may look for different specific rotation in the broken from that in the unbroken structure as the result of such a dislocation.

Cases are conceivable in which effective configurations of opposite hand are present in a structure, and those destroyed

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* Comp. p. 112.

† *Zeitschr. f. Kryst.* xxiii. p. 21.

by dislocation are the *dominant* ones. In this event the sign of the specific rotation will change as we pass from the unbroken to the broken condition.

Wyrouboff's discovery that in the case of rubidium tartrate, which is optically active in both states, the sense of the rotation is not the same in both, may be mentioned in this connexion*.

We see from the foregoing that exactly corresponding to the five classes into which substances that possess the power of converting plane-polarized light into circularly-polarized light can be divided, there are five classes of structure distinguishable by characteristic geometrical features.

It can therefore hardly be doubted that circular polarization is a mechanical effect depending on the relative situation of the ultimate parts of bodies, and that the disappearance of the property and the changes in it observed when the state of a body displaying it alters are also mechanical effects entirely due to changes in geometrical configuration.

XVII. *The Transitive Substitution Groups of Order* 8p, p *being any Prime Number. By* G. A. MILLER, *Ph.D.*†

IN a recent paper published in this Journal‡ we determined all the possible operation groups of order $8p$. The present paper is devoted to the more general problem of determining all the possible transitive substitution groups of this order. In solving this problem it is convenient to employ the results of the preceding paper together with the following two theorems.

THEOREM I.—*The number of transitive substitution groups which are simply isomorphic to an operation group* (G) *is equal to the number of different systems of subgroups of* (G), *such that each system includes all the subgroups that are transformed into each other when* G *is transformed by all the operations that are commutative to it, and none of these systems includes any invariant (self-conjugate) subgroup of* G *with the exception of identity. The substitutions of these simply isomorphic transitive groups can be directly obtained from* G, *and the degrees of these groups are the quotients obtained by dividing the order of* G *by the orders of the subgroups in the given systems* §.

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* *Journ. de Physique*, 1894, p. 451.

† Communicated by the Author.

‡ August 1896, vol. xlii. pp. 195–200.

§ *Cf.* Dyck, *Mathematische Annalen*, vol. xxii. p. 90.

THEOREM II.—*Every simple isomorphism of an operation group to itself can be obtained by transforming the group by means of operations that are commutative to it*[*].

It is evident that identity forms one of the systems of subgroups of theorem I., and that there is one and only one such system in each operation group. In other words, each operation group is simply isomorphic to one, and only one, regular substitution group. This substitution group is completely determined by the simply isomorphic operation group and *vice versâ*. It may happen that this is the only transitive substitution group that is simply isomorphic to a given operation group. This is clearly always the case when the operation group is a commutative group, as all the subgroups of such a group are invariant.

Unless the contrary is stated, p is supposed to exceed 2, There are then 3 commutative operation groups of order $8p$. These have been denoted by G_1, G_2, and G_9 in the paper to which reference has been made. The simply isomorphic regular groups may be conveniently obtained by forming three heads of order 8, each being obtained by writing one of the commutative groups of order 8 in p different systems of letters and placing the identical substitutions in correspondence, and by adding to each of these heads the substitution of order p which merely interchanges its systems of intransivity.

G_1 contains the following subgroups :—

Order of subgroups	$4p$	$2p$	p	8	4	2	1
Number ,, ...	1	1	1	1	1	1	1

G_2 contains the following subgroups :—

Order of subgroups	$4p$	$2p$	p	8	4	2	1
Number ,, ...	3	3	1	1	3	3	1

G_9 contains the following subgroups :—

Order of subgroups	$4p$	$2p$	p	8	4	2	1
Number ,, ...	7	7	1	1	7	7	1

G_3[†] contains the following subgroups :—

Order of subgroups	$4p$	$2p$	p	8	4	2	1
Number ,, ...	3	3	1	p	$2p+1$	$2p+1$	1

G_3 contains one system of $2p$ subgroups of order 2 that does not include an invariant subgroup besides identity. It

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[*] *Cf.* Frobenius, *Sitzungsberichte der Berliner Akademie*, 1895, p. 184.

[†] These group symbols have the same meaning throughout this paper as they have in the paper to which reference has been made.

is therefore simply isomorphic to *two* transitive groups whose degrees are $8p$ and $4p$ respectively. All the substitutions of the group of degree $8p$ are regular, and $8p-1$ of them are of degree $8p$. Their group properties are known from the corresponding operations of G_3. Hence the regular simply isomorphic group is completely determined. Since each one of the subgroups of order 2 in the given system is transformed into itself by 8 operations of G_3, the transitive group of degree $4p$ which is simply isomorphic to G_3 must contain p substitutions whose class is $4(p-1)$. All its other substitutions, except identity, are of class $4p$. It may be conveniently constructed by means of a cyclical substitution of order $4p$, and any substitution that transforms this into its $2p-1$ power. It may be observed that while the given $2p$ subgroups of order 2 are conjugate in the largest group that is commutative to G_3, these subgroups are not all conjugate in the largest group that transforms the given group of degree $4p$ into itself.

G_4 contains the following subgroups :—

Order of subgroups ……	$4p$	$2p$	p	8	4	2	1
Number ,, ……	3	5	1	1	3	5	1

G_5 contains the following subgroups :—

Order of subgroups ……	$4p$	$2p$	p	8	4	2	1
Number ,, ……	3	5	1	p	$2p+1$	$4p+1$	1

Each of these two groups is simply isomorphic to *two* transitive groups whose degrees are $8p$ and $4p$ respectively. The two non-regular transitive groups may be constructed by means of a cyclical substitution of order $4p$, and any substitutions that transform it into its $2p+1$ and $4p-1$ powers respectively. The group that is simply isomorphic to G_4 contains 2 substitutions of degree $2p$, and that which is simply isomorphic to G_5 contains $2p$ substitutions of degree $4p-2$.

G_6 contains the following subgroups :—

Order of subgroups ……	$4p$	$2p$	p	8	4	2	1
Number ,, ……	1	1	1	p	1	1	1

G_7 contains the following subgroups :—

Order of subgroups ……	$4p$	$2p$	p	8	4	2	1
Number ,, ……	3	1	1	1	3	1	1

G_8 contains the following subgroups :—

Order of subgroups ……	$4p$	$2p$	p	8	4	2	1
Number ,, ……	3	1	1	p	$2p+1$	1	1

Since each of these three groups contains only one subgroup of order 2, none of them can be simply isomorphic to any transitive group besides the regular group, for the degree of a simply isomorphic group can clearly not be less than $4p$, since such a group has to contain an operation of order $4p$. We have now considered the 8 operation groups which contain an operation of order $4p$, and found that only three of them are simply isomorphic to a non-regular transitive group.

G_{10} contains the following subgroups:—

Order of subgroups ……	$4p$	$2p$	p	8	4	2	1
Number ,, ……	7	7	1	p	$6p+1$	$4p+3$	1

G_{11} contains the following subgroups:—

Order of subgroups ……	$4p$	$2p$	p	8	4	2	1
Number ,, ……	3	3	1	p	$2p+1$	3	1

G_{12} contains the following subgroups:—

Order of subgroups ……	$4p$	$2p$	p	8	4	2	1
Number ,, ……	3	5	1	p	$2p+1$	$2p+3$	1

These three groups and G_9 are the only operation groups of order $8p$ that contain the non-cyclical commutative group of order $4p$, but not the cyclical group of this order. G_{10} is simply isomorphic to one transitive group of degree $4p$, G_{11} is not simply isomorphic to any transitive group besides the regular one, while G_{12} is simply isomorphic to two non-regular transitive groups. The group of degree $4p$ which is simply isomorphic to G_{10} may be constructed by adding to the non-cyclical commutative regular group of order $4p$ any substitution that transforms all its substitutions into their $2p-1$ power. It contains p substitutions of degree $4(p-1)$. The rest of its substitutions, excepting identity, are of degree $4p$. The two groups of degree $4p$ which are simply isomorphic to G_{12} may be constructed by adding to the same regular group two substitutions that transform one of its subgroups of order $2p$ into itself and interchange the other two subgroups of this order. One of these contains 2 substitutions of degree $2p$, while the other contains $2p$ substitutions of degree $4p-2$. The rest of the substitutions, except identity, are of degree $4p$.

We have now considered the 12 operation groups of order $8p$ which exist for all values of p, and found the 18 simply isomorphic transitive substitution groups. Hence there are just 18 transitive substitution groups of order $8p$ that contain an invariant subgroup of order p when $p-1$ is not divisible by 4. 7 of these are not simply isomorphic to any transitive group except to themselves, 4 are simply isomorphic to one other transitive group, and 1 is simply isomorphic to two other transitive groups.

G_{13} contains the following subgroups:—

Order of subgroups ……	$4p$	$2p$	p	8	4	2	1
Number ,, ……	3	3	1	p	$3p$	$2p+1$	1

G_{14} contains the following subgroups:—

Order of subgroups ……	$4p$	$2p$	p	8	4	2	1
Number ,, ……	1	1	1	p	p	1	1

Since G_{13} contains one system of subgroups of order 4 and two systems of order 2 that do not contain any invariant subgroup besides identity, it is simply isomorphic to four transitive substitution groups. Each of the substitutions, except identity, in any one of these $2p$ subgroups of order 4 is transformed into itself by 8 substitutions. Hence the simply isomorphic group of degree $2p$ contains $3p$ substitutions of degree $2(p-1)$, $2p$ of order 4, and p of order 2. This group may be constructed by means of the cyclical substitution of order $2p$ and a substitution which transforms it into any power that belongs to exponent 4 with respect to mod. $2p$.

Each of the two groups of degree $4p$ which are simply isomorphic to G_{13} contains p substitutions of degree $4(p-1)$. In one of the groups these substitutions are the squares of its substitutions of order 4. Its substitutions of order 4 therefore consist of $p-1$ cycles of order 4 and two cycles of order 2. In the other group the substitutions of order 4 are composed of p cycles of order 4. These two groups may be constructed by making a cyclical group of order $2p$ simply isomorphic to itself, and adding to this intransitive head two substitutions which interchange its systems of intransitivity and at the same time transform a cyclic substitution of order $2p$ into a power which belongs to exponent 4 mod. $2p$.

G_{14} is not simply isomorphic to any transitive group besides the regular group. These two groups, G_{13} and G_{14}, are the only groups of order $8p$ which occur only when $p-1$ is divisible by 4 but not by 8. It remains to consider the single group (G_{15}) which occurs only when $p-1$ is divisible by 8.

G_{15} contains the following subgroups:—

Order of subgroups ……	$4p$	$2p$	p	8	4	2	1
Number ,, ……	1	1	1	p	p	p	1

Since G_{15} contains one system of subgroups of each of the orders 8, 4, 2, 1 that does not include any invariant subgroup besides identity, it is simply isomorphic to one transitive substitution group of each of the degrees p, $2p$, $4p$, $8p$. These groups can be directly constructed as each of them contains an invariant subgroup of order p, and a substitution of order 8 which transforms the substitutions of this subgroup into a power which belongs to exponent 8 mod. p. The group of degree p is the only primitive group of order $8p$ that contains an invariant subgroup of order p.

Hence there are 18 transitive substitution groups of order $8p$ that contain an invariant subgroup of order p when $p-1$ is not divisible by 4. When $p-1$ is divisible by 4 but not by 8 there are 23 such groups, and when $p-1$ is divisible by 8 the number of these groups is 27. The operation groups which do not contain an invariant subgroup of order p occur only when p is equal to 7 or 3. When $p=7$ there is only one such group (G_{16}).

G_{16} contains the following subgroups :—

Order of subgroups ……	$4p$	$2p$	p	8	4	2	1
Number ,, ……	0	0	8	1	7	7	1

As G_{16} contains one system of subgroups of each of the orders p, 4, 2, 1 that does not include any invariant subgroup besides identity, it is simply isomorphic to one transitive substitution group of each of the degrees 8, $2p$, $4p$, $8p$. These groups can be readily constructed by means of the invariant subgroup of order 8, which contains 7 substitutions of order 2, and a substitution of order 7 which interchanges its substitutions of order 2 cyclically. Hence there are 22 transitive substitution groups of order 56; 13 of these are regular. The group of degree 8 is primitive. The others are non-primitive since the subgroups of the orders 4, 2, 1 are clearly not maximal[*].

When $p=3$ there are three operation groups (G_{17}, G_{18}, G_{19}) that do not contain an invariant subgroup of order 3.

G_{17} contains the following subgroups :—

Order of subgroups ……	$4p$	$2p$	p	8	4	2	1
Number ,, ……	1	4	4	3	7	9	1

G_{18} contains the following subgroups :—

Order of subgroups ……	$4p$	$2p$	p	8	4	2	1
Number ,, ……	1	4	4	1	7	7	1

G_{19} contains the following subgroups :—

Order of subgroups ……	$4p$	$2p$	p	8	4	2	1
Number ,, ……	0	4	4	1	3	1	1

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[*] Dyck, *Mathematische Annalen*, vol. xxii. p. 91.

G_{17} contains one system of subgroups of each of the three orders $2p$, p, 1, and two systems of each of the two orders 4, 2 that do not contain an invariant subgroup except identity. It is therefore simply isomorphic to the following transitive groups :—

Degree of groups	4	8	24	6	12
Number „	1	1	1	2	2

These seven simply isomorphic transitive groups, in order, have been denoted as follows[*] :—*(abcd)* all, *(abcd . efgh)* pos. *(ae . bg . cf . dh)*, *(am . bn . cp . do . ex . fw . gu . hv . is . jt . kr . lq .* H_4, $(+abcdef)_{24}$, $(\pm abcdef)_{24}$, $(abcdef\,.\,ghijkl)_{12_2}$ *(ahcj . bidg) ek . fl)*, $(abcd\,.\,efgh\,.\,ijkl)_4$ *(afk . bgi . cej . dhl) (af . be . cg . dh . ij)*.

G_{18} contains one system of subgroups of each of the three orders, p, 4, 1, and two systems of order 2 that do not contain any invariant subgroup except identity. It is therefore simply isomorphic to the following transitive groups :—

Degree of groups	8	6	24	12
Number „	1	1	1	2

These five simply isomorphic transitive groups, in order, have been denoted as follows :—
(abcd . efgh) pos. *(ae . bf . cg . dh)*, $(abcdef)_{24_5}$, *(am . bn . co . dp . eq . fr . gs . ht . iu . jv . kw . lx)*H_4, $(abcdef\,.\,ghijkl)_{12_2}$ *(ag . bh . ci . dj . ek . fl)*, $(abcd\,.\,efgh\,.\,ijkl)_4$ *(afk . bgi . cej . dhl) (ab . cd . il . jk)* [†].

G_{19} contains only two systems of subgroups that do not contain an invariant subgroup except identity. The orders of these subgroups are 3 and 1 respectively. The simply isomorphic transitive groups have been denoted by

(ab . cd . ef . gh) (ABCD) pos., *(aceg . bdjh . ikmo . jlnp . qsuw . rtvx)(abef . chgd . ijmn . kpol . qruv . sxwt)(akr . bis . cjq . dlt . eov . fmw . gnu . hpx)*.

Hence there are 32 transitive substitution groups of order 24. 15 of these groups are regular. The group of degree 4 is primitive. All the others are nonprimitive.

We have thus far excluded the special case when $p=2$.

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[*] *Cf.* Cayley, 'Quarterly Journal of Mathematics,' vol. xxv. p. 71.

[†] This group is not included in the list of transitive substitution groups of degree 12 recently published in the 'Quarterly Journal of Mathematics.' It seems to be the only transitive group of order 24 that has not yet been published. It contains three systems of nonprimitivity.

For the sake of completeness we shall consider these groups very briefly, although all of them are known.

Since 4! is not divisible by 16 it follows from theorem I. that every subgroup of order 4 contained in a group of order 16 must include an invariant subgroup which differs from identity. All the subgroups of order 8 contained in such a group are known to be invariant. It remains therefore only to consider the subgroups of order 2.

We may now show directly by means of the groups of order 8 that an operation group of order 16 cannot be simply isomorphic to more than one non-regular transitive group. We may regard a commutative subgroup of order 8 as the head of such a group. When this is cyclical the truth of the statement is evident, since any operation of order 2 in the tail may be made to correspond to any other of its operations of this order. When the head contains four operations of order 4 and an operation of the tail is commutative to its three operations of order 2, the statement is true for the same reason. Finally, when the tail contains no operation that is commutative to the three operations of order 2 in the head, and also no operation of order 8, it must contain 4 operations of order 4 and 4 of order 2, since any operation of the tail and the subgroup of the head which is generated by its operations of order 2 must generate the non-commutative group of order 8 which contains five operations of order 2.

In this last case we have that an operation of order 2 in the tail multiplied into two operations of order 4 in the head gives two operations of order 2. Hence the operations of the tail transform one of the cycles of order 4 in the head into its third power, and are commutative to the other two operations of order 4 in the head. Hence any operation of order 2 in the tail may be made to correspond to either of the two non-commutative operations of this order in the head as well as to any other operations of the same order in the tail. As the only group which does not contain at least one of the two given commutative groups of order 8 is commutative, the statement is proved.

We have now proved that every operation group of order 16 that contains non-commutative operations of order 2 is simply isomorphic to one and only one non-regular transitive group. Hence there are 20 transitive substitution groups of order 16 ; 14 of these are regular.

Summary.

When $p=2$ there are 20 transitititive substitution groups of order $8p$. Six are of degree 8 and fourteen are of degree 16.

When $p=3$ there are 32 such groups. They occur as follows:—

Degree of groups	4	6	8	12	24
Number ,, ...	1	3	3	10	15

When $p=7$ there are 22 such groups. They occur as follows:—

Degree of groups	8	14	28	56
Number ,, ...	1	1	7	13

When $p-1$ is divisible by 8 there are 27 such groups. They occur as follows:—

Degree of groups	p	$2p$	$4p$	$8p$
Number ,, ...	1	2	9	15

When $p-1$ is divisible by 4 but not by 8 there are 23 such groups. They occur as follows —

Degree of groups	$2p$	$4p$	$8p$
Number ,, ...	1	8	14

When $p-1$ is not divisible by 4 and p does not have one of the three values 2, 3, 7, there are 18 such groups. They occur as follows:—

Degree of groups	$4p$	$8p$
Number ,, ...	6	12

The three groups whose degrees are 4, 8, and p respectively are primitive. All the others are nonprimitive. When $p=2$ there are five commutative groups, but when $p>2$ there are only three such groups. When $p=2$ there are three non-commutative groups that are not simply isomorphic to any non-regular transitive group. When $p-1$ is divisible by 4 there are five such groups. When this condition is not satisfied and $p>2$ there are four such groups.

Paris, December 1896.

XVIII. *On the Passage of Electric Waves through Tubes, or the Vibrations of Dielectric Cylinders.* By Lord Rayleigh, *F.R.S.**

General Analytical Investigation.

THE problem here proposed bears affinity to that of the vibrations of a cylindrical solid treated by Pochhammer† and others, but when the bounding conductor is

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* Communicated by the Author.

† Crelle, vol. xxxi. 1876.

regarded as perfect it is so much simpler in its conditions as to justify a separate treatment. Some particular cases of it have already been considered by Prof. J. J. Thomson*. The cylinder is supposed to be infinitely long and of arbitrary section; and the vibrations to be investigated are assumed to be periodic with regard both to the time (t) and to the coordinate (z) measured parallel to the axis of the cylinder, *i. e.*, to be proportional to $e^{i(mz+pt)}$.

By Maxwell's Theory, the components of electromotive intensity in the dielectric (P, Q, R) and those of magnetic induction (a, b, c) all satisfy equations such as

$$\frac{d^2\mathrm{R}}{dx^2}+\frac{d^2\mathrm{R}}{dy^2}+\frac{d^2\mathrm{R}}{dz^2}=\frac{1}{\mathrm{V}^2}\frac{d^2\mathrm{R}}{dt^2}, \quad . \quad . \quad . \quad . \quad (1)$$

V being the velocity of light; or since by supposition

$$\frac{d^2\mathrm{R}}{dz^2}=-m^2\mathrm{R}, \qquad \frac{d^2\mathrm{R}}{dt^2}=-p^2\mathrm{R},$$

$$\frac{d^2\mathrm{R}}{dx^2}+\frac{d^2\mathrm{R}}{dy^2}+k^2\mathrm{R}=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where

$$k^2=p^2/\mathrm{V}^2-m^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)\dagger$$

The relations between P, Q, R and a, b, c are expressed as usual by

$$\frac{da}{dt}=\frac{d\mathrm{Q}}{dz}-\frac{d\mathrm{R}}{dy}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

and two similar equations; while

$$\frac{da}{dx}+\frac{db}{dy}+\frac{dc}{dz}=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

$$\frac{d\mathrm{P}}{dx}+\frac{d\mathrm{Q}}{dy}+\frac{d\mathrm{R}}{dz}=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

The conditions to be satisfied at the boundary are that the components of electromotive intensity parallel to the surface shall vanish. Accordingly

$$\mathrm{R}=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

$$\mathrm{P}\frac{dx}{ds}+\mathrm{Q}\frac{dy}{ds}=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

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* 'Recent Researches in Electricity and Magnetism,' 1893, § 300.

† The k^2 of Prof. J. J. Thomson (*loc. cit.* § 262) is the negative of that here chosen for convenience.

dx/ds, dy/ds being the cosines of the angles which the tangent (ds) at any point of the section makes with the axes of x and y.

Equations (2) and (7) are met with in various two-dimensional problems of mathematical physics. They are the equations which determine the free transverse vibrations of a stretched membrane whose fixed boundary coincides with that of the section of the cylinder. The quantity k^2 is limited to certain definite values, k_1^2, k_2^2, . . ., and to each of these corresponds a certain normal function. In this way the possible forms of R are determined. A value of R which is zero throughout is also possible.

With respect to P and Q we may write

$$\mathrm{P}=\frac{d\phi}{dx}+\frac{d\psi}{dy}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

$$\mathrm{Q}=\frac{d\phi}{dy}-\frac{d\psi}{dx}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

where ϕ and ψ are certain functions, of which the former is given by

$$\nabla^2\phi=\frac{d\mathrm{P}}{dx}+\frac{d\mathrm{Q}}{dy}=-\frac{d\mathrm{R}}{dz}=-im\mathrm{R}. \quad . \quad . \quad (11)$$

There are thus two distinct classes of solutions; the first dependent upon ϕ, in which R has a finite value, while $\psi=0$; the second dependent upon ψ, in which R and ϕ vanish.

For a vibration of the first class we have

$$\mathrm{P}=d\phi/dx, \qquad \mathrm{Q}=d\phi/dy, \quad . \quad . \quad . \quad . \quad (12)$$

and

$$(\nabla^2+k^2)\phi=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

Accordingly by (11)

$$\phi=\frac{im}{k^2}\mathrm{R}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

and

$$\mathrm{P}=\frac{im}{k^2}\frac{d\mathrm{R}}{dx}, \qquad \mathrm{Q}=\frac{im}{k^2}\frac{d\mathrm{R}}{dy}, \quad . \quad . \quad . \quad . \quad (15)$$

by which P and Q are expressed in terms of R supposed already known.

The boundary condition (7) is satisfied by the value ascribed to R, and the same value suffices also to secure the fulfilment of (8), inasmuch as

$$\mathrm{P}\frac{dx}{ds}+\mathrm{Q}\frac{dy}{ds}=\frac{im}{k^2}\frac{d\mathrm{R}}{ds}=0.$$

The functions P, Q, R being now known, we may express a, b, c. From (4)

$$\frac{da}{dt}=ipa=im\mathrm{Q}-\frac{d\mathrm{R}}{dy}=-\frac{m^2+k^2}{k^2}\frac{d\mathrm{R}}{dy};$$

so that

$$a=-\frac{m^2+k^2}{ipk^2}\frac{d\mathrm{R}}{dy},\qquad b=\frac{m^2+k^2}{ipk^2}\frac{d\mathrm{R}}{dx},\qquad c=0.\,.\quad(16)$$

In vibrations of the second class $\mathrm{R}=0$ throughout, so that (2) and (7) are satisfied, while k^2 is still at disposal. In this case

$$\mathrm{P}=d\psi/dy,\qquad \mathrm{Q}=-d\psi/dx,\quad .\ .\ .\ .\ (17)$$

and

$$(\nabla^2+k^2)\psi=0.\quad .\ .\ .\ .\ .\ .\ .\ .\ (18)$$

By the third of equations (4)

$$\frac{dc}{dt}=ipc=\frac{d\mathrm{P}}{dy}-\frac{d\mathrm{Q}}{dx}=\nabla^2\psi=-k^2\psi;$$

so that $\psi=-ipc/k^2$, and

$$\mathrm{P}=-\frac{ip}{k^2}\frac{dc}{dy},\quad \mathrm{Q}=\frac{ip}{k^2}\frac{dc}{dx},\quad \mathrm{R}=0.\quad .\ .\ .\ (19)$$

Also by (4)

$$a=\frac{im}{k^2}\frac{dc}{dx},\quad b=\frac{im}{k^2}\frac{dc}{dy}.\quad .\ .\ .\ .\ .\ (20)$$

Thus all the functions are expressed by means of c, which itself satisfies

$$(\nabla^2+k^2)c=0.\quad .\ .\ .\ .\ .\ .\ .\ (21)$$

We have still to consider the second boundary condition (8). This takes the form

$$\frac{dc}{dy}\frac{dx}{ds}-\frac{dc}{dx}\frac{dy}{ds}=0,$$

requiring that dc/dn, the variation of c along the normal to the boundary at any point, shall vanish. By (21) and the boundary condition

$$dc/dn=0,\quad .\ .\ .\ .\ .\ .\ .\ .\ (22)$$

the form of c is determined, as well as the admissible values of k^2. The problem as regards c is thus the same as for the two-dimensional vibrations of gas within a cylinder which is bounded by rigid walls coincident with the conductor, or for the vibrations of a liquid under gravity in a vessel of the same form*.

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* Phil. Mag. vol. i. p. 272 (1876).

All the values of k determined by (2) and (7), or by (21) and (22), are real, but the reality of k still leaves it open whether m in (3) shall be real or imaginary. If we are dealing with free stationary vibrations m is given and real, from which it follows that p is also real. But if it be p that is given, m^2 may be either positive or negative. In the former case the motion is really periodic with respect to z; but in the latter z enters in the forms $e^{m'z}$, $e^{-m'z}$, and the motion becomes infinite when $z=+\infty$, or when $z=-\infty$, or in both cases. If the smallest of the possible values of k^2 exceeds p^2/V^2, m is necessarily imaginary, that is to say no periodic waves of the frequency in question can be propagated along the cylinder.

Rectangular Section.

The simplest case to which these formulæ can be applied is when the section of the cylinder is rectangular, bounded, we may suppose, by the lines $x=0$, $x=\alpha$, $y=0$, $y=\beta$.

As for the vibrations of stretched membranes,* the appropriate value of R applicable to solutions of the first class is

$$\mathrm{R}=e^{i(mz+pt)}\sin(\mu\pi x/\alpha)\sin(\nu\pi y/\beta)\ ; \quad . \quad . \quad . \quad (23)$$

from which the remaining functions are deduced so easily by (15), (16) that it is hardly necessary to write down the expressions. In (23) μ and ν are integers, and by (13)

$$k^2=\pi^2\left(\frac{\mu^2}{\alpha^2}+\frac{\nu^2}{\beta^2}\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

whence

$$m^2=p^2/\mathrm{V}^2-\pi^2\left(\frac{\mu^2}{\alpha^2}+\frac{\nu^2}{\beta^2}\right). \quad . \quad . \quad . \quad . \quad (25)$$

The lowest frequency which allows of the propagation of periodic waves along the cylinder is given by

$$\frac{p^2}{\mathrm{V}^2}=\frac{\pi^2}{{}^2}+\frac{\pi^2}{\beta^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (26)$$

If the actual frequency of a vibration having its origin at any part of the cylinder be much less than the above, the resulting disturbance is practically limited to a neighbouring finite length of the cylinder.

For vibrations of the second class we have

$$c=e^{i(mz+pt)}\cos(\mu\pi x/\alpha)\cos(\nu\pi y/\beta), \quad . \quad . \quad . \quad . \quad (27)$$

the remaining functions being at once deducible by means of (19), (20). The satisfaction of (22) requires that here again

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* 'Theory of Sound,' § 195.

μ, ν be integers, and (21) gives

$$k^2=\pi^2\left(\frac{\mu^2}{\alpha^2}+\frac{\nu^2}{\beta^2}\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

identical with (24).

If $\alpha>\beta$, the smallest value of k corresponds to $\mu=1$, $\nu=0$. When $\nu=0$, we have $k=\mu\pi/\alpha$, and if the factor $e^{i(mz+pt)}$ be omitted,

$$a=-\frac{im}{k}\sin kx, \quad b=0, \quad c=\cos kx, \quad . \quad . \quad (29)$$

$$\text{P}=0, \quad \text{Q}=-\frac{ip}{k}\sin kx, \quad \text{R}=0\,; \quad . \quad . \quad . \quad (30)$$

a solution independent of the value of β. There is no solution derivable from $\mu=0$, $\nu=0$, $k=0$*.

Circular Section.

For the vibrations of the first class we have as the solution of (2) by means of Bessel's functions,

$$\text{R}=\text{J}_n(kr)\cos n\theta, \quad . \quad . \quad . \quad . \quad . \quad . \quad (31)$$

n being an integer, and the factor $e^{i(mz+pt)}$ being dropped for the sake of brevity. In (31) an arbitrary multiplier and an arbitrary addition to θ are of course admissible. The value of k is limited to be one of those for which

$$\text{J}_n(kr')=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (32)$$

at the boundary where $r=r'$.

The expressions for P, Q, a, b, c in (15), (16) involve only $d\text{R}/dx$, $d\text{R}/dy$. For these we have

$$\frac{d\text{R}}{dx}=\frac{d\text{R}}{dr}\cos\theta-\frac{d\text{R}}{r\,d\theta}\sin\theta=k\,\text{J}_n'(kr)\cos n\theta\cos\theta$$

$$+\frac{n}{r}\text{J}_n(kr)\sin n\theta\sin\theta$$

$$=\tfrac{1}{2}k\cos(n-1)\theta\left\{\text{J}_n'+\frac{\text{J}_n}{kr}\right\}+\tfrac{1}{2}k\cos(n+1)\theta\left\{\text{J}_n'-\frac{\text{J}_n}{kr}\right\}$$

$$=\tfrac{1}{2}k\cos(n-1)\theta\,\text{J}_{n-1}(kr)-\tfrac{1}{2}k\cos(n+1)\theta\,\text{J}_{n+1}(kr), \quad . \quad (33)$$

according to known properties of these functions; and in

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* For (18) would then become $\nabla^2\psi=0$; and this, with the boundary condition $d\psi/dn=0$, would require that P and Q, as well as R, vanish throughout.

like manner

$$\frac{d\mathrm{R}}{dy}=\frac{d\mathrm{R}}{dr}\sin\theta+\frac{d\mathrm{R}}{r\,d\theta}\cos\theta=-\tfrac{1}{2}k\sin(n-1)\theta\,\mathrm{J}_{n-1}(kr)$$
$$-\tfrac{1}{2}k\sin(n+1)\theta\,\mathrm{J}_{n+1}(kr). \quad . \quad (34)$$

These forms show directly that $d\mathrm{R}/dx$, $d\mathrm{R}/dy$ satisfy the fundamental equation (2). They apply when n is equal to unity or any greater integer. When $n=0$, we have

$$\mathrm{R}=\mathrm{J}_0(kr), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (35)$$

$$\frac{d\mathrm{R}}{dx}=-k\,\mathrm{J}_1(kr)\cos\theta, \quad \frac{d\mathrm{R}}{dy}=-k\,\mathrm{J}_1(kr)\sin\theta. \quad . \quad (36)$$

The expressions for the electromotive intensity are somewhat simpler when the resolution is circumferential and radial:

$$\text{circumf. component }=\mathrm{Q}\cos\theta-\mathrm{P}\sin\theta=\frac{im}{k^2}\frac{d\mathrm{R}}{r\,d\theta}$$
$$=-\frac{imn}{k^2 r}\mathrm{J}_n(kr)\sin n\theta, \quad . \quad . \quad (37)$$

$$\text{radial component }=\mathrm{P}\cos\theta+\mathrm{Q}\sin\theta=\frac{im}{k^2}\frac{d\mathrm{R}}{dr}$$
$$=\frac{im}{k}\mathrm{J}_n'(kr)\cos n\theta. \quad . \quad . \quad . \quad (38)$$

If $n=0$, the circumferential component vanishes.

Also for the magnetization

$$\text{circ. comp. of mag. }=b\cos\theta-a\sin\theta=\frac{m^2+k^2}{ipk^2}\frac{d\mathrm{R}}{dr}$$
$$=\frac{m^2+k^2}{ipk}\mathrm{J}_n'(kr)\cos n\theta, \quad . \quad . \quad (39)$$

$$\text{rad. comp. of mag. }=a\cos\theta+b\sin\theta=-\frac{m^2+k^2}{ipk^2}\frac{d\mathrm{R}}{r\,d\theta}$$
$$=\frac{n(m^2+k^2)}{ip\,k^2 r}\mathrm{J}_n(kr)\sin n\theta. \quad . \quad . \quad (40)$$

The smallest value of k for vibrations of this class belongs to the series $n=0$, and is such that $kr=2{\cdot}404$, r being the radius of the cylinder.

For the vibrations of the second class $\mathrm{R}=0$, and by (21),

$$c=\mathrm{J}_n(kr)\cos n\theta, \quad . \quad . \quad . \quad . \quad . \quad . \quad (41)$$

k being subject to the boundary condition

$$\mathrm{J}_n'(kr')=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (42)$$

As in (33), (34),

$$\frac{dc}{dx}=\frac{dc}{dr}\cos\theta-\frac{dc}{r\,d\theta}\sin\theta=\tfrac{1}{2}k\cos(n-1)\theta\,\mathrm{J}_{n-1}(kr)$$
$$-\tfrac{1}{2}k\cos(n+1)\theta\,\mathrm{J}_{n+1}(kr),\quad . \quad . \quad (43)$$

$$\frac{dc}{dy}=\frac{dc}{dr}\sin\theta+\frac{dc}{r\,d\theta}\cos\theta=-\tfrac{1}{2}k\sin(n-1)\theta\,\mathrm{J}_{n-1}(kr)$$
$$-\tfrac{1}{2}k\sin(n+1)\theta\,\mathrm{J}_{n+1}(kr),\quad . \quad . \quad (44)$$

so that by (19), (20) all the functions are readily expressed.

When $n=0$, we have

$$\frac{dc}{dx}=-k\mathrm{J}_1(kr)\cos\theta,\quad \frac{dc}{dy}=-k\mathrm{J}_1(kr)\sin\theta.\quad . \quad . \quad (45)$$

For the circumferential and radial components of magnetization we get

$$\text{circ. comp. of mag.}=b\cos\theta-a\sin\theta=\frac{im}{k^2}\frac{dc}{r\,d\theta}$$
$$=-\frac{imn}{k^2r}\mathrm{J}_n(kr)\sin n\theta,\quad . \quad . \quad (46)$$

$$\text{rad. comp. of mag.}=a\cos\theta+b\sin\theta=\frac{im}{k^2}\frac{dc}{dr}$$
$$=\frac{im}{k}\mathrm{J}_n'(kr)\cos n\theta,\quad . \quad . \quad (47)$$

corresponding to (37), (38) for vibrations of the first class.

In like manner equations analogous to (39), (40) now give the components of electromotive intensity. Thus

$$\text{circ. comp.}=\mathrm{Q}\cos\theta-\mathrm{P}\sin\theta=\frac{ip}{k^2}\frac{dc}{dr}=\frac{ip}{k}\mathrm{J}_n'(kr)\cos n\theta,\quad (48)$$

$$\text{rad. comp.}=\mathrm{P}\cos\theta+\mathrm{Q}\sin\theta=-\frac{ip}{k^2}\frac{dc}{r\,d\theta}=\frac{ipn}{k^2r}\mathrm{J}_n(kr)\sin n\theta.\quad . \quad . \quad . \quad . \quad (49)$$

The smallest value of k admissible for vibrations of the second class is of the series belonging to $n=1$, and is such that $kr'=1{\cdot}841$, a smaller value than is admissible for any vibration of the first class. Accordingly no real wave of any kind can be propagated along the cylinder for which p/V is less than $1{\cdot}841/r'$, where r' denotes the radius. The transition case is the two-dimensional vibration for which

$$c=e^{ipt}\mathrm{J}_1(1{\cdot}841\,r/r')\cos\theta,\quad . \quad . \quad . \quad . \quad . \quad (50)$$

$$p=1{\cdot}841\,\mathrm{V}/r'.\quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (51)$$

XIX. *Researches on Photographic Action inside Discharge Tubes.* *By* ANGELO BATTELLI*.

THE great similarity that exists between the action of the *x*-rays and the action that Herr Lenard, of Hungary, obtained with tubes having one end closed with aluminium, induces us to suppose that the kathodic rays may directly produce within the tube both photographic action and electric dispersion.

To demonstrate the photographic action a cylindrical tube was used, having sealed to it near the middle a wide glass stem (C). Through the said glass stem was introduced into the tube a photographic film, surrounded by black paper, and placed on a metallic cylinder. Four different designs of wire were fixed longitudinally on the roll of black paper, upon four lines placed at a distance of 90° the one from the other, so that one design faced the kathode or negative pole, another the anode or positive pole, and the other two designs the lateral walls of the tube.

In all the experiments made a very strong impression occurred on the face turned to the kathode, and a very slight one on the other faces of the film.

We might, however, explain the fact by assuming that the kathodic rays striking on the sheet of black paper made it apt to emit the *x*-rays with greater efficiency than the end of the tube, which is more distant from the kathode and the photographic film than the black paper.

To meet this objection I tried, first of all, placing two similar sensitive cylinders the one inside the tube, the other outside it, in order to have the two anterior faces of the films in very nearly the same condition, the first in relation to the end of the tube, the second in relation to the black paper; that is, I placed two of the cylinders before-mentioned in connexion with the end of the tube exposed to the kathodic rays, but the one inside it and the other outside.

It resulted that the face of the film turned to the kathode remained strongly acted on after a very short exposure, while the exterior film showed a very feeble impression.

To render the condition of the inner film still more similar to that of the exterior one, I constructed a double tube divided by the means of a glass partition into two longitudinal compartments. The first compartment contained the anode and kathode, and on the two faces of the glass partition were

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* Communicated by the Author

set the two cylinders surrounded by black paper, the one in the first compartment the other in the second respectively. The two compartments were meanwhile exhausted by the same pump. The results were the same as in the preceding experiments. But to place beyond any doubt that the photographic action was due to the paper struck by the kathodic rays, I fixed on the paper surrounding the cylinders some glass threads of various thickness and quality.

As glass is known to emit the *x*-rays as well as paper when it is struck by the kathode-rays, it follows that the said threads should appear as lines of greater impression on the photographic plate.

On the contrary, in all the experiments I have conducted I always obtained on the film well-marked shadows of the glass threads. They are little less perceptible than those of iron or copper wires.

Thin glass plates also, about 1/10 of a millimetre thick, cast their shadow on the photographic film.

There remains still a remarkable difference observed by Röntgen between the *x*-rays and the kathodic rays, namely, that the *x*-rays are not deflected by the magnet, while the kathodic rays are.

Also on this point I have had recourse to experiment.

I constructed a tube of a spherical form in which the kathode and anode were placed within two tubes situated along the same diameter of the sphere. A cylinder surrounded by a photographic film, protected by black paper, was introduced into the sphere through the stem and suspended there within the upper hemisphere. A strong electromagnet was fixed on the sides before the lower hemisphere, so that the kathodic rays were strongly deflected. From what the eye could perceive they touched slightly the inferior brim of the sensitive cylinder.

The different experiments conducted all gave a strong action on the film on the face turned towards the kathode, and a less strong one on the face turned to the fluorescent disk struck by the kathodic rays, and no action at all on the rest of the surface.

We must then conclude that the rays which inside the tube are capable of photographic action are not (at least a part of them) deflected by the magnet.

Then I introduced into the spherical tube a sensitive cylinder which occupied the whole height of the sphere, and applied the magnet to the same place as before. The films when developed showed that the photographic action was much more intense in the region to which the kathodic rays had been attracted. Hence, amongst the rays deflected by the magnet there are some endowed with photographic action.

In addition, I tried to investigate how the photographic action inside the tube varies with the variation of the rarefaction.

The result of the experiments proved that such action takes place even when all the conditions that we judge to be ordinarily connected with the production of the kathodic rays are not satisfied.

Beginning with the internal pressure of 3/10 of a millimetre, at which the tube is filled with a white-violet light that ends at the two electrodes, where from time to time little sparks are visible, the photographic action increases at first rapidly with the rarefaction, and then, from 1/100 of a millimetre onwards, much more slowly.

Finally, to eliminate the doubt that the photographic action inside the tube was due to an electric effluvium between the paper and the metallic cylinder*, I surrounded the latter with a thimble of brass-wire gauze connected to a copper wire dipping into the mercury of the pump, which, in its turn, was put in connexion with the ground.

All the films showed on development a darkening of the same order of intensity as that which was produced without the metallic screen, while the shadow of the whole gauze remained well delineated on the film itself.

While these experiments point to facts noteworthy in themselves, they lead us to conclude that *Röntgen's rays are already existing inside the tube as well as those studied by Lenard. The ones would pass better than the others through the walls. Hence they alone would pass out if the tube were of a somewhat thick glass.*

XX. *The Multiple Spectra of Gases.*

By John Trowbridge *and* Theodore Wm. Richards †.

In a recent paper upon the spectra of argon ‡ we have shown that the two different spectra of this gas are dependent primarily upon the electrical conditions which cause the gas to glow. The continuous discharge of a high-tension accumulator through the gas produces the red spectrum, while the discharge of a condenser, provided that its oscillations are

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* This was already sufficiently contradicted by the fact that the impression came out enormously stronger on the face turned towards the kathode.

† Communicated by the Authors.

‡ *Suprà*, p. 77.

not damped by the resistance of the tube, or other resistance or impedance, produces the blue spectrum.

It became now a matter of great interest to determine whether the same conclusions apply to other gases, a subject which has already been studied in detail by Wüllner and others. The chief differences between our work and the earlier investigations are: first, the use of a high-tension accumulator instead of an electrical machine or Ruhmkorff coil as the source of electricity; and secondly, the introduction of varying ohmic resistance or impedance between the plates of the condenser in order to study the damping of the spark. It is the object of this paper to emphasize anew the importance of the electrical conditions of the circuit, and to call attention once more to the fact that the behaviour of most elementary gases is in every respect similar to that of argon.

With regard to the spectrum of nitrogen, it has been known for a long time that two spectra could be obtained by means of appropriate changes in the density of the gas, as well as by the introduction of the condenser; but not all investigators have put the same interpretation upon their results. So varying are the views that Ångström * and Thalén † believed the familiar channelled spectrum to be due to impurities in the gas. Plücker and Hittorf ‡, Wüllner § and Salet ‖ have proved this view to be false, but they had not at hand the constant current of high tension at our disposal, and their nitrogen was obtained from air containing argon, so that a revision of their work promised to be of great interest.

With our Planté battery of ten thousand volts we have obtained the usual two different spectra of nitrogen by varying suitably the electrical conditions of the discharge. By means of a continuous discharge with no spark-gap or brush-discharge in the circuit through nitrogen under varying pressure we always obtained the channelled spectrum. The glow in the capillary tube, as well as the positive and negative light, was of a delicate pink colour under these conditions—a colour not unlike the red glow of argon. When an air-gap, over which the battery discharges in a brush, is introduced into this circuit, the glow becomes more violet in tinge, and

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* Pogg. *Annalen,* cxliv. p. 300.

† *Bull. Soc. Chim.* [2] xxv. p. 183.

‡ Roy. Soc. Proc. xiii. p. 153; Phil. Mag. [4] xxviii. p. 64.

§ Pogg. *Ann.* cxxxv. p. 497, cxxxvii. p. 337, cxlvii. p. 321, cxlix. p. 103, cliv. p. 149.

‖ *Ann. Chim. Phys.* [4] xxviii. p. 52. Hasselberg, Ames, and others have also studied the nitrogen spectra.

the spectroscope shows that the red bands are relatively much lower in intensity than before. By increasing the size of this air-gap to its utmost limit, the red bands almost, if not wholly, disappear, while the blue and green ones retain their positions. Under these conditions the capillary tube is filled with a pure blue glow, less intense and vivid than that of argon, however.

When the condenser is introduced, the whole appearance of the tube is utterly transformed. The blue colour of the tube at once changes to a rich bluish green, and the channelled spectrum gives place to bright lines, already well known and mapped. This line spectrum corresponds to the blue spectrum of argon. When the oscillations of the condenser-discharge are damped by means of a suitable resistance or self-induction interposed between the condenser plates, a channelled spectrum reappears; but in this case the glow in the tube is of a bluish-white colour, the positive and negative lights being of a bright yellow. Whether or not this channelled spectrum is, as it seems, exactly like the one obtained by means of a continuous discharge, photographic measurement will show. This last appearance is probably the usual one obtained by means of the Ruhmkorff coil, for then the oscillations induced by the primary condenser are damped by the impedance of the secondary coil and the resistance of the tube.

The spectrum of hydrogen is usually supposed to consist of four bright lines:—H α 6563·0 (C), H β 4861·5 (F), H γ 4340·7 (G), H δ 4101·9 (H), as well as several in the extreme violet and ultra-violet[*].

Other spectra have been observed also; but owing to the partial understanding of the conditions required to produce them, the voluminous literature[†] upon the subject leaves a confused idea in the mind of the reader. The continuous discharge of a high tension accumulator through hydrogen gas at tensions varying from 0·05 mm. to 3 mm. and more yields a beautiful white glow in the capillary of a Geissler tube, while the strata in the positive and negative light are often alternately pale pink and pale blue. When examined by a spectroscope with a broad slit, the light from this discharge appears to consist of bands similar to that of nitrogen, as well as of bright lines; but when the slit is narrowed every band is resolved into a multitude of sharp lines of varying

[*] Ames, Phil. Mag. [5] xxx. p. 48 (1890).

[†] Ångström, Vogel, Lockyer, Fievez, Wiedemann, Higgins, Wüllner, Hasselberg, Balmer, Grunwald, Villari, Schuster, Salet, Smyth, and others. For references see O. Dammer, *Anorgan. Chem.* i. p. 369.

intensities*, among which the four usual hydrogen lines, although present, are by no means specially prominent. A large capacity is required to change this spectrum into the familiar four-line spectrum which is comparable with the blue spectrum of argon. The change is marked by a sharp alteration in the colour of the glow from white to a deep red. In the process, the bluish-green line (Hβ), as well as the two in the violet, which retain their early position unaltered, becomes nebulous at its edges †; while the red H-line remains sharp and clear. The most marked change in the spectrum, however, is the complete obliteration of all the host of other lines covering the whole spectrum; and the obvious contrast between the oscillatory and non-oscillatory spectra of this gas is quite as striking as in the case of nitrogen, although somewhat different in nature. This four-line deep red glow appears satisfactorily in a tension of gas of about a millimetre—when the tension of the gas is much higher or lower the resistance is increased, the oscillations are damped, and other lines begin to appear. Curiously enough, however, the damping of the oscillatory discharge does not at first replace all the lines which were extinguished by the introduction of the condenser. At first only a sharp line in the yellow and one in the green begin to appear, and gradually others are added as the impedance is increased.

The relation of these conclusions to the varying spectra of hydrogen observed in stars leads to interesting speculations regarding the nature of the electrical and thermal conditions in the photospheres of these bodies ‡.

Each of the halogens gives two spectra, one with, and one without the condenser. With iodine, if any of the solid itself is present in the tube, the vapour-tension is so soon altered by the heat of the discharge that the oscillatory discharge is damped and the non-oscillatory substituted. Hence the former can be obtained only for a few moments.

A tube of helium made by Professor Ramsay, the kind gift of the Hon. R. J. Strutt, gave a brilliant yellow glow under the influence of the continuous discharge, and a brilliant blue with the condenser discharge; but since the bright helium lines remained in each, and every other important line in the blue spectrum proved to be an argon line, it is evident that the oscillations produced no considerable effect upon the helium.

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* Smyth, Pogg. *Ann.*, *Beiblätter* [2] vii. p. 286. Wüllner observed this spectrum but did not measure the lines.

† E. Villari, Fievez, and Salet.

‡ E. Ebert, Wied. *Ann.* liii. 1894.

As Crookes and others have already pointed out, since many gases yield different spectra under the influence of varying electrical conditions, it is evident that the fact of the existence of two well-marked spectra of argon gives not the slightest presumption in favour of the hypothesis that the new gas is a mixture. In order to discover whether argon possesses a dual nature, the gas must be split up in such a way that its components give different spectra under like electrical conditions; then alone would the evidence of the spectroscope be of weight in proving the dissimilarity of the several parts.

The results of this work are thus far only those which were to have been expected from a high tension galvanic battery, reasoning from the work of other investigators with the Toepler-Holtz machine. The battery, however, gives a current so admirably constant and so easily regulated as to its tension, that we hope to be able to use it as a means of determining whether the oscillatory discharge produces its effect simply by increasing the temperature, or because of some inherent property in the manner of the discharge.

It is our intention to extend the investigation by the systematic photographic study of the action of the varying discharge upon all the elementary gases in the purest condition, as well as upon mixtures, under widely varying conditions of temperature and pressure.

Harvard University, Cambridge, Mass. U.S.

XXI. *On the Generality of a New Theorem.*

To the Editors of the Philosophical Magazine.

Gentlemen,

A CORRESPONDENT writing to me on the 14th of last December pointed out to me that the first of my propositions in the paper on "Microscopic Vision" in the October number of the Philosophical Magazine, p. 335, viz.:—

> However complex the contents of the objective field, and whether it or parts of it be self-luminous or illuminated in any way however special, the light which emanates from it may be resolved into undulations each of which consists of uniform plane waves

(in which mention is made of only one kind of motion, viz. light), may be further generalized into the following :—

> "The *most* general motion of given period within any space may be analysed into trains of" uniform "plane waves."

In this enunciation the words "of given period" are unnecessary and may be omitted.

I had this generalization in view when writing the paper, and it was it that led me to state on p. 335 that my theorem is in ultimate analysis an extension of Fourier's theorem. In fact to show this, it only needs to enunciate the new theorem in its symbolical form, when it becomes

$$\mathrm{F}(x, y, z, t) = \Sigma \mathrm{A} \sin\left(2\pi \frac{lx + my + nz - vt}{\lambda} + \alpha\right),$$

where the A's are directed quantities; and where, for each periodic time λ/v, there will in general be three values of A and α in each direction lmn. These belong to the normal and two transverse waves, or to whatever correspond to them. This equation may be written

$$\mathrm{F}(x, y, z, t) = \Sigma \mathrm{A} \sin\left(2\pi \frac{p - vt}{\lambda} + \alpha\right),$$

where p is the perpendicular distance from the origin to the plane whose director cosines are lmn and which passes through the point xyz.

The analogy of the theorem when in this form with Fourier's expansion for a string vibrating in one plane, viz.

$$\mathrm{F}(x, t) = \Sigma\left[\mathrm{A}_n \sin\left(2n\pi \frac{x - vt}{l} + \alpha_n\right) + \mathrm{B}_n \sin\left(2n\pi \frac{x + vt}{l} + \beta_n\right)\right],$$

is at once apparent, and becomes still clearer when we remember that the two terms in the Fourier's expansion for each value of n correspond to the two opposite directions of a line, the only possible directions in dealing with a line. These are of course only particular cases of the directions designated by the director cosines lmn of the new theorem, so that the new theorem may legitimately be described as an extension of Fourier's theorem.

It may further be remarked that the new theorem has somewhat similar relations with expansions by Spherical Harmonics, by Bessel's Functions, *et hoc genus omne.* This is to be expected; for just as Fourier's Theorem enables us to analyse the displacements of a line, and as Spherical Harmonics &c. deal with displacements of certain surfaces, so does the new theorem deal in a similar manner with displacements or motions pervading a space.

In one respect the new theorem is less perfect than its predecessors, Fourier's Theorem, &c., since as yet the constants of the new theorem have not been evaluated in the form of definite integrals, or rather of expressions each of which may involve several definite integrals.

However, to treat these matters adequately an author must enter into mathematical details, and as I was writing a paper which I hoped would be read by microscopists as well as physicists, I thought it best to avoid every mathematical detail and to exclude every abstract consideration, the discussion of which could be kept out of the paper without impairing it for the special object I had in view, viz. the explanation of microscopic vision. It was on this account (that is, to make my paper intelligible to a wider circle) that the theorem is in my memoir enunciated in the most limited form that would serve the immediate purpose of interpreting microscopic vision, and also that I had to content myself with a bare statement of the relation between it and Fourier's theorem.

The proof, however, which is given in my paper completely proves the theorem in its fuller form. It does not require to be modified in any respect except that the term light must be understood in the generalized sense of any kind of wave-motion.

Here, and in my correspondent's enunciation of the theorem, the word motion must be understood in the generalized sense employed in my paper on "The Kinetic Theory of Gas regarded as illustrating Nature," which will be found in the Phil. Mag. for October 1895, p. 336. It therefore includes any event that may be propagated through the medium in waves, whether motions proper, alternations of electromagnetic or dynamic stresses, or any other, and may therefore be represented mathematically by any function of the coordinates and time.

It may be useful to state here the limitations within which the theorem is true. These are :—

1. That the medium be uniform. It may, however, be either monotropic or crystalline, see No. 4 below.

2. That the motions (using the word motion in its generalized sense) be such that the simple geometrical superposition at each point within the medium of the motions reaching that point is legitimate : as is, for example, the case with dynamical motions of small amplitude.

3. That the whole energy be employed in the wave propagation. This third condition excludes such a case as that of sound in air (in which the theorem is, however, approximately true *); because in air the compressions produce heat,

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* There may be an escape of energy as referred to in No. 3, provided it do not cause the values of the velocities a, b, &c., of No. 4 to include

and the heat involves the diversion of part of the energy into the forms of radiation, convection, &c., in fact into other forms than wave propagation.

4. The fourth condition is that the wave surface propagated from each physical centre within the medium be one which can be represented by an equation of which the parameters are linear quantities of the form at, bt, &c., where a, b, &c., are constant velocities. This obviously includes the wave-surface with two or three sheets in crystals, as well as the spherical wave-surfaces of monotropic media. It also includes such waves as are thrown off by an advancing missile, whether in a monotropic or crystalline medium—a kind of wave which probably exists in the æther: and innumerable others. In fact, the theorem is one of perfect generality, and *makes it possible physically to resolve any motion however complex which pervades a given space into trains of absolutely uniform plane waves*, provided the four conditions enumerated above are fulfilled. The second and third of these conditions are equivalent to saying that the displacements must be such as are not affected by the second order of differential coefficients, and that in general external forces must not intervene. These conditions are essential where the motion is a real physical one, and where the resolution is intended to be into real physical waves. But if the problem be merely kinematical they need not be attended to. This corresponds to the similar statement that has to be made in regard to applications of Fourier's theorem, &c.

I am, Gentlemen,
Yours very faithfully.
G. Johnstone Stoney.

8 Upper Hornsey Rise,
London, N.
1897, January 16.

XXII. *Thermal Transpiration and Radiometer Motion.*

By Osborne Reynolds*.

In Part I. of a paper, contributed to the Phil. Mag. for November 1896, by Mr. W. Sutherland, there are references (pp. 373, 374) to my paper "On certain Dimensional Properties of Matter in the Gaseous State" (Phil. Trans. R.S. part ii. 1879).

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t, and provided it do not disturb the *uniformity* of the plane waves. These requirements are approximately fulfilled in air, so that the theorem applies approximately to sound waves in air, which means that the plane waves into which sound waves in air may be resolved are approximately uniform.

* Communicated by the Author.

After most flattering expressions respecting the importance and the effect of this paper, followed by severe comments on its mathematical form, Mr. Sutherland says:—

"But what appears to me to be the fatal objection to Reynolds's mathematical method, is that it takes the mind away from definite physical concepts of the actual operation of the causes of thermal transpiration and radiometer motion; and the object of the present paper is to construct a theory of these that will fall into line with the current kinetic theory of gases and keep the physics of the phenomena to the fore."

After this Mr. Sutherland proceeds to describe the foundation on which he has reconstructed the theory.

From his references to my paper, together with the somewhat confusing and sketchy preamble to the definite expression of his foundation, it may well appear that, except for the mathematical form in which it is expressed, this foundation is really the same as that set forth in my paper. It seems therefore to me to be important to point out that there is not the least connexion between these foundations.

According to my showing the sole cause of thermal transpiration is the lateral action of those bounding surfaces which are parallel to the direction in which heat is being conducted; while Mr. Sutherland expressly excludes the action of these walls (or the walls themselves) from any part in thermal transpiration, which he rests solely on the action of direct conduction of heat.

It may seem that the results obtained in the application of theories so differently founded to any particular case must be different; but this depends on the object sought.

If, as in my paper, the object is to determine the nature of the action, which results in transpiration, from the kinetic theory of gases the results from the different theories will be different; but if, as is Mr. Sutherland's object, it is merely required to express the results of transpiration "in line with the current kinetic theory of gases," then, if the transpiration he has assumed is the same as that obtained by the true theory, the results will to a certain extent correspond.

There is thus danger of the similarity of the results so obtained being taken as a verification of the soundness of the foundation on which Mr. Sutherland has built his theory, and of confusion being added to the inherent difficulties of the subject.

It has therefore seemed to me to be important not only to point out the difference of the foundations, but also to show clearly that the foundation on which Mr. Sutherland has constructed his theory has no real existence whatever; being the result of a somewhat common and, certainly, historical error in mechanical analysis.

In order to make this clear I will recur somewhat to the history of the kinetic theory of gases from 1860. In that year the Phil. Mag. contained a paper by Maxwell in two parts, the first containing his law of distribution, the law of logarithmic-decrement of inequalities in distribution for rigid particles, and the theory of viscosity, and the second part containing the theories of diffusion and the conduction of heat.

After this there appeared (Pogg. *Ann.* 1862) a paper by Clausius "On the Conduction of Heat by Gases." This was avowedly in answer to a challenge by M. Jochmann, but in reality for the purpose of correcting Maxwell's somewhat ill-considered theory of conduction and replacing it by another on a sound foundation.

In the first paragraph of the second part of his paper Maxwell says:—

"We have shown, in the first part of this paper, that the motions of a system of many small elastic particles are of two kinds: one, a general motion of translation of the whole system, which may be called the motion in mass; and the other a motion of agitation, or molecular motion, in virtue of which velocities in all directions are distributed among the particles according to a certain law. In the cases we are considering, the collisions are so frequent that the law of distribution of the molecular velocities, if disturbed in any way, will be reestablished in an inappreciably short time; so that the motion will always consist of this definite motion of agitation, combined with the general motion of translation."

According to the assumption in the second sentence quoted, if the gas were in steady condition and at rest, no matter what were the space variations of condition, the molecules would leave a spherical element, taken about a point P, after having undergone collision within it, with the same mean characteristics in all directions as if the gas about P had been uniform. And in this case the molecules would reach a plane with mean characteristics as from uniform gas with the mean characteristics of the gas at the points at which they last underwent collision. Maxwell seems to have made an error in his analysis which somewhat obscured the results which should follow from this assumption, otherwise it would have appeared to him (in equation 28) that according to the assumption gas could not be at rest and at the same time conducting heat.

Clausius carefully points out these facts both in a note to art. (6) in his paper and in a note to art. (24).

The fact that a gas or any body can be at rest, and at the same time be transmitting energy, presented a paradox which caused the property of conduction to be the last of the general properties of matter to receive kinetic explanation. This was not for want of perception of the latent motions of the molecules, but for want of a recognition of the only characteristic in such latent motion, or any motion of agitation, which could cause such transmission—the failure to realize that the only means by which matter can transmit energy while in a mean state of rest is that in which the latent motion considered with respect to any plane consists of two streams of matter crossing the plane from opposite sides, the velocity of the one stream being greater than that of the other, and the ratio of the densities being inversely as their velocities.

Clausius seems to have first recognized this. After reading Maxwell's paper and pointing out his errors, he frames a theory of conduction, the same as Maxwell's in so far as the energy carried across the plane by the molecules is taken to represent the heat conducted, but differing in so far as he recognizes the necessity for a difference in the velocity of the opposite streams, such that their velocities shall be inversely proportional to the densities. And taking into account the principle that the mean velocities of molecules after encounter are the same as before, he infers that the characteristic velocities of the two opposite groups of molecules, leaving an element in the direction in which the temperature varies, are such as they would have, if the gas were uniform, plus a velocity in the direction of the fall in temperature depending on the slope of temperature and the mean path of the molecules.

From this start Clausius develops the theory of conduction for gas at rest, using the condition of rest to define the velocity common to the two groups, through a layer of gas of any definite thickness, but only in the case of indefinite lateral extension. Thus he only studies the action in one dimension, and he nowhere refers to the existence of any solid surfaces.

Maxwell followed with his classical paper of 1866*, in which he acknowledges Clausius's corrections, and then obtains the equation of continuity, the equations of motion and of energy, and also the equation for the state of conduction of energy, for gas in three dimensions in terms of the logarithmic-decrements of the inequalities, having obtained the constants for these rates for a particular law of force between the molecules.

These equations are applicable to any varying condition of gas provided there is no discontinuity, but they afford no

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* Phil. Trans. Roy. Soc. 1867, p. 49.

means of taking account of the action of a solid surface on the gas to which it is adjacent.

When the idea of thermal transpiration first presented itself I was acquainted with Maxwell's paper, and through this knew of Clausius's work; but I had not read his paper and knew nothing of his method, so that I was not aware that he had already introduced expressions for the inequalities of the opposite groups of molecules in form of a series of ascending powers of the mean path of the molecules, the coefficients being the differential coefficients of the varying characteristics, in the case of varying temperature.

But having formed my conception of the mechanical actions, from which I inferred the existence of the property of thermal transpiration as the consequence of a definitely dimensioned structure when the condition of the gas varied in more than one dimension in space, my object being the study of the dimensional properties which can only be studied in three dimensions in space, I adopted a form of equation which, while perfectly general, admitted of the expression of the inequalities resulting from the variation of any characteristic whatsoever of the gas for any group of molecules distinguished by direction, in series of ascending powers of the mean-range (or parameter of the dimensions of structure) in three dimensions in space.

This mathematical system admits of the consideration of such discontinuity as results when the gas is bounded by a solid surface. Since at points in the gas of which the distances from the surface are small compared with the normal mean-range, the range of any group leaving the surface is limited by the distance of the point from the surface.

The action on which thermal transpiration depends when gas at constant pressure but conducting heat is bounded by a solid surface parallel to the direction of conduction, say x, is the sum of the mean components of momentum in the direction x of the two groups of molecules distinguished by the signs of their component velocities parallel to x, which are brought up to the surface by their component velocities perpendicular to the surface, say in direction z. If the component momenta of these groups are equal and opposite, then the condition of the gas at constant pressure will be steady; but if they are not equal and opposite, and if the corresponding groups leaving the surface have equal and opposite momenta in direction x of the surface, in consequence of the action there will be a tangential force between the surface and the gas. And it thus appears that, although the prime cause of thermal transpiration is that relation between the rates of variation of molecular velocities and density which causes conduction, there are also involved the action of the lateral components of motion in bringing the molecules up to the surface, and the action of the surface in equalizing the opposite momenta of the opposite groups.

Bearing these incidents and steps in the history of the kinetic theory in mind the precise errors into which Mr. Sutherland has fallen become evident. In the paragraph beginning in middle of p. 375 he begins: "In the kinetic theory the molecules which are considered characteristic of an element are those which have experienced collision in it."

Then he gives reasons for thinking that the walls of the tube produce no effect on the characteristics of the molecules which rebound from them. And thus having cut away the only cause of thermal transpiration he proceeds:—

"Thus, then, if we do not have to take account of reflexion from the walls of the tube, we can consider the gas in it as part of an indefinite mass such that the temperature throughout a plane perpendicular to the axis is the same as that in the section of the tube made by the plane. We wish to find the number of molecules crossing any section of the tube. This is done by Clausius in his theory of conduction in gases, and with greater refinements of accuracy by Tait; but for the sake of clearness we will make the calculation here to a degree of accuracy suitable for present requirements."

It is to be noticed here that he has not stated whether or not the infinite mass of gas is at rest or in motion with respect to the plane; but in the next paragraph he proceeds to consider the quantity of matter carried across the plane by each of the opposite groups of molecules, having respectively the mean characteristics of uniform gas at rest in the mean condition of the actual gas at a distance from the plane on either side equal to the half of the mean component of the free path—thus defining the gas as at rest; and at the same time showing that he has in no way realized the point of Clausius's paper, which he quotes—showing, in fact, that he cannot even have read it. Having thus fallen into the error from which Clausius released Maxwell, the inevitable result is that he arrives at the absurd conclusion that in a gas at rest more molecules cross the fixed plane from the cold side than from the hot side. But instead of recognizing the absurdity of this conclusion, he says—"which amounts to the same thing as if the gas had a velocity," and then proceeds to make this unfounded and self-induced velocity the foundation of thermal transpiration, introducing a hypothetical traction between the gas and the solid surface, the effect of which he had previously shown to be nothing.

As shown (art. 88) in my paper, the velocity thus erroniously attributed as an inherent property to gas conducting heat is exactly the velocity which would result from the action of the walls of the tube if the motion of the gas were unrestricted, and hence the danger that the error in Mr. Sutherland's theory might pass unnoticed.

That in adopting this erroneous foundation Mr. Sutherland is not quite satisfied is shown in the last paragraph, p. 374. This begins:—"The most convenient starting-point is the laws discovered by Clausius (Pogg. *Ann.* cxv. 1862) for the conduction of heat in gases. In a vertical cylinder of gas, bounded by a solid wall impermeable to heat and two conducting plane ends, the lower at a temperature θ_1 the higher at the temperature θ_2, when the flow of heat has become steady and the pressure throughout the cylinder is contant, &c." Clausius expressly excludes solid walls.

Then, again, in the next sentence:—"Now in the establishment of the law of temperature it was shown by Clausius that in a mass of gas which is not uniform in temperature there is motion of the gas in the direction of variability; but it is assumed (as can easily be proved) that under ordinary circumstances this motion can never produce an appreciable departure from uniformity of pressure, &c."

This is a complete misstatement. Clausius showed that the velocity which would otherwise arise from the excess of the rate of variation of density over the opposite rate of variation of the velocity of the molecules is balanced by the difference in the opposite velocities of the molecules emerging from the hot and cold sides of an element of the varying gas respectively, the excess being in the direction of the colder side; but he never mentions a velocity of the gas except as zero. And Mr. Sutherland does not notice that the velocity resulting from the difference just mentioned is towards the colder side, and is opposite to that which he has found, which is towards the hotter side.

In pointing out these errors, although my chief object has been to prevent unnecessary confusion in this abstruse subject, it has also been done with a view to assist Mr. Sutherland in his effort (with which I sympathise). And I hope he will now see that the difficulties of the subject do not exist in mathematical form, but in the conception of the *mechanical* actions involved; and after mastering these, that he may be able to produce a translation from the general analysis into a group of particular examples treated in one dimension. For I have no doubt that the great difficulty which many find in this subject arises from unfamiliarity with the expressions in the analysis in three dimensions—a difficulty which is not likely to be removed.

XXIII. *Notices respecting New Books.*

Die elektrodynamischen Grundgesetze und das eigentliche Elementargesetz. By Franz Kerntler. Buda-Pesth: Lloyd-Gesellschaft, 1897.

THIS work is a pamphlet of nearly 70 pages, devoted to the discussion of expressions for the values of the forces exerted by an element of current on a second element situated at a distance from it. While investigating experimentally the laws of the mutual action of closed circuits, Ampère was led to assume that the mutual action of two current elements is a single force acting along the line joining them. This assumption has been greatly criticised; according to it no force is exerted between two elements at right angles to each other in the same plane, and the possibility of the mutual action giving rise to couples is not considered. Stefan generalized the theory by including forces between elements at right angles, and Korteweg introduced the consideration of couples, but in each case the expressions contain more constants than can be determined by experiment.

The author poses as a strong advocate for Stefan's theory, and points out that, in Stefan's equations, two of the constants which have hitherto been supposed different are necessarily equal; this, however, still leaves the constants indeterminate. No mention is made of Korteweg's work, nor does the author anywhere contemplate the possibility of couples. He is apparently aggrieved that the work of Stefan has received slight recognition, except in Germany, and especially because Maxwell makes no reference to it in his treatise. Such negligence is perhaps sufficiently atoned for, as far as England is concerned, by the exhaustive Report on Electrical Theories which Professor J. J. Thomson communicated to the British Association in 1885. Since that time the æther theory of electricity has received so much experimental confirmation that all others possess little more than historical interest. J. L. H.

Practical Electricity. A Laboratory and Lecture Course for First Year Students of Electrical Engineering. Vol. I. *By* W. E. Ayrton, *F.R.S., Assoc. M. Inst. C.E.* London: Cassell & Co., 1896.

In some respects Professor Ayrton's new work may be described as the second edition of a volume with the same title which he wrote ten years ago. A comparison of the two books will, however, immediately reveal the great amount of alteration and enlargement which has been rendered necessary by a decade of advance in electrical technology. The standard instruments of those days are now classified as early forms, electric power and energy are ranked in importance along with current, potential, and resistance, and—unfortunately for the first year student!—the subject is too large to be dealt with in a single volume. The text has been largely re-written, and the author tells us in his preface that three-fourths of the illustrations in the present volume are new.

As the treatise is intended for students of electrical engineering, everything which has no immediate practical bearing is excluded; this may account for the somewhat meagre treatment of electrostatics, unless perhaps the author intends to return to it when writing of capacity in the second volume. In explaining the laws of conduction, hydrodynamic analogies, some of them very ingenious, have been freely used, the student being properly cautioned against pushing them too far. A description of the various primary batteries is followed by an interesting calculation concerning the relative cost of electric energy, when supplied by primary batteries or by steam-engine and dynamo. After giving the batteries every advantage, by neglecting their resistance and local action and selling their waste products, the cheapest of them (Daniell's) costs elevenpence per Board of Trade unit, while a London generating station is able to produce more than six units for the same expenditure. In an appendix to the volume Professor Ayrton gives a short history of the absolute unit of resistance and of the Board of Trade electrical standards.

The book has been thoroughly revised, and appears likely to meet with a success equal to that of the first edition. J. L. H.

XXIV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 75.]

November 18th, 1896.—Dr. Henry Hicks, F.R.S., President, in the Chair.

THE following communications were read:—

1. 'On *Cycadeoidea gigantea*, a new Cycadean Stem from the Isle of Portland.' By A. C. Seward, Esq., M.A., F.G.S.

2. 'The Fauna of the Keisley Limestone.—Part II. Conclusion.' By F. R. C. Reed, Esq., M.A., F.G.S.

December 2nd.—Dr. Henry Hicks, F.R.S., President, in the Chair.

The following communications were read:—

1. 'Another possible Cause of the Glacial Epoch.' By Prof. Edward Hull, LL.D., F.R.S., F.G.S.

In the introductory portion of the paper the author gives an account of the submarine topography of the area east of North America, and summarizes Dr. J. W. Spencer's work upon a submerged Antillean continent; he then deals with the effects which would be produced upon the Gulf Stream by the uprising of this continent in the Glacial Period, and maintains that, as the current could not pass into the Gulf of Mexico (being debarred by a coast of high continental land), it would flow directly northwards into the North Atlantic, and thereby be deprived of about 10° (Fahr.) of heat: the effects of which may be practically illustrated by supposing the isothermal line of 32° to take the place of that of 42° in the northern hemisphere. He argues that the increased snowfall which would thus be caused over certain areas would tend to intensify the cold through all the adjoining tracts.

To the effects produced in this way must be added those due to the elevation of the land of Eastern North America and to an elevation of North-western Europe, which is supposed to have occurred at the end of Pliocene times. These elevations would intensify the glaciation caused by the difference of direction taken by the Gulf Stream.

2. 'On the Affinities of the Echinothuridæ, and on *Pedinothuria* and *Elikodiadema*, two new subgenera of Echinoidea.' By J. W. Gregory, D.Sc., F.G.S.

3. 'On *Echinocystis* and *Palæodiscus*, two Silurian Genera of Echinoidea.' By J. W. Gregory, D.Sc., F.G.S.

XXV. *Intelligence and Miscellaneous Articles.*

ON THE EXPLOSION OF THIN LAYERS OF EXPLOSIVE GASES. BY PROF. F. EMICH.

THE observation that the small electrical sparks produced by shaking mercury in glass vessels frequently do not ignite detonating gas, led the author to investigate the lengths of the shortest sparks which can ignite explosive gas mixtures under various conditions.

The electrodes between which the spark passed were so constructed that their distance apart at the moment of explosion was identical with the distance in which the explosion could just travel.

For pure detonating gas this thickness under normal conditions was found to be 0·22 millim. For other conditions experiments gave the following results :—

1. It is approximately inversely proportional to the pressure of the gas—that is, to the concentration.
2. It increases somewhat with increase of temperature.
3. If detonating gas is mixed with hydrogen, nitrogen, or car-

bonic acid it increases about in proportion to the partial pressure of the detonating gas.

4. If, on the contrary, detonating gas is mixed with oxygen, the length of the shortest spark producing ignition at first decreases and then increases. The minimum is attained with a mixture of equal volumes of hydrogen and oxygen.

The investigation is being continued.—*Wiener Berichte*, Dec. 17, 1896.

THE BRESSA PRIZE.

The Royal Academy of Sciences of Turin, in accordance with the last will and testament of Dr. Cesare Alessandro Bressa, and in conformity with the Programme published December 7th, 1876, announces that the term for competition for scientific works and discoveries made in the four previous years 1893–96, to which only Italian Authors and Inventors were entitled, was closed on December 31st, 1896.

The Academy now gives notice that from the 1st of January 1895 the new term for competition for the eleventh Bressa Prize has begun, to which, according to the testator's will, scientific men and inventors of all nations will be admitted. A prize shall therefore be awarded to the scientific Author or Inventor, whatever his nationality, who during the years 1895–98, "according to the judgment of the Royal Academy of Sciences of Turin, shall have made the most important and useful discovery, or published the most valuable work on physical and experimental Science, Natural History, Mathematics, Chemistry, Physiology and Pathology, as well as Geology, History, Geography and Statistics."

The term will be closed at the end of December 1898.

The sum fixed for the prize, deducting income tax, will be 9600 (nine thousand and six hundred) francs.

Competitions must be sent in within the above-stated time, accompanied by a letter to the President of the Academy. Works must be in print; manuscripts are discarded. Unsuccessful competitive works are not returned.

None of the national members, resident or non-resident, of the Turin Academy can obtain the prize.

The Academy awards the prize to the most worthy scientific person, even if not entering into competition.

The President of the Academy,
G. Carle.

Turin, January 1, 1897.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
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[FIFTH SERIES.]

MARCH 1897.

XXVI. *The Genesis of Dalton's Atomic Theory.*

By H. E. Roscoe *and* Arthur Harden *.

THE criticism by Debus (Phil. Mag. 1896, xlii. p. 350) of the opinions expressed in our 'New View of the Origin of Dalton's Atomic Theory' demands some notice at our hands. The problem of the origin of the atomic theory is one not only of importance but of some complexity, dealing as it does with the gradual progress of thought in the author's mind, and depending for its solution to a great extent on indirect and fragmentary evidence.

In the first place Debus regards the whole question from a point of view which differs from ours. After showing that Dalton in his earliest papers applied the idea of atoms to explain the physical properties of gases, Debus adds (p. 352):—"In 1803 he discovered a method how to determine the relative weight of atoms, and added to the atomic philosophy a series of principles. The group of principles so added by Dalton I propose to call 'Dalton's Atomic Theory.'" By "group of principles," Debus means Dalton's well-known rules of chemical synthesis (N. S. p. 211).

We, on the other hand (R. and H. p. 51), have clearly stated that we consider the essential feature of Dalton's atomic theory to be "the idea that chemical combination takes place between particles of different weights," and it is the genesis of this idea, and not merely the origin of the

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* Communicated by the Authors.

empirical rules of chemical synthesis, that we have endeavoured to trace.

In doing so we were led to the conclusion that, in contradiction to the usually received view, Dalton's atomic theory was suggested by certain physical phenomena, especially the diffusion of gases, and not by the results of chemical analysis. Debus objects (p. 350) that this does not constitute a new view, inasmuch as he had himself previously arrived at a similar general conclusion, to which, indeed, we referred in our original work (R. and H. p. 6). We differ, however, from Debus as to the nature of the train of reasoning which led Dalton to the foundation of his theory, and it is our view of the actual genesis of the theory, taken together with the conclusion stated above, which justifies the wording of our title, "A New View of the Origin of Dalton's Atomic Theory."

Turning now to the evidence at our disposal, Debus and ourselves are in agreement up to a certain point. There seems to be no question that it was the study of the diffusion of gases which gave the primary impulse to Dalton's mind. At first he endeavoured (1801) to account for this phenomenon by supposing "that the particles of one gas are not elastic or repulsive in regard to the particles of another gas, but only to the particles of their own kind" (N. S. p. 154), so that each gas acted as a vacuum to the other. Moreover, as he states in the N. S. p. 188, he had in 1801 "a confused idea, as many have I suppose, at this time, that the particles of elastic fluids are all of the same size; that a given volume of oxygenous gas contains just as many particles as the same volume of hydrogenous; or if not, that we had no data from which the question could be solved"*.

Relying solely on this "confused idea," Debus, and here our differences begin, makes the following statement (p. 358): "In order to explain equilibrium in a mixture of gases, Dalton had adopted, in the year 1801, the hypothesis $M/S=C$" (M=molecular weight, S=specific gravity: the symbols $M/S=C$ represent the empirical law that equal volumes of different gases contain, at normal temperature and pressure, an equal number of molecules).

As a matter of fact, and as can at once be seen from the quotation given above, "the hypothesis $M/S=C$" is not in any way involved in the explanation of the phenomenon given by Dalton, and could not therefore be adopted in order to explain it. The precise expression of Dalton's "confused idea" by

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* It may be here noticed that Debus in quoting this passage (pp. 361, 362) invariably omits the last clause, which materially affects the sense of the whole passage.

the formula $M/S=C$, which Debus employs throughout his paper, is moreover misleading, since it implies that the idea of relative molecular weight was at that time present to Dalton's mind, whereas we have no evidence whatever that this was the case.

On this point therefore we retain our original position (R. and H. p. 47) that Dalton "never appears to have believed in the 'law of equal volumes,' and this only occurred to him as a possible alternative, at once shown to be inconsistent with fact, to the statement which he recognized as the true one, viz., 'that no two elastic fluids agree in the size of their particles.'"

Our view of the origin of the theory, on the other hand, is based upon a passage of Dalton's MSS. (1810) in which he professedly gives "a brief historical sketch of the train of thought and experience which led me to the conclusions about to be detailed."

Dissatisfied with the explanation of diffusion which he had given in 1801, he, at a later date, came to the conclusion that the phenomenon might be explained on the supposition that the atoms of the different gases were of different sizes. We here quote the passage in full (R. and H. pp. 16, 17):—

"Upon reconsidering this subject, it occurred to me that I had never contemplated the effect of difference of size in the particles of elastic fluids *. By size I mean the hard particle at the centre and the atmosphere of heat taken together. If, for instance, there be not exactly the same number of atoms of oxygen in a given volume of air, as of azote in the same volume, then the sizes of the particles of oxygen must be different from those of azote. And if the sizes be different, then on the supposition that the repulsive power is heat, no equilibrium can be established by particles of unequal sizes pressing against each other.

"This idea occurred to me in 1805. I soon found that the sizes of the particles of elastic fluids must be different. For a measure of azotic gas and one of oxygen, if chemically united, would make nearly two measures of nitrous gas, and those two could not have more atoms of nitrous gas than the one measure had of azote or oxygen. Hence the suggestion that all gases of different kinds have a difference in the size of their atoms; and thus we arrive at the reason for that diffusion of

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* Debus in quoting this passage (p. 355) here inserts the words "or when the expression $M/S=C$ is of different value for different gases." These words are not present in the original, and their insertion is a procedure which might lead the reader to suppose that the formula was used by Dalton.

every gas through every other gas, without calling in any other repulsive power than the well-known one of heat.

"This, then, is the present view which I have of the constitution of a mixture of elastic fluids.

"The different sizes of the particles of elastic fluids under like circumstances of temperature and pressure being once established, it became an object to determine the relative sizes and weights, together with the relative number of atoms in a given volume. This led the way to the combinations of gases, and to the number of atoms entering into such combinations, the particulars of which will be detailed more at large in the sequel.* Other bodies besides elastic fluids, namely liquids and solids, were subjected to investigation, in consequence of their combining with elastic fluids. Thus a train of investigation was laid for determining the number and weight of all chemical elementary principles which enter into any sort of combination one with another."

As far as quoted, the above narrative is perfectly continuous, so that the statement may fairly be taken as an authoritative one as to the train of reasoning which led Dalton to the attempt to determine the relative weights and sizes of the atoms.

The only difficulty in the way of regarding the above as a final settlement of the question of the origin of Dalton's theory arises from the date (1805) which Dalton assigns to his ideas about the different sizes of the atoms. The difficulty is this. In Dalton's laboratory note-books there is a table dated September 19th, 1803, in which is given a list of the weights of the ultimate atoms of a number of elements and compounds, together with the specific gravities of such as are gases, and the diameters of their elastic particles, compared with those of water = 1, the very matters mentioned in the concluding paragraph of the foregoing passage.

The existence of this table shows quite clearly that Dalton had formed definite conceptions regarding the different sizes of atoms as early as 1803, and had not only been confronted with the problem of determining their relative weights and sizes, but had actually obtained numerical values for these, and that, moreover, by the very method which he always afterwards employed.

These circumstances are in our opinion sufficient to justify us in the belief that Dalton in 1810 by a slip, either of the pen or of his memory, wrote 1805 by mistake instead of 1803.

The existence of a table such as that just described, drawn

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* The remainder of the passage is omitted by Debus (p. 355).

up in 1803, is otherwise in absolute contradiction with Dalton's "brief historical sketch."

Debus, however, refuses to agree in this conclusion, but maintains that the date 1805 really represents Dalton's meaning. The argument which he brings forward to explain away the inconsistency which this involves is that the last paragraph of the account quoted above from Dalton's "brief historical sketch" is a disjointed note, quite distinct from the rest, and does not bear any historical relation to the preceding narrative. Making this assumption, Debus admits that Dalton may have busied himself with the determination of atomic weights and diameters at an earlier date, but supposes that he did not conclude that the atoms *must* have different diameters until 1805, and this in face of the fact that Dalton in 1803 drew up a table showing that the diameters of the particles of all the gases he examined were different!

It is, however, clear from the MSS. (1810) that this contention cannot be upheld; the last paragraph follows in due order upon the foregoing one, and its contents bear an obvious relation to the preceding remarks.

Another argument of Debus in favour of the later date is that in 1805, in passing for the press a paper read in 1802, in which reference is made to the theory of selective repulsion, Dalton did not alter this, although he is known to have taken advantage of this opportunity to make additions and alterations in several papers published at the same period. This will hardly surprise us when we remember that even as late as 1808 (N.S. p. 154) Dalton thought it worth while not only to give a full account of the old theory, but even of the way in which many objections to it might be met.

Summing up the evidence as to the date at which Dalton changed his view about the nature of diffusion, it is clear that we must either admit the existence of a slip in the date 1805, written by Dalton in 1810, or else assume an absolute discontinuity and lack of coherence in a statement of which the continuity, both of sense and position, is obvious. Of the two alternatives we still prefer the former as by far the more reasonable.

Debus, moreover, retaining the date 1805 for Dalton's changed view about the diffusion of gases, further argues that he retained his belief in the hypothesis $M/S=C$ "more or less, from 1801–1805" (p. 362). Support for this idea is sought in two or three passages contained in Dalton's note-book (1803) and in Thomson's account of the atomic theory ('System of Chemistry,' vol. iii. 1807). The first of these passages occurs on p. 246 of the Note-book, i., and is as follows (R. and H. p. 27):—

"Enquiry into the specific gravity of the ultimate particles or elements.

Though it is probable that the specific gravities (*sic*) of different elastic fluids has some relation to that of their ultimate particles, yet it is certain that they are not the same thing; for the ult. part. of water or steam are certainly of greater specific gravity than those of oxygen, yet the last gas is heavier than steam."

The second passage (also 1803) occurs on p. 260 (R. and H. p. 42) in the form of a heading to a table which runs: "Ultimate atoms of gases in order of their specific gravities." Then follow numbers which represent the relative *weights* of the various atoms.

Finally, Thomson, in his account of the atomic theory, consistently uses the expression density or relative density of the atom where we should rather expect him to use the term relative weight.

Debus professes not to be able to understand the first of these passages, and argues from the others that if the specific gravities or relative densities of the atoms are identical with their relative weights, it follows that their volumes must all be equal, and hence Dalton must have retained his belief in the theory $M/S=C$ until at least 1804, since it was not until that year that he communicated the atomic theory to Thomson.

It will at once be seen that in these passages Dalton uses the term "specific gravity" as applied to an atom, with the meaning of "relative weight." The passages when thus interpreted run as follows:—"Enquiry into the relative weights of the ultimate particles or elements.

"Though it is probable that the specific gravities of different elastic fluids have some relation to the relative weights of their ultimate particles, yet it is certain that they are not the same thing; for the ult. part. of water or steam are certainly of greater relative weight than those of oxygen, yet the last gas is (specifically) heavier than steam."

The second passage becomes:—"Ultimate atoms of gases in the order of their relative weights."

In this form both extracts are quite comprehensible and consistent, both with each other and with Dalton's other expressions. It was in this way that we at first interpreted them (R. and H. pp. 27, 43), and Debus himself (p. 362) makes the same suggestion for the first passage. He then, however, takes this amended passage, contrasts it with the second in its original form, and points to the inconsistency between the two. Far from showing that Dalton at this time believed in the theory that "M/S = C," the first of these statements presents a striking proof to the contrary. It is in fact most probable that this is the very "train of reasoning" which convinced Dalton "that different gases have not their particles of the same size."

The fact that Dalton was not in this passage thinking of the volume of the elastic particle is strikingly brought out by a note which he had written on the page opposite to this passage (Note-book, i. p. 245; R. & H. p. 27):—

"N.B.—The ultimate atoms of bodies are those particles which in the gaseous state are surrounded by heat; or they are the centres or *nuclei* of the several small elastic globular particles."

As regards the second passage it may be noted that although the heading is "Ultimate atoms of gases in the order of their specific gravities," and if the argument of Debus is well founded the order of the specific gravities of the gases should be identical with that of their relative weights, this is not actually the case. The specific gravities (referred to air) of nearly all of these very gases are given only two pages before this list by Dalton, and an inspection of them at once shows that they do not follow the same order as the relative molecular weights.

Substance.	Relative molecular weight (Note-book, p. 260).	Specific gravity (Note-book, p. 258).
Hydrogen	1	·077
Azote	4	·966
Carbonated Hydrogen Gas	5·4	·660
Oxygen	5·5	1·127
Nitrous Gas	9·5	1·102
Gaseous Oxide of Carbon	10·1	1·000
Nitrous Oxide	13·5	1·610
Sulphuretted Hydrogen Gas	15·4	1·106
Carbonic Acid Gas	15·4	1·500

A further and conclusive proof that Dalton (and Thomson) did not (in 1803–4) believe in the hypothesis M/S = C is afforded by the formula ascribed to water, which they invariably wrote in symbols equivalent to the formula OH, although they were well aware that the volume relations were nearly as 1 : 2, according to which, if the contention of Debus were correct, the formula should have been H_2O.

Thomson, moreover, in his account of the atomic theory invariably used the term relative density or density of an atom, not only for gaseous, but also for liquid and solid bodies.

So obvious did it appear that Thomson used the expression density of an atom to mean its relative weight that we assumed that Debus, in his original pamphlet, had interpreted it in the same manner as ourselves, and it was upon this supposition that our criticism on this point (R. and H. p. 11) was founded. As it now appears that Debus considers that the words must be interpreted in their literal sense, our former criticism naturally falls. This interpretation, however, as we have just shown, cannot be maintained, and hence the argument which Debus bases upon it is fallacious.

As regards the process of thought or experience by which Dalton arrived at his empirical rules, of which Debus "wants to know" the origin, there is little to be added to the views we have already expressed. Dalton, with his "corpuscular turn of mind," seeking for a method of determining the relative sizes of his "atoms," no doubt perceived that two factors were concerned in the problem: the densities of the gases and the relative weights of the atoms of which they are made up. For the determination of the latter Dalton naturally turned to chemical analysis, but again found that two factors were involved: the relative weights of the two substances which combined and the relative number of the atoms of each which took part in the combination. Unable to devise any positive method of determining the latter, he had recourse to a series of empirical rules, which are based on the principle that, in the absence of distinct evidence, it may be assumed that combination always takes place in the simplest possible way.

The fact of the combination of two elements in several different proportions had long been familiar to chemists, and Dalton, with his profound belief in indivisible atoms of characteristic weight, could hardly fail at once to see that this might be explained by combination between varying numbers of the atoms of the two elements.

The question of the influence of Dalton's experiments on nitrous gas and oxygen upon his speculations (Debus, p. 360) has already been fully treated by us in our original work (R. and H. pp. 31–38), and we have there given the reasons which render it probable that these experiments did not play any very important part in the development of the atomic theory.

We may sum up our conclusions as follows:—

1. Dalton was led in 1803 to the conception of atoms of characteristic weight and size by his investigations on the diffusion of gases. From these same investigations he also received the incentive to determine the relative weights and sizes of the atoms of different substances.
2. In order to render possible the determination of the relative weights of the atoms from the results of analysis, he adopted his principle of "greatest simplicity," upon which the empirical laws of chemical synthesis are founded.
3. Dalton never definitely adopted the hypothesis $M/S=C$.

XXVII. *Discussion of the Currents in the Branches of a Wheatstone's Bridge, where each branch contains Resistance and Inductance, and there is an harmonic impressed electromotive force. By* Albert Cushing Crehore, *Ph.D., Assistant Professor of Physics, Dartmouth College, and* George Owen Squier, *Ph.D., First Lieutenant, U.S. Army*[*].

THE complete discussion of the currents flowing in the branches of a Wheatstone's Bridge, with a simple harmonic E.M.F., leads by the analytical method to equations which are too cumbersome to be of much practical use. For instance, by the direct method there would be formed as many differential equations as there are branches to the bridge, namely six, and these six simultaneous equations would, by a process of elimination, lead to a single differential equation of the sixth order, the solution of which would give the current in any desired branch. The practical obstacles in the way of finding the single differential equation from the six simultaneous equations, and after that obtaining its solution, are so great that if the same results can be obtained in a simpler way it would be welcomed. Having once written down the equations of current flow in the branches, it is a comparatively easy matter to determine the conditions for zero current in the galvanometer.

It happens that for this important particular case of zero current in the galvanometer there is a very simple way of determining these conditions for an harmonic E.M.F. Either the analytical or the graphical method will give the result, but the graphical method apparently gives so much more at the same time that it is preferred.

Referring to fig. 1, let the six branches of the bridge be

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[*] Communicated by the Authors.

denoted by the numbers (1) to (6), and denote the resistances and inductances of the branches by the letters

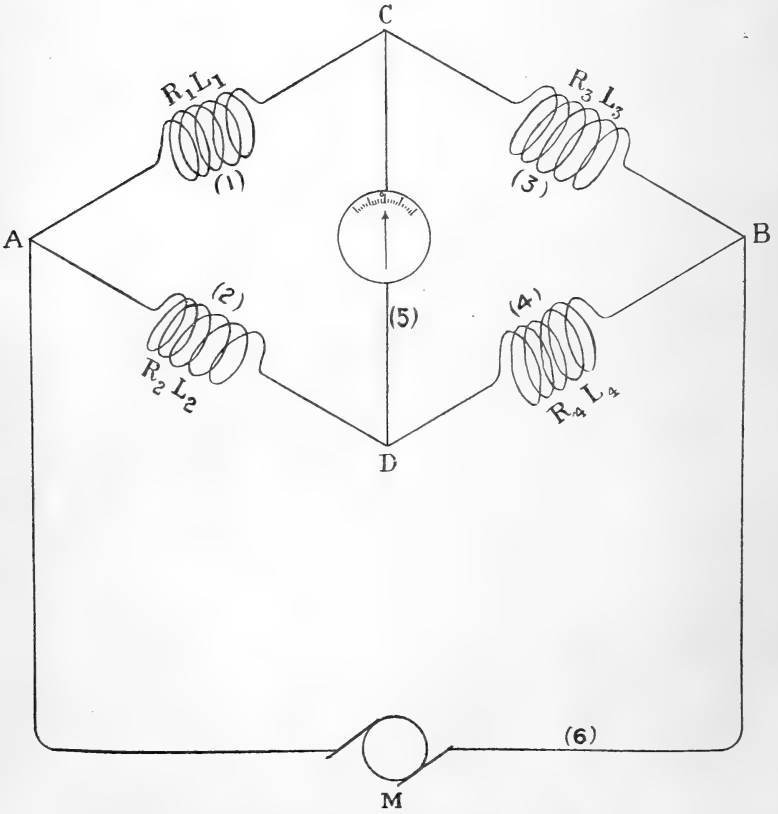
$$R_1, R_2, R_3, R_4, R_5, R_6, \text{ for resistances,}$$

and

$$L_1, L_2, L_3, L_4, L_5, L_6, \text{ for inductances.}$$

Let n denote the number of complete periods per second of the E.M.F. of the generator, and let T be the time of one period, then the angular velocity ω is equal to $2\pi/T = 2\pi n$.

Fig. 1.

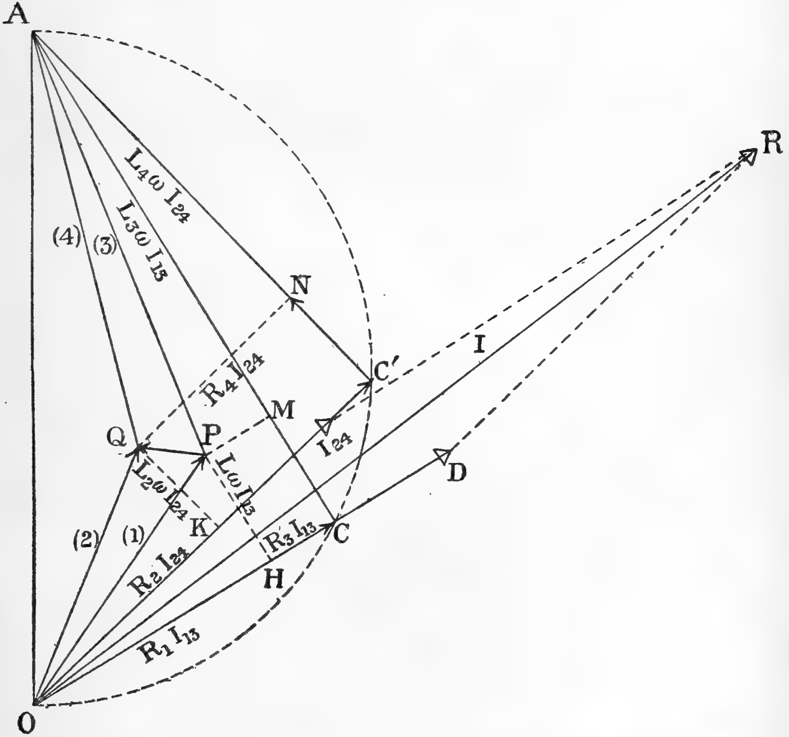


In fig. 2, let the line OA represent the impressed E.M.F., E, at the terminals AB (fig. 1) of the Wheatstone's bridge. Since in the present discussion no current flows in the galvanometer circuit, then no change will take place in the other currents which are flowing, if the galvanometer circuit is broken. Imagine it to be so broken, and we have merely the case of a divided circuit of two branches, each branch containing two coils with impedance. To find the currents in each branch independently we have

$$I_{1,3}=\frac{E}{\Sigma J}=\frac{E}{\sqrt{(\Sigma R)^2+(\Sigma L\omega)^2}}, \quad . \quad . \quad . \quad . \quad (1)$$

in which $I_{1,3}$ denotes the current in the branches (1) and (3) (fig. 1) and ΣJ the total impedance of these branches.

Fig. 2.—General diagram, illustrating the currents and potential-differences in the two branches of a divided circuit, each branch containing two coils with impedance; and showing the condition for zero current in the galvanometer of a Wheatstone's bridge.



The angle by which this current lags behind the impressed E.M.F., OA, is

$$\tan\theta_{1,3}=\frac{\Sigma_{1,3}\,L\omega}{\Sigma_{1,3}\,R}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

In fig. 2, make the angle AOC equal to arc tan $\theta_{1,3}$ of equation (2), and this determines the direction of the current $I_{1,3}$. Then upon the line so drawn take the point D so that OD represents in length the number of units contained in

$I_{1,3}$ equation (1). The line OD then represents the current flowing in the circuits (1) and (3). A similar calculation may be made for the branches (2) and (4), and another current OD′ drawn upon the diagram. Having determined the two branch currents OD and OD′, the current in the main line is the geometrical sum of these currents, represented by the line OR.

The potential-difference between the points C and D of the bridge is by hypothesis zero, and that between the terminals A and B is E. It remains to find the potential-differences between the points A and C, A and D, C and B, and D and B. Draw a perpendicular from A upon the line OD, then OC represents the whole of that component of the total E.M.F., E, which goes to overcome the resistance of the two circuits (1) and (3). This E.M.F. following Ohm's law is equal to the product of the current and resistance. Hence $OC=(R_1+R_3)I_{1,3}$. Similarly a point C′ may be found upon the line OD′, and $OC'=(R_2+R_4)\,I_{2,4}$. These lines OC and OC′ may then be divided into two parts proportional to the resistances R_1, R_3 and R_2, R_4. Such a division gives the points H on OC, and K on OC′, so that $OH=R_1\,I_{1,3}$; $HC=R_3\,I_{1,3}$; $OK=R_2\,I_{2,4}$; and $KC'=R_4\,I_{2,4}$.

Just as OC represents the component of the total E.M.F., E, which is in the direction of the current, and may be called the "power E.M.F.," so CA represents that component which is at right angles to the current, and is called the "reactive E.M.F." This part of the total E.M.F. is due to the presence of the magnetic field in the circuits, and it may be divided up in a similar manner into parts which represent the separate effects of the coils (1), (2), (3), and (4). The line CA is equal to $(\Sigma_{1,3}\,L\omega)\,I_{1,3}$, and may be divided at M into the two parts $CM=L_1\,\omega\,I_{1,3}$ and $MA=L_3\,\omega\,I_{1,3}$. Similarly C′A is divided at N into two parts $C'N=L_2\,\omega\,I_{2,4}$ and $NA=L_4\,\omega\,I_{2,4}$.

To find the potential-difference at the terminals of any coil as AC (fig. 1) we need only combine the component E.M.F.'s for that coil, one in the direction of the current and the other at right angles to it. This gives for circuit AC (1) the line $OH=R_1\,I_{1,3}$ in the direction of the current, and $HP=L_1\,\omega\,I_{1,3}$ at right angles to the current to be geometrically added together, making the resultant OP as the potential-difference at the terminals of the coil AC (1). A similar process gives OQ for circuit AD (2), and PA for CB (3), and QA for DB (4).

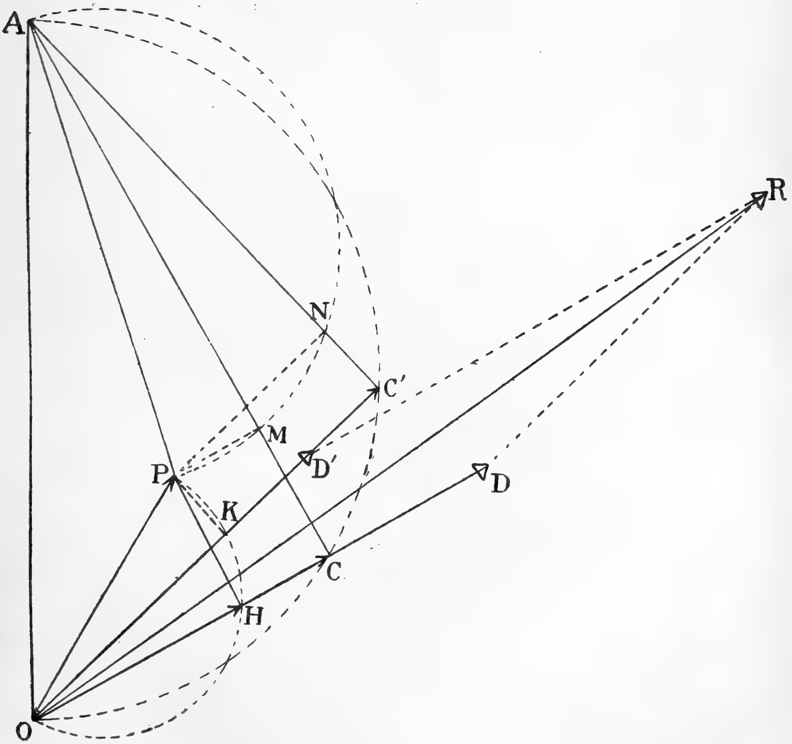
Fig. 2 is drawn to represent the case of a divided circuit, which becomes the Wheatstone's bridge by joining a circuit across between the branches.

If such a connexion were made, the arrangement of the lines as drawn would be disturbed by the current which flows across the new circuit. Before the connexion is made, however, it is evident that the potential-difference is represented by the line PQ in the figure; for the closed circuit ACD (fig. 1) contains no impressed E.M.F. Hence the total fall of potential around the circuit must be equal to zero. The three E.M.F.'s in these branches must therefore form a triangle, and as the E.M.F.'s OP and OQ are determined, it follows that the E.M.F. PQ must be that between the terminals CD (5).

We therefore have:

That the condition for no current in the galvanometer is that the points P *and* Q *in the diagram shall coincide, or that the line* PQ *shall be zero.*

Fig. 3.—Diagram the same as fig. 2, except that the points P and Q are brought into coincidence; a necessary condition for zero current in the galvanometer of a Wheatstone's bridge.



In fig. 3 is represented another diagram constructed like fig. 2, except that the points P and Q are brought into coincidence at P. The condition that P and Q shall coincide, by referring to the figure, and observing that the points H and K lie upon a semicircle having OP as a diameter, while M and N lie upon one having PA as a diameter, may be expressed by the equations derived from the right triangles OHP and OKP, viz.:—

$$\overline{OP}^2=\overline{OH}^2+\overline{HP}^2=\overline{OK}^2+\overline{KP}^2,$$

or

$$I_{1,3}^2(R_1^2+L_1^2\,\omega^2)=I_{2,4}^2(R_2^2+L_2^2\omega^2). \quad . \quad (3)$$

Again

$$\overline{PA}^2=\overline{PM}^2+\overline{MA}^2=\overline{PN}^2+\overline{NA}^2,$$

or

$$I_{1,3}^2(R_3^2+L_3^2\,\omega^2)=I_{2,4}^2(R_4^2+L_4^2\,\omega^2). \quad . \quad (4)$$

Dividing equation (3) by (4), member for member, we obtain

$$\frac{R_1^2+L_1^2\,\omega^2}{R_3^2+L_3^2\,\omega^2}=\frac{R_2^2+L_2^2\,\omega^2}{R_4^2+L_4^2\,\omega^2}, \quad . \quad . \quad . \quad . \quad (5)$$

or denoting the impedances of the branches by J_1, J_2, J_3, and J_4 respectively, and remembering that $J_1=\sqrt{R_1^2+L_1^2\,\omega^2}$; $J_2=$&c., we have

$$\frac{J_1}{J_3}=\frac{J_2}{J_4}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

as the necessary condition for zero current in the galvanometer.

This equation asserts:

When an harmonic electromotive force is impressed upon one of the branches of a Wheatstone bridge, a galvanometer in the conjugate branch of the bridge can only indicate zero current when the impedances of the remaining four branches of the bridge form a simple proportion.

This is entirely analogous to the well-known condition when the direct current is used in the Wheatstone bridge, namely, that the resistances in the branches of the bridge form a proportion.

Another expression which the diagram makes apparent, and will be useful to note, is that derived from the right triangles OAC and OAC′ inscribed in the semicircle. These give

$$\overline{OA}^2=\overline{OC}^2+\overline{CA}^2=\overline{OC'}^2+\overline{C'^2A}^2,$$

or

$$\begin{aligned}E^2&=I_{1,3}^2[(R_1+R_3)^2+(L_1+L_3)^2\,\omega^2].\\&=I_{2,4}^2[(R_2+R_4)^2+(L_2+L_4)^2\,\omega^2]. \quad . \quad . \quad (7)\end{aligned}$$

Adding (3) and (4) we have

$$\begin{aligned} &I_{1,3}^2[R_1^2+L_1^2\,\omega^2+R_3^2+L_3^2\,\omega^2] \\ &=I_{2,4}^2[R_2^2+L_2^2\,\omega_2^2+R_4^2+L_4^2\,\omega^2]. \quad . \quad . \quad . \quad . \quad . \qquad (8) \end{aligned}$$

Subtracting (8) from (7) we have

$$I_{1,3}^2[R_1R_3+L_1L_3\,\omega^2]=I_{2,4}^2[R_2R_4+L_2L_4\,\omega^2]. \quad . \qquad (9)$$

This may be written so as to express the ratio of the currents in the branches as

$$\frac{I_{1,3}^2}{I_{2,4}^2}=\frac{R_2R_4+L_2L_4\,\omega^2}{R_1R_3+L_1L_3\,\omega^2}. \quad . \quad . \quad . \quad . \quad . \qquad (10)$$

Equations (3) and (4) may also be so written as to express this ratio, and we have

$$\begin{aligned} \frac{I_{1,3}^2}{I_{2,4}^2}=\frac{R_2^2+L_2^2\,\omega^2}{R_1^2+L_1^2\,\omega^2}=\frac{R_4^2+L_4^2\,\omega^2}{R_3^2+L_3^2\,\omega^2}=\frac{R_2R_4+L_2L_4\,\omega^2}{R_1R_3+L_1L_3\,\omega^2} \\ =\frac{J_2^2}{J_2^2}=\frac{J_4^2}{J_3^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (11) \end{aligned}$$

In experimenting with a Wheatstone's bridge with reference to its use with the alternating current for the purposes of developing a new range-finder for coast defence, this whole problem of the Wheatstone's bridge in its more general form for alternating currents has come under our consideration. In looking over the literature of the subject nothing has come to notice which treats so explicitly of this general problem as of the particular case of its use with the direct current. Although Professor J. J. Thomson treats the subject in his recent work, 'Experimental Researches in Electricity and Magnetism,' and there derives the conditions for zero current in the galvanometer, yet the graphical method of viewing the problem adds so much to the bare analytical statements, especially where one is experimenting with the bridge and desires to know how any particular quantity varies for a given variation of any other quantity, that this discussion became desirable. In particular, it was desired to know how a variation of the inductance in one arm of the bridge affected the inductance of a second arm, other constants remaining unchanged, while the galvanometer continually indicates zero.

The above equation (11) contains the solution of this, but it may be transformed so as to represent more clearly the exact relation between the variables. Let us assume that all the constants of the bridge except the inductances L_3 and L_4 of branches (3) and (4) (fig. 1) remain unchanged. Let the inductance L_3 be the independent variable, and let it be required to calculate the value of L_4 for zero current in the galvanometer.

Denote the reactance $L_3\omega$ by x, and the reactance $L_4\omega$ by y, then equation (11) may be written in the form

$$k=\frac{m+y^2}{n+x^2}=\frac{p+qy}{r+sx}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

where k is a constant equal to $\frac{I^2_{1,3}}{I^2_{2,4}}=\frac{R_2^2+L_2^2\omega^2}{R_1^2+L_1^2\omega^2}$, and $m\equiv R_4^2$; $n\equiv R_3^2$; $p\equiv R_2R_4$; $q\equiv L_2\omega$; $r\equiv R_1R_3$; $s\equiv L_1\omega$.

Equation (12) may be reduced to

$$qx^2y-sxy^2+px^2-ry^2+ex+fy+g=0, \quad . \quad (13)$$

where

$$e\equiv-ms=-R_4^2L_1\omega\,;\quad f\equiv qn=R_3^2L_2\omega\,;$$

and

$$g\equiv pn-mr=R_2R_4R_3^2-R_1R_3R_4^2.$$

This is evidently an equation of the third degree between two variables, the reactances of the coils, and it enables one to find the inductance y of one coil which will just balance a given inductance x in the other.

This equation is of use in calculating points of maximum sensitiveness, and it also shows other useful relations. For instance, if the resistances R_1 and R_2 are made approximately equal as well as the inductances L_1 and L_2, the equation shows that not only is it necessary for the inductances of the other two circuits (3) and (4) to be equal for zero current, but the resistances of those branches must also be equal. Substituting $x=y$ in (13) satisfies the equation if $R_1=R_2$; $R_3=R_4$; and $L_3=L_4$.

This point may be clearly seen in the diagrams. In fig. 3, in which the points P and Q of fig. 2 are merely brought into coincidence, it will be seen that

$$\tan \mathrm{POH}=\frac{L_1\omega}{R_1}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

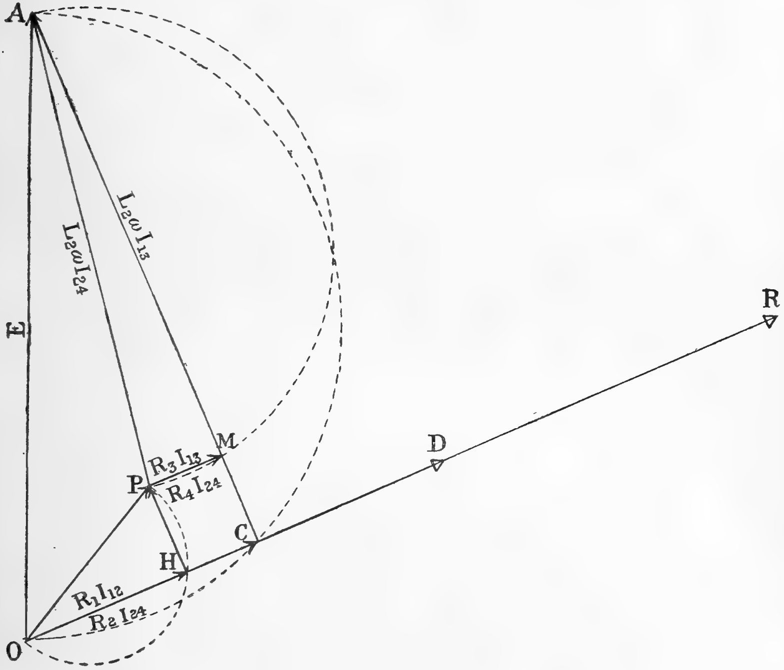
$$\tan \mathrm{POK}=\frac{L_2\omega}{R_2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

$$\tan \mathrm{APM}=\frac{L_3\omega}{R_3}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

$$\tan \mathrm{APN}=\frac{L_4\omega}{R_4}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

By making $R_1=R_2$ and $L_1=L_2$ we reduce (14) and (15) to equality, so that, as the angles are the same, the points H and K are brought into coincidence at H (fig. 4) upon their circle OHKP. Since the points C and C′ (fig. 3) lie in the lines OHC and OKC′ as well as upon the circle OCC′A they are also brought into coincidence at C (fig. 4). As the points

Fig. 4.

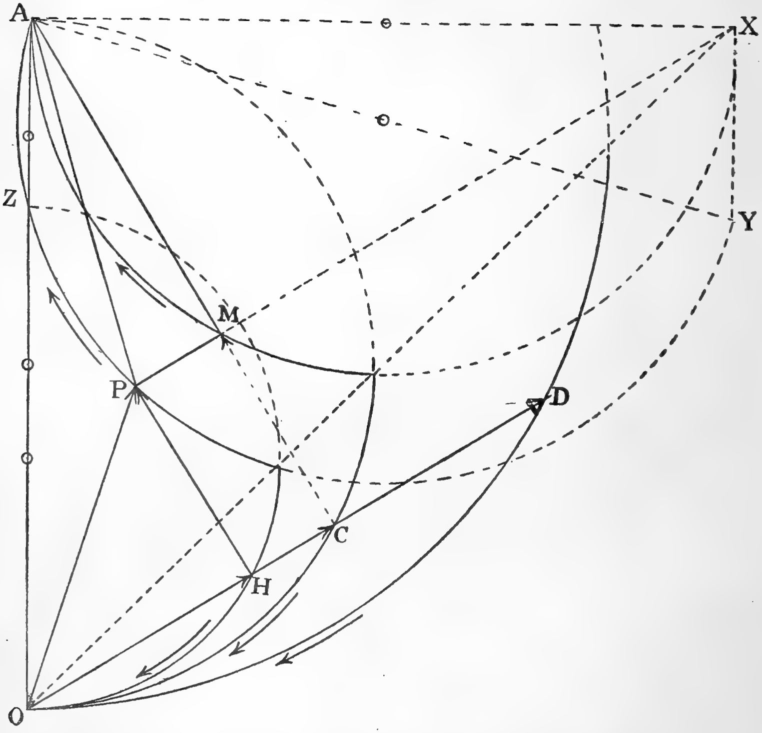


M and N lie both upon the lines AMC and ANC and the circle ANMP, these points are also brought into coincidence at M (fig. 4). By equation (11) the branch currents OD and OD′ are made equal by these conditions, so that the points D and D′ coincide in the figure, while OR=2OD and has the same direction.

Variation Diagrams.

In fig. 4 are represented the currents and potentials for a single value of the resistances and inductances only. If different inductances L_1 and L_2 are used, while all the other quantities remain the same, the diagram will be completely altered, and it is very desirable to know how the various points in the diagram move with a gradual change in the two inductances, provided always the galvanometer remains at zero. Fig. 5 illustrates this variation, and shows that the point P moves along the circle AZPY in the direction of the arrow as the inductances L_1 and L_2 increase. The point M moves upon the circle XMA in the direction of the arrow as the inductances increase. The points H, C, and D move along the circles ZHO, ACO, and DO as the inductances increase. This diagram enables one to draw the true diagram for any value of the inductances. The points do not move on the dotted parts of the circles, but are limited to the heavy portions. The diameter AX is at right angles to OA, and the line PM produced passes through the point X. The triangle APM is similar to itself in all positions, for the angle at P must be constant and equal to arc tan $L_3\omega/R_3$. Hence the diameter AY makes an angle XAY with AX, equal to MAP.

Fig. 5.—Showing the variation of the points of fig. 4 as the inductances L_1 and L_2 vary.



The point C always lies on the semicircumference OCA. Hence the points H and D move on similar circles, their distances from O always being proportional to OC.

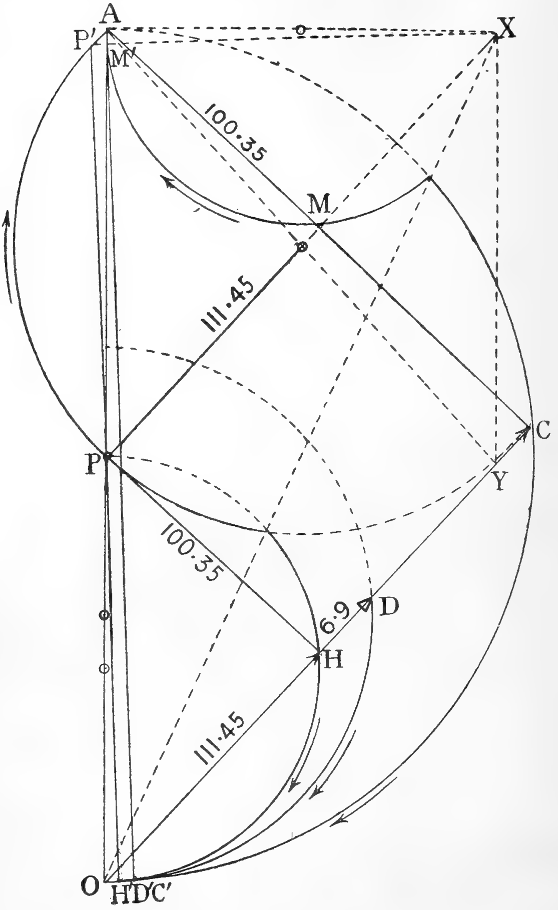
For experimental purposes four coils were constructed approximately alike, of the following dimensions:—The spools were 44 centims. long, 2·5 centims. in diameter to the windings; the round hole through the centre 2·3 centims. in diameter; the wire No. 22 B. and S. single cotton in 8 layers of 492 turns per layer, making a total of 3936 turns. Four iron cores were made alike of soft iron wire No. 16, insulated with single cotton, and cut into uniform lengths of 44 centims. The mass of each core was 407 grams. The resistance of each coil was measured to be about 16·2 ohms. The reactances and inductances were also measured for each coil both when the iron cores are removed for minimum reactance, and when they are in the central position for maximum reactance. When no iron core is inserted, the induction of each coil is ·0166 henrys, and with a value of $\omega = 876$ this gives the reactance $= 14·5$ ohms. Combining the resistance and the reactance we have 21·6 ohms impedance. When the iron core is inserted the current with a constant potential source falls rapidly owing to the increased impedance. When the core is in the central position, the inductance becomes 1·084 henrys, corresponding to a reactance of 950 ohms, and an impedance the same as the reactance to within 0·15 of an ohm. It is interesting to note that the inductance is increased 65 fold by inserting the iron core.

Fig. 6 represents to scale the case when no iron is in either coil, and shows the variation of the points as two cores are inserted one into each coil, until they are entirely in. The angle AOC is laid off for no iron, equal to

$$\text{arc tan} \frac{\Sigma L\omega}{\Sigma R} = \frac{29}{32{\cdot}31} = {\cdot}9 \text{ approx.}$$

Hence $\theta = 42°$ approximately. The E.M.F. OC is therefore 222·9 volts, and as the resistances are approximately equal, the point H (the letters of all the figures denoting corresponding points) bisects OC. As the reactances of all coils are approximately equal the point M also bisects AC, and hence P lies at the middle point of OA. The current OD is 6·9 amperes, in each circuit, making a total of 13·8 amperes in the main line. Any change made by introducing the two iron cores has the effect of moving the points upon the heavy portions of their respective circles in the directions of the

Fig. 6.—Showing the actual variation of the quantities used from when the iron cores are all withdrawn until two of them are entirely in.



Scale ·75 cm. = 20 volts. ·75 cm. = 1 ampere.

arrows as the cores are introduced, until the point P arrives at P′ when the cores are entirely in. The points H, C, M, and D move to H′, C′, M′, and D′ when the iron cores are inserted into one pair of coils.

XXVIII. *Applications of Physics and Mathematics to Seismology.* *By* C. Chree, *Sc.D.*[*]

Sections CONTENTS.

Introductory.

§ 1. THE existence of apparent movements in the earth's surface-strata appeals in the first instance to seismologists. Prof. J. Milne, however, and others have been of late attempting to bring it home to astronomers and meteorologists that they too may have a vital interest in the matter. The presentations of the subject which have come under my notice take little or no heed of the theoretical aspects of the case, in which, as an elastician, I have long been interested. As the neglect of theoretical results may be due not so much to their defects as to the slowness with which mathematical investigations become generally known, I have decided to group together and discuss in a more or less popular way the theoretical conclusions which seem to me the most closely connected with the subject in question.

§ 2. The mathematical work by which these conclusions were deduced refers to material which is homogeneous, isotropic, and elastic; while the body in whose phenomena the seismologist is interested is the earth.

Now it must not be supposed that I fail to appreciate the differences between the material of theory and that of nature. The certainty of the departure of many of the surface-strata from the attributes ascribed to isotropic elastic solids, and the uncertainty as to the density, solidity, and other physical properties of $\frac{999}{1000}$ of the earth's mass I perfectly realize. The conditions under which the deep-seated materials of the earth exist are fundamentally different from those we are

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[*] Communicated by the Physical Society: read December 11, 1896.

familiar with at the surface. The enormous pressure, and the presumably high temperature, very likely combine to produce a state to which the terms solid, viscous, liquid, as we understand them, are alike inapplicable. But be the state what it may, the material must respond to the action of periodic forces; such forces must produce varying strains and stresses; and these strains and stresses can hardly fail to produce effects at the surface. No numerical estimate of these effects can claim to be in any sense final, as the mathematical work by which it is evolved must depend on physical data which are at best unproved. It appears desirable, however, that such numerical estimates should be made, on the least objectionable physical basis available, if only for the reason that their existence supplies a guide and incentive to direct observation.

In my opinion, for reasons previously discussed*, the treatment of the earth as an incompressible elastic solid is exposed to perhaps a minimum of objections. Most probably the material increases in density and temperature as we approach the centre, and a treatment which assumes the material to vary with the radial distance would possess higher *à priori* claims to regard than one which treats the earth as homogeneous throughout. The problem of a gravitating mass of varying elastic properties has, however, still to be published, and as the assumed law of variation would probably be mainly guess-work, the advantages for practical ends might be less than would appear at first sight.

Variation of the material with the radial distance, I may add, could hardly affect the general character of the phenomena. Surface heterogeneity, in which the material varies rapidly with latitude or longitude, is not unlikely to modify largely the magnitude of some of the results at individual stations, but is most unlikely to produce a large effect on the order of magnitude of the mean lunar or solar tidal effects at a moderate number of stations scattered over the earth's surface.

The treatment of local surface pressures in the first part of the paper is in some respects on a less uncertain basis. We can assure ourselves, if need be, of the solidity of the ground surrounding a station; and though the mathematical work treats the solid as going down to infinity, this only means in practice that the depth must be large compared to the shortest distance of the loaded area. There are reasons, however, even here for regarding the numerical results as in general but rough approximations.

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* Phil. Mag. Sept. 1891, p. 232.

They might possess high accuracy if the surface material were bare rock in horizontal strata, and the recording apparatus were supported directly on the rock ; but uncertainties are introduced when the load is applied at the surface of ordinary soil, and the support of the apparatus is stonework inside a building whose foundations go to an appreciable depth.

§ 3. The observed facts on which our investigations are most likely to bear are certain slow changes in the indications of spirit-levels or delicately suspended pendulums. Some of these Prof. Milne is disposed to attribute to meteorological agencies such as rainfall or evaporation.

A relative excess of moisture to the west, say, of an observatory is, he considers, equivalent to a surface load on that side, tending to make the ground on which the building stands slope downwards from east to west. Such want of symmetry may arise from the peculiarities of the soil, or through the ground being sheltered by trees or modified by cultivation. In sunshine the shadow of the building itself, by retarding evaporation, may set up such a difference as Prof. Milne has in view.

I am not sure that an excess of evaporation from the east, say, of a building is necessarily equivalent to the withdrawal of a surface load from that side, at least to the exact extent of the surplus evaporation. The withdrawal of moisture from the soil has a decided influence on its conductivity for heat—not to speak of electricity—and so may exert a very appreciable influence on the temperature near the surface, the consequences of which it would be difficult to follow. There is also, presumably, underground circulation both of air and moisture, which may not unlikely counteract to some extent differences of surface evaporation.

Though these and other uncertainties exist, it is certainly worth while considering the numerical magnitude of the results to be expected from the agencies postulated by Prof. Milne. The theoretical results will also, I hope, suggest the way in which the best use may be made of experimental determinations of the effects of surface loading over limited areas of convenient shape.

Influence of Surface Load on the observed Level.

§ 4. This influence is not so simple as might appear at first sight. The weight of the loading material is equivalent to a pressure normal to the surface, which we suppose horizontal. But, in addition, we must allow for the fact that the gravitational attraction of the loading material slightly alters the direction of "gravity" at the surface. Consider, for example, the influence of the ordinary ocean tide at a point inland near the shore. At high tide there is on the sea-bottom a pressure exceeding the mean by an amount corresponding to the height of the water above its mean level; this will tend to make a naturally horizontal plane dip towards the sea. At the same time the surplus volume of water will give a horizontal component to what we may regard as normal "gravity" in the neighbourhood. This second effect has been called attention to, in this very case, in Thomson and Tait's 'Natural Philosophy,' art. 818, where will be found a numerical estimate for a specified set of conditions. What a spirit-level shows is the plane perpendicular to gravity—including "centrifugal force" and all disturbing forces. We are thus obliged to consider both effects before attempting numerical estimates.

Direct Pressure Effect, Fundamental Formulæ.

§ 5. In the following calculations the earth is treated as an isotropic elastic solid, principal weight being attached to the results obtained by supposing the material incompressible. Also, as we are primarily interested in the consequences of pressure applied over limited areas, the loaded surface is treated as a horizontal plane, on the lower side of which the material extends to infinity. On these hypotheses we are enabled to make use of the very interesting and important results established by Professors Cerruti and Boussinesq.

A convenient English abstract[*] of Boussinesq's work is contained in Todhunter and Pearson's 'History of Elasticity,' vol. ii. part 2, arts. 1492 *et seq.*, from which the following formulæ are quoted, the only variation being the use of Thomson and Tait's notation for the elastic constants.

The origin of coordinates lies in the undisturbed surface, taken as the plane of xy, the positive direction of the z axis being downwards into the earth. The normal pressure applied to the element $d\omega$ of surface is $p\,d\omega$, where p is supposed of course a known function of x, y.

u, v, w denote the components of elastic displacement, n the rigidity, and η Poisson's ratio for the material.

The displacements at any point x, y, z in the material are as follows:

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[*] Chapter ix. vol. i. of Love's 'Treatise on....Elasticity' may also be usefully consulted.

$$u = -\frac{1}{4\pi n}\left\{\frac{d^2}{dx\,dz}\iint rp d\omega + (1-2\eta)\frac{d}{dx}\iint \log(z+r)p d\omega\right\}, \quad (1)$$

$$v = -\frac{1}{4\pi n}\left\{\frac{d^2}{dy\,dz}\iint rp d\omega + (1-2\eta)\frac{d}{dy}\iint \log(z+r)p d\omega\right\}, \quad (2)$$

$$w = \frac{1}{4\pi n}\left\{2(1-\eta)\iint \frac{p}{r} d\omega + z^2\iint \frac{p}{r^3} d\omega\right\}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Here r is the distance between the element $d\omega$ where p is applied, and the point x, y, z where u, v, w are measured. The integration extends to all parts of the surface where p differs from zero.

The simplification in the formulæ when $\eta = \cdot 5$, or the material is incompressible, should be noticed.

The *slope*—*i.e.*, inclination to the plane of xy—introduced into any horizontal plane depends only on the vertical displacement w. In particular, the slope of the surface depends only on w_0, the value of the vertical displacement when $z=0$; and by (3) we obviously have

$$w_0 = \{(1-\eta)/(2\pi n)\}\iint (p/r) d\omega. \quad . \quad . \quad . \quad (4)$$

In (4) r is the distance between the element $d\omega$ and the point x_0, y_0 on the surface to which w_0 refers.

Relation between Pressure and Gravitational Effects.

§ 6. If we suppose t the thickness, ρ the density of the material loading the surface, its gravitational forces are derived from the potential

$$V = \gamma\iint (t\rho/r) d\omega, \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where γ is the attraction between two unit masses at unit distance.

The pressure exerted by the load is

$$p = g\rho t,$$

where g is gravity at the surface. Here we may regard g as the undisturbed value prior to the application of the load, as the alteration in the vertical component is negligible for our present object. Thus

$$V = (\gamma/g)\iint (p/r) d\omega.$$

Comparing this with (4), we find for the surface value V_0 of V the simple relation

$$V_0 = 2\pi n\gamma w_0/\{g(1-\eta)\}. \quad . \quad . \quad . \quad . \quad . \quad (6)$$

This holds true of V_0 and w_0 all over the surface, and so applies likewise to their differential coefficients with respect to x_0 and y_0—so far at least as concerns points outside the loaded area.

The direction-cosines of the normal to the surface after the application of the load are, to a first approximation,

$$\frac{dw_0}{dx_0},\quad \frac{dw_0}{dy_0},\quad 1;$$

so that the slope at the point x_0, y_0 is given by

$$\psi_1 = \left\{ \left(\frac{dw_0}{dx_0}\right)^2 + \left(\frac{dw_0}{dy_0}\right)^2 \right\}^{\frac{1}{2}} \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Again, to a first approximation the presence of the loading-material has altered the direction-cosines of the line of action of gravity from

$$0,\ 0,\ 1 \text{ to } \frac{1}{g}\frac{dV_0}{dx_0},\quad \frac{1}{g}\frac{dV_0}{dy_0},\quad 1.$$

Thus gravity has become inclined to the vertical at the angle

$$\psi_2 = \frac{1}{g}\left\{ \left(\frac{dV_0}{dx_0}\right)^2 + \left(\frac{dV_0}{dy_0}\right)^2 \right\}^{\frac{1}{2}} \quad . \quad . \quad . \quad . \quad (8)$$

Employing (6) in (7) and (8), we obtain the elegant relation

$$\psi_1/\psi_2 = (1-\eta)g^2/(2\pi n\gamma). \quad . \quad . \quad . \quad . \quad (9)$$

The spirit-level measures $\psi_1+\psi_2$, which always exceeds the true change of level ψ_1.

Since

$$\frac{dw_0}{dx_0}\Big/\frac{dw_0}{dy_0} = \frac{dV_0}{dx_0}\Big/\frac{dV_0}{dy_0}, \quad . \quad . \quad . \quad . \quad (10)$$

the final directions of gravity and of the normal to the surface lie in the same vertical plane (*i.e.* plane through z). This result may facilitate experimental investigations, as a rough idea of the direction of the resultant attraction of the loading-material will generally be obtainable by eye. The possible influence of want of symmetry in the contour of the ground, or variability of the surface-strata, must of course be borne in mind.

The relation (9), so far as I know, is new. Its discovery was due to a faint impression that a formula I had obtained for the effect of a loaded rectangle resembled something I had seen before, the something proving on investigation to be result (7) in Thomson & Tait's Nat. Phil., art. 818.

§ 7. To form an idea of the relative importance of ψ_1 and ψ_2 in the case of the earth, I have made the following selection of hypothetical values for η and n, the latter quantity being measured in grammes weight per square centimetre :—

(i.)	$\eta = \cdot 25$,	$n = 80 \times 10^7$;
(ii.)	$\cdot 25$,	35×10^7 ;
(iii.)	$\cdot 5$,	11×10^7.

According to the table in Lord Kelvin's Encyclopædia Article on Elasticity, (i.) may be regarded as representing iron or steel, (ii) as representing brass or slate of somewhat low modulus, while (iii.) represents an incompressible material* such as seems most compatible with the hypothesis of a homogeneous earth, naturally spherical but for rotation. According to Lord Kelvin's table, the value of n in (iii.) is similar to what one should expect in rock of somewhat low elasticity.

If a be the earth's radius, $\bar{\rho}$ its mean density,

$$g/\gamma = 4\pi\bar{\rho}a/3.$$

Supposing $\bar{\rho} = 5{\cdot}5$, and $a = 64 \times 10^7$ cms., we have approximately $g\bar{\rho}a = 35 \times 10^8$ grammes weight per square centimetre.

With the above figures, I find

case	(i.)	$\psi_1/\psi_2 = 35/16 =$	2 roughly,
	(ii.)	$10/2 =$	5,
	(iii.)	$350/33 =$	11 roughly.

The last result is likely, I think, to prove the nearest to what ordinarily occurs in practice, so that the gravitational effect may be expected to prove as a rule relatively small ; still it ought not to be disregarded without due consideration of the special circumstances.

Pure Pressure Effects.

§ 8. In all cases when the largest dimension of the loaded area is small compared to its shortest distance from the point of the surface where the slope is required, a good first approximation † to the surface vertical displacement—obvious on inspection of (4)—is

$$w_0 = (1-\eta)\mathrm{P}/(2\pi n\mathrm{R}), \quad . \quad . \quad . \quad . \quad (11)$$

where R denotes the distance from the centre of mass of the total load P.

---

* See Phil. Mag. Sept. 1891, p. 250, remembering $\mathrm{E}/n = 2(1+\eta)$.

† See Todhunter & Pearson's 'History,' vol. ii. pt. 2, art. 1498.

The corresponding approximation to the slope, viz.,

$$-\frac{dw_0}{d\mathrm{R}}=(1-\eta)\mathrm{P}/(2\pi n\mathrm{R}^2), \quad . \quad . \quad . \quad (12)$$

shows that at considerable distances from a small loaded area the slope varies approximately as the inverse square of the distance. In (11) and (12) the distribution of load is not assumed uniform.

The fact that (11) holds only when the distance of the loaded area is so large that its effect is relatively small diminishes its value in practice.

§ 9. The determination of w from (3) entails the evaluation of two integrals, neither very manageable. For points on the surface there is, however, only the single integral (4). This has been converted by Boussinesq into two alternative forms—one for points outside, the other for points inside the loaded area—which are convenient when the load, though not necessarily uniform, is distributed symmetrically round a point (see Todhunter & Pearson's 'History,' arts. 1501 and 1502). In this way the depression can be easily determined at the centre and edge of a circular depressed area for a variety of laws of loading, and the depression at other points can be expressed in terms of infinite series or elliptic integrals (see Todhunter & Pearson, *l. c.*, especially art. 1504).

The slope in these cases, at any distance from the centre of the loaded area, can be obtained in the form of infinite series or elliptic functions; but results of this kind are more apt to repel than to enlighten the unmathematical reader.

Fortunately, when the load is uniform, and the loaded area rectangular, it proves possible to express the components of slope dw/dx, dw/dy at any point of the surface in terms of ordinary Napierian logarithms. I shall accordingly devote attention almost exclusively to this case.

§ 10. Returning to (3), let x', y' be coordinates of the element $d\omega$, so that

$$d\omega=dx'dy',$$
$$r^2=(x-x')^2+(y-y')^2+z^2.$$

The loading being supposed uniform, we have

$$\frac{4\pi n}{p}\frac{dw}{dx}=2(1-\eta)\iint\frac{d}{dx}\left(\frac{1}{r}\right)dx'dy'+\iint z^2\frac{d}{dx}\left(\frac{1}{r^3}\right)dx'dy'.$$

But

$$\frac{d}{dx}\left(\frac{1}{r}\right)=-\frac{d}{dx'}\left(\frac{1}{r}\right), \text{ and } \frac{d}{dx}\left(\frac{1}{r^3}\right)=-\frac{d}{dx'}\left(\frac{1}{r^3}\right);$$

hence

$$\frac{4\pi n}{p}\frac{dw}{dx}=2(1-\eta)\int\left(\frac{1}{r_1}-\frac{1}{r_2}\right)dy'+z^2\int\left(\frac{1}{r_1{}^3}-\frac{1}{r_2{}^3}\right)dy', \quad . \ (13)$$

where r_1 and r_2 are the inferior and superior limits of r in the integration with respect to x'.

Suppose the origin vertically over the point where the slope is to be found, or $x=y=0$, and draw the axes of x and y parallel to the sides of the loaded rectangle. Take for the coordinates of the corners of the rectangle—

$$x_1, y_1; \ x_2, y_1; \ x_2, y_2; \ x_1, y_2;$$

and suppose

$$x_2 > x_1, \ y_2 > y_1.$$

The following result is then easily obtained from (13):

$$\frac{4\pi n}{p}\left(\frac{dw}{dx}\right)^{x=y=0}_{z=z}=2(1-\eta)\log\frac{(y_2+\sqrt{y_2{}^2+x_1{}^2+z^2})\ (y_1+\sqrt{y_1{}^2+x_2{}^2+z^2})}{(y_1+\sqrt{y_1{}^2+x_1{}^2+z^2})\ (y_2+\sqrt{y_2{}^2+x_2{}^2+z^2})}$$

$$+z^2\left\{\frac{1}{x_1{}^2+z^2}\left(\frac{y_2}{\sqrt{y_2{}^2+x_1{}^2+z^2}}-\frac{y_1}{\sqrt{y_1{}^2+x_1{}^2+z^2}}\right)\right.$$

$$\left.+\frac{1}{x_2{}^2+z^2}\left(\frac{y_1}{\sqrt{y_1{}^2+x_2{}^2+z^2}}-\frac{y_2}{\sqrt{y_2{}^2+x_2{}^2+z^2}}\right)\right\}. \ . \ (14)$$

This combined with the corresponding expression for dw/dy, which can be written down by symmetry, supplies complete information as to the slope at all depths. By putting $z=0$ in (14), or by direct calculation, we get

$$\frac{dw}{dx}\text{ (at origin)}=\frac{(1-\eta)p}{2\pi n}\log\frac{(y_2+\sqrt{y_2{}^2+x_1{}^2})\ (y_1+\sqrt{y_1{}^2+x_2{}^2})}{(y_1+\sqrt{y_1{}^2+x_1{}^2})\ (y_2+\sqrt{y_2{}^2+x_2{}^2})}, \ . \ . \ (15)$$

where y_1, y_2 must be treated algebraically. Thus ABCD representing the loaded rectangle, DM, CN perpendiculars on Ox, we have, in the case shown in fig. 1:—

$$\frac{dw}{dx}\text{ (at O)}=\frac{(1-\eta)p}{2\pi n}\log\frac{(\mathrm{OD}+\mathrm{DM})(\mathrm{OB}+\mathrm{BN})}{(\mathrm{OA}+\mathrm{AM})(\mathrm{OC}+\mathrm{CN})}; \ . \ (16a)$$

in the case shown in fig. 2:—

$$\frac{dw}{dx}\text{ (at O)}=\frac{(1-\eta)p}{2\pi n}\log\frac{(\mathrm{OD}+\mathrm{DM})(\mathrm{OB}-\mathrm{BN})}{(\mathrm{OA}-\mathrm{AM})(\mathrm{OC}+\mathrm{CN})}. \ (16b)^*$$

---

* An equivalent but somewhat longer form for the logarithm (leading directly, however, to [17]) is given in eqn. (7) art. 818 of Thomson and Tait's 'Natural Philosophy.'

In the case of symmetry shown in fig. 3, where the centre of the rectangle lies on Ox,

$$(\mathrm{OD}+\mathrm{DM})/(\mathrm{OA}-\mathrm{AM})=(\mathrm{OD}+\mathrm{DM})^2/\mathrm{OM}^2, \text{ \&c.},$$

and (16 b) reduces to

$$\frac{dw}{dx}(\text{at O})=\frac{(1-\eta)p}{\pi n}\log\left(\frac{\mathrm{OD}+\mathrm{DM}}{\mathrm{OC}+\mathrm{CN}}\cdot\frac{\mathrm{ON}}{\mathrm{OM}}\right). \quad . \quad . \quad (17)$$

Fig. 1.

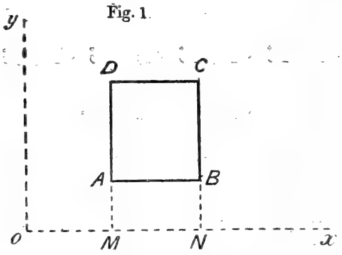


Fig. 2.

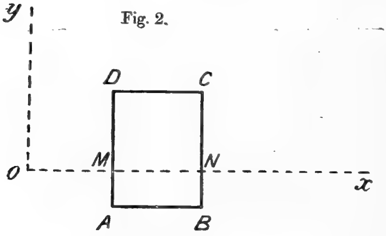
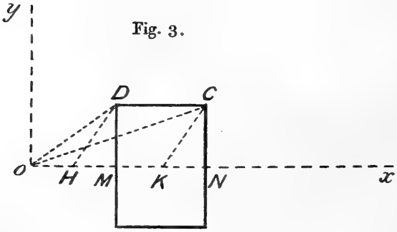


Fig. 3.



In this last case of course $dw/dy=0$, and Ox is the line of greatest slope at O.

If in fig. 3 we draw DH and CK, bisecting the angles ODM, OCN, we easily throw (17) into the elegant form

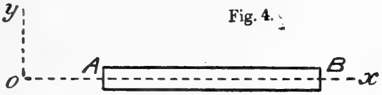
$$\frac{dw}{dx}(\text{at O})=\frac{1-\eta}{\pi n}p\log\left(\frac{\mathrm{NK}}{\mathrm{MH}}\right). \quad . \quad . \quad . \quad (18)$$

It will frequently be possible to divide nearly the whole of a loaded area, not itself rectangular, into a small number of rectangles, so that the results obtained above could doubtless be utilized for obtaining approximate values of the slope in many cases where the loaded area is not rectangular.

Subcases when one Dimension of Loaded Rectangle small.

§ 11. In fig. 4, AB represents an elongated loaded area symmetrical about Ox. If we suppose the breadth $2b$ small

Fig. 4.



compared to the distance OA$=c$, and denote the length AB by $2a$, we easily deduce from (17) as a first approximation

$$\frac{dw}{dx}(\text{at O})=(1-\eta)\text{P}\div 2\pi n\text{OA}\,.\,\text{OB}, \quad . \quad . \quad . \quad (19)$$

where P$=p\,.\,2a\times 2b$ is the total load over the area.

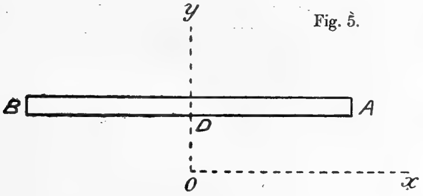
If, further, c be small compared to $2a$, we have

$$\frac{dw}{dx}(\text{at O})=(1-\eta)pb/\pi nc=(1-\eta)\text{P}\div 4\pi nac. \quad . \quad (20)$$

When (20) applies, the slope along the axis of symmetry varies inversely as the shortest distance from the loaded area.

§ 12. In fig. 5, AB represents an elongated area perpendicular to the axis of symmetry Oy.

Fig. 5.



Supposing first that OD($=c$) and the breadth $2b$ are comparable, but both small compared to the length $2a$, we easily find from the formula corresponding to (17)

$$\frac{dw}{dy}(\text{at O})=\frac{(1-\eta)p}{\pi n}\log\frac{c+2b}{c}. \quad . \quad . \quad . \quad (21)$$

If, further, the breadth be small compared to the shortest distance from O, we reduce (21) to

$$\frac{dw}{dy}\text{ (at O)} = 2(1-\eta)pb/\pi nc = (1-\eta)\mathrm{P}/2\pi nac, \quad . \quad (22)$$

where P denotes as before the total load. Comparing (20) and (22) we observe that for equal distances *c*, the position of the loaded area in fig. 5 is twice as effective as the position in fig. 4.

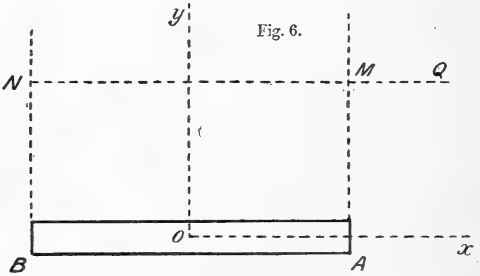


Fig. 6.

§ 13. In fig. 6 the elongated loaded area has its centre at the origin of coordinates, and the axes of Ox and Oy are along its length $2a$ and breadth $2b$ respectively. The slope is required at a point Q(x, y) whose shortest distance from the area is considerable compared to b.

Draw through Q a parallel to AB cutting in M and N the lines AM and BN drawn perpendicular to AB.

From (15) and the corresponding equation we have as first approximations to the components of slope

$$-\frac{dw}{dx} = \frac{(1-\eta)pb}{\pi n}\left(\frac{1}{\mathrm{QA}} - \frac{1}{\mathrm{QB}}\right), \quad . \quad . \quad . \quad (23)$$

$$-\frac{dw}{dy} = \frac{(1-\eta)pb\mathrm{AM}}{\pi n}\left\{\frac{1}{\mathrm{QA}(\mathrm{QA}+\mathrm{QM})} - \frac{1}{\mathrm{QB}(\mathrm{QB}+\mathrm{QN})}\right\} \quad (24)$$

In (24), QM and QN are to be treated algebraically, and the formula must not be applied to cases in which either QA + QM or QB + QN tends to become very small (*cf.* § 11).

Numerical Illustrations.

§ 14. Suppose in the case of symmetry, illustrated by fig. 3, that the loaded area is a square 100 metres in the side, and that OM, its shortest distance from O, is 1 metre.

Suppose the load to arise from a sheet of water 1 cm. deep, or that

$$p=1 \text{ gramme wt.}$$

As in case (iii.) of § 7, let us put

$$\eta=\cdot 5,\quad n=11\times 10^7 \text{ grammes wt. per sq. cm.}$$

Then we have approximately, in absolute measure,

$$\frac{dw}{dx}=(2\pi\times 11\times 10^7)^{-1}\log_e(10100/163)\,;$$

or, as unit angle $=206\times 10^3$ seconds of arc approximately,

$$\text{slope at O}=0''\cdot 0012 \text{ approximately.} \quad . \quad . \quad (25)$$

The result would be the same if the side and the least distance of the loaded square were altered in the same proportion, *e. g.* if the side were altered to 1000 and the shortest distance to 10 metres.

The slope increases directly as the load. It would, however, require an abnormally large differential rainfall or evaporation to appreciably influence by direct pressure a level inside a building situated on strata similar to the material of our calculation.

§ 15. The differential effect of barometric pressure during the approach or retirement of a deep cyclonic depression would appear a more probable disturbing cause. We might very easily have a mean differential excess or diminution of pressure of 1 or 2 cm. of *mercury* over an area whose greatest dimension was very large compared to the shortest distance from the observing station, and consequently effects 10 or 20 times that appearing in (25) might not unreasonably be expected in disturbed weather.

In the case of a large cyclonic area it would be desirable to apply a formula applicable to a loaded *spherical* surface, but (17) would probably give a very fair idea of the order of magnitude of the result.

§ 16. As an illustration of a different kind, suppose in fig. 5 that O is a station near a long straight portion AB of a tidal river, and that we desire the difference of slope at O at high and low tide. It will suffice to take the difference of level at high and low water as the same all along AB. Suppose this difference to be 5 metres, and assume η and n to be the same as in the last example.

Taking first $c=4\times 2b$, we get approximately from (21)

$$\frac{dw}{dy}(\text{at O})=\frac{5\times 10^2\times 7}{2\times 22\times 11\times 10^7}\log_e 10\times\log_{10}(1\cdot 25),$$

or in seconds of arc

$$\text{slope at O} = 0''{\cdot}033. \quad . \quad . \quad . \quad . \quad . \quad . \qquad (26)$$

Taking next $c=2b$, we replace (26) by

$$\text{slope at O} = 0''{\cdot}10. \quad . \quad . \quad . \quad . \quad . \quad . \qquad (27)$$

If, for instance, the river be 100 yards broad, the first station is 400, the second 100 yards from the bank.

§ 17. As rivers are seldom straight, I have supplemented the above by calculating the slope at the centre of a semicircular channel of width $2b$, supposed small compared to the radius R. For a difference h in the level of high and low water, I find as a first approximation

$$\text{difference of slope at centre of semicircle} = 2(1-\eta)gbh/\pi n\text{R}. \qquad (28)$$

To aid the imagination, the river may be supposed to enter and leave the semicircle by straight channels forming continuations of the limiting diameter, so that the semicircular portion alone need be considered.

It will be noticed that (22) and (28) are identical if

$$c=\text{R}, \text{ and } p=gh.$$

In other words, the semicircular tidal river has exactly the same influence on the slope of the station at its centre as it would have if the channel were straight throughout and came within the same distance of the station.

§ 18. The results of the last two paragraphs point to changes of water-level in tidal rivers and estuaries as more likely to appreciably affect the level of neighbouring observatories than any probable differential peculiarities of rainfall or evaporation. In making this observation I exclude of course the direct action of water on the foundations of the building.

In all cases similar to those treated in §§ 14–17 the direct gravitational action of the load must be taken into account to obtain the full result. For instance, in the case of an anticyclone, the horizontal attraction of the surplus air must be considered as well as the excess of pressure over the area covered by the anti-cyclone.

Pressure Effects below the Surface.

§ 19. As the foundations of most buildings are below the ground-level, the slope at some little depth possesses considerable interest. The general formula (14) for the depression at any depth due to a loaded rectangle, though easily evaluated for specified numerical values of z, x, &c., is somewhat complicated. Its general character will be sufficiently comprehended from the results in the case of symmetry, when

$$y_2 = -y_1 = b.$$

Putting $$x_1 = c,\ x_2 = c + 2a,$$

we then convert (14) into

$$\frac{4\pi n}{p}\left(\frac{dw}{dx}\right)_{z=z}^{x=y=0}$$
$$= 2(1-\eta)\log\frac{(b+\sqrt{b^2+c^2+z^2})\,(-b+\sqrt{b^2+(c+2a)^2+z^2})}{(-b+\sqrt{b^2+c^2+z^2})\,(b+\sqrt{b^2+(c+2a)^2+z^2})}$$
$$+ 2bz^2\{(c^2+z^2)^{-1}(b^2+c^2+z^2)^{-\frac{1}{2}}$$
$$-((c+2a)^2+z^2)^{-1}(b^2+(c+2a)^2+z^2)^{-\frac{1}{2}}\}. \quad . \quad . \quad (29)$$

So long as z/c is small the right-hand side of (29) can be expanded in a rapidly converging series of the form

$$A + B(z/c)^2 + \ldots,$$

where A and B are independent of z.

There is no tendency in B/A to become very large for finite values of a/c and b/c.

When we neglect $B(z/c)^2$ &c., we simply get the slope at the surface. We thus see that at depths small compared to the shortest distance of the loaded area the slope is nearly the same as at the surface itself.

The value of B is easily obtained in special cases. As an example, take the sub-case of fig. 4, in which c/a and b/c are both supposed small. We then get for the slope

$$\left(\frac{dw}{dx}\right)_{z=z}^{x=y=0} = \frac{pb}{\pi nc}\left(1-\eta+\tfrac{1}{2}\eta\frac{z^2}{c^2}\right). \quad . \quad . \quad . \quad (30)$$

In (30) constant terms of the order $(b/c)^3$ are omitted, though possibly more important than the variable term.

Again, in the sub-case of fig. 5, when b/c and c/a are supposed both small, we find

$$\left(\frac{dw}{dy}\right)_{z=z}^{x=y=0} = \frac{2pb}{\pi nc}\left(1-\eta+\eta\frac{z^2}{c^2}\right). \quad . \quad . \quad . \quad (31)$$

In both these instances the slope *increases* with the depth. The formulæ hold only so long as z/c is small, so that the phenomenon is rather of theoretical than practical importance. Though somewhat opposed to *à priori* conceptions, this result would not appear exceptional. Thus, take the case of an isolated load $p\,d\omega$ at a point P on the surface, and consider the vertical displacement at a point Q at depth z. Join $QP = r$, and draw $QM = x'$ perpendicular on the vertical PM. Then denoting the angle QPM by α, we have at Q

$$w = (p\,d\omega/4\pi n r)\{2(1-\eta) + \cos^2\alpha\}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (32)^*$$

$$= (p\,d\omega/4\pi n x')\{2(1-\eta)(1+z^2/x'^2)^{-\frac{1}{2}} + (z/x')^2(1+z^2/x'^2)^{-\frac{3}{2}}\}.$$

Thus when z/x' is small, we find, neglecting powers of z/x' above the second,

$$w = (p\,d\omega/2\pi n x')\{1-\eta+\tfrac{1}{2}\eta(z/x')^2\}; \quad . \quad . \quad . \qquad (33)$$

whence

$$-\frac{dw}{dx'} = (p\,d\omega/2\pi n x'^2)\{1-\eta+\tfrac{3}{2}\eta(z/x')^2\}. \quad . \quad . \qquad (34)$$

Here the slope $-dw/dx'$ increases at first with the depth as in the case of (30) and (31).

§ 20. When the depth is of the same order of magnitude as the horizontal distance of the nearest point of the loaded area, individual cases of (14) or (29) require separate consideration.

When the depth becomes large compared to the horizontal distance of the remotest point of the loaded area, we easily find from (14) as a first approximation

$$\left(\frac{dw}{dx}\right)^{x=y=0}_{z=z} = \frac{(5-2\eta)p}{8\pi n}\,\frac{(y_2-y_1)(x_2{}^2-x_1{}^2)}{z^3}, \quad . \qquad (35)$$

showing that the slope now diminishes as the inverse cube of the depth.

If P denote the total load, $\bar{x}, \bar{y}$ the coordinates of the C.G. of the loaded area, we have at once from (35)

$$\left(\frac{dw}{dx}\right)^{x=y=0}_{z=z} = (5-2\eta)\mathrm{P}\bar{x}/(4\pi n z^3), \quad . \quad . \quad . \qquad (36)$$

and by symmetry

$$\left(\frac{dw}{dy}\right)^{x=y=0}_{z=z} = (5-2\eta)\mathrm{P}\bar{y}/(4\pi n z^3). \quad . \quad . \quad . \qquad (37)$$

The line of greatest slope is thus in the vertical plane which contains the C.G. of the loaded area, and if R be the horizontal distance of the C.G., the slope is given by

$$(dw/d\mathrm{R})_{z=z} = (5-2\eta)\mathrm{PR}/(4\pi n z^3). \quad . \quad . \quad . \qquad (38)$$

The conditions assumed in (35) are practically tantamount to those of the elementary loaded area, and (38) can in fact be deduced from (32) by supposing α small.

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* Todhunter & Pearson's 'History,' vol. ii. eqn. (xxiv.) of art. 1497.

Luni-Solar Effects[*].

§ 21. Another possible cause affecting the indications of pendulums and spirit-levels is the gravitational action of the heavenly bodies, especially the sun and moon. If we regard the earth as a sphere of mass E and radius a, and suppose the moon's mass to be M and its distance from the earth R, there exists in the earth a system of bodily forces of which the principal come from a potential

$$g(\mathrm{M/E})(a/\mathrm{R})^3(r^2/a)(3\cos^2\theta-1)/2, \quad . \quad . \quad . \quad (39)$$

where g is "gravity" at the earth's surface, neglecting "centrifugal force." The moon is supposed to lie in the line $\theta=0$, the earth's centre being origin, and r, θ ordinary polar coordinates. As explained in art. 812 of Thomson and Tait's 'Natural Philosophy,' there results at the earth's surface a component force radially outwards

$$g(\mathrm{M/E})(a/\mathrm{R})^3(3\cos^2\theta-1),$$

and a component along the tangent

$$\mathrm{T}'=3g(\mathrm{M/E})(a/\mathrm{R})^3\sin\theta\cos\theta, \quad . \quad . \quad . \quad . \quad (40)$$

directed towards the point under the moon ($\theta=0$).

Both components being small compared to g, the direction of gravity is, owing to the direct attraction alone, deflected through the angle

$$\delta\psi'=\tan^{-1}\{3(\mathrm{M/E})(a/\mathrm{R})^3\sin\theta\cos\theta\} \quad . \quad . \quad (41)$$

from the vertical. The angle being very small may be replaced by its tangent.

Thomson and Tait suppose

$$(\mathrm{M/E})(a/\mathrm{R})^3=10^{-5}/182, \quad . \quad . \quad . \quad . \quad (42)$$

and thence draw the following conclusion:—"the plummet is deflected towards the point of the horizon under either moon ($\theta=0$) or antimoon ($\theta=\pi$), by an amount which reaches its maximum value ... $0''\cdot017$ when the altitude is 45°." They add—"The corresponding effects of solar influence are of nearly half these amounts." According to this conclusion direct luni-solar influence should make itself felt in any system of pendulum or spirit-level observations in which the accuracy is of the order $0''\cdot02$.

§ 22. The data on which the above calculation is based are pretty accurately known, which constitutes a reason for

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[*] Strictly the problem is a dynamical one; as yet only an "equilibrium" solution is available.

treating the direct effect by itself. It must, however, be borne in mind that the luni-solar influence is not confined to the pendulum bob, but extends to the material of the earth itself. Consequently the result calculated by Lord Kelvin and Prof. Tait is part only of a composite effect, which there is no very obvious way of analysing in practice into its components.

In the actual earth the most obvious consequence of luni-solar action is the ocean tides, and, as we saw in § 4, any station near the sea-shore has its apparent level affected by these in two distinct ways. Even at an inland station ocean tides must exert some influence, though presumably it is very small.

In addition, however, to ocean tides there must be tides in the earth's mass, whether solid throughout or not, and it is to these I shall now call attention.

The potential term (39) is only one of a series. The numerical values of the coefficients diminish rapidly as the order of the harmonic increases ; still it is desirable not wholly to neglect the higher harmonics, if only to make sure that the comparative smallness of the disturbing forces answering to them is not compensated in any instance by great effectiveness. I shall thus consider in the first place the results of the general problem when the degree of the harmonic appearing in the disturbing forces is unrestricted, making use of the results contained in a paper communicated to the Cambridge Philosophical Society[*] in 1887.

§ 23. Before entering, however, on this investigation, it is desirable to consider briefly the relation between the results of theory and the phenomena we may expect to encounter in direct observation.

Surface-points on the undisturbed surface, regarded as spherical, transform into surface-points on the strained surface ; thus a very small surface-area, *e. g.* a square decimetre, may be regarded as a tangent plane in both conditions.

Suppose, now, this area to have rigidly attached to it a spirit-level, consisting of part of a circular arc filled with liquid and with a minute air-bubble. In the undisturbed condition suppose the bubble exactly at the central division O of the arc, while in the disturbed condition it is at an angular interval $\delta\theta$ from O. This an observer would naturally attribute to a change $\delta\theta$ of level. The true interpretation is that in the disturbed condition the resultant of the forces at the surface makes with the normal the angle $\delta\theta$. In a rigid earth $\delta\theta$ would be the angle of Thomson and Tait's calcula-

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[*] See the Society's Transactions, vol. xiv. p. 278.

tion; but in an elastic earth allowance must be made for the fact that the attraction of the disturbed earth is not along the normal.

The effect on astronomical observations is still more complicated. Thus let an observer take the altitude of a star in the same vertical plane as the moon, using a mercury surface for his horizontal plane. The observed altitude will differ from the theoretical—*i. e.* the true altitude if the disturbing influence were absent—by an amount equal to the angle between the disturbed and undisturbed mercury surfaces. This is the algebraical sum of the inclination of the resultant gravitational force to the radius-vector in the disturbed condition and of the inclination of this radius-vector to its undisturbed position.

This explanation will show what the quantities are of which we require to know the theoretical values.

§ 24. To return to the problem. The earth is treated as truly spherical when undisturbed, "centrifugal force" being neglected, and as posssesed when disturbed of uniform density ρ, and of uniform isotropic elastic qualities throughout, determined by the elastic constants m, n.

The assumption of natural sphericity and the neglect of the centrifugal force answer merely to the neglect of small quantities of the second order of magnitude relative to those of the first; the other assumptions have been discussed in § 2. In our ultimate applications the material will be supposed incompressible, i. e. $n/m=0$, but it is undesirable to introduce unnecessary limitations in the mathematical results themselves. Further, absolutely incompressible material is merely a mathematical fiction, so it is desirable to have the means ready to hand to apply a correction to mathematical results based on such an hypothesis.

Supposing the typical term in the potential of the disturbing forces to be

$$r^i \mathrm{V}_i' \sigma_i, \quad \dots \dots \quad (43)$$

where σ_i is a known surface-harmonic of degree i, and V_i' a given numerical magnitude, we easily see that the equation to the strained surface will take the form

$$r = a + \textstyle\sum a_i \sigma_i. \quad \dots \dots \quad (44)$$

At the present stage all we know is that a_i is small compared to a, the mean radius of the strained surface.

The bodily forces consist in part of the disturbing forces, but mainly of the self-gravitational action of the "earth."

The complete value of the potential V is given by

$$V=-\tfrac{1}{2}ga^{-1}r^2+\sum 3g(r/a)^i a_i\sigma_i/(2i+1)+\sum r^i V_i'\sigma_i{}^{*}. \quad . \quad (45)$$

In ordinary circumstances we are supposed to be given the unstrained surface, with full information as to the force system, and it is customary to regard the surface equations as applying to the unstrained surface. In the present instance—and I daresay as a rule in practice—the forces depend to some extent on the disturbed form of the body. It is thus convenient, to say the least of it, to suppose that the surface equations apply in the present instance to the disturbed surface. This implies nothing more serious than the replacing the ordinary definition of strain, viz.

(final length—initial length)/(initial length),

by

(final length—initial length)/(final length).

The two definitions are equivalent so long as it is justifiable to apply the mathematical theory, which assumes the square of a strain negligible[†].

§ 25. The problem whose results I am about to use was more general than the one at present before us, inasmuch as the surface was not assumed to be naturally spherical. The notation employed in its solution was also somewhat different, the potential being given in the form

$$V=-\tfrac{1}{2}ga^{-1}r^2+\sum a_i V_i\sigma_i r^i. \quad . \quad . \quad . \quad . \quad (46)$$

Thus in utilizing the results we must put

$$V_i=3ga^{-i}/(2i+1)+V_i'/a_i. \quad . \quad . \quad . \quad . \quad (47)$$

In the general problem V_i was unrestricted, but I contented myself with giving the two arbitrary constants a_iY_i, a_iZ_i explicitly in terms of σ_iV_i and $ga^{-i}a_i$. The expressions for the displacements freed from arbitrary constants were given (*l. c.* equations (13) to (15), pp. 280, 281) only for the case when

$$V_i=3ga^{-i}/(2i+1),$$

or when V_i' in (47) is zero.

It is easy, however, to add the terms containing V_i'. For if in the equations (11) and (12) of p. 280, *l. c.*, we substitute for V_i the right-hand side of (47) and multiply up by $a_i\sigma_i$, we notice that $V_i'\sigma_i$ appears with the same coefficients as V possessed in the earlier equations (32) and (33) (*l. c.* p. 264), which determined the unknowns Y_i and Z_i—treated in that

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[*] See Prof. G. H. Darwin, Phil. Trans. 1879, Part I.

[†] See Phil. Mag. Sept. 1891, pp. 246–7.

instance as each the combined product of surface harmonic and arbitrary coefficient—for a perfect sphere acted on by bodily forces. Thus for terms in V_i' in the displacements, we have only to take the results (36), (37), and (38), *l. c.* pp. 264–265, and in them replace V_i by $V_i'\sigma_i$ and S_i by zero.

Doing so, we find for the components of displacement, measured respectively in the directions of the fundamental polar elements dr, $r\,d\theta$, $r\sin\theta\,\delta\phi$, the following results :—

$$\begin{aligned} u= & \frac{g\rho r}{10a(m+n)}\left\{r^2-\frac{a^2(5m+n)}{3m-n}\right\} \\ & +g\rho\Sigma\frac{a_i\sigma_i}{2n(2i+1)D_i}\big[r^{i+1}a^{-i}\{10m^2i(i^2-1) \\ & \qquad -mn(4i^4+4i^3+34i^2+29i+10)+n^2(8i^3+8i^2+13i-2)\} \\ & -r^{i-1}a^{-i+2}i\{10m^2i(i+2)-mn(4i^3+4i^2-2i+1)-n^2(4i-3)\}\big] \\ & +\frac{5(m+n)}{2n}\Sigma\frac{\rho i V_i'\sigma_i}{D_i}\left[a^2r^{i-1}i\frac{m(i+2)-n}{i-1}-r^{i+1}\{m(i+1)-n\}\right], \end{aligned} \qquad (48)$$

$$v=\frac{d}{d\theta}\Sigma\Psi_i,\quad w=\frac{1}{\sin\theta}\frac{d}{d\phi}\Sigma\Psi_i; \quad . \quad . \quad . \qquad (49)$$

where

$$D_i=5(m+n)\{m(2i^2+4i+3)-n(2i+1)\}, \quad . \quad . \qquad (50)$$

$$\begin{aligned} \Psi_i= & \frac{g\rho a_i\sigma_i}{2n(2i+1)D_i}\big[r^{i+1}a^{-i}\{10m^2(i-1)(i+3) \\ & \qquad -mn(4i^3+12i^2-6i+17)-n^2(8i^2-17)\} \\ & -r^{i-1}a^{-i+2}\{10m^2i(i+2)-mn(4i^3+4i^2-2i+1)-n^2(4i-3)\}\big] \\ & +\frac{5(m+n)\rho V_i'\sigma_i}{2nD_i}\left[a^2r^{i-1}\frac{i\{m(i+2)-n\}}{i-1}-r^{i+1}\{m(i+3)-n\}\right]. \end{aligned} \qquad (51)$$

§ 26. Before utilizing these results we must determine a_i in terms of V_i', which is easily done as follows :—The surface being supposed originally spherical, the terms $\Sigma a_i\sigma^i$ in (44) arose solely from the action of the disturbing forces, and so must be identical with the variable terms in the surface-value of u.

Thus writing $a+\Sigma a_i\sigma_i$ for r in the principal terms in (48), and a for r in the subsidiary, then equating the separate harmonic terms to the corresponding ones in $\Sigma a_i\sigma_i$ and reducing, we find

$$a_i=\frac{\rho a^{i+1}V_i' i\{(2i+1)m-n\}\div\{2(i-1)n((2i^2+4i+3)m-(2i+1)n)\}}{1+\dfrac{g\rho a}{n}\,\dfrac{15i(2i+1)m^2-(8i^3+6i^2-2i-9)mn+(4i^3-2i^2-3i-3)n^2}{5(2i+1)(3m-n)\{(2i^2+4i+3)m-(2i+1)n\}}}. \qquad (52)$$

If the self-gravitation were negligible, the denominator in (52) would become unity, the numerator remaining unchanged. Thus self-gravitation reduces the change of form produced by the disturbing forces depending on the harmonics of degree i in the ratio

$$1:1+\frac{g\rho a}{n}\,\frac{15i(2i+1)m^2-(8i^3+6i^2-2i-9)mn+(4i^3-2i^2-3i-3)n^2}{5(2i+1)(3m-n)\{(2i^2+4i+3)m-(2i+1)n\}}. \quad (53$$

If $$n/m=0,$$

or the material be incompressible, (52) reduces to

$$a_i=\frac{\rho a^{i+1}V_i'i(2i+1)/\{2(i-1)(2i^2+4i+3)n\}}{1+(g\rho a/n)i/(2i^2+4i+3)}, \quad . \quad . \quad (54)$$

and the ratio (53) becomes

$$1:1+(g\rho a/n)i/(2i^2+4i+3). \quad . \quad . \quad . \quad (55)$$

When $i=2$, (52) becomes

$$a_2=\frac{\rho a^3 V_2'(5m-n)/\{n(19m-5n)\}}{1+3(g\rho a/n)(10m^2-5mn+n^2)/\{5(3m-n)(19m-5n)\}}. \quad (56)$$

A result equivalent to (56), with the notation

$$\lambda=m-n, \qquad \mu=n,$$

was given by Prof. Karl Pearson, in Todhunter and Pearson's 'History,' vol. ii. part 2, p. 425. An obvious misprint of 4μ for 14μ occurs, however, in the denominator of his formula.

§ 27. We may safely assume $m-n$ positive, so the numerator in (52) has clearly the same sign as V_i'; also for a given value of V_i' it diminishes as i increases. Thus so long as the denominator in (52) exceeds unity there is no risk lest the relative smallness of the forces proceeding from any higher harmonic may be compensated in any way. It is obvious, however, that the coefficient of $g\rho a/n$ in the denominator can be made negative by taking i large enough, for ordinary values of n/m. For instance, if $n/m=1/2$,—the hypothesis of uniconstant isotropy,—the coefficient of $g\rho a/n$ is negative when i exceeds 9, and with i infinite the denominator as a whole would vanish and change sign as n passed through the value $6g\rho a/100$.

If we take as before

$$g\rho a=35\times10^3 \text{ grammes wt. per sq. cm.,}$$

this critical value of n has the very ordinary value 21×10^7 grammes wt. per sq. cm.

Thus if such a value as $m/n=2$ were admissible the contingency of a_i/V_i' becoming enormously large for a high value of i would be quite a possible one. Unless, however, as I have previously pointed out, n/m be very small, the term in u, independent of the angular coordinates, would in a body of the earth's mass be enormously greater than is consistent with the mathematical theory of elasticity. Therefore, so long as the present calculation is justifiable, the denominator in the value of a_i/V_i' can differ but little from that occurring in (54), and we are thus thoroughly justified in neglecting all the higher harmonic terms in the potential relative to the term containing the second harmonic.

§ 28. As a small departure of n/m from 0 would exercise but little influence on numerical values, it will be best, as we are dealing with data so uncertain, to neglect n/m altogether.

Thus, putting

$$i=2,\quad \sigma_2=P_2,\quad n/m=0,$$
$$V_2'=g(M/E)(a^2/R^3), \quad . \quad . \quad . \quad . \quad . \quad (57)$$

we have for the displaced surface

$$r=a+a_2P_2, \quad . \quad . \quad . \quad . \quad . \quad . \quad (58)$$

where

$$a_2/a=\frac{5}{19}(g\rho a/n)(M/E)(a/R)^3\div\left\{1+\frac{2}{19}(g\rho a/n)\right\}. \quad (59)$$

An equivalent result is given in Thomson and Tait's 'Natural Philosophy,' art. 840. The result will also be found, along with that answering to $i=2$, $m=2n$, in Mr. Love's 'Treatise on Elasticity,' vol. i. pp. 302, 303.

The corresponding surface displacements are

$$u=\frac{5}{19}aP_2(g\rho a/n)(M/E)(a/R)^3/\{1+2g\rho a/19n\}, \quad (60)^*$$

$$v_a=-\frac{9}{38}a\sin\theta\cos\theta(g\rho a/n)(M/E)(a\ R)^3/\{1+2g\rho a/19n\}. (61)^*$$

The term in u independent of the angular coordinates absolutely vanishes for $n/m=0$, and both components of the surface displacement, and so the resultant displacement itself, are reduced owing to the self-gravitation in the common ratio

$$1:1+2g\rho a/(19n). \quad . \quad . \quad . \quad . \quad (62)^*$$

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* The material being as here incompressible, it may be proved that for any value of i in (45) the *displacements* are everywhere the same as in a *sphere* of radius a, over whose surface act purely normal tractions equal to $\rho a^i\sigma_i V_i'\div\{1+(g\rho a/n)i/(2i^2+4i+3)\}$.

The angle through which the radius-vector is rotated from its undisturbed position, in the direction away from $\theta=0$, is equal to v_a/a, and so is known from (61). As v_a/a is negative for all values of θ between 0 and $\pi/2$, this rotation is really *towards* the moon at every point of the illuminated hemisphere.

§ 29. We next require to find the inclination of the resultant force to the radius-vector over the surface.

Employing (57) and (59) in (45) we find for the complete value of the potential

$$V=-\tfrac{1}{2}g(r^2/a)[1-2P_2(M/E)(a/R)^3(1+5g\rho a/19n) \div(1+2g\rho a/19n)]. \quad . \quad (63)$$

The component forces along and perpendicular to the radius-vector are

$$\overline{R}=\frac{dV}{dr}, \qquad \overline{\Theta}=\frac{1}{r}\frac{dV}{d\theta}.$$

Thus at a point on the surface the principal terms, which alone we require, are

$$\overline{R}=-g,\ \overline{\Theta}=-3g\sin\theta\cos\theta(M/E)(a/R)^3(1+5g\rho a/19n) \div(1+2g\rho a/19n), \quad . \quad (64)$$

and the inclination of the resultant force to the radius-vector is to a first approximation

$$\delta\chi=\overline{\Theta}/\overline{R}=3\sin\theta\cos\theta(M/E)(a/R)^3(1+5g\rho a/19n) \div(1+2g\rho a/19n). \quad . \quad (65)$$

For the apparent change of altitude, $\delta\alpha$, in a star we have, as already explained in § 23,

$$\delta\alpha=\delta\chi+v_a/a=3\sin\theta\cos\theta(M/E)(a/R)^3\left\{1+\frac{7}{38}(g\rho a/n)\right\} \div(1+2g\rho a/19n). \quad . \quad (66)$$

§ 30. For the apparent change of level $\delta\psi$, we require the inclination of the resultant force to the normal. To obtain this we may employ the result (65) in conjunction with the inclination $\delta\psi_1$ of the normal to the radius-vector, the latter being given to a first approximation by

$$\delta\psi_1=\left(-\frac{1}{r}\frac{dr}{d\theta}\right)_{r=a}=\frac{15}{19}\sin\theta\cos\theta(g\rho a/n)(M/E)(a/R)^3 \div(1+2g\rho a/19n). \quad (67)$$

Thus we have finally

$$\begin{aligned}\delta\psi &= \delta\chi - \delta\psi_1 \\ &= 3\sin\theta\cos\theta(\mathrm{M}/\mathrm{E})(a/\mathrm{R})^3 \div (1+2g\rho a/19n). \qquad (68)\end{aligned}$$

This result can also be got by noticing that

$$\begin{aligned}\delta\psi &= \tan^{-1}(\mathrm{T}/g), \\ &= \mathrm{T}/g, \text{ to a first approximation,}\end{aligned}$$

where

$$\begin{aligned}\mathrm{T} &\equiv \overline{\mathrm{R}}\sin\delta\psi_1 - \overline{\Theta}\cos\delta\psi_1 \\ &= 3g\sin\theta\cos\theta(\mathrm{M}/\mathrm{E})(a\mathrm{R})^3 \div (1+2g\rho a/19n) \quad . \quad (69)\end{aligned}$$

is the tangential component of the surface-force.

Comparing (68) with (41) we have

$$\delta\psi : \delta\psi' :: 1 : 1+2g\rho a/19n, \quad . \quad . \quad . \quad (70)$$

or the self-gravitational forces reduce the apparent change of level, as calculated in Thomson and Tait's 'Natural Philosophy,' in precisely the same ratio as they reduce the ellipticity of the surface.

This last result might probably be deduced at once from the fact that u_a and v_a are reduced in the same proportion, but I have preferred an explicit mathematical proof.

Comparing (66) and (68), we have

$$\delta\alpha/\delta\psi = 1+\frac{7}{4}(2g\rho a/19n), \quad . \quad . \quad . \quad . \quad (71)$$

showing that the apparent change in star's altitude—the star being, it will be remembered, in the same vertical plane with the moon—is always in excess of the apparent change of level.

Numerical Estimates.

§ 31. As before, we shall take

$$\begin{aligned}a &= 64\times10^7, \\ g\rho a &= 35\times10^8 \text{ grammes wt. per sq. cm.,} \\ (\mathrm{M}/\mathrm{E})(a/\mathrm{R})^3 &= 1/(182\times10^5).\end{aligned}$$

We shall consider only the greatest and least values of n specified in § 7, exhibiting the results side by side ; θ, it will be remembered, is measured from the line joining the centres of the earth and moon.

Numerical Results: Lunar Influence.

$n =$	80×10^7 grammes wt. per sq. cm.	11×10^7 grammes wt. per sq. cm.
$2g\rho a/19n =$	35/76	700/209
$1 : 1+2g\rho a/19n$ (approx.) $=$	11 : 16	3 : 13
$u_a =$	$28\frac{3\cos^2\theta-1}{2}$ cms.	$68\frac{3\cos^2\theta-1}{2}$ cms.
$v_a =$	-2 [illegible] $\theta\cos\theta$ cms.	$-61\sin\theta\cos\theta$ cms.
Polar less equatorial radius $=$	[illegible] cms.	102 cms.
Apparent change level, $\delta\psi =$	$0''\cdot$[illegible] $\sin 2\theta$	$0''\cdot004\sin 2\theta$
Apparent change of star's altitude, δa $=$	$0''\cdot021\sin 2\theta$	$0''\cdot027\sin 2\theta$

The reduction effected by the self-gravitational forces in Thomson and Tait's estimate, $0''\cdot017\sin 2\theta$, for the apparent change of level increases conspicuously as the rigidity diminishes. In fact, for the lower value of n, $\delta\psi$ would be insensible unless with an instrument recording to $\frac{1}{250}$ of a second of arc.

On the other hand, the changes in the shape of the earth and in the star's apparent altitude are very decidedly larger for the lower value of n.

Corresponding results of about half the numerical size of the above would be obtained in the case of solar influence.

Final Conclusions.

§ 32. The results obtained indicate at least the directions in which luni-solar effects may be profitably looked for. If the earth's elasticity for luni-solar influence be perfect, apparent changes of level or star's altitude will be *nil* when the moon or sun, as the case may be, is either in the zenith or on the horizon, while they will be a maximum when the altitude is 45°. If the elasticity be not perfect, a lag in the tides may be expected. As regards star's altitude, a hopeful feature is that the influence, being the same for all stars in the same vertical, should be easily separable from terrestrial refraction. The further fact that the apparent change is proportional to the cosine of the star's azimuth measured from the vertical plane containing the moon, or sun, may prove of assistance.

It would appear that luni-solar effects are not unlikely to prove of as much consequence as the direct pressure or gravitational effects of any ordinary differential meteorological action in the neighbourhood of an observatory, though not nearly so important as ocean or estuary tides for observatories situated within a few hundred yards of high-water-mark.

The considerable fluctuation of the calculated luni-solar effects with the value ascribed to the earth's rigidity may lead eventually to interesting speculations as to the state of the earth's interior.

Subsidiary Remarks.

§ 33. Whilst attention has been confined to surface-pressure and luni-solar action, it is not intended to imply the non-existence of other agents capable of producing similar phenomena. The sun's direct heating effect is doubtless in some cases a most effective agent in altering the level. *A priori* one would expect a diurnal variation from this cause, most sensible at stations on rocky ground exposed to the south.

§ 34. Before quitting the subject, it is desirable to consider what light existing seismological data throw on the credibility of the hypothetical theory adopted.

It appears pretty generally believed that wave-velocities calculated from observations near and distant from the epicentre of an earthquake are usually different, and the existence of at least two widely different wave-velocities seems on some occasions well established at the distant stations. One of the two wave-velocities has been regarded (on, I think, mistaken grounds) as postulating an elasticity incredibly high for an elastic solid medium.

These phenomena are easily reconciled with the elastic solid hypothesis. When waves travel between two distant points through the interior of a sphere of large radius they may be expected to behave much as if the medium were infinite. Now in an infinite isotropic medium*, as is well known, there are two wave-velocities, v_1 and v_2, given in our previous notation by

$$v_1 = \sqrt{(m+n)/\rho} \quad v_2 = \sqrt{n/\rho}.$$

Thus, under the conditions supposed, we should expect two earthquake-waves with velocities similar to v_1 and v_2. For definiteness, suppose that the velocities are actually v_1 and v_2,

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* See, for instance, Love's 'Treatise on Elasticity, vol. i. pp. 133, 134.

and suppose them to be respectively 12·5 and 2·5 kilometres per second, this appearing a fair estimate.

Then, in absolute C.G.S. measure,

$$\sqrt{(m+n)/\rho} = 125 \times 10^4, \quad \sqrt{n/\rho} = 25 \times 10^4.$$

Taking $\rho = 5{\cdot}5$ for the earth, we have the approximate results,

$$n = m/24 = 35 \times 10^7 \text{ grammes wt. per sq. cm.}$$

For E, Young's modulus, and k, the bulk modulus (resistance to compression), we have similarly,

$$\begin{aligned} E &\equiv n(3-n/m) &= 10 \times 10^8 \text{ grammes wt. per sq. cm.,} \\ k &\equiv m-n/3 &= 83 \times 10^8 \quad " \quad " \quad " \end{aligned}$$

The rigidity and Young's modulus—the quantities from whose magnitudes our conception of a material's elasticity is usually derived—are in no ways remarkable, being much below the average magnitude observed in iron. The only abnormal feature is the enormous resistance to compression. Any one, however, who considers the enormous pressures presumably in continuous operation on the earth's deep-seated material, will appreciate the probability that it responds uncommonly little to any slight increase in pressure.

A difference between the velocities calculated at stations near and distant from the epicentre is only what we should expect. Lord Rayleigh * has shown that waves with a velocity somewhat less than $\sqrt{n/\rho}$ may be propagated through the material close to the surface of a medium bounded by an infinite plane; and a similar phenomenon may be expected in a sphere, so long at least as the distance from the epicentre is small compared to the radius. In such waves the velocity must depend mainly on the density and elastic properties of the surface material, which in general must differ largely from the corresponding quantities in the deep-seated material. Thus the velocities calculated from the observed effects must depend largely on whether the waves propagated along the surface or those propagated through the interior are the dominant ones; in other words, on whether the distance of the station from the epicentre is or is not small compared to the earth's radius.

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* Proc. London Math. Soc. vol. xvii. (1886). See also Love's 'Treatise,' vol. i. pp. 328–330.

XXIX. *The Spontaneous Change of Oxygen into Ozone and a Remarkable Type of Dissociation.* *By* WILLIAM SUTHERLAND[*].

IN 1886, in connexion with a physiological inquiry, Bohr (Wied. *Ann.* xxvii.) came across a singular discontinuity in the behaviour of rarefied oxygen, as well as a very pronounced departure from Boyle's law; between 14° and 11·4° C. the discontinuity occurs at a pressure of about ·7 millim. of mercury, above which Boyle's law is replaced by $(p+\cdot 109)\mathrm{B}=k$, which was experimentally tested up to 15 millim., and below ·7 millim. the pressure relation is $(p+\cdot 07)\mathrm{B}=k'$, where $k'=1\cdot 045k$, this being experimentally verified down to ·1 millim.; thus the discontinuity consists in a change of volume from $k/\cdot 809$ to $k'/\cdot 77$ at a constant pressure of ·7 millim. It is to be remembered that Bohr's pressures were all directly read with a cathetometer from the difference of level of mercury in two wide tubes, so that there is no question of the departure from Boyle's law being only an apparent one, due to a fallacious step in a train of reasoning. At a pressure of ·07 millim. the departure from Boyle's law is no trifle, for a reliable M'Leod gauge worked on the assumption of Boyle's law would make a true pressure of ·07 millim. appear to be twice as large.

We have already seen in "Thermal Transpiration and Radiometer Motion"[†] that in Crookes's study of radiometer repulsion for different gases with his torsion radiometer, oxygen at a pressure about ·76 millim. or $1000/10^6$ atmo exhibits a remarkable difference from other gases, showing a deflecting force 12 times as great as that of N_2 or CO_2, and 6 times as great as that of CO, and the anomaly continues till a pressure of about $300/10^6$ to $200/10^6$ atmo in Crookes's list of pressures. Here are the complete results for the anomalous region, with the addition of what the deflecting force ought to be according to (26) of "Thermal Transpiration and Radiometer Motion" with the values of c', A$''$, and B$'''$ there given for oxygen, as derived from the region of lower pressures, where all is apparently regular; the so-called pressures are given in terms of $1/10^6$ atmo as unit.

p	1000	803	658	623	613	360	297	190
10^4 log. dec....	1102	1093	1088	1086	1085	1070	1058	1038
Defl. force......	12	12	13	13	13	13	14	20
Defl. force cal.	4·2	5·2	6·3	6·6	6·7	11·1	13·3	19·8

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[*] Communicated by the Author.

[†] Phil. Mag. vol. xlii.

Of course, Crookes in working his M'Leod gauge to get the pressures just given, had no knowledge of Bohr's subsequent discovery of the failure of Boyle's law in oxygen at low pressures, so that the numbers given in the first row as the pressures are not the true pressures; though starting nearly true at 1000 they end up at 190, nearly twice as large as Bohr's formula would give. But, as has been pointed out in "Two New Pressure-Gauges"*, this erroneous yield of pressures does not affect our application of them in our formulæ for log. dec. and deflecting force, because in these formulæ p merely replaces $1/B$, and what the M'Leod gauge really gives is $1/B$, whether Boyle's law holds or not; if Boyle's law holds the values of $1/B$ are given in such a unit that they are equal to p, but if Boyle's law fails, though they no longer give p, they give $1/B$ in an arbitrary unit. Thus, for example, according to the formula for log. dec.

$$\{(L-\mu)/(l-\mu)-1\}$$

is proportional to λ, the mean free path, which is directly as the volume B, so that

$$\left(\frac{L-\mu}{l-\mu}-1\right)/B=2a\lambda_0/B_0D,$$

wherein Crookes's values of p may be taken as correct relative values of $1/B$, so that we can form the products

$$\{(L-\mu)/(l-\mu)-1\}/B$$

from the values of l, the log. dec. in the last table, taking L as ·1120 and μ as ·004, and $1/B$ as given by p; thus for $2a\lambda_0/B_0D$ we have the values 17, 21, 19·7, 20·6, 20·9, 17·3, 18·1, and 15·6, which for a pure gas ought all to be the same, but as l is not very different from L at the higher pressures we cannot expect accurate constancy, but can state that there is no disturbance in $2a\lambda_0/B_0D$ at all comparable to the disturbance in the deflecting force. To assure ourselves of this we will trace the values of $2a\lambda_0/B_0D$ down to the lowest pressures.

p ……	171	110	82	70	48	31	28	22	16	12	4	1·6	·3
$10^4\,l$…	1033	988	940	912	840	744	724	670	621	585	433	348	302
	14·9	15·3	16·4	16·8	16·4	16·5	16·2	15·7	13·8	11·8	7·0	4·0	·94

These show that $2a\lambda_0/B_0D$ has an approximately constant value at about 16 down to about a so-called pressure of $20/10^6$ atmo, below which there is a rapid diminution.

Being now in possession of the essential facts connected

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* *Suprà*, p. 83.

with the abnormality of oxygen at low pressures we can consider the theory of it. Bohr's results mean that pB decreases with increasing volume B, and, according to the kinetic theory, $pB = Nmv^2/3$, where N is the number of molecules in volume B; now it is not necessary that the molecules should be all alike, for whatever m may be $mv^2/2$ is the same for all molecules at the same temperature; thus then the diminution of pB with increasing B discovered by Bohr would be most simply explained by supposing N to diminish with increasing B, seeing that mv^2 must remain constant, that is to say, by supposing association or combination of the oxygen-molecules to occur so as to produce molecules of the composition O_{2x}. Let there be N_1 molecules of O_2 and N_2 of O_{2x}, then

$$N_1 + xN_2 = N,$$

and

$$pB = (N_1 + N_2)mv^2/3 = Nmv^2/3 - (x-1)N_2 mv^2/3,$$

to be compared with Bohr's $pB = k' - \alpha' B$, with the result that k' is identified with $Nmv^2/3$ and $\alpha' B$ with $(x-1)N_2 mv^2/3$, that is to say that N_2/B at a given temperature is constant, which gives us the simple law of combination, that the number of molecules of O_{2x} per unit volume is to remain constant; of course this law will not carry us up to the limit at which N_2 is zero; but we can state the law of combination in the following terms:—At a certain degree of rarefaction the molecules of O_2 begin to combine to form O_{2x} and when these amount to a certain number per unit volume the effect of further rarefaction is to cause just so much further combination as keeps this number per unit volume constant; this process goes on till all the O_2 molecules are used up, that is, till $xN_2 = N$, after which the pure O_{2x} ought to obey Boyle's law. Now the degree of rarefaction for the new appearance of Boyle's law was not reached in Bohr's experiments, but in Crookes's observations of the deflecting force in oxygen we found the anomaly to disappear at a so-called pressure of between $300/10^6$ and $200/10^6$ atmo; but as these so-called pressures are nearly $p + \alpha'$, where α', being ·07 in millim. of mercury, is $92/10^6$ atmo, the anomaly may be said to cease when $p = 2\alpha'$; thus to determine the value of x for which the combination is complete we have the conditions that then $p = 2\alpha'$, and therefore

$$(N_2/B)mv^2/3 = 2(x-1)(N_2/B)mv^2/3,$$

so that

$$x = 3/2 \quad \text{and} \quad N_2 = 2N/3.$$

Thus, then, a process of association or combination of the O_2 molecules to form O_3, that is, ozone, explains one of Bohr's equations, and the cessation of anomaly in Crookes's experiments on deflecting force. It is necessary that in compressing pure O_3 a pressure should be reached at which the O_3 begins to dissociate into O_2, and progressive increase of pressure produces progressive dissociation of O_3 into O_2. This result is so contrary to the ordinary experience of dissociation amongst gases that it calls for the closest examination before we proceed farther. Now the most striking result so far is that over a considerable range of values of B there is to be a constant value of N_2/B, and this gives us at once an insight into the remarkable character of this dissociation; for N_2/B is proportional to the number of collisions of a molecule of O_3 with other molecules of O_3 in unit time, and as long as this is below a certain value $6\alpha'/mv^2$, as we have just seen, there is no dissociation, but when by compressing the pure O_3 a volume is reached at which $N_2/B = 6\alpha'/mv^2$, that is, at which the number of encounters of O_3 with its fellow molecules attains a certain value, some of the molecules are on the point of being dissociated, and if the pressure is increased the dissociation proceeds until the number of collisions of an O_3 with its fellows falls to the value given by $N_2/B = 6\alpha'/mv^2$, at which the number of molecules of O_3 remains constant until further compression is attempted; thus, then, we have evidence that there is a certain periodic collision with one another which the O_3 molecules cannot stand, and this implies that the period is identical with some natural period of vibration in the molecule: we have a case of molecular resonance leading to the destruction of the resonating molecules.

So far as we have gone it appears as though a collision of an O_3 with an O_2 occurring between its collisions with other O_3 is of no account, and we can understand how the presence of a number of O_2 molecules is really of little importance to the final result; for even if a collision with an O_2 has an inimical result as regards the dissociation, still, according to the laws of probability, there will be a number of successive collisions of some of the O_3 molecules with others without intermediate collisons with O_2 which would suffice to produce the destruction of the O_3 molecules, and therefore the only effect of the O_2 molecules is to make the process of dissociation slower.

But at last a stage will be reached at which the dissociation is so slow that it is only able to neutralise the combination that goes on, and then further diminution of volume will make the dissociation less rapid than the combination, and thus both on account of diminution of volume and combination N_2/B increases, so that the collisions of O_3 with O_3 cease to occur at the destructive periodicity, and the special cause of destruction or dissociation being removed, we have now to do with a more ordinary case, where the dissociation which takes place arises out of accidentally favourable collisions of O_3 with O_3 or with O_2, and combination out of accidentally favourable collisions of O_2 with O_2; now the number of collisions per second of an O_3 with an O_3 is

$${}_2\nu_2 = 2(N_2/B)(a_2+a_2)^2\pi^{\frac{1}{2}}(\kappa_2^2+\kappa_2^2)^{\frac{1}{2}},$$

where a_2 is radius and $3\kappa^2/2$ mean squared velocity of molecule of O_3; and the number of collisions per second of an O_3 with an O_2 is

$${}_1\nu_2 = 2(N_1/B)(a_1+a_2)^2\pi^{\frac{1}{2}}(\kappa_1^2+\kappa_2^2)^{\frac{1}{2}};$$

and as

$$a_2^3 = 2a_1^3 \text{ and } \kappa_1^2/\kappa_2^2 = m_2/m_1 = 2,$$

the coefficients of N_2/B and N_1/B in the two expressions are nearly identical, and ${}_2\nu_2 + {}_1\nu_2$ is proportional to $(N_2+N_1)/B$; but according to Bohr's discovery $(N_2+N_1)/B$ is constant after the discontinuity occurs, so that it appears that on diminishing volume after the discontinuity, combination and dissociation are in equilibrium when the total number of collisions of an O_3 per second has a certain constant value. Thus a periodicity in which the O_2 molecules play as important a part as the O_3 molecules gets established as the one which the O_3 molecules can just stand, and diminution of volume goes on so as to keep $(N_2+N_1)/B$ constant. But when the diminution of volume has gone a certain length another discontinuity appears which leads into the region of Bohr's equation $(p+\alpha)B=k$, to which the same explanation must apply as to his other equation, so that a new periodicity, which is to the former one as α to α', suddenly appears amongst the collisions of O_3 with O_3, as specially destructive to the O_3 molecules.

But one consequence of our theory is that k ought to be the same in the two equations of the form $(p+\alpha)B=k$, whereas Bohr's values make $k'=1{\cdot}045k$, because in the theory k in both cases stands for $(N/B)mv^2/3$: accordingly we must revert to Bohr's experimental data to ascertain whether k is necessarily different from k'. These are arranged in two series, the first at 14° C., and the second at 11°·5, Bohr giving the preference for reliability to the first; but if we plot them both with B as abscissa and pB as ordinate the points thus obtained ought, according to Bohr's equations, to lie on two straight lines, and if in these equations k and k' are the same, both lines ought to pass through the same point in the axis of pB : a glance at the points when plotted shows that two straight lines passing through such a point is the best representation for the second series of experiments, and although Bohr's two equations for the first series, representing two lines cutting the pB axis in two distinct points, form a possible expression of the mean locus of the points, still a representation which causes these points to coincide is quite as good, the difference between the two being less than the obvious errors of experiment. Thus for Bohr's series I. we have the equations

$$(p+\alpha)\mathrm{B}=k, \quad (p+\alpha')\mathrm{B}=k,$$

with $\alpha=\cdot 11$ and $\alpha'=\cdot 043$, while in series II. $\alpha=\cdot 10$ and $\alpha'=\cdot 057$; the value of k is not given because the mass of the gas is unknown. It would appear as though α increased with increasing temperature and α' decreased, while theoretically we should expect both α and α' to decrease, but the temperature interval between the two series is too small to allow of a safe inference as to the effect of temperature on α and α'. According to the theory the ratio of α to α' is that of the frequencies of encounters of O_3 molecules with one another before and after the region of discontinuity, and from the above values it appears to be greater than 2 to 1 at 14° and less than 2 to 1 at 11°·5, with very nearly a mean value of 2 to 1 like the octave in music. Thus, when pure O_3 is compressed, dissociation into O_2 begins when N_2/B attains a certain value, and goes on in such a manner as to keep N_2/B constant till a point is reached at which, on further reduction of volume, N_2 increases, but N_1 decreases in such a manner as to keep $(N_2+N_1)/B$ constant, so that although volume diminishes pressure remains constant ; thus the process of dissociation by encounters of the old periodicity proportional to N_2/B ceases and there is fresh combination sufficient to keep $(N_2+N_1)/B$ constant, but when by diminution of volume and combination N_2/B has risen to double its old constant value, dissociation begins again to occur on further diminution of volume, as though the molecules of O_3 were now resonating to the octave of the old periodic collision with a vigour that makes the presence of a great excess of O_3 of little account ; this more rapid period is probably a natural period of vibration of O_3, the other being an octave lower. An interesting point about the discontinuity is the similarity which it presents to the discontinuity of liquefaction, although, as we have just seen, the mechanism for keeping pressure constant is very different. But the similarity extends a little farther than to the one point of constant pressure, for Bohr found that on starting at a pressure below ·7 millim. and suddenly diminishing the volume the pressure could be raised to ·8 millim., although in the course of some hours it would fall back about ten per cent., a case which corresponds to that of supersaturation of a vapour with slow deposition of liquid ; on the other hand, when the pressure was at ·7 millim. and the volume was increased the pressure remained at ·7 millim. for a while and then began to fall off. These facts prove that dissociation goes on with great rapidity, while combination is slow, in agreement with our theory, because dissociation being produced directly by the collisions must be a very rapid process, whereas combination, as it must depend on collisions of the O_2 molecules under very favourable circumstances, being compelled to wait for the accidental occurrence of these rare conditions, must go on very slowly.

We have now to see how the theory explains the anomalous deflecting force discovered by Crookes, and also the slightness of the disturbance of the viscosity. Suppose that we have two chambers at temperatures θ_a and θ_b connected by a tube and containing a mixture of O_3 and O_2 in the cooler chamber, there will be equilibrium between the O_3 and the O_2 when $N_2/B = 3\alpha_a'/2mv_a^2$, where α_a' is the value of α' appropriate to the temperature θ_a; thermal transpiration will carry the mixture of N_2/B molecules of O_3 and N_1/B of O_2 per unit volume along the tube to the hotter chamber, where the resulting increase of pressure, as well as the increase of temperature, will dissociate some of the N_2, with still further tendency to increase the pressure ; now, when equilibrium is reached between the O_3 and the O_2 in the hotter chamber, $N_2/B = 3\alpha_b'/2mv_b^2$ in it ; and the final steady state of the whole system is determined by the conditions that N_2/B in the cool chamber and in the hot chamber has the values given ; therefore, the circulation which was proved to accompany thermal transpiration will tend to carry fewer molecules of O_3 from the hot chamber to the cold than from the cold to the hot, in the proportion of α_b'/mv_b^2 to α_a'/mv_a^2, and therefore, to preserve the state of equilibrium, the circulation will have to carry more O_2 molecules from the hot chamber to the cold than from cold to hot, the gain of O_3 in the hot chamber being dissociated to keep up the supply of O_2, and the gain of O_2 in the cold chamber being combined to keep up the supply of O_3 there, and thus the excess of N_1/B, the number of molecules of O_2 per unit volume in the hot chamber, over its value in the cold chamber must be 3/2 the corresponding difference in the values of N_2/B, that is to say, the difference of the partial pressure of the O_2 in the hot and the cold must be 3/2 the difference of the partial pressure of the O_3 in cold and hot; so that the total excess of pressure in the hot chamber over that in the cold is equal to half the difference between the partial pressures α'_a and α'_b; but we have just seen that with the temperatures constant these are constant over a large range of pressure from the pressure at which dissociation just begins up to the pressure at which the first discontinuity occurs. Thus, then, as the regions of the bulb on the two sides of the mica plate in Crookes's experiment correspond to our hot and cold chambers, we see theoretically that the deflecting force ought to be constant from a pressure of about ·7 millim. or $920/10^6$ atmo down to about $2\alpha'$ or ·11 millim. or $144/10^6$ atmo, according to our equations with one value of k and ·055 millim. as the value of α'. Now in Crookes's data already given we saw that the deflecting force remained practically constant at pressures from $1000/10^6$ atmo down to between $300/10^6$ and $200/10^6$ in his list of apparent pressures, which are really $p+a'$; so that the deflecting force remains constant down to a pressure between $228/10^6$ and $128/10^6$ in close enough agreement with the $144/10^6$ given above. When the degree of rarefaction is reached at which dissociation is just impossible in the hotter chamber, then the deflecting force will become that of pure O_3, and its laws will be those of a single gas as given in "Thermal Transpiration and Radiometer Motion"; in the region of pressure between that at which dissociation becomes impossible in the hot chamber and that at which it becomes impossible in the cool chamber, there will be a fall of deflecting force from that due to pure O_3 to the constant value which rules up to the pressure of discontinuity.

As to the comparatively small variations of $2a\lambda_0/B_0D$ which we found in our study of the log. dec., their smallness would be explained by the supposition that the viscosity of O_3 is very nearly the same as that of O_2, for then the limiting constant value L of the log. dec. of O_2 would also be approximately a correct value for all the different values of L appropriate to all the different mixtures of O_3 and O_2, which must have been experimented on at pressures between $1000/10^6$ and $144/10^6$ atmo. But it must be mentioned that Crookes drew attention to a slight irregularity in the values of the log. dec. of oxygen, for after maintaining a value nearly constant at about ·1120 near a pressure of 20 millim., it rose to ·1124 at 7·5 millim., after which it fell away in the usual manner; now as this irregularity occurs just at the region of pressure where Bohr's experiments have shown us that dissociation is beginning, it is confirmatory of the other evidence. In the absence of experimental values of the viscosity of ozone, we can compare the viscosities of O_3 and O_2 by means of the kinetic theory, since for forceless spherical molecules of mass m and radius a the viscosity is proportional to mv/a^2 or to $m^{\frac{1}{2}}/a^2$, so that if we assume that O_3 is a sphere of double the volume as well as double the mass of O_2 regarded as a sphere, then the viscosity of O_3 is $2^{-\frac{1}{6}}$ or ·9 that of O_2, so that we should be prepared to admit that the viscosities of ozone and oxygen are nearly equal. This being so, we have next to inquire as to the relative values of λ, the mean free path, at some standard value of the number of molecules per unit volume of O_3 and O_2; for spheres λ varies inversely as a^2, and therefore λ for O_3 ought to be about $2^{-\frac{2}{3}}$ or ·63 of that for O_2; so that if $2a\lambda_0/B_0D$ for pure O_2 is 20, it ought to fall to about 13 for pure O_3, while we found actually a fall from about 20 to 15 and then a slight rise: therefore, seeing that we are not strictly correct in assuming viscosity, and therefore L, for mixtures of O_3 and O_2 to be the same as for O_2, we cannot expect any closer agreement between the experimental results and our general theoretical reasoning about the comparative slightness of the perturbation in the log. dec. despite profound change going on in the gas, which in the end converts the oxygen into ozone.

We have now to discuss the anomalous expansion of rare oxygen observed by Baly and Ramsay (Phil. Mag. [5] xxxviii.), whose experiments on hydrogen and nitrogen have been discussed in "Boyle's Law at Low Pressures"*, where it was shown that the apparent diminution of the coefficient of expansion of these gases at low pressure is perhaps not real; the increase which they found in the coefficient of expansion of oxygen at low pressures is therefore real, for probably the same cause is acting to produce an apparent diminution of the coefficient as in the case of the other gases. The following are the reciprocals of the mean coefficients of expansion of oxygen between 12° C. and 132°, as given by Baly and Ramsay at various pressures calculated from the indications of a M'Leod gauge according to Boyle's law, and therefore exceeding the true pressure when above ·7 millim. by ·11 millim. and when below ·7 millim. by ·055 millim.

Apparent p	5·1	5·3	4·0	2·5	1·4	·083	·07
True p	5·0	5·2	3·9	2·4	1·3	·028	·015
	261	260	262	251	233	244	240

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* *Suprà*, p. 11.

As we have seen, at all the pressures given we have to do at a temperature of 12° with mixtures of O_2 and O_3, which on being raised to 132° will have the whole or part of the ozone dissociated into oxygen, and will therefore give a larger coefficient of expansion than belongs to a pure gas. In fact, if there is complete dissociation of the O_3 at the higher temperature, the number of molecules is increased in the proportion of $p+\alpha'$ to p, so that the mean coefficient of expansion is increased to $1/273(1+\alpha'/p)+\alpha'/120p$; thus with $\alpha'=\cdot 055$ the reciprocal of the coefficient of expansion becomes 263 at a pressure of 5 millim. and 231 at 1 millim., numbers which are in close agreement with those of experiment. At the two lowest pressures we ought to have to do with pure ozone at the lower temperature, and if it is entirely dissociated at the higher temperature the coefficient of expansion will be $3/273\times 2+1/240$, and its reciprocal 104 in place of about 240 as found by experiment; accordingly it appears that at the lowest pressures the ozone probably is only partially dissociated at 132° C. Thus, although we cannot draw definite quantitative results from these experiments on the expansion of oxygen, we have ample qualitative evidence that dissociation accompanies the expansion of what has hitherto been considered to be pure rarefied oxygen, but which is either a mixture of oxygen or ozone, or, at a low enough pressure, pure ozone.

In their experiments with oxygen Baly and Ramsay had an extraordinary experience which will be traced to dissociation. They had two M'Leod gauges in connexion with a supply of oxygen whose pressure had been reduced to ·75 millim., which is in the region of Bohr's discontinuity; on raising the mercury so as to shut off communication between the two gauges, and then compressing each of the equal volumes thus isolated at the same pressure into the fine graduated tubes of the gauges where both pressure and volume could be measured, they found that the mean value of pB in the one gauge was 8·8 times as large as in the other, whereas the two were expected to be equal; it was only after putting the gauges frequently in communication with one another during 78 hours that the values of pB became equal. The total volume of each gauge was about 90 cub. centim., so that the original value of pB in both gauges was about 67·5, a rough estimate on account of the uncertainty in measuring p near the discontinuous region; but after compression into the volume-tubes the value of pB in the one was 53·1 and in the other 6·04, so that practically the whole of the remarkable disturbance was confined to the one gauge.

The phenomenon would be explained if we supposed that in the very unstable state of the gas at a pressure near ·7 millim., where a mere change of volume suffices to dissociate some of the O_3 molecules, during the dissociation a number of free ions of O were liberated, and on account of slight want of electrical neutrality in the apparatus only in the one gauge. We have therefore to consider the effect of a number of atoms charged with electricity when mixed with a number of molecules. The atoms repelling one another on account of their electric charges will tend to accumulate near the walls of the vessel, while the molecules will remain evenly distributed through the vessel, for if they did not a diffusion stream would be set up in them until they were uniformly distributed. Thus then we can separate the mixture in imagination into two media determining the distribution of partial pressure amongst the atoms as if the molecules were absent, and then adding the uniform partial pressure due to the molecules. The differential equation for the distribution of pressure amongst atoms in a spherical vessel is easily written down, but I will defer its discussion to a more appropriate occasion. For the present it will suffice to say that obviously the effect of electrical repulsion will be to force the majority of the atoms out to the surface, where a layer of rapidly altering density and pressure will be formed merging into the more gradually altering body of the medium. Let us consider the exaggerated case where, in the surface-layer, the pressure increases from one side of it to the other by P; then if Q is the total charge of the whole layer of surface $4\pi R^2$, the repulsion experienced by unit area of the layer is $(Q/R^2)(Q/4\pi R^2)$, which is to be equal to P. If, then, in Baly and Ramsay's experiments in one of the gauges at the moment at which they were isolated from one another the value of P, due to free oxygen ions in it, was about 8/9 of the recorded initial pressure of about ·75 millim. then the mass of oxygen in this gauge, with the assistance of the ions, could equilibrate the pressure due to nine times as great a mass of oxygen in the other gauge, for we shall see immediately that the mass of the ions is negligible. Supposing Baly and Ramsay's cylinder of 90 cub. centim. to be replaced by an equal sphere, then R is 2·78 centim., and P being 8/9 of ·001 atmo is in round numbers 889 dynes per sq. cm., and therefore Q in absolute electrostatic units is 817. Now the electrochemical equivalent of oxygen is ·00082, that is, there is one absolute electromagnetic unit of electricity or 3×10^{10} electrostatic units in ·00082 gram of oxygen ions, and therefore the mass of the ions containing our charge of

817 is $2{\cdot}2/10^{11}$ gram; now the total mass of oxygen filling the other gauge at about ·001 atmo is about $1{\cdot}3/10^4$ gram. Thus a conversion of less than a millionth of their mass of gas into ions collected into one of the gauges would explain the strange phenomenon encountered by Baly and Ramsay, if it is allowed that between the isolation of the gauges from one another and the compression into the fine tubes the charge Q got discharged, so that in both fine tubes after compression there was only pure oxygen, O_2. We have also to account for the disappearance of the equal and opposite charge to Q. But it is not worth while occupying space to speculate on these minor matters, as the phenomenon is so important as to entitle it to a thorough experimental investigation.

There are one or two consequences of the spontaneous formation and decomposition of ozone at low pressures which deserve to be touched on briefly. The first is, that as there is no theoretical reason why the presence of nitrogen should suppress the actions which we have been discussing, there ought to be slight departure from Boyle's law in rarefied air; in fact, let p_n and p_o be the partial pressures of nitrogen and oxygen in the air, then

$$p_n\mathrm{B}=k_n;\quad \text{but } p_o\mathrm{B}=k_o-\alpha\mathrm{B},$$

so that

$$(p_n+p_o)\mathrm{B}=p\mathrm{B}=k_n+k_o-\alpha\mathrm{B},$$

and thus for air the equation is of the same form as for oxygen, but the departure from Boyle's law will make itself conspicuous in a different region of pressure: Bohr found the departure in oxygen from a pressure of 15 millim. down to ·7 millim. where the first discontinuity occurred, and so for air it might be looked for from about 75 millim. down to 3·5 millim. where a discontinuity should be expected, but of a different nature from that with oxygen, in fact p_o becomes constant for a short range of volume, and so

$$(p+p_o)\,\mathrm{B}=p\mathrm{B}=k_n+p_o\mathrm{B}$$

till the region of volume is reached for which α in oxygen has the smaller value α', and then $p\mathrm{B}=k_n+k_o-\alpha'\mathrm{B}$, which lasts until $p_o=2\alpha'$, at which point all the oxygen will have been changed into ozone, and therefore p_n will be $4p_o\times3/2$, seeing that in the atmosphere at 760 millim. $p_n=4p_o$; thus the pressure at which Boyle's law will become re-established for the air, now a mixture of nitrogen and ozone, will be $13\alpha'$ or about ·715 millim. of mercury.

The only accessible data suitable for comparison with these conclusions about air are those of Amagat (*Ann. de Chim. et de Phys.* [5] xxviii.) who between 12 millim. and 1 millim. of mercury finds departures from Boyle's law in the same direction as those indicated, but not of the amount that our theory requires, Amagat not considering the departures to be outside of the limits of experimental error, but it is hardly worth while occupying space with reproducing these data, since the Russian investigators Mendeléeff, Kirpitscheff, and Hemilian have found the departure for air to be always in the direction just indicated but of variable amount. One reason for this variability is shown in a pronounced form in the remarkable experience which Baly and Ramsay had with air in the capillary tube of one of their M'Leod gauges, in which, while the pressure varied from 4·1 millim. to 8·0 the product pB fell from 100 to 9·4, a result to be explained, like the corresponding one for oxygen, by the supposition that in the unstable transition of part of the O_2 into O_3 some ions of O got liberated whose electrical repulsion produced about 91 per cent. of the initial pressure of 4·1 millim., the loss of the charge during the subsequent compression causing the part of the pressure due to the electricity to diminish, and pB, therefore, to diminish likewise. In view of the possibility of a perturbing cause of this magnitude it is obvious that special precautions will have to be taken in studying the compressibility of rarefied air to get rid of all electrical charge in the gas or the apparatus, and we can understand the baffling nature of the variations encountered by the Russian experimenters in their devoted work at this difficult experimental research.

According to our reasoning the amount of ozone in the air at the surface of the earth ought to be $\alpha/760$ or $\cdot 11/760$ per unit volume of air, or about one volume in 7000 if the air were protected from all chemical actions. The estimates of the actual amount of ozone in the air near the earth's surface are very uncertain, but seem to indicate about one volume in a million; thus we are led to believe that oxidation must be responsible for the destruction of the greater part of the ozone that might theoretically be expected in the air near the earth. But as we rise in the atmosphere to a place where the pressure is p, the amount of ozone per unit volume ought to be α/p till a region of discontinuity is reached, after which the amount is α'/p till a point is reached where the pressure is about ·715 millim. of mercury, at which and above which the whole of the oxygen exists as ozone, forming about one-seventh of the volume of the air there. These deductions have some hygienic importance and explain the reason for the current belief that the higher regions of the atmosphere and winds which come from them are richer in ozone than the surface air: they also show that there must be enough ozone in the whole atmosphere to have an important bearing on the blue colour of the sky. Hartley drew attention to this matter (Journ. Chem. Soc. xxxix. 1881), but as recent experiments have shown that oxygen in sufficient quantity shows a blue colour by transmitted light, the claims of ozone to a serious share in the blueness of the sky have been rather neglected; but if it is remembered that the blueness of ozone is enormously stronger than that of oxygen under the same conditions, it becomes apparent that the quantity of ozone which has been theoretically shown to have a probable existence in the atmosphere must exercise a considerable influence on the colour of the sky and the colour of distant objects.

From what we have seen we have also to contemplate the possibility of the existence of free ions of oxygen in the outer regions of the atmosphere, but a discussion of the effects of such must be reserved for a future paper.

Melbourne, Aug. 1896.

XXX. *On the Division of Energy in the Emission-Spectrum of a Black Body.* *By* WILLY WIEN*.

ALTHOUGH the influence of temperature on the radiation of a black body and the division of this radiation into its component wave-lengths can be deduced from the electromagnetic theory of light by a purely thermodynamic method, the application of the same process to the division of the energy itself has not up to the present been successful.

The cause of this lies in the fact that the dependence of intensity on wave-length must be completely determinable from the properties of the radiation, because the latter only depends on the temperature, and not on the special properties of single bodies.

The radiation of a black body corresponds to the condition of thermal equilibrium, and consequently to the maximum of entropy. If, for example, a process were known by which a change of wave-length could be brought about without any expenditure of work, and without absorption in the sense of an increase of entropy, then the division of energy in the

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* Translation furnished by Mr. J. Burke from Wiedemann's *Annalen*, vol. lviii. p. 662 (1896). Communicated by Prof. G. F. FitzGerald, F.R.S.

spectrum of a black body could be completely determined from the law of the maximum of entropy. As I have shown in an earlier paper, the entropy of radiation of a known intensity and colour can be determined, but there is no obvious physical process by which an alteration in colour such as that desired can be observed to be taking place. A determination of the distribution of energy is therefore impossible without hypotheses.

An attempt has been made by E. von Lommel* and W. Michelson† to found a complete law of radiation on certain premises. For this purpose the latter makes the following stipulations :—

(1) Maxwell's Law of the division of velocities among a great number of molecules holds also for solids.

(2) The period of oscillation τ, which is excited by a molecule, is connected with its velocity of propagation v by the equation

$$\tau=\frac{4\rho}{v},$$

where ρ is a constant. (This assumption is based on a definite conception with regard to the excitation of the radiation.)

(3) The intensity of the radiation sent out from a molecule is proportional to the number of molecules having the same time of oscillation, is further an undetermined function of the temperature and a likewise unknown function of the kinetic energy, which by a further hypothesis is restricted to a power of v^2.

The law which Michelson obtains from these assumptions gives for the wave-length λ_m of the maximum of energy

$$\lambda_m=\frac{\text{const.}}{\sqrt{\theta}},$$

where θ denotes the absolute temperature. As for the rest, this law leaves the total emission as a function of the temperature undetermined.

I have now endeavoured to carry out the idea of Michelson, of making use of Maxwell's law of the division of velocities as a basis for the law of radiation, and at the same time to lessen the number of the hypotheses which, on account of our total ignorance of the cause of the radiation, are particularly uncertain, by utilization of the results obtained by Boltzmann and myself by pure thermodynamic treatment.

The remaining hypotheses, however, still possess some

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* Wied. *Ann.* iii. p. 251 (1877)

† *Journal de Physique* [2] vi. (1887).

uncertainty in their theoretical groundwork, but have the advantage that the deductions from them can be directly compared over a very wide range with the results of experience. Their confirmation or contradiction by experiment will therefore decide the question of the correctness or otherwise of the hypotheses, and thus far be useful as a further development of the molecular theory.

The law that in an exhausted vessel the radiation is the same as that from a black body at the same temperature as the walls of the vessel, holds also if the radiating body be a gas which is shut off from the vacuous space by transparent, and from the exterior by reflecting walls.

But this gas must possess a finite absorptive power for all wave-lengths. There remains, however, no doubt that there are gases, such as carbonic acid and water-vapour, which, by mere elevation of temperature, emit heat rays*. Strongly superheated vapours may be regarded as gases, and by suitable mixing of different substances, it is possible to conceive of a mixture of gases which possesses a finite absorptive power for all wave-lengths. In this case one must not, however, consider that radiation which gases send out under the influence of electrical or chemical processes.

If one radiating body be a gas, then Maxwell's law of the division of velocities will hold if we take as our basis the kinetic theory of gases. The absolute temperature will be proportional to the mean kinetic energy of the gaseous molecule. This assumption has been rendered highly probable by the labours of Clausius † and Boltzmann ‡, and is still further supported by the researches of Helmholtz § on monocyclic systems, according to which researches both the kinetic energy and the absolute temperature have the property of being the integrating denominator of the differential of the added energy.

To avoid the unecessary prolixity which would result from a consideration of the different constituents of a mixture of gases, let us imagine a mixture of such a kind that the homogeneous radiation under consideration is sent out by *one* only of the gases forming the mixture.

The number of molecules whose velocity lies between v and $v+dv$ is proportional to the quantity

$$v^2 e^{-\frac{v^2}{a^2}} dv,$$

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* Paschen, Wied. *Ann.* l. p. 409 (1893).

† Pogg. *Ann.* cxlii. p. 433 (1871).

‡ Wien. *Ber.* [2] liii. p. 195 (1866).

§ *Gesammelte Abhandlungen,* iii. p. 119.

where α denotes a constant, which can be deduced from the mean velocity* $\bar{v}$ by means of the equation

$$\bar{v}^2=\tfrac{3}{2}\alpha^2.$$

The absolute temperature is therefore proportional to α^2. But the vibrations sent out by a molecule whose velocity is v are completely unknown in their dependence on the condition of the molecule. A now-a-days generally accepted view is that the electric charges of the molecules can excite electromagnetic waves.

We make the hypothesis, that each molecule sends out vibrations of a wave-length which only depends on the velocity of the molecule moved and whose intensity is a function of this velocity.

It is possible to obtain this deduction by several different special hypotheses with regard to the process of radiation; as, however, such premises at this preliminary stage are completely arbitrary, it appeared to me to be the safest method to make the necessary hypothesis as simple and general as possible.

As the wave-length λ of the radiation sent out by any molecule is a function of v, v is also a function of λ.

The intensity ϕ_λ of the radiation whose wave-length lies between λ and $(\lambda+d\lambda)$ is therefore proportional

(1) To the number of molecules which send out radiations of this period;

(2) To a function of the velocity v, therefore also to a function of λ.

Consequently

$$\phi_\lambda=\mathrm{F}(\lambda)e^{-\frac{f(\lambda)}{\theta}};$$

where F and f denote two unknown functions, and θ denotes the absolute temperature.

Now the change of radiation with temperature is composed, according to the theory given by Boltzmann† and myself‡, of an increase of total energy in proportion to the fourth power of the absolute temperature and of a change of wave-length of the whole energy comprised between λ and $(\lambda+d\lambda)$ in such a direction that the wave-length belonging to it alters in inverse ratio to the absolute temperature. If we imagine the energy at any temperature plotted as a function of the wave-length, then the curve obtained would remain

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* $\bar{v}$ is the square root of mean square of velocity.—Transl.

† Wied. *Ann.* xxii. p. 291 (1884).

‡ Wien, *Ber. d. Berlin. Akad.* 9th Feb., 1893.

unaltered at a different temperature, if the scale of the drawing were so changed that the ordinates were decreased in the relation of $1/\theta^4$ and the abscissæ increased as θ. The latter is with our value of ϕ_λ only possible if λ and θ occur in exponents only as the product $\lambda\theta$.

If c denote a constant, then

$$\frac{f(\lambda)}{\theta}=\frac{c}{\lambda\theta}.$$

The increase of total energy determines the value of $\mathrm{F}(\lambda)$. Indeed the relation must hold

$$\int_0^\infty \mathrm{F}(\lambda)e^{-\frac{c}{\theta\lambda}}d\lambda=\text{const. }\theta^4.$$

$\mathrm{F}(\lambda)$ can be found by the method of undetermined coefficients. We imagine $\mathrm{F}(\lambda)$ expanded into a series and make $\lambda=c/y\theta$, then

$$\mathrm{F}(\lambda)=\mathrm{F}\left(\frac{c}{y\theta}\right)=a_0+a_{+1}\frac{\theta y}{c}+a_{+2}\frac{\theta^2y^2}{c^2}+\ldots a_n\frac{\theta^ny^n}{c^n}\ldots$$

$$+a_{-1}\frac{c}{\theta y}+a_{-2}\frac{c^2}{\theta^2y^2}+\ldots a_{-n}\frac{\theta^{-n}y^{-n}}{c^{-n}}.$$

Integration of this gives

$$\int_0^\infty \mathrm{F}(\lambda)e^{-\frac{c}{\theta\lambda}}d\lambda=\frac{c}{\theta}\int_0^\infty \mathrm{F}\left(\frac{c}{y\theta}\right)e^{-y}\frac{dy}{y^2}=\Sigma_n a_n\frac{\theta^{n-1}}{c^{n-1}}\int_0^\infty e^{-y}y^{n-2}dy.$$

Therefore
$$\text{const. }\theta^4=\Sigma_n a_n\frac{\theta^{n-1}}{c^{n-1}}\Gamma(n-1).$$

All the coefficients are therefore nothing except one of them, the coefficient of $\theta^{n-1}=\theta^4$;

therefore
$$n=5.$$

Consequently
$$\mathrm{F}(\lambda)=\frac{\text{const.}}{\lambda^5}.$$

Accordingly the equation for ϕ_λ is

$$\phi_\lambda=\frac{\mathrm{C}}{\lambda^5}e^{-\frac{c}{\lambda\theta}}.$$

From this follows :—

$$\frac{d\phi}{d\lambda}=-\frac{\mathrm{C}e^{-\frac{c}{\lambda\theta}}}{\lambda^6}\left(5-\frac{c}{\lambda\theta}\right),$$

$$\frac{d^2\phi}{d\lambda^2}=\frac{\mathrm{C}e^{-\frac{c}{\lambda\theta}}}{\lambda^7}\left(30-\frac{12c}{\lambda\theta}+\frac{c^2}{\lambda^2\theta^2}\right);$$

for
$$\lambda=\frac{c}{5\theta},\quad \frac{d\phi}{d\lambda}=0,$$
$$\frac{d^2\phi}{d\lambda^2}=-\frac{5Ce^{-5}}{\lambda^7};$$
$\frac{d^2\phi}{d\lambda^2}$ is negative, therefore the value corresponds to a maximum. Let this value be called λ_m. The corresponding value of ϕ is
$$\phi_m=\frac{C}{\lambda_m^5}e^{-5}.$$
As both ϕ and $d\phi/d\lambda$ vanish for $\lambda=\infty$, the curve is an asymptote to the λ-axis.

Further, $d^2\phi/d\lambda^2=0$ for the roots of the equation
$$30\lambda^2\theta^2-12c\lambda\theta+c^2=0;$$
therefore for
$$\lambda=\lambda_m\left(1\pm\sqrt{\frac{1}{6}}\right).$$
For these two points the curve has points of inflexion. If we put $\lambda=\lambda_m(1+\epsilon)$, then
$$\phi_\lambda=\frac{Ce^{-\frac{c}{\lambda_m(1+\epsilon)\theta}}}{\lambda_m^5(1+\epsilon)^5}=\frac{Ce^{-\frac{5}{1+\epsilon}}}{\lambda_m^5(1+\epsilon)^5};$$
therefore
$$\log\frac{\phi}{\phi_m}=-5\left(\log(1+\epsilon)-\frac{\epsilon}{1+\epsilon}\right)=-5(\tfrac{1}{2}\epsilon^2-\tfrac{2}{3}\epsilon^3+\tfrac{3}{4}\epsilon^4\ldots).$$
If we put $-\epsilon$ for ϵ, then
$$\log\frac{\phi}{\phi_m}=-5(\tfrac{1}{2}\epsilon^2+\tfrac{2}{3}\epsilon^3+\tfrac{3}{4}\epsilon^4\ldots).$$
In this case the absolute total of the series is greater and therefore ϕ/ϕ_m less than when ϵ is positive. So far as $\epsilon<1$, the ordinates at an equal distance from the maximum are less on the side of small wave-lengths.

In an earlier work* I showed that the energy curves of black bodies at different temperatures cannot cut one another.

From this it may be deduced that the curve must fall away slower toward the side of the long waves than the curve
$$\frac{\text{const.}}{\lambda^5}.$$
But this is in reality the case with our curve: $d\phi_\lambda/d\lambda$ is in absolute magnitude always less than $5C/\lambda^6$, and only reaches

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* Wied. *Ann.* lii. p. 159 (1894).

the maximum value for $\theta=\infty$. For infinitely increasing temperature ϕ_λ would equal C/λ^5, and the maximum of energy would approach infinitely near to the wave-length zero.

While I had deduced the formula for ϕ_λ from the theoretical considerations just brought forward, Prof. Paschen found independently that the formula

$$\phi_\lambda=\frac{C}{\lambda^\alpha}e^{-\frac{c}{\lambda\theta}},$$

where α is a constant, was the one which reproduced best the results of his observations, and was kind enough to communicate this to me and to allow me to publish his formula here. Prof. Paschen intends to determine the value of the constant α from a complete calculation and comparison of his experiments. If α is not equal to 5, the total emission would not follow Stefan's law.

Charlottenburgh, June 1896.

XXXI. *On Lagrange's Determinantal Equation.*

By THOMAS MUIR, *LL.D.*[*]

1. VARIOUS proofs[†] have been given of the reality of the roots of the equation

$$\begin{vmatrix} a-x & b & c & \dots \\ b & d-x & e & \dots \\ c & e & f-x & \dots \\ \cdot & \cdot & \cdot & \cdot \end{vmatrix}=0,$$

and more than one extension[‡] of the theorem has been made. Apparently, however, no departure from axi-symmetry of the determinant has ever been contemplated until quite recently. This new and important step is due to Professor Tait, who in a paper read before the Royal Society of Edinburgh in May is led to the conclusion that the cubic equation

$$\begin{vmatrix} \frac{A}{p}-x & \frac{c}{p} & \frac{b}{p} \\ \frac{c}{q} & \frac{B}{q}-x & \frac{a}{q} \\ \frac{b}{r} & \frac{a}{r} & \frac{C}{r}-x \end{vmatrix}=0$$

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[*] Communicated by the Author.

[†] For three of them see Salmon's 'Modern Higher Algebra,' 4th edit. pp. 28, 48-56.

[‡] See Sylvester, *Crelle's Journal*, lxxxviii. pp. 6-9. Routh, 'Dynamics of a System of Rigid Bodies,' part ii. 4th edit. pp. 36-38, 41. Muir, 'Messenger of Math.' xiv. pp. 141-143.

has all its roots real, and thereupon adds that "a somewhat similar process shows that the roots of the equation

$$\begin{vmatrix} a-x & b & c \\ d & e-x & f \\ g & h & i-x \end{vmatrix} = 0$$

are always all real, provided the single condition

$$cdh = bfg$$

be satisfied."

On examination of this latter statement in the light of former researches of my own, I found that it was scarcely correct to say that only one condition was necessary, the further requirements being that b and d be of like sign, c and g of like sign, and therefore also f and h of like sign,—in other words, *that the determinant should, so far as sign is concerned, be axi-symmetric*; and from this I passed to the consideration of similar equations of higher degree, with the following results :—

2. Taking the equation of the nth degree, but for shortness' sake writing it only of the fifth, viz.

$$\begin{vmatrix} a_1-x & a_2 & a_3 & a_4 & a_5 \\ b_1 & b_2-x & b_3 & b_4 & b_5 \\ c_1 & c_2 & c_3-x & c_4 & c_5 \\ d_1 & d_2 & d_3 & d_4-x & d_5 \\ e_1 & e_2 & e_3 & e_4 & e_5-x \end{vmatrix} = 0,$$

and multiplying the rows by $\omega_1, \omega_2, \omega_3, \omega_4, \omega_5$ respectively, and the columns by $\omega_1^{-1}, \omega_2^{-1}, \omega_3^{-1}, \omega_4^{-1}, \omega_5^{-1}$ respectively, we have the equation in the form

$$\begin{vmatrix} a_1-x & \omega_1\omega_2^{-1}a_2 & \omega_1\omega_3^{-1}a_3 & \omega_1\omega_4^{-1}a_4 & \omega_1\omega_5^{-1}a_5 \\ \omega_2\omega_1^{-1}b_1 & b_2-x & \omega_2\omega_3^{-1}b_3 & \omega_2\omega_4^{-1}b_4 & \omega_2\omega_5^{-1}b_5 \\ \omega_3\omega_1^{-1}c_1 & \omega_3\omega_2^{-1}c_2 & c_3-x & \omega_3\omega_4^{-1}c_4 & \omega_3\omega_5^{-1}c_5 \\ \omega_4\omega_1^{-1}d_1 & \omega_4\omega_2^{-1}d_2 & \omega_4\omega_3^{-1}d_3 & d_4-x & \omega_4\omega_5^{-1}d_5 \\ \omega_5\omega_1^{-1}e_1 & \omega_5\omega_2^{-1}e_2 & \omega_5\omega_3^{-1}e_3 & \omega_5\omega_4^{-1}e_4 & e_5-x \end{vmatrix} = 0.$$

The determinant here is in substance exactly the same as before ; but we have now five disposable quantities, $\omega_1, \omega_2, \omega_3, \omega_4, \omega_5$, and the question is whether these can be so determined as to make the array of elements axi-symmetric. The conditions for this clearly are

$$\omega_1\omega_2^{-1}a_2 = \omega_2\omega_1^{-1}b_1, \quad \omega_1\omega_3^{-1}a_3 = \omega_3\omega_1^{-1}c_1, \ldots$$

or, better,

$$\begin{array}{llll}
\omega_1^2a_2=\omega_2^2b_1, & \omega_1^2a_3=\omega_3^2c_1, & \omega_1^2a_4=\omega_4^2d_1, & \omega_1^2a_5=\omega_5^2e_1: \\
 & \omega_2^2b_3=\omega_3^2c_2, & \omega_2^2b_4=\omega_4^2d_2, & \omega_2^2b_5=\omega_5^2e_2; \\
 & & \omega_3^2c_4=\omega_4^2d_3, & \omega_3^2c_5=\omega_5^2e_3; \\
 & & & \omega_4^2d_5=\omega_5^2e_4:
\end{array}$$

from which it is evident that, whatever the full and final answer to our question may be, it will be necessary that

$$\begin{array}{llllll}
a_2 & \text{have the same sign as} & & & & b_1, \\
a_3 & ,, & ,, & ,, & & c_1, \\
\cdot & \cdot\ \cdot\ \cdot & \cdot\ \cdot & \cdot\ \cdot & & \cdot\,,
\end{array}$$

—in other words, that *conjugate elements of the original determinant be alike in sign.*

As, however, we have got to ascertain whether a set of non-zero values can be found for ω_1, ω_2, ω_3, ω_4, ω_5 which will satisfy all the equations, it is desirable to arrange them formally as a set of (ten) homogeneous equations in these five unknowns. The result of this is

$$\left.\begin{array}{llllll}
a_2\omega_1^2 & -b_1\omega_2^2 & & & & =0, \\
a_3\omega_1^2 & & -c_1\omega_3^2 & & & =0, \\
a_4\omega_1^2 & & & -d_1\omega_4^2 & & =0, \\
a_5\omega_1^2 & & & & -e_1\omega_5^2 & =0, \\
 & b_3\omega_2^2 & -c_2\omega_3^2 & & & =0, \\
 & b_4\omega_2^2 & & -d_2\omega_4^2 & & =0, \\
 & b_5\omega_2^2 & & & -e_2\omega_5^2 & =0, \\
 & & c_4\omega_3^2 & -d_3\omega_4^2 & & =0, \\
 & & c_5\omega_3^2 & & -e_3\omega_5^2 & =0, \\
 & & & d_5\omega_4^2 & -e_4\omega_5^2 & =0.
\end{array}\right\}$$

Now the equations being homogeneous, only the *ratios* of the five unknowns have to be found,—that is to say, only *four* magnitudes are wanted, and for this the first four equations are evidently sufficient. The existence of the remaining equations implies that conditions of consistency have to be fulfilled; and these conditions are at least six in number, for each of the remaining equations determines a ratio already determined, and does so in terms of coefficients not previously met with. Thus equation (6) determines ω_2^2/ω_4^2, which has already been got from equations (1) and (2): hence we have the condition

$$\begin{vmatrix} a_2 & -b_1 & \cdot \\ a_3 & \cdot & -c_1 \\ \cdot & b_3 & -c_2 \end{vmatrix} = 0.$$

i. e. $a_2b_3c_1 = a_3b_1c_2,$

the others being

$$a_2b_4d_1 = a_4b_1d_2,$$
$$a_2b_5e_1 = a_5b_1e_2,$$
$$a_3c_4d_1 = a_4c_1d_3$$
$$a_3c_5e_1 = a_5c_1e_3,$$
$$a_4d_5e_1 = a_5d_1e_4.$$

Further, no other conditions are necessary: for, these being complied with, the values of the ratios obtained from the first four equations will satisfy the remaining six.

3. Any conditions of consistency, therefore, which may be obtained from the last six equations only must be dependent on the conditions already obtained. For example, from (5), (6), (8) we have the condition

$$b_3c_4d_2 = b_4c_2d_3\ ;$$

but this is merely a result derivable by multiplication and division from three of the previous six conditions, viz. the three

$$a_3c_4d_1 = a_4c_1d_3, \quad a_2b_3c_1 = a_3b_1c_2, \quad a_4b_1d_2 = a_2b_4d_1.$$

The number of such dependent conditions is four, viz.

from (5), (6), (8) $\quad b_3c_4d_2 = b_4c_2d_3,$
from (5), (7), (9) $\quad b_3c_5e_2 = b_5c_2e_3,$
from (6), (7), (10) $\quad b_4d_5e_2 = b_5d_2e_4,$
and from (8), (9), (10) $\quad c_4d_5e_3 = c_5d_3e_4.$

4. Again, the ten conditions regarding the signs of the elements, viz.

$$a_2b_1 = +, \quad a_3c_1 = +, \ldots.$$

are not all independent. Only the first *four* are necessary when taken along with the six equations of § 2. For the conditions

$a_2b_3c_1 = a_3b_1c_2,\quad a_2b_1 = +,\quad a_3c_1 = +$ give $b_3c_2 = +,$
$a_2b_4d_1 = a_4b_1d_2,\quad a_2b_1 = +,\quad a_4d_1 = +$ give $b_4d_2 = +,$
$a_2b_5e_1 = a_5b_1e_2,\quad a_2b_1 = +,\quad a_5e_1 = +$ give $b_5e_2 = +,$
$b_3c_4d_2 = b_4c_2d_3,\quad b_3c_2 = +,\quad b_4d_2 = +$ give $c_4d_3 = +,$
$b_3c_5e_2 = b_5c_2e_3,\quad b_3c_2 = +,\quad b_5e_2 = +$ give $c_5e_3 = +,$
$b_4d_5e_2 = b_5d_2e_4,\quad b_4d_2 = +,\quad b_5e_2 = +$ give $d_5e_4 = +.$

5. The state of matters thus is that the quintic equation

$$\begin{vmatrix} a_1-x & a_2 & \cdots \\ b_1 & b_2-x & \cdots \\ \cdot & \cdot & \cdot \end{vmatrix} = 0$$

will have all its roots real if *ten* conditions be complied with: viz.

six as to magnitude,	and *four* as to sign.
$a_2b_3c_1=a_3b_1c_2,$	$a_2b_1=+,$
$a_2b_4d_1=a_4b_1d_2,$	$a_3c_1=+,$
$a_2b_5e_1=a_5b_1e_2,$	$a_4d_1=+,$
$a_3c_4d_1=a_4c_1d_3,$	$a_5e_1=+;$
$a_3c_5e_1=a_5c_1e_3,$	
$a_4d_5e_1=a_5d_1e_4,$	

and that the six conditions as to magnitude imply four others like themselves, viz.

$$b_3c_4d_2=b_4c_2d_3,$$
$$b_3c_5e_2=b_5c_2e_3,$$
$$b_4d_5e_2=b_5d_2e_4,$$
$$c_4d_5e_3=c_5d_3e_4;$$

and the said six together with the four conditions as to sign imply six others similar to the latter, viz.

$$b_3c_2=+,$$
$$b_4d_2=+,$$
$$b_5e_2=+,$$
$$c_4d_3=+,$$
$$c_5e_3=+,$$
$$d_5e_4=+.$$

6. The law of formation of the magnitude conditions is not readily apparent, but a change to the double-index notation for the elements of the determinant suffices to clear up the difficulty.

Writing $|a_1b_2c_3d_4e_5|$ in the form

$$\begin{vmatrix} 11 & 12 & 13 \cdots \\ 21 & 22 & 23 \cdots \\ 31 & 32 & 33 \cdots \\ \cdot & \cdot & \cdot \end{vmatrix},$$

the conditions in question are

$12 . 23 . 31 = 21 . 32 . 13,$
$12 . 24 . 41 = 21 . 42 . 14,$
$12 . 25 . 51 = 21 . 52 . 15,$
$13 . 34 . 41 = 31 . 43 . 14,$
$13 . 35 . 51 = 31 . 53 . 15,$
$14 . 45 . 51 = 41 . 54 . 15,$
$23 . 34 . 42 = 32 . 43 . 24,$
$23 . 35 . 52 = 32 . 53 . 25,$
$24 . 45 . 52 = 42 . 54 . 25,$
$34 . 45 . 53 = 43 . 54 . 35.$

Each condition is thus seen to have its origin in a triad of the five indices 1, 2, 3, 4, 5. For example, from the triad 1 2 3 we form the elements 12, 23, 31, and from these the conjugate elements 21, 32, 13, and so arrive at the condition

$$12 . 23 . 31 = 21 . 32 . 13,$$

which, in order that the distinctive character of its formation may be more apparent, would be still better written

$$\genfrac{}{}{0pt}{}{1}{2}\cdot\genfrac{}{}{0pt}{}{2}{3}\cdot\genfrac{}{}{0pt}{}{3}{1}=\genfrac{}{}{0pt}{}{2}{1}\cdot\genfrac{}{}{0pt}{}{3}{2}\cdot\genfrac{}{}{0pt}{}{1}{3}.$$

In this way the number of conditions is seen to be $C_{5,3}$, the necessary conditions being $C_{4,2}$ in number, and the dependent conditions $C_{4,3}$.

7. The general theorem may consequently be enunciated as follows :—

The n*thic equation*

$$\begin{vmatrix} 11-x & 12 & 13 & \dots \\ 21 & 22-x & 23 & \dots \\ 31 & 32 & 33-x & \dots \\ \dots & \dots & \dots & \dots \end{vmatrix}=0$$

will have all its roots real, if in the case of every pair μ, ν *of the indices* 2, 3, 4, ..., n *we have*

$$\genfrac{}{}{0pt}{}{1}{\mu}\cdot\genfrac{}{}{0pt}{}{\mu}{\nu}\cdot\genfrac{}{}{0pt}{}{\nu}{1}=\genfrac{}{}{0pt}{}{\mu}{1}\cdot\genfrac{}{}{0pt}{}{\nu}{\mu}\cdot\genfrac{}{}{0pt}{}{1}{\nu},$$

and

$$\genfrac{}{}{0pt}{}{1}{\mu}\cdot\genfrac{}{}{0pt}{}{\mu}{1}=+\ ;$$

these conditions implying that in the case of every triad μ, ν, ρ *of the indices* 1, 2, 3, ..., n *we shall have*

$$\genfrac{}{}{0pt}{}{\mu}{\rho}\cdot\genfrac{}{}{0pt}{}{\nu}{\mu}\cdot\genfrac{}{}{0pt}{}{\rho}{\nu}=\genfrac{}{}{0pt}{}{\rho}{\mu}\cdot\genfrac{}{}{0pt}{}{\mu}{\nu}\cdot\genfrac{}{}{0pt}{}{\nu}{\rho},$$

and

$$\genfrac{}{}{0pt}{}{\mu}{\nu}\cdot\genfrac{}{}{0pt}{}{\nu}{\mu}=+.$$

8. Finally, it should be carefully noted that the essence of the whole matter lies in the fact that when the specified conditions hold, the given determinant is transformable into an axi-symmetric determinant in which x is involved in the same way as before.

Mowbray Hall, Capetown, S.A.,
November 30, 1896.

XXXII. *On the Influence of Magnetism on the Nature of the Light emitted by a Substance.* *By* Dr. P. Zeeman[*].

1. SEVERAL years ago, in the course of my measurements concerning the Kerr phenomenon, it occurred to me whether the light of a flame if submitted to the action of magnetism would perhaps undergo any change. The train of reasoning by which I attempted to illustrate to myself the possibility of this is of minor importance at present[†], at any rate I was induced thereby to try the experiment. With an extemporized apparatus the spectrum of a flame, coloured with sodium, placed between the poles of a Ruhmkorff electromagnet, was looked at. The result was negative. Probably I should not have tried this experiment again so soon had not my attention been drawn some two years ago to the following quotation from Maxwell's sketch of Faraday's life. Here (Maxwell, 'Collected Works,' ii. p. 790) we read :—"Before we describe this result we may mention that in 1862 he made the relation between magnetism and light the subject of his very last experimental work. He endeavoured, but in vain, to detect any change in the lines of the spectrum of a flame when the flame was acted on by a powerful magnet." If a Faraday[‡] thought of the possibility of the above-mentioned relation, perhaps it might be yet worth while to try the experiment again with the excellent auxiliaries of spectroscopy of the present time, as I am not aware that it has been done by others[§]. I will take the liberty of stating briefly to the readers of the Philosophical Magazine the results I have obtained up till now.

2. The electromagnet used was one made by Ruhmkorff and of medium size. The magnetizing current furnished by accumulators was in most of the cases 27 amperes, and could

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[*] Communicated by Prof. Oliver Lodge, F.R.S., with the remark that he had verified the author's results so far as related to emission spectra and their polarization.

[†] *Cf.* § 15 and § 16.

[‡] See Appendix for Faraday's own description of the experiment.

[§] See Appendix.

be raised to 35 amperes. The light used was analysed by a Rowland grating, with a radius of 10 ft., and with 14,938 lines per inch. The first spectrum was used, and observed with a micrometer eyepiece with a vertical cross-wire. An accurately adjustable slit is placed near the source of light under the influence of magnetism.

3. Between the paraboloidal poles of an electromagnet, the middle part of the flame from a Bunsen burner was placed. A piece of asbestos impregnated with common salt was put in the flame in such a manner that the two D-lines were seen as narrow and sharply defined lines on the dark ground. The distance between the poles was about 7 mm. If the current was put on, the two D-lines were distinctly widened. If the current was cut off they returned to their original position. The appearing and disappearing of the widening was simultaneous with the putting on and off of the current. The experiment could be repeated an indefinite number of times.

4. The flame of the Bunsen was next interchanged with a flame of coal-gas fed with oxygen. In the same manner as in § 3, asbestos soaked with common salt was introduced into the flame. It ascended vertically between the poles. If the current was put on again the D-lines were widened, becoming perhaps three or four times their former width.

5. With the red lines of lithium, used as carbonate, wholly analogous phenomena were observed.

6. Possibly the observed phenomenon (§§ 3, 4, 5) will be regarded as nothing of any consequence. One may reason in this manner: widening of the lines of the spectrum of an incandescent vapour is caused by increasing the density of the radiating substance and by increasing the temperature *. Now, under the influence of the magnet, the outline of the flame is undoubtedly changed (as is easily seen) hence the temperature and possibly also the density of the vapour is changed. Hence one might be inclined to account in this manner for the phenomenon.

7. Another experiment is not so easily explained. A tube of porcelain, glazed inside and outside, is placed horizontally between the poles with its axis perpendicular to the line joining the poles. The inner diameter of the tube is 18 mm., the outer one 22 mm. The length of the tube is 15 cm. Caps are screwed on at each end of the tube †; these caps are closed by plates of parallel glass at one end, and are surrounded by little water-jackets. In this manner, by means

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* *Cf.*, however, also Pringsheim (Wied. *Ann.* xlv. p. 457, 1892).

† Pringsheim uses similar tubes in his investigation concerning the adiation of gases, *l. c.* p. 430.

of a current of water, the copper caps and the glass plates may be kept sufficiently cool while the porcelain tube is rendered incandescent. In the neighbourhood of the glass plates, side-tubes provided with taps are fastened to the copper caps. With a large Bunsen burner the tube could be made incandescent over a length of 8 cm. The light of an electric lamp, placed sideways at about two metres from the electromagnet, in order to avoid disturbing action on the arc, was made to pass through the tube by means of a metallic mirror. The spectrum of the arc was formed by means of the grating. With the eyepiece the D-lines are focussed. This may be done very accurately, as in the centre of the bright D-lines the narrow reversed lines are often seen. Now a piece of sodium was introduced into the tube. The Bunsen flame is ignited and the temperature begins to rise. A coloured vapour soon begins to fill the tube, being at first of a violet, then of a blue and green colour, and at last quite invisible to the naked eye. The absorption soon diminishes as the temperature is increased. The absorption is especially great in the neighbourhood of the D-lines. At last the two dark D-lines are visible. At this moment the poles of the electromagnet are pushed close to the tube, their distance now being about 24 mm. The absorption-lines now are rather sharp over the greater part of their length. At the top they are thicker, where the spectrum of the lower, denser vapours was observed. Immediately after the closing of the current the lines *widen* and are seemingly *blacker*; if the current is cut off they immediately recover their initial sharpness. The experiment could be repeated several times, till all the sodium had disappeared. The disappearance of the sodium is chiefly to be attributed to the chemical action between it and the glazing of the tube. For further experiments therefore unglazed tubes were used.

8. One may perhaps try to account for the last experiment (§ 7) in this direction:—It is true that the tube used was not of the same temperature at the top and at the bottom; further, it appears from the shape of the D-lines (§ 7) that the density of the vapour of sodium is different at different heights. Hence certainly convection-currents caused by difference of temperature between the top and bottom were present. Under certain plausible suppositions one may calculate that, by the putting on of the electromagnet, differences of pressure are originated in the tube of the same order of magnitude as those caused by the difference of temperature. Hence the magnetization will push *e. g.* the denser layer at the bottom in the direction of the axis of the tube. The lines

become widened. For their width at a given height is chiefly determined by the number of incandescent particles at that height in the direction of the axis of the tube. Although this explanation still leaves some difficulties, certainly something may be said for it.

9. The explanation of the widening of the lines attempted in § 8 is no longer applicable to the following variation of the experiment, in which an unglazed tube is used. The inner diameter of the tube, about 1 mm. thick, was 10 mm. The poles of the electromagnet could be moved till the distance was 14 mm. The tube now was heated by means of the blowpipe instead of with the Bunsen burner, and became in the middle part white hot. The blowpipe and the smaller diameter of the tube make it easier to bring the upper and lower parts to the same temperature. This is now higher than before (§ 7), and the sodium lines remain visible continuously*. One now can wait till the density of the sodium vapour is the same at various heights. By rotating the tube continuously round its axis I have still further promoted this. The absorption-lines now are equally broad from the top to the bottom. When the electromagnet was put on, the absorption-lines immediately widened along their whole length. Now the explanation in the manner of § 8 fails.

10. I should like to have studied the influence of magnetism on the spectrum of a solid. Oxide of erbium has, as was found by Bunsen or Bahr, the remarkable property of giving by incandescence a spectrum with bright lines. With the dispersion used, however, the edges of these lines were too indistinct to serve my purpose.

11. The different experiments from §§ 3 to 9 make it more and more probable that the absorption- and hence also the emission lines of an incandescent vapour are widened by the action of magnetism. Now if this is really the case, then by the action of magnetism on the free vibrations of the atoms, which are the cause of the line-spectrum, other vibrations of changed period must be superposed. That it is really inevitable to admit this specific action of magnetism is proved, I think, by the rest of the present paper.

12. From the representation I had formed to myself of the nature of the forces acting in the magnetic field on the atoms, it seemed to me to follow that with a band-spectrum and with external magnetic forces the phenomenon I had found with a line-spectrum would not occur.

It is, however, very probable that the difference between a band- and a line-spectrum is not of a quantitative but of a

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* Pringsheim, *l. c.* p. 456.

qualitative kind [*]. In the case of a band-spectrum the molecules are complicated, in the case of a line-spectrum the widely separated molecules contain but a few atoms. Further investigation has shown that the representation I had formed of the cause of the widening in the case of a line-spectrum in the main was really true.

13. A glass tube, closed at both ends by glass plates with parallel faces and containing a piece of iodine, was placed between the poles of the Ruhmkorff electromagnet in the same manner as the tube of porcelain in § 7. A small flame under the tube vaporized the iodine, the violet vapour filling the tube.

By means of electric light the absorption-spectrum could be examined. As the temperature is low this is the band-spectrum. With the high dispersion used, there are seen in the bands a very great number of fine dark lines. If the current round the magnet is closed, *no* change in the dark lines is observed, which is contrary to the result of the experiments with sodium vapour.

The absence of the phenomenon in this case supports the explanation, that even in the first experiment, with sodium vapour (§ 7), the convection-currents had no influence. For in the case now considered, the convection-currents originated by magnetism, which I believed to be possible in that case, apparently are insufficient to cause a change of the spectrum; yet, though I could not see it in the appearance of the absorption-lines (*cf.* § 7), the band-spectrum is, like the line-spectrum, very sensible to changes of density and of temperature.

14. Although the means at my disposal did not enable me to execute more than a preliminary approximate measurement, I yet thought it of importance to determine approximately the value of the magnetic change of the period.

The widening of the sodium lines to both sides amounted to about $\frac{1}{40}$ of the distance between the said lines, the intensity of the magnetic field being about 10^4 C.G.S. units. Hence follows a positive and negative magnetic change of $\frac{1}{40,000}$ of the period.

15. The train of reasoning mentioned in (1), by which I was induced to search after an influence of magnetism, was at first the following:—If the hypothesis is true that in a magnetic field a rotatory motion of the æther is going on, the axis of rotation being in the direction of the magnetic forces (Kelvin and Maxwell), and if the radiation of light may be imagined as caused by the motion of the atoms, relative to

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[*] Kayser in Winkelmann's *Handbuch*, ii. 1, p. 421.

the centre of mass of the molecule, revolving in all kinds of orbits, suppose for simplicity circles; then the period, or, what comes to the same, the time of describing the circumference of these circles, will be determined by the forces acting between the atoms, and then deviations of the period to both sides will occur through the influence of the perturbing forces between æther and atoms. The sign of the deviation of course will be determined by the direction of motion, as seen from along the lines of force. The deviation will be the greater the nearer the plane of the circle approximates to a position perpendicular to the lines of force.

16. Somewhat later I elucidated the subject by representing to myself the influence exercised on the period of a vibrating system if this is linked together with another in rapid rotatory motion. Lord Kelvin (now 40 years ago*) gave the solution of the following problem:—Let the two ends of a cord of any length be attached to two points at the ends of a horizontal arm made to rotate round a vertical axis through its middle point at a constant angular velocity, and let a second cord bearing a material point be attached to the middle of the first cord. The motion now is investigated in the case when the point is infinitely little disturbed from its position of equilibrium. With great angular velocity the solution becomes rather simple. Circular vibrations of the point in contrary directions have slightly different periods. If for the double pendulum we substitute a luminiferous atom, and for the rotating arm the rotational motion about the magnetic lines of force, the relation of the mechanical problem to our case will be clear.

It need not be proved that the above-mentioned considerations are at most of any value as indications of somewhat analogous cases. I communicate them, however, because they were the first motive of my experiments.

17. A real explanation of the magnetic change of the period seemed to me to follow from Prof. Lorentz's theory†.

In this theory it is assumed that in all bodies small electrically charged particles with a definite mass are present, that all electric phenomena are dependent upon the configuration and motion of these "ions," and that light-vibrations are vibrations of these ions. Then the charge, configuration, and motion of the ions completely determine the state of the æther. The said ion, moving in a magnetic field, experiences mechanical forces of the kind above mentioned, and these must

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* Proc. Roy. Soc. 1856.

† Lorentz, *La Théorie électromagnétique de Maxwell.* Leyde, 1892; and *Versuch einer Theorie der electrischen und optischen Erscheinungen in bewegten Körpern.* Leiden, 1895.

explain the variation of the period. Prof. Lorentz, to whom I communicated these considerations, at once kindly informed me of the manner in which, according to his theory, the motion of an ion in a magnetic field is to be calculated, and pointed out to me that, if the explanation following from his theory be true, the edges of the lines of the spectrum ought to be circularly polarized. The amount of widening might then be used to determine the ratio between charge and mass, to be attributed in this theory to a particle giving out the vibrations of light.

The above-mentioned extremely remarkable conclusion of Prof. Lorentz relating to the state of polarization in the magnetically widened lines I have found to be fully confirmed by experiment (§ 20).

18. We shall now proceed to establish the equations of motion of a vibrating ion, when it is moving in the plane of (x, y) in a uniform magnetic field in which the magnetic force is everywhere parallel to the axis of z and equal to H. The axes are chosen so that if x is drawn to the east, y to the north, z is upwards. Let e be the charge (in electromagnetic measure) of the positively charged ion, m its mass. The equations of relative motion then are :—

$$\left.\begin{aligned} m\frac{d^2x}{dt^2} &= -k^2x + e\mathrm{H}\frac{dy}{dt} \\ m\frac{d^2y}{dt^2} &= -k^2y - e\mathrm{H}\frac{dx}{dt} \end{aligned}\right\} . \quad . \quad . \quad . \quad (1)^*$$

The first term of the second member expresses the elastic force drawing back the ion to its position of equilibrium ; the second term gives the mechanical force due to the magnetic field. They are satisfied by

$$\left.\begin{aligned} x &= \alpha e^{st} \\ y &= \beta e^{st} \end{aligned}\right\} , \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

provided that

$$\left.\begin{aligned} ms^2\alpha &= -k^2\alpha + e\mathrm{H}s\beta \\ ms^2\beta &= -k^2\beta - e\mathrm{H}s\alpha \end{aligned}\right\} . \quad . \quad . \quad . \quad . \quad (3)$$

where m, k, e are to be regarded as known quantities.

For us the period T is particularly interesting. If $\mathrm{H}=0$, it follows from (3) that

$$s = i\frac{k}{\sqrt{m}} = i\frac{2\pi}{\mathrm{T}},$$

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* These equations are like those of the Foucault pendulum, and of course lead to similar results.

or

$$T=\frac{2\pi\sqrt{m}}{k}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

If H is not 0, it follows from (3) approximately that

$$s=i\frac{k}{\sqrt{m}}\left(1\mp\frac{eH}{2k\sqrt{m}}\right).$$

Putting T′ for the period in this case, we have

$$T'=\frac{2\pi\sqrt{m}}{k}\left(1\pm\frac{eH}{2k\sqrt{m}}\right). \quad . \quad . \quad . \quad . \quad (5)$$

Hence the ratio of the change of period to the original period becomes

$$\frac{eH}{2k\sqrt{m}}=\frac{e}{m}\cdot\frac{HT}{4\pi}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

A particular solution of (1) is that representing the motion of the ions in circles. If revolving in the positive direction (viz., in the direction of the hands of a watch for an observer standing at the side towards which the lines of force are running) the period is somewhat less than if revolving in the negative direction. The period in the first case is determined by the value of (5) with the minus sign, in the second with the plus.

The general solution of (1) shows that the ions describe, besides circles, also slowly rotating elliptical orbits. In the general case, the original motion of the ion having an arbitrary position in space, it is perfectly clear that the projection of the motion in the plane of (x, y) has the same character. The motion resolved in the direction of the axis of z is a simple harmonic motion, independent of and not disturbing the one in the plane of (x, y), and hence one not influenced by the magnetic forces. Of course, the consideration of the motion of an ion now given is only to be regarded as the very first sketch of the theory of luminiferous motions.

19. Imagine an observer looking at a flame placed in a magnetic field in a direction such that the lines of force run towards or from him.

Let us suppose that the said observer could see the very ions of § 18 as they are revolving; then the following will be remarked :—There are some ions moving in circles and hence emitting circularly polarized light; if the motion is round in the positive direction the period will, for instance, be longer than with no magnetic field; if in the negative direction, shorter. There will also be ions seemingly stationary and really moving parallel to the lines of force with unaltered period. In the third place there are ions which seem to move in rotating elliptical orbits.

If one desires to know the state of the æther originated by the moving ions one may use the following rule, deduced by Prof. Lorentz from the general theory:—Let us suppose that in a molecule an ion P—of which the position of equilibrium is P_0—has two or more motions *at the same time*, viz. let the vector P_0P always be obtained by adding the vectors P_0P which should occur in each of the component motions at that moment; then the state in the æther at a very great distance in comparison with P_0P will be obtained by superposing the states which would occur in the two cases taken separately.

Hence it follows in the first place that a circular motion of an ion gives circularly polarized light to points on the axis of the circle.

Further, one may choose instead of the above-considered elliptical orbits a resolution more suited to our purpose. One may resolve the motion of the ion, existing before the putting on of the magnetic force, into a rectilinear harmonic motion parallel to the axis of z and two circular (right-handed and left-handed) motions in the plane of (x, y).

The first remains unchanged under the influence of the magnetic force, the periods of the last are changed.

By the action of the grating the vibrations originated by the motion of the ions are sorted according to the period, and hence the complete motion is broken up into three groups. The line will be a triplet. At any rate one may expect that the line of the spectrum will be wider than in the absence of the magnetic field, and that the edges will give out circularly-polarized light*.

20. A confirmation of the last conclusion may be certainly taken as a confirmation of the guiding idea of Prof. Lorentz's theory. To decide this point by experiment, the electromagnet of § 2, but now with pierced poles, was placed so that the axes of the holes were in the same straight line with the centre of the grating. The sodium lines were observed with an eyepiece with a vertical cross-wire. Between the grating and the eyepiece were placed the quarter-undulation plate and nicol which I formerly used in my investigation of the light normally reflected from a polarly magnetized iron mirror†.

The plate and the nicol were placed relatively in such a

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* I saw afterwards that Stoney, Trans. Roy. Soc. Dublin, iv., endeavours to explain the existence of doublets and triplets in a spectrum by the rotation of the elliptical orbits of the "electrons" under the influence of perturbing forces.

† Zeeman, 'Communications of the Leyden Laboratory,' no. 15.

manner that right-handed circularly-polarized light was quenched. Now according to the preceding the widened line must at one edge be right-handed circularly-polarized, at the other edge left-handed. By a rotation of the analyser over 90° the light that was first extinguished will be transmitted, and *vice versâ*. Or, if first the right edge of the line is visible in the apparatus, a reversal of the direction of the current makes the left edge visible. The cross-wire of the eyepiece was set in the bright line. At the reversal of the current the visible line moved! This experiment could be repeated any number of times.

21. A small variation of the preceding experiment is the following. With unchanged position of the quarter-wave plate the analyser is turned round. The widened line is then, during one revolution, twice wide and twice fine.

22. The electromagnet was turned 90° in a horizontal plane from the position of § 20, the lines of force now being perpendicular to the line joining the slit with the grating. The edges of the widened line now appeared to be plane polarized, at least in so far as the present apparatus permitted to see, the plane of polarization being perpendicular to the line of the spectrum. This phenomenon is at once evident from the consideration § 19. The circular orbits of the ions being perpendicular to the lines of force are now seen on their edges.

23. The experiments 20 to 22 may be regarded as a proof that the light-vibrations are caused by the motion of ions, as introduced by Prof. Lorentz in his theory of electricity. From the measured widening (§ 14) by means of relation (6), the ratio e/m may now be deduced. It thus appears that e/m is of the order of magnitude 10^7 electromagnetic C.G.S. units. Of course this result from theory is only to be considered as a first approximation.

24. It may be deduced from the experiment of § 20 whether the positive or the negative ion revolves.

If the lines of force were running towards the grating, the right-handed circularly-polarized rays appeared to have the smaller period. Hence in connexion with § 18 it follows that the positive ions revolve, or at least describe the greater orbit.

25. Now that the magnetization of the lines of a spectrum can be interpreted in the light of the theory of Prof. Lorentz, the further consideration of it becomes specially attractive. A series of further questions already present themselves. It seems very promising to investigate the motion of the ions for various substances, under varying circumstances of temperature and pressure, with varying

intensities of the magnetization. Further inquiry must also decide as to how far the strong magnetic forces existing according to some at the surface of the sun may change its spectrum.

The experiments described have been made in the Physical Laboratory at Leyden, to the Director of which, Prof. Kamerlingh Onnes, I am under great obligations for continuous interest in the present subject.

Amsterdam, Jan. 1897.

Appendix.

Since the publication of my original paper in the Proceedings of the Academy at Amsterdam, and while the present paper was in the press, I have become acquainted with two attempts, till now unknown to me, in the same direction, and also with the original account of Faraday's experiment referred to in § 1. The last is to be found in Faraday's 'Life' by Dr. Bence Jones, vol. ii. p. 449 (1870), and as it is extremely remarkable I will reprint it here :—

"1862 was the last year of experimental research. Steinheil's apparatus for producing the spectrum of different substances gave a new method by which the action of magnetic poles upon light could be tried. In January he made himself familiar with the apparatus, and then he tried the action of the great magnet on the spectrum of chloride of sodium, chloride of barium, chloride of strontium, and chloride of lithium."

On March 12 he writes :—"Apparatus as on last day (January 28), but only ten pairs of voltaic battery for the electromagnet.

"The colourless gas-flame ascended between the poles of the magnet, and the salts of sodium, lithium, &c. were used to give colour. A Nicol's polarizer was placed just before the intense magnetic field, and an analyser at the other extreme of the apparatus. Then the electromagnet was made, and unmade, but not the slightest trace of effect on or change in the lines in the spectrum was observed in any position of polarizer or analyser.

"Two other pierced poles were adjusted at the magnet, the coloured flame established between them, and only that ray taken up by the optic apparatus which came to it along the axis of the poles, *i. e.* in the magnetic axis, or line of magnetic force. Then the electromagnet was excited and rendered neutral, but not the slightest effect on the polarized or unpolarized ray was observed."

"This was the last experimental research that Faraday made."

In 1875 we have a paper by Prof. Tait, who has kindly sent me a copy, "On a Possible Influence of Magnetism on the Absorption of Light, and some correlated subjects" (Proc. Roy. Soc. of Edinburgh, Session 1875–76, p. 118). Prof. Tait remarks that a paper by Professor Forbes, read at the Society, and some remarks upon it by Maxwell, have recalled to him an experiment tried by him several times, but which hitherto has led to no result. Then the paper proceeds:—

"The idea is briefly this.—The explanation of Faraday's rotation of the plane of polarization of light by a transparent diamagnetic requires, as shown by Thomson, molecular rotation of the luminiferous medium. The plane-polarized ray is broken up, while in the medium, into its circularly-polarized components, one of which rotates with the æther so as to have its period accelerated, the other against it in a retarded period. Now, suppose the medium to absorb one definite wave-length only, then—if the absorption is not interfered with by the magnetic action—the portion absorbed in one ray will be of a shorter, in the other of a longer, period than if there had been no magnetic force; and thus, what was originally a single dark absorption line might become a double line, the components being less dark than the single one."

Hence here the idea is perfectly clearly expressed of the experiment, tried in vain; an idea closely akin to that of § 15 above, both being in fact founded on Kelvin's theory of the molecular rotation of the luminiferous medium, though not directly applicable to the experiment of § 9, in which case the lines of magnetic force are perpendicular to the axis of the tube.

In the second place I have to mention two papers by the late M. Fievez, to which attention has been drawn by M. van Aubel, in a letter to Prof. Onnes and intended for communication to the Academy of Sciences, Amsterdam. Prof. Onnes read the letter at the January meeting, and made at the same time some explanatory remarks of which in the following I make free and extensive use. The papers referred to are:—M. Fievez, "De l'Influence du Magnétisme sur les caractères des Raies spectrales" (*Bulletin de l'Acad. des Sciences de Belgique*, 3^e^ série, tome ix. p. 381, 1885); and Fievez, "Essai sur l'Origine des Raies de Fraunhofer, en rapport avec la Constitution du Soleil" (*l. c.* 3^e^ série, tome xii. p. 30, 1886). Here experiments are described as in §§ 4 and 13 of the present paper. Nothing, however, is observed about the widening of the absorption-lines, nor about the polarization of the emitted light. The results obtained by M. Fievez merit careful attention and consideration. He has observed with a flame in a magnetic field not only widening but reversal and double reversal of the lines of the spectrum, the lines at the same time becoming more brilliant. Unfortunately quantitative details are not given. The facts observed in some cases by Fievez are qualitatively not in accordance with my observations or what is to be deduced from my results. Hence even in the cases where the results are qualitatively in accordance, the question remains whether Fievez has observed *the same phenomenon.* The field used by Fievez seems to have been more intense than the one I had at my disposal. Is it possible perhaps to account in this manner for the "double renversement (c'est-à-dire l'apparition d'une raie brillante au millieu de la raie noire élargie)"? I think the answer must be in the negative. For, arguing from § 19, a line must widen, or else, the field being very intense, become a triplet. We cannot but understand from Fievez's description of the experiment that the light was emitted perpendicular to the lines of force. Now the double reversed line of Fievez is not the triplet to be expected from theory, for it is expressly stated by Fievez that the line experimented upon is not the simple line of the spectrum, but one previously widened and reversed (by some agency independent of magnetism). By the action of magnetism a brilliant line in the centre of the black line appears. Hence perhaps one may interpret the case of double reversal as a direct action of magnetism, but then only as a doubling of the absorption-line and not as a division of the original line into three parts. As the application of Lorentz's theory given in § 18 is confessedly only a very first sketch, further theoretical and experimental evidence is wanted before we are able to decide whether in the experiment of Fievez a specific action of magnetism on light or perturbing circumstances have been prevalent. Indeed one may make the same objection to M. Fievez's experiment as I myself have made to my own analogous experiment in § 6.

The whole of the phenomena observed by Fievez can readily be attributed to a change of temperature by the well-known actions of the field upon the flame (change in its direction or outline, magnetic convection, &c.); and the last sentence of his paper states that "les phénomènes qui se manifestent sous l'action du magnétisme sont identiquement les mêmes que ceux produits par une élévation de température." The negative result obtained by Fievez with absorption-spectra would without further consideration (as in § 12) point in the same direction. The inference to be drawn from Fievez's experiments alone would rather be, I think, that the temperature of the flame is changed in his experiments than that a specific action of magnetism on the emission and absorption of light exists. By experiments already in progress I hope to settle the dubious points.

Summarizing we may say:—Had the experiments of Fievez come to my knowledge they would have been a motive for me to further investigation, Fievez not having prosecuted his inquiry up to a decisive result. At least at present it remains even doubtful whether the phenomenon observed by Fievez with a magnetized flame is really to be attributed to *the specific action of the magnetic field on the period of the vibrations of light*, which I have found and undoubtedly proved by the experimental confirmation of Lorentz's predictions.

Amsterdam, February 1897.

XXXIII. *Notices respecting New Books.*

Das Wesen der Elektrizität und des Magnetismus, auf Grund eines einheitlichen Substanzbegriffes. *By* J. G. Vogt. Leipsic: Wiest, 1897.

THE author of this treatise is obliged to borrow from the English language expressions strong enough to characterize the ordinarily accepted notions concerning the æther and matter, which he designates as "absurd" and "stupid." His philosophic mind refuses, much more emphatically than that of a certain eminent British statesman, to admit the existence of an æther and of atoms in a state of perpetual vibration. He sees no difficulty, however, in filling space with a continuous, contractile æther, containing nuclei which undergo periodic fluctuations of density; matter consists of the permanently condensed portions of this æther. We are not told how these centres of condensation differ from the rest of the æther, or by what process they pull the surrounding medium; their only function is to perpetually expand and contract. In fact, the theory is only the opposite extreme of the kinetic theory; every point in the æther is supposed to be the seat of potential- or strain-energy instead of kinetic energy.

The author avoids any mathematical statement of his theory, sometimes by remarking that he is not a physicist, which is an obvious fact, and sometimes by explaining that his work is intended as a popular exposition. The volume is printed in German characters, and its illustrations are so extremely bad that the author finds it necessary to make an apology for them. J. L. H.

XXXIV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 151.]

Dec. 16th, 1896.—Dr. Henry Hicks, F.R.S., President, in the Chair.

THE following communications were read:—

1. 'On the Subdivisions of the Carboniferous Series in Great Britain, and the True Position of the Beds mapped as the Yoredale Series.' By Dr. Wheelton Hind, F.R.C.S., F.G.S.

In this paper the author gives a summary of our knowledge of the local divisions of the Carboniferous system, and criticizes the present classifications in vogue, laying special stress upon the local variations in the lithological characters of the rocks, and summing up to a large extent the fossil evidence which is available. He maintains that the Yoredale Beds are largely the equivalents of the beds which have elsewhere been referred to the Mountain Limestone Series, though some local beds which have been included in the Yoredale Series may rather be the equivalents of the Millstone Grit. He advocates the abolition of the term 'Yoredale Series' as applied to a subdivision of the Carboniferous strata comparable in importance to such divisions as Carboniferous Limestone and Millstone Grit, but believes that the term may be usefully employed to denote the changes in character of the beds due to causes which operated locally, and to represent the Carboniferous Series as it occurs in Wensleydale, Swaledale, Teesdale, and the upper parts of Wharfedale and Ribblesdale.

He would divide the rocks of the Carboniferous System into an Upper Carboniferous or Anthraciferous Series, and a Lower Carboniferous or Calcareous Series; and indicates the occurrence of three very different faunas in the Carboniferous rocks, viz.:—(i) a Coal-Measure fauna rich in fish-remains, with the molluscan genera *Carbonicola*, *Anthracomya*, and *Naiadites* (essentially a freshwater fauna); (ii) the Lower Coal-Measure and Grit fauna, largely marine but littoral, with *Aviculopecten*, *Posidonella*, *Nautilus*, *Goniatites*, and peculiar gasteropoda; and (iii) a Limestone fauna, essentially marine, very rich in brachiopods, and containing *Pecten*, *Avicula*, *Edmondia*, *Sanguinolites*, and many other lamellibranchs, gasteropods, such as *Euomphalus*, *Pleurotomaria*, *Murchisonia*, and *Loxonema*, and certain peculiar cephalopods.

2. 'Note on Volcanic Bombs in the Schalsteins of Nassau.' By Prof. E. Kayser, Ph.D., For.Corr.G.S.

The bombs forming the subject of this communication occur in two localities in the neighbourhood of Oberscheld near Dillenburg. They are generally rounded, though sometimes angular, and vary in size from that of a nut to that of a man's head. Each consists of a kernel of coarse-grained rock representing a fragment of limestone altered by metamorphism, surrounded by a rind of amygdaloidal rock due to the inclusion of the fragment in molten lava. They demonstrate the pyroclastic origin of the Schalsteins, and also prove the similarity between the old Devonian volcanoes and those which are now active.

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

[FIFTH SERIES.]

APRIL 1897.

XXXV. *On the Electrification of Gases exposed to Röntgen Rays, and the Absorption of Röntgen Radiation by Gases and Vapours.* *By* E. Rutherford, *M.A.*, 1851 *Exhibition Science Scholar, University of New Zealand, Trinity College, Cambridge*[*].

In a recent paper by Prof. J. J. Thomson and myself "On the Passage of Electricity through Gases exposed to Röntgen Rays" (Phil. Mag. Nov. 1896), a method of obtaining electrified air by means of the Röntgen rays was very briefly explained.

The present paper deals with further experiments which have been made to investigate more fully the way in which electrified gases can be obtained by means of the Röntgen rays, and also to examine the properties of the charged gas. The opacity of gases for Röntgen radiation has also been examined.

A gas becomes a temporary conductor under the influence of the Röntgen rays, and preserves its power of conducting some short time after the rays have ceased to act; since the conduction in the gas is probably due to the convection of charged particles which travel through the gas with a velocity of the order of 1 cm. a second for a potential gradient of one volt per cm., it is not surprising that we can separate the positive from the negative conducting particles before they give up their charges to the electrodes.

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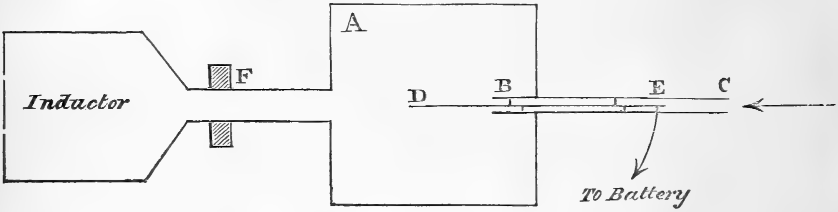
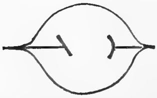
[*] Communicated by Prof. J. J. Thomson, F.R.S.

The method of separation used was to direct a rapid current of air or other gas along the surface of the charged electrode of a vessel exposed to the Röntgen rays; a large metal cylinder was taken, either of thin metal to allow the rays to readily pass through the side, or a piece was cut out and a sheet of very thin metal substituted to serve the same purpose.

Fig. 1.





A (fig. 1) was the metal cylinder, B C a glass tube fixed centrally inside the cylinder. The wire D E was fixed in the glass tube B C and supported by thin metal spikes in the centre of the tube. Several inches of wire, B D, projected from the glass tube. A current of gas was sent from a pair of bellows or a gas reservoir along the tube C B, and then along a metal tube into an insulated conductor connected with one pair of quadrants of an electrometer, the other pair being connected to earth. The wire D E was connected to one pole of a battery of small lead cells, the other pole being to earth. The outside of the cylinder was connected to earth, and the bulb and Ruhmkorff coil were placed inside a metal tank, so as to completely screen the outside apparatus from electrostatic disturbances. A hole was cut in the tank, and the bulb arranged so as to allow the rays to fall on the part B D of the charged wire.

When the bulb was not working, however rapid a current of air was sent along the charged electrode, no electrification was obtained in the inductor, but the moment the rays were turned on the inductor became charged *opposite* in sign to the charged wire. The deflexion of the electrometer continuously increased as long as the rays and blast of air were acting.

The inductor was generally placed some feet from the generating vessel A, the air passing to the inductor through a metal tube of 3 cm. diameter. Since the electrification of the inductor is opposite in sign to the charged wire, the effect can in no way be due to conduction through the Röntgenized air from the charged electrode to the inductor, since the inductor would then be charged to the *same* sign as the electrode.

A small plug of glass wool placed in the metal tube between the generator and the inductor completely stopped all electrification, and the inductor received no charge, however rapid a blast of air was sent through the apparatus.

On account of the large quantity of air blown through the inductor in order to obtain a convenient deflexion on the electrometer (an amount sufficient to fill the inductor many times over), only a small proportion of the charge could be blown out. If, however, a gentle current of electrified air was blown into the inductor for two or three seconds, and the rays then stopped, it was found possible to blow out most of the charge again, after a short interval, provided there was a fairly wide opening in the inductor. If the opening was stopped with a plug of glass wool, it was found impossible to blow out the slightest amount, since the electrified particles gave up their charge freely to the glass wool.

Since the glass wool has the power of completely discharging the electrification both positive and negative, a short wide metal cylinder lightly packed with glass wool was used instead of the inductor for testing the amount of the electrification in most of the experiments that follow.

The amount of electrification obtained varied with the potential of the charged wire and the velocity of the current of air. The relation between the amount of electrification and the E.M.F. of the charged wire is shown in the Table below:—

E.M.F. in volts.	Amount of Electrification in scale-divisions.
17	60
35	100
70	130
200	82

The amount of electrification increases up to a certain point and then diminishes. The maximum amount of electrification is closely connected with the value of the E.M.F. which is just sufficient to give the saturation-value of the current through the gas. In the above table the saturation-value of the E.M.F. was about 70 volts, and this corresponds to the maximum amount of electrification.

Since the velocity of the conducting particles increases with the E.M.F. but the current through the gas remains constant, when the E.M.F. is raised above its saturation-value it is to be expected that a greater proportion of the conducting particles would reach the electrode. This agrees with experiment, for as the E.M.F. is increased above a certain value the amount of electrification obtained steadily diminishes.

The amount of electrification obtained for a given E.M.F. increases at first with the velocity of the blast, and then tends to a maximum value, which cannot be increased, however rapid a blast is sent along the wire.

An experiment proving conclusively that the amount of electrification is intimately connected with the conduction of electricity through the gas is as follows:—The electrode along which the air was blown was carefully insulated and connected to one pair of quadrants of the electrometer. The two pairs of quadrants were charged up to the same potential and then insulated from each other, and the rate of leak of the charged wire determined. The rate of leak steadily diminished with increase of velocity of the blast; when the air issuing from the glass tube had a velocity of about 1000 cm. per second the rate of leak was only one fourth of its value when the air was still, and the amount of electrification in the air passing from the wire, as tested by the glass wool cylinder, was nearly equal to the quantity of electricity corresponding to the difference between the two rates of leak.

We should not expect them to be exactly equal, since some of the Röntgenized air containing both positively and negatively charged particles is also blown out.

The charged gas obtained in this way is thus due to an excess of the positive or negative conducting particles, whatever they may be, to which conduction in gas under the Röntgen rays is due.

In all these experiments precautions were taken against dust. It was found that the amount of electrification obtained was independent of the quantity of dust in the air provided the velocity of the issuing blast was kept constant. The air in one case was sent through a long tube filled with glass wool into the gas reservoir, which was then allowed to stand for a couple of days without being disturbed. The air in passing from the reservoir to the generator was again passed through glass wool, but the effect obtained was exactly the same as if the dust-charged air from the room were sent directly through the apparatus.

Electrification from Charged Insulators.

If the central electrode through which the air was blown was coated with paraffin or sealing-wax, it was found that the amount of electrification obtained was at first about equal to the amount with the bare electrode. If the bulb was kept working the amount of electrification diminished after a time. The central electrode was then connected to earth, and when the x-rays were acting electrification could still be obtained, but of *opposite* sign to that obtained before. If the wire with the coating of dielectric on its surface was kept charged to a high potential and the rays continued for some time, on applying a smaller E.M.F. to the wire in the same direction the sign of the electrification is generally changed.

The explanation of these and similar phenomena is simple if we consider that the conducting particles of the gas either give up their charge to the surface of the insulator, or adhere to the surface which becomes charged opposite in sign to the wire itself. If the bulb is kept working, the electromotive intensity acting on the gas is diminished, owing to the effect of the oppositely charged insulator. The amount of electrification obtained therefore diminishes if the E.M.F. is not well above the saturation-value. If the central electrode be then connected to earth, the charged insulator causes a current through the gas in the opposite direction, and thus changes the sign of the charge in the gas blown out. If the charge on the insulator is large, as is the case if the central wire has been raised, for example, to a potential of 200 volts and exposed to the rays for some time, on applying an E.M.F. of 30 volts, say, in the same direction the electrification changes sign. In this case, the electromotive intensity due to the charged insulator is greater and opposite in sign to that due to the 30 volts, and so the current through the gas is reversed.

The sign of the electrification obtained when the wire is covered with insulating material is thus dependent on the amount and sign of the charge on the surface of the dielectric.

From a charged wire coated with paraffin or sealing-wax which had been exposed to the Röntgen rays for several minutes, it was found possible to obtain electrified air, by directing a current of air along its surface, several hours after the central electrode had been connected to earth.

Properties of the Charged Gas.

Since the charged gas obtained is due to the separation of the oppositely charged conducting particles to which conduction in a gas under the x-rays is probably due, we should expect the positively and negatively electrified gas to closely resemble Röntgenized air in its properties, and such is found to be the case.

The gas completely loses its charge in its passage through the pores in a plug of glass wool; while Röntgenized air, after being forced through glass wool, loses all trace of conductivity.

The gas readily gives up its charge to any conducting or insulating surface against which it impinges. The greater amount of electrification is discharged in the passage of the gas down a long tube. If the electrified air is allowed to impinge against the surface of an insulated metal plate, it gives up a portion of its charge to the metal. The facility with which the gas is discharged is to be expected, since no evidence of polarization has been found in the conduction of the gas exposed to the Röntgen rays when metal electrodes are used.

A remarkable property of the electrified gas is that positive and negative electrification are not discharged with equal facility by all metals. When the charged gas was passed through a long zinc tube, the amount of negative electrification on the issuing gas was always less than the amount of positive for the same velocity of the blast. By insulating the zinc tube it was found that it received a greater charge of negative than of positive. In order to test this difference, cylinders of zinc, tin, and copper were made of the same size, and the charged gas forced through them. It was found that zinc and tin discharged negative electrification more rapidly than positive, the difference in general amounting to about 20 per cent. Copper apparently discharged the positive and negative with about equal facility, but many experiments seemed to point to the conclusion that even in the case of copper negative was slightly more readily discharged than positive. If the electrified gas impinged against insulated plates of different metals, the same general results were obtained.

Not only was there a difference in the discharging powers of positive and negative electrification for any particular metal, but a copper plate, for example, discharged positive more readily than a zinc plate placed exactly in the same position, while the zinc plate discharged more negative than the copper.

Other metals, like aluminium, lead, were tested, and in all cases negative electricity was discharged with slightly more facility than positive.

The variable discharging power of the different metals agrees in some respects with the results obtained by Minchin ('Electrician,' March 27, 1896), who found that under the influence of the Röntgen rays insulated metal plates were all charged up to a small potential. According to his results copper was charged positively and zinc negatively, while sodium was highly negative. He also found that the potential to which some of the metals could be raised depended on the degree of polish of the exposed surface. In the experiments on the discharging power of the metals, the results were dependent to some extent on the brightness of the surface, especially in the case of tin and zinc.

The amount of electrification discharged by a metal tube one inch in diameter and a foot long is very large, amounting in some cases to over one fourth of the whole charge on the gas. It must be remembered, however, that the current of air conveying the charged gas is travelling at a high velocity, and is, in consequence, in a state of violent eddying motion, so that probably a large proportion of the gas approaches near the surface in its passage along the tube.

The charge is taken from the gas not only when it passes through metal tubes, but also when it passes through tubes coated with an insulator. A metal tube was taken and coated with a thin layer of paraffin, and it was found that the charge on the cylinder was about the same as with the clean metal. It was difficult to determine with certainty whether insulators exhibited similar properties to metals in regard to discharging power. The amounts of positive and of negative electrification discharged were approximately the same, but the differences were too small to make certain of.

The *conductivity* of the charged gas was tested by placing an insulated wire kept at a constant potential inside a metal vessel through which the electrified gas was blown. It was found that when the electrification was of the same sign as the charged wire, the gas gave up its charge to the outside vessel, and when of the opposite sign, to the charged wire. The current through the gas was only temporary, and ceased as soon as the current of electrified gas was stopped.

Electrification of Different Gases.

All the gases which were experimented with could be electrified in the same way as air. A gas-bag was filled with the gas to be tested, and then forced along the electrode as in the case of air, care being taken to allow the gas to run through some time before the rays were turned on, in order to remove the air as far as possible from the generating vessel.

The amounts of electrification obtained for a given velocity of the gas and intensity of the rays varied with the conductivity of the gas under the x-rays. Gases that have a greater conductivity than air gave more electrification than air. Oxygen and coal-gas gave slightly less electrification than air, while carbon dioxide gave slightly more—the amounts being sensibly proportional to their conductivities.

The vapour of methyl iodide was tried, which has a very high conductivity—over 20 times that due to air. Only a partial test could be made of it, as sufficient quantity of the vapour was not obtainable. Some of the liquid was placed in the generator (fig. 1), and gently heated to its boiling-point, till the vessel was filled with vapour. The rays were then turned on, and a rapid current of air sent for 3 or 4 seconds along the central electrode. The amount of electrification obtained was over 5 times the amount from air in the same time. After a few seconds the highly conducting vapour was blown out and the electrification became sensibly that due to air alone. If a current of the vapour could have been sent instead of a current of air, it is probable that the amount of electrification obtained would have been over 20 times that of air in the same time.

The experiments on hydrogen were interesting as bearing on the question of the relative velocity of the conducting particles of hydrogen and air. For a given small weight on the gas-bag the amount of electrification from air was 2·5 times that due to hydrogen in the same time. As the weight was increased the ratio fell to 1·5. This is as we should expect if the velocity of the hydrogen ions was greater than those of air. For small velocities of the blast a much smaller proportion of the hydrogen than the air ions escape. As the velocity is increased the amount of electrification from the air increases slowly, as nearly all of the ions are blown out, while the amount from hydrogen increases rapidly as the velocity increases.

In a previous paper (*loc. cit.*) it was shown that hydrogen was saturated for a much lower value of the E.M.F. than air, while the velocity of the hydrogen ion was much greater than that of air. The experiments on the amount of electrification with variation of E.M.F. and velocity of the blast confirm the previous results which were obtained in an entirely different way.

Velocity of the Ions.—An approximate determination of the velocity of the conducting particles for air can be made by determining the rate of leak of a charged wire connected to an electrometer when air is blown at varying velocities from a tube of known diameter along the charged wire. If we assume the current of air of high velocity from the tube to be confined within narrow limits for a short distance from the orifice, the velocity of the ions in order that a known proportion of the ions should reach the electrode can easily be deduced. The velocity of the blast issuing from a tube ·8 cm. in diameter was 800 cm. per second, and the wire was charged to a potential of 35 volts. With this velocity the rate of leak of the wire was only one third of the natural leak; so that two thirds of the conducting particles of one sign were blown out. The length of the exposed wire B D (fig. 1) was 6·3 cm., and knowing the diameter of the wire B D and of the cylinder, it can be shown that the velocity of the conducting particles for air is about 1 cm. per second for a potential gradient of one volt. This is of the same order as the rough determination made in the previous paper by Professor Thomson and myself.

The positive and negative conducting particles of air travel with the same velocity, for when the sign of the charged wire is reversed the rate of leak is the same as before with the same velocity of the blast. When different amounts of positive and negative electrification were obtained, it was at first thought that part of the difference might be due to inequality in the velocity of the ions, but later experiments showed that it was entirely due to the greater facility with which metals discharged the negative electrification.

Volume-Density of Electrification of the Charged Gas.

Only a very minute portion of the gas conveys the charge in the cases we have been considering. In the paper previously referred to it has been shown that assuming the conducting particles convey an atomic charge, only about one billionth of the gas is required to be split up to give the conductivity observed. In the previous experiments the conducting air is still further diluted by the blast along the electrode which conveys the charged particles with it. From data of the capacity of the electrometer and velocity of the blast it can be shown that the amount of charge per c.c.

of air was about 10^{-4} electrostatic units. In the case of the better conducting gases and vapours the volume-density is greater. For the vapour of methyl iodide the volume-density would be over 20 times as great.

The facility with which the electrified gas is discharged by metals and insulators may at first sight lead to the conclusion that we are dealing with electrified dust, which, as is well known, is completely discharged by glass wool, and also readily gives up its charge to whatever it comes in contact with. It has been shown, however, that the amount of electrification is quite independent of the amount of dust in the air, and that therefore the electrification can be in no way due to electrified dust.

The theory has been advanced that the discharge of electricity from the surface of a metal under the action of ultra-violet light is due to the disintegration of metallic particles or vapour from the surface, and that these carry off the charge. The discharge of electrification by the Röntgen rays might possibly be due to a similar cause, and this was fully investigated. In the first place, there are many experiments which negative this view. It has been shown in a previous paper that the current through a gas conducting under the x-rays increases with the distance between the electrodes although the surface exposed to the gas is unaltered. The amount of electrification obtained from a gas was found to be quite independent of the nature of the electrodes. The inside electrode (fig. 1) of the cylinder was coated with paraffin or wax, and provided we do not allow the charge to collect on the surface of the insulator, the amount of electrification was unaltered. Similarly, if the inside of the cylinder was coated with an insulator, no difference in amount could be detected immediately after the rays were turned on; but after the rays had been acting for a short time the amount decreased, owing to the charging of the surfaces of the insulators. These conclusions show that the conductivity in a gas is independent of the nature of the surface of the electrodes, for it is extremely improbable that the same amount of dust would be dislodged by the rays from the surface of all metals and insulators.

The most conclusive experiments on this subject are some which I recently made on the diminution of the intensity of the Röntgen rays due to the absorption in their passage through gases and vapours.

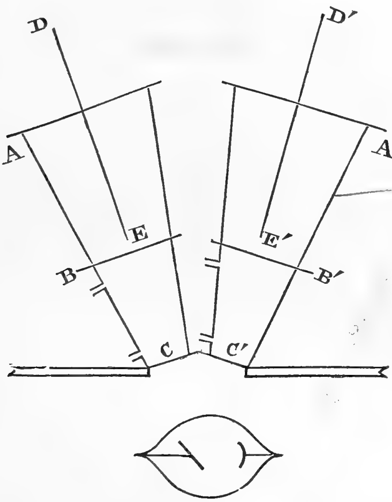
Absorption of Energy by Gases and Vapours.

Since gases all conduct under the influence of the x-rays, it was interesting to investigate the relative absorption in order to make the gases conductors, and whether the absorption was in any way related to the constitution or conductivity of the gas.

The absorption of energy in gases like air, hydrogen, oxygen is small and is not easily detected unless a delicate null method is used.

Two equal and similar conical-shaped vessels A B C, A′B′C′ (fig. 2), much larger in diameter at the top than the bottom, were placed in such positions that the axis of each cone passed as nearly as possible through the anode of the focus-bulb. From experiments it was found that the *x*-rays appeared to emanate in all directions from the anode. The upper parts of each vessel, A B, A′B′, were made of lead, and were separated from the lower portions B C, B′C′, which were made of glass, by thin ebonite plates. Thin ebonite plates also covered the ends of the glass cylinders at C and C′, so that the vessels B C, B′C′ were air-tight, and could be exhausted when required.

Fig. 2.



The lead cylinders A B, A′B′ were used to compare the rate of leak after the rays had passed through the glass cylinders. Insulated wires D E, D′E′ formed the electrodes, and these were connected to opposite pairs of quadrants of the electrometer, and both quadrants were at first charged up to the same potential, which in practice was generally 200 volts. The outsides of the vessels A B, A′B′ were connected to earth.

The position of the bulb was so adjusted that the rate of leak in each cylinder was exactly the same, so that since the potential of each pair of quadrants fell at the same rate, the needle of the electrometer remained at rest while the rays were kept acting. If another gas was introduced into one of the glass vessels BC it was found that the balance was disturbed, owing to the variation of intensity of the rays in the vessel AB, which was caused by the less or greater absorption of the rays in their passage through the gas. In the experiments the only rays which caused conduction in the lead cylinders had to pass through the gas, and all stray radiation was carefully screened off.

If we assume that the absorption of energy in passing through a thin layer of gas of thickness dl is proportional to the intensity of the rays I at that point and to the length of the gas dl traversed, the decrease of intensity of the rays due to *absorption of energy* in the gas is equal to $\lambda I dl$, where λ is a constant for any particular gas but varies for different gases, and may be called the coefficient of absorption of the gas.

Experimentally it was found that the *rate of leak* of a gas is proportional to the *intensity* of radiation at any point.

From these considerations it can readily be shown that the ratio of the rate of leak when the rays pass through a length l of the gas, to the rate of leak when the gas is removed and a vacuum substituted is $e^{-\lambda l}$ where $e=2{\cdot}7$, and this result is independent of any metal or insulators which the rays pass through in both cases before reaching the testing vessels.

The ratio of the rates of leak can be readily deduced from the movement of the electrometer needle, and since the length of the gas traversed is known, the coefficient λ is thus determined.

Experiments were first made to see whether air absorbed any appreciable amount of energy of the radiation. The balance was obtained and then one of the glass vessels was exhausted by an air-pump; the electrometer slowly moved in one direction while the rays were kept acting. If the other vessel was also exhausted, the balance was again restored, and if air was then let into the vessel first exhausted, the electrometer needle moved in the opposite direction. The variation of the rate of leak after passing through 10 cm. of air was about one per cent., but it was a difficult matter to determine such a small variation with accuracy. It will be at once seen that the approximate value of λl is 10^{-2}, and therefore for air $\lambda = 10^{-3}$, since $l=10$ cm.

If we suppose x-radiation to be emitted by the sun, assuming the radiation would have to pass through four miles of homogeneous atmosphere, the intensity at the surface of the earth would be approximately 10^{-260} of the intensity of the radiation before it reached the earth's atmosphere. This is an excessively minute proportion, and it is not surprising therefore that experiments, made even on the highest mountains, to detect any Röntgen rays in solar radiation (Cajori, Phil. Mag. Nov. 1896) should have been unsuccessful, even if the intensity of the x-radiation at the limits of our atmosphere were greater than could be produced at the surface of a Crookes tube.

Gases like oxygen, coal-gas, carbon dioxide, whose leakage-rates are about the same as that of air, absorb about the same amount of energy.

Sulphuretted hydrogen, which has a conductivity six times as great as air, diminishes the intensity of the radiation by about 4 per cent. in passing through 10 cm. of the gas. Chlorine, whose conductivity is eighteen times that of air, diminishes the intensity about 12 per cent. for the same distance.

The absorption of energy in these cases is not necessarily selective, for the same results were obtained whatever gas was used in the testing vessels. After the radiation had passed through sulphuretted hydrogen the same diminution in intensity was obtained whether air or sulphuretted hydrogen was used in the testing vessels.

Mercury vapour, which is one of the best conductors of electricity under the x-rays, is also one of the best absorbers of the radiation.

In the case of mercury it was not necessary to use a null method. A glass bulb 7 cm. in diameter was taken and a small amount of mercury introduced. The bulb was slowly heated till it was filled with boiling mercury vapour, care being taken that no mercury was allowed to condense on the sides of the glass through which the rays passed. The rate of leak was then taken in a conducting vessel for the radiation which had passed through the mercury vapour. The mercury was then removed and the bulb now filled with air was heated to the same temperature. The rate of leak in the testing vessel with air in the bulb was found to be twice as great as when the bulb wasfilled with mercury vapour at the temperature of boiling mercury.

In the passage therefore through 7 cm. of mercury vapour sufficient energy is absorbed to reduce the intensity of radiation by one half.

The vapour of methyl iodide, which is even a better conductor than mercury vapour, is also a powerful absorber of the radiation.

When the temperature of the vapour was raised above the boiling-point of the liquid, the intensity of the radiation after passing through 13 cm. of the vapour was only ·4 of the intensity when the vapour was removed.

From experiments on the absorption of energy of different lengths of the vapour of methyl iodide, and also of the gases sulphuretted hydrogen and chlorine, the ratio of the intensity of radiation after passage through the vapour or gas to the intensity of the radiation when the gas was removed was found to be in agreement with the theoretical ratio $e^{-\lambda l}$. For short lengths of the gas the absorption is proportional to the length.

It has been shown that for the vapour of methyl iodide $e^{-\lambda l}=\cdot 4$ when $l=13$ cm.; therefore $\lambda=\cdot 07$. The intensity of the radiation after passing through a length of 1 metre of the vapour is only one thousandth part of its value when the vapour is removed.

The absorption of energy varies with the pressure of the gas. The vapour of methyl iodide was used, and it was found that the values of λ were roughly proportional to the pressures down to a pressure of one quarter of an atmosphere. Results of this kind, however, are difficult to determine with accuracy on account of the variation of the Crookes tube during a series of observations.

The following table gives the values of λ and relative conductivities of some of the gases :—

Gas.	λ.	Conductivities.
Hydrogen	(small)	·5
Air	·001	1
Oxygen	about ·001	1·2
Nitrogen		·9
Coal-gas		·8
Carbon dioxide		1·2
Sulphur dioxide	·0025	4
Sulphuretted hydrogen	·0037	6
Hydrochloric acid	·0065	11
Chlorine	·0095	18

These experiments show that good conductors under the *x*-rays are good absorbers of the radiation. The absorbing powers for the gases examined had the same relative order as their conductivities. The absorption does not seem to depend to any great extent on the molecular weight of the gas. Hydrochloric acid is nearly twice as good an absorber as sulphuretted hydrogen, although their densities are nearly equal; while it is more than ten times as good an absorber as carbon dioxide, a gas of greater density. It is interesting to observe that vapours like mercury and methyl iodide which allow light to pass through freely are very opaque to Röntgen radiation.

Since the absorption of energy of the radiation varies with the length of the gas traversed and with the conductivity of the gas, it is very strong evidence that the discharge of electrification by the Röntgen rays is due to a process going on throughout the volume of the gas, and is not due to the disintegration of charged dust from the electrodes.

Cavendish Laboratory, December 28, 1896.

Note on the preceding Paper, by J. J. THOMSON.

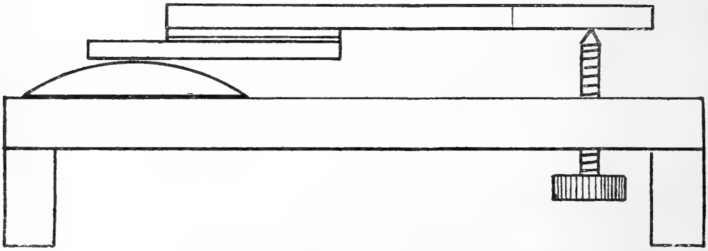
The connexion obtained by Mr. Rutherford between the coefficient of absorption and the saturation current through the gas admits of an interesting method of expression on the theory that the Röntgen rays so far resemble light as to be of the nature of an electromagnetic wave or impulse. We may regard such a wave or impulse as consisting of groups or a group of Faraday tubes travelling outwards through space. These tubes are of equal strength, the strength of each corresponding to the atomic charge carried by a univalent atom. If a molecule of the gas through which the rays are passing gets dissociated into ions by the electric field produced by the tubes, one and only one of the tubes will get detached from the group and will be anchored by having its ends attached to the ions into which the molecule is dissociated. The dissociation of one molecule, or the production of one positive and one negative ion, will withdraw just one tube from those in the group forming the Röntgen rays. Now Mr. Rutherford's result, if we can extend it to all gases, shows that the production of each ion corresponds to a weakening of the Röntgen rays (by the same amount) whatever may be the gas from which the ion is formed. The intensity of the rays is supposed to be measured by the conductivity they produce in a standard gas at standard temperature and pressure. Thus Mr. Rutherford's result may be expressed by saying that the weakening of the rays is proportional to the number of Faraday tubes stopped ; and hence that the intensity of the rays is proportional to the number of Faraday tubes.

XXXVI. *The Tangent Lens-Gauge.*

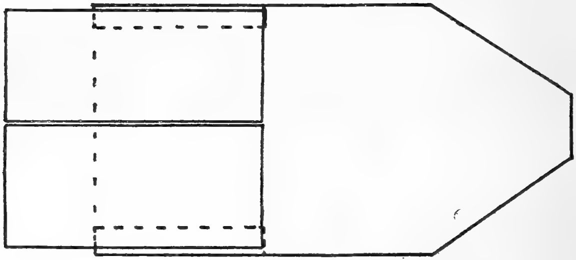
By GEORGE J. BURCH, *M.A.*[*]

THIS simple piece of apparatus, which for want of a better name may be called the Tangent Lens-gauge, is intended to afford an inexpensive means of measuring the radius of curvature of convex lenses.

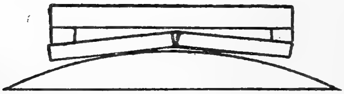
It consists of two pieces of stout plate-glass about 4 cms. wide, cemented with marine glue on to a third and larger piece, as in the annexed figures.



Side Elevation.



Plan.



End Elevation.

A strip of ordinary window glass is placed under the outer edge of each of the square pieces, so that they are inclined at a small angle to each other, and they both project equally beyond the end of the third piece. The lens to be

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[*] Communicated by the Author.

measured is placed on a board raised a couple of inches from the table, and the inclined plates are allowed to rest upon its curved surface, the other end of the instrument being supported on the point of an adjustable brass screw which passes through the board from beneath.

On examining the arrangement by reflected light, Newton's rings can be seen at the two points where the plane surfaces touch the sphere. Since the angle between the planes is constant, the distance between the two points of contact is directly proportional to the radius of the curved surfaces, being in fact the chord of the angle which they subtend at the centre of the sphere. The distance between the centres of the two systems of Newton's rings is measured with a micrometer microscope furnished with a vernier. Such instruments, reading to $\frac{1}{20}$ mm., can now be obtained at a moderate cost, and are required for several purposes in a course on practical physics. They should, however, be furnished with a jointed arm so that the tube may be inclined from the vertical—this I effect by unscrewing the microscope body from the arm, and screwing in its place a short wooden plug to the side of which I attach, with a small bolt and nut, a brass plate carrying a split tube to hold the microscope, which can by this means be fixed at any angle.

Writing l for the distance between the two systems of Newton's rings, and θ for the angle between the normals to the plane surfaces of the lens-gauge, the radius of curvature of the sphere is given by the formula

$$R = l \div \text{chord } \theta,$$

and since θ is constant the calculation is extremely simple, when once the value of chord θ has been determined. There are three methods by which this may be conveniently effected:—

(1) The tangent gauge may be applied to a sphere of which the diameter has been accurately measured with the spherometer.

(2) The angle between the normals to the plates may be determined with the goniometer, and the ratio found from a table of chords.

(3) A modification of the method sometimes used for finding the index of refraction of glass may be employed. The lens-gauge is supported vertically at one end of a drawing-board, and a pin or needle P_1 fixed upright in the board a few centimetres from it. Two other pins P_2 and P_3 are then arranged at equal distances from the first, so that the reflexion

of each in the plane opposite is exactly in a line with it and the first pin.

In the triangle $P_1 P_2 P_3$,

$$P_1 P_2 = P_1 P_3 \text{ by the construction,}$$

$$\text{and } \angle P_2 P_1 P_3 = \theta,$$

$$\text{so that chord } \theta = \frac{P_2 P_3}{P_1 P_2}.$$

It is best to place the eye at some distance from the pins, which should be as fine as possible, and strongly illuminated. A large drawing-board should be used.

Using this method, which is obviously the least accurate, and measuring the distance between the points of contact with a cheap micrometer microscope reading to $\frac{1}{20}$ mm., I find it easy to get results within one per cent. of the truth. It is important, especially in working with artificial light, to focus the microscope accurately upon the actual surface of the lens, as the Newton's rings are visible without any striking alteration of appearance some distance beyond the focus, and under these circumstances a large collimation error is introduced. A small correction, constant for each instrument, must be made for refraction. This is found by subtracting the actual length of a short rod as measured in air from its apparent length as seen through the glass plates. In the instrument I have made it amounts to $\frac{1}{20}$ mm.

My object in designing the tangent lens-gauge was to provide a simple and cheap form of apparatus by which students could measure the curvature of a lens in order to determine its index of refraction. The instrument lends itself readily to teaching purposes. It affords an opportunity of using the goniometer, and familiarizes the student with Newton's rings. It illustrates a geometrical principle, and while by the third method it is rendered independent of the spherometer and of the goniometer, the results so obtained can easily be checked by the more accurate methods. It has a greater range than the spherometer, and can be used for lenses of from 2 cms. to 2 metres focal length, the percentage of error being smaller in the latter case than in the former, owing to the greater distance between the points of contact. It is, in fact, considerably more accurate for such lenses than the majority of cheap spherometers.

With lenses of short focus the Newton's rings are so small that students sometimes have a difficulty in finding them. In such cases it is best to search for them with a hand lens while applying gentle pressure to the gauge. With long focus lenses, especially if the polish is impaired, the central spot may be somewhat distorted, but its centre can be found pretty accurately by taking the semi-diameter of the outer rings. The angle between the planes may conveniently be fixed so that chord $\theta = 0{\cdot}1$ or thereabouts. This will serve for the majority of lenses, but the apparatus is so simple that several gauges of different angles may be kept ready.

I have not found any inconvenience from the use of marine glue to fasten the plates together, any variation of the angle that may result from changes of temperature being too small to affect the result appreciably.

21, Norham Road, Oxford,
January 1897.

XXXVII. *On the Passage of Waves through Apertures in Plane Screens, and Allied Problems.* *By* Lord Rayleigh, *F.R.S.*[*]

THE waves contemplated may be either aerial waves of condensation and rarefaction, or electrical waves propagated in a dielectric. Plane waves of simple type impinge upon a parallel screen. The screen is supposed to be infinitely thin, and to be perforated by some kind of aperture. Ultimately one or both dimensions of the aperture will be regarded as infinitely small in comparison with the wave-length (λ); and the method of investigation consists in adapting to the present purpose known solutions regarding the flow of incompressible fluid.

If ϕ be a velocity-potential satisfying

$$d^2\phi/dt^2 = \mathrm{V}^2 \nabla^2 \phi, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where

$$\nabla^2 = d^2/dx^2 + d^2/dy^2 + d^2/dz^2,$$

the condition at the boundary may be (i.) that $d\phi/dn = 0$, or (ii.) that $\phi = 0$. The first applies directly to aerial vibrations impinging upon a fixed wall, and in this connexion has already been considered[†].

If we assume that the vibration is everywhere proportional to e^{int}, (1) becomes

$$(\nabla^2 + k^2)\phi = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where

$$k = n/\mathrm{V} = 2\pi/\lambda. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

It will conduce to brevity if we suppress the factor e^{int}.

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[*] Communicated by the Author.

[†] 'Theory of Sound,' § 292.

On this understanding the equation of waves travelling parallel to x in the positive direction, and accordingly incident upon the negative side of a screen situated at $x=0$, is

$$\phi=e^{-ikx}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

When the solution is complete, the factor e^{int} is to be restored, and the imaginary part of the solution is to be rejected. The realized expression for the incident waves will therefore be

$$\phi=\cos(nt-kx). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Perforated Screen.—Boundary Condition $d\phi/dn=0$.

If the screen be complete, the reflected waves under the above condition have the expression $\phi=e^{ikx}$.

Let us divide the actual solution into two parts χ and ψ, the first the solution which would obtain were the screen complete, the second the alteration required to take account of the aperture; and let us distinguish by the suffixes m and p the values applicable upon the negative (*minus*) and upon the positive side of the screen. In the present case we have

$$\chi_m=e^{-ikx}+e^{ikx}, \qquad \chi_p=0. \quad . \quad . \quad . \quad . \quad . \quad (6)$$

This χ-solution makes $d\chi_m/dn=0$, $d\chi_p/dn=0$ over the whole plane $x=0$, and over the same plane $\chi_m=2$, $\chi_p=0$.

For the supplementary solution, distinguished in like manner upon the two sides, we have

$$\psi_m=\iint\Psi_m\frac{e^{-ikr}}{r}dS, \qquad \psi_p=\iint\Psi_p\frac{e^{-ikr}}{r}dS, \quad . \quad . \quad (7)$$

where r denotes the distance of the point at which ψ is to be estimated from the element dS of the aperture, and the integration is extended over the whole of the area of aperture. Whatever functions of position Ψ_m, Ψ_p may be, these values on the two sides satisfy (2), and (as is evident from symmetry) they make $d\psi_m/dn$, $d\psi_p/dn$ vanish over the wall, viz. the unperforated part of the screen; so that the required condition over the wall for the complete solution $(\chi+\psi)$ is already satisfied. It remains to consider the further conditions that ϕ and $d\phi/dx$ shall be continuous across the *aperture*.

These conditions require that on the aperture

$$2+\psi_m=\psi_p, \qquad d\psi_m/dx=d\psi_p/dx. \quad . \quad . \quad . \quad (8)^*$$

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* The use of dx implies that the variation is in a fixed direction, while dn may be supposed to be drawn outwards from the screen in both cases.

The second is satisfied if $\Psi = -\Psi_m$; so that

$$\psi_m = \iint \Psi_m \frac{e^{-ikr}}{r}\, dS, \qquad \psi_p = -\iint \Psi_m \frac{e^{-ikr}}{r}\, dS, \quad . \quad (9)$$

making the values of ψ_m and ψ_p equal and opposite at all corresponding points, viz. points which are images of one another in the plane $x=0$. In order further to satisfy the first condition it suffices that over the area of aperture

$$\psi_m = -1, \qquad \psi_p = 1, \quad . \quad . \quad . \quad . \quad (10)$$

and the remainder of the problem consists in so determining Ψ_m that this shall be the case.

In this part of the problem we limit ourselves to the supposition that all the dimensions of the aperture are small in comparison with λ. For points at a distance from the aperture e^{-ikr}/r may then be removed from under the sign of integration, so that (9) becomes

$$\psi = \frac{e^{-ikr}}{r} \iint \Psi_m dS, \qquad \psi_p = -\frac{e^{-ikr}}{r} \iint \Psi_m dS. \quad . \quad (11)$$

The significance of $\iint \Psi_m dS$ is readily understood from an electrical interpretation. For in its application to a point, itself situated upon the area of aperture, e^{-ikr} in (9) may be identified with unity, so that ψ_m is the potential of a distribution of density Ψ_m on S. But by (10) this potential must have the constant value -1; so that $-\iint \Psi_m dS$, or $\iint \Psi_p dS$, represents the electrical *capacity* of a conducting disk having the size and shape of the aperture, and situated at a distance from all other electrified bodies. If we denote this by M, the solution applicable to points at a distance from the aperture may be written

$$\psi_m = -\mathrm{M}\frac{e^{-ikr}}{r}, \qquad \psi_p = \mathrm{M}\frac{e^{-ikr}}{r}. \quad . \quad . \quad (12)$$

To these are to be added the values of χ in (6). The realized solutions are accordingly

$$\phi_m = 2\cos nt \cos kx - \mathrm{M}\frac{\cos(nt-kr)}{r}, \quad . \quad . \quad . \quad (13)$$

$$\phi_p = \mathrm{M}\frac{\cos(nt-kr)}{r}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

The value of M may be expressed* for an ellipse of semi-

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* 'Theory of Sound,' §§ 292, 306, where is given a discussion of the effect of ellipticity when area is given.

major axis a and eccentricity e. We have

$$M=\frac{a}{F(e)}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

F being the symbol of the complete elliptic function of the first kind. When $e=0$, $F(e)=\frac{1}{2}\pi$; so that for a circle $M=2a/\pi$.

It should be remarked that Ψ in (9) is closely connected with the normal velocity at dS. In general,

$$\frac{d\psi}{dx}=\iint\Psi\frac{d}{dx}\left(\frac{e^{-ikr}}{r}\right)dS. \quad . \quad . \quad . \quad . \quad (16)$$

At a point (x) infinitely close to the surface, the neighbouring elements only contribute to the integral, and the factor e^{-ikr} may be omitted. Thus

$$\frac{d\psi}{dx}=-\iint\Psi\frac{x}{r^3}dS=-2\pi x\int_x^\infty\Psi\frac{rdr}{r^3}=-2\pi\Psi\ ;$$

or

$$\Psi=-\frac{1}{2\pi}\frac{d\psi}{dn}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

$d\psi/dn$ being the normal velocity at the point of the surface in question.

Boundary Condition $\phi=0$.

We will now suppose that the condition to be satisfied on the walls is $\phi=0$, although this case has no simple application to aerial vibrations. Using a similar notation to that previously employed, we have as the expression for the principal solution

$$\chi_m=e^{-ikx}-e^{ikx}, \qquad \chi_p=0, \quad . \quad . \quad . \quad (18)$$

giving over the whole plane $(x=0)$, $\chi_m=0$, $\chi_p=0$, $d\chi_m/dx=-2ik$, $d\chi_p/dx=0$.

The supplementary solutions now take the form

$$\psi_m=\iint\frac{d}{dx}\left(\frac{e^{-ikr}}{r}\right)\Psi_m\,dS, \quad \psi_p=\iint\frac{d}{dx}\left(\frac{e^{-ikr}}{r}\right)\Psi_p\,dS. \quad (19)$$

These give on the *walls* $\psi_m=\psi_p=0$, and so do not disturb the condition of evanescence already satisfied by χ. It remains to satisfy over the *aperture*

$$\psi_m=\psi_p, \quad -2ik+d\psi_m/dx=d\psi_p/dx. \quad . \quad . \quad (20)$$

The first of these is satisfied if $\Psi_m=-\Psi_p$, so that ψ_m and ψ_p are equal at any pair of corresponding points upon the two sides. The values of $d\psi_m/dx$, $d\psi_p/dx$ are then opposite, and the remaining condition is also satisfied if

$$d\psi_m/dx = ik, \qquad d\psi_p/dx = -ik. \quad . \quad . \quad . \quad (21)$$

Thus Ψ_m is to be such as to make $d\psi_m/dx = ik$; and, as in the proof of (17), it is easy to show that in (19)

$$\Psi_m = \psi_m/2\pi, \qquad \Psi_p = -\psi_p/2\pi, \quad . \quad . \quad . \quad (22)$$

where ψ_m, ψ_p are the (equal) surface-values at dS.

When all the dimensions of S are small in comparison with the wave-length, (19) in its application to points at a sufficient distance from S assumes the form

$$\psi_p = \frac{ikx\, e^{-ikr}}{2\pi r^2} \iint \psi_p\, dS, \quad . \quad . \quad . \quad . \quad (23)$$

and it only remains to find what is the value of $\iint \psi_p\, dS$ which corresponds to $d\psi_p/dx = -ik$.

Now this correspondence is ultimately the same as if we were dealing with an absolutely incompressible fluid. If we imagine a rigid and infinitely thin plate (having the form of the aperture) to move normally through unlimited fluid with velocity u, the condition is satisfied that over the remainder of the plane the velocity-potential ψ vanishes. In this case the values of ψ at corresponding points upon the two sides are opposite; but if we limit our attention to the positive side, the conditions are the same as in the present problem. The kinetic energy of the motion is proportional to u^2, and we will suppose that twice the energy upon one side is hu^2. By Green's theorem this is equal to $-\iint \psi \,.\, d\psi/dn \,.\, dS$, or $-u \iint \psi dS$; so that $\iint \psi dS = -hu$. In the present application $u = -ik$, so that the corresponding value of $\iint \psi_p dS$ is ihk. Thus (23) becomes

$$\psi_p = -\frac{hk^2 x\, e^{-ikr}}{2\pi r^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

The same algebraic expression gives ψ_m, if the *minus* sign be omitted; for as x itself changes sign in passing from one side to the other, the values of ψ_m and ψ_p at corresponding points are then equal.

The value of h can be determined in certain cases. For a circle* of radius c

$$h = \frac{4c^3}{3}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (26)$$

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* Lamb's 'Hydrodynamics,' § 105.

so that for a circular aperture the realized solution is

$$\phi_p = -\frac{8\pi c^3}{3\lambda^2}\frac{x}{r^2}\cos(nt-kr), \quad . \quad . \quad . \quad . \quad (27)$$

$$\phi_m = 2\sin nt \sin kx + \frac{8\pi c^3}{3\lambda^2}\frac{x}{r^2}\cos(nt-kr). \quad . \quad . \quad . \quad (28)$$

It will be remarked that while in the first problem the wave (ψ) divergent from the aperture is proportional to the first power of the linear dimension, in the present case the amplitude is very much less, being proportional to the cube of that quantity.

The solution for an elliptic aperture is deducible from the general theory of the motion of an ellipsoid (a, b, c) through incompressible fluid*, by supposing $a=0$, while b and c remain finite and unequal; but the general expression does not appear to have been worked out. When the eccentricity of the residual ellipse is small, I find that

$$h = \tfrac{4}{3}(bc)^{\frac{3}{2}}\left(1-\tfrac{3}{64}e^4\right), \quad . \quad . \quad . \quad . \quad . \quad (29)$$

showing that the effect of moderate ellipticity is very small when the *area* is given.

From the solutions already obtained it is possible to derive others by differentiation. If, for example, we take the value of ϕ in the first problem and differentiate it with respect to x, we obtain a function which satisfies (2), which includes plane waves and their reflexion on the negative side, and which satisfies over the wall the condition of evanescence. It would seem at first sight as if this could be no other than the solution of the second problem, but the manner in which the linear dimension of the aperture enters suffices to show that it is not so. The fact is that although the proposed function vanishes over the plane part of the wall, it becomes infinite at the *edge*, and thus includes the action of *sources* there distributed. A similar remark applies to the solutions that might be obtained by differentiation of the second solution with respect to y or z, the coordinates measured parallel to the plane of the screen.

Reflecting Plate.—$d\phi/dn=0$.

We now pass to the consideration of allied problems in which the transparent and opaque parts of the screen are interchanged. Under the above-written boundary condition

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* Lamb's 'Hydrodynamics,' § 111.

the case is that of plane aerial waves incident upon a parallel infinitely thin plate, whose dimensions are ultimately supposed to be small in comparison with λ. The analytical process of solution may be illustrated by the following argument. Suppose a motion communicated to the plate identical with that which the air at that place would execute were the plate absent. It is evident that the propagation of the primary wave will then be undisturbed. The supplementary solution, representing the disturbance due to the plate, must then correspond to the reduction of the plate to rest, that is to a motion of the plate equal and opposite to that just imagined. The supplementary solution is accordingly analogous to that which occurs in the *second* of the problems already treated.

Using a similar notation, we have for the principal solution upon the two sides

$$\chi_m = \chi_p = e^{-ikx}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (30)$$

giving when $x=0$

$$\chi_m = \chi_p = 1, \qquad d\chi_m/dx = d\chi_p/dx = -ik.$$

The supplementary solution is of the form (19), and gives upon the aperture, viz. the part of the plane $x=0$ unoccupied by the plate, $\psi_m = \psi_p = 0$, and so does not disturb the continuity of ϕ. But in order that the continuity of $d\phi/dx$ may be maintained it is necessary that $\Psi_p = \Psi_m$; and then the values of ψ_m and ψ_p are *opposite* at any pair of corresponding points upon the two sides.

It remains to satisfy the necessary conditions at the plate itself. These are

$$\frac{d\chi_m}{dx} + \frac{d\psi_m}{dx} = 0, \qquad \frac{d\chi_p}{dx} + \frac{d\psi_p}{dx} = 0 ;$$

or, since $d\psi_m/dx$, $d\psi_p/dx$ are equal,

$$d\psi_m/dx = d\psi_p/dx = ik. \quad . \quad . \quad . \quad . \quad . \quad . \quad (31)$$

It follows that ψ_p has the opposite value to that expressed in (25); and the realized solution for a circular plate of radius c becomes

$$\phi_p = \cos(nt-kx) + \frac{8\pi c^3}{3\lambda^2}\frac{x}{r^2}\cos(nt-kr), \quad . \quad . \quad . \quad . \quad (32)$$

$$\phi_m = \cos(nt-kx) + \frac{8\pi c^3}{3\lambda^2}\frac{x}{r^2}\cos(nt-kr), \quad . \quad . \quad . \quad . \quad (33)$$

the analytical form being the same in the two cases.

It is important to notice that the reflexion from the plate is utterly different from the transmission by a corresponding aperture in an opaque screen, as given in (14), the former varying as the cube of the linear dimension, and the latter as the first power simply.

Reflecting Plate.—$\phi=0$.

For the sake of completeness it may be well to indicate the solution of a fourth problem defined by the above heading. This has an affinity with the *first* problem, analogous to that of the third with the second. The form of χ is the same as in (30), and those for ψ_m, ψ_p the same as in (7). These make $d\psi_m/dx$, $d\psi_p/dx$ vanish on the aperture, and so do not disturb the continuity of $d\phi/dx$. But in order that the continuity of ϕ may also be maintained, we must have $\Psi_m=\Psi_p$, and not as in (9) $\Psi_m=-\Psi_p$. On the plate itself we must have

$$\psi_m=\psi_p=-1.$$

Accordingly ψ_m is the same as in (12), while ψ_p in (12) must have its sign reversed. The realized solution is

$$\phi_p=\phi_m=\cos(nt-kx)-\mathrm{M}\frac{\cos(nt-kr)}{r}. \quad . \quad (34)$$

Two-dimensional Vibrations.

In the class of problems before us the velocity-potential of a point-source, viz. e^{-ikr}/r, is replaced by that of a linear source; and this in general is much more complicated. If we denote it by $\mathrm{D}(kr)$, the expressions are *

$$\mathrm{D}(kr)=-\left(\frac{\pi}{2ikr}\right)^{\frac{1}{2}}e^{-ikr}\left\{1-\frac{1^2}{1.8ikr}+\frac{1^2.3^2}{1.2.(8ikr)^2}-\ .\ .\ .\right\}$$

$$=\left(\gamma+\log\frac{ikr}{2}\right)\left\{1-\frac{k^2r^2}{2^2}+\frac{k^4r^4}{2^2.4^2}-\ .\ .\ .\right\}$$

$$+\frac{k^2r^2}{2^2}\mathrm{S}_1-\frac{k^4r^4}{2^2.4^2}\mathrm{S}_2+\frac{k^6r^6}{2^2.4^2.6^2}\mathrm{S}_3-\ .\ .\ .\ , \quad . \quad (35)$$

where γ is Euler's constant (·5772 . . .), and

$$\mathrm{S}_m=1+\tfrac{1}{2}+\tfrac{1}{3}+\ .\ .\ .\ +1/m.$$

Of these the first is "semiconvergent," and is applicable when kr is large; the second is fully convergent and gives the form of the function when kr is small.

Since the complete analytical theory is rather complicated, it may be convenient to give a comparatively simple deriva-

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* See for example 'Theory of Sound,' § 341.

tion of the extreme forms, which includes all that is required for our present purpose, starting from the conception of a linear source as composed of distributed point-sources. If ρ be the distance of any element dx of the linear source from O, the point at which the potential is to be estimated, and r be the smallest value of ρ, so that $\rho^2=r^2+x^2$, we may take as the potential, constant factors being omitted,

$$\psi=-\int_0^\infty \frac{e^{-ik\rho}\,dx}{\rho}=-\int_r^\infty \frac{e^{-ik\rho}\,d\rho}{\sqrt{(\rho^2-r^2)}}. \quad . \quad (36)$$

We have now to trace the form of (36) when kr is very great, and also when kr is very small. For the former case we replace ρ by $r+y$, thus obtaining

$$\psi=-\int_0^\infty \frac{e^{-ikr}\,e^{-iky}\,dy}{\sqrt{y}\,.\,\sqrt{(2r+y)}}. \quad . \quad . \quad . \quad (37)$$

When kr is very great, the approximate value of the integral in (37) may be obtained by neglecting the variation of $\sqrt{(2r+y)}$, since on account of the rapid fluctuation of sign caused by the factor e^{-iky} we need attend only to small values of y. Now, as is known,

$$\int_0^\infty \frac{\cos x\,dx}{\sqrt{x}}=\int_0^\infty \frac{\sin x\,dx}{\sqrt{x}}=\sqrt{\left(\frac{\pi}{2}\right)},$$

so that in the limit

$$\psi=-(1-i)\sqrt{\left(\frac{\pi}{2kr}\right)}e^{-ikr}=-\sqrt{\left(\frac{\pi}{2ikr}\right)}e^{-ikr}, \quad (38)$$

in agreement with (35).

We have next to deduce the limiting form of (36) when kr is very small. For this purpose we may write it in the form

$$\psi=-\int_r^\infty \frac{e^{-ik\rho}\,d\rho}{\rho}-\int_r^\infty e^{-ik\rho}\left\{\frac{1}{\sqrt{(\rho^2-r^2)}}-\frac{1}{\rho}\right\}d\rho. \quad (39)$$

The first integral in (39) is well known. We have

$$-\int_r^\infty \frac{e^{-ik\rho}\,d\rho}{\rho}=\mathrm{Ci}(kr)-i\{\tfrac{1}{2}\pi+\mathrm{Si}(kr)\}$$

$$=\gamma+\log kr-\frac{k^2r^2}{2^2}+\frac{k^4r^4}{2\,.\,3\,.\,4^2}-\,.\,.\,.$$

$$+i\left\{\frac{\pi}{2}-kr+\frac{k^3r^3}{2\,.\,3^2}-\,.\,.\,.\right\}.$$

In the second integral of (39) the function to be integrated vanishes when ρ is great compared to r, and when ρ is not great in comparison with r, $k\rho$ is small and $e^{-ik\rho}$ may be identified with unity. Thus in the limit

$$\int_r^\infty e^{-ik\rho}\left\{\frac{1}{\sqrt{(\rho^2-r^2)}}-\frac{1}{\rho}\right\}d\rho=\left[\log\frac{\rho+\sqrt{(\rho^2-r^2)}}{\rho}\right]_r^\infty=\log 2;$$

and (39) becomes

$$\psi=\gamma+\log kr+\tfrac{1}{2}i\pi-\log 2=\gamma+\log(\tfrac{1}{2}ikr), \quad . \quad (40)$$

in agreement with (35).

When kr is extremely small (40) may be considered for some purposes to reduce to $\log kr$; but the term $\frac{1}{2}i\pi$ is required in order to represent the equality of work done in the neighbourhood of the linear source and at a great distance from it.

We may now proceed to solve four problems relative to narrow slits and reflecting blades analogous to the four already considered in which the aperture or the reflecting plate was small in *both* its dimensions in comparison with the wave-length.

Narrow Slit.—Boundary Condition $d\phi/dn=0$.

As in the former problem the principal solution is

$$\chi_m=e^{-ikx}+e^{ikx}, \qquad \chi_p=0, \quad . \quad . \quad . \quad . \quad (41)$$

making $d\chi_m/dn$, $d\chi_p/dn$ vanish over the whole plane $x=0$ and over the same plane $\chi_m=2$, $\chi_p=0$. The supplementary solution, which represents the effect of the slit, may be written

$$\psi_m=\int\Psi_m\mathrm{D}(kr)dy, \qquad \psi_p=\int\Psi_p\mathrm{D}(kr)dy, \quad . \quad (42)$$

Ψ_m, Ψ_p being certain functions of y to be determined, and the integration extending over the width of the slit from $y=-b$ to $y=+b$.

These additions do not disturb the condition to be satisfied over the wall. On the aperture continuity requires, as in (8), that

$$2+\psi_m=\psi_p, \qquad d\psi_m/dx=d\psi_p/dx.$$

The second of these is satisfied by taking $\Psi_p=-\Psi_m$, so that at all corresponding pairs of points $\psi_m=-\psi_p$. It remains to determine Ψ_m so that on the aperture $\psi_m=-1$; and then by what has been said $\psi_p=+1$.

At a sufficient distance from the slit, supposed to be very narrow, $\mathrm{D}(kr)$ may be removed from under the integral sign and also be replaced by its limiting form given in (35). Thus

$$\psi_m=-\left(\frac{\pi}{2ikr}\right)^{\frac{1}{2}}e^{-ikr}\int\Psi_m dy. \quad . \quad . \quad . \quad . \quad (43)$$

The condition by which Ψ_m is determined is that for all points upon the aperture

$$\int_{-b}^{+b} \Psi_m \mathrm{D}(kr)dy = -1, \quad . \quad . \quad . \quad . \quad (44)$$

where, since kr is small throughout, the second limiting form given in (35) may be introduced.

From the known solution for the flow of incompressible fluid through a slit in an infinite plane we may infer that Ψ_m will be of the form $\mathrm{A}(b^2-y^2)^{-\frac{1}{2}}$, where A is some constant. Thus (44) becomes

$$\mathrm{A}\left[(\gamma+\log \tfrac{1}{2}ik)\pi + \int_{-b}^{+b} \frac{\log (r)dy}{\sqrt{(b^2-y^2)}}\right] = -1. \quad . \quad (45)$$

In this equation the first integral is obviously independent of the position of the point chosen, and if the form of Ψ_m has been rightly taken the second integral must also be independent of it. If its coordinate be η, lying between $\pm b$,

$$\int_{-b}^{+b} \frac{\log r\,dy}{\sqrt{(b^2-y^2)}} = \int_{-b}^{\eta} \frac{\log (\eta-y)dy}{\sqrt{(b^2-y^2)}} + \int_{\eta}^{b} \frac{\log (y-\eta)dy}{\sqrt{(b^2-y^2)}},$$

and must be independent of η. This can be verified without much difficulty by assuming $\eta=b \sin \alpha$, $y=b \sin \theta$; but merely to determine A in (45) it suffices to consider the particular case of $\eta=0$. Here

$$\int_{-b}^{+b} \frac{\log r\,dy}{\sqrt{(b^2-y^2)}} = 2\int_{0}^{b} \frac{\log y\,dy}{\sqrt{(b^2-y^2)}}$$

$$= 2\int_{0}^{\frac{1}{2}\pi} \log (b \sin \theta)\, d\theta = \pi \log (\tfrac{1}{2}b).$$

Thus

$$\mathrm{A}(\gamma+\log \tfrac{1}{4}ikb)\pi = -1,$$

and

$$\int_{-}^{+b} \Psi_m\, dy = \mathrm{A}\int_{-b}^{+b} \frac{dy}{\sqrt{(b^2-y^2)}} = \pi \mathrm{A};$$

so that (43) becomes

$$\psi_m = \frac{e^{-ikr}}{\gamma+\log (\frac{1}{4}ikb)}\left(\frac{\pi}{2ikr}\right)^{\frac{1}{2}}. \quad . \quad . \quad (46)$$

From this ψ_p is derived by simply prefixing a negative sign.

The realized solution is obtained from (46) by omitting the imaginary part after introduction of the suppressed factor e^{int}. If the imaginary part of $\log (\frac{1}{4}ikb)$ be neglected, the result is

$$\psi_m = \left(\frac{\pi}{2kr}\right)^{\frac{1}{2}} \frac{\cos (nt-kr-\frac{1}{4}\pi)}{\gamma+\log (\frac{1}{4}kb)}, \quad . \quad (47)$$

corresponding to

$$\chi_m = 2\cos nt \cos kx. \quad . \quad . \quad . \quad . \quad . \quad . \quad (48)$$

The solution (47) applies directly to aerial vibrations incident upon a perforated wall, and to an electrical problem which will be specified later. Perhaps the most remarkable feature of it is the very limited dependence of the transmitted vibration on the *width* ($2b$) of the aperture.

Narrow Slit.—Boundary Condition $\phi=0$.

The principal solution is the same as in (18); and the conditions for the supplementary solution, to be satisfied over the aperture, are those expressed in (21). In place of (19)

$$\psi_m = -\int \frac{d\mathrm{D}}{dx}\Psi_p\,dy, \qquad \psi_p = \int \frac{d\mathrm{D}}{dx}\Psi_p\,dy; \quad . \quad (49)$$

the values of Ψ_m and Ψ_p being opposite, and those of ψ_m and ψ_p equal at corresponding points. At a distance we have

$$\psi_p = \frac{d\mathrm{D}}{dx}\int_{-b}^{+b}\Psi_p\,dy, \quad . \quad . \quad . \quad . \quad . \quad (50)$$

in which

$$\frac{d\mathrm{D}}{dx} = \frac{ikx}{r}\left(\frac{\pi}{2ikr}\right)^{\frac{1}{2}} e^{-ikr}. \quad . \quad . \quad . \quad . \quad (51)$$

There is a simple relation between the value of Ψ_p at any point of the aperture and that of ψ_p at the same point. For in the application of (49) to any point of the narrow aperture, $d\mathrm{D}/dx = x/r^2$, showing that only those elements of the integral are sensible which lie infinitely near the point where ψ_p is to be estimated. The evaluation is effected by considering in the first instance a point for which x is finite, and afterwards passing to the limit. Thus

$$\psi_p = \int \frac{x}{x^2+y^2}\Psi_p\,dy = \Psi_p\left[\tan^{-1}\frac{y}{x}\right]_{-b}^{+b} = \pi\Psi_p;$$

so that (50) becomes

$$\psi_p = \frac{1}{\pi}\frac{d\mathrm{D}}{dx}\int_{-b}^{+b}\psi_p\,dy. \quad . \quad . \quad . \quad . \quad (52)$$

It remains only to express the connexion between $\int\psi_p\,dy$ and the constant value of $d\psi_p/dx$ on the area of the aperture; and this is effected by the known solution for an incompressible fluid moving under similar conditions. The argument is the same as in the corresponding problem where the perforation is circular. In the motion (u) of a lamina of width ($2b$) through infinite fluid, the whole kinetic energy per unit of length may be denoted by hu^2, and it appears from Green's theorem that $\int \psi_p dy = ihk$. The value of h [*] is $\frac{1}{2}\pi b^2$; so that

$$\psi_p = -\frac{k^2b^2x}{2r}\left(\frac{\pi}{2ikr}\right)^{\frac{1}{2}} e^{-ikr}. \quad . \quad . \quad . \quad . \quad . \qquad (53)$$

The same algebraic expression gives ψ_m, if the *minus* sign be omitted.

The realized solution from (53) is

$$\psi_p = -\frac{k^2b^2x}{2r}\left(\frac{\pi}{2kr}\right)^{\frac{1}{2}} \cos\,(nt - kr - \tfrac{1}{4}\pi), \quad . \quad . \quad . \qquad (54)$$

corresponding to

$$\chi_m = 2 \sin nt \sin kx. \quad . \quad . \quad . \quad . \quad . \quad . \qquad (55)$$

Reflecting Blade.—Boundary Condition $d\phi/dn = 0$.

We have now to consider two problems which differ from the last in that the opaque and transparent parts of the screen are interchanged. As in the case of the circular aperture, we shall find that the correspondence lies between the reflecting blade under the condition $d\phi/dn = 0$ and the transmitting aperture under the condition $\phi = 0$, and reciprocally.

The principal solution remains as in (30). The supplementary solution must satisfy (31), where

$$\psi_m = \int \frac{d\mathrm{D}}{dx} \Psi_p dy, \qquad \psi_p = \int \frac{d\mathrm{D}}{dx} \Psi_p dy, \quad . \quad . \qquad (56)$$

since Ψ_m and Ψ_p must be equal in order that the continuity of $d\phi/dx$ over the aperture may be maintained. Thus ψ_m and ψ_p have opposite values at any pair of corresponding points.

If we compare these conditions with those by which (53) was determined, we see that ψ_m has the same value as in that case, but that the sign of ψ_p must be reversed. Thus in the present problem

$$\psi_m = \psi_p = \frac{k^2b^2x}{2r}\left(\frac{\pi}{2kr}\right)^{\frac{1}{2}} \cos\,(nt - kr - \tfrac{1}{4}\pi), \quad . \qquad (57)$$

corresponding to

$$\chi_m = \chi_p = \cos\,(nt - kx). \quad . \quad . \quad . \quad . \quad . \qquad (58)$$

Reflecting Blade.—Boundary Condition $\phi = 0$.

In this case χ still remains as in (30). The general forms for ψ_m, ψ_p are as in (42), which secure that $d\psi_m/dx$, $d\psi_p/dx$ shall vanish on the aperture (*i. e.* the part of the plane $x = 0$ unoccupied by the blade). But in order that the continuity of ϕ may also be maintained over that area we must have $\Psi_m = \Psi_p$. Thus ψ_m, ψ_p have equal values at corresponding points. On the blade itself $\psi_m = \psi_p = -1$.

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[*] Lamb's 'Hydrodynamics,' § 71.

A comparison of these conditions with those by which (46) was determined shows that in the present case

$$\psi_m = \psi_p = \frac{e^{-ikr}}{\gamma + \log\left(\tfrac{1}{4}ikb\right)} \left(\frac{\pi}{2ikr}\right)^{\frac{1}{2}}. \quad . \quad . \quad (59)$$

When $\log i$ in the denominator of (59) may be omitted, the realized form is that expressed by (47), and this corresponds to

$$\chi_m = \chi_p = \cos(nt - kx). \quad . \quad . \quad . \quad . \quad (60)$$

Various Applications.

Of the eight problems, whose solutions have now been given, four have an immediate application to aerial vibrations, viz. those in which the condition on the walls is $d\phi/dn = 0$. The symbol ϕ then denotes the velocity-potential, and the condition expresses simply that the fluid does not penetrate the boundary. The four problems relating to two dimensions have also a direct application to electrical vibrations, if we suppose that the thin material constituting the screen (or the blade) is a perfect conductor. For if R denote the electromotive intensity parallel to z, the condition at the face of the conductor is $\mathrm{R} = 0$; so that if R be written for ψ in (53), (59), we have the solutions for a narrow aperture in an infinite screen, and for a narrow reflecting blade respectively, corresponding to the incident wave $\mathrm{R} = e^{-ikx}$. A narrow aperture parallel to the electric vibrations transmits very much less than is reflected by a conductor elongated in the same direction.

The two other solutions relative to two dimensions find electrical application if we identify ϕ with c, the component of magnetic intensity parallel to z. For when the other components a and b are zero, the condition to be satisfied at the face of a conductor is $dc/dn = 0$. Thus (46), (57) apply to incident vibrations represented by $c = e^{-ikx}$. In this case the slit transmits much more than the blade reflects.

It may be remarked that in general problems of electrical vibration in *two dimensions* have simple acoustical analogues*. As an example we may refer to the reflexion of plane electric waves incident perpendicularly upon a *corrugated* surface, the acoustical analogue of which is treated in ‘Theory of Sound,’ 2nd ed. § 272 *a*, and to the reflexion of electric waves from a conducting cylinder (§ 343).

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* The comparison is not limited to the case of perfect conductors, but applies also when the obstacles, being non-conductors, differ from the surrounding medium in specific inductive capacity, or in magnetic permeability, or in both properties.

XXXVIII. *Discussion of a New Theorem in Wave Propagation.* By G. JOHNSTONE STONEY, *M.A., D.Sc., F.R.S.*[*]

IN the first of three papers on Microscopic Vision in the Philosophical Magazine of last year, on p. 335 of the October number, the theorem is enunciated that any disturbance in the luminiferous æther may be resolved into undulations of *plane* waves, each wave being of unlimited extent and *uniform* throughout its extent.

The theorem is of course not limited to disturbances of the special kinds that can exist in the luminiferous æther, but applies generally to any disturbance within a given space, or to any system of displacements that may prevail throughout it. (See the Philosophical Magazine for February 1897, p. 139.)

The symbolical expression of this theorem is that

$$F(x, y, z, t) = \iint\Sigma\left[M \sin\left(2\pi\frac{lx+my+nz-vt}{\lambda}+\alpha\right)\right] . d\omega\dagger,$$

where $d\omega$ is the element of solid angle round the direction lmn. If written in polar coordinates the theorem takes the somewhat simpler form

$$F(x, y, z, t) = \iint\Sigma\left[M \sin\left(2\pi\frac{r-vt}{v\tau}+\alpha\right)\right] . \sin\theta\, d\theta\, d\phi.$$

In this enunciation of the theorem r stands for $lx+my+nz$, or its equivalent $x\cos\theta+y\sin\theta\cos\phi+z\sin\theta\sin\phi$; in it also $v\tau$ is written instead of the wave-length λ, the successive values of τ being the periods of the successive terms. M the transversal of one of the terms, α its phase, and v its velocity‡, are all functions of θ, ϕ, and τ. The transversal M is a directed quantity and of such a kind that it may be resolved into three principal positions, which will usually be the position $\theta\phi$ and two others at right angles to it and to one another. Hence the aggregate of all terms like that within

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[*] Communicated by the Author.

[†] This way of writing the theorem is to be preferred to that given on p. 140 of this volume of the Philosophical Magazine, because by writing $Md\omega$ instead of A it keeps clearly before our minds the circumstance that when treating of a resolution into plane waves the elements of the summation are not individual undulations of plane waves but sheafs of such undulations (see the November number of the Philosophical Magazine, p. 436). So, in plane geometry, the element of area is not a line y but a strip $y\,dx$.

[‡] In monotropic media where the wave-surface is a sphere, v for each position of transversal becomes a function of τ only,

brackets in the formula, in any one direction $\theta\phi$, is in effect the sum of three separate series, corresponding to the three principal positions of the transversals of waves travelling in that direction. There are accordingly three times as many terms in the summation as there are values of τ. If the original disturbance $F(x, y, z, t)$ is periodic, each of the three series may be further simplified by supposing τ to take in succession all the values of T/n as in Fourier's theorem, T being the period of the disturbance and n an integer. But it seldom happens that $F(x, y, z, t)$ is periodic. If it be not periodic, the simplest conception to entertain is that M and α vary either abruptly from time to time or continuously; but practically a much better treatment, though not the simplest, is to regard the values of τ as indefinitely close to one another*, and M and α as not varying with the time. This is equivalent to regarding T as infinite.

Every theorem of this kind can be investigated in various ways; and these will furnish proofs, some of them symbolical, others geometrical. The symbolical type of proof is chiefly of use if it can be made to furnish expressions for the M's, the α's, and the v's in each direction lmn. It may be hoped that this will soon be accomplished. The chief value of the geometrical form of proof is that it gives us a more continuous view of what is going on in nature, inasmuch as the steps of the geometrical proof of a physical problem keep throughout their whole progress in close proximity to what actually takes place, whereas a symbolical proof is in contact with nature only at its commencement and at its close. The intermediate steps are in general mere paper work. Each method accordingly has its special advantage, and it is desirable that both shall be studied. Two examples of geometrical proofs of the theorem have been given in the October number of the Philosophical Magazine, one on p. 335 and one in the footnote on the following page, but neither of these is the most direct of that kind of proof. The geometrical proof seems to reach its simplest and most direct form, and therefore the form which gives to us the fullest insight, when presented as follows:—

Alternative proof of the theorem that *Any disturbance within a given space may be resolved into undulations of uniform plane waves.*

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* By regarding matters in this way it can be seen that, where lines in a spectrum arise (as they always do) from a jumble of molecular events, they *must* each have some physical width in the spectrum: even in the case where the molecular events taken separately emit waves of strictly definite periodic times only, which if it were not for the breaks and intervals between the events could only furnish lines in the spectrum devoid of physical width.

If the problem be merely kinematical the properties of the medium in which the motion takes place need not be considered, since the form of wave-surface round a point, the law for the propagation of waves through the space and the legitimacy of the geometrical superposition of motions within it may all, in a kinematical investigation, be arbitrarily assumed. But the case is different when we are dealing with nature. Here we must take into account the physical properties of the medium—the form of the wave-surface round each centre of wave propagation, the number of sheets it has, and the character of the wave motion in each of these; and we must limit ourselves to the cases in which the geometrical superposition of the motions is legitimate.

We shall begin with the simplest case, that of a monotropic medium. Here the wave-surface round any centre of disturbance is one or more concentric spheres—usually two, one belonging to the longitudinal the other to the transverse waves.

Again, an undulation of spherical waves of any kind emitted from a centre of disturbance may be divided into a longitudinal and two transverse trains of waves along each radius with transversals at right angles to one another, if the motion be a dynamical one; or into whatever correspond to these if the undulation be of any other kind; and each of these may by Fourier's theorem be regarded as due to the coexistence of trains of waves of the simple pendulous kind. We accordingly need only concern ourselves with these last.

Let then p, p', &c. be puncta, *i. e.* centres of disturbance, in such a medium. Draw any plane AB at a distance from them and perpendiculars r, r', &c. from the puncta to this plane. Also draw a cylinder KQ with one of these, say r, as its axis, and with a radius k large enough to include within the cylinder all the puncta. Then draw spheres round the individual puncta, touching the plane AB at b, b', &c. to represent the waves which had emanated from the puncta* and which have reached the plane AB at a given epoch T.

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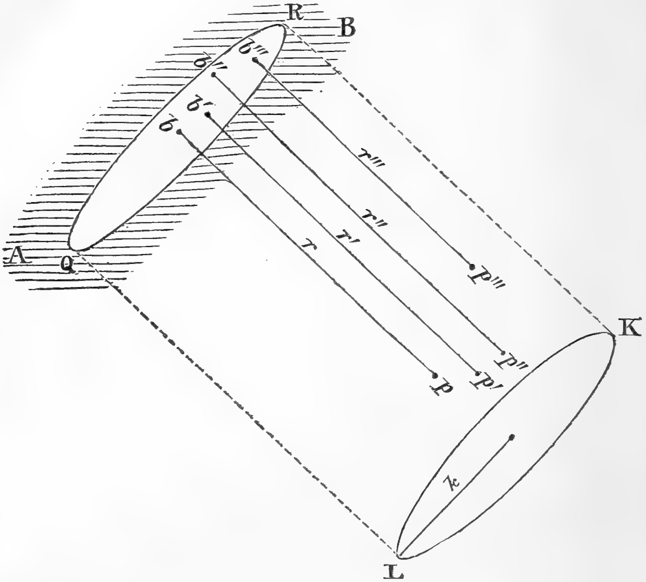
* The contribution from each punctum is to be estimated as that due to the *difference* between its motion, and the motion which would have reached its situation from the operation of all the other puncta after making a similar allowance in their case. See on this subject a "Note on the Propagation of Waves" in the Transactions of the Royal Irish Academy for 1860, p. 37, in which what is here called the contribution from a punctum is there called its influence.

The necessity for estimating the contributions in this way has been much overlooked in textbooks on Light, &c., where the subject is usually so presented as to lead to erroneous conclusions, such as that a wave ought to propagate itself backwards as well as forwards.

The times at which they had started from their respective centres may be represented by T$-t$, T$-t'$, &c. Let now the plane AB be removed so far from the puncta that the radii r, r', &c. become large quantities of the first order; and consider the sectors of the spherical waves which are cut off by the cylinder KQ. The sagittas of the spherical waves, *i. e.* the intervals between the spheres and their common tangent-plane, nowhere exceed small quantities of the first order over these sectors: and accordingly *plane waves lying in the plane* AB *may be substituted for them**. Now these by



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* The legitimacy of this substitution is usually assumed. It may be proved as follows:—It arises from the circumstance that in the propagation of waves which are approximately plane and uniform to distances however great, differences of phase which were infinitesimal at starting remain infinitesimal to whatever distance they may be propagated; and that changes of phase which are infinitesimals of the first order can only involve changes of amplitude of the second order of small quantities.

Hence if all motions were at a given instant reversed in one of the sectors of spherical waves cut off by the cylinder—suppose in that which emanated from p,—and simultaneously reversed in the wave we have substituted for it in the plane QR (viz. in that part of the tangent-plane cut off by the cylinder): then the corresponding elements of these differ in phase only by infinitesimals and differ in no other respect. For simplicity suppose the medium otherwise undisturbed. Then these the theorem in section 38, p. 435 of the Philosophical Magazine for last December, combine into a single plane wave in that position.

Again, spherical waves round p, p', &c. are not uniform, but the departure from uniformity, since it depends on the position of the transversal, varies *gradually* over each spherical wave; and when the radii r, r', &c. are large quantities of the first order, this want of uniformity is only a small quantity of the first order over the sectors cut off by the cylinder. Hence the individual waves and therefore the resultant wave are uniform within those limits.

Hence, finally, the disturbance proceeding in the direction of the axis of the cylinder becomes ultimately an undulation of uniform plane waves.

From the proof it appears that the lateral extent which may legitimately be given to the cylinder KQ, and therefore to its sections QR and LK, increases, and increases without limit, when the distance of the plane AB from the puncta is increased without limit. Hence the disturbance proceeding in the direction pb becomes *at the limit* an undulation of uniform plane waves, each of unrestricted extent in the plane of the wave, *i.e.* perpendicular to the direction pb. It is in fact one of the waves included in the summation

$$\iint \Sigma \left[\mathrm{M} \sin\left(2\pi \frac{lx+my+nz-vt}{\lambda} + \alpha \right) \right] . d\omega.$$

This being true whatever direction be taken for pb, the proposition is proved in the case we have been considering, *i.e.* in the case of monotropic media.

It is very instructive to conceive all the motions reversed at a given instant, so that the plane waves of infinite extent travel inwards*, reproducing at each stage of their inward progress the same disturbed state of the medium as had

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propagate undulations backwards, which as they reach any other section of the cylinder, LK, *however distant*, will still differ in phase from one another only by infinitesimals, *i. e.* by quantities which it is legitimate to disregard, and will differ in no other respect except by quantities of a still higher order of small quantities.

* In carrying out this conception it should be borne in mind that, in a monotropic medium, *each part* of an infinite undulation of uniform plane waves simply advances *without change* in the direction perpendicular to the plane of the waves. If the medium be not monotropic, the undulation as a whole still advances without loss of intensity perpendicularly to the plane of the waves; but this is now by reason of each part of it advancing without change in an oblique direction.

existed on the outward journey, except that the motions are now reversed. This conception makes it specially easy to picture the undulations of plane waves as flowing in from all directions and across the space that had been occupied by the original disturbance, and while crossing it reproducing in it the original disturbance with all its motions reversed. We may, if we choose, complete the picture by imagining another reversal of all the motions afterwards to take place, when the undulations will again cross the situation of the original disturbance, reproducing it as it originally existed and without any reversal of its motions.

A very simple contrivance will here enable us to realise more fully what it is that occurs in nature. To this end let us imagine a sphere to be described including the space throughout which we have supposed the original disturbance to prevail, and across and beyond which we now know that undulations of uniform plane waves of indefinite extent are advancing in all or some directions. This sphere may include also any portion of the surrounding space of which we may desire to investigate the motion. Next imagine cylinders enveloping this sphere to be pointed in *all* the directions in which the undulations advance. Each cylinder will then cut what we may call a *beam* out of the undulation proceeding in its direction, and the whole of each undulation may be distinguished into the two parts—the beam or part inside the cylinder, and the rest of the undulation which lies outside it. Now after the reversal it is only these beams that can reach any part of the space inside the sphere; they are therefore the only parts of the undulations with which we need concern ourselves if our only object is to investigate the events which occur within the sphere. We do not need to take into account the cooperation of other parts of the undulations unless we want to investigate events that lie beyond the sphere. We may call the beams the effective part of the whole system of undulations, and the rest of the undulations the inoperative part with respect to events within that sphere.

It will accordingly suffice to think of the sphere as emitting these *beams* on their outward journey, and again to picture to ourselves these beams after the reversal as converging in upon the space within the sphere*.

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* The only effect of the rest of the undulations upon these beams is to prevent the formation along their bounding cylinders of those special conditions which are met with over the sides of ordinary beams. The plane waves of which they consist remain uniform quite up to their bounding cylinders.

This is a condition of affairs which has the advantage of being easily conceived with distinctness. We can see the beams on their outward journey becoming more and more disentangled from one another as they advance, and can picture them on their return journey as converging, so as more and more to overrun one another, and thus produce a more complex disturbance in the medium the farther they advance inwards, which, although it will fall short of presenting the actual disturbance anywhere outside the sphere, becomes the actual disturbance (with motions reversed) everywhere within that space. What takes place is substantially of the same kind as what occurs in the simpler case of a straight string set vibrating; where the motion whatever it is may be represented by waves of fixed forms simultaneously travelling in opposite directions over the length of the string. (See Chapter V. of Donkin's 'Acoustics,' where the case of transverse vibration in one plane is worked out.)

We can also picture to ourselves what will occur if there be also sources of disturbance outside the sphere. Every such addition will make an alteration* throughout *all* the undulations, and therefore in those portions of them included in the beams that travel across our sphere; but will nevertheless not change in the least the effect produced by their coexistence within that space.

It is evident, on this and other accounts, that a resolution into undulations of plane waves may occur in numberless ways. The choice between them which nature will make in any particular case is, as in applications of Fourier's theorem &c., to be ascertained by considerations affecting the energy received by, stored in, and escaping from, the system; as, for example, the necessity of complying with the Principle of Least Action. It may be of interest to mention that the "crumples" which Helmholtz observed in the motion of violin strings, but left unexplained (see 'Sensations of Tone,' part i. chap. v. § 4), can be accounted for and their forms determined by including in the investigation the considerations here referred to.

Extension to other than monotropic media.—Hitherto we have supposed the medium to be monotropic. If it be of

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* This alteration has a twofold source. It is primarily due to direct contributions to the wave at AB made by the new sources of disturbance; but it is also due in part to the changed circumstances of the puncta within the sphere, since the motion within the sphere which is occasioned by the sources of disturbance outside it has now to be deducted before we determine what contributions the puncta within the sphere will make to the wave at AB. See footnote, p. 275.

other kinds, the wave-surface round a centre of disturbance is no longer a sphere or spheres: it is a wave-surface of some other form. If it be a surface which when referred to rectangular coordinates has an equation of which the parameters are of the form *at*, *bt*, &c., where *a*, *b*, &c. are constant velocities, then when referred to polar coordinates the velocity of propagation in the various directions is a function of θ and ϕ only, and remains unchanged in each direction. Accordingly, as the wave advances and the surface enlarges, its form remains the same. Hence when these wave-surfaces are drawn round the puncta *p*, *p′*, &c. of the diagram on p. 276, their common tangent-plane need no longer be perpendicular to the axis of the cylinder, but will, in general, be oblique to it. It appears, then, that the only change that needs to be made is to allow the plane AB to assume in the diagram any position, whether perpendicular or oblique to the axis of the cylinder. The rest of the proof then proceeds as before, so that the theorem is true in all media in which the wave-surface belongs to the family of surfaces described above. This includes all known uniform media.

If we could in any experiment isolate one of the undulations of plane waves we should see it extending indefinitely sideways, *i.e.* in the direction of the plane of the waves. But the most we can effect experimentally is to isolate small sheafs of these undulations, of which the axial rays (*i.e.* perpendiculars from the origin on the planes of the waves) lie within a small cone; as in the experiment described in § 42 of the paper on Microscopic Vision in the December Number of the Philosophical Magazine, p. 525. In that experiment we find that if two such sheafs of undulations are allowed to interfere and produce a ruling, this ruling will be seen to extend more and more laterally the smaller we make the sheafs; and they would extend laterally to an indefinite extent if we could indefinitely decrease the angle of the cones within which lie the axial rays of the undulations which make up the sheafs.

The method of investigation followed in the preceding pages enables us to acquire a singularly clear insight into how it comes to pass that a disturbance, however complex, can be resolved into undulations of absolutely uniform plane waves. Of course it follows as an easy corollary that resolutions into undulations of curved waves are also possible; but none of these has the two supreme advantages of consisting of waves which are *uniform* over the whole of each wave-front, and which when they advance to distances small or great *undergo no change* either in intensity or in any other respect.

XXXIX. *On the General Extension of Fourier's Theorem.*
By THOMAS PRESTON, *M.A., F.R.U.I.*[*]

IN a recent number of the Philosophical Magazine Dr. G. J. Stoney[†] has announced the following important optical theorem in relation to microscopic vision:—"*However complex the contents of the objective field, and whether it or parts of it be self-luminous or illuminated in any way, however special, the light which emanates from it may be resolved into undulations each of which consists of uniform plane waves.*"

In this theorem it is explicitly stated as a general principle that any disturbance however complex within a given region of space may be resolved into a system of plane wave components, or that a system of trains of plane waves can be determined such that when compounded they will reproduce any arbitrarily specified disturbance throughout a given region.

When the disturbance is a function of a single variable this statement forms the verbal expression of Fourier's Theorem, viz. that any arbitrary function of a variable can be expressed as a series of sines or cosines of multiples of that variable, between assigned limits ; and when the disturbance is a function of two or more variables the theorem in its most general form implies that any function of any number of variables can be expanded in a series, each term of which can be expressed in terms of a linear function of the variables.

Since the time of Fourier it has been customary to represent a general function of any number of variables by a series each term of which is a continued product of cosines of multiples of the variables ; thus

$$f(x, y, z) = \Sigma \mathrm{A} \cos lx \cos my \cos nz. \quad . \quad . \quad . \qquad (1)$$

This form in fact was used by Fourier in his investigation of "the movement of heat in a solid cube." Now if the exponential values of $\cos lx$ &c. be substituted for them, it is seen at once that the product of the cosines $\cos lx \cos my \cos nz$ resolves itself into terms of the type $\cos (lx \pm my \pm nz)$, and consequently the form of expansion (1) in the case of any number of variables is equivalent to

$$f(x, y, z, \ldots) = \Sigma \mathrm{A} \cos (lx + my + nz + \ldots) ;$$

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[*] Communicated by the Author.

[†] Phil. Mag. vol. xlii. p. 332 &c. (1896), and vol. xliii. p. 139 (1897).

which is the analytical expression of the foregoing general statement when the variables are taken to be x, y, z, t, namely the coordinates of a point in space and the time t.

In order to investigate this expansion we return to the case of a single variable and write Fourier's expansion in the form

$$\pi f(x) = \Sigma \cos lx \int_0^{2\pi} f(\alpha) \cos \alpha \, d\alpha + \Sigma \sin lx \int_0^{2\pi} f(\alpha) \sin \alpha \, d\alpha$$

$$= \Sigma \int_0^{2\pi} f(\alpha) \cos l(x-\alpha) \, d\alpha, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (2)$$

where l is any whole number positive or negative.

Now, if in (2) we replace $f(x)$ by a function of two variables $f(x, y)$, we have

$$\pi f(x, y) = \Sigma \int_0^{2\pi} f(\alpha, y) \cos l(x-\alpha) \, d\alpha \, ; \quad . \; . \; . \quad (3)$$

and if $f(\alpha, y)$ in the right-hand member of (3) be replaced by its expansion in a Fourier series of cosines and sines of multiples of y, as denoted in equation (2), we have

$$\pi^2 f(x, y) = \Sigma\Sigma \int_0^{2\pi} \int_0^{2\pi} f(\alpha, \beta) \cos l(x-\alpha) \cos m(y-\beta) \, d\alpha \, d\beta \quad . \; . \; . \; . \quad (4)$$

where l and m are any whole numbers. Similarly the equation for any number of variables may be written down at once. Since the general term of (4) is of the form $Ae^{i(lx+my)}$, it is clear that any function of any number of variables can be expanded in the form

$$f(x, y, \ldots) = \Sigma \mathrm{A} \cos (lx + my + \ldots) + \Sigma \mathrm{B} \sin (lx + my + \ldots) \, ; \quad . \; . \; . \; . \quad (5)$$

where l, m, &c. are any whole numbers positive or negative, and the coefficients A, B, &c. are given by (4).

Thus, in the case of two variables the coefficients of $\cos (lx + my)$ and $\sin (lx + my)$ are respectively

$$\mathrm{A}_{lm} = \frac{2}{(2\pi)^2} \int_0^{2\pi} \int_0^{2\pi} f(\alpha, \beta) \cos (l\alpha + m\beta) \, d\alpha \, d\beta, \quad . \; . \quad (6)$$

$$\mathrm{B}_{lm} = \frac{2}{(2\pi)^2} \int_0^{2\pi} \int_0^{2\pi} f(\alpha, \beta) \sin (l\alpha + m\beta) \, d\alpha \, d\beta \, ; \quad . \; . \quad (7)$$

and similarly in the case of n variables we have

$$A_{lm\ldots}=\frac{2}{(2\pi)^n}\int_0^{2\pi}f(\alpha,\beta,\gamma\ldots)\cos(l\alpha+m\beta+n\gamma+\ldots)\,d\alpha\,d\beta\,d\gamma\ldots \quad . \quad . \quad . \quad . \quad . \quad (8)$$

$$B_{lm\ldots}=\frac{2}{(2\pi)^n}\int_0^{2\pi}f(\alpha,\beta,\gamma\ldots)\sin(l\alpha+m\beta+n\gamma+\ldots)\,d\alpha\,d\beta\,d\gamma\ldots \quad . \quad . \quad . \quad . \quad . \quad (9)$$

These values of the coefficients may also be obtained directly if we assume the expansion to be expressed in the form (5). For if we multiply both sides of (5) by $\cos(lx+my+\ldots)$, and integrate between the limits zero and 2π with respect to each variable, we obtain at once

$$\int_0^{2\pi}f(x,y,\ldots)\cos(lx+my+\ldots)\,dx\,dy\ldots=\tfrac{1}{2}A(2\pi)^n,$$

which agrees with (8).

When the variables are taken to be the coordinates x, y, z of a point in space and the time t, we have the form

$$f(x,y,z,t)=\Sigma A\cos(px+qy+rz+st)$$
$$+\Sigma B\sin(px+qy+rz+st),$$

or the equivalent form

$$f(x,y,z,t)=\Sigma a\cos(px+qy+rz+st+\alpha),$$

where a and α are constants expressible in terms of the definite integrals A and B by the equations

$$a=(A^2+B^2)^{\frac{1}{2}},\quad \tan\alpha=-B/A.$$

This, then, is the analytical expression of the general theorem enunciated by Dr. Stoney, and means that any disturbance whatsoever in a medium either uniform or heterogeneous can be resolved into systems of trains of plane waves.

That some such resolution should be possible is not surprising when the matter is regarded from the physical standpoint. For two trains of plane waves traversing space in different directions will produce illumination of a simple grating type on a screen placed perpendicular to the plane of the wave normals; that is, the pattern on the screen consists of parallel bars, and the amplitude as we pass across a bar varies according to the cosine law. If therefore any pattern, however complex, can be built up by superposing various simple grating patterns, it follows that any disturbance within a given region can be resolved into plane waves, and that any function of x, y, z, t can be resolved into a sum of cosines or sines of linear functions of the variables. It is not difficult to admit that a very complex pattern may be built up by superposing simple-grating patterns, but that any arbitrary pattern whatsoever may be so constituted requires demonstration.

When the disturbance is periodic throughout space the expansion in cosines or sines will represent the function completely throughout all space, and when the function is not periodic the expansion can be made to agree with the function at all points within assigned limits.

In the case of a periodic function the expansion may be derived from the condition of periodicity. Thus if $\lambda, \mu, \nu, \ldots$ be the periodic intervals and $l, m, n, \ldots$ any whole numbers, we have by the condition of periodicity,

$$f(x, y, z, \ldots) = f(x + l\lambda,\ y + m\mu,\ z + n\nu, \ldots). \quad . \quad . \quad (10)$$

If we denote the operator $\frac{d}{dx}$ by D_1, and $\frac{d}{dy}$ by D_2, &c., we have by applying Taylor's theorem to (10),

$$f(x, y, z, \ldots) = e^{l\lambda D_1 + m\mu D_2 + \&c.} f(x, y, z, \ldots),$$

that is*,

$$(e^{l\lambda D_1 + m\mu D_2 + \ldots} - 1) f(x, y, z, \ldots) = 0. \quad . \quad . \quad . \quad (11)$$

Now any differential equation of the form

$$F(D_1, D_2, D_3, \ldots) f(x, y, z, \ldots) = 0 \quad . \quad . \quad . \quad (12)$$

will be satisfied by $f = e^{ax + by + cz + \ldots}$ provided a, b, c, &c. satisfy the equation

$$F(a, b, c, \ldots) = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

Hence equation (11) will be satisfied by a sum of terms of the form

$$f(x, y, z, \ldots) = \Sigma A e^{az + by + ez + \ldots},$$

provided a, b, c satisfy the equation

$$e^{l\lambda a + m\mu b + n\nu c + \ldots} - 1 = 0.$$

That is $a, b, c, \ldots$ must satisfy the equation

$$l\lambda a + m\mu b + n\nu c + \ldots = 2ir\pi,$$

where r is any whole number. Thus in the case of three

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* This method was employed by Mr. J. O'Kinealy, for the case of a single variable (Phil. Mag. August 1874, pp. 95, 96.)

variables, the quantities ia, ib, ic may be the coordinates of any point on any one of the system of planes

$$l\lambda\xi + m\mu\eta + n\nu\zeta = 2r\pi,$$

in which l, m, n, and r are any whole numbers, positive or negative. The general expansion is consequently of the form

$$f(x, y, z, \ldots) = \Sigma \mathrm{A} \cos (ax + by + cz + \ldots).$$

In the particular, or rather *extreme*, case in which the function f has a given value at each of a system of equally-spaced points and has zero value at all other points of space, we have the disturbance contemplated in the lemma used by Dr. Stoney as the foundation of the demonstration sketched in his first paper*.

It is interesting to notice that any function of an odd number of variables can be expanded in a series of sines or a series of cosines of linear functions of multiples of the variables, as in the case of a single variable, the limits of integration being zero and π. On the other hand, when there is an even number of variables the limits of integration require to be zero and 2π.

In the case in which l, m, n, &c. are not whole numbers the function may still be expanded in the form of a series of sines, or a series of cosines; thus,

$$f(x, y \ldots) = \Sigma \mathrm{A} \sin (lx + my + \ldots),$$

where l, m, &c. are chosen so as to satisfy an equation of the form

$$\frac{\begin{matrix}\cos\\ \sin\end{matrix}\left\{(l+l')\,x + (m+m')y \ldots\right\}}{(l+l')(m+m')\ldots} = \frac{\begin{matrix}\cos\\ \sin\end{matrix}\left\{(l-l')x + (m-m')y \ldots\right\}}{(l-l')\,(m-m')\ldots},$$

and the coefficients A &c. are determined by definite integrals of the form (8) or (9). In the case of a single variable this equation reduces to

$$\frac{\tan lx}{lx} = \text{constant},$$

the form obtained by Fourier.

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* *Loc. cit.*

XL. *Note on the Variation of the Dissociation Coefficient with Temperature.* *By* S. Roslington Milner, *B.Sc.*, 1851 *Exhibition Scholar, University College, Bristol* *.

THE general theory of mass-action, when applied to the dissociation of a binary electrolyte, shows that the rate at which dissociation goes on is proportional to the concentration (amount in gramme molecules per unit volume of solution) of the undissociated part of the salt; while, along with this, a reverse process, the *association* of the separated ions to form the original undissociated salt again, proceeds at a rate proportional to the joint product of their concentrations—or, since the ions exist in equal amounts—to the square of the concentration of either. The true velocity of dissociation, which one would measure, is of course the difference between the rates of these two actions, and may be written

$$\frac{dc'}{dt}=kc-k'c'^2;$$

where dc'/dt is the velocity at any stage of the process, at which the concentrations of the undissociated and the dissociated parts of the salt are c and c' respectively. When a salt is dissolved in water and dissociates, c continually diminishes, and c' increases, until

$$kc-k'c'^2=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

a point being reached at which no further change with time is to be noticed.

Equation (1), therefore, which may be written

$$\mathrm{K}c=c'^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

is the relation which according to mass-action exists, when equilibrium is attained, between the undissociated and the dissociated parts of the solution of a salt, however the *total* concentration of the salt (*i. e.* gr. mols. actually present per litre of solution) may vary. The constancy of the coefficient K or c'^2/c was first arrived at by Ostwald, and experimentally proved by him in the case of a large number of organic acids of slight dissociation †; although in the highly dissociated inorganic acids and salts, formula (2) gives results which are considerably different from the experimental values of the

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* Communicated by the Author.

† *Zeit. Phys. Chem.* ii. pp. 36 and 270 (1888).

dissociation,—a want of agreement for which no satisfactory explanation has been given.

However, even in the cases where the value of the dissociation-coefficient remains constant with varying concentration, it still varies in a definite way with change of temperature. The law of this variation with temperature was first worked out by van 't Hoff, who investigated the general case of the coefficient of equilibrium of any chemical reaction, by applying the second law of Thermodynamics. The algebraical difficulties in the general case are so great, when the concentrations of the reacting substances are allowed to vary as they naturally would on taking the solution round a cycle, that one has to assume them kept constant by some artificial means during the process. This assumption of course does not in the least destroy the rigour of the deduction; still it is always satisfactory to have as many different methods of proof as possible, and in the simple case of a binary electrolyte whose isotherm of dissociation is (2), the variation with temperature of K may be obtained by the application of the second law in the ordinary way, without the necessity of the assumptions entailed in the proof for the general case.

The cycle is carried out by obtaining the maximum osmotic work W done by the solution in the expansion against a semipermeable partition from concentration C_1 to C_2, applying this work to compress it again to C_1 at a slightly lower temperature, and equating the excess to the work corresponding to the reversible thermodynamic cycle. As the electrolyte is diluted, the dissociation varies in accordance with (2), and the work done against the semipermeable partition consists of two parts, that done by the undissociated and that done by the dissociated molecules, which must be determined separately.

Let V be the volume of the solution in litres, and let 1 gr. mol. of the electrolyte be dissolved; so that if C is the total concentration,

$$VC=1. \qquad (3)$$

Taking, as before, c for the concentration of the undissociated electrolyte, and c' for that of either of the ions, we have

$$c+c'=C, \qquad (4)$$

and

$$Kc=c'^2. \qquad (2)$$

The work done by the undissociated part of the salt is

$$w=\int_{V}^{V_2} p\,dV=\int_{C_2}^{C_1} RTc\frac{1}{C^2}\,dC,$$

where p is the osmotic pressure of the undissociated molecules, and R the ordinary gas-constant. Transforming C into c, by means of (2) and (4), we have

$$w=\frac{\mathrm{RT}}{2\sqrt{\mathrm{K}}}\int_{c_2}^{c_1}\frac{\sqrt{c}(\mathrm{K}+2\sqrt{\mathrm{K}c})}{(c+\sqrt{\mathrm{K}c})^2}dc$$

$$=\mathrm{RT}\left\{2\log\frac{\sqrt{c_1}+\sqrt{\mathrm{K}}}{\sqrt{c_2}+\sqrt{\mathrm{K}}}-\frac{\sqrt{\mathrm{K}}(\sqrt{c_1}-\sqrt{c_2})}{(\sqrt{c_1}+\sqrt{\mathrm{K}})(\sqrt{c_2}+\sqrt{\mathrm{K}})}\right\}, \quad \ldots\ldots \quad (5)$$

where c_1 and c_2 are the initial and final concentrations of the undissociated part of the electrolyte. If we put $\mathrm{K}=0$, this of course coincides with the ordinary formula for the work of expansion of a non-dissociable salt,

$$w=\mathrm{RT}\log c_1/c_2.$$

The work done during expansion by the two ions of the electrolyte is

$$w'=2\int_{\mathrm{V}_1}^{\mathrm{V}_2}p'\,d\mathrm{V}=2\mathrm{RT}\int_{\mathrm{C}_2}^{\mathrm{C}_1}c'\frac{1}{\mathrm{C}^2}d\mathrm{C},$$

p' being the osmotic pressure of either of the ions. Transforming this as before into terms of c' only, we find

$$w'=2\mathrm{RTK}\int_{c_2'}^{c_1'}\frac{c'(\mathrm{K}+2c')}{(c'^2+\mathrm{K}c')^2}dc',$$

which gives on integration

$$w'=2\mathrm{RT}\left\{\log\frac{c_1'}{c_2'}\cdot\frac{c_2'+\mathrm{K}}{c_1'+\mathrm{K}}+\frac{\mathrm{K}}{c_2'+\mathrm{K}}-\frac{\mathrm{K}}{c_1'+\mathrm{K}}\right\}, \quad . \quad (6)$$

c_1' and c_2' being the concentrations of the ions before and after expansion. In (6) if we put $\mathrm{K}=\infty$, or the salt is completely dissociated, the work reduces to

$$w'=2\mathrm{RT}\log c_1'/c_2',$$

the ordinary form for a simple mixture of ions.

Before expressions (5) and (6) can be used, they must be changed into terms of the independent variables C_1 and C_2, the total concentrations, since c and c' are functions of K. This may be simply done by (2) and (4), substituting from which we obtain

$$w = \mathrm{RT}\left\{2\log\frac{\sqrt{\mathrm{K}+4\mathrm{C}_1}+\sqrt{\mathrm{K}}}{\sqrt{\mathrm{K}+4\mathrm{C}_2}+\sqrt{\mathrm{K}}} + \frac{2\sqrt{\mathrm{K}}}{\sqrt{\mathrm{K}+4\mathrm{C}_1}+\sqrt{\mathrm{K}}} - \frac{2\sqrt{\mathrm{K}}}{\sqrt{\mathrm{K}+4\mathrm{C}_2}+\sqrt{\mathrm{K}}}\right\},$$

$$w' = 2\mathrm{RT}\left\{\log\frac{\sqrt{\mathrm{K}+4\mathrm{C}_1}-\sqrt{\mathrm{K}}}{\sqrt{\mathrm{K}+4\mathrm{C}_2}-\sqrt{\mathrm{K}}}\cdot\frac{\sqrt{\mathrm{K}+4\mathrm{C}_2}+\sqrt{\mathrm{K}}}{\sqrt{\mathrm{K}+4\mathrm{C}_1}+\sqrt{\mathrm{K}}} - \frac{2\sqrt{\mathrm{K}}}{\sqrt{\mathrm{K}+4\mathrm{C}_1}+\sqrt{\mathrm{K}}} + \frac{2\sqrt{\mathrm{K}}}{\sqrt{\mathrm{K}+4\mathrm{C}_2}+\sqrt{\mathrm{K}}}\right\}.$$

The complete work of expansion W is $w+w'$, or

$$2\mathrm{RT}\left\{\log\frac{\sqrt{\mathrm{K}+4\mathrm{C}_1}-\sqrt{\mathrm{K}}}{\sqrt{\mathrm{K}+4\mathrm{C}_2}-\sqrt{\mathrm{K}}} - \frac{\sqrt{\mathrm{K}}}{\sqrt{\mathrm{K}+4\mathrm{C}_1}+\sqrt{\mathrm{K}}} + \frac{\sqrt{\mathrm{K}}}{\sqrt{\mathrm{K}+4\mathrm{C}_2}+\sqrt{\mathrm{K}}}\right. . \quad . \quad . \quad (7)$$

This is the external work done by the osmotic pressure while the solution expands isothermally from concentration C_1 to C_2, the equivalent W of heat being drawn from the surroundings. A further amount of heat H is also absorbed due to the dissociation of the dissolved substance as the dilution proceeds. The amount dissociated during the dilution is

$$\left(\frac{c_2'}{\mathrm{C}_2}-\frac{c_1'}{\mathrm{C}_1}\right) \text{ gr. mols.};$$

so that if Q is the heat absorbed in the dissociation of 1 gr. mol.,

$$\mathrm{H} = \mathrm{Q}\left(\frac{c_2'}{\mathrm{C}_2}-\frac{c_1'}{\mathrm{C}_1}\right),$$

or

$$\mathrm{H} = \mathrm{Q}\,.\,\frac{\sqrt{\mathrm{K}}}{2}\left\{\frac{1}{\mathrm{C}_2}\left(\sqrt{\mathrm{K}+4\mathrm{C}_2}-\sqrt{\mathrm{K}}\right) - \frac{1}{\mathrm{C}_1}\sqrt{\mathrm{K}+4\mathrm{C}_1}-\sqrt{\mathrm{K}}\right\} \quad . \quad . \quad (8)$$

by (2) and (4).

If this expansion against a semipermeable partition be carried out at temperature T, and then the solution be brought back to its original concentration C_1 by a reverse process at a temperature $\mathrm{T}-d\mathrm{T}$, the mechanical heat absorbed during this cyclic process is

$$W + JH,$$

while the external work performed is

$$\frac{dW}{dT}\,dT,$$

and by the second law of thermodynamics this last is equal to the fraction dT/T of the heat absorbed. Hence

$$\frac{dW}{dT}\,dT = \frac{dT}{T}(W + JH). \quad . \quad . \quad . \quad . \quad . \quad (9)$$

In equation (7) for W, C_1 and C_2 are constant, so that the only variables are T and K. The differential in T, however, produces $\frac{W}{T}\,dT$, which goes out with the similar expression on the right-hand side of (9). We therefore have

$$\frac{JH}{T} = \frac{dW}{dK} \cdot \frac{dK}{dT},$$

or, after a little algebraical simplification,

$$\frac{JH}{T} = \frac{2RT}{\sqrt{K}} \cdot \frac{dK}{dT} \left\{ \frac{1}{\sqrt{K+4C_2}+\sqrt{K}} - \frac{1}{\sqrt{K+4C_1}+\sqrt{K}} \right\}.$$

Substituting now the expression (8) for H, we obtain

$$Q\{(\sqrt{K+4C_2}-\sqrt{K})C_1-(\sqrt{K+4C_1}-\sqrt{K})C_2\}$$

$$= \frac{4RT^2}{K} \cdot \frac{dK}{dT} \cdot C_1C_2 \left\{ \frac{1}{\sqrt{K+4C_2}+\sqrt{K}} - \frac{1}{\sqrt{K+4C_1}+\sqrt{K}} \right\};$$

or, on simplification,

$$Q = \frac{RT^2}{K} \cdot \frac{dK}{dT},$$

which may be written

$$\frac{d(\log K)}{dT} = \frac{Q}{RT^2},$$

the expression already obtained by van 't Hoff.

XLI. *The Heats of Vaporization of Liquids. By* S. ROSLINGTON MILNER, *B.Sc.,* 1851 *Exhibition Scholar, University College, Bristol**.

IT is well known that at the surface of a liquid in contact with its vapour there exists, in consequence of the excess of the downward attraction of the molecules in the liquid over the upward attraction of those in the vapour part, a resultant downward pull on the thin surface-film, which is, of course, balanced by the ordinary hydrostatical variations of pressure and density. If we assume that no more degrees of freedom are opened up to the molecules in their passage from liquid to vapour, so that their average kinetic energies in the two conditions are the same, the difference in the potential energies of a gram of molecules, or the space integral of this surface-force per gram from the liquid to the vapour, may be taken as the same thing as the "internal" heat of vaporization (*i.e.* external work done excluded) expressed in mechanical units.

That with this assumption there must exist a definite relation between the heats of vaporization and the vapour-densities and temperatures of liquids may be seen at once by supposing the surface-film to be indefinitely thin. In this case it would follow that of the molecules striking against the surface of separation, only those whose kinetic energies resolved perpendicular to it were greater than the value of the above integral for a molecule would be able to pass through into the vapour. If the law of molecular motion in the liquid be known, the number passing per second into the vapour is thus a known function of the heat of vaporization; and evaporation goes on at this rate until the vapour-density reaches a stationary value such that the molecules pass back into the liquid at the same rate as they come out. Hence the value of the vapour-density at any temperature would be determined by its temperature and its latent heat.

The determinateness of this connexion it will be seen depends on the assumption that there is a difference only in degree, and not in kind, between a liquid and its vapour—that the only difference between the two, in fact, is that in the liquid the mean free path of the molecules is very small. However, the work of van der Waals has shown that this is approximately the case, and a relation obtained in this way may be expected to hold true to the same degree of approximation.

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* Communicated by the Author.

The actual calculation is more complicated than in the above simplified case in at least two respects. In the first place, instead of the liquid having an indefinitely thin surface, there must certainly in reality be a stratum of finite thickness in which the density varies continuously from that of the liquid to that of the vapour. Also, in dealing with liquids it is necessary to apply the corrections which van der Waals has indicated for the effect of the actual size of the molecules in altering their mean free paths.

With these modifications, consider therefore the region of varying density to be divided into infinitely thin layers of thickness dz, in passing upward through the boundary between two of which a molecule receives a sudden increase $d\phi$ in its mechanical potential, by doing work against the downward force. Putting N the number of molecules per c.c. of the liquid in the lower layer, we have

$$\mathrm{N}d\phi=\mathrm{J}\rho\, d\mathrm{L}_i, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

where $d\mathrm{L}_i$ is the element of internal latent heat per gram between the two layers, and ρ the density of the liquid at this point.

If there were no collisions among the molecules, the minimum velocity, v_1, which a molecule must have in order to pass upwards through the boundary plane of the layers, would be given by the condition that its kinetic energy perpendicular to the plane must be equal to $d\phi$, or

$$\tfrac{1}{2}mv_1^2\cos^2\theta=d\phi,$$

in which m is the mass, and θ the angle the direction of motion makes with the perpendicular to the plane.

As a matter of fact, however, collisions take place; and after a collision the molecule struck moves on from a position in advance of the centre of the striking molecule by an amount which (at constant temperature) is on the average a constant fraction of the distance between the centres of the molecules at impact. If s be this distance, and l the mean free path, then as van der Waals has shown *,

$$\frac{l-s}{l}=\frac{v-b}{v}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

where v is the volume of a gram of the liquid, and b a

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* Continuity of State: Phys. Soc. Translation, p. 374. l is the mean free path calculated by neglecting the effect of the extension of the molecules in the direction of their relative motion.

quantity proportional to the total volume of the molecules it contains.

A molecule moving up to the boundary plane at an angle θ to its perpendicular would experience in passing through a layer dz, $dz/l \cos\theta$ collisions. We may determine the effect of a collision by assuming that the molecule struck takes up a similar motion to that of the striking one at an average distance s in advance of it in the line of motion. But this distance s in a direction θ with the perpendicular to the plane is accomplished without motion against the molecular forces, so that for each collision the work necessary for the molecule to do in moving upwards may be considered to be diminished by

$$s \cos\theta \frac{d\phi}{dz}.$$

The minimum kinetic energy a molecule must have to pass upwards through the division plane of the layers, may therefore be written

$$\tfrac{1}{2} m v_1^2 \cos^2\theta = d\phi - \frac{s}{l} d\phi = d\phi \, . \, \frac{v-b}{v} \quad . \quad . \quad . \quad (3)$$

by (2). Eliminating $d\phi$ between (1) and (3), we obtain, since $\mathrm{N}m = \rho$,

$$v_1 = \sqrt{2\mathrm{J}d\mathrm{L}_i \, . \, \frac{v-b}{v}} \, . \sec\theta. \quad . \quad . \quad . \quad . \quad (4)$$

If we now take the motion of the molecules of the liquid to be that given by Maxwell's law of velocities, the number of molecules per c.c. of the layer dz, moving with velocities between q and $q+dq$, and in directions making angles between θ and $\theta + d\theta$ with the perpendicular to the boundary plane, is

$$\frac{2\mathrm{N}}{\sqrt{\pi\alpha^3}} \sin\theta \, . \, q^2 e^{-q^2/\alpha^2} dq d\theta, \quad . \quad . \quad . \quad . \quad (5)$$

where α is the "velocity of maximum number" of the molecules. If there were no collisions the number of these striking against the boundary plane of the layers per second would be obtained by multiplying this expression by $q \cos\theta$. The effect of the collisions may, however, be determined as before, by assuming that for every distance $l-s$ that a molecule moves, another molecule takes up its motion at a distance s in advance of it. The apparent velocity with which the molecules move towards the plane will therefore be increased in the ratio $\frac{l}{l-s}$ or $\frac{v}{v-b}$.

Multiplying (5), therefore, by $q \cos\theta \frac{v}{v-b}$ and integrating, we find for the number of molecules striking the division plane of the layers per second from below, and passing through it into the upper one,

$$\mathrm{N}_1 = \frac{2\mathrm{N}}{\sqrt{\pi}\alpha^3} \cdot \frac{v}{v-b} \int_0^{\pi/2} \int_{v_1}^{\infty} \sin\theta \cos\theta q^3 e^{-q^2/\alpha^2} dq\, d\theta, \quad . \quad (6)$$

in which the lower limit of q is the v_1 of (4). The integration in q, which must be performed first, since v_1 is a function of θ, gives

$$\mathrm{N}_1 = \frac{\mathrm{N}}{\sqrt{\pi}\alpha} \cdot \frac{v}{v-b} \int_0^{\pi/2} e^{-\frac{2\mathrm{J}d\mathrm{L}_i}{\alpha^2} \cdot \frac{v-b}{v} \sec^2\theta} \left\{ \alpha^2 \sin\theta \cos\theta + 2\mathrm{J}d\mathrm{L}_i \frac{v-b}{v} \tan\theta \right\} d\theta,$$

which on further integration reduces to

$$\mathrm{N}_1 = \frac{\mathrm{N}\alpha}{2\sqrt{\pi}} \cdot \frac{v}{v-b} e^{\frac{-2\mathrm{J}d\mathrm{L}_i}{\alpha^2} \cdot \frac{v-b}{v}} \quad . \quad . \quad . \quad . \quad . \quad (7)$$

In the upper layer, the number of molecules per c.c. has become $\mathrm{N} - d\mathrm{N}$, and the specific volume $v + dv$, and all the molecules which strike the boundary plane from above pass through it into the layer below. The number doing this per sec. is therefore,

$$\mathrm{N}_2 = \frac{2(\mathrm{N} - d\mathrm{N})}{\sqrt{\pi} \, . \, \alpha^3} \cdot \frac{v + dv}{v + dv - b} \int_0^{\pi/2} \int_0^{\infty} \sin\theta \cos\theta q^3 e^{-q^2/\alpha^2} dq\, d\theta,$$

or

$$\mathrm{N}_2 = \frac{(\mathrm{N} - d\mathrm{N})\alpha}{2\sqrt{\pi}} \cdot \frac{v + dv}{v + dv - b}, \quad . \quad . \quad . \quad . \quad (8)$$

where α is assumed to have the same value as before.

The total number passing through the boundary plane in either direction must be zero when the vapour is saturated, hence $\mathrm{N}_1 = \mathrm{N}_2$, and equating the right-hand sides of (7) and (8),

$$e^{\frac{2\mathrm{J}d\mathrm{L}_i}{\alpha^2} \cdot \frac{v-b}{v}} = \frac{v + dv - b}{v - b},$$

since $\qquad \mathrm{N}/(\mathrm{N} - d\mathrm{N}) = (v + dv)/v.$

Taking logarithms and expanding, and writing for α^2 its value $\frac{2}{3}\overline{q^2}$, or $\frac{2\mathrm{RT}}{\mathrm{M}}$, where R is the ordinary gas constant

(=1·979J), and M the molecular weight of the liquid, we have

$$dL_i = \frac{RT}{JM}\frac{vdv}{(v-b)^2},$$

or integrating from the interior of the liquid to that of the vapour,

$$L_i = \frac{RT}{JM}\left\{\log\frac{v'-b}{v-b} + \frac{b}{v-b} - \frac{b}{v'-b}\right\}, \quad . \quad . \quad (9)$$

in which v and v' are the specific volumes of the liquid and saturated vapour respectively.

By the method of its derivation, eq. (9) is general, and gives the relation between the specific volumes and the differences of potential JL_i, due to any system of bodily forces acting on a vapour the size of whose molecules is not negligible compared with their free paths.

The assumptions employed above as to the effect produced by the volume of the molecules are the same as those which lead to van der Waals's characteristic equation for fluids:

$$\left(p+\frac{a}{v^2}\right)(v-b) = \frac{RT}{M}.$$

Equation (9) may therefore also be derived from this, and the ordinary hydrostatical equations of equilibrium in a somewhat simpler way, although this gives no account of the molecular actions which constitute the process.

As the assumptions involved in the term $\frac{a}{v^2}$ are not necessary, let

$$p(v-b) = \frac{RT}{M} \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

be the relation between the pressure and volume of the fluid in the region of varying density, $-p$ being the actual pressure (molecular included with external). b may be not necessarily independent of the temperature, but if it be not variable with v, we can differentiate (10) at constant temperature and obtain

$$dp(v-b) + \frac{RT}{M}\cdot\frac{dv}{v-b} = 0. \quad . \quad . \quad . \quad . \quad (11)$$

The ordinary equation of hydrostatical equilibrium in the surface-film is

$$dp = \rho\, dV, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

where dV is the element of potential of the bodily forces on the liquid, and ρ the density. Substituting for dp from (11), and integrating from the interior of the liquid to that of the vapour, we obtain

$$V'-V=\frac{RT}{M}\int_{v}^{v'}\frac{v\,dv}{(v-b)^2},$$

which, with JL_i written for $V'-V$, is the same equation as (9) for the internal heat of vaporization.

Another expression for the latent heat might similarly be obtained from the other term of the characteristic equation by writing the pressure in the liquid as

$$p=p'+\frac{a}{v^2}, \qquad p' \text{ being the vapour-pressure.}$$

In the film of varying density, p would also contain terms depending on $\frac{dv}{dz}$; but on integration between places where the densities are constant these would vanish from the final result, and the internal latent heat would become*

$$\frac{1}{J}\int_{v}^{v}\frac{dp}{\rho}, \text{ by (12) } = \frac{1}{J}\cdot 2a\left(\frac{1}{v}-\frac{1}{v'}\right). \quad . \quad . \quad (13)$$

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* It seems to have been usual to assume that the total latent heat of a vapour is given by $\int_{v}^{v'} p\,dv$. Thus Nernst ('Theoretical Chemistry,' p. 209), writing the pressure in the liquid

$$p=p'+\frac{a}{v^2},$$

makes the internal heat of vaporization

$$\frac{1}{J}\int_{v}^{v'}\frac{a}{v^2}dv=\frac{a}{J}\left(\frac{1}{v}-\frac{1}{v'}\right),$$

a result only half as great as (13). Consideration of the process, however, seems to show that the internal heat is the same thing as the difference of potential $V'-V$, and that therefore its value is given by $\int_{v}^{v'}\frac{dp}{\rho}$ or $\int_{v}^{v'} v\,dp$. For the molecules in moving from the liquid to the vapour and doing work against the molecular forces change their kinetic energies by an amount $V'-V$, or $\int v\,dp$ per gram into potential energy. This amount of heat is therefore taken from the system, and remains in the vapour as potential energy. At the same time, as they move up through the region of varying density, they expand and lose kinetic energy $=\int p\,dv$ (although it is not necessary for a molecule to have the extra energy indicated by this to be able to pass from one layer to the next considered in the deduction of (9)—the expansion may be considered

This expression, however, does not seem to give very accurate results; it is of a different form from (9), although of course were van der Waals's equation strictly true it would be equivalent to it.

Equation (9) for the heat of vaporization contains what is to a certain extent an arbitrary constant b. b is not, however, in reality completely arbitrary, as it can be calculated approximately from the critical volume, although the approximation is very rough, or it could, if necessary, be estimated with sufficient exactness by substituting in van der Waals's equation the specific volumes of the liquid at two different pressures. As, however, there seems some theoretical reason to believe that b is slightly variable with the temperature*, it seems better, instead of calculating the heats of vaporization of different liquids with approximate values of b, to test the formula by calculating b for different temperatures of the same liquid from the other quantities of the equation, either to see if it keeps constant or to determine its variation. The values obtained in this way may be tested by applying them in the equation

$$\left(p+\frac{a}{v^2}\right)(v-b)=\frac{\mathrm{RT}}{\mathrm{M}}$$

to see if they give satisfactorily constant values of a (this equation being used here for the liquid state only, where it holds very approximately, although the constant may change considerably through such a large range as that from the liquid to the vapour).

The only two cases in which the heats of vaporization have been measured with any accuracy at different temperatures are water and benzene. Below are the values of b and a calculated for these substances by means of (9), the headings of the columns denoting the quantities expressed by those letters in the earlier part of the paper. (Volumes are given in c.cs., the latent heat in gr. calories, and a is in units such that $\frac{a}{v^2}$ is a pressure in dynes per sq. cm.)

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as going on after the molecules have reached the upper layer). The work $\int p\,dv$ done in this way, however, goes to increase the kinetic energies of other molecules of the system, and is immediately made up to the expanding molecules by radiation, so that the system as a whole does not lose this heat. If part of this work to the extent $p(v'-v)$ is done on the atmosphere, as when evaporation is actively going on, it must of course be supplied to the liquid as extra heat.

* Sutherland, Phil. Mag vol. xxxvi. (1893) p. 507.

Water.

t.	v.	v'.	L_i.	b.	a.
0	1·000	210600	575·7	0·838	$7·76 \times 10^9$
25	1·003	43960	556·0	·822	7·71
50	1·012	12050	536·0	·813	7·66
75	1·026	4102	516·3	·803	7·57
100	1·043	1650	496·5	·799	7·66
125	1·062	755·5	477·0	·794	7·72

Benzene, C_6H_6. Mol. wt. 77·84.

t.	v.	v'.	L_i.	b.	a.
0·2	1·1113	8141	100·10	0·8852	$1·66 \times 10^9$
17·3	1·1341	3523	96·96	·8864	1·605
32·0	1·1544	1874	94·30	·8862	1·614
48·4	1·1784	1001	91·32	·8862	1·628
67·0	1·2070	538·0	80·01	·8854	1·640
86·4	1·2382	304·7	84·57	·8835	1·654
95·6	1·2541	238·3	82·94	·8830	1·666
110·4	1·2812	164·2	80·39	·8837	1·682
121·6	1·3037	126·7	78·55	·8848	1·697

The latent heats have been calculated from the empirical formulæ

$$\text{Water} \quad . \quad . \quad L_i = 606·5 - 0·70t - \frac{p(v'-v)}{J},$$

$$\text{Benzene*} \quad . \quad L_i = 107·05 - 0·158t - \frac{p(v'-v)}{J}.$$

It will be seen that these cases, the only two in which the latent heats have been measured experimentally, give opposite results as regards the constancy of the numbers b and a. In water, while b shows a gradual decrease of about 5 per cent. in 125°, a remains fairly constant; in benzene b seems to remain constant, while a increases. The regularity of the variation of the numbers (such as the fall to a minimum with subsequent increase in a in water) is due to the fact that L_i has been calculated from an empirical formula the constants of which may not be quite accurate. The increase at high temperatures in the value of b for benzene is also due to extrapolating the empirical formula for L_i too far. The measurements of Griffiths and Marshall extended only up to 40°, so that the constancy of b at higher temperatures may be considered doubtful, since the linear form of their equation may

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* Griffiths and Marshall, Phil. Mag. January 1896.

not hold above 40°. To obtain more reliable values at higher temperatures, I have calculated the latent heats of benzene by means of Clausius's equation,

$$\mathrm{L}_i = \frac{1}{\mathrm{J}}(v'-v)\left(\mathrm{T}\frac{dp}{dt}-p\right),$$

by which the latent heat may be obtained up to the critical point. By this means one may calculate the latent heats at different temperatures of some other substances for which they have not been experimentally measured, but in which v, v', and p are known, and so obtain a fair number of cases by which to test the validity of (9). The data have been obtained almost completely from papers by Ramsay and Young, or Young and Thomas, in the Philosophical Magazine, Philosophical Transactions, and Journal of the Chemical Society.

The values of $\frac{dp}{dt}$ and of L have been given in one or two cases by the observers, being obtained from an empirical equation for the vapour-pressure. In the other cases $\frac{dp}{dt}$ has been determined by tabulating the differences in $\log_{10} p$ for 20° (the curve for $\log p$ being nearly a straight line, these differences do not differ greatly from the differential coefficients), and calculating $\frac{dp}{dt}$ from them by the relation

$$\frac{dp}{dt} = p \cdot \frac{\delta_{20} \log_{10} p}{20} \cdot \frac{1}{0·4343},$$

a method which gives dp/dt somewhat more accurately than taking differences on the vapour-pressure curve itself. The values of the latent heats have generally been smoothed by a curve before using in the equation for b.

Below are given the values of b and a for a number of substances, with L_i determined in this way.

Benzene.

t.	v.	v'.	L_i.	b.	a.
0·2	1·1113	8141	101·0	0·8900	
17·3	1·1341	3523	98·7	·8962	$1·671\times10^{9}$
32·0	1·1544	1874	95·9	·8970	1·682
48·4	1·1784	1001	92·9	·8975	1·690
67·0	1·2070	538·0	89·0	·8940	1·683
110·4	1·2812	164·2	80·1	·8820	1·675
163·4	1·4008	53·93	68·3	·8600	1·673
181·4	1·4523	38·84	64·0	·8512	1·677
202·3	1·5225	26·97	59·0	·8490	1·707

It will be seen that the latent heats calculated from Clausius's equation are slightly different from those given by Griffiths and Marshall, so that while *b* diminishes considerably at high temperatures, its substitution in van der Waals's equation gives a very constant value of *a* through a wide range of temperature. The calculation of *b* becomes very uncertain at low and at high temperatures, so that no reliance can be placed on the result at 202°·3 ; *b* would have to be still smaller than it is to bring *a* down to the normal value.

Ethyl Oxide, $(C_2H_5)_2O$. Mol. wt. 84·0.

t.	v.	v'.	L_i.	b.	a.
0	1·358	1209	86·16	1·054	
20	1·401	534·7	80·44	1·043	$1{\cdot}803\times10^9$
40	1·451	268·0	75·36	1·038	1·790
60	1·502	147·7	70·81	1·031	1·786
80	1·562	86·60	65·91	1·025	1·793
100	1·638	53·55	60·32	1·012	1·778
120	1·735	34·09	54·90	1·015	1·816
140	1·857	22·28	47·32	0·977	1·768
160	2·021	14·47	39·75	·977	1·818
170	2·147	11·45	34·16	·951	1·803
180	2·343	8·815	27·09	·912 } ?	
190	2·730	6·172	18·11	·976 }	

Methyl Alcohol, CH_3OH. Mol. wt. 31·92.

t.	v.	v'.	L_i.	b.	a.
20	1·265	5899	266·5	1·036	$5{\cdot}32\times10^9$
40	1·291	2276	258·9	1·043	5·46
60	1·324	993·8	249·6	1·052	5·57
80	1·360	479·8	238·6	1·060	5·64
100	1·401	251·0	225·2	1·063	5·62
120	1·449	140·0	211·0	1·066	5·58
140	1·506	82·25	195·3	1·066	5·51
160	1·577	50·16	178·2	1·067	5·45
180	1·672	31·40	158·2	1·065	5·35
190	1·733	24·94	147·4	1·066	5·32
200	1·808	19·70	134·9	1·065	5·40
210	1·903	15·33	119·5	1·055	

Stannic Chloride, $SnCl_4$. Mol. wt. 259·3.

t.	*v.*	*v′.*	L*i*.	*b.*	*a.*
140	0·5243	61·9	26·0	0·364	$2·26 \times 10^8$
160	·5411	39·9	24·5	·363	2·26
180	·5595	26·6	23·0	·359	2·25
200	·5806	18·35	21·5	356	2·24
220	·6065	12·94	20·0	·357	2·28
240	·6383	9·23	18·2	·354	2·29
260	·6781	6·58	16·0	·347	2·28
280	·7338	4·63	13·2	·335	2·26

Methyl Formate, $HCOOCH_3$. Mol. wt. 59·86.

t.	*v.*	*v′.*	L*i*.	*b.*	*a.*
60	1·0949	167·0	96·2	0·7760	$1·732 \times 10^9$
80	1·1360	95·05	87·8	·7590	1·668
100	1·1831	58·45	82·0	·7560	1·682
120	1·2392	37·25	79·1	·7475	1·681
140	1·3092	24·25	75·5	·7390	1·688
160	1·4013	16·05	68·9	·7130	1·660
180	1·5336	10·60	48·0	·6880	1·650
200	1·7675	6·56	34·0	·6650	1·687

Ethyl Formate, $HCOOC_2H_5$. Mol. wt. 73·83.

t.	*v.*	*v′.*	L*i*.	*b.*	*a.*
100	1·2327	97·0	76·88	0·8455	$1·638 \times 10^9$
120	1·2827	60·4	71·33	·8384	1·625
140	1·3427	39·1	65·64	·8326	1·622
160	1·4169	25·8	58·89	·8194	1·606
180	1·5129	17·4	50·69	·7900	1·560
200	1·6484	11·60	42·50	·7680	1·563
220	1·8902	7·25	30·14	·7485	1·597

Methyl Acetate, CH_3COOCH_3. Mol. wt. 73·83.

t.	*v.*	*v′.*	L*i*.	*b.*	*a.*
100	1·2163	103·5	79·67	0·8519	$1·694 \times 10^9$
120	1·2670	63·4	73·87	·8478	1·680
140	1·3276	40·7	67·78	·8421	1·663
160	1·4020	26·80	61·22	·8370	1·654
180	1·4991	17·60	53·22	·8329	1·648
200	1·6393	11·55	43·72	·8067	1·639
220	1·8936	7·06	30·43	·7780	1·660

Propyl Formate, $HCOOC_3H_7$. Mol. wt. 87·80.

t.	*v.*	*v'.*	L*i*.	*b.*	*a.*
80	1·199	325	79·3	0·898	$1·59 \times 10^9$
100	1·238	184	75·3	·899	1·59
120	1·280	111·5	70·5	·893	1·56
140	1·329	70·4	66·7	·899	1·59
160	1·387	45·9	61·1	·888	1·56
180	1·455	30·9	54·5	·860	1·51
200	1·542	21·2	49·0	·856	1·52
220	1·660	14·5	42·4	·849 } ?	1·52
240	1·839	9·6	34·3	·860 }	1·58

Ethyl Acetate, $CH_3COOC_2H_5$. Mol. wt. 87·80.

t.	*v.*	*v'.*	L*i*.	*b.*	*a.*
80	1·213	285	77·4	0·903	$1·58 \times 10^9$
100	1·254	162	74·5	·916	1·63
120	1·302	97·0	70·0	·918	1·63
140	1·355	60·6	65·2	·918	1·63
160	1·422	38·8	58·4	·900	1·57
180	1·503	25·75	52·1	·887	1·55
200	1·610	17·25	45·5	·886	1·56
220	1·770	11·23	37·5	·885	1·57

It will be seen by inspection of the tables, that while the value of *b* calculated from the heat of vaporization by equation (9) as a rule diminishes fairly considerably with the temperature, that of *a* determined by its substitution in van der Waals's equation remains approximately, and in some cases very accurately constant. The experimental quantities in the last two or three cases seem somewhat less accurate than in the others, and the numbers *b* obtained from them do not lie at all well on a curve—and as *a* shows itself very sensitive to the slightest changes in *b*, its values here show somewhat large divergences. Still an inspection of the numbers at low and at high temperatures shows that the considerable diminution in *b* that takes place is of such an amount as to make the *a* determined from it approximately constant. The slow fall to a minimum and subsequent rise of *a* in some cases may be due either to some extent to experimental inaccuracy, or more probably to equation (9) not holding completely through a wide range of temperature,—as, indeed, with its assumptions it can hardly be expected to do.

Several other substances tried give a similar diminution of b and constancy of a. I have only come across two so far which have failed to do so, propyl alcohol and carbon tetrachloride. In the first of these a shows a gradual and continuous decrease, and in the second an increase, for which it is difficult to see any reason. It may be as well to give the data for these substances:—

Propyl Alcohol, C_3H_7OH. Mol. wt. 59·86.

t.	v.	v'.	L_i.	b.	a.
80	1·330	958·0	161·5	1·120	4·12×10^9
100	1·365	443	152·1	1·130	4·09
120	1·406	225	141·0	1·132	3·93
140	1·455	124	130·4	1·137	3·81
160	1·515	72·3	117·1	1·132	3·60
180	1·591	44·5	104·8	1·139	3·52
200	1·689	28·3	91·4	1·120	3·30
220	1·823	18·0	75·7	1·095	3·12

Carbon Tetrachloride, CCl_4. Mol. wt. 153·45.

t.	v.	v'.	L_i.	b.	a.
60	0·6594	298·2	43·0	0·4765	4·29×10^8
80	·6773	163·5	41·2	·4760	4·33
100	·6972	97·5	39·55	·4757	4·42
120	·7193	61·2	37·76	·4749	4·48
140	·7435	40·3	35·76	·4720	4·51
160	·7703	27·4	33·50	·4665	4·53
180	·8019	19·05	30·90	·4565	4·49
200	·8412	13·48	28·20	·4505	4·54
220	·8907	9·62	25·31	·4460	4·59
240	·9575	6·83	22·02	·4460	4·72
260	1·0628	4·66	17·18	·4370	4·71

The examples given in the previous Tables seem sufficient to substantiate for the majority of liquids the validity of the assumptions on which the equation for the heat of vaporization is based. It is clear that, with the two exceptions mentioned, the internal heat of vaporization of these liquids will be accurately expressed by the equation

$$L_i = \frac{RT}{JM}\left\{\frac{1}{\cdot 4343}\log_{10}\frac{v'-b}{v-b} + \frac{b}{v-b} - \frac{b}{v'-b}\right\}, \quad . \quad (9)$$

in which b, although not constant, has a value satisfying van der Waals's equation,

$$\left(p+\frac{a}{v^2}\right)(v-b)=\frac{\mathrm{RT}}{\mathrm{M}}, \quad . \quad . \quad . \quad . \quad (14)$$

v being the volume of the liquid, and a an absolute constant.

This is not quite the same thing as if b were eliminated between (9) and (14), which would give

$$\mathrm{L}_i=\frac{\mathrm{RT}}{\mathrm{JM}}\log\frac{p+\frac{a}{v^2}}{p+\frac{a}{v'^2}}+\frac{a}{\mathrm{J}}\left(\frac{1}{v}-\frac{1}{v'}\right)-\frac{1}{\mathrm{J}}p(v'-v),$$

as there is reason to believe that a varies a good deal from the liquid to the vapour, although it remains fairly constant in the liquid. However, at low temperatures, a/v'^2 is negligible with regard to p, and p with regard to a/v^2, so that in this case the total heat of vaporization would be sufficiently expressed by

$$\mathrm{L}=\frac{\mathrm{RT}}{\mathrm{JM}}\log\frac{a}{pv^2}+\frac{a}{\mathrm{J}v}$$

(transferring $\frac{1}{\mathrm{J}}p(v'-v)$ to the left-hand side of the equation). A calculation indeed shows that an equation of this form expresses it with considerable accuracy at low temperatures.

Finally, the equation for the heat of vaporization throws an interesting light on Trouton's well-known law that $\frac{\mathrm{ML}}{\mathrm{T}}$ is roughly constant for different liquids, L being the heat of vaporization at the absolute temperature T of the boiling-point.

From equation (9) it will be seen that $\frac{\mathrm{ML}}{\mathrm{T}}$ is a quantity depending on v, v', and b. But v', which is the only quantity which would vary considerably from substance to substance, only enters in the logarithm, and in the denominator of a small term, and will consequently not greatly affect the result; again $\frac{b}{v}$, and therefore $\frac{b}{v-b}$, is not greatly different for different liquids at corresponding temperatures, which may be considered the same as their boiling-points. $\frac{\mathrm{ML}}{\mathrm{T}}$, therefore, at the boiling-point will not vary more than 20 or 30 per cent. for most liquids, which is about the extent to which Trouton's law applies.

XLII. *Notices respecting New Books.*

Vorlesungen über theoretische Physik. H. von Helmholtz. Band V. *Elektromagnetische Theorie des Lichts*, herausgegeben von Arthur König und Carl Runge. Leipzig: Leopold Voss, 1897.

THE publication of Helmholtz's course of lectures on theoretical physics will be welcomed by physicists of every nationality. To be students under a Maxwell or a Helmholtz is a privilege accorded to the comparatively few, and of these fortunate disciples only two or three may feel themselves able to undertake the presentation of the thoughts and ideas of their master in their original form. We heartily rejoice that in the present case such volunteers have been found as Prof. Runge and Dr. König; these will be assisted in the preparation of subsequent volumes by Dr. Krigar-Menzel. The volume now issued as a first instalment of the work is not the initial part of the course, but it was more easily edited than the rest because one of the students took down the lectures in shorthand at the time of their delivery. Nearly half the volume was revised by Helmholtz himself.

After a short introduction dealing with the emission and wave theories of light and leading up to the mention of Hertzian vibrations, the equations of plane transverse and longitudinal waves are briefly discussed. Maxwell's equations of the electromagnetic field are then obtained in a very simple manner, starting from the experimental facts that magnetic lines of force form closed curves round an electric current, and that a change of linear magnetization gives rise to electric currents in closed circuits surrounding it; the co-existence of electric and magnetic polarizations is thus established. The equations are afterwards transformed and applied to the case of a disturbed æther, with or without conducting matter, and it is shown that the waves set up correspond in type to the transverse waves in an elastic medium, the electric and magnetic displacements being at right angles to each other. The transition to optics is effected by means of Huyghens's principle, which Helmholtz deduces from electromagnetics by a generalization of Green's theorem, introducing time as a fourth variable in addition to the three space-coordinates. Diffraction, interference, reflexion, and refraction are treated as in ordinary optics, the author returning to electromagnetics in the discussion of polarization and dispersion.

A connexion between æther and matter is necessary in order to explain dispersion and magnetic rotation of the plane of polarization; the author assumes that the molecules of matter contain two ions with equal positive and negative charges, and that under the influence of the electric field these ions may be drawn asunder or rotated round their centre of mass, which remains fixed. In an alternating electric field some of the energy of the field is therefore used in maintaining the vibrations of these pairs of ions, which are damped by electrical and mechanical forces. By writing down the equations of energy and applying the principle of least action, results are obtained which represent the facts of dispersion and absorption with a fair degree of accuracy. The motion of the charged ions in a magnetic field gives rise to movements of the æther which set up electric and ponderomotive forces; the effects of these are compounded with the electric and magnetic vibrations in the light waves, giving rise to two circularly-polarized waves which travel with different velocities. The theory has recently received some confirmation from Zeeman's observations on the existence and polarization of broadened spectral lines from a source of light in a magnetic field.

The volume is well printed, and appears to be remarkably free from the typographical errors which often occur in works of a mathematical character. The series, when complete, will rank as one of the most important treatises on modern physics. J. L. H.

Magnetic Fields of Force. By H. EBERT, *Professor of Physics in the University of Kiel. Translated by* C. V. BURTON, *D.Sc.* Part I. London: Longmans, 1897.

So much has been written lately concerning scientific education in Germany, to the disparagement of this country, that it is encouraging to meet with a modern text-book for German students in which the principles adopted are essentially of British origin. By the labours of Faraday and Maxwell we were early taught the usefulness of the conception of lines of force, in explaining both electric and magnetic phenomena; it has, however, required the work of Hertz to finally eradicate from Germany and from science the theories of distance-action. Professor Ebert presents in this volume the principal facts pertaining to magnetism and electromagnetism, and shows how they may be explained qualitatively and quantitatively by assuming that the magnetic field contains energy, and that force exists at every point in it. Venturing a step further, he quotes Lord Kelvin's deduction from the phenomenon of magnetic rotation of the plane of polarization, that lines of force are kinematically comparable with axes of rotation, and advances the theory that magnetism is due to a rotational motion. This leads to an interesting chapter on vortex motion and on cyclic motions in general; the latter subject, which was first treated by Helmholtz and Boltzmann and has received further development in Hertz's 'Principles of Mechanics,' serves as an introduction to the second part of the author's work.

We regret to note that the dimensions of magnetic quantities are specified on the assumption that permeability is a mere ratio. This is all the more remarkable in a treatise which traces magnetic phenomena to a motion of the æther, because the nature of such motion determines to some extent the absolute dimensions of the permeability of the medium.

Dr. Burton has performed the duties of translator with great skill; he has resisted the temptation to render a too literal translation, without committing the more serious error of obscuring the author's meaning.—J. L. H.

Iowa Geological Survey. Vol. V. *Annual Report,* 1895, *with Accompanying Papers.* 8vo. 452 pages, with 7 maps, 14 plates, and 72 figures in the text. Des Moines, 1896.

THIS volume contains reports on six Counties, drawn up on a uniform plan. They treat of the situation and area; previous geological work in the district; the physiography, namely topography and drainage; stratigraphy, namely general relations of the strata, and the geological formations locally represented; typical exposures, including unconformities and deformations; economic products and water-supply. The Geology of Jones County is reported on by Dr Samuel Calvin, the State Geologist. Earlier observations on the district by D. D. Owen, J. Hall, J. D. Whitney, C. A. White, and W. J. McGee are duly noted. The topography is described as dependent chiefly on the superficial deposits, modified by the drainage-courses. Of the solid or indurated strata, there are some fragmentary relics of Carboniferous beds, and large areas of Pleistocene and Silurian (Niagara limestones). The last at some places have enormous Corals, exposed by weathering into monumental masses. The soils, including Geest, a "product of secular rock decay;" building-stone, extensively quarried; lime; clays; and other products, as well as water-supply, are carefully dealt with. In Washington County, by Mr. H. F. Bain, Assistant State Geologist, the rock-masses are of Carboniferous (Mississippian) age. In Boone County (by Dr. S. W. Beyer) the Upper (Des Moines) Carboniferous series has useful coal-seams at some places. In Woodbury County (Mr. H. F. Bain) the Cretaceous system supplies the hard rocks; and these have always been of much interest to Geologists in the State; especially the upper series, namely the Colorado stage (consisting of the Niobrara Chalk and Benton Shale), and the Dakota stage with its interesting fossil flora. The Pleistocene deposits ("preglacial, glacial, and post-glacial") are here, as elsewhere throughout these Reports, carefully defined and illustrated with a map, views, and sections. In Warren County (Professor J. L. Tilton), besides the Pleistocene deposits, there are Carboniferous (Pennsylvanian and Mississippian series) strata yielding some coal. Appanoose County (Mr. H. F. Bain) has many coal-mines in active work.

These Reports are full of well-described facts and careful considerations, useful to both student and teacher in geology, and, of course, highly valuable to the people of Iowa State.

Most of the plates and many of the figures, giving views of exposures and of various parts of the country, have been reproduced from satisfactory photographs. The geological sections are somewhat diagrammatic but useful. The maps are doubtlessly exact. The several tables of contents and the general index are very good and of great service to those consulting the work.

Autobiographical Sketch of JAMES CROLL, *LL.D., F.R.S., &c. With Memoir of his Life and Work, by* JAMES CAMPBELL IRONS, *M.A.* 8vo. 550 pages, with two portraits. 1896. Stanford, London.

THE autobiography (pages 9–41) given in James Croll's own words, as dictated to his wife three years before his death, contains the principal incidents of his life, as recollected and thought over himself, down to 1887. His chronic headaches, pressure of work, and disinclination for the task allowed of no diffuse treatment of the many interesting points in this brief personal history.

In the body of the book Mr. J. C. Irons, with the intimate knowledge of a warm and sympathetic friend, has elaborated this short life-sketch, enlarging it with collateral information from friends and correspondents into a very complete biography of James Croll, and a useful and interesting *résumé* of his intellectual work.

In his youth he had very little schooling and much hard work; first, in his father's croft, and afterwards as a wheelwright and joiner. He was not a sharp lad; but a taste for reading came when he got the first number of the 'Penny Magazine,' at Perth, in 1832, Dick's 'Christian Philosopher,' and Joyce's 'Scientific Dialogues.' Making a systematic attempt to learn something of the physical sciences, he was more attracted to the laws and conditions on which their facts and details depend than to the results and phenomena themselves. Thus directing his attention mainly to general principles, he found himself better able to grasp the meaning and bearings of the subject of study; and he states that his early acquaintance with the general principles of physical science was of great service in his researches in after years. He felt, however, that the strong natural tendency toward abstract thinking somehow unsuited him for the practical details of daily work.

Habitually meditative, with strong religious sentiments, and brought up in the thoughtful communion of his Scotch co-religionists, he felt keen interest in the controversies between Arminianism and Calvinism, to the former of which he was attached. But after careful study of Edwards's work on 'Free Will,' and Tappan's ineffectual criticism of that book, and after having "gone over thirty or forty treatises on the freewill controversy," he found Edwards's conclusions to be sound; and "became convinced that some moderate form of Calvanism was nearest the truth, not only of philosophy, but also of Scripture." Had he been able to afford it, he would have been inclined to devote his time to the study of Philosophy, his mind having been benefitted and ideas enlarged, first by Edwards and then by Kant.

A long-standing disease of the left elbow-joint became too bad in 1846 for manual labour; and James Croll found a friend (Dr. Irons) in Perth, who helped him with stock and advice to set up as a tea-dealer at Elgin. In 1848 he married Isabella Macdonald of Forres. With strong and persistent effort he gave up smoking;

he had always been an abstainer from drink. After this time the elbow-joint became badly inflamed, and ultimately was anchylosed. Having had to give up his business on account of bad health, he tried engagements with Assurance and other Societies at various towns, and a Temperance Hotel at Blairgowrie; and ultimately was engaged on the 'Commonwealth' newspaper at Glasgow (1858). Of late years he had had some leisure for reading, but it brought on pain in the eyes (instead of at the top of the head), and this continued for several years. His principal reading was on questions relating to "liberty" and "necessity;" and this led to Theology and Metaphysics. His thoughts on the metaphysics of Theism were published in 1857 in his 'Philosophy of Theism.'

In 1859 James Croll was appointed to the charge of the Andersonian College and Museum in Glasgow. He had already suffered from what appeared to be a heart-affection, interfering with active exertion; but in 1865, whilst stooping, a sudden twitch in the upper left side of the head was followed by a dull pain, which became unbearable if mental work was continued for any length of time. Though his general health was good, any overwork was followed by disability for some days. Nor could he ever afterwards concentrate his thoughts to the overcoming of a difficulty at one stretch.

The free use of scientific books belonging to the Institution itself and to the Glasgow Philosophical Society decided the balance in his mind between the love of physics and the love of philosophy, in favour of the former, at least for a time. Among the modern subjects of physical research, which were then discussed by a goodly band of sympathetic scientists at Glasgow, that relating to the cause of the Glacial Epoch especially attracted Croll's attention.

Once more settled, and with congenial surroundings, in the old Glasgow college, he was expecting to do some steady work in his favourite lines of thought; but the painful condition of head and eyes sadly checked him. He gave his energies conscientiously to the daily duties of the place (in which his brother helped him); and, courageously fighting against difficulties, as he had all his life through, he managed to write several papers, long or short, whilst he remained as keeper at the Andersonian College. The first of these, relating to Ampère's Electrical Experiment, was published in the Philosophical Magazine, April 1861. Other papers followed (many of them in the Phil. Mag.), treating of Electricity, Heat, Gases, Chemical Affinity, Tides, Climate, the Glacial Epoch, Submergence and Emergence of Land, and the Eccentricity of the Earth's Orbit.

In 1867 Mr. Croll was asked to give his services as Resident Surveyor and Clerk, or Secretary, in the Office of the Geological Survey of Scotland, at Edinburgh. After some hesitation he allowed himself to be nominated, and submitted to the regular Civil Service examination. Though he did not satisfy the everyday examiner in ordinary "arithmetic" and "English," his great calculations regarding the eccentricity of the earth's orbit and the precession of the equinoxes during many millions of years, and his book on the Philosophy of Theism, with his numerous published papers, were duly accepted (after some pressure against "red tape") as sufficient evidence of arithmetical capacity, and proof of ability to write good English. Thus the Director-General of the Survey secured the services of a highly-prized scientific and philosophic worker, in spite of the rigid rules of the Treasury and Civil Service Commission.

His researches on the causation of physical phenomena were continued in his spare time, chiefly in his walks, and evenings at home; and many other papers were published on the above-mentioned and other subjects, as Gravitation, Denudation, Interglacial periods, Glaciers, Gulf-stream, Oceanic Currents, Ice-sheets, Thickness of Sedimentary Rocks, Age of the Earth, Molecular Motion, Kinetic Energy, the Sun, Nebulæ, Climate and Cosmology, and Evolution. In 1875 the essence of his published notes and memoirs, and the results of his observations, had been embodied in the highly esteemed volume 'Climate and Time; A Theory of Secular Changes of the Earth's Climate.' This was written with great difficulty, for the cephalic pains greatly limited his opportunities of writing down his thoughts, and his health often failed.

In 1876 St. Andrew's University gave him the degree of LL.D., he became a Fellow of the Royal Society, Hon. Memb. New York Acad. Sci., of the Bristol Nat. Soc., Psychol. Soc. Gt. Brit., Glasgow Geol. Soc., Lit. Antiq. Soc. Perth, and the Perthshire Soc. Nat. Sci. The Geological Society of London awarded him the balance of proceeds of the Wollaston Fund in 1872, of the Murchison Fund in 1876, and of the Barlow-Jamieson Fund in 1884.

In 1880 Dr. Croll got a strain in the office, which, with his other painful affections, disabled him. On resigning his appointment he was, to his grief, not favoured with any more than a very meagre pension fixed for his thirteen years' service on the most stringent rules. Applications made to Government for a grant from the Civil List were without effect. After giving to the world a few more papers or memoirs on the physical subjects in which he had been so much interested, Dr. Croll published in 1885 in one volume, entitled 'Discussions on Climate and Cosmology,' 8vo, Edinburgh, the results of his labours since the publication of 'Climate and Time' in 1875.

With scrupulous and self-denying economy Dr. Croll utilized the proceeds of his published books in procuring an annuity, small as it was, for himself and his devoted and beloved wife. Some aid came from friends and from the Royal Society's special fund; and he settled quietly at Perth not far from the place of his first home. Suffering, but patient; clear-headed and desirous of imparting his knowledge, but unable to do so except by dictation to his always helpful wife, he remained long enough to see the printed sheets of his latest book.

He was always amiable, candid, consistent, and conscientious. The high respect with which he was regarded by all, and the warm affection and sympathy felt for him by his friends, were well known in his lifetime, and are now plainly shown by the voluminous, but valuable, correspondence and the obituary notices preserved in this Biography.

The titles of 92 of Dr. Croll's books and memoirs, from 1857 to 1890, are catalogued at pages 527–535. The following remarks are applicable to some of them, more particularly of 1857, 1864–75, and 1883–90.

"His first work, entitled 'The Philosophy of Theism,' published in 1851, at the age of thirty-six, endeavoured to define the relation of Theism to the determination of molecular motion. He tried to show that, for the production of any organism, two things are necessary,—first, motion; second, the determination of motion. Mere vital force might account for motion, but the determination of motion implies an idea, design, and a directing mind" (page 507).

"To Dr. Croll belongs the rare merit of showing that, though glacial cycles may not arise *directly* from cosmical causes, they may do so *indirectly*. His first contribution to the subject was published in 1864, but the development of his theory resulted in a series of brilliant researches extending over a period of eleven years, to 1875. He was led to investigate the problem of the eccentricity of the earth's orbit and its physical relations to the Glacial Period. By means of Leverrier's formulæ, he calculated tables of eccentricity for three million years in the past and one million years in the future, with the view of determining the periods of high eccentricity, which according to his theory were coincident with cycles of extreme cold. He was further led to consider the various physical agencies affecting climate, resulting from periods of high eccentricity, of which by far the most important is the deflexion of the ocean-currents" (see page 510). The cause of these was a subject of much discussion; and "these various lines of research are intimately associated with the fundamental question of the physical cause of climatic change."

"Dr. Croll's investigations into the geological history of terrestrial climate had led him to consider the question of the origin of the sun's heat, and thence to reflect on the possible condition and development of nebulæ and stars. The latter chapters of the" 'Discussions' &c. above mentioned "were devoted to these subjects, which he would fain have discussed more at length, had not the increasing failure of his bodily powers warned him that, if he wished still to return to that philosophy which was his first love, he must husband his remaining strength. Nevertheless the attraction of these astronomic problems proved insuperable. He continued to work at the subject, enlarging the scope of the investigation until it embraced not the earth and the sun merely, but the origin and development of the whole material universe. At last he followed his usual method,—gathered together his various contributions to the subject, trimmed, enlarged, and modified them, and published them in a separate volume, entitled 'Stellar Evolution in its Relation to Geological Time.' The publication of that work marks the close of his labours in more definite scientific inquiry.

He was now free with such remaining strength as he could command to re-enter the field of philosophic speculation, in which he had spent his earliest years of mental exertion, and which for nearly thirty years, through all the engrossing attractions of geological inquiry, had never lost its fascination for him. Accordingly he betook himself once more to the study of such subjects as force, matter, causation, determinism, evolution, &c., and proceeded to apply the facts and principles with which he had in the interval been dealing so actively to the problems in philosophy that had aroused his thoughts in the early years of his life. In spite of his increasing infirmity, he persevered in committing to writing the ideas which he had now formed, and he sent to press his last work, 'The Philosophical Basis of Evolution,'" in 1890. (See page 504.) He died December 15, 1890: aged 69 years.

XLIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 240.]

January 6th, 1897.—Dr. Henry Hicks, F.R.S., President, in the Chair.

THE following communications were read:—

1. 'On the Structure of the Skull of a Pliosaur.' By C. W. Andrews, Esq., B.Sc., F.G.S.

2. 'On the Pembroke Earthquakes of August 1892 and November 1893.' By Charles Davison, Sc.D., F.G.S.

Part I. of this paper deals with the earthquakes of August 1892, of which eleven are recorded, the principal being the third. The author gives an account of the preliminary shocks and after-shocks, and a detailed account of the principal earthquake, describing the disturbed area, the relationship of the earthquake to a north-and-south fault, hading to the west, that of sound to shock, and the occurrence of sea-waves.

Part II. treats of the earthquakes of November 1893; there were four undoubted earthquakes, of which the first was the chief one. Descriptions are given of the phenomena.

Part III. treats of the origin of the earthquakes and their connexion with faults; and the author points out the possible value of the study of earthquakes in supplementing geological surveys. For more than fifty years prior to the earthquakes of 1892–93 there appear to have been no slips of importance along the fault-system of the area. After this prolonged interval of repose, the earlier movements took place along transverse (north-and-south) faults, and the later along longitudinal (east-and-west) ones. The three faults of the latter series which the author connects with the disturbances lie successively one to the north of the other, as if the abrupt displacement of a rock-mass over one thrust-plane impelled the advance of those immediately below. There can be little doubt that the fault-slips of 1892 affected the conditions of stress along the neighbouring transverse fault, so that the displacements along it occurred earlier than they might otherwise have done.

3. 'Changes of Level in the Bermuda Islands.' By Prof. Ralph S. Tarr.

The author gives a summary of previous writings bearing upon the geology of the Bermudas; but his own researches point to a rather more complicated series of changes than those which have been inferred by other writers. The formation of the 'base-rock' or 'beach-rock' occurred at some period which cannot be accurately ascertained at present, owing to the fragmentary nature of the included fossils. It may have been formed in Pleistocene or even late Tertiary times. After its formation it was converted into a dense limestone and then eroded, probably by subaerial agents, and finally attacked by the waves at an elevation of at least 15 feet above present sea-level; during this stage it was covered by beach-deposits of pebbles and shells, which were accumulated in a period so recent that the contained fossils are of the same species as the organisms living in the neighbouring sea. Then followed an uplift, during which land-shells lived on the beach-deposits; but these were soon covered by blown sand—the principal accumulations of the islands, and the outline of the islands was perfected by the action of the winds. This was done at an elevation which was at one time certainly as much as 40 or 50 feet above present sea-level. The author adduces evidence of a depression since this accumulation, causing land to disappear and the outline of the area to become very irregular; and he proves that these changes cannot be accounted for solely by erosion, as some have maintained. There are indications that the land is at present quiescent. It appears, then, that most of the work of construction of the Bermudas has been done in recent times.

January 20th.—Dr. Henry Hicks, F.R.S., President, in the Chair.

The following communications were read:—

1. 'On Glacial Phenomena of Palæozoic Age in the Varanger Fiord.' By Aubrey Strahan, Esq., M.A., F.G.S.

The Gaisa beds of the Varanger Fiord consist of slightly altered quartz-grits, with red sandstones and shales, and rest upon a deeply denuded surface of the metamorphic rocks. In a section, first noticed by Dr. Reusch, a heterogeneous mixture of grit and clay with boulders of granitic and other rocks is seen to be intercalated between the quartz-grits, the bedding of the overlying grit proving that this boulder-rock was contemporaneously formed, and not subsequently wedged in. The surface of the grit below the rock is characteristically glaciated. Proof is given that the striated surface is not the floor of a thrust-plane, and that the boulder-rock is not a fault-breccia or a crush-conglomerate, but a 'till.' In the absence of fossils the Gaisa formation is doubtfully assigned to an early Palæozoic age. It exhibits the same sedimentary characters as the rocks of later date in other parts of the world in which glacial phenomena have been observed. The glacial episode is attributed to a temporary change of climate rather than to the high latitude in which the section lies.

2. 'The Raised Beaches and Glacial Deposits of the Varanger Fiord.' By Aubrey Strahan, Esq., M.A., F.G.S.

The Raised Beaches range up to nearly 300 feet above the sea. Though a number of impersistent shingle-banks occur at various heights, the highest is constant, and can be traced along the same level either as a shingly terrace or by a zone of wave-worn rocks. Evidence is furnished by the relative size of different parts of the beach that the prevalent wave-action was from the west, and by the greater abundance of erratics on or below the beach than above it, that floating ice was at work.

At the head of the fiord a blue clay dotted over with stones is now being formed, and the raised beach there consists of a similar material. Both here and elsewhere this clay simulates a Boulder Clay; but for reasons given it is believed to be a marine fiord-deposit, into which many stones have been dropped by floating ice.

Deposits of true glacial age, in the form of mounds of gravel, are described, and shown to have yielded the material out of which parts of the Raised Beaches are formed. The glaciation of the fiord is attributed to floating ice, and is shown to have taken place before the formation of the Raised Beaches, at a time when the sea surrounded this part of Finmark, by way of the Varanger Fiord, the Tana Valley, and the Tana Fiord.

February 3rd.—Dr. Henry Hicks, F.R.S., President, in the Chair.

The following communications were read:—

1. 'The Subgenera *Petalograptus* and *Cephalograptus*.' By Miss G. L. Elles.

2. 'On some Superficial Deposits in Cutch.' By the Rev. J. F. Blake, M.A., F.G.S.

The author arranges the deposits of which he treats under the following heads:—1. Subrecent Concrete; 2. Boulder Beds associated with the former; 3. Quartzite Reefs; 4. Infratrappean Grits; 5. Laterite; 6. Alluvium and Rănn.

1. The Subrecent Concrete consists of a calcareous, porous, laminated sand with milioline remains, which extends to a height of about 700 feet above the Rănn, and has a discontinuous distribution. The author gives reasons for regarding this as an æolian deposit, partly derived from recent marine accumulations and blown inland and uphill by the prevalent winds.

2. The Boulder Beds are next described, their distribution noted, and their occurrence with æolian deposits recorded. The author argues that the æolian deposits once had a greater slope, and acted as carriers, so that, under the influence of rain, the stones from the hills slipped to their present position.

3. The Quartzite Reefs are described as filling up cracks; the material is of æolian origin and derived from the surrounding rocks, and owing to this material having been formerly at a lower level than the latter, water stood on it for a sufficiently long time to permit of the materials being cemented by a siliceous deposit, and converted into quartzite.

4. The Infratrappean Grits are maintained to be superficial deposits on the pre-trappean land-surface, some being the ordinary results of weathering, others due to the washing down of débris to a water-covered level, and others again simply æolian drift. It is thus seen that there has been constancy in the meteorological conditions of Cutch from recent times as far back as the Cretaceous period.

5. The Laterite-deposits of the district occur to a height of only 120 feet above the Rănn. There is evidence that they were laid down in water at a time when the surface of the country was not very different from the present one. The material may have been partly derived from Jurassic rocks, but some of the constituents, as the eroded agates, must have come from the trap-rocks.

6. The Rănn is an area which has recently been abandoned by the sea, owing to unequal movements, but there is evidence that deposit has also taken place in it, and the depression has become shallower, so that in course of time the whole surface will be made of alluvial or æolian soil.

XLIV. *Intelligence and Miscellaneous Articles.*

GALVANOMETER DESIGN.

To the Editors of the Philosophical Magazine.

GENTLEMEN,

PERMIT me to acknowledge the courtesy and entire correctness of Professor Gray's recent letter (Phil. Mag. January 1897) respecting my note on Galvanometer Design (Phil. Mag. December 1895). Professor Gray perfectly apprehends my point of view in the matter, and very clearly indicates the nature of the omission which constitutes my error. That this was its nature I had already discovered in reflecting upon the note by Professors Ayrton and Mather (Phil. Mag. November 1896). It had been my intention to briefly point out the fact as soon as some additional experimental data could be obtained. But as this would probably cause some further delay, owing to my inability to participate in active work, I take the opportunity afforded by Professor Gray's remarks to recognize the correctness of the demonstration by Messrs. Ayrton, Mather, and Sumpner in their original paper. It may be well, however, to note that this conclusion does not vitiate the inferences which were the main point of my former paper, namely, the comparative inefficiency of the central portion of the coil, even if wound in reverse order, and the consequent importance of employing exceedingly short needles.

SILAS W. HOLMAN.

Boston, Mass., February 18, 1897.

ON MAGNETIC AFTER-ACTION. BY PROF. IGN. KLEMENÇIC.

The magnetic induction which is observed in soft iron wires which have been annealed for some time, and are placed in weak fields, is made up of two parts; a fact established by Ewing (Phil. Trans. 1885, p. 569, and Proc. Roy. Soc. 1889) and Lord Rayleigh (Phil. Mag. 1887). One part follows the production or cessation of magnetizing force; the second part begins after the termination of the former, and develops itself very slowly, so that the intensity of magnetization attains its final value only after some minutes. The phenomenon of a time retardation was denoted as "creeping" or viscous hysteresis; it is here called "magnetic after-action." It is very probable that the establishment of magnetic after-action in the middle of the wire proceeds more rapidly than at the ends.

Magnetic after-action occurs mostly in weak fields; it diminishes with the strength of the field, and the more rapidly the thinner the wire under investigation.

No regular connexion between the magnetic after-action and the thickness of the wire could be made out, probably owing to unequal annealing.

Strong magnetizations of the wires in no wise affect their magnetic after-action.

Magnetic after-action is a transitory phenomenon, which is only observed in freshly-annealed bars.—*Wiener Berichte*, March 1897.

MAGNETIC INFLUENCE ON LIGHT-FREQUENCY.

The footnote on page 232 (March no.) was by the communicator, not the author, of the paper; and he notes that the simplest way of putting the elementary theory, for an ion or electron revolving in an orbit of fixed size but any shape, is to write the radial magnetic force as

$$d(mr\omega^2) = e\,\mathrm{B}\omega,$$

whence

$$2\pi n = d\omega = e\mathrm{B}/2m,$$

n being the magnetically-caused change of frequency, and B the density of magnetic induction, or μH.—EDS.

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

[FIFTH SERIES.]

MAY 1897.

XLV. *On the Resolving Power of Telescopes and Spectroscopes for Lines of Finite Width.* *By* F. L. O. WADSWORTH [*].

THE question of the theoretical resolving power of optical instruments has been discussed by a number of writers—most fully and comprehensively by Rayleigh [†], who has shown that the theoretical angular resolving power of any instrument having an aperture of width b is $\alpha = m\frac{\lambda}{b}$, where α is the angle between two fine lines or points which can just be separated (two stars for example), λ is the mean wave-length of light, b the linear aperture of the instrument, and m a constant varying from unity for a rectangular aperture to about 1·1 for a circular aperture. It is possible to determine at once from this expression the spectral resolution or separation of a spectroscope, by remembering that the function of the dispersing train, which may consist either of prisms or of a transmission or reflection grating, is simply to form a series of spectral images of a single source,—the slit of the spectroscope. Of these images only those will be resolved or separated for which the difference in angular dispersion is equal to or exceeds the angular resolution α of the spectroscope aperture. In the case of the spectral images

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[*] Communicated by Lord Rayleigh, F.R.S.

[†] "Resolving or Separating Power of Optical Instruments," Phil. Mag. Oct. 1879, p. 261. "Resolving Power of Telescopes," Phil. Mag. Aug. 1880. "The Manufacture and Theory of Diffraction Gratings," Phil. Mag. 1874, p. 5. Also articles on Optics, vol. xvii., and Wave Theory, vol. xxiv. *Enc. Brit.*

of a slit this resolving power is in general less than the theoretical resolving power for infinitely narrow lines: (1) because of the finite angular width of the slit; (2) because of the dispersion of the spectroscope train, which for radiations which are not monochromatic produces the same effect as a widening of the slit. Theoretically we shall distinguish between four cases:—

1. The resolving power (theoretical) of a spectroscope train for an infinitely narrow slit and monochromatic radiations, *i. e.*, infinitely narrow spectral lines. This is the quantity usually denoted by r.

2. The resolving power (also theoretical) for a wide slit and monochromatic radiations. Usually denoted by p, the "purity" of the spectrum.

3. The resolving power (limiting) for an infinitely narrow slit, but for lines of finite width $\Delta\lambda$. This quantity we will denote by R.

4. The resolving power (practical) for a wide slit and non-monochromatic radiations ranging for each line over a small value $\Delta\lambda$ as in (3). This quantity, which represents the practical resolving power or purity of the spectrum, will be denoted by P.

Let $\mathrm{D}=\frac{d\theta}{d\lambda}$ be the angular dispersion of the spectroscope train. The spectroscopic resolution for any case is defined by the ratio $\frac{\lambda}{d\lambda}$, where $d\lambda$ is the difference in wave-length of two lines of mean wave-length, λ, that are just resolved. Therefore for the first case

$$\frac{d\theta}{d\lambda}(d\lambda)_1=\alpha=m\frac{\lambda}{b}$$

or

$$r=\frac{\mathrm{D}b}{m}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

a perfectly general relation which holds good whatever may be the nature, form, or arrangement of the spectroscope train. Introducing the values of $\mathrm{D}=\frac{d\theta}{d\lambda}=\frac{d\theta}{dn}\cdot\frac{dn}{d\lambda}$, we obtain at once the usual expressions

$$\left.\begin{aligned} r&=2\mathrm{N}b\frac{\sin\frac{\phi}{2}}{\sqrt{1-n^2\sin^2\frac{\phi}{2}}}\cdot\frac{dn}{d\lambda} \\ &=(t_1-t_2)\frac{dn}{d\lambda} \end{aligned}\right\} \quad . \quad . \quad . \quad (2)$$

for a train of N prisms of refracting angle ϕ through which the rays pass at minimum deviation, and

$$r=\frac{mn}{b'' \cos\theta}\cdot b=mn \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

for a grating of n lines.

In the case of the grating the expression for the resolving power may be put into a form which will bring out more clearly one fact that is not generally emphasized in the theory of the gratings, *i. e.*, that for a given position of the grating, the resolving power is independent of the number of lines n and is determined, as in any optical instrument, simply by the linear aperture b. This proposition may be very simply proved from the fundamental equation of the diffraction-grating,

$$m\lambda=s(\sin i+\sin\theta).$$

Multiplying both sides by n we get

$$mn\lambda=ns(\sin i+\sin\theta). \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

But $mn=r$ and $ns=b$ the linear aperture of the grating. Hence

$$r=\frac{b}{\lambda}(\sin i+\sin\theta), \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

an expression which is independent of n and depends only on b and on the position into which the grating is turned.

The maximum value of r is that for which $i=\theta=90°$. Then we have

$$r_{\text{max.}}=2\frac{b}{\lambda},$$

which shows that the resolving power of a grating is an expression of the same form as the corresponding expression for a microscope, telescope, and reflecting mirror. The maximum resolving power is the same (though expressed in different units) as that for a mirror of the same horizontal aperture.

This theoretical maximum, however, can never be realized, because for large angles of incidence and diffraction the angular aperture of the grating becomes very small, and the light consequently excessively faint. In practice the angle of incidence i never exceeds 60° for an angle of diffraction $\theta=0$, nor more than 45°–50° when the angles of incidence and diffraction are equal (Littrow type). Hence maximum practical resolving power, which we will call r_0, varies from

$$r_0=\frac{7}{8}\frac{b}{\lambda} \text{ to } r_0=\frac{3}{2}\frac{b}{\lambda}.$$

If we take the higher limit we find that the limit of resolving power of the best and largest gratings now in use (ruled surface $5\frac{1}{2}$ inches) is for the middle part of the spectrum ($\lambda = {\cdot}00055$ mm.) about 375000 units, just sufficient to "resolve" a double line whose components are about ·016 tenth-metres apart. The view at one time held that higher resolving powers than this were unnecessary because of the discontinuities in a train of light-waves is now known to be erroneous. Michelson's recent work has shown that some of the spectral lines which appear single in the most powerful spectroscopes yet constructed, are in reality very complex, consisting of three, four, or even more components whose distance apart in some cases is probably not much more than 0·006 tenth-metres. To resolve these by means of a grating, we need, therefore, instruments having at least three times the aperture of those now in use. Were the interferometer or wave-comparer universally applicable in spectroscopic analysis, there would be little occasion to attempt to rival its performance by gratings, but it is unfortunately only applicable to the more intense of the *bright* lines of a spectrum. For the more detailed study of faint lines, and absorption-lines, gratings of larger resolving power than have yet been constructed would seem to be the first essential. The mechanical difficulties to be overcome are very formidable. The chief difficulty does not seem to lie in the production of a screw of sufficient accuracy, since by Rowland's method we are enabled to produce a screw of the required length in which the errors of run, periodicity, &c. are less than those unavoidably introduced by eccentricities in the mountings and divided head*; but in avoiding the errors of spacing caused by unequal wear of the ways on which the ruling-point carriage moves, and in maintaining sufficiently constant temperature conditions during the ruling.

How great these difficulties actually are may be better appreciated when it is remembered that to rule a 15-inch grating (of 20000 lines per inch) the ruling-engine would have to run continuously for nearly two weeks (a 6-inch grating requires five days and nights), that in such a grating a displacement of one five-hundred-thousandth part of an inch in the position of the lines in any part of the grating would greatly impair the definition and resolution in any order higher than the second, and that such a change would be brought about by the smallest amount of unequal wear, or even by a slight change in thickness of the film of oil on one of the ways of the ruling-carriage, or by an unsymmetrical

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* See Rowland's article on the Screw, *Enc. Brit.* vol. xxi.

change of temperature of the grating or of parts of the ruling-engine of less than $\frac{1}{5}$° C. But the immense value that such large gratings would have in rendering possible a more detailed study of the complex character of spectral lines, and a more exact determination of their wave-length under varying conditions of production, would seem sufficient to amply justify any expenditure of time and money necessary to make their production possible and practicable*.

It is worth while remarking that the independence of resolving power of the fineness of ruling, already pointed out, makes it possible to considerably reduce the time and difficulties of ruling large gratings by very considerably increasing the grating space, provided only that ruling-points can be found (by trial) which will produce gratings sufficiently bright in the higher orders. The two objections usually urged to coarse-ruled gratings are the increased overlapping of the different orders of spectra, and the increased accuracy of spacing required. I have recently shown how the first objection may be overcome by a very simple and efficient optical device placed in front of the spectroscope slit †. The second objection is not a valid one. It has been shown (Rayleigh) that in a given grating the allowable error in the spacing s is $\frac{1}{4}s$ in the first order, $\frac{1}{8}s$ in the second, $\frac{1}{12}s$ in the third, or in general $\frac{1}{4m}s$. But for a given resolving power, *i.e.*, for a given aperture and given position of grating, we have from (4) $\frac{m}{s}=$constant, or for two gratings of the same aperture but of different spacing, s and s_1,

$$\frac{m}{s}=\frac{m_1}{s_1}.$$

The limiting absolute error of ruling is therefore the same in both fine and coarse-ruled gratings. If, for example, the absolute error of spacing of the ruling-machine is $\frac{1}{100,000}$ inch, equally good definition would be obtained by ruling the grating with 20,000 lines to the inch, and using the first order spectrum only, or by ruling it 4000 lines to the inch, and using the fifth spectrum. But the last grating would

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* The writer has just finished the design of a large ruling-engine, the money for the construction of which has been given by a friend of science in Chicago. Work on it has been begun in the instrument-shop of the Observatory, and every possible precaution will be taken to ensure success.

† The 'Astrophysical Journal,' March 1896, vol. iii. p. 169.

require only $\frac{1}{5}$ the time for ruling, and hence in general would be only $\frac{1}{5}$ as difficult to make as the first one. The question of the relative brightness of the spectra in the two gratings would be, as already stated, almost entirely a question of the selection of a ruling-point.

Let us now consider the resolving power of a spectroscope for wide slits (width s) and monochromatic radiations. The formula ordinarily given for this is

$$p=\frac{\lambda}{s\psi+\lambda}r. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

This is based on the assumption that for distinct resolution of wide lines, the angular distance between the contiguous edges of the two lines must be equal to the resolving power of the aperture through which they are viewed. According to this assumption the angular distance between the centres of the two lines of width s, which would be just resolved, would have to be

$$\frac{1}{f'}\left(s\frac{f'}{f}+m\frac{\lambda}{b}f'\right)=\left(\frac{s}{f}+m\frac{\lambda}{b}\right)=\frac{1}{b}\,(s\psi+m\lambda), \quad . \quad (7)$$

where ψ is the angular magnitude of the aperture b as viewed from the line s, f is the distance of the line itself from the lens, and f' the focal length of the observing telescope. But I have recently found that it can be shown, both by theory and by experiment, that this assumption is incorrect, and that the resolving power of an instrument for wide lines is considerably greater than is indicated by the above expression. As this point has apparently escaped notice heretofore it may be considered a little in detail.

The diffraction-pattern due to a line of width s, or angular width $\sigma=\frac{s}{f}$, is found by integrating the effect due to each linear element over the whole width of the line. In the case of a rectangular aperture the diffraction-pattern due to each linear element is represented, as is well-known, by the equation

$$\mathrm{I}=\mathrm{C}\frac{\sin^2\frac{\pi}{\alpha}\phi}{\left(\frac{\pi}{\alpha}\phi\right)^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

ϕ being the angular distance from the centre of the diffraction-image. The intensity at any point γ due to the effect of all of the elements of a line of uniform brightness will therefore be

$$\mathrm{I}'=\mathrm{C}\int_{-\frac{\sigma}{2}}^{\frac{\sigma}{2}}\frac{\sin^2\frac{\pi}{\alpha}(\gamma-\phi)}{\left[\frac{\pi}{\alpha}(\gamma-\phi)\right]^2}\quad.\ .\ .\ .\ .\quad(9)$$

$$=\mathrm{C}'\int_{\frac{\pi}{\alpha}\left(\gamma-\frac{\sigma}{2}\right)}^{\frac{\pi}{\alpha}\left(\gamma+\frac{\sigma}{2}\right)}\frac{\sin^2\chi}{\chi^2}\,d\chi=f(\gamma,\sigma).\quad.\ (10)$$

The value of the definite integral (10) cannot be found directly in terms of γ and σ, but it can easily be evaluated by mechanical quadrature for different values of these variables. For $\sigma=\alpha$, which is about as small a value as is ever used in practice, the values of $\mathrm{I}'=f(\gamma)$ are given in Table I. For the sake of comparison the values of I (from 8) are also included.

TABLE I.

γ.	$\mathrm{I}'=f(\gamma)$.	I.	γ.	$\mathrm{I}'=f(\gamma)$.	I.
·0	1·00	1·00	·8	·24	·055
·2	·92	·87	1·0	·11	·000
·4	·71	·57	1·2	·044	·024
·6	·45	·25	1·5	·030	·045

The diffraction-curves represented by these values are plotted in fig. 1 (p. 324). Like I, the curve I′ does not fall off regularly, but passes through a series of maxima and minima whose position is given by the general equation*

$$\gamma\tan\frac{\pi\gamma}{\alpha}=\frac{\sigma}{2}\tan\frac{\pi\sigma}{2\alpha}\text{ for }2m<\frac{\sigma}{\alpha}<2m+1,$$

$$\frac{1}{\gamma}\tan\frac{\pi\gamma}{\alpha}=\frac{2}{\sigma}\tan\frac{\pi\sigma}{2\alpha}\text{ for }2m-1<\frac{\sigma}{\alpha}<2m.$$

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* This part of the problem, *i. e.*, that of locating the position of the minima in the diffraction-patterns of a slit and of a circular aperture of finite width, was worked out by the writer (at the suggestion of Professor Michelson) about six years ago, while a student at Clark University. The results were published in Professor Michelson's paper on "Application of Interference Methods to Astronomical Measurements" (Phil. Mag. July 1890, p. 1, see pp. 14–17).

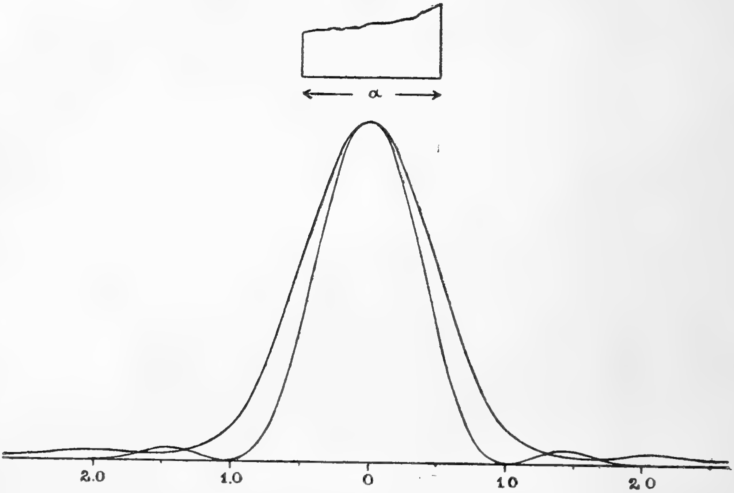
In this case $\sigma=\alpha$, and we have therefore

$$\tan\frac{\pi\gamma}{\alpha}=\infty,$$

or $$\gamma=1{\cdot}5\alpha,\ 2{\cdot}5\alpha,\ 3{\cdot}5\alpha,\ \&c.,$$

or the minima occur at points $\frac{1}{2}\alpha$ further from the centre than when the source is a line of negligible width*.

Fig. 1.



The integral was evaluated in the same manner for different values of σ both smaller and larger up to $\sigma=3\alpha$. The diffraction-patterns of two sources of width $\sigma=\alpha$ and $\sigma=3\alpha$ are shown in fig. 2, in dotted lines.

In order that a double line may be resolved it is necessary

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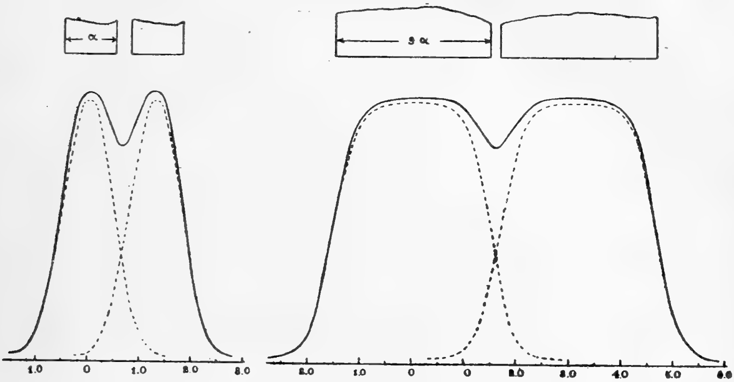
* Since the position of the minima in this case depends on the angular magnitude of the source σ as well as on the aperture of the telescope, it follows that by covering the objective of the latter by a rectangular opening of known size, and then measuring by means of a micrometer the positions of the minima of the diffraction-pattern, the value of σ may be determined from the above equations. Experiments on a large number of slits of varying width and holes of varying diameters (for which the positions of the minima are slightly different) showed that when the source was sufficiently bright to give well-marked minima, single observations gave results which were at least five times as accurate as could be obtained by direct micrometric measurement of the image with full aperture of the telescope. This method is, however, considerably less accurate than the refractometer method of Professor Michelson which is fully described in the earlier part of the paper referred to, and the observations are therefore not given at length.

that the intensity at the centre of the diffraction-pattern of the double source (shown in full lines in fig. 2) should be

Fig. 2.



about 0·8, the intensity at the maxima corresponding to the centres of the two geometrical images. In order that this may be the case the distance between these centres in the three cases $\sigma=\alpha$, $\sigma=2\alpha$, and $\sigma=3\alpha$ must be for

$\sigma=\alpha$, angular distance between centres $=1{\cdot}27\alpha=\sigma+0{\cdot}27\alpha$,
$\sigma=2\alpha$, „ „ „ „ $=2{\cdot}21\alpha=\sigma+0{\cdot}21\alpha$,
$\sigma=3\alpha$, „ „ „ „ $=3{\cdot}20\alpha=\sigma+0{\cdot}20\alpha$,
or in general „ „ „ $=\Sigma=\sigma+\delta$.

From these and intermediate values the curve in fig. 3 (p. 326), which represents the relation between the angular width of the lines and the angular distance δ between the contiguous edges necessary for distinct resolution, was plotted.

In order to test these results experimentally a fine black wire was stretched across the centre of an ordinary double motion slit, thus forming two parallel slits whose widths could be simultaneously varied (by opening the slit), while the distance between the contiguous edges (which was equal to the diameter of the wire) remained constant. The two slits were uniformly illuminated by the light of the sun or an electric arc passing through a screen of white paper, and were viewed by a telescope over whose objective was placed a rectangular opening of width b.

The slit was set at various measured widths, and the distance of the telescope from it varied until the two halves of the slit were just resolved. If D represents the distance of the telescope from the slit, d the diameter of the wire, and S the whole length of the slit, we have evidently

$$\frac{\sigma}{\alpha} = \frac{\mathrm{S}-d}{2\mathrm{D}} \cdot \frac{b}{\lambda}$$

and

$$\frac{\delta}{\alpha} = \frac{d}{\mathrm{D}} \cdot \frac{b}{\lambda}.$$

As a check, a few measurements were also taken with the telescope at a fixed distance from the slit, the aperture b being varied until the two elements were just resolved. The effect of varying the brightness of the slits by interposing additional screens and by removing them altogether was also tried, as well as the effect of varying the magnifying power of the telescope. As long as the images were bright enough to be clearly seen there was no appreciable effect produced by either of these changes. The results are presented in the following table :—

Fig. 3.

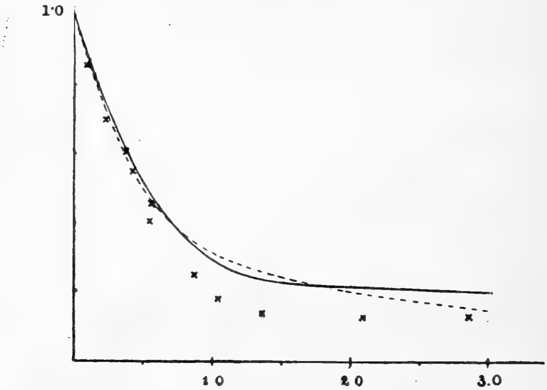


TABLE II.

S.	D.	d.	b.	σ/a.	δ/a.		
1·97	12760	0·045	20·5	2·8	·13	Sunlight, screened.	
1·46	12760	,,	,,	2·1	·13	,,	,,
0·97	12760	,,	,,	1·35	·13	,,	,,
0·60	9900	,,	,,	1·04	·17	,,	,,
0·36	6700	,,	,,	·84	·25	,,	screened and unscreened.
0·167	4210	,,	,,	·55	·40	,,	vibration very bad.
0·150	3280	,,	,,	·58	·51	,,	unscreened, cloudy.
0·100	2770	,,	,,	·37	·60	,,	,, ,,
0·075	2370	,,	,,	·23	·70	,,	,, ,,
0·250	11000	0·20	26·0	·11	·86	Arc light, screened.	
0·500	11000	0·20	17·0	·42	·56	,, ,, ,,	

These results are plotted (crosses) in fig. 3. The agreement with the first part of the curve obtained by theory is very good, but beyond the point $\sigma=a$ the experimental values are considerably less than the theoretical ones. These last were obtained, it will be remembered, on the assumption that in order to obtain resolution the difference in intensity between the centre and edges of the diffraction-pattern of a double source must be at least 20 per cent. These results indicate that when the lines are broad a falling off in intensity at the centre of considerably less than this is noticeable. Indeed this is what we should expect, since we know that on an extended bright background (such as a planetary surface) faint markings may be distinguished where the variation in intensity from the background itself is not more than from two to five per cent.

We are therefore certainly on the safe side in following the curve deduced from theory. The value of Σ, the angular resolution of the telescope for the wide lines, is, moreover, practically the same whichever curve be followed, because, for the values of σ for which the two curves begin to depart to any extent from each other, the value of δ is small compared to σ itself*.

The theoretical curve of fig. 3 may, up to the point $\sigma=3\alpha$, be closely represented by the hyperbola of the form

$$\frac{\sigma}{\alpha}+\frac{1}{2}=\frac{1}{2\left(\frac{\delta}{\alpha}\right)}, \quad \ldots\ldots \quad (11)$$

whence we get

$$\delta=\frac{\alpha^2}{2\sigma+\alpha}. \quad \ldots\ldots\ldots \quad (12)$$

But $\sigma=\frac{s}{f}$ and $\alpha=\frac{\lambda}{b}$ (for rectangular aperture $m=1$). Substituting these values we get

$$\delta=\frac{1}{b}\frac{\lambda^2}{2s\psi+\lambda}. \quad \ldots\ldots \quad (13)$$

The angular distance between two lines of width σ which can just be resolved is then

$$\Sigma=\sigma+\delta=\frac{1}{b}\left(s\psi+\frac{\lambda}{2s\psi+\lambda}\lambda\right). \quad \ldots \quad (14)$$

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* For the value of $\sigma=1{\cdot}5a$, for which the difference between the two curves is greatest, the two values of Σ differ by only about four per cent. For $\sigma=3a$ the difference in Σ is only about two per cent.

An examination of this result develops the interesting fact that the aperture required to separate the components of a double line is *less when the lines have a small finite width than when they are infinitely narrow.* For, as may be easily proved, the expression for Σ becomes a maximum when

$$s\psi=\frac{\lambda}{2(1+\sqrt{2})}\simeq\tfrac{1}{5}\lambda.$$

Thus for a line of angular width $\sigma=\frac{1}{5}\lambda$ we have

$$\Sigma=\cdot 91\frac{\lambda}{b}=\cdot 91\alpha,$$

or, what amounts to the same thing, a telescope of given aperture has 10 per cent. greater resolving power for lines of width $\frac{1}{5}\alpha$ than for lines infinitely narrow.

To find the width of line for which the resolving power of the instrument is the same as the theoretical resolving power we put

$$s\psi+\frac{\lambda^2}{2s\psi+\lambda}=\lambda,$$

which gives at once

$$s\psi=0,\quad\text{or }\tfrac{1}{2}\lambda,$$

or it is just as easy to resolve the components of a double line when these have a width equal to one-half the angular resolution of the telescope as when their width is zero. This increased resolving power resulting from increasing the width of the lines from 0 up to $\frac{1}{5}\alpha$ is due to the same effect as is produced by stopping out the central portion of the telescope objective, *i.e.*, by a strengthening of the centre of the resulting diffraction-pattern relative to the edges.

For the spectroscopic resolution we have, as in the first case,

$$\frac{d\theta}{d\lambda}(d\lambda)_2=\Sigma=\frac{1}{b}\left(s\psi+\frac{\lambda}{2s\psi+\lambda}\lambda\right),\quad . \quad . \quad (15)$$

or

$$\frac{\lambda}{(d\lambda)_2}=p=\frac{\lambda}{s\psi+\frac{\lambda}{2s\psi+\lambda}\lambda}r,\quad . \quad . \quad . \quad . \quad (16)$$

which differs from the expression ordinarily given for the purity of a spectrum by the presence of the factor $\frac{\lambda}{2s\psi+\lambda}$ as a coefficient of the second term of the denominator. The existence of this factor necessitates a considerable modification of certain statements based on the old formula for purity. Instead of diminishing continuously with increased slit-width, the purity of the spectrum first actually increases up to the point

$$s\psi = \tfrac{1}{5}\lambda,$$

and is still equal to the theoretical resolving power of the instrument when $s\psi = \frac{1}{2}\lambda$ [*]. As the slit is widened still further, the purity begins to diminish, although much less rapidly than is indicated by the old formula for purity. In his remarks on the practical purity of a bright line-spectrum in the article "Spectroscopy" (*Enc. Brit.* vol. xxii. p. 374), Schuster says:—"The maximum illumination for any line is obtained when the angular width of the slit is equal to the angle subtended by one wave-length at a distance equal to the collimator aperture. In that case $s\psi = \lambda$ and the purity is half the resolving power. Hence when light is a consideration we shall not as a rule realize more than half the resolving power of the spectroscope." Equation (16) shows, however, that under this condition for maximum illumination [†] the purity is really 75 per cent. of the theoretical resolving power instead of 50 per cent. as indicated by Schuster. A similar erroneous conclusion (based upon the commonly accepted formula for purity) was drawn by the writer in one of his earlier papers [‡], in which it was stated that the purity in case of stellar spectra could never exceed one-third the theoretical resolving power (unless the slit-width is made less than the diameter of the diffraction-image of the star). Equation (16) shows us that this limit should be nearly one-half instead of one-third.

Third Case.—If the radiation is not monochromatic, but is made up of wave-lengths ranging over a interval from λ to $\lambda + \Delta\lambda$, the dispersion of the spectroscope train will spread out the image of an infinitely narrow slit into a band in which the distribution of intensity (supposing the dispersion over the small range $\Delta\lambda$ to be strictly proportional to λ) will be the same as in the source of radiation. This image will be further broadened by diffraction, and the distribution of intensity in the image formed by the spectroscope objective

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[*] Unfortunately it is not generally possible to profit by this fact, because for such narrow slits the spectrum is in most cases too faint to be well seen.

[†] As is readily seen, this condition holds only for absolutely monochromatic sources of radiation (see 'Astrophysical Journal,' January 1895, pp. 62, 63).

[‡] 'Astrophysical Journal,' January 1895, pp. 68, 69.

will be given by an expression similar to (9), but containing a term $f(\phi)$ which represents the distribution of intensity in the source of radiation.

The law of distribution (in a normal source) is not yet definitely known. The one ordinarily assumed is that which follows from Maxwell's kinetic theory, which is *

$$f(\phi)=e^{-\kappa\phi^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (17)$$

where κ is a constant whose value varies with the substance emitting radiation, and with the temperature and pressure in the source. A law of distribution more recently proposed by Michelson is †

$$f(\phi)=e\frac{\sin^2 r\phi}{\phi^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (18)$$

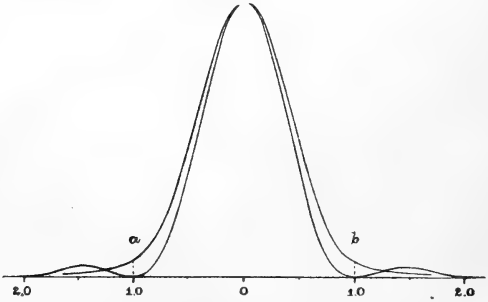
If the first law is assumed, we have for the intensity in the diffraction-pattern

$$\mathrm{I}_1=\int_{-\infty}^{+\infty} e^{-\kappa\phi^2}\frac{\sin^2\frac{\pi}{\alpha}(\gamma-\phi)}{\left\{\frac{\pi}{\alpha}(\gamma-\phi)\right\}^2}d\phi=\psi_1(\kappa,\gamma,\alpha,); \quad . \qquad (19)$$

and if the second,

$$\mathrm{I}_2=\int_{-\infty}^{+\infty}\frac{\sin^2 r\phi\sin^2\frac{\pi}{\alpha}(\gamma-\phi)}{\phi^2\left\{\frac{\pi}{\alpha}(\gamma-\phi)\right\}^2}d\phi=\psi_2(r,\gamma,\alpha,). \quad . \qquad (20)$$

Fig. 4.



I have not succeeded in integrating either of these integrals

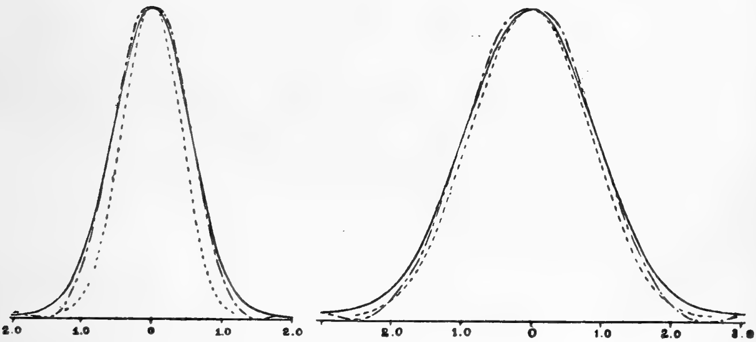
---

* See Rayleigh, Phil. Mag. April, 1889, p. 298; also Michelson, Phil. Mag. September 1892.

† 'Astrophysical Journal,' Nov. 1895, p. 251.

in finite form. They may be integrated by developing into a series, but I have found it easier and quicker to integrate by mechanical quadrature. Owing to the very close correspondence between the curves represented by (17) and (18) (see fig. 4), the result will be practically the same whichever law be adopted. The expression for I_1 is the one which has actually been integrated, and the resulting curves $\psi_1(\kappa, \gamma, \alpha,)$ for two values of κ are given in fig. 5. The dotted lines represent the curves $f(\phi)$ and the full lines the resulting diffraction-pattern $\psi_1(\gamma)$.

Fig. 5.



For convenience the values of κ are expressed in terms of the "half-width" of the line (Michelson) and α the limiting resolving power of the spectroscope objective. The "half-width" δ is defined to be the value of ϕ for which $f\phi=\frac{1}{2}$. Hence

$$\kappa=\frac{\text{Nap.}\log 2}{\delta^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (21)$$

What we may call the effective width of the line w is the width *ab* (fig. 4), which is equal to 4δ. At the points *a* and *b* the intensity $f(\phi)$ is only about one-twentieth the intensity at the centre, and the part of the curve beyond this point may therefore be considered as having but little effect either on the eye or on the photographic plate.

The values of w in the curves of fig. 5 are $w=2\alpha$, $w=4\alpha$.

In fig. 6 *a* the diffraction-curve for a double source, of which each component is of width $w=2\alpha$, is shown. Adopting the same rule as before, *i. e.* that for resolution the intensity at the middle of the diffraction-pattern must not be more than 0·8 the intensity at the two maxima on each side, we find that for resolution the distance between the components in different cases must be

$$
\begin{array}{ll}
w=\alpha, & \text{dist.}=1{\cdot}12\alpha=\Omega_1,\\
w=2\alpha, & \quad ,, \;=1{\cdot}45\alpha=\Omega_2,\\
w=3\alpha, & \quad ,, \;=1{\cdot}90\alpha=\Omega_3,\\
w=4\alpha, & \quad ,, \;=2{\cdot}45\alpha=\Omega_4.
\end{array}
$$

For lines so wide that the broadening by diffraction can be entirely neglected we find (fig. 6 *b*) that the distance between the components necessary for resolution is

$$2{\cdot}3\delta=0{\cdot}575w\simeq\tfrac{4}{7}w.$$

Fig. 6 *a*.

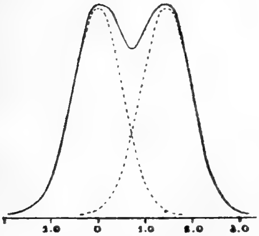
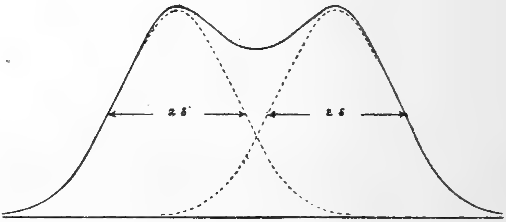


Fig. 6 *b*.



Expressing the preceding results in the form

$$\Omega=\tfrac{4}{7}w+f(w)\alpha,$$

we have

$$
\begin{array}{lll}
\text{for } w=0, & f(w)=1{\cdot}00, & \Omega=\alpha=\dfrac{\lambda}{b},\\[2ex]
\text{for } w=\alpha, & f(w)=0{\cdot}55, & \Omega=\dfrac{4}{7}w+0{\cdot}53\dfrac{\lambda}{b},\\[2ex]
\text{for } w=2\alpha, & f(w)=0{\cdot}31, & \Omega=\dfrac{4}{7}w+0{\cdot}31\dfrac{\lambda}{b},\\[2ex]
\text{for } w=3\alpha, & f(w)=0{\cdot}18, & \Omega=\dfrac{4}{7}w+0{\cdot}18\dfrac{\lambda}{b},\\[2ex]
\text{for } w=4\alpha, & f(w)=0{\cdot}15, & \Omega=\dfrac{4}{7}w+0{\cdot}15\dfrac{\lambda}{b},\\[2ex]
\text{for } w=\infty\,\alpha, & f(w)=0{\cdot}00, & \Omega=\dfrac{4}{7}w+0{\cdot}00.
\end{array}
$$

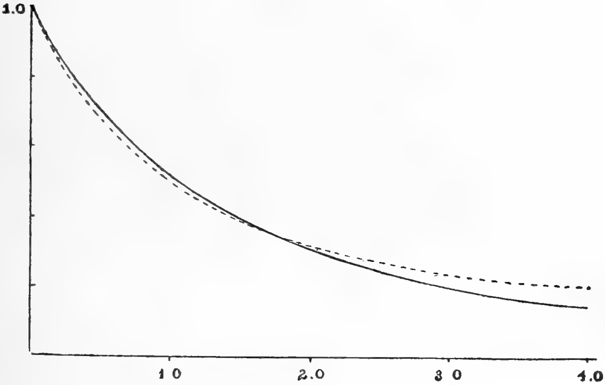
The coefficients $f(w)$ of the last term are plotted in fig. 7 as a function of w. The first portion of this curve may, as in the case of fig. 3, be closely represented by an empirical hyperbola (dotted curve) whose equation is

$$\frac{w}{\alpha}+1=\frac{1}{f(w)} \text{ or } f(w)=\frac{\alpha}{w+\alpha}, \quad . \quad . \quad . \quad . \quad (22)$$

whence

$$\Omega=\frac{4}{7}w+\frac{\alpha^2}{w+\alpha}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

Fig. 7.



The angular width, w, of the line, since this is produced by the dispersion of the spectroscopic train, is

$$w=\mathrm{D}\Delta\lambda=\frac{r}{b}\Delta\lambda, \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

$$\therefore \qquad \Omega=\frac{1}{b}\left(\frac{4}{7}r\Delta\lambda+\frac{\lambda}{r\Delta\lambda+\lambda}\lambda\right); \quad . \quad . \quad . \quad (25)$$

and therefore for the spectroscope resolution

$$\frac{d\theta}{d\lambda}(d\lambda)_3=\Omega,$$

and

$$\mathrm{R}=\frac{\lambda}{(d\lambda)_3}=\frac{\lambda}{\frac{4}{7}r\Delta\lambda+\frac{\lambda}{r\Delta\lambda+\lambda}\lambda}r, \quad . \quad . \quad . \quad (26)$$

a formula very similar in form to that derived for the purity P in the case of a wide slit and monochromatic radiations.

Since the spectrum lines must always have a certain "width," the expression for R last deduced, which for convenience we will call the limiting resolving power, is more generally useful in determining the greatest resolving power of a spectroscope under practical conditions than the usual expression for r (the theoretical resolution of the instrument). For very small values of $r\Delta\lambda$, *i. e.* for very small resolving powers or very narrow lines, the value of R will, as in the case of p, slightly exceed r. But for large values of either r or $\Delta\lambda$ the limiting resolving power will be very much less than the theoretical power of the instrument, particularly for large values of r. No matter how narrow the line may be there is a limit beyond which an increase in the theoretical resolving power is without effect in increasing R. This maximum value of R will evidently be

$$R_{\text{max.}} = \frac{\lambda}{\frac{4}{7}\Delta\lambda} = 1{\cdot}75\,\frac{\lambda}{\Delta\lambda},$$

or the maximum resolving power that can be attained with any instrument with infinitely narrow slit is not more than one and three-quarter times the ratio between the mean wave-length and "width" of the spectral lines under examination *.

Our knowledge of the width of spectral lines under different conditions is at present very limited. Various hypotheses, of which the most noted are those of Lommel, Jauman, Galitzin, and Michelson, have been advanced to account for the broadening of the lines under varying conditions of temperature and pressure, and to give us a numerical measure of the amount, but they are all more or less unsatisfactory. Michelson's recent experimental work with the interferometer has given us our most definite knowledge of the widths of some few bright lines in the spark-spectra of some of the metals under different pressures. In each case the exponential law of distribution is assumed, and the quantity given is δ, the "half-width" which has already been defined. It has been assumed as before that the effective range of wave-length $\Delta\lambda$ is about 4δ.

Table III. contains a brief summary of some of the results obtained.

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* As will be presently seen, however, we may attain a somewhat greater practical purity P than this.

TABLE III.

Substance.	Line λ.	Character of Source.	Pressure in mm.	δ (tenth-metres).	Δλ. 4δ.
Hydrogen	Hα* 6565	Vacuum tube.	Very low.	·047	Δλ′=0·328*
	” ”	”	50	·098	Δλ′=0·532*
	” ”	”	100	·134	Δλ′=0·696*
	” ”	”	200	·230	Δλ′=1·06*
Sodium	D_1* 5890	Vacuum tube.	Very low.	·005	0·020
	Not stated.	Not stated.	100	·09	0·36†
	”	”	200	·16	0·64†
	D_1* 5890	Bunsen flame.	Atmospheric.	About ·05 ‡	0·27*
Cadmium	Red 6439	Vacuum tube, temp. about 280°.	Very low.	·0065	0·026
	Green 5086	” ” ”	”	·0050	0·020
	Not stated.	Not stated, probably spark.	100	·05	0·200†
	”	” ”	200	·08	0·32†
	”	” ”	400	·14	0·56
Mercury	Green 5461	Vacuum tube, temp. about 100°.	Very low.	·003 ‡	0·12

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* The red hydrogen line is a double, the distance between the components being about 0·14 tenth-metre. The value given for δ is for each component, and the total effective width of the double line is therefore $\Delta\lambda' = 4\delta + 0{\cdot}14$. The same is true of each of the D lines (according to Michelson each is made up of at least four components), the distance between the centres of the principal components being 0·07. When the density is low, these components are therefore separated by much more than their own width; but when it is high (as in the Bunsen flame) each component broadens and overlaps the other, so that the total effective width is, as in the case of the H_α line, $\Delta\lambda' = 4\delta + 0{\cdot}07$.

† There would seem to be some discrepancy between these results, which are given in the Astrophysical Journal for November 1895, p. 251, and the results previously obtained with the vacuum tube (Phil. Mag. September 1892, p. 280).

‡ Calculated from data given in Phil. Mag. September 1892, p. 280.

For convenience the values of R, $R_{max.}$ and r/R, have been computed for various values of $\Delta\lambda$, ranging from 0·01 to 1·0 tenth-metre, and for values of r from 25000 to 1000000. They are given in Table IV.

The vertical columns show the decrease in the value of R with an increase in $\Delta\lambda$ for a given value of r; the horizontal lines show the increase in R with r for a given width of line. The last column gives the maximum resolving power $R_{max.}$ that can be attained when the lines have the width $\Delta\lambda$ given in the first column.

We see that in general we shall very nearly reach this limit when the theoretical resolving power r is about twice $R_{max.}$ The additional gain in R, obtained by a further increase in r, would not be worth the expense of the larger instruments required and the sacrifice in brightness necessary. Indeed, in most cases it would hardly be advisable to use a value of r greater than one to one and one-half times $R_{max.}$ as with this we shall have already attained from $\frac{3}{4}$ to $\frac{7}{8}$ of the limiting resolving power. The finest lines so far found (see Table IV.) have a width $\Delta\lambda$ of not less than 0·01 tenth-metre. For this width the value of $R_{max.}$ is 950000, and the maximum theoretical power which it would be advisable to use would therefore be about 1400000, corresponding in the case of a grating to an aperture of from 18 to 20 inches. On the other hand, for some of the wider lines, such as those of hydrogen in the vacuum tube, and of many bright metallic lines in arc spectra, there would be no advantage whatever for visual work in using a resolving power greater than 20000 to 25000, for which a grating of $\frac{1}{2}$-inch aperture, or 5 60-prisms of $1\frac{1}{4}$ inches aperture would suffice. For solar spectrum work, in which the lines are not likely to be narrower than $\frac{1}{20}$ tenth-metre*, our present 5 and 6 inch gratings will do nearly all that we could hope to attain with larger apertures†, unless indeed there should be some marked advantage in particular cases in the use of the first and second orders of spectra, rather than the higher orders.

The preceding conclusions are all based on the assump-

---

* In the case of faint lines the apparent width may sometimes be much less than this, because of the rapid falling off in intensity towards the edge of the line. Indeed, for faint lines, it is not likely that the apparent width of the line is greater than 2δ, and in some cases even less. Hence estimates of pressure based upon direct visual observations of the widening o lines may be considerably in error.

† The latter would, however, be advantageous in photographic work in giving increased accuracy and increased photographic resolution by reason of the greater linear dispersion. See 'Astrophysical Journal,' vol. i. p. 233, and vol. ii. p. 264.

TABLE IV.

$\lambda = 5500$ tenth-metres.

Δλ, tenth-metres.	$r=25000$.		$r=50000$.		$r=100000$.		$r=200000$.		$r=500000$.		$r=1000000$.		R_{max}.
	r/R.	R.	r/R.	R.	r/R.	R.	r/R.	R.	r/R.	R.	r/R.	R.	
0·01	0·98	25400	0·97	51600	0·95	105600	0·94	212800	1·04	480000	1·39	722000	962000
0·02	0·97	25800	0·95	52800	0·94	106400	1·00	200000	1·39	361000	2·29	437000	481000
0·04	0·95	26400	0·94	53200	1·00	100000	1·24	161700	2·29	219000	4·27	234000	240000
0·06	0·94	26600	0·96	52400	1·10	90900	1·56	128500	3·27	153000	6·30	159000	160000
0·08	0·94	26600	1·00	50000	1·24	80800	1·91	104600	4·27	117000	8·35	120000	120000
0·10	0·95	26400	1·04	48000	1·39	71900	2·29	87300	5·28	95000	10·41	96000	96000
0·12	0·96	26200	1·10	45500	1·56	64300	2·67	75000	6·30	79400	12·50	80000	80000
0·14	0·97	25800	1·16	42900	1·73	57700	3·06	65000	7·33	68000	14·50	69000	69000
0·16	1·00	25000	1·24	40400	1·91	52300	3·46	58000	8·35	60000	16·60	60000	60000
0·18	1·02	24600	1·31	38100	2·10	47700	3·86	52000	9·38	53000	18·70	53000	53000
0·20	1·04	24000	1·39	36000	2·29	43700	4·27	46800	10·41	48000	20·75	48000	48000
0·25	1·12	22400	1·60	31200	2·77	36100	5·28	37900	13·00	38000	25·9	38000	38000
0·30	1·20	20800	1·85	27000	3·27	30600	6·30	31800	15·60	32000	31·1	32000	32000
0·35	1·29	19300	2·05	24400	3·76	26600	7·33	27000	18·17	27000	36·3	27000	27000
0·40	1·39	18000	2·29	21800	4·27	23400	8·35	24000	20·75	24000	41·4	24000	24000
0·50	1·60	15600	2·77	18000	5·28	18900	10·41	19000	25·90	19000	51·8	19000	19000
0·60	1·82	13700	3·27	15300	6·30	15900	12·47	16000	31·1	16000	62·2	16000	16000
0·80	2·29	10900	4·27	11700	8·35	12000	16·61	12000	41·4	12000	82·9	12000	12000
1·00	2·77	9000	5·28	9500	10·41	9600	20·75	9600	51·8	9600	103·6	9600	9600

tion that the maximum practical resolving power r_0, which has been assumed to be equal to $1{\cdot}5\lambda/b$, and which corresponds to an angle of deviation of about 90° ($\theta=i=45°$ to $50°$), can be utilized. When for any reason this is not the case, whether because of the inaccuracies of ruling, the faintness of the higher orders of spectra, or the character of the mounting, a correspondingly larger aperture must be made use of. If, for example, we consider the maximum angle of deflexion to be 60° (which from purely mechanical considerations is about the largest possible angle that can be used in the ordinary Rowland mounting), we have for r_0

$$r_0 \simeq \frac{7}{8}\frac{b}{\lambda}.$$

In order to attain the same resolving powers, R, as before, the apertures must be increased about 75 per cent. If we assume a maximum angle of 45°, which in practice is not often exceeded in our present gratings, the apertures would have to be increased by over 100 per cent., and we should therefore need to attain the full limiting resolving power $R_{\max}$.

For lines $\Delta\lambda={\cdot}01$ tenth-metre, an aperture b of at least 1 metre
" $\Delta\lambda={\cdot}02$ " " b " 50 cm.
" $\Delta\lambda={\cdot}05$ " (solar work) " b " 25 cm.

Fourth Case.—In order to determine the limit of resolution or the practical purity P in this, the most important case, we must first determine the diffraction-curve resulting from a superposition of all the elements of the slit, each one of which has a dispersion-pattern similar to those represented in full lines in fig. 5. If, as before, these elements are equal in intensity, *i.e.*, if the illumination over the whole width of the slit is uniform, the intensity-curve of the diffraction-image will be

$$I_{//}=\int_{-\sigma/2}^{+\sigma/2}\psi_1(\xi-\gamma,\, w,\, \alpha)\,d\xi=\psi_{//}(\sigma,\, \gamma,\, w,\, \alpha), \quad . \quad (28)$$

where

$$\psi_1(\gamma,\, w,\, \alpha)=\int_{-\infty}^{+\infty} 2^{-\left(\frac{2\phi}{w}\right)^2}\,\frac{\sin^2\frac{\pi}{\alpha}(\gamma-\phi)}{\left[\frac{\pi}{a}(\gamma-\phi)\right]^2}\,d\phi, \quad . \quad (29)$$

as derived from (19) and (21).

Since the function ϕ_1 is not known in finite terms, $\psi_{//}$ cannot be directly found. We may, however, approximate very closely indeed to it by replacing the function $\psi_{//}$ by the function

$$\frac{\sin^2\frac{\pi}{\Omega}\gamma}{\left[\frac{\pi}{\Omega}\gamma\right]^2}, \quad \ldots \ldots \quad (30)$$

which between the points $\gamma=\frac{1}{3}\Omega$ and $\gamma=\frac{2}{3}\Omega$, or over all that part of the curve which is important in determining the resolution of a double line, coincides, as seen in fig. 5 (dashed curve), almost exactly with the curve $\psi_1(\kappa, \gamma, \alpha)$.

The expression for $I_{//}$ then becomes

$$I_{//}=\int_{-\sigma/2}^{+\sigma/2}\frac{\sin^2\frac{\pi}{\Omega}(\xi-\gamma)}{\frac{\pi}{\Omega}(\xi-\gamma)}\,d\xi, \quad \ldots \quad (31)$$

which is exactly similar in form to (9), the only difference being that α has been replaced by Ω.

We may therefore obtain at once the limit of resolution for this case from (12) and (14) by replacing α by Ω, giving us

$$\Sigma_1=\text{limiting angular resolution}=\sigma+\frac{\Omega^2}{2\sigma+\Omega}. \quad (32)$$

Replacing σ and Ω by their values in terms of s, ψ, R, r, and λ and reducing, we finally obtain for Σ_1,

$$\Sigma_1=\frac{1}{b}\left[s\psi+\frac{\left(\lambda\frac{r}{R}\right)^2}{2s\psi+\lambda\frac{r}{R}}\right], \quad \ldots \ldots \quad (33)$$

and for purity

$$P=\frac{\lambda}{(d\lambda)_4}=\frac{\lambda}{s\psi+\frac{\lambda\left(\frac{r}{R}\right)}{2s\psi+\lambda\frac{r}{R}}\lambda\frac{r}{R}}r. \quad \ldots \quad (34)$$

This expression differs from (16) only in the presence of the factor $\frac{r}{R}$ as a coefficient of λ in the denominator. When this ratio is unity $P=p$, or the practical purity is equal to the theoretical purity for monochromatic radiations.

By an inspection of Table IV. it will be seen that while for narrow lines and small resolving powers the ratio $\frac{r}{R}$ is very nearly unity, and that formula (16) therefore represents very closely the purity of the spectrum, the same is by no means true for wide lines and large resolving powers. In the extreme case figured in the table the value of this ratio rises as high as 100. In order to show more clearly the influence of this factor on the purity of the spectrum under different conditions, Table V. has been prepared, showing the values of P for different slit apertures, from 0·005 mm. to 0·3 mm., different widths of lines from 0·01 to 1·00 tenth-metres, and resolving powers varying from 25000 to 1000000. For comparison the values of p are given for each slit-width and resolving power, and also the value of p' calculated from the old formula for purity (6). An inspection of the table shows at once how greatly in error estimates of purity based upon this old formula may be in some very common cases.

Take for example the case of a spectroscope having a resolving power of 100000 (5-inch grating, 20000 lines, 2nd order); working with angular slit-width such that $s\psi = \cdot 005$ ($s = \frac{1}{50}$ mm., $\psi = \frac{1}{40}$, as in the concave grating). The value of p (16) is about 158000, while the value of P varies from 163000 to 10000. The value of p' (the old formula for purity) for the same case is only 105000. It is therefore in this case from 50 per cent. to 1000 per cent. in error. In case of larger resolving-powers ($r = 1000000$) it may be as much as 60 times too great. In general, of course, the large values of $r\Delta\lambda$ that give rise to the smaller values of P will not be used for visual work, as there is, as already indicated, but little gain in practical resolving power or purity when the value of r is greater than the value of $R_{\text{max.}}$ given in Table V. But in photographic work it is, as has already been shown in a previous paper, a great advantage to use (for extended sources) a short camera and very high resolving power, in order to attain a given degree of photographic purity. Another point which is of considerable practical importance in this connexion is that for these large values of $\frac{r}{R}$ the purity of the spectrum may be maintained constant or even actually improved over a wide range of those slit-widths actually used in practice. For the maximum value of P (as of p) will be attained when

$$s\psi \simeq \tfrac{1}{5}\lambda\left(\frac{r}{R}\right).$$

For $r=200000$, $\Delta\lambda=1{\cdot}00$, $\frac{r}{\mathrm{R}}=20{\cdot}75$, and the maximum value of P is therefore attained when the value of $s\psi$ is about $4{\cdot}15\,\lambda$ or about $\cdot 0023$, corresponding for the usual spectroscope ($\psi=\frac{1}{15}$) to a slit-width of about $\frac{1}{30}$ mm. Under the same circumstances the practical purity is still as great when the slit-width is $\frac{1}{13}$ mm. as when it is zero. For still higher resolving powers the maximum allowable widths of slit are still greater. Even with such low values of $\frac{r}{\mathrm{R}}$ as 2 or 3 (corresponding to lines as fine as those sometimes found in the solar chromosphere, *i. e.*, 0·2 to 0·25 tenth-metre), and resolving powers of only 100000, the purity remains undiminished up to values of $s\psi=\lambda$ to $1\frac{1}{2}\lambda$ ($\cdot 0005$ to $\cdot 0008$), or to slit-widths (with the concave grating) of from $\frac{1}{50}$ mm. to $\frac{1}{30}$ mm.

One further case remains to be considered, viz. that of a wide slit and non-monochromatic radiations in which the slit-image is not uniformly brought across the whole width. The expression for the intensity in the diffraction-pattern then becomes

$$\mathrm{I}_{\prime\prime\prime}=\int_{-\sigma/2}^{+\sigma/2} f(\xi)\psi_1(\xi-\gamma, w, \alpha)\,d\xi, \quad . \quad . \quad (35)$$

where $f(\xi)$ expresses the intensity at any part of the slit at a distance ξ from its centre. The only case of importance of this kind is the case of stars. If the star-image is perfect, *i. e.* unaffected by atmospheric or instrumental aberration, the distribution in intensity for any one wave-length is represented by the law

$$\frac{\sin^2\frac{\pi}{\alpha_0}\xi}{\left(\frac{\pi}{\alpha_0}\xi\right)^2},$$

α_0 being the resolving power of the telescope-lens which forms an image of the star.

As before, the integration could only be effected by mechanical quadrature or by development into a series (ψ_1 not being known in finite terms). It has not been thought worth while to go through the necessary labour of integration for the reason that, practically, such conditions are never realized, at least in stellar spectrographic work. There might be moments at which, if the star were kept perfectly centred on the slit, the full resolving power resulting from

TABLE V.

S.	ψ radians.	Sψ.		w 48	r 25000	r 50000	r 100000	r 200000	r 500000	r 1000000
				·01	20000	40200	81200	163200	389000	662000
·005	$\frac{1}{10}$			·05	20300	40600	77800	132400	194000	207000
			P	·10	20300	38900	66200	91400	103400	102200
·010	$\frac{1}{20}$	·0005		·50	15100	19400	20700	20400	19900	19900
				1·00	9700	10300	10200	10000	9800	9700
·020	$\frac{1}{40}$			p (from 16)	19800	39600	79100	158200	396000	792000
				p' (from 6)	13100	26200	52400	104800	262000	524000
				·01	12400	24800	49700	99400	243000	454000
				·05	12400	24800	48700	90900	166000	202000
·010	$\frac{1}{10}$		P	·10	12400	24400	45500	74300	101500	108000
		0 001		·50	10900	16600	20200	21500	20400	19900
·015	$\frac{1}{15}$			1·00	8300	10100	10800	10300	10000	9800
				p (from 16)	12300	24600	49100	98200	245000	491000
				p' (from 6)	8900	17800	35500	71000	177000	355000
				·01	6700	13400	26700	53500	133000	259000
				·05	6700	13400	26600	51900	113600	171000
·020	$\frac{1}{10}$		P	·10	6700	13300	25900	47800	85700	103000
		0·002		·50	6400	11400	17100	20600	21000	20600
·030	$\frac{1}{15}$			1·00	5700	8500	10300	10600	10300	10000
				p (from 16)	6650	13350	26700	53400	133500	267000
				p' (from 6)	5400	10800	21600	43200	108000	216000
				·01	4500	91000	18100	36200	90200	178000
				·05	4500	91000	18000	35600	83100	141000
·030	$\frac{1}{10}$		P	·10	4500	9000	17800	34200	75700	95300
		0·003		·50	4400	8300	14100	19100	21100	20800
·045	$\frac{1}{15}$			1·00	4200	7600	9500	10500	10400	10100
				p (from 16)	4500	9100	18100	36200	90500	181000
				p' (from 6)	3900	7800	15500	31000	77500	155000
				·01	2700	5400	10900	21800	54500	108000
				·05	2700	5400	10900	21800	52900	97300
·050	$\frac{1}{10}$		P	·10	2700	5400	10800	21400	48800	77100
		0·005		·50	2700	5300	9700	15400	20600	21100
·075	$\frac{1}{15}$			1·00	2600	4900	7700	9900	10500	10300
				p (from 16)	2700	5400	10900	21800	54500	109000
				p' (from 6)	2500	5000	9900	19800	49500	99000
				·01	1400	2800	5500	11000	27500	54900
				·05	1400	2800	5500	11000	27200	53000
·10	$\frac{1}{10}$		P	·10	1400	2800	5500	10900	26500	48800
		0·010		·50	1400	2700	5300	9800	17300	20600
·15	$\frac{1}{15}$			1·00	1400	2600	4900	7800	10300	10500
				p (from 16)	1400	2800	5500	11000	27500	55000
				p' (from 6)	1300	2600	5200	10400	26000	52000
				·01	700	1400	2800	5600	14000	27500
				·05	700	1400	2800	5500	13700	27200
·20	$\frac{1}{10}$		P	·10	700	1400	2800	5500	13600	26500
		0·020		·50	700	1400	2700	5300	11600	17300
·30	$\frac{1}{15}$			1·00	700	1400	2600	4900	8600	10300
				p (from 16)	700	1400	2800	5600	14000	28000
				p' (from 6)	650	1300	2600	5200	13000	26000

the superposition of two such diffraction-patterns as are represented by (35) might be realized, but in general the star-image will be so broadened and disturbed in position by continual atmospheric disturbance (to say nothing of chromatic aberration in the case of the image being formed with a lens), that the effect on the photographic plate will in the long run be practically the same as would be produced by a uniformly illuminated slit.

Yerkes Observatory,
University of Chicago,
February 1897.

XLVI. *On the Measurement of Alternate Currents by means of an obliquely situated Galvanometer Needle, with a Method of determining the Angle of Lag.* By LORD RAYLEIGH, *F.R.S.*[*]

IT is many years[†] since, as the result of some experiments upon induction, I proposed a soft iron needle for use with alternate currents in place of the permanently magnetized steel needle ordinarily employed in the galvanometer for the measurement of steady currents. An instrument of this kind designed for telephonic currents has since been constructed by Giltay; but, so far as I am aware, no application has been made of it to measurements upon a large scale, although the principle of alternately reversed magnetism is the foundation of several successful commercial instruments.

The theory of the behaviour of an elongated needle is sufficiently simple, so long as it can be assumed that the magnetism is made up of two parts, one of which is constant and the other proportional to the magnetizing force. If internal induced currents can be neglected, this assumption may be regarded as legitimate so long as the forces are small[‡]. In the ordinary case of alternate currents, where upon the whole there is no transfer of electricity in either direction, the constant part of the magnetism has no effect; while the variable part gives rise to a deflecting couple proportional on the one hand to the mean value of the square of the magnetizing force or current, and upon the other to the sine of twice the angle between the direction of the force and the

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[*] Communicated by the Author.

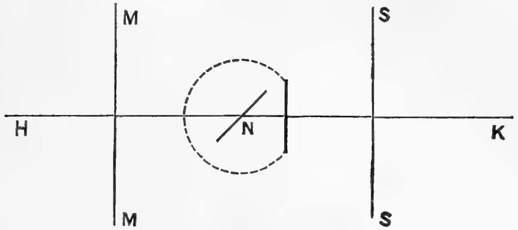
[†] Brit. Assoc. Report, 1868; Phil. Mag. vol. iii. p. 43 (1877).

[‡] Phil. Mag. vol. xxiii. p. 225 (1887).

length of the needle. The deflecting couple is thus evanescent when the needle stands either parallel or perpendicular to the magnetizing force, and rises to a maximum at the angle of 45°. For practical purposes the law of proportionality to the mean square of current would seem to be trustworthy so long as no great change occurs in the frequency or type of current; otherwise eddy currents in the iron might lead to error, unless the metal were finely subdivided.

It is hardly to be supposed that for ordinary purposes a suspended iron needle would compete in convenience with the excellent instruments now generally available; but having found it suitable for a special purpose of my own, I think it may be worth while to draw to it the attention of those interested. In experiments upon the oxidation of nitrogen by the electric arc or flame it was desired to ascertain the relation between the electric power absorbed and the amount of nitrogen oxidized. A transformer with an unclosed magnetic circuit was employed to raise the potential from that of the supply to the 3000 volts or more needed at the platinum terminals. Commercial ampere-meters and volt-meters gave with all needed precision the current and potential at the primary of the transformer; but, as is well known, these data do not suffice for an estimate of power. The latter depends also upon the angle of lag, or retardation of current relatively to potential-difference. If this angle be θ, the power actually employed is to be found by multiplying the product of volts and amperes by $\cos\theta$, so that the actual power may be less to any extent than the apparent power represented by the simple product. Various watt-meters have been introduced for measuring the actual power directly, but I could not hear of one suitable for the large current of 40 amperes used at the Royal Institution. Working subsequently in the country I returned to the problem, and succeeded in determining the angle of lag very easily by means of the principle now to be explained.

The soft iron needle of 2 centim. in length, suspended by a fine torsion-fibre of glass and carrying a mirror in the usual way, is inclined at 45° to the direction of the magnetic force. This force is due to currents in *two* coils, the common axis of the coils being horizontal and passing through the centre of the needle. As in ordinary galvanometers, the mean plane of each coil may include the centre of the needle; but it was found better to dispose the coils on opposite sides and at distances from the needle which could be varied. A plan of the arrangement is sketched diagramatically in the woodcut, where MM, SS represent the two coils, the common axis HK passing through the centre of the needle N. If the currents in the coils are of the same frequency and of simple type, the magnetizing forces along HK may be denoted by A cos nt,



B cos $(nt-\epsilon)$, ϵ being the phase-difference. If either force act alone, the deflecting couple is represented by A^2 or by B^2; but if the two forces cooperate the corresponding effect is

$$C^2 = A^2 + B^2 + 2AB\cos\epsilon, \quad . \quad . \quad . \quad . \quad (1)$$

reducing itself to $(A+B)^2$ or $(A-B)^2$ only in the cases where ϵ is zero or two right angles. The method consists in measuring upon any common scale all the three quantities A^2, B^2, and C^2, from which ϵ can be deduced by trigonometrical tables, or more simply in many cases by constructing the triangle whose sides are A, B, and C. The determination of the phase-difference between the currents is thus independent of any measurement of their absolute values.

The best method of estimating the deflecting couples may depend upon the circumstances of the particular case. The most accurate in principle is the restoration of the needle to the zero position by means of a torsion-head. But when the conditions are so arranged that the angular deflexions are moderate, it will usually suffice merely to read them, either objectively by a spot of light thrown upon a scale, or by means of a telescope. In any case where it may be desired to push the deflexions beyond the region where the law of proportionality can be relied upon, all risk of error may be avoided by comparison with another instrument of trustworthy calibration, one coil only of the soft iron apparatus being employed.

In certain cases the advantages which accompany the restoration of the zero position of the needle may be secured by causing the deflexions themselves to assume a constant value, *e. g.* by making known changes of resistance in one or both of the circuits, or by motion of the coils altering their efficiencies in a known ratio.

In the particular experiments for which the apparatus was set up the coil MM (see woodcut) was reduced to a single turn of about 17 centim. diameter and conveyed the main current (about 10 amperes) which traversed the primary circuit of the transformer. This, it may be mentioned, was a home-made instrument, somewhat of the Ruhmkorff type, and was placed at a sufficient distance from the measuring apparatus. The shunt-coil SS was of somewhat less diameter and contained 32 convolutions. The shunt-circuit included also two electric lamps, joined in series, and its terminals were connected with two points of the main circuit outside the apparatus, where the difference of potentials was about 40 volts. Provision was made for diverting the main current at pleasure from MM, and by means of a reverser the direction of the current in SS could be altered, equivalent to a change of ϵ by 180°. The measurements to be made are the effects of MM and of SS acting separately, and of MM and SS acting together in one or both positions of the reverser.

The best arrangement of the details of observation will depend somewhat upon the particular value of ϵ to be dealt with. If this be 60°, or thereabouts, the method can be applied with peculiar advantage. For by preliminary adjustment of the coils, if movable, or by inclusion of (unknown) resistance in the shunt-circuit, the deflexions due to MM and SS may be made equal to one another; so that in the case supposed the same deflexion will ensue from the simultaneous action of the two currents in one of the ways in which they may be combined.

This condition of things was somewhat approached in the actual measures relating to the electric flame. Thus in one trial the coils were adjusted so as to make the deflexions due to each of the currents acting singly equal to one another. The value was 40 divisions of the scale. When both currents were turned on, the deflexion was 26½ divisions. Thus

$$A^2=B^2=40, \qquad A^2+B^2-2AB\cos\epsilon=26\tfrac{1}{2};$$

whence

$$\cos\epsilon=\cdot 67, \qquad \text{or} \quad \epsilon=48^\circ.$$

In a second experiment the deflexion due to both currents acting together was made equal to that of the main acting alone. Here

$$A^2=40, \qquad B^2=71, \qquad A^2+B^2-2AB\cos\epsilon=40;$$

whence

$$\cos\epsilon=\cdot 665.$$

The accuracy was limited by the unsteadiness of the electric flame and of the primary currents (from a gas-driven De

Méritens) rather than by want of delicacy in the measuring apparatus.

When the phase-difference is about a quarter of a period, $\cos\epsilon$ is small, and its value is best found by observing the effect of reversing the shunt-current while the main current continues running. The difference is $4\mathrm{AB}\cos\epsilon$, from which, combined with a knowledge of A and B, the value of $\cos\epsilon$ is advantageously derived. If $\cos\epsilon$ is absolutely zero, the reversal does not alter the reading.

If the currents are in the same, or in opposite phases, it is possible to reduce the joint effect to zero by suitable adjustment of the coils or of the shunt resistance.

The application of principal interest is when the shunt-current may be assumed to have the same phase as the potential-difference at its terminals, for then $\cos\epsilon$ is the factor by which the true watts may be derived from the apparent watts. We will presently consider the question of the negligibility of the self-induction of the shunt-current, but before proceeding to this it may be well to show the application of the formulæ when the currents deviate from the sine type.

If a be the instantaneous current, and v the instantaneous potential-difference at the terminals, the work done is $\int av\,dt$. The readings of the soft iron galvanometer for either current alone may be represented by

$$\mathrm{A}^2=h^2\int a^2dt, \qquad \mathrm{B}^2=k^2\int v^2dt, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where h, k are constants depending upon the disposition of the apparatus. When both currents act, we have the readings

$$\mathrm{C}_1^2 \text{ or } \mathrm{C}_2^2=\int(ha\pm kv)^2dt. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Taking the first alternative, we find

$$\mathrm{C}_1^2=h^2\int a^2dt+2hk\int av\,dt+k^2\int v^2dt,$$

or

$$\frac{\mathrm{C}_1^2-\mathrm{A}^2-\mathrm{B}^2}{2\mathrm{AB}}=\frac{\int av\,dt}{\left\{\int a^2dt\times\int v^2dt\right\}^{\frac{1}{2}}}. \quad . \quad . \quad . \quad . \quad (4)$$

The fraction on the right of (4) is the ratio of true and apparent watts; and we see that, whether the currents follow the sine law or not, the ratio is given by $\cos\epsilon$, where, as before, ϵ is the angle of the triangle constructed with sides proportional to the square roots of the three readings.

Another formula for $\cos\epsilon$ is

$$\cos\epsilon = \frac{C_1^2 - C_2^2}{4AB} \quad . \; . \; . \; . \; . \; . \; . \; . \quad (5)$$

In the final formula (4) the factors of efficiency of the separate coils (h, k) do not enter. This result depends, however, upon the fulfilment of the condition of parallelism between the two coils. If the magnetic forces due to the coils be inclined at different angles χ, χ' to the length of the needle, we have in place of (3),

$$\begin{aligned} C^2 &= \int (a\cos\chi + v\cos\chi')(a\sin\chi + v\sin\chi')\,dt \\ &= \int [\tfrac{1}{2}a^2\sin 2\chi + \tfrac{1}{2}v^2\sin 2\chi' + av\sin(\chi+\chi')]\,dt \; ; \quad . \quad (6) \end{aligned}$$

while

$$A^2 = \tfrac{1}{2}\sin 2\chi \int a^2 dt, \qquad B^2 = \tfrac{1}{2}\sin 2\chi' \int v^2 dt. \; . \; . \quad (7)$$

Accordingly

$$\frac{\int av\,dt}{\{\int a^2 dt \times \int v^2 dt\}} = \frac{C^2 - A^2 - B^2}{2AB}\,\frac{\sqrt{\{\sin 2\chi \,.\, \sin 2\chi'\}}}{\sin(\chi+\chi')}, \; . \quad (8)$$

in which the second fraction on the right represents the influence of the defect in parallelism. If χ and χ' are both nearly equal to 45°, then approximately

$$\frac{\sqrt{\{\sin 2\chi \,.\, \sin 2\chi'\}}}{\sin(\chi+\chi')} = 1 - \tfrac{1}{2}(\chi-\chi')^2. \quad . \; . \; . \; . \quad (9)$$

We have now to consider under what conditions the shunt-current may be assumed to be proportional to the instantaneous value of the potential-difference at its terminals. The obstacles are principally the self-induction of the shunt-coil itself, and the mutual induction between it and the coil which conveys the main current. As to the former, we know* that if the mean radius of a coil be a, and if the section be circular of radius c, and if n be the number of convolutions,

$$L = 4\pi n^2 a \left\{ \log\frac{8a}{c} - \frac{7}{4} \right\}. \quad . \; . \; . \; . \; . \quad (10)$$

To take an example from the shunt-coil used in the experiments above referred to, where

$$a = 6 \text{ cm.}, \quad c = 1 \text{ cm.}, \quad n = 32,$$

L is of the order 10^5 cm. The time-constant of the shunt-circuit (τ) is equal to L/R, where R is the resistance in C.G.S.

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* Maxwell's 'Electricity,' § 706.

measure. If r be the resistance measured in ohms, $R=r\times 10^9$, so that

$$\tau=\frac{1}{r\times 10^4}.$$

What we are concerned with is the ratio of τ to the period of the currents; if the latter be $\frac{1}{100}$ second, the ratio is $1/100r$, so that if r be a good number of ohms—it must have exceeded 100 in the particular experiments—there is nothing to fear from self-induction. It would seem to follow generally that if the voltage be not too small, say not falling below 10 volts, there should be no difficulty in obtaining sufficient effect from a shunt-coil whose self-induction may be neglected. It may be remarked that since the efficiency of the coil varies as n, while L varies as n^2, it will be advantageous to keep n (and r) down so long as the self-induction of the whole shunt-circuit is mainly that of the coil.

If the main and the shunt-coils were wound closely together, the disturbance due to mutual induction would be of the same order of magnitude as that due to self-induction. If the coils are separated, as is otherwise convenient, the influence of mutual induction will be less, and may be neglected under the conditions above defined.

As to the effect of self-induction, if present, we know that the lag ϕ is given by

$$\tan\phi=Lp/R, \quad \ldots\ldots\ldots \quad (11)$$

where $p=2\pi\times$ frequency. The angle of lag of the main current (θ), which it is the object of the measurements to determine, is then given by

$$\theta=\epsilon+\phi, \quad \ldots\ldots\ldots \quad (12)$$

ϵ being the phase-difference of the two currents as found directly from the observations.

XLVII. *The Temperature and Ohmic Resistance of Gases during the Oscillatory Electric Discharge.* By JOHN TROWBRIDGE *and* THEODORE WM. RICHARDS[*].

IN our papers[†] on "The Spectra of Argon and The Multiple Spectra of Gases" we have emphasized the importance of considering the electrical condition of the circuit in which is placed the Plücker tube containing the gas under examination. We have pointed anew to the fact that in general the continuous discharge of an accumulator produces one spectrum, while

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[*] Communicated by the Authors.

[†] Phil. Mag. vol. xliii. pp. 77, 135.

the oscillatory discharge of a condenser produces another. In considering this question one is immediately struck by the fact that, although the gas acts as if it presented a resistance of several hundred thousand or even several million ohms to the current, while under the influence of the continuous discharge, nevertheless this same tube allows oscillations which are wholly damped by a few hundred ohms to pass through it under the influence of a condenser. These considerations led us to measure the resistance of such a tube to the oscillatory discharge, and we found by means of a novel method that in fact *a mass of gas at low tension contained in a capillary tube may act as though it opposed a resistance of only five or six ohms to the spark of a large condenser.*

In order the more clearly to grasp the situation, the potential-differences between the ends of the tube during a continuous discharge may well be considered first. A number of measurements of such potential-differences have been made by Hittorf * and others, but it may be well to give two of the many series of measurements which we have made, in order to facilitate comparison with the discharge of the condenser through the same tubes. The tubes employed throughout this research were of the ordinary type devised by Plücker, consisting of two cylindrical bulbs separated by a capillary 1·3 mm. in diameter and 7 cm. long. The electrodes were of aluminium. Unless otherwise stated all the experiments here recorded were made with tubes of exactly this shape and size ; and most of the experiments were made with a single tube. The voltmeter used for measuring the potential-differences was a Thomson electrostatic electrometer, and the current used was not much over a milliampere.

As the voltmeter was only graduated to 1800 volts, the readings above that amount are merely approximations.

Each gas evidently has its minimum of potential-difference, that of hydrogen lying at about 1 mm. of pressure, and that of nitrogen at about 0·3 mm. These minima, as well as the total potential-differences, are undoubtedly modified by the strength of the current; but the results given above are comparable with one another because they were all made under the same conditions. Hittorf found a minimum at about 0·35 mm. for nitrogen, and he pointed out by means of his extra electrodes that the fall of potential was very irregular, the greater part of it residing at the kathode. His results have been confirmed by others, and Wood † has shown that the heat evolved at different parts of the tube follows the same irregularities as these potential-differences.

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* Wied. *Ann.* xx. p. 705.

† *Ibid.* lix. p. 238.

Potential-differences between Electrodes of Spectrum tube.

Hydrogen.		Nitrogen.	
Pressure in mm.	Voltage.	Pressure in mm.	Voltage.
7·0	2600 (?)	8·5	very high.
6·0	2100 (?)	5·0	very high.
4·0	1900	4·0	2600 (?)
3·5	1500	3·0	2100 (?)
2·0	1340	2·5	1750
1·5	1260	1·7	1600
1·25	1220	1·4	1410
1·15	1150	1·2	1340
1·00	1100	1·0	1180
·70	1140	0·7	1140
·50	1220	0·6	1080
·13	very high.	0·5	1040
		0·3	980
		0·25	1030
		0·13	1700
		0·06	2800+ (?)

Neglecting the factors of the potential-difference which reside at the electrodes, the sum of which increase with the exhaustion of the tube, we find that according to Hittorf's results the resistance of the gas itself steadily diminishes as the exhaustion proceeds; for example, with a current of two milliamperes he found a fall of potential of about 120 volts between two parts of the middle of the tube eight centimetres apart, the tension of the nitrogen being 0·35 mm. When the current was about one milliampere, and the tension of the gas was only about 0·001 mm., the voltage sank to fifteen. These two figures correspond to resistances of 60,000 ohms and 15,000 ohms respectively, the resistance of the gas diminishing as the pressure is decreased. Of course we have no certainty as to how much of this opposition to the current is due to true resistance, and how much to a kind of polarization, but it is convenient for present purposes to count it all as resistance.

In any case this opposition, if maintained, is far too great to permit the passage of oscillations, even under the most favourable conditions. In order to prove that the opposition is not maintained, but is in fact broken down by the spark, it was only necessary to photograph the discharge with the help of a rapidly revolving mirror, after the method of Feddersen. Unfortunately, the light in the tube itself is too faint for direct instantaneous photography; but the light of the spark between two cadmium electrodes in the same circuit is quite bright enough for the purpose, and of course any oscillations which crossed the spark-gap must also go through the tube. Our next step, therefore, was to make a series of such photographs of a spark discharged through hydrogen, at first when the gas glowed with a white light and showed its many-line spectrum, and afterwards when it exhibited the characteristic red tint and a spectrum of only four lines in the visible portion of the spectrum.

In order to obtain the white light in the hydrogen tube, it was necessary to increase either the impedance or the resistance in the circuit containing the tube. With a definite very small amount of impedance we increased the resistance until the red glow disappeared in the tube, and discovered on developing the photographs which were obtained by means of the revolving mirror that the discharge was non-oscillatory. When, however, the resistance in the condenser circuit was diminished, the red glow began to appear, and the photographs taken when all the resistance except the tube itself was removed showed that the discharge was oscillatory. This also was evident from the peculiar crackle of the spark, which Hertz remarked was essential in performing his experiments on electric waves. The apparatus used in this and subsequent experiments is sketched in the accompanying diagram (fig. 1). An examination of our photographs showed the interesting fact that there were in general not more than two or three complete oscillations; the remaining ones which could have been obtained from the given capacity and self-induction having been damped by the resistance of the gas. The question immediately arose, What is the resistance of the gas at the instant of the discharge? For if an idea of this can be obtained we can get an estimate of the amount of heat developed in the gas during each oscillation. A Thomson Electrostatic Voltmeter connected to the ends of the hydrogen tube indicated a difference of potential of over 1800 volts, and this difference of potential could only be obtained by substituting for the Geissler tube a resistance of many thousand ohms. The indications, however, of this instrument in this case are of no value; for we discovered that a resistance of from ten to twenty ohms was sufficient to produce the same amount of damping which the gas exerted. The resistance of the gas, therefore, could not be greater than these amounts*. It is evident, therefore, why the voltmeter gives erroneous readings. On account of the inertia of the moving parts, and the very short time of the discharge, it

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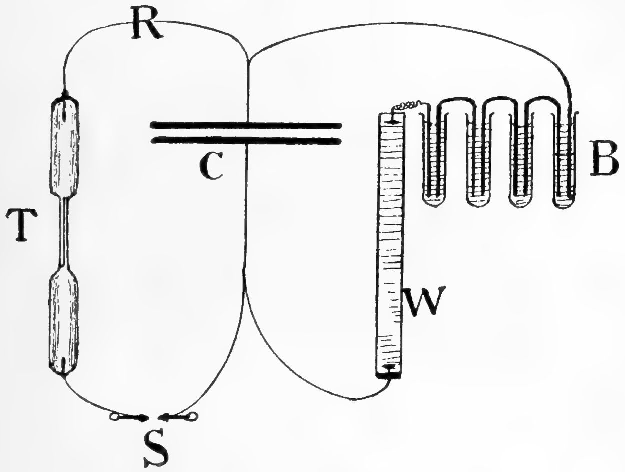
* "Damping of Electrical Oscillations," Proc. Amer. Acad. 1891.

does not indicate the fall of potential through the small resistance of the tube during the instant when the discharge passes, but maintains an indication of a high difference of potential.

In order to apply systematically this new method of measuring resistances, our next step was to prepare a series of standards—photographs of the oscillatory sparks of condensers of different sizes, damped by known resistances which were substituted for the Geissler tube in the condenser circuit. In all these experiments, of course, the small resistance on the left-hand side of the sketch was cut out by a suitable key. Three large leyden-jars, each 30 cm. in diameter and 50 cm. high, having a capacity of 6000 electrostatic units apiece, were used either singly or together to act as the condenser; the waves generated by these large capacities were much too long to interfere with one another upon so short a circuit. The resistances were wires of manganin 0·1 mm. in diameter, stretched on both sides of long strips of thin vulcanite plate, the idea of this arrangement being to eliminate self-induction and yet to prevent the short-circuiting of the high potential. The spark-gap usually consisted of cadmium terminals arranged in the focus of a revolving

Fig. 1.



B, battery of 5000 to 10,000 storage cells.
C, condenser of 1000 to 18,000 electrostatic units.
R, small resistance to damp oscillations.
S, spark-gap between cadmium terminals.
T, Plücker tube containing gas.
W, chief water resistance of 5 to 50 megohms.

mirror driven very rapidly by means of a small electric motor. In a few cases zinc terminals were used, with no appreciable difference in the results (Righi [*]). The terminals were re-pointed from time to time, and were always kept at a distance of 1·3 mm. apart. With this apparatus the photographs of perhaps 500 sparks were taken, and the results are recorded in the following table. As a general rule the spark containing the highest number of oscillations upon any plate was taken as the representative one.

The first column below records the resistance through which the discharge had to pass before reaching the spark-gap, while the second, third, and fourth record the number of half-oscillations observed upon the photographs.

Resistance Standards.

Resistance. Ohms.	Capacity=6000. No. of half-oscillations.	Capacity=12,000. No. of half-oscillations.	Capacity=18,000. No. of half-oscillations.
0	...	37	32
1	...	21 (?)	21
2	16·0	14	13
3	12·0	11	10
4	9·5	8·5	7
5	8·0	7	6
6	7·5	6	5
7	6·5	5	4
10	5·0	4	3
15	3·0	3	
20	2·0	2	1·7
30	1·0	1	1

These figures correspond in general tendency with the less precise determinations made by Feddersen [†]; they show, as his determinations did, that the larger the capacity the fewer the number of oscillations. This tendency is especially noticeable between two and ten ohms, the part of each curve which is most capable of accurate determination. While not perfectly regular, these curves manifestly furnish the means of measuring approximately any small resistance through which a spark, followed by as much as one-half of an oscillation, is able to pass.

Having now our scale of measurement, we substituted for our known resistances a Plücker tube attached to an admirable automatic Toepler air-pump (of Kiss, Budapest), as well as to receivers containing pure hydrogen and nitrogen. These gases could be delivered individually into the tube at any

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[*] *Nuovo Cimento* (2) xvi. p. 97.

[†] Pogg. *Ann.* cxiii. p. 437.

desired pressure. The bulbs of the pump, aggregating over a litre in volume, were always in communication with the Plücker tube while the circuit was closed, so that the discharge took place under essentially constant pressure. The hydrogen was made electrolytically and purified by passing through a solution of potash and over fused potash and phosphoric anhydride; the nitrogen was made by passing ammonia over an excess of heated cupric oxide, through much water,

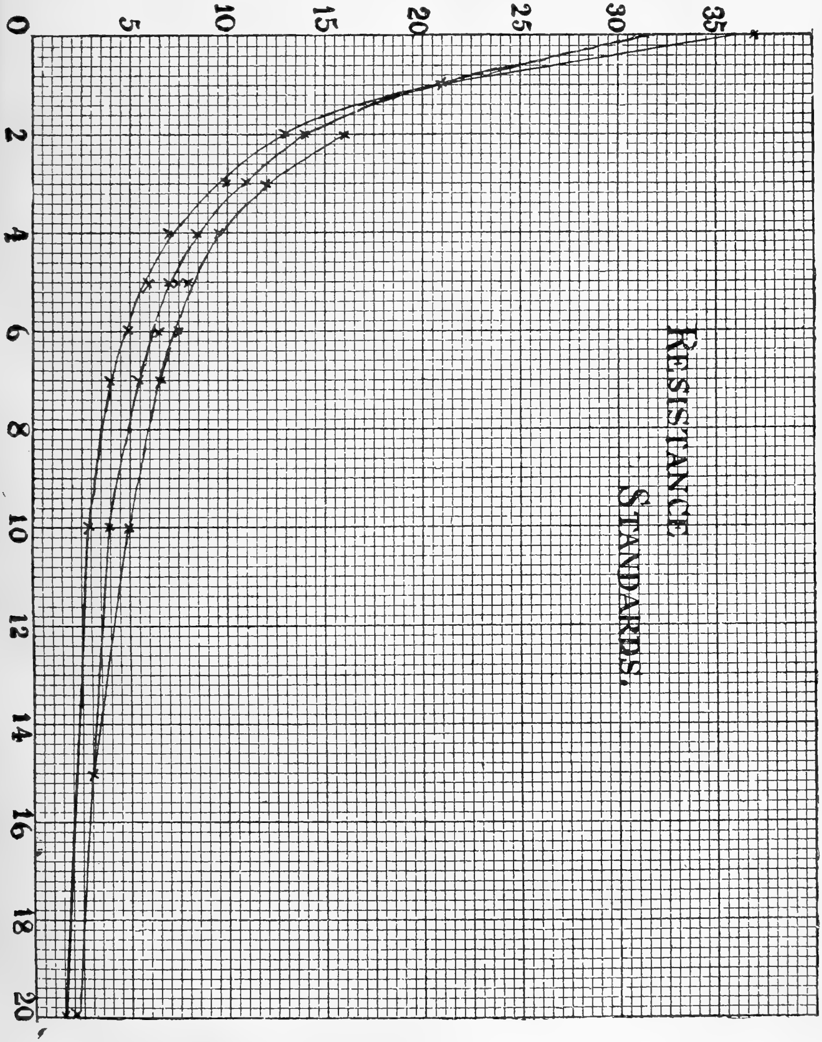


Fig. 2.—Ohms are plotted horizontally; half-oscillations are plotted vertically.

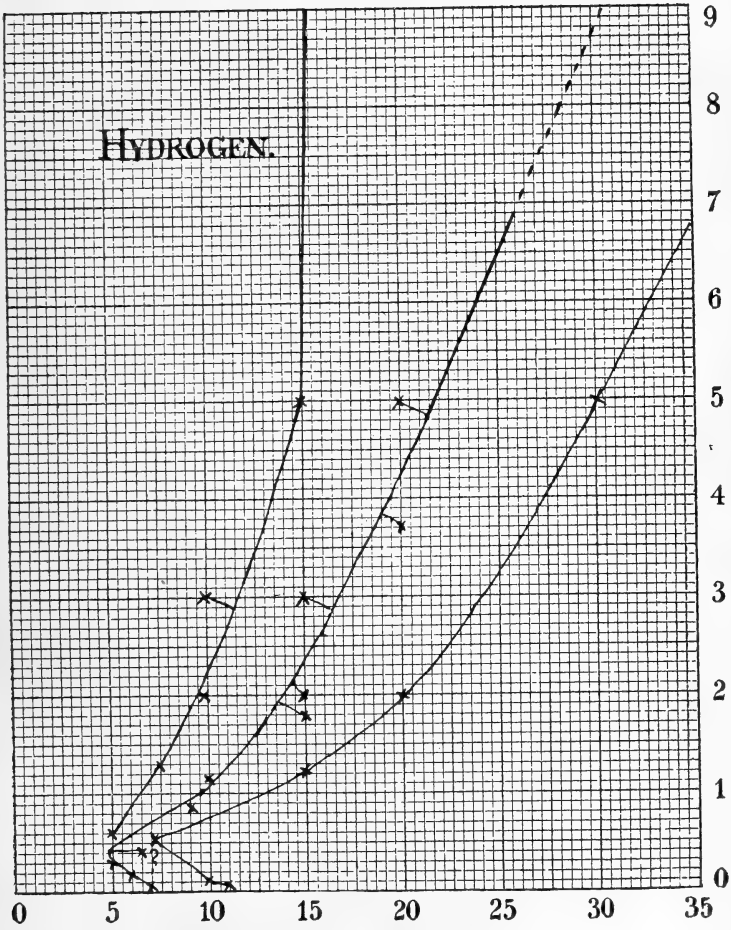
and over the same two driers as the hydrogen. The length of the spark-gap remained always the same, excepting for the very lowest and the very highest pressures of gas, through which the electricity refused to pass unless the spark-gap was narrowed.

In the first column of each table below is recorded the tensions of gas, in the second, third, and fourth are recorded the numbers of half-oscillations obtained with the three different capacities respectively, while in the fifth, sixth, and seventh of these columns are to be found the resistances corresponding to these oscillations, each value being taken from its proper curve in fig. 2. In order to give a better idea of the comparison and the way in which the oscillations are damped, reproductions from two photographs are appended (p. 360).

The Resistance of Hydrogen.

Pressure of gas.	Number of half-oscillations.			Resistance in ohms.		
	Capacity =6000.	Capacity =12,000.	Capacity =18,000.	Capacity =6000.	Capacity =12,000.	Capacity =18,000.
millim.				(ohms.)	(ohms.)	(ohms.)
13·5	...	0	...	...	over 100	
10·0	2½	...	2	50 ?	...	15
5·0	1	2	2	30	20	15
3·6	...	2	...	...	20	...
3·0	...	3	3	...	15	10
2·0	2	3	3	20	15	10
1·8	...	3	...	...	15	...
1·25	3	3	4	15	15	7
1·15	...	4	...	...	10	...
0·85	...	5½	...	...	9	...
0·75	...	6½	...	...	5·5	...
0·60	...	7	6	...	5	5
0·40	...	6	...	...	6	...
0·31	6½	7	6	7	5	5
0·21	...	6	...	...	6	...
0·15	5	5½ (?)	6+	10	...	5
0·10	...	...	...	...	7	
0·05	4½	no spark.	no spark.	11	...	

Fig. 3.



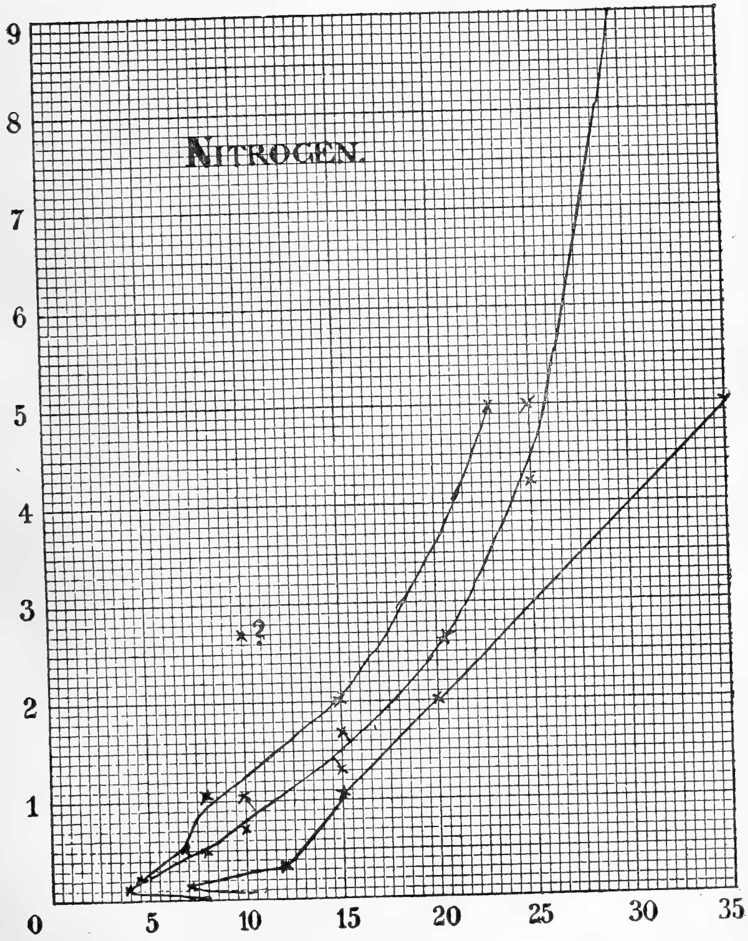
Ohms are plotted horizontally.
Millimetres of pressure are plotted vertically.

The Resistance of Nitrogen.

Pressure of gas.	Number of half-oscillations.			Resistance in ohms.		
	Capacity =6000.	Capacity =12,000.	Capacity =18,000.	Capacity =6000.	Capacity =12,000.	Capacity =18,000.
millim.				(ohms.)	(ohms.)	(ohms.)
9·5	...	1	...	...	30	...
5·0	1 (faint)	1½	1½	35	25	23
4·2	...	1½	...	...	25	...
2·7	2	...	3	20	...	10
2	2	2	2 (?)	20	20	15
1·7	...	3	...	...	15	...
1·3	...	3	...	...	15	...
1·04	3	4	3½	15	10	8
0·70	...	4	...	...	10	...
0·50	3½	5	4	...	8	7
0·30	4	...	6	12	...	5
0·26	5½	...	5½ (?)	...	...	5·5
0·22	...	8	...	...	4·5	
0·15	6½	8½	...	7	4	
0·07	5	8	...	10	4·5	
0·05	...	7	...	...	5	
0·03	...	8	...	...	4	
0·02	...	5	...	...	8	
0·01	...	5	...	...	8	
0·005	no spark.	no spark.	no spark.	very high	very high	

Besides these measurements of hydrogen and nitrogen, several photographs were made of sparks sent through some of Lord Rayleigh's argon contained in sealed tubes. Since the capillaries were not in every case equal in diameter, the results are not wholly comparable with one another, or with those in the two tables given above. Two half-oscillations each were observed in the photographs of argon at 1, 2, and 3 mm. pressure contained in tubes with very fine capillaries, while six half-oscillations were observed in tubes about like those used for nitrogen and hydrogen. This shows that the form of the tube influences very materially the resistance. As a very small jar will provide enough electricity to give the blue spectrum of argon, the resistance of a tube containing the gas at 1 mm. pressure was determined with a capacity of about 1000 as well as with the usual capacities 6000, 12,000, and 18,000 electrostatic units. Somewhat over six half-oscillations were observed in each case, corresponding to resistances of about thirteen, eight, six-and-a-half, and five ohms respectively.

Fig. 4.



Ohms are plotted horizontally.
Millimetres of pressure are plotted vertically.

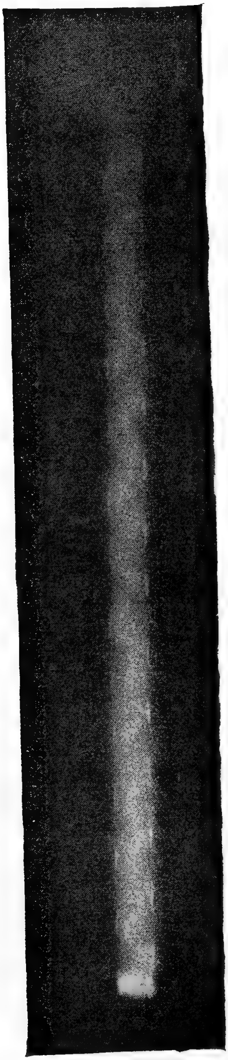


Fig. 5.

Fig. 6.

PHOTOGRAPHS OF OSCILLATIONS.

Fig. 5. Not damped.
Fig. 6. Damped by 5 ohms resistance of hydrogen at point of least resistance.

The evidence of all these experiments is unequivocal, and may be summed up under the following heads :—

I. *The resistance of a gas at low pressure to the oscillatory discharge is equivalent to only a very small ohmic resistance.*

II. *This resistance is in general greater the less the quantity of electricity.*

III. *Down to a very small pressure this resistance decreases with the tension of the gas.* At a pressure considerably below the minima in the potential curves given on p. 351, the resistance seems to reach a minimum, but the irregularity of the sparks in this region makes this last minimum somewhat uncertain. The minima of resistance probably vary with the quantity of electricity discharged as well as with the specific properties of the gas. The minimum for nitrogen is attained at a much lower pressure than that of hydrogen.

IV. *The form of the tube has an important effect upon the resistance of the gas.*

V. *With the oscillatory discharge it is evident that the electrodes produce far less effect than with the continuous discharge.*

These conclusions are not wholly without support in the literature of the subject. J. J. Thomson's researches with tubes without electrodes * show that a rarefied gas must have an extremely low resistance to the oscillatory discharge of electricity, and Jaumann † found that the "electric strength" of a gas increased as the quantity of electricity diminished. But all earlier accurate measurements have been made with continuous currents, whose relations are very different from those of the sudden discharge of a condenser which we have studied. The continuous current meets with great opposition, especially at the negative electrodes, where much heat is developed. The oscillatory discharge meets with very little opposition, and correspondingly we find that here the greatest heat is developed in the gas itself, especially in the capillary tube, the electrodes remaining comparatively cool. This experiment we have tried repeatedly, sending exactly equal amounts of electricity through the tube in each fashion, and observing great differences in the heating effects. Moreover, even in the continuously glowing gas itself apart from the electrodes, the potential-difference, if due to resistance alone, corresponds to vastly greater resistance than that opposed to

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* 'Recent Researches in Electricity and Magnetism,' p. 92.

† *Sitz.-berichte Wien. Akad.* vol. xcvii. p. 765.

the condenser discharge, according to Hittorf's results already quoted.

It is clear that the quantity of electricity going through the tube in a given time is almost incredibly different in the two cases. This difference has not been enough emphasized in the literature upon the subject. Suppose that the battery and other resistance are so regulated as to supply a milliampere of current, and the condenser is of such a size that when it is connected the spark passes ten times a second. These conditions were frequently those of our experiments. The spark, judging from our photographs, certainly does not last more than one millionth of a second, hence the current-strength at the instant of the discharge must be at least 100,000 times as great as that of the continuous discharge without the condenser, or must amount to 100 amperes.

Jaumann's observation that the opposition to the current is less as the current increases, and our conclusion (III.) that the resistance is less with the larger capacity, are in reality observations which may be represented as the two extremities of a long curve. This curve is formed by the relation of milliamperes to megohms on one end, and of hundreds of amperes to ohms upon the other. The part of the curve between these two extremities is very hard to investigate with our present means, and indeed it seems to behave differently with different gases. For these two conditions are represented in any given case by the two spectra of the gases, and as we increase the current we observe varying relations between these spectra. In the case of hydrogen the spectrum of four visible lines gradually increases in brilliancy with the gradual fading of the many-lined spectrum of the lower temperatures as the current is increased; and only when the current-strength becomes very great do the extra lines disappear. In other words, the change from one condition to the other is gradual. In the case of nitrogen, upon the other hand, the change is abrupt: and often when the gas is near its sensitive point some sparks will go through with little opposition, while others give the banded spectrum and the non-oscillatory photograph, showing that the resistance was large. Argon is like nitrogen in the suddenness of the transition, but its transition takes place with much weaker currents than with either hydrogen or nitrogen. We have repeatedly found argon to give the pure blue spectrum under the influence of the discharge of the full battery with very little resistance in the circuit without any condenser, or with less than eight amperes; for the resistance of each cell of the battery is about the quarter of an ohm.

One of the great difficulties of investigating the intermediate part of the curve lies in the fact that no tube is strong enough to stand the continuous application of temperatures as high as those developed by the corresponding current. On the other hand, the repeated instantaneous discharge, which the tube will stand, cannot be estimated when the resistance rises above the very small amount necessary to damp out the oscillations.

The question whether the change in the spectrum, upon increasing the current, is due to greater heat or to the oscillatory motion, is one which is not easily settled, because the last trace of the return oscillation requires hundreds of ohms for its damping; and under these conditions this oscillation is not easily photographed. The fact that argon offers no less resistance than hydrogen or nitrogen to electric oscillations, but nevertheless is much more sensitive to increased current, tends to show that the important factor in the question is not the oscillatory nature of the discharge, but only the great quantity which is always attendant upon oscillatory discharges.

Since gases do not strictly follow Ohm's law, we cannot assume that the formula $R=\sqrt{\frac{4L}{C}}$, in which R = resistance, L = self-induction, and C = capacity, and which gives the limiting value of R for non-oscillatory discharges, rigidly holds. If, however, we obtain a white glow in hydrogen which is due to the unidirectional discharge of a large condenser through a large resistance besides that of the tube, and then proceed to increase the electromotive force, and consequently the strength of the current in our unidirectional discharge, we can determine whether this form of discharge is competent to produce the red glow in hydrogen.

The apparatus which was used for this purpose consisted of a step-up transformer consisting of two secondaries of many turns of fine wire, which were slipped upon a long primary. When the secondaries were coupled in series the electromotive force of the discharge was doubled without any considerable change in capacity. As a matter of fact the glow was seen to be perceptibly redder with two coils than with one, showing that the change in the quantity is the essential feature in the change of the spectrum.

While this conclusion interferes with the strict application of the word talantoscope to an argon-tube, the use of the tube as a talantoscope nevertheless remains; for while the oscillations and the blue spectrum are not strictly dependent upon one another they are both dependent upon the same final cause.

The conclusion that the large quantity of electricity, and therefore the high temperature caused by the discharge, is the cause of the very much diminished resistance of the tube and the corresponding spectrum, leads us at once to consider the energetics of the problem. On the assumption that the departure from Ohm's law is not large*, since $C=\frac{E}{R}$, and the amount of impedance in the circuit is so small as to be neglected, we shall have an amount of energy developed in the tube for perhaps the millionth part of a second equal to CE. With an electromotive force of 10,000 volts and a resistance of ten ohms, a current of 1000 amperes must be obtained, and this multiplied by 10,000 and divided by 746 gives the electrical horse-power if the current were maintained for a second. The corresponding value is over 10,000, and this corresponds to an excessively high temperature for a very brief space of time.

Of course a bolometer or any other thermometer in the tube could not indicate this energy, for it is of very short duration, and even in its brief existence undoubtedly does not affect the whole mass of the gas through which the discharge passes. J. J. Thomson has called attention to this fact†. In the case of the continuous discharge the temperature is undoubtedly vastly lower, but even here it is probable that the pale brush does not concern all the particles of the rarefied gas, for otherwise the discharge in a wide tube should be as bright as the discharge in a narrow tube. Therefore calculations or experimental determinations of the average temperature of a large tube, such as those of Warburg ‡ and Wood, while interesting as relative considerations, give no clue as to the kinetic energy of the molecules which actually carry the current. For such a clue one must refer to experiments of the sort we describe.

All the results recorded in this paper support the well-known hypothesis that the current when disruptive is carried by dissociated molecules. The continuous discharge is best

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* Moreover, we find that the electrostatic capacity of the Geissler tube is not sufficient to affect the period of the oscillatory movement. To decide this, we arranged a rocking key which interposed first the Plücker tube, and then, immediately afterwards, a wire of self-induction equal to that of the tube, and photographed the oscillatory discharge through the two circuits. No change could be perceived in the period of the two discharges. If the electrostatic capacity of a Geissler tube were large, an argon tube would not be so sensitive as it is to slight changes in the electrostatic capacity in the circuit.

† 'Recent Researches,' p. 167.

‡ Wied. *Ann.* liv. p. 265.

explained by conceiving of a polarized condition, in which the molecules are in some way bound together by the electric energy which is striving to force itself through them. As the current increases the amount of the heat increases, until it reaches a stage when some of the gas is freed from this bondage—when the molecules not only separate from their electric embrace, but split into their component atoms. Then, if a large quantity of electricity is at hand to discharge itself, the rate of discharge increases with enormous rapidity, resulting in more dissociation, and the resistance is almost entirely broken down. A good *resumé* of the present state of this hypothesis is given in 'Nature,' January 28th, 1897, p. 310, and to this statement our determination of the resistance makes an important addition.

Hydrogen and oxygen cannot be dissociated to any appreciable extent at ordinary temperatures and pressures, otherwise water would form when they were mixed. We have no vapour-densities of hydrogen or oxygen at temperatures which show dissociation, but this is no reason for believing that at temperatures of 3000 degrees or more dissociation does not take place. Indeed, the burning of hydrogen and oxygen gives us every reason for believing that the tendency of both hydrogen and oxygen molecules to dissociate increases with the temperature. Chlorine, bromine, and iodine are all known to dissociate at high temperatures and to conduct electricity well under those conditions.

Another point in favour of ascribing the red glow of hydrogen to dissociation is to be found in the fact that rarefied aqueous vapour gives the pure "four-line" spectrum much more easily than hydrogen itself. In order to give any hydrogen spectrum at all, the vapour must be dissociated. Of course the dissociation takes place only at the moment of the discharge, the atoms combining again when cold. It is caused by the heat of the discharge, and not by electrolysis, although that too may take place at the electrodes. In short, there is every reason to believe that at temperatures as high as those with which we are dealing, the hydrogen is split apart into hydrogen atoms, and that these atoms, or perhaps the energy involved in the act of splitting them, are responsible for the "four-line" red spectrum. The fact cannot be too strongly emphasized that this sort of dissociation is very different indeed from the electrolytic dissociation of solutions.

In order to find if the structure which is dissociated by the spark is the molecule or some more complex structure, mercury vapour was subjected first to the continuous discharge and then to the oscillatory. Since the spectra obtained were widely different, the conclusion, at least in the case of the mercury, is that the structure is complex; for mercury molecules are monatomic. This conclusion is reinforced by many other facts known about the continuous discharge. Hence the existence of two argon spectra does not give any reason for disbelieving the evidence of specific heat with regard to the monatomic nature of argon.

From the point of view of a mechanical conception of the causes producing the two spectra of a gas, it is easy to imagine that when the atoms are bound together in the polarized condition, the electricity by a succession of readjustments may travel step by step from one end of the tube to the other, at a comparatively low temperature, and so cause quite a different set of electromagnetic vibrations from those depending upon the breaking down of this polarized system. The evidence that the second spectrum given by the oscillatory discharge is due either to the act of separating the atoms from one another, or to the passage of the electricity through the atoms already set free by the heat, has been given above. Hittorf's experiments, in which he was able to send a very strong continuous current through a gas without the production of light, would seem to indicate that the light is due to energy involved at the moment of dissociation, but the spectra of the solar prominences lead to the opposite inference, and conclusive evidence upon this point is wanting. The dissimilar behaviour of different gases is easily accounted for by considering the two causes which are supposed to resist the dissociation: in the first place, the "polarized" condition of the molecules, and in the next place, the chemical affinity of the atoms for one another. This last force is usually admitted to be greater in the case of nitrogen than in that of hydrogen, hence the difficulty, the irregularity and the abruptness of the transition in the former case. One should expect that a monatomic gas, like argon, where the polarization alone prevents the passage of the current, would be easier to change in this respect, as indeed it is. The fact that the second spectrum of mercury is not very easily obtained militates against this explanation, however.

Assuming, then, that the red spectrum of hydrogen is due to the sudden occurrence of the reaction

$$H_2 = H + H,$$

it is very interesting to note that our results agree with the necessary deductions from the law of mass action as applied to this case. If the reaction is supposed to take place isothermally at a very high temperature, it is manifest that the progress of the reaction from right to left must increase as the concentration of the hydrogen—in other words, the tension of gas—is diminished. This we find to be the case: the resistance of the gas increases, and the purity of the "four-line" spectrum diminishes as the tension of the gas increases, except when the gas is exceedingly rarefied. In this case it is probable that the number of atoms present, even if all were free, would be insufficient to carry all the current. Hence we should expect to find this minimum at a lower pressure when the capacity of the condenser employed was less; but, unfortunately, the spark is too uncertain at these very low tensions, even with 20,000 volts from the complete battery, to give definite results.

Since hydrogen undoubtedly requires a very large amount of heat for its dissociation, it follows that when the temperature is raised while the pressure is kept constant, more atoms should be set free. We find, as a matter of fact, that the resistance diminishes as we increase the capacity of the condenser—that is to say, the heat of the discharge. The case is exactly analogous to the dissociation of nitric peroxide observed by E. and L. Natanson *.

Our work leads one to infer that since a very high temperature is needed to produce the "four-line" spectrum of hydrogen, this high temperature must be present whenever this spectrum appears, for example, in the solar prominences and in many fixed stars. The higher the tension of the gas, the higher the temperature required; hence one must know the atmospheric pressures in these heavenly bodies before attempting to guess at the actual temperature attained; but there can be no doubt that this temperature is in any case far beyond the reach of any earthly means except the electric discharge which we have been considering.

Harvard University, Feb. 23rd, 1897.

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* Wied. *Ann.* xxiv. p. 454, and xxvii. p. 606.

XLVIII. *On a Supposed Proof of a Theorem in Wave-motion.*

To the Editors of the Philosophical Magazine.

GENTLEMEN,

AT p. 281 of this volume of the Philosophical Magazine, Mr. Thomas Preston announces an extension of Fourier's theorem whereby a scalar function of *any number* of variables may be expanded in a series of sines and cosines of linear functions of the variables, with coefficients expressible as definite integrals. This remarkable theorem is of much interest from the mathematician's point of view, and is likely, when handled correctly, to be of use to physicists also. But, in applying it, it is essential that we should attend to the fact that it deals with scalar quantities only, and cannot be employed in physical problems except so far as the mathematical discussion of the problem, throughout whatever is its extent in space, admits of being brought into a scalar form. Another consideration must also be kept in mind, viz.: that what presents itself in optics as diffraction is encountered in every real wave-motion. Both of these considerations, and the distinction between forced wave-motion and wave-motion which the medium is able to propagate, seem to have been overlooked in two attempts which Mr. Preston has made to employ his theorem to prove certain physical theorems which treat of real wave-motions, one attempt on p. 283 and the other on p. 285; and unfortunately the oversights vitiate the proofs which he offers. They also affect what is said in the third paragraph of his paper, on p. 281. This paragraph contains two statements, of which the first is erroneous owing to the omission of these considerations, and the second can only be rendered correct by interpreting the word "function" to mean a function containing vectors as well as scalars, in which case the observation although true would have no relation to what follows it.

In the first paragraph of his paper, on p. 281, Mr. Preston quotes the enunciation of a theorem in wave-propagation as follows:—

"However complex the contents of the objective field, and whether it or parts of it be self-luminous, or illuminated in any way however special, the light which emanates from it may be resolved into undulations each of which consists of uniform plane waves."

This enunciation is taken from a paper on Microscopic Vision, and is in reality a general theorem in wave-motion though presented there in its optical form only. It may be freed from limitation while keeping the enunciation substantially the same, and then becomes :—

If space be occupied by any *uniform medium capable of propagating waves, and if a disturbance however complex is called into existence within a defined portion of the space, then the undulations which emanate from the disturbance and spread through the rest of space may be resolved into undulations of the medium each of which consists of uniform plane waves.*

This may be called Theorem A. It is a theorem which the present writer proved geometrically and of which a symbolical proof is much wanted. A correct symbolical proof, whenever it is discovered, will probably lead to useful determinations of the coefficients of the expansion in Theorem A, involving vectors as well as scalars, but otherwise somewhat like (though necessarily more complex than) the expressions for the coefficients of a Fourier's expansion. To accomplish this would be an important service. Mr. Preston endeavours to supply this desideratum in the paper now under discussion.

Each of the plane waves of Theorem A has been shown by the present writer to be of *unlimited* extent in its plane and uniform *over its whole extent.* It is therefore a wave which as it travels forward remains unchanged whatever be the medium that occupies the space, provided only that the medium be uniform. Observe also that what the waves are does not depend exclusively upon what the disturbance is, but upon this *in conjunction with the physical properties of the medium.* Accordingly any expressions for the coefficients of the terms of the expansion which supposes these coefficients to be functions of the disturbance only, such as the values given by Mr. Preston at the top of p. 283, must be wrong. They can only belong to some kinematical resolution consisting of forced vibrations; a mathematical exercise of little use in physics, since it supplies no information about the real resolution into plane waves effected by nature which is what is dealt with in Theorem A.

Of the consequences of Theorem A, that which is of most value to the physicist, is that the radiations *outside* the region of disturbance resolve themselves into undulations of uniform plane waves. It is of somewhat less importance to the physicist, though equally true, that the theorem also resolves the disturbance itself into *these same* undulations, if the disturbance be of such a kind that it expends all its energy in propagating waves. (See the condition numbered 3, on page 141 of this volume.) The truth of this second part of the theorem is shown in the paper on Microscopic Vision referred to by Mr. Preston, where it is demonstrated that the undulations of plane waves are competent to form what is there called the Standard Image: an image which is identical with the original disturbance if the latter be one which expends all its energy in the propagation of the waves. (See p. 339 of the Philosophical Magazine for October 1896.)

Accordingly the theorem may be enunciated under either of two aspects. It is immaterial into which form it is put, since each implies the other, so that either being established both are proved. Mr. Preston adopts the second form in paragraph 2 of his paper, on p. 281, where he puts the enunciation into the following terms, to which I have made additions within brackets which are introduced to make the meaning unmistakable :—

"*Any disturbance however complex within a given region of space*" [*provided only that it be one which expends all its energy in generating waves*] "*may be resolved into a system of plane-wave components*" [*which are real*]; that is, which belong to the undulations actually generated in the medium that pervades the space—undulations which if unobstructed spread from the disturbance through the whole of space.

Such being Theorem A, we have now to compare it, or rather contrast it, with Theorem B, which Mr. Preston supposes to be its analytical expression. Theorem B will be found at p. 283 of Mr. Preston's paper, and is as follows :—

$$\left.\begin{aligned} f(x,y,z,t) = \Sigma \mathrm{A} \cos(px+qy+rz+st) \\ + \Sigma \mathrm{B} \sin(px+qy+rz+st) \end{aligned}\right\}, \quad . \quad . \quad (\mathrm{B})$$

in which the coefficients, the A's and B's, have the purely scalar values assigned to them at the top of the same page. Accordingly vectors have no place anywhere in equation (B); and as a consequence $f(x,y,z,t)$ is incompetent to represent the "*disturbance however complex* within a given region," which is what we have to analyse. In fact any single expression, like the first or the second member of equation (B), which purports to represent a "disturbance however complex," must include the vectors of the transversals as well as their scalar values. Moreover, in using equation (B) the coordinates x, y, z must be restricted to points *within* the "given region of space." The supposition then that the scalar equation given by Mr. Preston can possibly be the analytical expression of Theorem A, falls to the ground.

It may at first sight appear as if these difficulties could be met by the familiar expedient of representing the motion within a given space not by the one function $\mathrm{F}(x,y,z,t)$ which is not scalar, but by three purely scalar equations, such as

$$\xi=f_1(x,y,z,t),$$
$$\eta=f_2(x,y,z,t),$$
$$\zeta=f_3(x,y,z,t),$$

(where ξ, η, and ζ may be displacements, or velocities, or so on, in the three coordinate directions); and by then expanding each of these by Mr. Preston's theorem. But on a close scrutiny we find that though this furnishes an apparent solution, in the form of forced vibrations, or rather a group of such solutions, this group unfortunately does not include the solution which would be selected by nature under any conceivable circumstances. The analysis furnishes undulations which could not propagate themselves through any medium. The motions which it furnishes are the non-natural motions of a mere forced kinematical resolution, of no use in physics. That this is so can be made plain by taking any very simple example, such as the following.

Let the "given region of space" within which the disturbance is maintained be a thin circular disk perpendicular to the axis of z; let the origin of coordinates be at the centre of the disk, let the disturbance maintained within it be of the simple kind represented by

$$\xi=f(vt-z)+f(vt+z),$$

and let the medium be the æther.

The undulations which will be generated by this disturbance will propagate themselves both forwards and backwards and both within and beyond the disk; and as from symmetry the two groups of undulations will be exactly alike, it will suffice to ascertain what those travelling forwards will be. This is easily done from the circumstance that they closely approximate to being identical with the radiations forwards from a circular opening in a screen of the size of the disk, when the light from a star, or rather that part of it polarized in one direction, is allowed to fall perpendicularly on the back of the screen.

Before reaching the screen the light from the star of any one wave-length is as near an approximation as can be realised to being a single undulation of uniform plane waves with each wave of infinite extent in its own plane. A cylindrical beam out of this undulation is what reaches the opening in the screen. Until it reaches the opening it is an absolutely single train of uniform plane waves. But at the opening it ceases to be this single beam. From that situation forwards it spreads in a highly complex way over what we may call a cone of dispersion, and becomes that sheaf of innumerable radiations which produce the well-known diffraction effects. That they form a very complex system and are infinite in number may be seen from the considerations in the subjoined footnote*.

Let us now turn to the events that would arise if the disturbance $\xi=f(vt-z)+f(vt+z)$ were maintained throughout the disk-like region of space. Here a highly complex sheaf of radiations almost identical with that described in the last paragraph would be emitted forwards by the disturbance in the disk, and an exactly similar one backwards. What Theorem A tells us is that these two complex systems of radiations can be resolved into *innumerable* undulations, each of perfectly uniform plane waves, each of infinite extent laterally, and each advancing in its own direction through space without undergoing change. Further, that if all of these were made to cross the disk, they by their mutual interference would unite to produce within that small portion of space the extremely simple motion represented by $\xi=f(vt-z)+f(vt+z)$, while everywhere else in space, whether in the plane of the disk or outside it, they by their interference develop not that motion, but the radiations which emanate from it †. This is information of importance. It is a true analysis of events that are really going on.

Contrast this with the kinematical information supplied by Theorem B, viz. :—The motion within the disk represented by

$$\xi=f(vt-z)+f(vt+z)$$

can be forcibly resolved into two mathematical series, the

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* The innumerable direct and diffracted undulations which advance from the opening in the screen and which are furnished by Theorem A, could be each concentrated into a point by an aplanatic lens of infinite aperture placed in front of the opening in the screen so as to receive all the light which emerges from it. In practice the lens need not be of infinite aperture, since the same *resultant* effect is produced by a lens whose aperture is somewhat larger than the opening in the screen. This furnishes as the image of the star a spurious disk surrounded by rings. Accordingly *every point* of this complex image is the concentration of one of the undulations of uniform plane waves of infinite extent laterally, which are furnished by Theorem A.

† Along with the radiations which converging upon the disk would produce in it the motion $\xi=f(vt-z)+f(vt+z)$. In fact, each infinite undulation necessarily consists of a moiety of the undulation flowing in towards the region of disturbance, of a small portion travelling across it, and of the rest travelling past or from it. But practically the presence of the inflowing portions causes no inconvenience, because in the applications of the theorem the radiations that are outward bound, and those inward bound, are easily discriminated.

terms of which represent mere artificial undulations which advance in only two directions, one perpendicularly forwards the other perpendicularly backwards across the disk, which may not be carried one step outside it, which inside it are of a kind that no medium could propagate, which are in fact a mere mathematical fiction, and not any physical analysis whatever of events going on in nature. Yet these two analyses, one by Theorem A the other by Fourier's Theorem, though so utterly unlike, are identified with one another in the first of the two statements made in the third paragraph of Mr. Preston's paper.

In the paper preceding Mr. Preston's it is shown on p. 273 that Theorem A may be expressed symbolically by the equation

$$\mathrm{F}(x, y, z, t) = \iint \Sigma \left[\mathrm{M} \sin\left(2\pi \frac{r - vt}{v\tau} + \alpha\right)\right] . \sin\theta \, d\theta \, d\phi, \quad \text{(A)}$$

where $r = x \cos\theta + y \sin\theta \cos\phi + z \sin\theta \sin\phi$, and in which the M's are *directed* quantities. The values of v and the vector components of the M's depend on the properties of the medium, and may be expressed as functions of θ and ϕ when we know the equation of the wave-surface in the medium. On the other hand the α's and the scalar components of the M's depend on the originating disturbance. Now what is wanted is such an analytical proof of Theorem A as will give us symbolical expressions for these quantities as functions of θ and ϕ; and it may be hoped that Mr. Preston, with his experience in dealing with this class of problem, will yet be able to substitute the really valuable proof which will accomplish this for the illusory proof which, on a first view, he has mistaken for it.

I have made a slight attempt by adopting polar coordinates, but hitherto without success, to find some use for Mr. Preston's extension of Fourier's Theorem. It is perhaps not impossible that a fuller search in this direction may bear fruit. But whatever the issue, it is plain that Mr. Preston's extension of Fourier's Theorem, though it may be of limited applicability in physics, is of interest as a mathematical theorem.

I am, Gentlemen,
Faithfully yours,
G. JOHNSTONE STONEY.

8 Upper Hornsey Rise, N.,
April 12, 1897.

XLIX. *Liquid Coherers and Mobile Conductors.*

By ROLLO APPLEYARD*.

IN a communication† made three years ago to the Physical Society, I described some experiments illustrating the change in electrical resistance of certain complex bodies under the influence of oscillatory discharges. All the substances dealt with were solids; and the coherence was invisible. The change of condition had therefore to be demonstrated either by measuring the resistance before and after discharge, or by connecting the coherers permanently in series with a battery and galvanometer.

The three experiments now brought before you have regard to "coherers" formed of liquid dielectrics, and mobile conductors. By choosing a transparent dielectric and an opaque conducting substance, it is possible to examine the process of coherence by direct observation. But it may be well to premise that the similarity of *results* obtained with solid and liquid "coherers," respectively, in no way proves a similarity of *process*. The two sets of phenomena are probably related, but are not necessarily identical. The term "dielectric" is here to be understood as signifying merely a substance of low conductivity.

Experiment 1.—A glass tube about eighteen inches long, and half-an-inch wide, is sealed at one end and corked at the other. Platinum electrodes are inserted at each end. The tube is nearly filled with about equal volumes of paraffin-oil and mercury. If it is laid upon a flat table and shaken, horizontally, for a few minutes, the mercury breaks up into small spheroids; and, by a little manipulation, these can be disposed as a chain of particles lying evenly between the platinum electrodes. The resistance of the chain of mercury spheroids, measured under these conditions, is several megohms.

If we now connect the electrodes to a battery of about two hundred volts, the whole regime is suddenly altered. At the moment of applying the current, the spheroids of mercury, within the tube of oil, are visibly impelled, as though a mechanical tap had been administered to the glass; and, almost simultaneously, they coalesce into large globules. The resistance is now represented by a few ohms.

Exactly the same result can be brought about by supporting the tube near a Hertz oscillator; or, still more simply, by passing a spark into one or other, or both, of the

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[*] Communicated by the Physical Society: read March 26, 1897.

[†] "Dielectrics," Phil. Mag. xxxviii. p. 396 (1894); Proc. Physical Society, xiii. p. 155 (1895).

electrodes. In order to retard the spontaneous coherence of the mercury, resulting from mutual pressure of the spheroids, it is well to keep the tube horizontal. If, however, it is desired quickly to convert a body of mercury from the subdivided to the ordinary state, sparks may be passed into the tube while it is more or less vertical. The running-together of subdivided mercury is more leisurely to be observed with large globules. These form separate, elongated, conductors. The way in which they unite will be referred to in describing Experiment 3.

Experiment 2.—A glass tube, similar to the first, but somewhat wider, is nearly filled with a mixture of paraffin-oil and water, and vigorously shaken. I propose to call this a "rain" tube. If it is kept at rest, the oil, in the common course of events, floats to the top in a few minutes. The "rain" tube, however, shows that the separation, especially towards the final stage, is accelerated by the passage of a spark, or by a direct current from a battery of about a thousand volts. If the conditions are right, the water particles suspended in the oil cohere, at the moment of electrification, to form larger drops. The frictional resistance to falling is thereby diminished, and the water is consequently precipitated in and through the oil. It may sometimes be seen descending in a rapid succession of globules, precisely as large rain-drops are precipitated after thunder. About equal parts of oil and water is a good proportion. The containing vessel may be either a tube or a flask; it should not be more than three-quarters full of liquid. This free space facilitates the mixing when the tube or flask is shaken. The phenomenon is rendered much more striking by colouring the oil with alkanet-root. I have to thank Prof. McLeod for suggesting this pigment.

Experiment 3.—The behaviour of a mobile conductor, when electrified in a partially conducting liquid, is readily examined by pouring a little mercury into a flat photographic dish containing a stratum of paraffin-oil and water. The presence of the oil is necessary to prevent the mercury from running together too freely of its own accord. A battery of from, say, one volt to two hundred is required, and a pair of wires to dip into the dish. A reversing-key, such as is used for cable transmission, may be included in the circuit.

Suppose we begin with a large globule of mercury in each of any pair of corners of the photographic dish, several inches apart; and let the globules be connected one to each pole of the battery, by means of the dipping wires. A momentary tap of the key causes instantaneous deformation of the mercury in each corner, especially of that connected to the negative pole; and there is evident attraction between the globules.

Sometimes the mercury gets into a lethargic condition; but it can always be roused by mechanical agitation of the surface. Now let the current be kept on for a few seconds; the negative globule sends forth a tentacle towards the positive globule, the length of the tentacle depending upon the current and the distance between the globules. Under favourable circumstances it may extend from corner to corner, and thus establish contact; or *fissure* may occur, the tentacle breaking into spheroids; and these spheroids may cross over between the globules. This is the order of things usually to be observed, but the action is sometimes erratic.

Let us now bring back the scattered globules of mercury to their respective corners, and distribute a few isolated spheroids in the interspace. In addition to the effects previously noticed at the terminal globules, we now see that, when the current is applied, each intermediate spheroid extends a "finger" towards the positive globule. This is the process of mobile coherence; the short "fingers," or long "tentacles," form links between consecutive spheroids, and finally a complete conducting circuit is established.

By successive applications of the current, any elongated bodies of mercury between the terminal globules can be made to creep along like caterpillars; the successive forward motions of the tentacle, or tail, cause a corresponding retrogression of the globule as a whole.

Any small spheroids scattered about the dish may be urged in a direction depending upon the direction of the successive current impulses; and a "finger" will always appear on the side towards the positive electrode. So that by choosing a convenient stray spheroid, and operating a battery-reverser as a transmitting-key, a telegraphic receiver is improvised from no other apparatus than a drop of mercury and a little oil. By some such means the awakening genius of primitive man may have contrived all the subtle machinery of a telegraph-instrument upon the smooth surface of an oyster-shell.

L. *The Effect of Great Current-Strength on the Conductivity of Electrolytes.* *By* THEODORE WILLIAM RICHARDS *and* JOHN TROWBRIDGE [*].

IN our paper on the temperature and ohmic resistance of gases during the oscillatory electric discharge [†], we have described a method of determining resistance by

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[*] Communicated by the Authors.

[†] Page 349, *suprà*.

measuring its damping effect upon electric oscillations. The method is obviously one which will apply to electrolytes also, provided that the resistance to be measured is less than twenty ohms; and it seemed to be very well worth while to determine if the intense current involved in the discharge of a large condenser is capable of causing any change in the condition of an electrolyte.

In our first experiment, two copper plates of sixteen square centimetres area were clamped at a distance of three centimetres apart by means of vulcanite. Upon being immersed in a saturated solution of pure cupric sulphate at 15° C., the plates allowed about ten oscillations from one of our large leyden-jars, nine from two jars, and eight from three jars to pass through it. According to the scale of standards, given in our last paper (see fig. 2, p. 355), each of these results corresponds to a little less than four ohms' resistance. By means of Kohlrausch's method, using a very small inductorium, this cell gave an extremely poor minimum at a point corresponding to a resistance of about ten ohms. The plates, which had purposely been left very dirty, in order to test the efficiency of the method, were now scrupulously cleaned with alkali and acid, and were then both carefully plated with pure copper. With Kohlrausch's method the cell now gave an excellent minimum at exactly four ohms' resistance, and further cleaning and plating caused no further change. New photographs of the sparks from the two jars sent through the cell showed again about nine half-oscillations, corresponding to about 3·8 ohms. It is evident, then, that the resistance of concentrated cupric sulphate is not essentially altered by great alterations in the strength of the current.

Experiments with zincic sulphate gave similar results, and a solution of cadmic sulphate between cadmium electrodes which possessed a resistance of 4·7 ohms according to Kohlrausch's method, gave nine, seven, and six half-oscillations with one, two, and three jars respectively, corresponding to about 5 ohms in each case.

Undoubtedly the reason why the strong instantaneous current, which alters so much the resistance of gases, has so little effect upon solutions, is because of the great mass and specific heat of the material which must be warmed in the latter case. The average temperature of the solution rose during our experiments only at the rate of about 1° in three minutes.

A similar, although smaller, heat-capacity prevents the wire resistances which are used as standards from becoming seriously altered in resistance by the heat. We had used manganin wire in our tests; but in order to be sure that our fine short wires had not been overheated, we constructed a five-ohm resistance of four strands of coarse manganin wire about 0·25 millim. diameter and 3·5 metres long. This was stretched upon each side of a thin vulcanite plate to avoid self-induction, but it allowed essentially the same number of oscillations to pass as did the short fine wire. A short german-silver wire, with a very high temperature-coefficient, showed a conductivity only a very little less; thus the error from the heating of the wire may be neglected.

In order to show that common electrolytic polarization does not interfere with the accuracy of our method, we measured with the help of our 20,000 volt storage-battery and condensers the resistance between two bright platinum plates similar in size to the copper ones described above in a cupric sulphate solution. This was found to be four ohms, and after plating the electrodes with copper the resistance remained unchanged. Kohlrausch's method gave no satisfactory result with both electrodes free from copper, but when both were plated it indicated a resistance of 3·9 ohms.

Our method may therefore be a useful one for the approximate determination of conductivities in cases where impurities or polarization render Kohlrausch's method unsatisfactory. For accuracy, of course pains must be taken to develop all the photographs in the same fashion, and in general to arrange the conditions of the exposure alike in all cases.

Our conclusion that the conductivity of electrolytes is *not* greatly affected by great changes in current-strength only emphasizes all the more strongly the conclusion of our last paper, that the conductivity of gases is very much affected by changes in the current-strength.

Harvard University,
March 8, 1897.

LI. *The Electrical Conductivity of the Æther.*

By JOHN TROWBRIDGE*.

THE electrical conductivity of the æther has been maintained by Edlund and has been apparently disproved by various recent investigations—notably those of Prof. J. J. Thomson†. The latter writer, in his treatise entitled 'Recent Researches in Electricity and Magnetism,' also remarks, p. 98:—"Again, if we accept Maxwell's Electromagnetic

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* Communicated by the Author.

† Roy. Soc. Proc. vol. xlv. 1888, p. 290.

Theory of Light, a vacuum cannot be a conductor or it would be opaque, and we should not receive any light from the sun or stars."

The experiments which have been made hitherto on this subject have been conducted with comparatively feeble electrostatic forces. By means of a storage-battery of 10,000 cells in connexion with a Planté rheostatic machine* I have studied the resistance of highly rarefied media under disruptive discharges, and I am led to the conclusion that with a sufficiently powerful electrical stress, what we term a vacuum can be broken down, and that the disruptive discharge during its oscillations encounters very little resistance. In the case of a highly exhausted Crookes tube I have measured this resistance and find it in the special case I considered less than three ohms.

My experiments lead me to the conclusion that the chief resistance is encountered at the surface of the electrodes, and that when this is overcome the æther offers little resistance. The method I have employed seems to me to be a very useful one for the study of electrical discharges. It may be termed the damping of the additional Spark Method, or the comparison of resistances by the estimation of the damping of electrical oscillations†. The electrical circuit is provided with two spark-gaps. One of these is placed in a gas, or under the conditions which are to be examined, while the other is photographed according to Feddersen's method by a revolving mirror. With cadmium terminals this method enables one to estimate the resistance of a spark in air or in rarefied media to one half an ohm.

Having at my command a battery giving a voltage of twenty thousand, with an internal resistance of only one quarter of an ohm per cell, and capable therefore of giving a very powerful current, I first studied the behaviour of Crookes tubes which were connected to the terminals of this battery. I found that no Röntgen rays could be obtained with a voltage of twenty thousand. On heating the Crookes tubes, they were filled with a pale white light, which showed very faint bands in the green when examined by the spectroscope. Then the entire strength of the battery appeared to be manifested in the tubes, the electrodes became red-hot—the medium broke down and offered no resistance to the current of the battery. This white discharge showed even at its culminating point no Röntgen rays. I then employed

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* *Comptes. Rend.* t. lxxxv. p. 794, Oct. 1877.

† "Damping of Electrical Oscillations on Iron Wires" (Phil. Mag. Dec. 1891).

the Planté rheostatic machine. This apparatus, I think, has not received sufficient attention from physicists. In connexion with a large battery it is very efficient and it enables one to form an estimate of the high electromotive force that one employs in the study of the Röntgen rays. I have slightly modified the form of the machine as it is given by Planté. The main principle consists in charging leyden-jars in multiple and then discharging them in series. The proportion of the length of spark to the number of jars is very close. Knowing the electromotive force of the battery which charges the jars we can estimate the voltage necessary to produce sparks of different lengths. I speedily found that at least one hundred thousand volts were necessary to produce the Röntgen rays, and they were produced more intensely as I increased the voltage, certainly to the point of five thousand volts.

In order to ascertain whether the discharges through the Crookes tubes when the Röntgen rays were apparently produced most strongly were oscillatory, I first placed a Geissler tube in the circuit with the Crookes tube and carefully observed the appearances of the two electrodes of the Geissler tube. They were quite alike and indicated an oscillatory discharge. I then replaced the latter tube by a small spark-gap and photographed the spark in a rapidly revolving mirror. The photograph showed at least ten oscillations with a period of about one millionth of a second with the Crookes tube and the circuit I employed. Furthermore, applying the method of estimating resistances by the method of damping, I found that the resistance of the rarefied medium was less than five ohms. The energy, therefore, at the moment of the emission of the Röntgen rays was not far from three million horse-power acting for one millionth of a second. I employed also a Crookes tube with an aluminium mirror of about two centimetres focus. The resistance of this tube to the discharge was the same as that in which the mirror had a focal length of six centimetres. Incidentally, there seems to be no advantage in shortening the distance between the kathode and the anode by employing a mirror of short focus. Struck by the fact that the distance between the electrodes did not appear to make any appreciable difference in the resistance of the Crookes tube, I replaced the latter by a spark-gap of six inches in length in air and photographed the spark in another gap in air in the same circuit. This latter gap was one quarter of an inch. The photographs showed on an average the same number of oscillations whether the secondary spark-gap was six inches in length or one inch in length. I found moreover, that on increasing the electromotive force the resistance of the sparks in air decreased. By quickly drawing apart the terminals of my large battery I can produce a flaming discharge in air of about three feet in length. Righi has also observed the same phenomenon with sparks from an electrical machine. We see that no increase in resistance results. I then placed the secondary spark-gap in a receiver and studied the resistance offered by rarefied air at the point when long ribbon-like white disruptive discharges can be obtained. This point is at about 100 millim. pressure. The resistance of such discharges of about six inches in length in a receiver containing air at this pressure is two or three ohms more than sparks of one quarter of an inch in air; the latter have a resistance of from two to three ohms. On measuring by the above method the resistance of sparks of different lengths in the receiver at this pressure, no difference in resistance could be perceived between a spark of six inches in length and one of three inches in length.

The secondary spark-gap was next placed in a chamber of air which was compressed to four atmospheres. This amount of compression made no difference in the resistance to the disruptive discharges. The additional spark was also obtained in hydrogen gas generated by electrolysis at atmospheric pressure, and no appreciable difference in resistance between this gas and air was noticed. The length of spark which could be obtained with a given voltage was somewhat more in hydrogen than in air. It was interesting, in the next place, to determine by this method whether differences in the material of the spark-gaps made any difference in the resistances observed in the case of disruptive discharges*. I accordingly employed terminals of platinum, iron, aluminium, brass, cadmium, and zinc, and could perceive no difference. Moreover, any difference of resistance between spheres and between pointed terminals, or between a point and a plane, seemed to be inappreciable. With powerful discharges such differences, if they exist, apparently disappear. The additional spark was next placed in a heated flame. It is well known that the spark-length can be thus greatly increased. On photographing a spark in an additional gap the resistance appeared to be slightly increased in the flame; doubling the length of this spark, however, made no change in the resistance that was encountered in the heated medium. The phenomenon was exactly analogous to that observed in the receiver exhausted to 100 millim. I was interested to observe whether

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* Righi, *Nuovo Cimento* (2) xvi. p. 97 (1876); De La Rue and Hugo Müller, Phil. Trans. clxix. pt. i. p. 93 (1878).

heating the spark in the primary of a Thomson-Tesla transformer produced any marked change in the high-tension spark of its secondary. It was evident that it was detrimental. The high-tension sparks immediately ceased to jump at the extreme sparking-distance of the terminals. Following this train of thought I next placed a spark-gap of the primary of the above-mentioned transformer between the poles of a very powerful magnet, giving a field of certainly ten thousand lines to the centimetre. It is well known that when such a field is excited, the primary spark appears to be blown out with a loud report and a great increase of length of spark is obtained in the secondary of the transformer. Applying the same method, I photographed the spark of the additional spark-gap and found no difference in resistance whether the magnetic field was excited or not: or when the spark jumped across the magnetic lines or in the direction of the latter. Is it possible that the æther being already under a magnetic stress, the addition of a powerful electrostatic stress serves to suddenly break down the æther? It is well known that a blast of air imitates the action of the magnetic field. It probably does so by blowing out the voltaic arc which tends to form. It may be that the electrodynamic repulsion compels the spark not to follow, so to speak, the voltaic arc and its current of heated air. The loud report may indicate a sudden stress in the medium, and in the case of the Crookes tube the highly rarefied medium within it would effectually prevent our hearing a similar report.

I next placed the primary spark of the Thomson-Tesla transformer near a Crookes tube which was giving out the Röntgen rays. I could not perceive any mutual effect. The effect, moreover, of ultra-violet light on the resistance of sparks in air could not be detected.

The method I have outlined enables one to form an estimate of the energy incident upon the production of the Röntgen rays. It can also measure with greater accuracy than has been possible hitherto the resistance of sparks in air and different media. It shows conclusively that the discharge in a Crookes tube at the instant when the Röntgen rays are being emitted most intensely is an oscillatory discharge. In popular language, it can be maintained that a discharge of lightning a mile long, under certain conditions, encounters no more resistance during its oscillations than one of a foot in length. In other words, Ohm's law does not hold for electric sparks in air or gases. Disruptive discharges in gases and in air appear to be of the nature of voltaic arcs. Each oscillation can be considered as forming an arc. It is well known that a minute spark precedes the formation of the voltaic arc in air. The medium is first broken down and then the arc follows the drawn apart carbons. I believe that this process occurs also in a vacuum, and that absolute contact is not necessary to start the arc. My experiments lead me to conclude that under very high electrical stress the æther breaks down and becomes a good conductor.

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LII. *On the Effect of Capacity on Stationary Electrical Waves in Wires.* *By* W. B. Morton, *M.A.*[*]

WHILE working recently at stationary electrical waves in wires produced in Blondlot's manner, I was led to make some measurements on the effect produced when a capacity is inserted at a point of the secondary circuit. The positions of the successive nodes were explored in the usual way by a bridge, the indicator being a vacuum tube which was placed across the wires and which showed a maximum of brightness when the bridge was at a node. When two opposite points of the parallel secondary wires were joined to the plates of a small air condenser, the effect was to bring closer together the nodes on the two sides of the condenser, the amount of this shortening of the apparent half wave-length depending on the position of the inserted capacity. The effect was nil when the condenser was at a node, and maximum when it was midway between two nodes. This influence of the increased capacity of the wires is of course of the same nature as the shortening of the wave-length when the wires pass from air into a dielectric liquid. Drude and others have made use of this way of measuring directly the index of refraction of different liquids for the electric waves; but the influence of an isolated capacity does not seem to have been much studied. Salvioni has published [†] some measurements on the effect of capacity inserted at a point between the end condenser and a bridge. When the second condenser was put in it was necessary, in order to restore resonance, to alter the distance of the plates of the end condenser. Von Geitler [‡] got rid of the reflected waves by using a terminal resistance

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[*] Communicated by the Physical Society: read April 9, 1897.

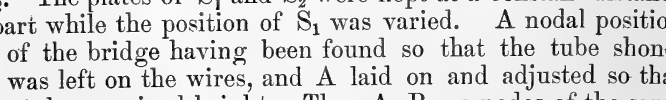
[†] *Rend. Acc. Linc.* 1892, pp. 250–253; Wied. *Beibl.* xvii. p. 485.

[‡] Wied. *Ann.* xlix. pp. 184–195 (1893); *cf.* Barton and Bryan, Proc. Phys. Soc. xv. p. 23.

so that no standing oscillations were formed in the wires. He found that when a condenser was inserted it caused reflexion of the waves, and a series of nodes and loops could be found in front of the condenser. Mazzotto* in a recent research has availed himself of the effect of an isolated capacity to produce a gradual change in the wave-length of the oscillations. He uses pieces of wire hung on to the parallel wires.

The theory of electric waves in wires has recently been treated in an exhaustive manner by Drude†. His method consists in following out in detail the various reflexions undergone at the bridges by a wave-train which starts from the end of the wires. The state of affairs at a point of the circuit is obtained by summation of a series of separate disturbances due to the different direct and reflected trains. The calculations are rather complicated. In obtaining a formula with which to compare my observations I have used a method adapted from some work of Mr. Heaviside's‡. Apart from the actual results obtained, the investigation is perhaps of some interest as showing how easily some problems connected with oscillations in wires can be attacked by this method.

The experiments were made, for the most part, at the end of the parallel secondary wires remote from the oscillator, the arrangement being as shown in the diagram.



C is the end condenser of capacity S_2. S_1 an interposed condenser. The vacuum tube was placed across the plates of S_2. The plates of S_1 and S_2 were kept at a constant distance apart while the position of S_1 was varied. A nodal position B of the bridge having been found so that the tube shone, B was left on the wires, and A laid on and adjusted so that the tube remained bright. Then A, B are nodes of the same system. Owing to the finite length of the bridge the potential-difference at its ends is not quite zero, the true node falling at the centre of the bridge. Accordingly, if the bridge B is taken off the wires the nodes will fall a short distance to the left of B—roughly half the length of the bridge. This distance Drude has called the "bridge-shortening." In

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* *Nuov. Cim.* [4] ii. pp. 296–311 (1895); Wied. *Beibl.* xx. p. 392.

† *Abhandl. der Sächs. Ges. der Wiss.* xxiii. pp. 64–168 (1896); Wied. *Ann.* lx. pp. 1–46 (1897).

‡ 'Electrical Papers,' ii. p. 194 *et seq.*

order to restore maximum brightness in the tube, the bridge A must now be displaced in the same direction through *twice* the "bridge-shortening." The difference in the positions of A, according as B is on or off the wires, gives us therefore a means of finding the correction to be applied to the observed bridge-positions to get the true nodes in the wires.

We want to express that the circuits AB and BC are in resonance. In order to find approximately the period of oscillation of such circuits we can proceed as follows:—Suppose a simple harmonic potential-difference $V_0 \sin nt$ to be kept up between the wires at one end of the circuit, and find an expression for the oscillations produced in the circuit. The amplitude of these oscillations will become infinite when n corresponds to the natural period of the system. A formula has been obtained by Cohn and Heerwagen for a circuit like BC. I have not found any discussion of the circuit AB.

We neglect the resistance of the wires and put S for their capacity and L for their induction per unit-length, both supposed constant. This will be only approximately true as we approach the ends of the circuits. The equations connecting the current C and potential-difference V are

$$-\frac{dV}{dx} = L\frac{dC}{dt}$$

$$-\frac{dC}{dx} = S\frac{dV}{dt},$$

dx being the element of length,

$$\therefore \quad \frac{d^2V}{dx^2} = LS\frac{d^2V}{dt^2};$$

or if V varies as $\sin nt$

$$\frac{d^2V}{dx^2} = -LSn^2V = -q^2V,$$

where $$q = n\sqrt{LS} = \frac{n}{v} = \frac{2\pi}{\lambda};$$

v is the velocity of radiation, and λ the wave-length along free wires;

$$\therefore \quad V = (A\cos qx + B\sin qx)\sin nt.$$

This gives $$-C = \frac{q}{Ln}(A\sin qx - B\cos qx)\cos nt.$$

Take first the circuit BC. Here we have

$$V = V_0 \sin nt \text{ when } x = 0,$$

and $$C = S_2\frac{dV}{dt} \text{ when } x = c,$$

$$\therefore\ A=V_0,$$

and
$$-\frac{q}{Ln}(A\sin qc-B\cos qc)=S_2n(A\cos qc+B\sin qc).$$

Putting
$$\tan\alpha=\frac{LS_2n^2}{q}=LS_2v^2q=\frac{S_2}{S}q$$

we get
$$B=A\tan(qc+\alpha),$$

and
$$V=V_0\frac{\cos\{q(c-x)+\alpha\}}{\cos(qc+\alpha)}\sin nt.$$

The amplitude becomes infinite when $qc+\alpha=\frac{\pi}{2}$,

$$\cot qc=\tan\alpha;$$

$$i.e.\quad \cot\frac{2\pi c}{\lambda}=\frac{2\pi}{\lambda}\cdot\frac{S_2}{S}. \quad . \quad . \quad . \quad . \qquad (1)$$

This is Cohn and Heerwagen's formula.

Taking now circuit AB, suppose the impressed potential-difference to act at A.

Let $V_1=(A_1\cos qx+B_1\sin qx)\sin nt$ between $x=0$ and $x=a$,
$V_2=(A_2\cos qx+B_2\sin qx)\sin nt$ between $x=a$ and $x=b$.

The conditions to be satisfied are:—

$$\text{at } x=0,\quad V_1=V_0\sin nt,$$

$$\text{at } x=a,\quad V_1=V_2,$$

and
$$C_1-C_2=S_1\frac{dV}{dt}$$

$$\text{at } x=a+b,\quad V=0.$$

Putting in the values we get

$$A_1=V_0,$$

$$A_1\cos qa+B_1\sin qa=A_2\cos qa+B_2\sin qa$$
$$=\frac{1}{t}\{(-A_1+A_2)\sin qa+(B_1-B_2)\cos qa\},$$

$$A_2\cos q(a+b)+B_2\sin q(a+b)=0,$$

t has been put for $\frac{qS_1}{S}$.

On solving these equations for the constants we find

$$V_1=\frac{\sin q(a+b-x)-t\sin q(a-x)\sin qb}{\sin q(a+b)-t\sin qa\sin qb}V_0\sin nt,$$

$$V_2=\frac{\sin q(a+b-x)}{\sin q(a+b)-t\sin qa\sin qb}V_0\sin nt.$$

Equating the denominator to zero we find

$$\cot qa + \cot qb = t,$$

or

$$\cot\frac{2\pi a}{\lambda} + \cot\frac{2\pi b}{\lambda} = \frac{2\pi}{\lambda}\cdot\frac{S_1}{S}. \quad . \quad . \quad . \quad . \qquad (2)$$

This formula, which connects the frequency of the oscillation with the position and capacity of the condenser, might have been deduced from formula (1). For we can imagine the capacity S_1 divided into two parts, σ and σ', in such a way that when these parts are attached to the parts a and b of the circuit respectively, the two partial systems oscillate independently with the same frequency. We have thus from formula (1),

$$\cot\frac{2\pi a}{\lambda} = \frac{2\pi}{\lambda}\frac{\sigma}{S},$$

$$\cot\frac{2\pi b}{\lambda} = \frac{2\pi}{\lambda}\frac{\sigma'}{S}.$$

On adding we get equation (2).

In seeking to test the agreement between the theory and the observations, formula (2) was written

$$\frac{\lambda}{2}\left\{\cot\frac{2\pi a}{\lambda} + \cot\frac{2\pi b}{\lambda}\right\} = \frac{\pi S_1}{S} = \text{constant}.$$

A small error in observation of the node position causes an error in the left-hand side very large in proportion, and of amount varying with the position of the condenser in the circuit. In view of this the method adopted was to find the mean value of $\frac{S_1}{S}$ from the observations and, using it, to calculate the values of b corresponding to each a. A comparison of the observed and calculated values of b shows a sufficiently good agreement, the discrepancy being greatest when the condenser is too near a bridge.

One set of observations involving only the circuit AB and formula (2) were taken on a small apparatus at the end near the oscillator. One bridge was kept fixed and the wave-length was the same throughout. Thus only the quantity b was liable to the error in determining the node. The uncertainty in this determination amounted to about 5 millim. The wires were about 15 millim. apart, and the condenser consisted of two small copper strips, 1 centim. by 5 centim., hung on the wires. The half wave-length was 37·5 centim. The following table gives the results measured in centimetres:—

TABLE I.

a.	b. Calc.	b. Obs.
2·0	34·7	33·1
6·0	19·8	17·5
8·0	13·4	12·9
10·0	10·3	10·5
12·0	8·5	8·7
14·0	7·7	7·9
20·0	5·9	5·9
30·0	3·7	3·9
33·0	2·7	3·1

The other observations were made at the end of the wires of a large oscillator in the manner already described. In the calculation both formulæ had to be used. First, a measurement without the interposed condenser gave directly a value of λ for a given value of c. Then from formula (1) by use of these values the constant $\frac{S_2}{S}$ was found. In working up the subsequent observations with condenser S_1 in position, knowing c gave λ from formula (1), then λ, a, b in (2) gave a value of $\frac{\pi S_1}{S}$. Using the mean of such values the calculated b's were got as already indicated.

The parallel wires were about 20 m. long, 2 millim. diameter, and 10 centim. apart. The plates of condensers S_1 and S_2 were of $8\frac{1}{2}$ and 20 centim. diameter respectively. Uncertainty of node position from one to two centimetres.

TABLE II.

Distance of plates of S_1, 0·5 centim.; of S_2, 3·7 centim.

Without S_1, $c=44{\cdot}5$; $a+b=\frac{\lambda}{2}=302{\cdot}5$. Hence $\frac{S_2}{S}=13{\cdot}2$.

c. Obs.	$\frac{\lambda}{2}$. Calc.	a. Obs.	b. Calc.	b. Obs.
45·1	304·6	4·4	299·5	296·0
51·3	326·6	54·9	82·5	77·8
50·7	324·5	79·7	55·3	54·7
50·1	322·4	113·5	43·3	42·9
48·5	316·8	161·5	34·4	35·7
46·3	308·9	224·3	28·1	27·9
44·7	303·2	290·3	9·3	10·1

TABLE III.

The same capacities, a different node-system.

Without S_1, $c=54{\cdot}5$; $\frac{\lambda}{2}=337{\cdot}5$, giving the same value, $\frac{S_2}{S}=193{\cdot}2$.

c. Obs.	$\frac{\lambda}{2}$. Calc.	*a.* Obs.	*b.* Calc.	*b.* Obs.
61·5	360·6	54·9	179·5	169·9
62·9	365·0	79·7	55·3	54·7
62·7	364·4	100·9	65·7	67·3
60·9	358·7	149·1	49·0	50·1
56·3	343·3	278·7	26·4	27·2
55·3	340·0	305·7	19·0	19·9

TABLE IV.

Distance of plates of $S_1=1$ cm., of S_2 unchanged $=3{\cdot}7$ cm. Same node-system as in Table III.

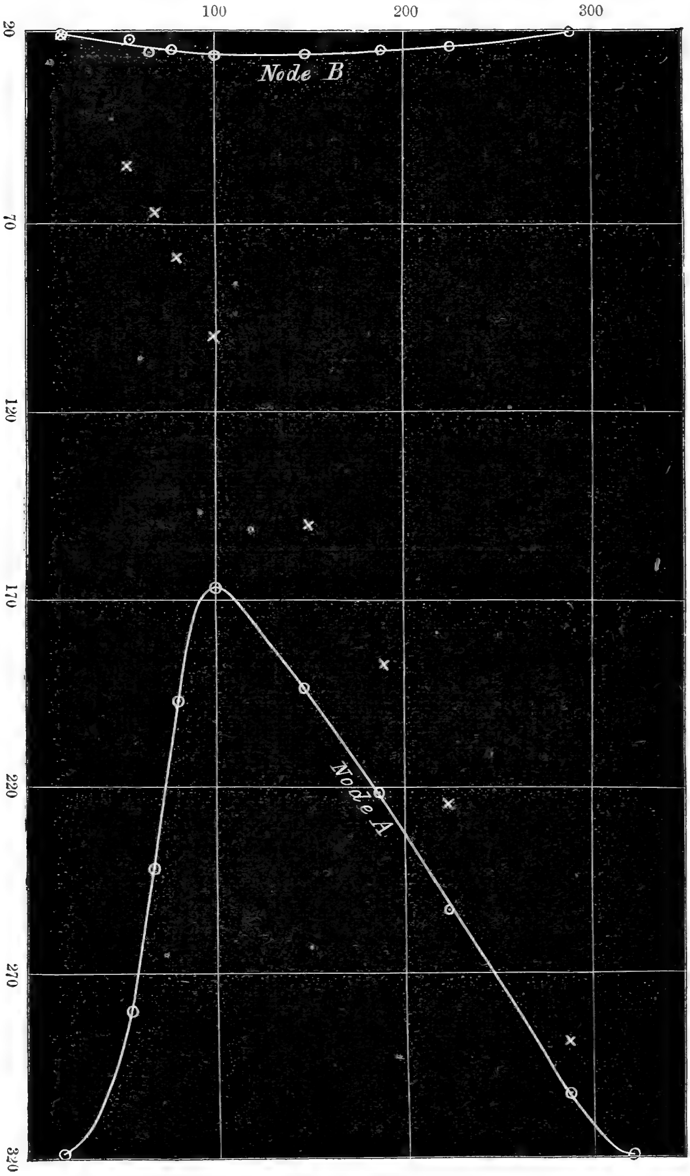
c. Obs.	$\frac{\lambda}{2}$. Calc.	*a.* Obs.	*b.* Calc.	*b.* Obs.
58·1	349·4	83·9	192·8	187·6
58·5	350·6	148·1	110·7	111·5
57·5	347·1	194·5	81·3	81·0
56·5	344·0	240·5	60·0	61·0
55·3	340 0	288·2	36·4	36·8
55·0	339·0	321·3	15·4	15·2

TABLE V.

Distance of plates of $S_1={\cdot}5$ cm., of $S_2=6{\cdot}2$ cm.

Without S_1, $c=60{\cdot}0$; $\frac{\lambda}{2}=300{\cdot}0$; $\frac{S_2}{S}=131{\cdot}4$.

c. Obs.	$\frac{\lambda}{2}$. Calc.	*a.* Obs.	*b.* Calc.	*b.* Obs.
62·8	307·8	27·6	233·1	231·7
65·9	316·4	36·6	186·5	180·0
65·5	315·3	49·1	113·2	122·9
67·0	319·4	68·0	77·2	73·5
66·0	316·7	118·8	48·3	49·7
65·5	315·3	157·1	40·4	40·5
63·9	310·9	196·6	33·7	34·6
60·5	301·4	262·9	19·6	20·8


100
200
300
20
70
120
170
220
270
320
Node B
Node A

The general course of the changes referred to is best seen from the annexed figure, which corresponds to the last series of observations. The ordinates are distances of the inserted condenser from an arbitrary origin near the end of the wires. the abscissas of the points on the two curves are the distances of nodes B and A from the same origin on twice the scale, the crosses between the curves showing the position of the condenser. It will be seen that the apparent half wave-length, or distance A B, is least when the condenser bisects the distance. When the condenser coincides with either node, A B is the full half-wave. When the condenser passes outside A B, the curve of A turns in again.

I have to express my thanks to Prof. Ebert of Kiel, in whose laboratory and under whose kind direction the experiments were carried out of which these observations form part.

Queen's College, Belfast,
27th February, 1897.

LIII. *Notices respecting New Books.*

Grundzüge einer thermodynamischen Theorie elektrochemischer Kräfte. Dr. Alfred H. Bucherer. Freiberg in Saxony: Craz and Gerlach, 1897.

THE dissociation theory of electrolytes has received so much attention during recent years, and the case in its favour has been so ably stated by Arrhenius, Nernst, and Ostwald, that we need to be reminded of the existence of the older chemical or association theory of solution. The author of the present volume expresses his objections to the newer theory, and especially to the way in which it sets aside previously established ideas concerning chemical combination. He indicates how the laws of thermodynamics, so freely used by the founders of the dissociation theory, can be applied with equal success in the development of the older ideas. According to his view, an electrolyte consists of molecules of salt, molecules of solvent, and complex molecules containing both salt and solvent; the relative proportion of the complex molecules increases with increasing dilution and the conductivity of the solution depends upon their presence. The aim of the treatise seems, however, to be destructive criticism of the views of the new school, rather than the construction of a theory in accordance with the requirements of the chemist. J. L. H.

Physics: An Elementary Text-book for University Classes. *By* C. G. Knott, *D.Sc., F.R.S.E.* London: Chambers, 1897.

Some time ago Dr. Knott published an elementary treatise on magnetism and electricity intended chiefly for university medical students in their science year. He now presents a re-issue of that work as a portion of a treatise on experimental physics for junior students, and has added sections on mechanics, heat, sound, and light. The volume is divided into two parts, purely material phenomena being discussed in the first part, which is consequently devoted to mechanics, heat, and sound; while the second part treats of the æther, and includes magnetism, electricity, and radiation. In order to compass the whole subject in the space of 650 small pages much compression and some omissions are, of course, necessary, the effects of which are most obvious in the chapters devoted to heat, and especially in the treatment of fusion and evaporation. Some of the statements require revision; for example, we read on page 229 that "water can be kept liquid at any high temperature by simply heating it in a closed vessel capable of standing the pressure," whereas critical temperature is mentioned and defined on the next page but one.

A collection of exercises is appended to each chapter and is a good feature of the book; its usefulness might, however, be increased by giving the answers to the numerical problems. J. L. H.

Theory of Physics. *By* Joseph S. Ames, *Ph.D.* New York: Harper, 1897.

According to Dr. Ames three things are necessary in the teaching of physics—a text-book, a course of lectures, experimental demonstrations and tutorial classes, and laboratory work by the students themselves. He believes that the demonstrations and laboratory work afford sufficient instruction in the details of experiments, and a separate text-book may in any case be provided for them. As a supplement to the lectures, however, the student requires a text-book in which the facts of the subject and the explanations of them are arranged in logical order: such a book may, in the opinion of the author, relieve the student from the necessity of taking lecture-notes. The present volume has been written in accordance with these ideas; almost all points which would be mentioned in a first-year course are to be found in it, experimental details being altogether omitted. The result is not, as might be expected, a mere cram-book, but a very concise and readable treatise. Perhaps the greatest fault in the book is the suggestion that it should serve instead of lecture-notes, because the taking of notes has an educational value, not only by impressing facts upon the student's memory, but also by training him to observe the relative importance of the various facts and arguments, to which a text-book could only impart differing degrees of prominence by the employment of large and small types, footnotes, and other equally unsatisfactory devices. J. L. H.

Elektricität direkt aus Kohle. *By* ETIENNE DE FODOR. Vienna: Hartleben, 1897.

THE great waste which accompanies the conversion of the chemical energy of coal into electrical energy, by means of the steam- or gas-engine and dynamo, has led many inventors to consider the possibility of directly producing electricity from cheap combustible materials such as coal or carbon. The author gives an account of attempts to accomplish this, classifying them according to the manner in which the carbon has been employed; thermoelectric, thermomagnetic, and hydroelectric machines are thus included, as well as primary batteries having a carbon electrode. J. L. H.

Atomismus, Hylemorphismus und Naturwissenschaft. *By* Dr. A. MICHELITSCH. Graz: Michelitsch, 1897.

Ueber den Urstoff und seine Energie. I. Teil: *Die theoretische Bedeutung der Gesetze von Dulong-Petit und Kopp.* *By* Dr. H. KELLER. Leipsic: Teubner, 1896.

THESE pamphlets both deal with the question of the ultimate constitution of matter. That of Dr. Michelitsch is philosophical and metaphysical in character; he seeks to show by a consideration of the laws of nature that a single primary substance (*Urstoff*) exists, and that the various kinds of matter are merely different forms of it. Matter consists of substance and form: in chemical reactions substance persists, but the old form is destroyed and a new one created, the substance possessing the power of assuming its form, just as the power to form the fruit is contained in the seed. The author objects to the atomic theory of matter on the ground that it does not afford a sufficient distinction between mixture and combination; the rearrangement of atoms during chemical combination merely consists in alteration of their positions in space, and a similar change would take place on mechanically mixing the substances.

Dr. Keller, unlike Dr. Michelitsch, accepts the atomic hypothesis, and finds in the law of Dulong and Petit that the atomic heats of all elements are equal, an argument in favour of the existence of a primary atomic substance. The atoms of all elements consist of an integral number of atoms of the primary substance (*Uratome*), which act as centres of force, and are capable of rotatory or vibratory motion. The temperature is due to the energy of vibration of these primary atoms; the author shows, however, that in gases this atomic vibration determines the velocity of translation of the molecule, the energies of the two kinds of motion being proportional to each other, so that the results of his theory are not inconsistent with the ordinary kinetic theory of gases.

J. L. H.

Anleitung zur mikrochemischen Analyse organischer Verbindungen. *By* H. BEHRENS. Part IV. Leipsic: Voss, 1897.

A PREVIOUS volume of this work on the investigation by means of the microscope of aromatic amines was noticed some months ago; the present work relates to uric acid and urea and their derivatives, and the more important organic acids. J. L. H.

LIV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 315.]

February 24th, 1897.—Dr. Henry Hicks, F.R.S., President, in the Chair.

THE following communications were read:—

1. 'On the Nature and Origin of the Rauenthal Serpentine.' By Miss Catherine A. Raisin, B.Sc.

This serpentine has been already described by Herr Weigand as one of those which occur in regions of gneiss or schist related in their origin to these rocks. In order to test this hypothesis as to the formation of the serpentine, the author has examined the district and has studied its rocks with the microscope.

Herr Weigand asserted that transitions could be recognized from typical gneiss to a peculiar amphibolite, and that the latter rock has been changed to serpentine. The author could find in the field no evidence of a passage from gneiss to amphibolite, and calls attention to the general difficulty of the supposition. She states that when the serpentine is examined microscopically, the greater part shows the usual appearance of serpentine derived from olivine, and can be distinguished from included parts, which have resulted from change in hornblende or other pyroxenic constituents. Further, that several accessory minerals occur which are usually found in peridotites. The rock also contains a peculiar chlorite. This she thinks the result of the modification of a biotite, for the latter mineral occasionally occurs as a constituent in a neighbouring serpentine, and, both there and in the Rauenthal, forms apparently intermediate can be detected. It seems to her that the chemical analyses already published are not in harmony with the supposed change of a hornblende-rock into serpentine.

While it is true that a hand-specimen sometimes shows a transition (generally rapid) from a rock consisting mainly of hornblende to one which has consisted mainly of olivine, she maintains that this appearance is far more consonant with partial differentiation of the original magma, followed by fluxional movements and flow-brecciation. It is not that a mass of amphibolite passes into one of serpentine, but that a specimen of the latter is occasionally streaked by the former.

By comparison with other serpentines of the Vosges and elsewhere, which are admittedly developed from olivine-rocks, so much likeness is found between them and the Rauenthal mass, that the origin must be similar.

2. 'On Two Boulders of Granite from the Middle Chalk of Betchworth (Surrey).' By W. P. D. Stebbing, Esq., F.G.S.

The author notices cases of occurrence of boulders in Chalk which have been previously described; and records the occurrence of two boulders which were obtained from the Chalk of the *Terebratulina-gracilis* zone. The largest weighed 7 lb. 7 oz., measured $5''\cdot8 \times 6''\cdot25 \times 4''\cdot125$, and consisted of decomposed granite; valves of *Spondylus latus* and *Serpula* were still attached. The other, also granite, though of a different character, weighed 3 lb. 12 oz., and measured $3''\cdot6 \times 5''\cdot8 \times 4''\cdot5$. Prof. Bonney has furnished a description of the microscopic characters of the two boulders, which are possibly of Scandinavian origin. The author discusses the mode of transport to their present position, and favours the agency of floating ice.

3. 'Coal: a new Explanation of its Formation; or the Phenomena of a New Fossil Plant considered with reference to the Origin, Composition, and Formation of Coal-beds.' By W. S. Gresley, Esq., F.G.S.

The author argues that the brilliant black laminæ in coal and similar materials to those that form these laminæ, which are found in earthy coals, shales, and clays, point to the former existence of an aquatic plant, having the general shape of the modern *Platycerium alcicorne*, which grew *in situ*. He believes that much coal was formed by this aquatic 'coal-plant,' which grew amongst the mechanical sediments and the débris of the terrestrial vegetation that accumulated on the floors of sheets of water.

March 10th.—Dr. Henry Hicks, F.R.S., President, in the Chair.

The following communications were read:—

1. 'Volcanic Activity in Central America in relation to British Earthquakes.' By A. Gosling, Esq., H.M. Minister & Consul-General in Central America.

The author of the communication points out that the volcano of

Izalco, in the Republic of Salvador, which has been in active eruption for over one hundred years, suddenly ceased to be so within a fortnight of the period at which the communication was sent (Dec. 20th, 1896), and he notes the occurrence of seven shocks of earthquake in England on December 17th, 1896. He quotes remarks concerning the volcano, which were contributed by him to the 'North American Review' in January 1896.

2. 'The Red Rocks near Bonmahon on the Coast of Co. Waterford.' By F. R. C. Reed, Esq., M.A., F.G.S.

The rocks which are considered in this paper have been regarded by some authorities as deposits interstratified with the Lower Palæozoic rocks of the district, while others have maintained that they are of Old Red Sandstone age. It is the object of the author to show the correctness of the latter supposition, and he brings forward evidence to prove that the red rocks rest unconformably upon the Lower Palæozoic rocks, or are faulted against them, and that the breccias of the red rocks contain fragments of the Lower Palæozoic rocks, and also of intrusive rocks which break through the latter. The red rocks also resemble deposits which are known to be of Old Red Sandstone age.

The Old Red Sandstone rocks of the district form an irregular and incomplete elliptical ring around a denuded plateau of older rocks. The incompleteness is due to the concealment of the southern part of the ring beneath the sea; but if the southern part of this ring be as irregular as the northern portion, faulted patches of the Old Red Sandstone rocks may well come in among the older rocks of the cliffs in the positions where the beds which are discussed in this paper occur.

3. 'On the Depth of the Source of Lava.' By J. Logan Lobley, Esq., F.G.S.

The author contends that lava cannot have been brought to the surface from a depth of 30 miles, as fissures which would serve as conduits could not exist at that depth, and, moreover, the lava would be consolidated before it reached the surface, owing to contact with cool rock for a considerable period. He argues that the pressure of the overlying rocks would cause the rocks even at a depth of 10 miles to be practically plastic, as shown by M. Tresca's experiments, and that no continuous fissure could occur in such rocks. Estimates of the volumes of ascending lava-columns were given, with a diagram comparing them with a 30-mile thickness of rocks.

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

[FIFTH SERIES.]

JUNE 1897.

LV. *The Thermo-electric Properties of some Liquid Metals.*

By WILLIAM BECKIT BURNIE, *lately* 1851 *Exhibition Science Research Scholar, Nottingham* *.

THE object of the experiments here to be described was to compare the thermo-electric properties of solid metals with those of the same metals when melted. The four metals employed, tin, lead, bismuth, and mercury, were each thermo-electrically compared with copper, the tested metal being contained in a hard glass tube, so that the observations could be pushed to temperatures considerably above those of the melting metals, and the changes in the thermo-electric properties during the process of melting observed. Two sets of experiments were made, the first set with the greater part of the metal under test at ordinary temperatures, and the second set with all the metal under test at high temperatures.

In the first set of experiments with tin, lead, and bismuth, the glass tube containing the metal was W-shaped, the metal filling the central part, but only rising about one-third of the height in the outside limbs. To fill this tube one end was dipped into a crucible full of the melted metal, which was then allowed to cool. The crucible and tube were immersed in a bath of linseed oil, which was raised to a temperature above that required to melt the metal. When the metal was quite liquid, air was withdrawn from the upper end of the tube till the metal had risen to the right height. The tube was next slowly withdrawn from the still hot oil, to prevent

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* Communicated by the Physical Society: read Feb. 26, 1897.

cracking due to the freezing of the metal, and allowed to cool. The superfluous metal was then melted out of the open limbs. The two copper-metal junctions were to be in the two open limbs of the tube, and a number of thick copper wires were so arranged round the outside of the tube as to conduct away the heat from all parts not in the immediate neighbourhood of the hot junction. By this means all the metal except a small portion was kept solid, and the level of the melted metal was always higher in the open than in the closed limb. This was necessary, as the solid metal was not a satisfactory cork for the tube, and a slight liquid pressure was needed to keep good contact between the liquid and solid metal.

The temperature-differences between the hot and cold junctions were measured with a thermo-element of platinum and a platinum-rhodium alloy (Pt 90 per cent., Rd 10 per cent.). This thermo-element was calibrated between the temperatures of 0° C. and 263°·5 C. by comparison, in linseed oil, with a Reichsanstalt standardized thermometer. The calibration curve was then extrapolated as far as 430° C. by means of an equation. For reasons which will be explained later, it was useless to adopt any more accurate method than this for the first set of experiments. The thermo-element was insulated within and from without with asbestos paper, and each junction was bound to one of the standard copper wires. These copper wires were also insulated with asbestos, so that only their ends, which were in close proximity to the junction of the temperature-measuring thermo-element, were uncovered.

Some experiments were made to determine the electrical insulating properties of asbestos paper at high temperatures, and it was found that the errors introduced by the electrical conductivity of the supposed insulator were, up to 600° C., negligibly small. Other experiments were made to determine whether or not the temperature of the junction in asbestos differed greatly from the temperature of the metal surrounding the asbestos. One junction of the temperature-determining thermo-element was immersed in melted lead, and the other in melting ice. The parts of the wires just outside the lead were alternately heated with a burner, whereby the temperature-difference between the junction and the metal would be decreased, and cooled with an air blast, whereby the temperature-difference would be increased. Readings of the temperature-difference between the two junctions were meanwhile taken ; and it was found that by varying the temperature of the leading-in wires the temperature of the hot junction was not altered by more than $\frac{1}{10}$ of a degree. As the leading-in wires were hotter, when warmed by the flame, than the melted lead, $\frac{1}{10}$ degree was greater than the greatest error introduced during the experiments by the heat-insulating capability of the asbestos paper.

Each of the copper wires with its thermo-element junction was melted into the metal in its respective limb, and held in the centre of the tube by asbestos paper. One limb of the W was heated in a sand bath, and the other was kept cold in melting ice.

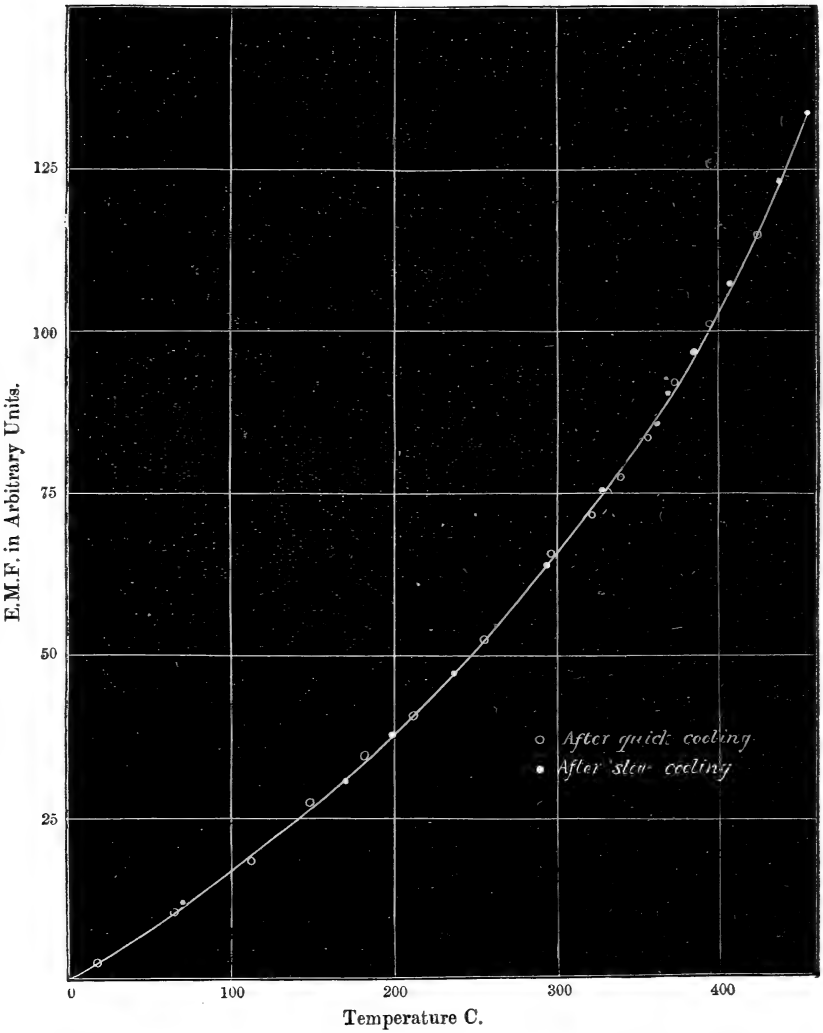
The two standard copper wires and the two free wires from the temperature-determining thermo-element were led to a paraffin switch-block, so that either pair could be connected, through a reversing key and a resistance, to a reflecting galvanometer. This resistance was so great as to render no correction necessary for the resistance variations, due to heat, in the circuit. For each reading the temperature of the hot junction was made nearly constant; then, using each thermo-electric combination alternately, galvanometer deflexions were noted with the current direct, and the current reversed. After the necessary correction, the mean of the readings of the platinum-platinum-rhodium thermo-element gave the temperature, and the mean of the readings of the thermo-element under test gave the E.M.F. in arbitrary units. After each complete experiment, the constant of the temperature-measuring thermo-element was determined by removing the crucible containing sand, putting in its place one containing melted lead, and allowing the lead to cool, meanwhile taking readings.

The curves for copper-lead, copper-tin, and copper-bismuth, which were obtained by this method, are given in figs. 1, 2, and 3 respectively. The abscissæ are temperatures and the ordinates E.M.F.'s in arbitrary units. These curves will be discussed when the mercury experiments have been described.

The W-shaped tube was not suitable for the application of a freezing-mixture, and so in the mercury experiments the junctions were each in a small test-tube about half filled with the metal. An inverted U-tube also full of mercury, with one end dipping into the metal in each of the test-tubes, connected the two. One of the test-tubes was surrounded by melting ice, and the other was cooled with a mixture of solid carbon dioxide and ether. A mass of copper wire was wrapped round the tube to be cooled, in order that it might warm up slowly after the evaporation of the carbon dioxide.

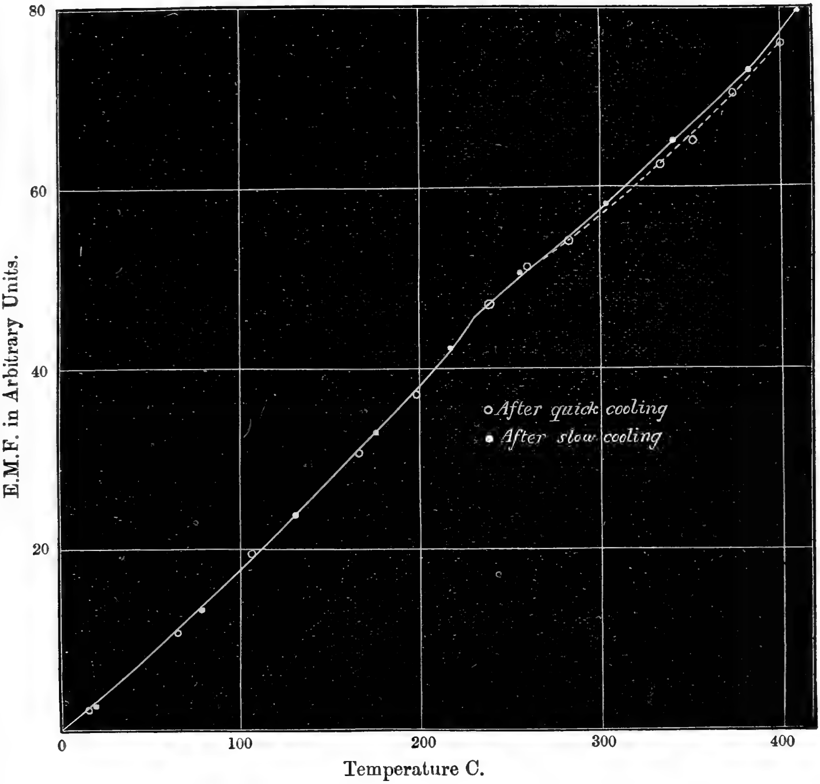
As the temperatures to be measured approached the neutral point of the thermo-element used in the last experiments, an iron-constantan thermo-element was employed to measure

Fig. 1.—Copper-Lead Thermo-element.



the temperature-differences between the two junctions. This thermo-element was calibrated by the observation of the galvanometer deflexions when one junction was in melting ice and the other junction first in melting mercury and then in boiling carbon dioxide and ether. As these two deflexions were nearly proportional to their respective temperature-differences, the calibration curve equation was assumed to be of the form, deflexion $=at+bt^2$, where t is the temperature, and a and b constants. These two constants were determined from the two observations, and the calibration curve for the thermo-element was plotted from the equation. The element and the standard copper wires were insulated as in the last experiment, and bound to the glass tubes dipping into the mercury. The freezing-mixture was contained in a double-walled test-tube, the inner and outer walls being separated by cork distance-pieces, and the whole being wrapped in flannel. The electrical connexions were the same as in the last experiment.

Fig. 2.—Copper-Tin Thermo-element.



In the experiments, the tube containing the cold junction was cooled as far as possible with the freezing-mixture, and allowed to warm slowly up. Readings of the galvanometer were meanwhile taken every thirty seconds, as follows:—

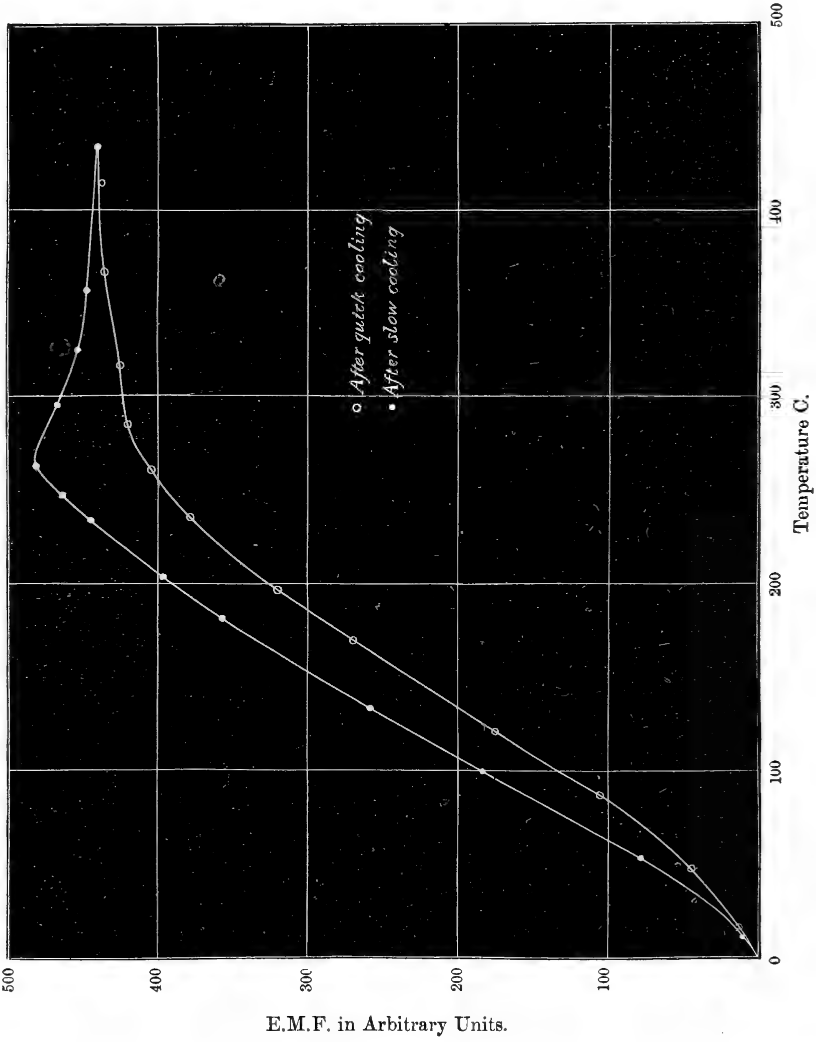
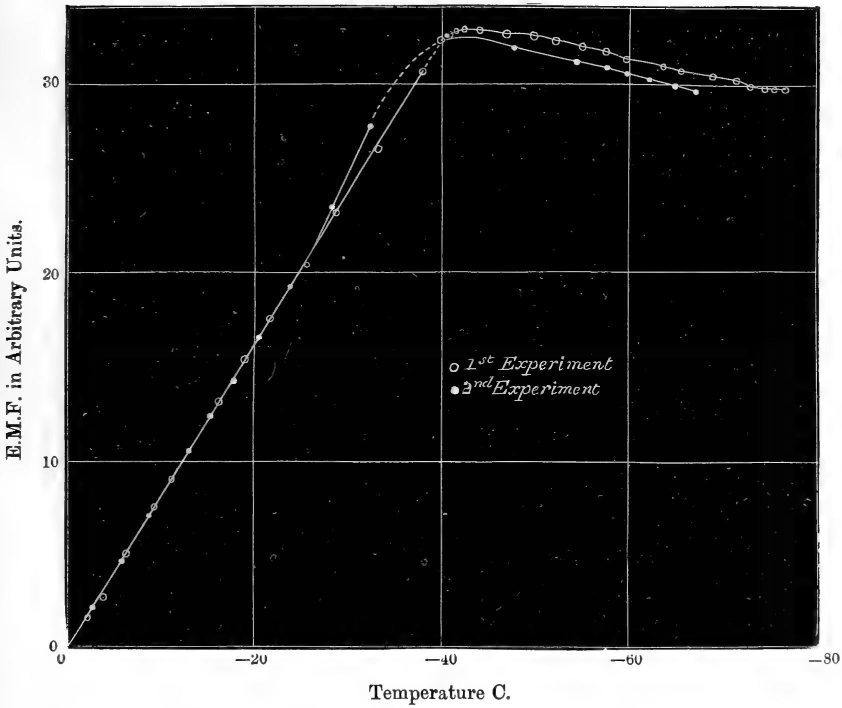


Fig. 3.—Copper-Bismuth Thermo-element.

First, a reading from the temperature-determining thermo-element with the current direct; then a reading from the same element with the current reversed; then a reading from the copper-mercury element with the current direct; and lastly, a reading from this element with the current reversed. The cycle then began again. From the means of each pair of readings, reduced, two curves were plotted, one giving the relation between temperature and time, and the other giving the relation between E.M.F. in arbitrary units, and time.

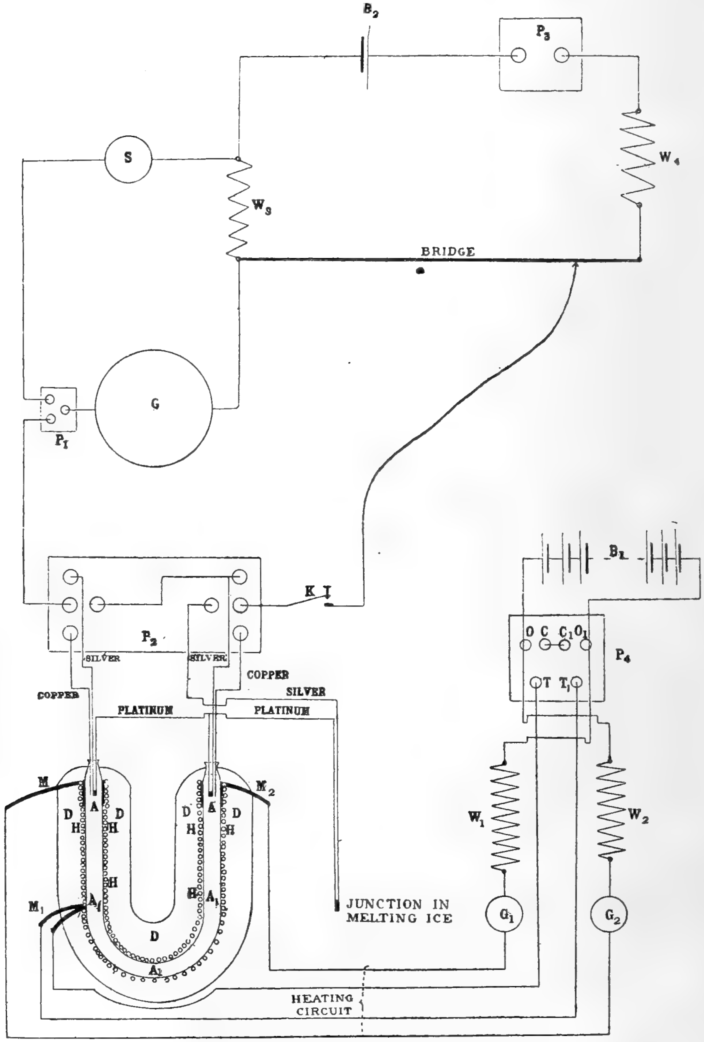
The curves (fig. 4) give the relation between E.M.F. and temperature deduced from these two curves, for two experiments.

Fig. 4.—Copper-Mercury Thermo-element.



In figs. 2, 3, 4 two curves are in each case given. These two curves were obtained from the same metal. The effect is most marked with bismuth (fig. 3). At the conclusion of one experiment the apparatus was cooled very rapidly by removing the sand and blowing air on to the tube containing the melted metal. Another set of readings was then taken, with the temperature of the hot junction rising, and the lower of the two curves in fig. 3 was obtained. At the conclusion of this experiment the apparatus was cooled very slowly, about two hours being occupied, and the result of the next experiment was the upper curve. It was found possible, moreover, with intermediate rates of cooling to produce intermediate curves. This effect is probably due to the

Fig. 5.



variations in the crystalline structure of the metal under test, dependent on the rapidity of its solidification; and it may be that the various thermo-electric properties which have been observed for different pieces of bismuth are largely due to this same cause. With tin the effect was less marked, and with lead it was unnoticeable. In the mercury experiments it was not possible to regulate the rate of cooling; but the two most widely differing curves obtained are given in fig. 4.

As this effect of its previous treatment upon the metal rendered accuracy impossible, the experiments were repeated with three of the metals, viz. tin, lead, and bismuth, with a method so arranged that the whole mass of the metal to be tested was in a molten state, and thus homogeneous.

The apparatus is shown in fig. 5. The metal was contained in a U-tube A A_1 A_1 A, and was heated by a current of electricity in german-silver wires H H H. The metal was introduced into the tube by means of a glass pipette, both tube and pipette being warmed in a bunsen flame. The part of the glass tube where the junctions were to be, A A, was covered with copper to render the heat distribution more uniform. The german-silver wire was insulated with asbestos paper, and wound spirally round the tube in two sections, M M_1, M_1 M_2. Over this were wrapped several layers of asbestos paper and asbestos string, DD. The connexions of the heating circuit are shown in fig. 5. In the paraffin switch-block, P_4, if T C and T_1 C_1 were connected, the same current from the heating battery passed through both ammeters, G_1 G_2, both resistances, W_1 W_2, and both sections of the heating coil. This caused a saving in current when it was merely required to melt the necessary connexions into the metal, or to burn out the asbestos insulation. If, however, T O was connected the current passed through the ammeter G_1, the resistance W_1, and the long section of the heating coil; and if T_1 O_1 was connected the current went through the other ammeter, resistance, and heating-coil section. By adjusting the resistances W_1 and W_2 the temperatures at the junctions could be varied as required. The diameter of the german-silver wire was 0·5 mm., and the largest current required was 4 amperes. The U-tube is shown about one half actual size.

For measuring the temperatures a platinum-silver thermo-element was employed, as this gave E.M.F.'s of the same order of magnitude as those to be measured. This element had three junctions, as shown in fig. 5, so that either the temperature of one of the junctions or the temperature-difference between the two junctions of the thermo-couple under test could be measured. The thermo-element was calibrated with linseed oil up to 288° C.; and one point on the calibration curve, at 441°·4 C., was determined with sulphur vapour. Each limb was calibrated separately, but they were found to be similar. The part of the calibration curve from 288° C. to 430° C. was plotted from the equation :—

$$\text{E.M.F. (in volts)} \times 10^5 = 0{\cdot}1407\, t + 0{\cdot}001227\, t^2.$$

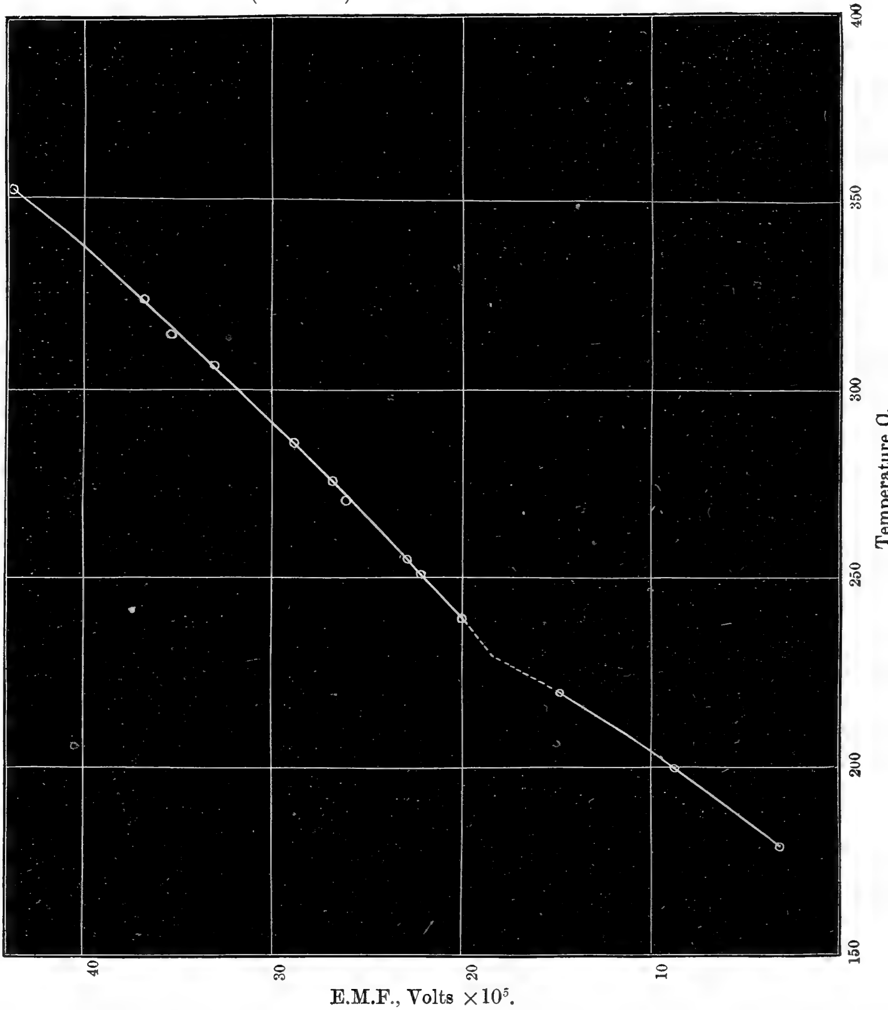
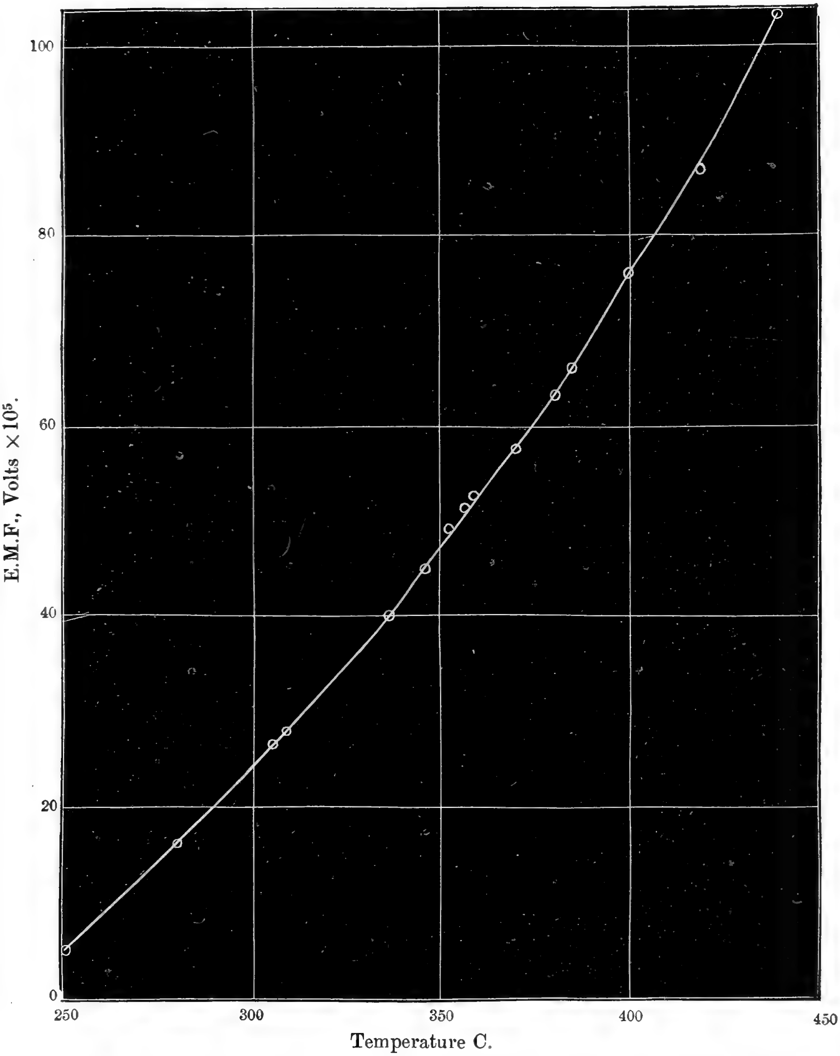


Fig. 6.—Copper-Tin Thermo-element.

This equation was obtained from the point on the curve determined with sulphur, and the point where $t = 220^\circ{\cdot}7$ C. on the curve as drawn in between 0° C. and 288° C. By the application of the method of least squares to all the calibration observations a somewhat different equation was obtained, viz. :—

$$\text{E.M.F. (in volts)} \times 10^5 = 0{\cdot}1463\, t + 0{\cdot}001238\, t^2 ;$$

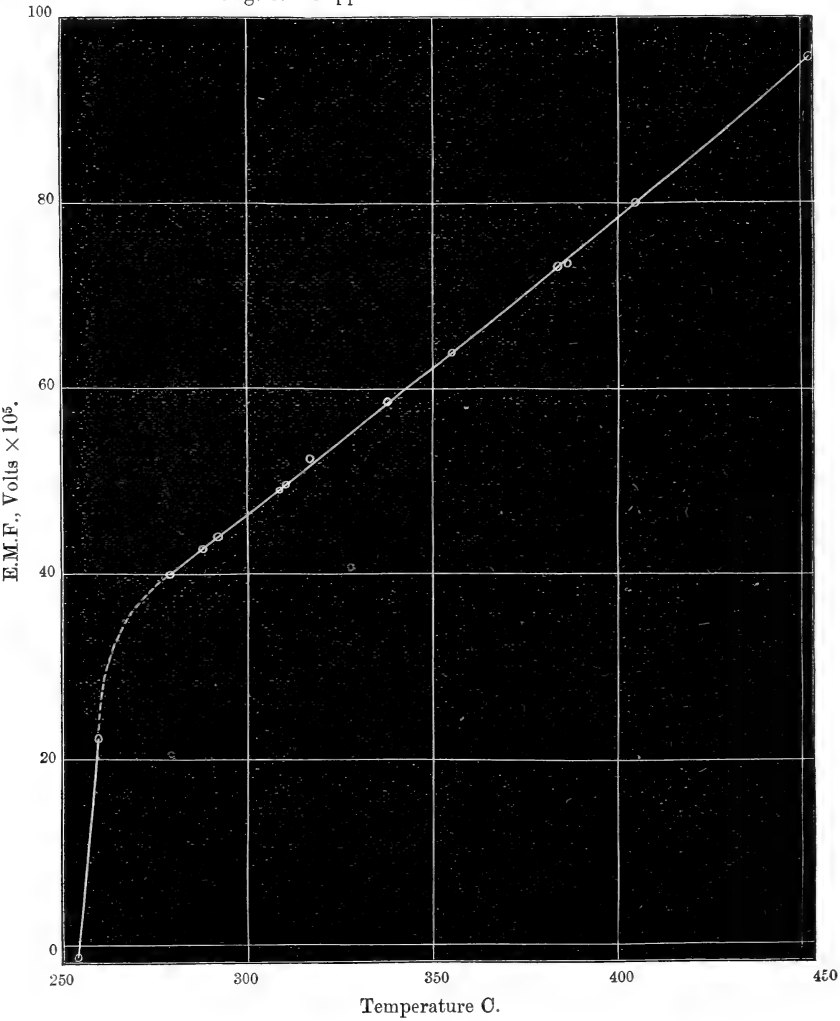
Fig. 7.—Copper-Lead Thermo-element.



but as the calibration curve was not exactly a parabola the upper equation was judged the better.

The junctions and standard copper wires were insulated as in previous experiments and melted into the metals. The two standard wires and the three free wires from the temperature-determining thermo-element were led to a paraffin switch-block, P_2, which was connected to an arrangement of

Fig. 8.—Copper-Bismuth Thermo-element.



a galvanometer, G, a standard cell, S, a key, K, a bridge battery, B_2, resistances, W_4 and W_3, switch-blocks, P_1 and P_3, and a metre bridge, as shown in fig. 5, for measuring the E.M.F.'s.

The current in the long heating section was adjusted before each reading till the temperature of the fixed temperature junction was at a certain point, slightly above the melting-point of the tested metal. Readings were taken with the temperature of the variable temperature junction both above and below the melting temperature of the metal. With tin and lead this latter was easy, but with bismuth, owing to the crystallization effect before mentioned, the readings were rather uncertain. Above the melting-point, however, they were perfectly constant.

The curves plotted from the results of the experiments by this method are given in figs. 6, 7, and 8 respectively.

In each of these curves we see that in a small variation of temperature, about the melting-point, there is a considerable change in the direction of the thermo-electric curve. The effect is smallest with lead, with tin it is larger, and with bismuth it is very remarkable; that metal changing, during melting, from an exceedingly active thermo-electric metal to one very similar to lead in its thermo-electric properties. With mercury also (fig. 4) we see that a great change takes place at the melting-point. This indicates that there is a difference between the Peltier effect for the solid and for the melted metal. But the change in curvature is not sufficiently marked for us to be able to say whether the specific heat of electricity remains proportional to the absolute temperature. To decide this point a direct determination of the Thomson effect is needed.

The worked-out observations from which the curves in figs. 6, 7, and 8 were plotted are given below.

Copper-Tin Element. (Fig. 6.)

Temperature of constant temperature junction 239° C.

Temperature of variable temperature junction.	E.M.F. in volts × 10^5.
255·8	2·86
276	7·01
286·1	9·0
306·1	13·2
323·2	16·92
353·7	23·63
251	2·0
272·2	6·35
315·1	15·45
219·7	−5·1
200·1	−11·15
179·5	−16·86

Copper-Lead Element. (Fig. 7.)

Temperature of constant temperature junction 336°·1 C.

Temperature of variable temperature junction.	E.M.F. in volts × 10^5.
346·1	5·215
357	11·13
370·6	17·92
385·2	26·5
400·5	35·86
418·3	47·4
440·3	63·2
359·1	12·25
352·8	8·5
380·2	23·75
310·3	−11·9
307·3	−13·4
280·5	−23·6
251	−35

Copper-Bismuth Element. (Fig. 8.)

Temperature of constant temperature junction 278°·5 C.

Temperature of variable temperature junction.	E.M.F. in volts × 10^5.
287	2·6
291·4	3·97
308	9·01
315·7	12·05
356·7	24·5
385·1	33·68
405·9	40·15
453·6	56·25
388·3	34·18
339·2	18·94
309·2	9·73
259	−18·01
254·6	−41·2

In each case a constant has been added to the E.M.F.'s before plotting.

These experiments were conducted in the Physical Laboratory of the Eidgenössisches Polytechnikum, Zürich, under the supervision of the director, Prof. Dr. H. F. Weber, to whom my best thanks are due.

LVI. *On the Photography of Ripples.* *By* J. H. VINCENT, *B.Sc., A.R.C.Sc., Assistant Demonstrator in Physics at the Royal College of Science, London, S.W.**

[Plates I.–III.]

MANY of the phenomena described in this paper have been exhibited at public lectures by Mr. C. V. Boys, using the stroboscopic method, which was first applied to the study of ripples by Lord Rayleigh. I am indebted to Mr. Boys for having recommended to me the work of photographing these effects, and also for many valuable suggestions.

Lord Kelvin defines a ripple as a wave whose length is less than that of the wave which is propagated with the minimum velocity. For ordinary mercury, waves less than 1·3 centim. long are ripples. Capillary ripples are those whose length is so small as to render negligible, in the value of the velocity squared, the term due to gravity. These definitions are rendered clearer by reference to Mr. Boys's Logarithmic Wave Chart. The portion of the curve, representing the relation of the velocity and wave-length, to the left of the point of minimum velocity, refers to ripples. The straight-line portion to the left of the chart represents capillary ripples.

In order to obtain ripples it is necessary to use vibration-frequencies above a certain value. Thus in the case of ordinary clean-looking mercury, with its damp and probably greasy surface, the surface-tension of which may be between 300 and 400 C.G.S. units, a frequency of about 15 per second causes the biggest waves which the above definitions include as ripples; while frequencies of about 200 and upwards give rise to waves whose propagation is practically controlled by surface-tension, and these waves are capillary ripples.

Now the duration of the sensation produced by a luminous impression on the retina lasts for about one eighth of a second; thus we are unable to see ripples on the surface of mercury. The frequencies employed are generally many times the maximum visible frequency. It is not the high velocity of propagation which renders ripples invisible; ripples produced by a disturbance of a frequency of about 200 do not travel very quickly, a foot a second being about the order of magnitude of the velocity on the surface of ordinary mercury.

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* Communicated by the Physical Society: read Feb. 26, 1897.

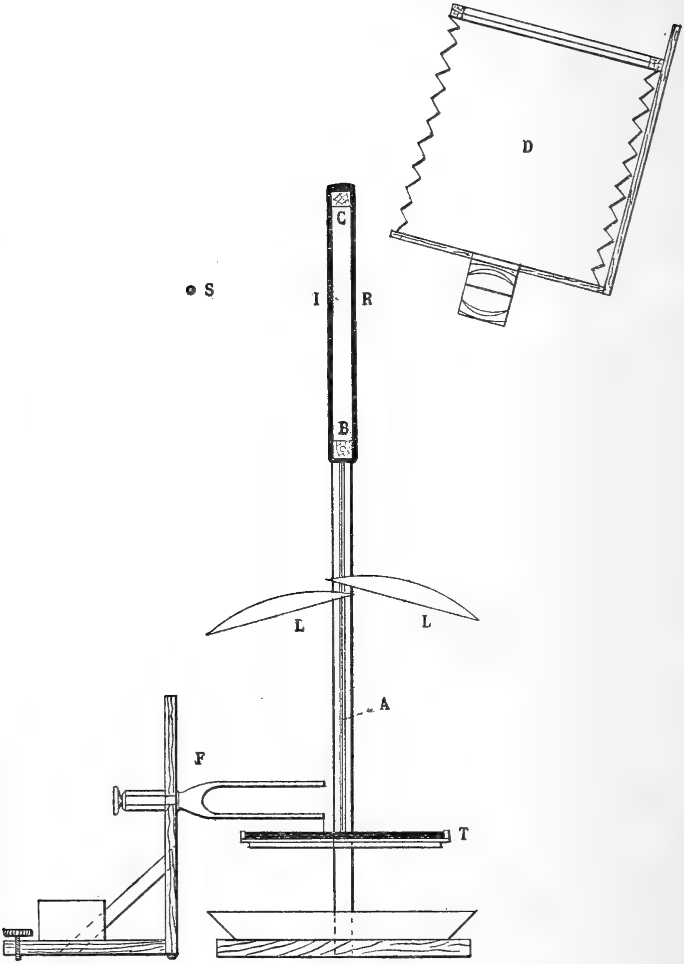

Diagram of Apparatus. $\frac{1}{10}$ nat. size.

T. Mercury trough.
A. Standard of retort-standard.
B. Wooden block through which passes the bent down portion of A.
C. Upper bar of wooden stand.
IR. India-rubber loop.
S. First spark-gap.
LL. Lenses.
D. Camera.
F. Tuning-fork and stand.

Description of the Apparatus.

A rectangular wooden trough, about 1 centim. deep and 12 × 15 centim. area, contains the mercury upon the surface of which the ripples are produced. This trough rests upon the rectangular base of a retort-stand, the upper portion of whose standard is bent at right angles over the base so as to be parallel with the latter. This horizontal portion is passed through a rectangular block of wood, and the whole is then slung from a gallows-like wooden structure one metre high.

The spark-gap from which the light proceeds to illuminate the mercury surface is placed near the top and to one side of the wooden stand. Light from this gap falls upon a lens to the left of the stand, so that the emergent light is parallel; after reflexion the light is collected by a second lens similar in all respects to the first, so that an image of the spark would be produced at the primary focus of the second lens; the focal length of these lenses was 44 centim. The camera is placed in such a position as to enable the first achromatic lens of the combination to collect the rays and converge them so as to come to a focus at the aperture in the lens stop. The stop used was the smallest of the set belonging to the camera (F 64). The camera is then focussed upon a fine thread laid on the surface of the mercury.

In addition to the first spark-gap, which was about ·5 centim. across, a second gap was used in order to increase the brightness of the spark in the first gap. The second gap was varied from time to time, but was generally 1·5 centim. across. The first gap was shunted by a piece of stout thread soaked in calcium-chloride solution. This prevented small sparks due to induction. The knobs of a Wimshurst machine were connected, one with a terminal of the second spark-gap and the inside coats of a battery of four half-gallon leyden-jars; the other Wimshurst terminal was connected to the outer coats, one side of the first spark-gap, and to earth. A wire joining the other terminals of the two gaps completes the spark arrangements.

Method of Causing the Ripples.

The ripples are due to the agitation of the surface of the mercury by a style of glass attached to one prong of a vibrating tuning-fork. The fork was in most cases struck with a rubber-shod hammer; but in the last two experiments, in which a strip of cover-glass attached to the fork acts as a line-source, it was found that the irregular large waves caused by the concussion entirely masked the phenomena which it was sought to photograph. In these cases the fork was maintained in synchronous vibration with another similar fork which was electrically excited. The two forks, placed approximately parallel, are tied together by a piece of thread about two feet long, so that the thread is at right angles to both forks. This thread is then adjusted to an appropriate tension by trial, when the maintained fork causes the other to vibrate for any length of time. This simple device, which has been used in the Royal College of Science laboratory for some time, is due to Mr. W. Watson.

Description of Photographs.

The plates used were of various kinds, and any of the well-known plates gave good results. Some of the negatives were intensified previous to printing. The figures are about $\frac{1}{2}$ natural size. My best thanks are due to Mr. R. Chapman, who has assisted me throughout with great zeal.

Photographs showing a series of circular waves set up by a single style attached to a fork of known frequency were taken with a view to quantitative measurements of surface-tension, wave-length, and velocity. A bar of wood was so placed that two needle-points which it carried nearly touched the surface of the mercury. These points are a known distance apart, and by measuring the negatives we may find the scale of reduction of lengths along this line. The particulars of the motion are obtained from the equations

$$v^2 = n^2\lambda^2 = \frac{g\lambda}{2\pi} + \frac{2\pi T}{\lambda\rho};$$

from which we find, in a particular experiment,

$$n = 180 \text{ per second,}$$
$$\lambda = {\cdot}165 \text{ centim.,}$$
$$v = 29{\cdot}7 \text{ centim. a second,}$$
$$T = 306 \text{ dynes per linear centim.}$$

This low value of the surface-tension was obtained from mercury which had stood in the apparatus for some days. The values obtained in a similar way previously were 420, 421, 365. The tension falls as the mercury gets more contaminated. The value of the surface-tension of pure dry mercury is usually quoted at 540.

Fig. 1. Two styles are attached to the same prong of a fork, the frequency of which is 120. One centre is unfortunately hidden by the fork. The approximately straight dark lines which are seen to radiate from the region between the centres of oscillation are lines of minimum disturbance; they are hyperbolas of which the centres of disturbance are the foci. This photograph illustrates the interference phenomena in Optics produced by Young's or Fresnel's methods.

Fig. 2. The frequency of the fork is 256. Both styles are

attached to the same prong. The photograph shows two series of interference-curves, one a family of hyperbolas analogous to those shown in fig. 1, and the other a family of ellipses.

The hyperbolas are the radiating light lines seen on the side of the photograph remote from the fork. They are fixed in position, the little dark facets moving along between pairs of hyperbolas.

The light oval curves in the region between the centres of disturbance are ellipses, since they are the loci of points of intersection of the two series of circles whose radii grow uniformly, and at the same rate with time. Their method of production here is similar to a well-known geometrical construction for ellipses. Unlike the system of hyperbolas, these ellipses are not at rest. They travel outwards in such a way that any ellipse occupies a position which was filled previously by its predecessor a whole period before. That semiaxis of any ellipse which passes through a centre of disturbance grows with the same velocity as that with which the ripples are propagated. The other semiaxis grows with a velocity which is infinite at the commencement, but which gradually decreases to the same uniform velocity of growth as that of the first. The law of decrease of velocity is the same as the law of decrease of the lengths of whole-period elements of a linear wave with respect to a point.

In order to render these ellipses stationary it would be necessary to change one of the sources into a sink to which the circular waves converge. This could be experimentally realized with ripples by causing a circular arc and a style to be agitated by the same prong of a fork, when the effects would be analogous to M. Meslin's experiment in Optics.

Fig. 3. Frequency 256.

This photograph is very similar to fig. 1; but in addition to showing interference phenomena like those of Fresnel and Young, it also illustrates interference effects in which the direction of propagation of light is parallel to the line joining the point-sources. Thus in the photograph, if we consider the disturbance anywhere on a right line drawn perpendicular to a line joining the two point-sources produced, we see that the places of no disturbance are symmetrical about the line joining the sources. They are points on the system of hyperbolas already mentioned.

In M. Meslin's method of producing interference-fringes the screen is placed between the two point-centres, one a source and the other a sink. The bands are circular, and are sections of ellipsoids of revolution, and not of hyperboloids, such as the fringes in the photograph would become if the whole picture were rotated about the line joining the point-sources. These fringes are not seen on the screen in M. Meslin's experiment with the split lens, even when the screen is placed beyond the second focus, because the pencils do not there overlap. It seems that modifications of M. Meslin's experiment could be devised so as to enable complete circular fringes to be seen, and also to render the sections of hyperboloids visible. For example, it appears probable that if a circular portion of a convex lens were cut out and the central portion moved towards the original point-source, the sections of hyperboloids of revolution would be visible on a screen placed beyond the second focus.

Fig. 4. The two sets of ripples are produced by a fork of frequency 128 and another of frequency about 112. These two forks then produce 16 beats a second. The curved light lines represent places of minimum disturbance at the instant when the spark occurred. These lines are not stationary as in No. 3, but rotate towards their convexities. The centre of disturbance from which they move is the one of higher frequency. If we consider a point anywhere on the surface of the mercury, beats occur at that point with the same frequency as the passage of these lines of minimum disturbance takes place over the point. Thus, 16 of these lines cross any point per second.

Fig. 5. This shows ripples produced by two forks, the higher of which has a frequency four times as great as the lower, the frequency of which is 128. If we neglect the effect of gravity,

$$v^2 = n^2\lambda^2 = \frac{2\pi T}{\lambda\rho},$$

from which it follows that the wave-length of the ripples from the higher fork should be half that due to the lower. This relation is approximately true for these ripples.

Fig. 6. Frequency 180.

This photograph shows a point-source and a reflecting-line, the latter is a side of a triangular piece of microscope cover-glass, which is kept in position by a small splinter of wood. The interference-lines which are shown are due to the mutual action of the primary and the reflected waves. The phenomena exhibited are analogous to Lloyd's single-mirror fringes in Optics.

Faint signs of diffraction invading the geometrical shadow of the obstacle can be seen. The region of shadow is covered by faint lines parallel to the nearest side of the triangle acting as a line-source. The wave-length is the same as that of the primary waves, and the effect is due to forced vibrations.

Fig. 7. This photograph illustrates reflexion and forced

vibrations. The light curved lines in the region between the source and the nearest side of the triangle are similar to those between the two sources in fig. 2. They are due to the interference between the source and its virtual image.

Fig. 8. Frequency 256.

Here we have a shallow circular reflector with the source placed approximately at the principal focus. The reflected waves are circles of large radii; the very slight outward curvature at the ends shows that even when the reflector has an arc of about 60° the effect of spherical aberration is small. Since the reflected waves come from a virtual point-source, we have, as interference-lines, a series of confocal ellipses and confocal hyperbolas; the latter are fixed, but the former travel away from the line joining the source and its image. If the reflected waves had been rectilinear, both these sets of curves would have become parabolas.

Fig. 9. Frequency 256.

The centre of disturbance here coincides very closely with the principal focus of the central portion of the semicircular reflector. The reflected ripples are straight lines in the middle, but are bent outwards from the reflector towards the ends. This illustrates spherical aberration.

Fig. 10. Frequency 256.

The obstacle is a small round cover-slip floating on the mercury. The ripple-shadow is slightly encroached upon by the waves bending round the edge of the obstacle. One side of the disk acts as a convex circular mirror, and the interference-fringes are due to the mutual action of the source and its virtual image situated within the circumference of the disk.

Fig. 11. The frequency in this and fig. 12 is 120.

Here straight-line waves are originated by the agitation of a slip of cover-glass, one side of which dips into the mercury. The waves are reflected from the shallow circular mirror, and converge to the principal focus. Two series of parabolic interference-fringes are shown. They are confocal, and have their concavities directed towards the source and reflector respectively.

Fig. 12. Similarly excited waves are reflected at an angle of about 45° from a straight edge. The long black mark running from one end of the dipping edge to the corner of the print is due to a depression in the surface caused by a floating needle, put there to screen off the circular waves coming from the end of the strip of glass.

Diffraction is well shown in this photograph.

LVII. *Conductance produced in Gases by Röntgen Rays, by Ultra-Violet Light, and by Uranium, and some consequences thereof.*

By J. Carruthers Beattie, *D.Sc., F.R.S.E.,* 1851 *Exhibition Scholar, Research Fellow of the University of Glasgow, and* M. Smoluchowski de Smolan, *Ph.D., Research Fellow of the University of Glasgow*[*].

§ 1. WE propose in the following paper to give the results of experiments carried out by us at Lord Kelvin's suggestion, and with his help, in the Glasgow University Physical Laboratory. We shall give first results which relate to the conductance produced in gases by Röntgen rays, by ultra-violet light, and by uranium. Secondly, results bearing on the quasi-conductance produced in solid dielectrics by Röntgen rays. Thirdly, we shall give an account of experiments which we made to measure the difference of potential between wires of the same metal connected metallically with two plates of any two metals between which Röntgen rays, ultra-violet light, or "uranium rays" pass.

§ 2. *On the Conductance produced in Air,* at ordinary pressure and at different voltages, by Röntgen rays, by uranium, and by ultra-violet light.

To measure the conductance produced in air by Röntgen rays and by uranium, we used an arrangement consisting of two quasi leyden-jars, A and B, with their inside coatings connected together. The outside coating of A was connected to the case of a quadrant electrometer, the outside coating of B, which was insulated on a block of paraffin, to the insulated terminal of the electrometer (see fig. 1).

In all the experiments in which the two-leydens arrangement was used the leyden B remained the same. It consisted of a cylindrical lead can, 25 centim. long, 4 centim. diameter. A metal bar about 1 centim. diameter, 25 centim. long, was supported centrally on paraffin filling the whole space between the metal bar and the containing lead. The metal bar was connected by a wire to the internal coating of A. To protect this wire from inductive effects it was surrounded by a tube of lead connected to the case of the electrometer.

In the experiment with Röntgen rays the leyden A con-

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[*] Communicated by Lord Kelvin, having been read before the Glasgow Philosophical Society, April 14th, 1897. The chief results were described in several papers communicated to the Royal Society of Edinburgh since the beginning of the present year.

sisted of an aluminium cylinder, 16 centim. long, 3 centim. in diameter. This cylinder was connected to the case of the electrometer. The insulated metal inside it, which was a flat strip of aluminium about 10 centim. long and $1\frac{1}{2}$ centim.

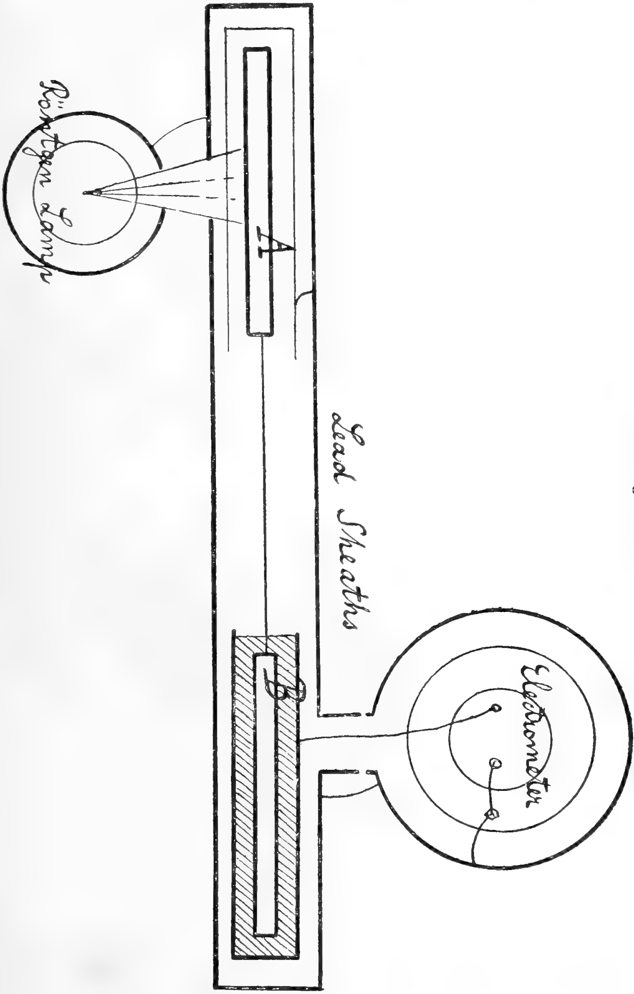

Fig. 1.

[From 'Nature,' Mar. 25, 1897.]

wide, cut from the same sheet as the surrounding aluminium tube, was supported at one end by a small piece of paraffin so placed as to be out of reach of the action of the Röntgen lamp[*]. The rays from the lamp were allowed to pass from a lead cylinder surrounding it and connected to the case of the electrometer by a small hole about ·3 square centim. in area. They fell on the aluminium sheath transparent to them and rendered the air between it and the insulated aluminium within conductive.

To get a definite difference of potential, the two pairs of quadrants of the electrometer were placed in metallic connexion. Then one terminal of a battery or of an electrostatic induction machine was connected to the internal coatings of the two quasi leyden-jars, and the other terminal to the case of the electrometer. The difference of potential produced was measured by a multicellular voltmeter in the case of voltages under 500 volts, and on a vertical single-vane voltmeter for higher differences.

When the desired difference of potential had been established, the metallic connexion of the battery, or of the electric machine, with the internal coatings of A and B was broken, and this charged body left to itself. To find the loss due to imperfect insulation the pair of quadrants in metallic connexion with the outside coating of B was insulated in the ordinary way, and the deviation of the electrometer reading from the reading obtained when the quadrants were metallically connected—which we shall call the metallic zero—per minute was observed. To find the loss when the rays were acting, the two pairs of quadrants were again placed in metallic connexion and the Röntgen lamp set going; then the pair of quadrants connected to the outside coating of B was insulated from the other pair, and the deviation from the metallic zero again observed per minute.

We tried various differences of potential, ranging from a few volts to 2200 volts. The results we obtained showed that the rate of leak did not appreciably increase from a voltage of about 6 volts to 2200 volts.

Positive and negative charges leaked away at the same rate. These results confirm and extend through a very wide range of voltage the result announced by Thomson and McClelland in a paper communicated to the Cambridge Philosophical Society March 1896.

To test the conductivity induced in air by uranium, we first used the two-leyden method described at the beginning of this section. The leyden A was a cylinder of aluminium with one end covered with aluminium. This cylinder formed

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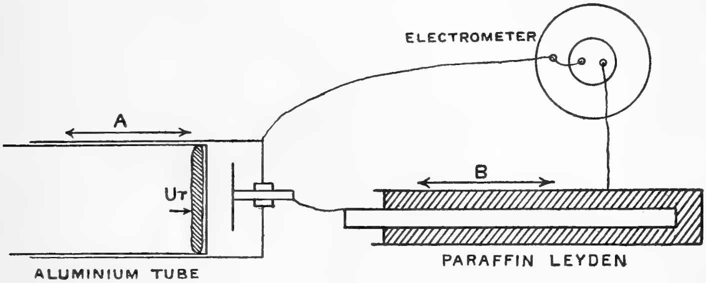
[*] The Röntgen lamp was a focus tube of the Jackson pattern.

the external coating of the leyden-jar. The internal coating was a disk of aluminium insulated on paraffin. The uranium, which was a disk 5·5 centim. diameter, ·5 centim. thick, was placed inside a cardboard cylinder with one end open and the other covered with aluminium, thin enough to be transparent to the uranium influence, so as to touch the aluminium end (see fig. 2).

Fig. 2.



This cardboard cylinder could be moved backwards and forwards in the aluminium cylinder, so that the distance between the insulated disk in the latter and the aluminium end of the former could be varied. The uranium thus acted through the aluminium end of the cardboard box and made the air between the end and the insulated aluminium disk conductive. The leakage was in this way made slow enough to be easily observed in the electrometer. The rate of leak was not perceptibly increased when the piece of uranium was heated nor when the sunlight fell on it.

The aluminium end of the cardboard box and the outside coating of the aluminium cylinder were connected to the case of the electrometer. The insulated aluminium disk was connected to the inside coating of the leyden B. These inside coatings were charged to a known potential and then left to themselves. The air-space between the insulated aluminium disk and the aluminium end of the cardboard box was 2 centim. The voltages used were therefore voltages per two centim. of this air-space. With this arrangement the leakage per minute—the necessary correction due to the natural leakage with uranium removed having been made—at various voltages was :—

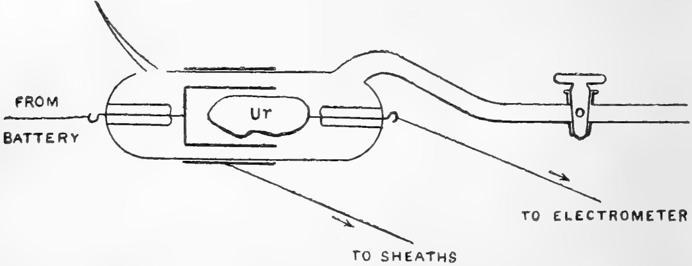
(*a*)

Voltage.	Leakage per minute in sc. divs.
6	56·0
10	65·5
44	113·0
88	128·0
176	156·0
750	219·0
1250	229·0
2000	260·0
3000	276·0

[Sensibility of electrometer 24 sc. divs. per volt of subsidence of difference of potentials between coatings of A.]

We also investigated the conductivity produced in air by a second piece of uranium 3 centim. long, 1 centim. broad, and about ·5 centim. thick. This was mounted firmly in a glass bulb 6 centim. long, 3 centim. diameter, on a platinum wire fused into one end of the bulb. The uranium in the bulb was surrounded throughout two-thirds of its length by a zinc cylinder 1·5 centim. in diameter. This zinc cylinder was kept in position by a stiff platinum wire fused into the other end of the glass (see fig. 3). Two glass tubes were fixed on to the bulb; by means of these any desired gas could be introduced or any desired vacuum produced. Round the outside of the glass bulb a strip of tin-foil was placed and connected to the case of the electrometer. This prevented vitiation of our results by a leak between the two electrodes over the outside of the glass. The bulb was first evacuated and then filled with dry air. The uranium was then connected to the insulated terminal of the electrometer and the zinc to one terminal of a battery or of an electrostatic inductive machine, the other terminal being connected to the case of the electrometer. For voltages up to 100 volts the terminal was kept connected to the zinc while the leakage due

Fig. 3.



to the presence of the uranium was being observed. For higher voltages the zinc was first brought to the voltage given in the table and then disconnected and left to itself.

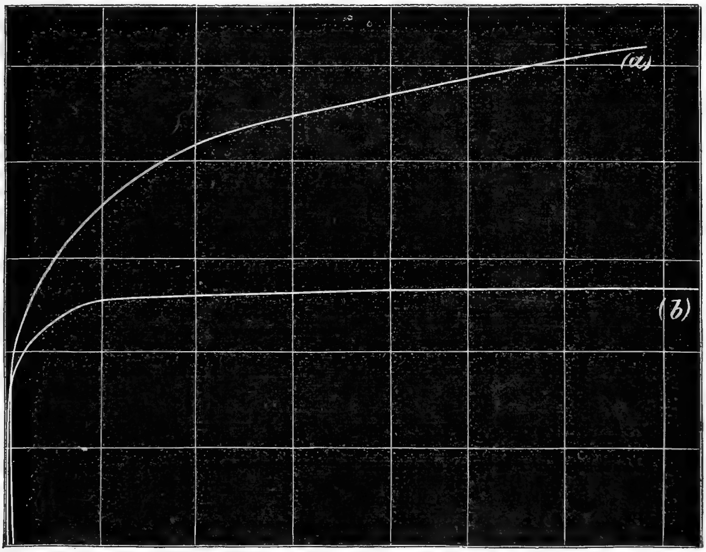
(*b*)

Voltage.	Leakage per minute in sc. divs.
2	92
4	100
22	120
92	129
132	138
200	130
300	137
415	136

[Sensibility of electrometer 140 sc. divs. per volt.]

The appended curves (fig. 4) were drawn by taking the leakage per minute as ordinate, the voltage as abscissa. Curve (*a*) represents the results of the first series of experiments (*a*) reduced to voltages per 2 millim. between the outside coatings of A. Curve (*b*) gives the results of the second series of experiments (*b*). It will be seen that with uranium, as with Röntgen rays, the leakage through air is not proportional to the E.M.F. We found also that both positive and negative charges leaked away at the same rate.

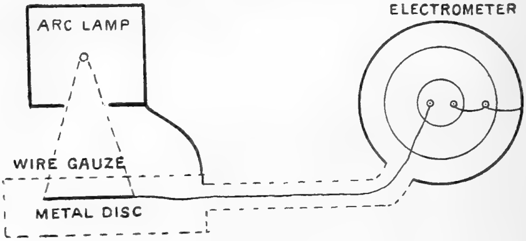
Fig. 4.



With ultra-violet light we have as yet only observed the rate of leak from a charged body for voltages up to two or three volts. The method we employed is one originally used by Righi.

A cage of brass wire gauze was made and connected to the case of the electrometer. Inside it the insulated metal was placed on a block of paraffin, and connected to the insulated terminal of the electrometer by a wire protected against inductive effects. The light from an arc lamp was then let shine through the gauze so as to fall on the insulated metal perpendicular to its surface (see fig. 5).

Fig. 5.



With this arrangement we found when the insulated metal was zinc, aluminium, or copper, and when a positive or negative charge was given to any one of these metals when insulated, that positive and negative charges leaked away at the same rate when the light from the arc lamp fell on the charged metal, the positive or negative charges being reckoned from the steady electrometer reading which is obtained when the two quadrants of the electrometer are insulated and the ultra-violet light shining. Our results on leakage through air from a body illuminated by ultra-violet light agree with those obtained by Branly.

§ 3. *Effect of Röntgen Rays on the Conductance of Paraffin and of Glass.*

In our first experiments with paraffin we used a brass ball of about an inch in diameter, connected to the insulated terminal of the electrometer by a thin wire soldered to the ball. The ball and the wire were both coated to the depth of about an eighth of an inch with paraffin. The ball was then laid on a block of paraffin in a lead box with an aluminium window, both of which were in metallic connexion with the case of the electrometer.

The paraffined ball was then charged positively, and the rays caused to play on it. After two minutes the electrometer reading was steady at 0·5 of the initial reading. The electrometer was then discharged by metallic connexion and again charged positively. Its reading remained steady after three minutes at 0·63 of the initial charge. In the third and fourth experiments the readings after three minutes were ·81 and ·90 of the initial charges respectively.

The ball was next charged negatively. When the rays were played on it a steady reading was obtained after four minutes at ·18 of the initial charge. In the second, third, and fourth experiments the steady readings after four minutes were ·45, ·70, and ·78 of the initial charges respectively.

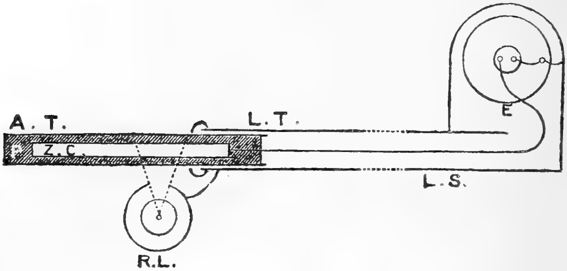
The paraffin was then removed and the brass ball polished with emery-paper; whether the charge was positive or negative it fell in about five seconds to one definite position, ·437 of a volt, on the positive side of the metallic zero, when the rays were played on the charged ball.

These experimental results demonstrate that, for the low potentials—usually 2 or 3 volts—we here used, the Röntgen rays did not produce conductance between the brass ball, when it was coated with paraffin, and the surrounding metal box. We have already seen in § 2 that air is rendered temporarily conductive by the rays, and Röntgen's comparison of the effect of the rays with that of a flame shows that our experimental results are explained by the augmentation of the electrostatic capacity (quasi-condenser) of the brass ball by the outside surface of its coat of paraffin being put into conductive communication with the surrounding lead box and the connected metals.

In our second series of experiments we endeavoured to eliminate the influence of the varying capacity of the quasi-condenser. For this purpose we placed a strip of metal connected to the insulated terminal of the electrometer inside an aluminium cylinder; the space between the metal and the cylinder was first filled with air, afterwards with paraffin. The aluminium cylinder was connected to the case of the electrometer, and inductive disturbances were avoided by surrounding the copper wire connected to the insulated terminal with a lead sheath in metallic connexion with the electrometer case.

In our first experiments with this apparatus we had air, instead of the main mass of paraffin, separating the insulated metal from the surrounding aluminium cylinder, as shown in fig. 6, and we had only small disks of paraffin serving as insulating supports for the ends of the metal, and not played on by the Röntgen rays. When the metal thus supported was charged, whether positively or negatively, the Röntgen rays diselectrified it in about five seconds; not, however, to

Fig. 6.



the metallic zero, but to a zero depending on the nature of the insulated metal and of the metal surrounding it. On the other hand, if the interior insulated metal had initially no charge given to it, yet when the Röntgen rays were played on it through the walls of the surrounding aluminium cylinder, the reading on the electrometer deviated to the same zero to which in the previous case it had fallen, and there remained steady.

With paraffin between the aluminium cylinder and the insulated metal within (see fig. 6) we found no perceptible increase of conductance produced by the Röntgen rays above the natural conductance of the paraffin when undisturbed by them. If the insulated metal was not charged and the Röntgen rays played on it through the aluminium and the paraffin, no deviation from the metallic zero took place when the two pairs of quadrants of the electrometer were insulated from one another.

To make a similar series of experiments with glass we used a piece of glass tubing 9·5 millim. internal diameter, 70 centim. long, and 10 millim. external diameter. The inside of this tube was coated with a deposit of silver, which was placed in metallic connexion with the insulated terminal of the electrometer. The outside of the glass was covered with wet blotting-paper connected to the case of the electrometer. No increase of conductance was produced in the glass when the Röntgen rays were played on it.

We next removed a part of the wet blotting-paper from the outside of the glass, and, after charging the insulated interior metal deposited on the inside of the glass, we heated the exposed part with a spirit flame, in this way making the glass a conductor. The charge was completely removed in

$2\frac{1}{2}$ minutes. We thus see that our method is amply sensitive to the conductance produced in glass by heating.

The differences of potential concerned in the experiments described in the last paragraphs were not more than two or three volts per centimetre of paraffin or per half-millimetre of glass.

To extend our experiments to higher voltages we used the two-leyden method described in § 2. In the experiments on paraffin the leyden A was the aluminium cylinder filled with paraffin in which an insulated metal—now connected to the inside coating of B—was embedded, already referred to. With this arrangement we found that with a difference of potential up to 2500 volts per centimetre of paraffin, no increase of conductance was produced by the Röntgen rays.

In the experiments with glass the leyden A consisted of the glass tube already used. Its inside coating of silver was now connected to the inside coating of B. With glass also we could not find any increase of conductance produced by the Röntgen rays with differences of potential reaching up to 2000 volts per half-millimetre of glass.

§ 4. *Analogous Effects produced by Flame and by Röntgen Rays.*

Two similar sticks of paraffin, which we shall call C and D respectively, each of about 4 sq. centim. cross section, were coated throughout half their lengths with tinfoil. These tinfoils ought to be each metallically connected to the case of the electrometer.

To obtain a sufficiently delicate test for their electric state, a metal disk of 3 centim. diameter was fixed horizontally to the insulated terminal of the electrometer.

The two pieces of paraffin were first diselectrified by being held separately in the flame of a spirit-lamp. Their non-tinfoiled ends were then pressed together, and their electric state tested after separation. It was found that they were still free from electric charge. After this, D was charged by being held over the pointed electrode of an inductive electric machine. The quantity of electricity given to it in this way was roughly measured by noting the electrometer reading when the paraffin was held at a distance of 4 centim. above the metal disk connected to the insulated terminal of the electrometer.

The free ends of C and D were again held together, and, after separation, both pieces were tested separately. The charged one, D, had suffered no appreciable loss, and the other, C, induced an electrometer reading of a few sc. divs. in the same direction, when held as near as possible to the metallic disk without touching it. This showed that an exceedingly minute quantity of electricity had passed from D to C when they were in contact.

C was then diselectrified by being held in the flame. The ends of C and D were again put together—D still having the charge previously given to it—and in this position were passed through the flame. They were tested with their ends still pressed together, and it was found that when held as near as possible to the metal disk without touching it, no reading was produced on the electrometer. After this they were separated and tested separately ; and it was found that D, when held over the disk, gave a large reading in the same direction as before the two with their free ends together had been passed through the flame, and C (which was previously non-electrified) gave a large reading in the opposite direction.

Exactly similar results were obtained with the two paraffin sticks when Röntgen rays were substituted for flame, and when glass or ebonite was used instead of paraffin.

The explanation clearly is this :—The flame or the Röntgen rays put the outer paraffin surfaces of C and D temporarily in conductive communication with the tinfoils, but left the end of D, pressed as it was against the end of C, with its charge undisturbed. This charge induced an equal quantity of the opposite electricity on the outer surfaces of the paraffin of C and D between the tinfoils ; half on C, half on D.

When the application of flame or of Röntgen rays was stopped, this electrification of the outer paraffin surfaces became fixed. D, presented to the electrometer, shewed the effect of the charge initially given to its end, and an induced opposite charge of half its amount on the sides between the end and the tinfoil. C showed on the electrometer only the effect of its half of the whole opposite charge induced on the sides by the charge on D's end.

We have here another proof that paraffin is not rendered largely conductive by the Röntgen rays. Had it been made so, then the charge given to the end would have leaked through the body of the paraffin to the outside, and have been carried away either by the tinfoil or by the conductive air surrounding the non-tinfoiled parts.

To show that the induced charges were fixed on the sides, the two sticks, C and D, were next coated with tinfoil throughout their whole length, only one end of each being uncovered. The uncoated end of D was then charged and pressed against that of C, and the two were held either in the flame of a spirit-lamp or in the Röntgen rays. When taken out of the flame or the Röntgen rays, and then separated and tested separately, it was found that D had retained its charge practically undiminished, and that C had acquired a very slight charge of the opposite kind.

§ 5. *Leakage of Electricity from an Electrified Body* in gases other than air at ordinary pressure, due to the presence of uranium.

We were able to investigate the rate of leak in different gases by means of the smaller piece of uranium mounted in a glass bulb as described in § 2 (fig. 3). The gas used was first stored in a reservoir over water. It was then bubbled through strong sulphuric acid and passed over caustic potash, calcium chloride, and phosphoric anhydride into the glass bulb. The bulb was first exhausted to an atmospheric pressure of about 6 millim.; then the gas to be used was passed into it. It was again evacuated and refilled. This was repeated about twenty times. Finally, it was strongly heated so as to draw off any adhering layers of the gas which had previously been in the bulb, and then allowed to cool in an atmosphere of the gas at 760 millim. pressure. One of the tubes was then sealed up; the other was closed by a well-fitting and well-greased glass stop-cock.

The following tables give the results obtained with the gases we have experimented on:—

Hydrogen.

Voltage.	Leakage per minute in sc. divs.
2 volts.	32
4 ,,	37
22 ,,	39
34 ,,	38
100 ,,	39
135 ,,	38

Oxygen.

4 ,,	125
96 ,,	157

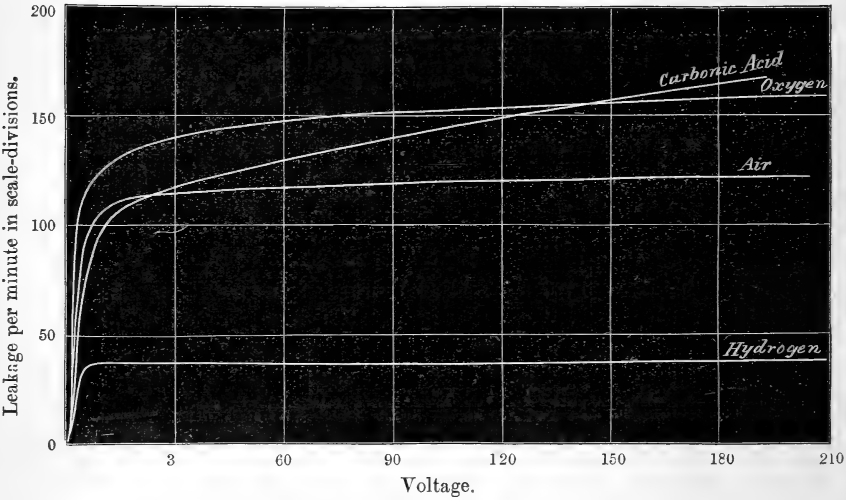
Carbonic Acid.

4 ,,	94
95 ,,	167
238 ,,	183
255 ,,	180
2900 ,,	Spark discharge.

[Sensibility of electrometer 140 sc. divs. per volt.]

The results given for these three gases are comparable to the second series of results given in § 2 for conductance produced in air by uranium. We see that the rate of leak is greater in oxygen than in air. No comparative figures need be given as these would vary according to the voltage chosen. The leakage in hydrogen is less than in air ; in carbonic acid it is less for 4 volts but greater for 90 volts than it is in air. For the latter voltage the leakage in carbonic acid is greater even than the corresponding leakage for oxygen. The appended curves show the peculiarities of the leakage in the different gases (fig. 7).

Fig. 7.



§ 6. *Leakage in different Gases at different Pressures due to Uranium.*

The method of filling the glass bulb with any given gas has already been described in § 5. The vacuums up to 2 millim. were produced by a double-barrelled air-pump ; higher vacuums were produced by a Töpler pump.

The following tables give the results obtained with the gases we have used :—

Air.

α. Atmospheric pressure in mms.	β. Leakage per minute for 4 volts.	γ. Leakage per minute for 96 volts.	$\frac{\beta}{\alpha}$.	$\frac{\gamma}{\alpha}$.
760	100	131	·132	·172
240	44	46	·183	·192
190	40	39	·210	·205
121	24	26	·197	·214
64	12	13·5	·187	·212
58	11·0	10·0	·189	·172
23	4·4	3·75	·191	·163
3·6	1·2	1·2	·339	·339

It will be seen from the last two columns of the table that the rate of leak at 4 volts and at 96 volts is nearly proportional to the atmospheric pressure. The results obtained at 3·6 millim. are not very trustworthy. With lower pressures no appreciable leakage at these two voltages was observed.

Hydrogen.

α. Atmospheric pressure in mms.	β. Leakage per minute at 4 volts.	$\frac{\beta}{\alpha}$.	$\frac{\beta}{\sqrt{\alpha}}$.
760	37	·041	1·43
197	11	·056	·77
66	4	·061	·50
8	1·5	·187	·55

With lower pressures no leakage was observed. The rate of leak is at higher pressures somewhat approximately proportional to the pressure, at lower ones to the square root of the pressure.

Oxygen.

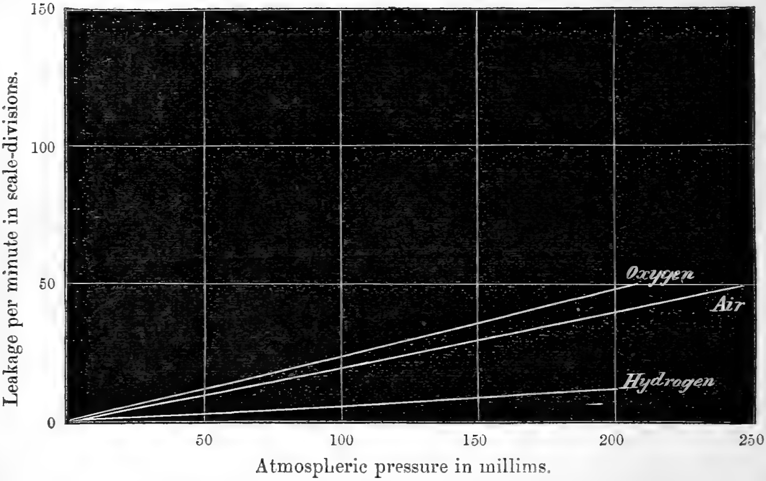
α. Atmospheric pressure in mms.	β. Leakage per minute for 4 volts.	$\frac{\beta}{\alpha}$.	$\frac{\beta}{\sqrt{\alpha}}$.
760	125	·16	4·5
205	48·5	·25	3·3
64	15·0	·24	2·9
2	2·0	1·00	·71

Carbonic Acid.

Atmospheric pressure in mms.	Leakage per minute for 4 volts.	Leakage per minute for 100 volts.
760	94	167
62	18	21
2	not observable.	

The curves for air, oxygen, and hydrogen, given in fig. 8, were obtained by taking the atmospheric pressure in millim. as abscissa and the leakage per minute for four volts as ordinate.

Fig. 8.

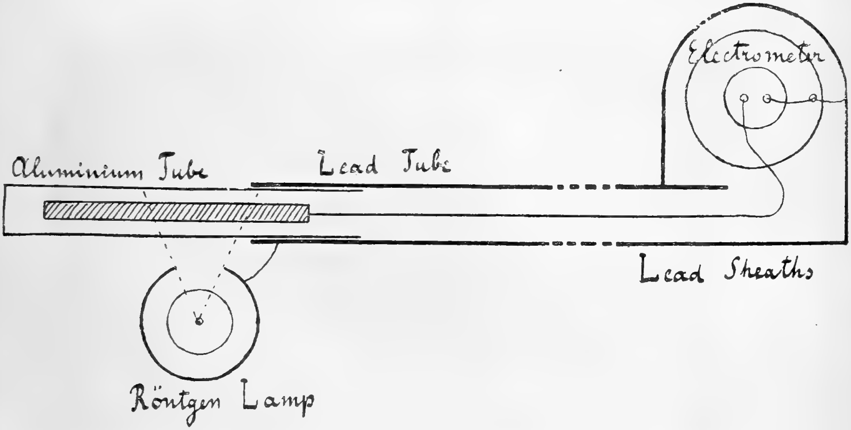


§ 7. *Measurement of the Difference of Potential* between wires of one metal connected with two mutually insulated metals when the air between them is rendered conductive by Röntgen rays, by ultra-violet light, and by uranium.

The fact that gases are made conductive by Röntgen rays, by ultra-violet light, and by uranium supplies us with a means of measuring the difference of potential between wires of one metal connected with two mutually insulated conductors. This method has already been used by Righi. He determined the difference of potential of wires of one metal connected to two mutually insulated conductors by rendering the air between them conductive under the influence of ultra-violet light. Minchin, Righi, and Murray have made experiments of a similar kind with Röntgen rays.

In our experiments to measure this difference of potential between wires of one metal connected to two mutually insulated conductors by rendering the air between two mutually insulated conductors conductive by means of Röntgen rays, we used a cylinder of unpolished aluminium connected to the case of the electrometer. Along the axis of this a conductor was placed, supported by its ends on small blocks of paraffin. This insulated conductor was connected by a copper wire to the insulated terminal of the electrometer. Suitable means were taken to protect this connecting wire from inductive effects (see fig. 9).

Fig. 9.



[From 'Nature,' Feb. 11, 1897.]

The Röntgen lamp was placed in a lead cylinder connected to the case of the electrometer. The rays passed into the cylinder of aluminium through a window in the lead cylinder 2 centim. broad and 4 centim. long. This window could be screened or unscreened at will.

The course of the experiment was the same with each insulated conductor. The conductor was charged first positively, then negatively; the Röntgen rays were then caused to shine on it through the aluminium cylinder surrounding it and the electrometer-readings taken at fixed intervals, until a steady reading on the electrometer was obtained. The point at which the electrometer-reading remained steady with the rays acting we shall call the rays-zero.

Finally, the insulated conductor was discharged by metallic connexion in the electrometer and re-insulated; the rays were again caused to shine on it till the deviation from the metallic zero reached the rays-zero and there remained steady.

This deviation from the metallic zero was not stopped by placing an aluminium screen over the window of the lead cylinder surrounding the Röntgen lamp; on the other hand, it was stopped if a lead screen was used.

In the following table, column II. gives the potential-differences of the rays-zero from the metallic zero for twelve different metals insulated within the unpolished aluminium cylinder as described above. Column III. gives the differences for two of the same metals in the interior but with the surrounding aluminium cylinder altered by its inner surface being polished by emery-paper.

I. Insulated metal.	II.	II.
Magnesium tape........	−0·671 of a volt.	
Amalgamated zinc......	−0·66 ,,	
Polished aluminium	−0·465 ,,	
Polished zinc..........	−0·343 ,,	
Unpolished aluminium ..	−0·349 ,,	+0·35 of a volt.
Polished lead..........	−0·257 ,,	
Polished copper........	+0·129 ,,	
Polished iron nail......	+0·182 ,,	
Palladium wire	+0·255 ,,	
Gold wire	+0·264 ,,	+0·930 ,,
Carbon	+0·429 ,,	

It will be noticed that the difference of potential depends very much on the state of polish of the two mutually insulated conductors.

To make similar experiments with ultra-violet light we used the brass wire gauze cage arrangement described in § 2. That is, we have now air between the wire gauze and the insulated conductor rendered conductive by ultra-violet light. The insulated conductor was 2 centim. distant from the gauze. The steady electrometer-reading after the two pairs of quadrants were insulated and the ultra-violet light shining (which we shall hereafter refer to as the ultra-violet-light-zero) was observed. The difference of potential indicated on the electrometer between the rays-zero and the metallic zero does *not* give, however, the contact-force between the gauze and the insulated conductor within. The reason for this we shall see in the next section.

The following table shows the steady potential-differences in the electrometer due to the conductive effect produced by ultra-violet light in the air between the brass wire gauze and the insulated conductor.

Insulated metal.	Potential-difference.
Polished zinc............	−0·75 of a volt.
Polished aluminium	−0·66 ,,
German silver	−0·19 ,,
Gilded brass............	+0·04 ,,
Polished copper..........	+0·12 ,,
Oxidized copper..........	+1·02 ,,

When the insulated metal was charged either positively or negatively and the ultra-violet light let fall on it, the electrometer-reading deviated until the ultra-violet-light-zero was reached. The rate of deviation was the same for a positive or a negative charge if we reckon the charge from the ultra-violet-light-zero.

We have used two different methods to measure the potential-difference between wires of the same metal connected to two mutually insulated metals when the air between them is rendered conductive by the presence of uranium. The more convenient method is to take uranium as one of the mutually insulated metals. To do this we fixed a metallic disk, 3 centim. diameter, to the insulated terminal of the quadrant-electrometer. Opposite this disk, and separated from it by air, we placed the disk of uranium, 5·5 centim. diameter, connected to the case of the electrometer. With this arrangement we found, after contact between the quadrants was broken at the electrometer, a deviation from the metallic zero. This deviation took place gradually till a steady reading was reached. This steady reading we shall call the uranium-conductance-zero, or shortly the uranium-zero. If the insulated conductor had a charge given to it of such an amount as to cause the electrometer-reading to deviate from the metallic zero beyond the uranium-zero, the reading quickly fell to this conductance zero and there remained steady. When no charge was given to the insulated metal the steady uranium-zero was reached in about half a minute.

The following table gives the potential-differences found in this way :—

Metal.	Potential-difference, in volts.
Polished aluminium (1) immediately after being polished	−1·13
Polished aluminium (1) next day	−0·90
Polished aluminium (2)	−1·00
Amalgamated zinc	−0·80
Polished zinc	−0·71
Unpolished zinc	−0·55
Polished lead	−0·54
Tinfoil	−0·49
Unpolished aluminium (1)	−0·41
Polished copper	−0·17
Silver coin	+0·05
Unpolished copper	+0·07
Carbon	+0·20
Oxidized copper (*a*)	+0·42
Oxidized copper (*b*)	+0 90

The great effect of the surfaces as to polish is very evident in the above table.

With a third specimen of oxidized copper a potential-difference of $+0{\cdot}35$ of a volt was obtained. This specimen was afterwards connected to the case of the electrometer; a piece of polished aluminium was placed opposite it and connected to the insulated terminal of the electrometer. The uranium disk, insulated on paraffin, was then placed between them, and the deviation observed was equivalent to a potential-difference of $-1{\cdot}53$ volts; that is, we obtained an effect equal to the sum of the effects we obtained when the metals were separately insulated in air opposite uranium.

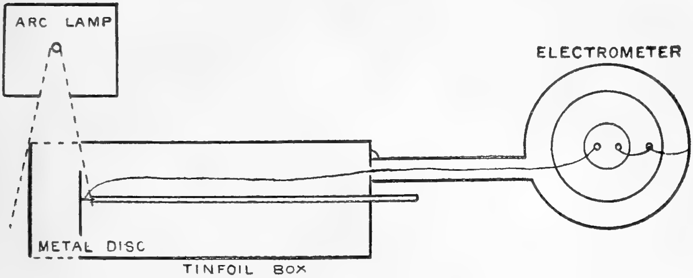
Instead of placing the uranium directly opposite the insulated metal in air, we also observed the uranium-zero by mutually insulating two metals in air, one of which was transparent to the uranium influence.

For this purpose we made a tinfoil box with tinfoil sufficiently thin to be transparent to the uranium influence. The tinfoil forming the box was connected to the electrometer-case. Inside it another metal was insulated on a glass stem and placed so as to be parallel to one end of the tinfoil box. This metal was connected to the insulated terminal of the electrometer. The uranium was placed outside the box about half a centimetre distant from the end to which the insulated metal was parallel. The same uranium-zero was obtained whether the uranium was insulated or connected to the case of the electrometer. The time required to reach the uranium-zero with this arrangement was usually four or five minutes. A charge given to the insulated metal large enough to produce a deviation beyond the uranium-conductance-zero was discharged till this zero was reached. A charge causing the electrometer-reading to deviate in the opposite direction was discharged to the metallic zero and thence on to the uranium-conductance-zero, where it remained steady.

§ 8. *Dependence of the Difference of Potential* as measured in § 7 on distance between the two mutually insulated conductors.

A cardboard box, 46 centim. long, 19 centim. square (see fig. 10), lined with tinfoil, connected to the case of the electrometer was used. Inside this box an insulated disk of oxidized copper 10 centim. diameter was supported in such a way as to allow of its being fixed at different distances from the tinfoil-coated end-wall of the box, facing it. Two windows were cut in the side of the box as shown in the diagram. The Röntgen lamp was placed outside the box at the line joining the windows. These windows were covered with tinfoil gauze to prevent inductive effects from the lamp. With the Röntgen rays shining in between the insulated disk and the opposing tinfoil wall so as to illuminate both, the following results were obtained :—

Fig. 10.



Difference of potential between rays-zero and metallic zero.	Distance between the surfaces.
+0·168 of a volt	1·2 centim.
+0·179 ,,	2·2 ,,
+0·165 ,,	3·8 ,,
+0·165 ,,	6·0 ,,

With a polished zinc disk in place of the oxidized copper disk we found :—

−0·580 of a volt	at 1 centim. distance.
−0·565 ,,	,, 1·5 ,, ,,
−0·580 ,,	,, 3·0 ,, ,,
−0·640 ,,	,, 7·0 ,, ,,
−0·640 ,,	,, 7·5 ,, ,,

The effect of changing the distance between the opposed surfaces is to vary the capacity of our arrangement. Had the Röntgen rays given a charge to the insulated conductors other than that necessary for the equalization of the volta difference of the two mutually insulated conducting surfaces, this would have been shown by a variation of the potential-difference observed in the electrometer when the distances were changed. We should not, however, be justified in concluding that the difference between the rays-zero and the metallic zero is *the contact-difference* between the electrically effective surfaces of the mutually insulated conductors.

We used the same arrangement with ultra-violet light. A glance at fig. 10 will show how the light was placed so as to fall on both surfaces. The insulated conductor employed was the same oxidized copper disk as used in the Röntgen ray experiments. The difference between the metallic zero and the ultra-violet-light-zero was found to depend on the distance between the two surfaces. This will be seen from the following table :—

Difference of potential between ultra-violet-light-zero and metallic zero.	Distance between the surfaces.
+0·615 of a volt.	0·6 centim.
+0·730 „	1 „
+0·805 „	2 „
+0·955 „	3 „
+1·205 „	4 „
+1·07 „	4·3 „
+1·15 „	5·0 „
+1·42 „	7·0 „

The fact that the ultra-violet-light-zero depends on the distance between the two mutually insulated conductors was first observed by Righi. We have here something in reality much greater than a mere equalization of the volta difference between the two surfaces, and we cannot say that the difference of potential between the ultra-violet-light-zero and the metallic zero at any arbitrary distance is *the volta difference* between the electrically effective surfaces of the two metals.

To observe the uranium-conductance-zero at different distances an aluminium box connected to the case of the electrometer was substituted for the tinfoil one. The insulated metal was again oxidized copper—not, however, the same specimen as was used with ultra-violet light and with Röntgen rays. The uranium was placed outside the aluminium box about 5 millim. from the end, to which the oxidized copper was kept parallel.

Potential-difference between uranium-conductance-zero and metallic zero.	Distance between the mutually insulated surfaces.
+0·96 of a volt.	0·5 centim.
+0·97 „	1·5 „
+0·95 „	2·0 „
+0·98 „	4·0 „
+1·03 „	8·0 „

We see that the potential-difference does not depend on the distance. We cannot, however, infer that therefore the difference between the conductance-zero and the metallic zero is *the contact-difference* between the electrically effective surfaces of the mutually insulated conductors.

§ 9. *Difference of Potential due to Uranium in different Gases at different pressures.*

To observe the effect of different gases at different pressures on the uranium-conductance-zero we used the small piece of uranium mounted in a glass bulb as described in § 2. The precautions taken in filling the bulb with gas are also described in the same section.

The uranium was connected to the insulated terminal of the electrometer and the zinc cylinder to the case of the electrometer. In the following table the results obtained with air, hydrogen, and oxygen are given.

Pressure, in mm.	Difference of potential between the uranium-conductance-zero and the metallic zero.		
	Hydrogen.	Oxygen.	Air.
760	+·17 of a volt (in about a minute; afterwards steady).	+·105 of a volt (in about a minute; afterwards steady).	+·11 of a volt (in about a minute; afterwards steady).
193	+·12 of a volt (in about a minute; afterwards steady).		
66	+·05 of a volt (6 minutes).	+·11 of a volt (3 minutes).	
8	+·04 of a volt (8 minutes).		
2	. . . . . .	+·10 of a volt in 27 minutes.	
$<\frac{1}{1000}$	·05 of a volt in 28 minutes.		

The uranium-conductance-zero between mutually insulated uranium and zinc differs much less from the metallic zero than it did with the arrangement described in § 7. This is probably due to the oxidation of the zinc of the zinc cylinder. The conductance zero, however, it will be noticed is approximately the same in all three gases.

§ 10. *Voltage necessary to produce a Spark between Uranium and Zinc* at different atmospheric pressures when the distance apart was constant.

The small piece of uranium before referred to was used. The distance between it and the surrounding zinc cylinder was about 2 or 3 millim. We found that at ordinary atmospheric pressure sparking took place in air at 4800 volts. At 232 millim. pressure the potential necessary to produce a spark fell to between 1500 and 2000 volts. At 127 millim. it had fallen to between 1100 and 1300 volts. At 54 millim. it was 700 volts; at 7 millim. 420 volts. At about $\frac{1}{1000}$ millim. the voltage had risen again to 2000 volts.

LVIII. *Influence of Proximity of Substances upon Voltaic Action.* *By* Dr. G. Gore, *F.R.S.**

IN the year 1849 I made some attempts to discover an effect of gravity upon voltaic action at about the same time that Faraday was seeking to demonstrate by experiment a connexion between gravity and electricity. Recently I have shown (see "Relation of Volta-electromotive Force to Pressure," Phil. Mag. Feb. 1893, pp. 97, 98) that the difference of pressure due to gravity at the upper and lower ends of a vertical column of an electrolyte about three metres high, upon two perfectly similar electrodes of the same metal at the upper and lower ends of the column, in a series of six glass tubes, produced a very feeble electric current; and that whilst in fully one half of the experiments no current was perceptible, in forty-two instances a current occurred, and in thirty-nine of these it was in an upward direction through the liquid, the lower electrodes being positive. In my opinion these results indicated that the energy of mechanical pressure produced by gravity altered the volta-electromotive force and enabled an electric current to be produced.

It is manifest, if these experiments and statements are reliable, and the effects were really due to pressure:—1st, that gravity, by producing pressure, exerts an extremely minute influence upon chemical and voltaic action; and, 2nd, that similar effects, though excessively minute ones, must be produced by the gravitative action of a large mass of metal or other substance upon a voltaic electrode at the end of a *horizontal* column of electrolyte presented to it.

I have roughly calculated the probable amount of voltaic effect of pressure by the attraction of a massive cube of lead weighing 74 cwt., with its centre at a distance of fifteen inches from the ends of a series of fourteen horizontal tubes of liquid similar to those above referred to. Taking the weight of the earth as being about 12,500,000 million million million pounds, the proportional weight of the cube to that of the earth as being about 1 to 1511,000000,000000,000000, and the proportional distance of its centre from the electrodes to that of the earth's centre from them as being about 1 to 17,000000, the proportional strength of gravitative influence of the cube to that of the earth upon them would be about 1 to 5300,000000, and after correction for the greater number of tubes in the horizontal than in the vertical apparatus the probable amount of voltaic effect would still be only about

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* Communicated by the Author.

$\frac{1}{2400,000000}$ part of that obtained with the six vertical tubes, and quite unrecognizable by means of a galvanometer, especially in the presence of a variable amount of unbalanced voltaic action which cannot be entirely excluded.

Numerical calculations, however, frequently lead to error unless they are sufficiently supported by facts ; notwithstanding that the excessively feeble action of gravity, and the relatively large amount of purely voltaic action, rendered it highly improbable that any perceptible electric effect would be produced, on a number of occasions between the years 1849 and 1894, I devised and constructed various apparatus, and made numerous series of experiments and observations with them, in the hope of rendering such an effect perceptible. One was a vertical cylinder of guttapercha six feet high and six inches diameter, fitted with terminal electrodes attached to a galvanometer, filled with an electrolyte, and capable of revolving on a horizontal axis ; a second was a massive wooden frame, provided with a high resistance-coil of very fine insulated copper wire leading to a galvanometer, and capable of being revolved upon a horizontal axis at a speed of more than 4000 revolutions per minute ; a third consisted of two very compact and well insulated voltaic batteries of one hundred cells each, connected in opposition with a high resistance galvanometer, and separately attached to the two ends of a cord passing over a pulley fixed to a ceiling, so that one might be raised whilst the other was lowered through a distance of about twenty-two feet ; and several others which need not be mentioned. The first really positive results were obtained in June 1894 ; and I will briefly describe some of the experiments made with three of the successful apparatus, substantially in the same chronological order in which they were made ; the most perfect ones are those of the most recent date.

EXPERIMENTS WITH APPARATUS NO. 1.

In this section of experiments the apparatus consisted essentially of a massive influencing or "attracting" body ; a series of fourteen tubes of electrolyte similar to those already mentioned, and a Thomson reflecting galvanometer having a resistance of 3040 ohms. The influencing mass, already mentioned, was composed of 72 pigs of lead, having a total weight of 8271 lb., or about 74 cwt., supported upon a solid brick floor and free from any perceptible vibration. The glass tubes containing the liquid were each about 75 inches long and ¼ inch internal diameter, fixed upon a perfectly horizontal and stout board which was capable of smooth rotation upon a vertical axis at its middle part, so that either end of the series of tubes might be presented to the centre of the near face of the cube without being shaken. By omitting one of the pieces of lead a horizontal groove was left in that face of the cube, in order to increase the expected effect by permitting the ends of the tubes to penetrate about an inch within the mass. The electrodes were formed of zinc wire ·072 inch diameter, all cut from the same piece, not amalgamated, and each fixed in a paraffined cork and projecting about $\frac{3}{4}$ of an inch into the tube; they were all connected in series by means of silk-covered thin copper wires.

By means of this arrangement, whilst the previous experiments with the *vertical* tubes referred to above (*loc. cit.*) included the effect of considerable difference of pressure, and almost entirely excluded the influence of difference of terrestrial gravitative attraction, those with the *horizontal* ones included the effect of difference of gravitative attraction of the lead, and completely eliminated that of difference of pressure caused by difference of attraction of the earth.

The electrolyte employed consisted of 38 ounces of thoroughly boiled distilled water and 75 grains of potassium chloride, the mixture being subsequently about $\frac{1}{16}$ saturated with washed chlorine gas. This liquid was used in all the following experiments with this apparatus, and was at all times carefully screened from the light. The use of the chlorine was to prevent the liberation of hydrogen.

After excluding or rendering negliglible nearly all the numerous sources of error (which need not be here specified), the only remaining disturbances were due to unequal and variable voltaic action at the electrodes, changes of temperature, periodical movements of the tubes, and slightly to variations of terrestrial magnetism. These influences, however, were sufficiently small to permit the detection of a very minute amount of deflexion of the galvanometer needles due to the presence of the lead, provided a proper method of averaging the magnitudes of the deflexions was employed.

The usual method of manipulation adopted with this apparatus was to place the tubes in a line with the centre of the near face of the cube, close the circuit, allow any voltaic current to subside or become steady (this was often a tedious matter), take periodical observations every hour or less, and reverse the ends of the tubes once a day or more frequently. All these experiments were made in a cool room free from draughts, and the temperature of the cube and of the air of the room were recorded regularly with the deflexions.

These experiments were continued with but little intermission during 37 days; and a total of 631 trustworthy observations of the galvanometer was taken, the number per day varying from 3 to 20. The ends of the tubes presented to the cube were reversed a total of 47 times, and the periods of time between the reversals varied from 2 to 52 hours. The total average magnitude of deflexion of the galvanometer needles whilst one end of the tubes was presented to the cube was greater than that of the other, and the proximity of the lead slightly increased the voltaic current when one set of electrodes was near the cube and slightly decreased it when the other set was near.

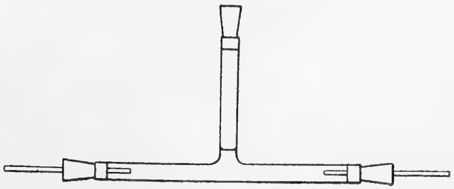
The correct interpretation I consider of these results is, that whilst nearly the whole of the deflexion of the needles in each case was due to a minute voltaic current produced by inequality of chemical action upon the two sets of electrodes and by motion of the tubes, the proximity of the lead to the ends of the tubes tended to increase the positive or decrease the negative electromotive force. (This conclusion was subsequently confirmed by results obtained with other apparatus.) The results appear to indicate a loss of energy of the mutually approaching masses of zinc and lead, attended by an extremely minute alteration of the molecular conditions of the two substances.

The chief defects of this form of apparatus were :—1st, the large mass of lead required ; 2nd, too great distance of the lead from the electrodes ; 3rd, the narrowness and considerable length of the glass tubes causing too much electric conduction-resistance ; and, 4th, the irregular differences of temperature attending the use of so large an apparatus ; I therefore designed another arrangement.

APPARATUS NO. 2.

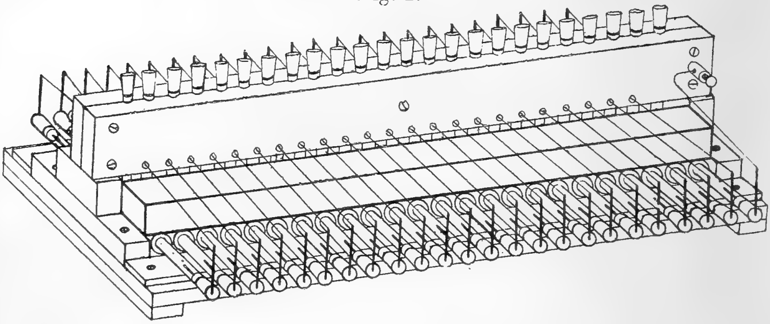
This apparatus was essentially the same in principle as the first one ; it was, however, very much smaller, being about 18 inches long, 7 inches wide, and 6 inches high. It consisted of 24 ⊥-shaped glass tubes, each $5\frac{1}{2}$ inches long and about $\frac{5}{16}$ inch diameter (see figs. 1 and 2), fitted with zinc

Fig. 1.



wire electrodes ·072 inch diameter, all cut from the same piece, not amalgamated, fixed in paraffined corks, and connected in series by means of silk-covered copper wire ·022 inch diameter. The tubes were held together in a single row by means of two horizontal bars of soft wood having grooves on their inner opposed surfaces; they were arranged so as to slide to and fro through a space of 1⅜ inches, into and out of leaden tubes ⅞ inch long and ⅛ inch thick, so as to obtain the simultaneous effects of approach and recession of the two sets of electrodes into and out of the influencing substance on each side of the apparatus. The influence of the lead tubes was reinforced by that of removable strips or bars of lead in contact with them above and beneath. The tubes and electrodes were constantly screened from daylight.

Fig. 2.



The apparatus was designed so that the electrodes and the lead might be brought very much nearer together than in the previous one. Its greatly increased compactness considerably and sufficiently diminished the disturbance caused by difference of temperature of its different parts. The electrolyte was that used in the previous apparatus; its advantages were that it did not give rise to formation of insoluble salts or bubbles of gas upon the electrodes. The same galvanometer was used as in the previous experiments; an ordinary astatic one of 1000 ohms resistance was not sufficiently sensitive. The two series of electrodes were respectively denominated "A" and "B." With this apparatus numerous experiments were made.

1*st Series of Experiments.*

With strips of sheet lead ·25 inch thick and one inch wide in contact above and below with the lead tubes. The "A" electrodes happened to be feebly volta positive to the "B" ones.—Ninety-seven observations were taken during the afternoons and evenings of 13 days, the position of the lead with regard to the "A" and "B" electrodes being usually reversed between the hours of 2 and 3 each day, and the circuit was left open every night.

In nearly all these cases the proximity of the lead to the "A" electrodes was attended by an *increase* of the deflexion, and to the "B" electrodes by a *decrease*; and whilst the average magnitude of deflexion in the former case was 21·7, in the latter it was 13·3; in some cases with the "B" electrodes the influence of the lead was sufficient to overcome the previous current and slightly reverse the deflexion. These results are consistent with the view that the proximity of the lead to either series of electrodes *increased* its electro-positive or *decreased* its electro-negative state, and they agree with those obtained with the larger apparatus (see *ante*, p. 443). In these experiments the usual period after reversal of position of the electrodes in which the effect of the lead attained its maximum was about 2¼ hours.

In some additional experiments 181 observations were made during 12 forenoons, the circuit being left open all night and closed each following morning. The average magnitude of deflexion obtained with the "A" electrodes was 27·5, and with the "B" ones 1·4. In some of these cases also the influence of the lead was sufficient to reverse the deflexion. It usually required about 2¼ hours for the effect of the lead to attain its maximum. These results agree with the immediately preceding ones.

2nd Series.—Influence of Mass of Lead, &c.

With *bars* of lead about 1 inch wide and 1 inch thick substituted for the strips, and all the other conditions remaining the same.—During the afternoons and evenings of a further period of 9 days 57 observations were made, the periods of reversal of position of the electrodes and of closing the circuit being as above.

In nearly every case the deflexions obtained in the presence of the lead with the "A" electrodes were larger than those obtained with the "B" ones, and whilst the average magnitude of those obtained with the "A" ones was 34·4, that with "B" was 16·1; *i.e.*, the proximity of the lead increased the positive condition of the "A" electrodes and decreased the negative condition of the "B" ones, and the presence of the larger mass of lead was attended by a greater amount of effect upon the electromotive force. The usual period required to attain a maximum after reversal was about two hours.

In some additional experiments the circuit was closed as usual each following morning, the "A" electrodes having been under the influence of the lead with the circuit open all night, and 115 observations were taken during 8 mornings.

The average amount of deflexion obtained with the "A" electrodes was 32·4 and with the "B" ones −2·9. The usual period after the circuit was closed in which the amount of effect attained its maximum was in these cases also nearly two hours. The results agreed with the previous ones and with the further conclusion that the influence of the lead upon the electromotive force was increased by time up to a certain limit.

3rd Series.—Influence of Period of Reversal.

The hour of the day at which the reversals were made instead of being later was now changed to 9 A.M. after the circuit including the galvanometer had been *closed* all night, the *bars* of lead being still used and all the other conditions remaining the same.—Ninety-seven observations were made during 5½ days. The average amount of deflexion with the "A" electrodes during 2½ days was 31·0, and with the "B" ones during three days 1·4. These results confirmed the previous ones and showed that altering the period of reversing the electrodes had no conspicuous effect. The usual period after each reversal in which the effect of the lead attained its maximum was about two hours. In one instance the influence of proximity of the lead was sufficient to reverse the direction of the deflexion. The results showed that the differences of effect attending the presence and absence of the lead were not due to diurnal changes of magnetism.

Influence of Conduction-resistance.

In order to ascertain the influence of extra total resistance upon the time required for the lead to produce its full effect; whilst the "A" electrodes were between the bars of lead and the full effect had been attained, an extra resistance of about 6000 ohms was suddenly inserted in the circuit, and the effect allowed to develop. At the end of 3½ hours the influence of the lead was still increasing and had not arrived at its usual maximum.

After allowing the circuit to remain closed all night the maximum effect of the lead appeared to be about the same as when the 6000 ohms were not in the circuit. The "B" electrodes were now substituted for the "A" ones and the apparatus allowed to remain undisturbed. The maximum effect was now attained in a period of about 3½ hours.

These results, when compared with previous ones, indicated that the larger the total resistance in the circuit, the longer was the period of time required for the lead to produce its full effect.

General Influence of Time.

In all the experiments of these three series, a short period of time elapsed before any effect of the proximity of the lead upon the deflexion was manifest; in some cases it was distinct in less than five minutes, whilst in others it was not observable in less than a quarter of an hour. (N.B. In later experiments with apparatus "No. 3" offering very much less resistance, the visible effect commenced at once.) As the delay could not be attributed to any other cause, and as in all cases the maximum effect with the total amount of resistance in the circuit was usually only attained in about two hours, I conclude that a state of *strain* of the superficial molecules of the electrodes and lead bars was probably produced, and required that period of time to be completely overcome.

Both with the lead bars and with the plates, by leaving the circuit open all night (equal to about 15 hours), with the electrodes in proximity to the bars, the effects of the lead had disappeared, and it required about two hours to entirely recover after the circuit had again been closed through the galvanometer. As the bars produced a much larger maximum effect than the thin plates (see *ante*, p. 444), and produced it in about the same period of time, viz. two hours, they must have produced it at a much faster rate.

Effect of Short-circuiting the Apparatus.

As the maximum effect of the lead was more quickly attained the smaller the total amount of resistance in the circuit, the effect of short-circuiting the pile alone whilst the electrodes were under the influence of the lead was tried. In four separate experiments, by short-circuiting the apparatus, with an external resistance of only 60 ohms and no galvanometer in the circuit, during ten minutes, and then at once including the galvanometer, the maximum effect was attained in about 25 minutes; this result indicated that the retardation was largely due to the total resistance.

Influence of Temperature, &c.

A few experiments were made with the same apparatus to examine this. A strip of lead 16½ inches long, 1 inch wide, and ⅛ inch thick was uniformly heated throughout to a temperature varying in different cases from 5 to 16 Centigrade degrees above that of the apparatus, and placed in immediate contact with the glass tubes above the "A" and "B" electrodes separately, and allowed to remain a sufficient period of time. In each of the five trials, including a total of 28 observations, the heat made the electrodes negative; *i. e.* it decreased the electromotive force at the positive "A" electrodes, and increased that at the negative "B" ones, and thus in every case it produced an opposite effect to that produced by the lead. In one of the experiments, with the warmed strip of lead at a temperature of 16 Centigrade degrees above that of the apparatus, the difference of deflexion produced was equal to 32 degrees on the galvanometer scale; and in two instances the effect of the heat upon the current reached its maximum in about 15 to 20 minutes. The temperature of the room would not account for much of the effect, because it affected both sets of electrodes equally; a rise of it increased the current, not by increasing the voltaic action, for the reason just mentioned, but by diminishing the conduction-resistance of the liquid, and in averaging the results an allowance of five degrees of average deflexion on the galvanometer scale had to be made for each Centigrade degree average rise of temperature. These results prove that the permanent effect of the proximity of the lead upon the voltaic action was not due to heat from any source. At one period readings were constantly and regularly taken of a very sensitive thermometer placed upon the glass bulbs of the apparatus and plotted as curves, and these curves were compared with curves representing the changes of the voltaic current, and with curves of that current whilst under the influence of the lead bars. On all subsequent occasions also observations of the temperature of the apparatus were taken simultaneously with those of the amounts of galvanometric deflexion.

As in each of four different experiments the light and heat of burning magnesium, at a distance varying from three to six inches from the nearest electrodes, slightly decreased their electro-positive state, the light and heat had an opposite effect to that of the influencing bars.

[Whether temperature affects gravitation is an interesting question. "Von Sternack found, on comparing his observations in two mines, that the increase of gravity on descending was much more nearly in proportion to the rise of temperature than to the depth of descent." (H. Poynting, 'A History of the Methods of Weighing the Earth,' Proc. Birmingham Phil. Soc. vol. ix. p. 13.)]

Influence of various other circumstances.

With the object of further improving the apparatus and the method of using it, numerous experiments were made to test the influence of electrolytes of different composition, of amalgamating the electrodes, of including a very feeble voltaic cell or a thermoelectric couple in the circuit to balance the voltaic disturbance; also of tilting the apparatus, of moving the tubes to and fro during absence of the lead bars and during their presence, of comparatively rough motion of the tubes, of varying the length of the electrodes, of fixing the electrodes in the glass tubes by means of sealing-wax and of shellac, the influence of bubbles of gas or deposits of zinc oxide upon the electrodes, the use of a galvanometer of 6117 ohms resistance, &c.

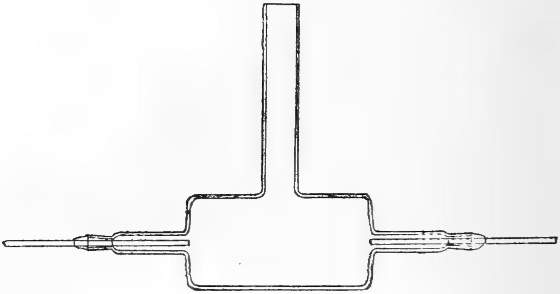
It was found that a solution of zinc sulphate was better than one of potassium chloride; that whilst a small proportion of chlorine, by forming hydrochloric acid, prevented bubbles and production of zinc oxide, too large an amount caused bubbles; that a solution of ammonium nitrate produced deposits of zinc oxide; that a small proportion of hydrochloric acid caused hydrogen; that amalgamation appeared to make the electrodes and, consequently, the solution more durable (it probably also diminished voltaic disturbance); that even a suitable liquid, if constantly used, required either renewal or a fresh addition of chlorine after a period of about a month or six weeks; that motion of the tubes disturbed the electric current; that using the apparatus whilst it was in a tilted position caused no difference; and that the galvanometer of a greater resistance gave larger deflexions. The defects of "No. 2" apparatus were that it offered too much resistance, was too slow in yielding the maximum effect, contained too small a stock of electrolyte, and the liquid was too quickly exhausted by continuous use.

Changes of terrestrial magnetism had very little effect; the apparatus was placed east, west, north, and south, without causing any perceptible difference. The chief variations were due to voltaic change and temperature, and the effect of the bars appeared to be a constant quantity.

APPARATUS "No. 3."

As in Apparatus "No. 2" the total conduction-resistance of the electrolyte was very large, a third one was now made offering very much less. It contained 20 tubes of the annexed form (see fig. 3). Each tube was 4 inches long, with its middle part 2 inches long and 1 inch diameter. The zinc-wire electrodes, all cut from the same piece, were fixed in the tubes by means of melted shellac, and their external portions were cotton covered and varnished. The electrolyte was composed of an 18-per-cent. preboiled solution of zinc sulphate $\frac{1}{23}$ saturated with washed chlorine gas.

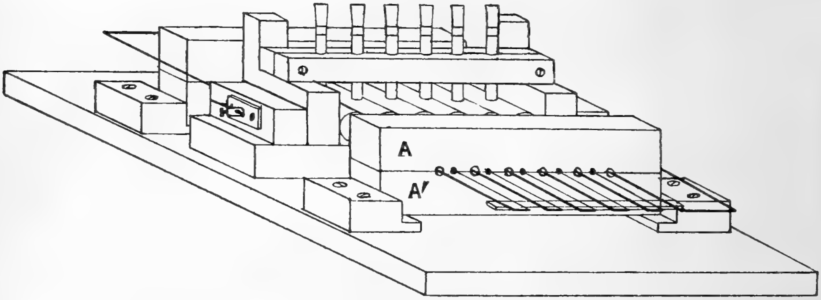
Fig. 3.



Description of Apparatus.

The annexed sketch (fig. 4) represents the form of apparatus, but only six tubes are included in order to simplify the drawing. The entire apparatus was about 24 inches long, 12 inches wide, and 5 or 6 inches high. A, A′ represent the bars of lead; each bar was 22 inches long, 1 inch wide, and $1\frac{1}{4}$ inch deep, and weighed about $10\frac{1}{2}$ lb. Each bar also had 40 semicircular grooves filed in one of its surfaces so as to form circular holes when in opposition, through which the glass tubes and connecting wires could freely pass without touching the lead *. The

Fig. 4.



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* Subsequently, plain bars without grooves, each weighing about 31 lb., were regularly employed, and only one pair was used.

outer ends of the insulated zinc and silk-covered copper wires were fixed by means of melted sealing-wax upon a narrow strip of very thin wood for the purpose of keeping them in their proper positions. The tubes and wires were carefully adjusted so that none of them came into contact with the lead. The frame moved in parallel wooden guides, not shown in the sketch. The sliding contact-parts were coated with blacklead, so that the to-and-fro movement was very smooth.

The sketch represents the glass tubes as being midway between the bars of lead.

The inner surfaces of the two pairs of bars were 5 inches apart. The electrodes were amalgamated. The galvanometer employed in all the subsequent experiments with this apparatus was a Thomson tripod one of 6117 ohms resistance, made by Elliott. Two other forms of apparatus were constructed and employed, but they were unsuitable and need not be described.

Method of Manipulation.

Immediately the circuit was closed the amount of deflexion caused by voltaic disturbance was recorded; the image of light was then brought to zero; and this was repeated until the voltaic current either ceased or became quite steady. When the liquid had only been recently made the disturbance was considerable, and required an hour or more to subside; but after it had been in use a week or more the irregularity subsided in a few minutes. In every case, however, the image was ultimately brought to zero by means of the controlling magnet, and the experiments were then made.

Whilst sliding the movable part to and fro, great care was taken not to suddenly stop or jerk it, and to observe that neither the glass tubes nor insulated zinc wires came into contact with the lead bars; the insulated copper wires were so thin and flexible that their occasional slight contact produced no disturbance. In consequence of the extreme sensitiveness of the arrangement, considerable experience was necessary in using the apparatus so as to obtain reliable results. This great sensitiveness to movement was found to be due to bubbles of air or gas upon the electrodes and was subsequently obviated.

With this apparatus the maximum deflexion caused by motion of the tubes, either in the presence or absence of the lead bars, commenced almost immediately and was usually obtained in about fifteen minutes; whilst with Apparatus "No. 2" it required about 2¼ hours, probably in consequence of the much greater total electric conduction-resistance. The amounts of deflexion were recorded at regular intervals of time, usually 2, 5, 10, 15, 30, and 60 minutes.

Causes of Variation of the Current.

Four chief causes operate to move the needles:—1st variation of the voltaic action; 2nd, approach or recession of the influencing substance; 3rd, change of temperature; and 4th, diurnal variation of terrestrial and atmospheric magnetism. Of these the first and second were the most powerful. The use of cadmium electrodes might diminish the first one.

Experiments.

Several experiments, including a total of 28 observations, were made during two days, to ascertain the effect of the usual to-and-fro movements of the tubes alone in the *absence* of the lead bars, the circuit being as usual left open all night. The motion had a very nearly uniform effect of making when in one direction ←—— the "A" electrodes positive, and when in the opposite direction ——→ the "B" ones positive; but in each case the electrical effect was opposite to that produced by the presence of the lead.

Similar experiments, including 46 observations, were then made during four days, with the lead bars *present*, the circuit being again left open all night. After due corrections had been made for the effects of motion of the tubes in the absence of the lead, the influence of presence of lead upon the "A" electrodes was still found to render them electro-positive, and upon the "B" ones to render them also electro-positive, and therefore to produce, or tend to produce, opposite currents with the two sets of electrodes, as in the experiments with the two previous apparatus. The disturbance caused by motion was subsequently eliminated by preventing the presence and formation of bubbles upon the electrodes.

The results of numerous additional experiments fully proved that the effect of the proximity of the lead was either to diminish or reverse the deflexions produced by the movements of the tubes alone. The opposite effect produced by the presence of the lead to that produced by its absence was not due to polarization, because the latter should have been the same in both cases. Polarization was in all cases of the research too minute to perceptibly affect the results.

After having fixed to the apparatus buffers of very elastic india-rubber, to diminish the sudden stoppage of motion, several series of experiments were made; in one series of 81 observations the lead bars were present, and in another of

103 observations they were absent, but the results obtained only confirmed the previous ones.

Exclusion of Air-Bubbles, &c.

Bubbles of air, which had caused considerable disturbance of the current, and which, owing to the narrowness of the spaces round the electrodes, were difficult to remove, were at a later period of the research perfectly excluded by the following means :—A few minims of the liquid, sufficient only to fill the annular space, were poured into each dry empty tube before placing it in the frame, and allowed to trickle very slowly down the inclined tube and displace the air ; if this did not succeed it was run back into the bulb and poured again, and the action repeated with shaking if necessary until all air was expelled; the opposite end was then treated similarly, and the bulb filled and the tube placed in the frame. Bubbles of hydrogen, due to corrosion of zinc, and the formation of insoluble subsalts of zinc, were prevented by employing a suitable electrolyte. The most improved liquid consisted of 750 c.c. of thoroughly boiled distilled water, 2990 grains of ordinary zinc sulphate, the solution filtered, and 25 c.c. of similar boiled water saturated with chlorine added to it.

Prevention of Leakage of Liquid.

To obviate any disturbance or want of insulation by leakage of solution, the bar of wood which supported the tubes was thoroughly varnished ; but even when a large number of tubes slightly leaked, the essential effects were clearly distinguishable. Leakage of liquid was, however, entirely prevented at a later period by the following method :—The ends of the dry tubes were coated inside with thick petroleum black varnish and allowed to dry ; shellac was then melted upon the corresponding parts of the wires and thoroughly fused when the wires were in the tubes ; and after cooling the junctions were thickly coated with the same varnish and allowed to dry ; the elasticity of the varnish prevented any cracking or separation of the rigid shellac.

Incidental Circumstances.

It was fully proved in various ways, and by several special series of experiments and observations, that the movements of the galvanometer-needles were not perceptibly due to the placing or removal of heavy bodies, nor to air-currents, the influence of light, or of magnets or articles of iron near the galvanometer, &c.

Effect of Magnetism.

In order to ascertain whether open magnetic circuits had any special degree of effect upon the electromotive force, seventy horseshoe magnets, each 8 inches long from poles to bend, 1 inch wide, $\frac{1}{4}$ inch thick, and their limbs $\frac{5}{16}$ inch apart at the poles, were clamped tightly together with india-rubber washers between them, face to face on their edges in a wooden frame by means of four brass rods and screws, in a horizontal row with all their similar poles above, thus forming a series 20 inches long, with a straight open highly magnetic space $\frac{5}{16}$ inch wide between the dissimilar poles to receive the glass tubes.

Several series of observations were made in the usual manner with the tubes between the poles, and with them removed to a distance of about 5 inches. The magnets produced precisely similar effects to the lead bars, *i. e.* they increased the electro-positive or diminished the electro-negative state of the approximated electrodes according as the latter happened to be electro-positive or negative at the time. Subjecting the electrodes to the influence of the magnets during 14 hours previous to closing the circuit had no perceptible effect.

To determine the effect of closed magnetic circuits, the magnets were taken apart and repacked in a similar manner except that each row of poles now consisted of alternately north and south ones, with a series of horizontal soft-iron armatures placed upon all of them, and as large a quantity of soft-iron filings as they would attract, thus leaving a nearly non-magnetic space between them for the tubes.

Several series of observations were made as usual under these conditions, but the effects were precisely the same as with the active magnets, and no difference of magnitude of influence due to different strength of magnetism could be detected.

Mode of Action of the Influence.

In order to ascertain whether the proximity of the influencing body acted by altering the voltaic electromotive force or by changing the conduction-resistance of the liquid, two plain rectangular bars of copper, each being 22 inches long, 2 inches wide, and 2 inches thick, were employed. They were first placed in the usual positions as near as possible to the row of electrodes which happened to be electro-positive, and the usual series of observations taken; and then placed near the negative ones and other series taken. The effect of their proximity was, as usual, to increase the current in the first case and decrease it in the second, thus proving that the influencing bars acted upon the electromotive force and not upon the conduction-resistance ; had the whole or even the greater portion of the effect been to diminish the amount of conduction-resistance, the current would have been increased in each case. This experiment of placing the copper bars near the negative electrodes was repeated by another 36 observations on a subsequent day, and the same result—viz. diminution of the electro-negative state—was obtained.

Degrees of Influence of Various Substances.

The apparatus having been made sufficiently perfect, and the mode of treating the observations satisfactory, numerous series of experiments were made to approximately ascertain the relative magnitudes of effect of equal volumes of different substances, by employing pairs of plain rectangular bars, each bar being 22 inches long, $1\frac{3}{4}$ inch wide, and 2 inches deep, placed above each other at a distance of $\frac{5}{16}$ inch apart, so that the glass tubes might slide without friction into and out of the horizontal space between them. The substances used included copper, lead, bismuth, antimony, cast iron, wrought iron, brass, magnesium, zinc, flint glass, slate, ebony, deal, and gypsum-plaster—every one of which gave the usual kind of effect.

It usually occupied five days to obtain the final average value of effect of a substance. In order to make proper correction for the value of the voltaic current alone, which was usually the largest element (unless the solution had been recently made), an average value of that current alone was obtained on the first day ; a ditto for that of the current *plus* the effect of the substance on the second and third days ; for the voltaic current alone on the fourth day ; and for the current *plus* influencing substance on the fifth day.

On the first day, the tubes being 5 inches distant from the bars and the circuit having been left open all night, the latter was closed in the morning, and after five minutes the spot of light, having become steady, was brought to zero on the scale. Observations were then taken every fifteen minutes, both of the temperature of the tubes and the amount of deflexions. Immediately after each observation the circuit was opened during a few seconds, the needles allowed to become steady, the deflexion again noted, the first deflexion corrected for the amount of the second one, and the true amount thus obtained. This was done about forty times, and the average value of the corrected current obtained by taking the average of all the observations.

To find the average value of voltaic current *plus* that of the effect of the substance:—On the second day, the tubes being 5 inches distant from the bars, and the circuit having been left open all night, the latter was closed in the morning, and after five minutes the spot of light was brought to zero. The tubes were then immediately slid into the space between the bars, and the observations of deflexion and temperature taken forty times during the day in the above manner.

In order to correct the average values of the currents of the first and second days for difference of temperature, an allowance of five degrees of the average deflexion for each Centigrade degree difference of average temperature of the two days was made to bring the average temperature of the one day to that of the other. The corrected average value of the voltaic current alone was then subtracted from that of the voltaic current *plus* the effect of the substance, in order to obtain the value of the latter. This was done three times for each week, and the average of the three taken as being the final value of effect of the particular substance.

In consequence of the final average number for each substance varying considerably with the age and amount of use of the solution, I was unable to obtain a reliable series of relative values of effect of all the substances; but, by comparing the values obtained of substances immediately after each other, it was found that heavy ones usually gave greater effects than light ones, approximately in the following order:—copper; wrought iron; cast iron; lead; bismuth and antimony; flint glass; brass and magnesium; zinc and slate; ebony, deal, and plaster. This general relation of behaviour of heavy substances and light ones is not unlike that of their degrees of opacity to Röntgen rays. As the effect did not approximate very strictly to the order of the specific gravities of the substances, I venture to suggest that whilst the greater proportion of effect may be due to rays of gravity, other rays may assist in producing it; it is also probable that the rays proceeding from one substance differ somewhat in kind from those proceeding from another and produce a different amount of voltaic change.

Effect of Mass of Substance.

With the object of ascertaining whether the influence varied directly as the mass of the substance, or proceeded only from its external surface, the effect of two sheets of copper, 22 inches long, 2 inches wide, and ·013 inch thick, was compared with that of the usual copper bars, the surfaces of each pair being $\frac{5}{32}$ inch from the axis of the electrodes. The result of this experiment was that the sheets gave somewhat less than one-half—viz. a proportion of 1·0 to 2·69—of that of the bars, but a much larger proportion than that which was due to their relative mass, which was 1·0 to 153·84, doubtless because their mean distance was much less than that of the bars, viz. as 1·0 to 8·838 from the surfaces of the electrodes. The influence therefore came from the internal mass of the influencing substance as well as from its superficial particles, and so far agrees with the action of gravity.

Influence of Distance.

The only experiments on the influence of distance of the bars from the electrodes were made with a pair of bars of wrought iron of the usual dimensions. With the nearest surfaces of the bars at a distance of $\frac{5}{32}$ inch from the axis of the electrodes, and at $\frac{10}{32}$ inch, the numbers obtained were respectively 63·06 and 20·31, the latter being considerably greater than the calculated theoretical amount.

Influence of Screens.

A number of series of experiments and observations were made in the usual manner for the purpose of detecting, if possible, an influence of screens. Each series with a single pair of bars and a single pair of screens extended over five days. The bars included copper, bismuth, lead, and zinc; and the screens comprised copper, lead, zinc, and tinned iron; the bars were of the usual dimensions; the screens were 22 inches long and 2 inches wide, and the thicknesses of them were:—copper ·013 inch, lead ·009 inch, zinc ·010 inch, and tinned iron ·006 inch. The nearest surfaces of them were at a distance of $\frac{5}{32}$ inch from the axis of the electrodes, and the bars were in immediate contact with them. Copper bars were used with screens of lead, zinc, and tinned iron; lead ones with copper, zinc, and tinned iron; bismuth with copper, lead, zinc, and tinned iron; and zinc with those of lead: no screening effect was detected in any of the eleven instances. As the radiant influence was not manifestly intercepted by screens, it appears to partake so far of the nature of gravity.

I have to express thanks to Messrs. Stock & Taylor, of Birmingham, for the loan of 74 cwt. of lead; to Messrs. Osler for the preparation and loan of bars of glass, brass, and plaster; and to the Magnesium Company, Patricroft, Manchester, for preparing and lending the bars of magnesium, bismuth, and antimony.

LIX. *On a Supposed Proof of a Theorem in Wave-Motion.*

To the Editors of the Philosophical Magazine.

GENTLEMEN,

IN a letter printed at page 368 of this volume, under the above title, Dr. Stoney directs attention to a communication* in which I expressed the possibility of expanding any function of any number of variables in the form

$$f(x, y \ldots) = \Sigma A \cos(lx + my + \ldots) + \Sigma B \sin(lx + my + \ldots). \quad . \quad (1)$$

He admits this to hold good for "scalar functions," but for such functions only; and he concludes that the applications of it "to prove certain physical theorems which treat of real wave-motions" are erroneous.

In reply, I may state first of all that I fear Dr. Stoney has misunderstood my communication. What I intended to convey was, that if we take the variables to be x, y, z, t—namely, the coordinates of a point in space and the time—then the above expansion enables us to resolve any specified disturbance, existing throughout any given region, into a system of simple harmonic plane waves. When I say a specified disturbance, I mean simply that the disturbance at every point of the region is expressed in the ordinary way in terms of its three component velocities or displacements in the form

$$\xi = F_1(x, y, z, t), \quad \eta = F_2(x, y, z, t), \quad \zeta = F_3(x, y, z, t),$$

and I did not think it necessary to state such an obvious proceeding. Each of these functions can be expanded, as Dr. Stoney admits, and the components of the simple harmonic plane waves are then to hand.

Of course a velocity (or a displacement) is a directed quantity, and a function which represents a velocity is a vector function; but it is here a vector function of scalar variables, and accordingly the analysis which I have employed holds good. In fact, if F be such a vector function, we may write it in the form

$$F = iF_1 + jF_2 + kF_3,$$

where F_1, F_2, and F_3 are scalar functions which, by admission, may each be expanded in the form (1), so that F is thrown at once into a sum of simple harmonic terms of the type

$$(iA_1 + jA_2 + kA_3) \cos(lx + my + \ldots) = A \cos(lx + my + \ldots),$$

---

* "On the General Extension of Fourier's Theorem," p. 281 of this volume.

where A is a directed quantity and is obviously determined by the definite integral given on p. 283, being related to F in the same way as A_1 is to F_1, &c.

Dr. Stoney in fact admits as much as this, but he then goes on to say on p. 371 that "on a close scrutiny we find that although this furnishes an apparent solution, in the form of forced vibrations, or rather a group of such solutions, the group unfortunately does not include the solution which would be selected by nature under any conceivable circumstances. The analysis furnishes undulations which could not propagate themselves through any medium. The motions which it furnishes are the non-natural motions of a mere forced kinematical resolution, of no use in physics. That this is so can be made plain by taking a very simple example . . . ;" and he then takes an example and discusses it in a manner which shows clearly that his letter was written under some misunderstanding as to my interpretation of the expansion. What I state is, that when the disturbance which exists throughout any given region is specified, then a system of plane waves can be determined which will produce the same disturbance at every point of that given region. Now in his example Dr. Stoney takes the given region of space to be a circular disk having its plane perpendicular to the axis of z, and he specifies the disturbance throughout the disk to be

$$\xi = f(vt-z)+f(vt+z);$$

so that in this case the specified disturbance throughout the given region is already in the form of plane waves, and these can be resolved into simple harmonic components when the form of the function f is given; and nothing more remains to be done. As to what happens outside the disk, this is quite another question and belongs to the class of problems dealt with by Sir G. G. Stokes in his classical paper on the Dynamical Theory of Diffraction.

So far there has been no necessity whatever for considering the nature of the medium or the manner in which it propagates waves, plane or curved, or even whether it propagates waves at all. We simply ask for the disturbance, and this being given we can determine the equivalent simple harmonic plane-wave system.

But if the given disturbance happens to be the actual disturbance existing in some medium capable of propagating plane waves unaltered, then the specification of this disturbance will involve the properties of the medium. The equivalent plane-wave system will represent the actual disturbance, and these waves will be propagated through the medium and will continue to represent the disturbance.

Thus when the disturbance is given and the plane-wave components are determined so as to satisfy the initial conditions throughout all space, then this same plane wave system will continue to represent the subsequent disturbance in all its stages, and the waves will be propagated as plane waves.

As Dr. Stoney contrasts the method adopted in my communication with that employed by himself elsewhere, I feel bound before concluding to take this opportunity of protesting against the method adopted by Dr. Stoney in his proof of the theorem on p. 276. I object to the ease and freedom with which he rides off to infinity on a spherical wave and comes back on a plane wave. One does not feel quite sure as to what has happened in the meantime. Why go to infinity in order to find out what is going on about home? Why deal with a very long cylinder of finite width rather than a very narrow one of moderate length? If it is true that a curved wave may be replaced by its tangent planes, considered as infinite plane waves, this should be demonstrated about home rather than at infinity. A spherical sector of moderate area certainly approximates to the corresponding area in the tangent plane as the radius of the sphere increases. But this sector travels out as part of a complete spherical wave, while, when reversed, it returns as a segment of a wave. How does it return? Is it supposed to be geometrically reversed, so as to focus at the original centre, or does it diffuse throughout space by diffraction during the whole time of its return? Is it evident by any method that this sector, when reversed, will produce the same effect at the original centre as the whole tangent plane wave would?

Such are some of the difficulties which I recognize in the method, and these are real and great difficulties to those who are less deeply versed than Dr. Stoney in the philosophical aspect of the subject.

I am, Gentlemen,
Faithfully yours,
Thomas Preston.

Bardowie,
Orwell Park, Dublin.

LX. *Notices respecting New Books.*

The Phase Rule. By Wilder D. Bancroft. Ithaca, New York. The Journal of Physical Chemistry, 1897.

We have been chiefly indebted to Dutch and German workers for the great advances made in our knowledge of physical chemistry during the past few years. Now, however, the school of physical chemists in America has shown its existence and activity by the publication of a monthly journal of which Professor

Trevor and the author are joint editors, and the present volume is issued by the publishers of the journal.

The work consists of a general discussion, without mathematics, of the conditions of equilibrium of chemical mixtures, in which the amount of any component, the temperature, and the pressure are considered as variable. Van 't Hoff, in his 'Studies in Chemical Dynamics,' and others have examined a few such cases of equilibrium quantitatively, while Roozeboom has done much experimental work of a more qualitative character; but, so far as we are aware, no attempt has been made until now to classify the phenomena.

The author defines a *phase* as a mass of uniform concentration, and its *components* as the substances of independently variable concentration contained in it. For a mixture of phases in equilibrium every phase will furnish an equation involving functions of its components together with the temperature and pressure. The number of equations will thus be equal to the number of phases, while the number of independent variables will be $n+2$ if the mixture contains n components. A system consisting of $n+2$ phases will thus give as many equations as there are variables; such a system can only exist at a single temperature and pressure and is called *non-variant*. An equilibrium mixture of ice, water, and steam belongs to this type. If the number of phases is only $n+1$ the system is monovariant, and is fully determined by the specification of one of its variables; with n phases the system is divariant, and so on. The scope of the present volume is limited to non-, mono-, and di-variant systems, with at most four components; and in the discussion of changes of equilibrium the general theorem of Le Chatelier is made use of, namely, that whenever a system is disturbed from without, it adjusts itself to the new conditions by the occurrence of a corresponding reverse change within itself; for example, an increase of external pressure will cause an increase in the amount of the denser phases. The author demonstrates the extreme usefulness of this theorem in predicting the effect of any given change.

The value of Mr. Bancroft's treatise lies not so much in the fact that it presents the results of a large amount of experimental work in physical chemistry, but rather that it indicates a scientific method of classifying all such work; he has, in fact, taken the first step towards the formation of order out of chaos in this department of science. J. L. H.

LXI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 396.]

March 24th, 1897.—Dr. Henry Hicks, F.R.S., President, in the Chair.

THE following communications were read:—

1. 'Notes on some Volcanic and other Rocks which occur near the Baluchistan-Afghan Frontier, between Chaman and Persia.'

By Lieut.-Gen. C. A. McMahon, V.P.G.S., and Capt. A. H. McMahon, C.I.E.

In the first part of this paper Capt. McMahon describes briefly the physical geography of the Baluchistan deserts, which extend along the south of the Helmund River, between Quetta and Persia. Taking first the plains and their drainage-system, he shows how the wide alluvial plains of Shorawak and Chagai were probably in earlier times one large lake. North and west of these plains, as far as Persia, lie vast deserts of sand, which in places are gradually encroaching upon and burying the mountain-ranges which rise up like islands in the desert. He shows how the sand has intercepted all the drainage from the mountains and prevented it from making its way, as it would otherwise do, into the Helmund River and the God-i-Zirreh Lake. Turning next to the mountains, Capt. McMahon describes a well-defined line of fault, which he traced for a distance of about 120 miles from north of Chaman, along the Khwaja Amran and Sarlat mountain-ranges to Nushki. East of this fault all the rocks appear to be sedimentary; while those to the west are all, with few exceptions, volcanic and igneous.

The mountain-ranges in the desert described appear to be all volcanic, and reference is made to the Koh-i-Taftan, 12,600 feet high, lying south-west of them, which is still an active volcano. The curious, grotesquely-shaped peaks of the Koh-i-Sultan range are then briefly described, and especially that named Neza-i-Sultan —a gigantic natural pillar of volcanic agglomerate many hundreds of feet high.

After thus describing the general character of the country, Capt. McMahon points out the very remarkable force and activity with which certain natural agents are at present at work there—namely, water, wind, sand, and extremes of heat and cold.

In the second part of the paper Gen. McMahon describes the microscopical characters of the rocks, which consist of lavas, ashes, pumice, igneous intrusive, and sedimentary rocks. The localities in which ores of lead and copper, gypsum, sulphur, etc. occur abundantly are also mentioned.

Some andesites are described, which are especially interesting from a petrological point of view. Rosenbusch mentions that a brown hornblende occurs in some rocks in which the angle of extinction varies from small to *nil*. Some of the andesites described abound in amphibole, red-brown in transmitted light, which possesses the optical properties and specific gravity of anthophyllite, and which uniformly exhibits straight extinction. It is an original, and not a secondary mineral.

These anthophyllite-bearing augite-andesites also contain olivine —a mineral rare, but not unknown, in this class of rocks.

2. 'On the Association of *Sigillaria* and *Glossopteris* in South Africa.' By A. C. Seward, Esq., M.A., F.G.S., University Lecturer in Botany, Cambridge.

3. 'Notes on the Occurrence of *Sigillaria*, *Glossopteris*, and other Plant-remains in the Triassic Rocks of South Africa. By David Draper, Esq., F.G.S.

April 7th.—Dr. Henry Hicks, F.R.S., President, in the Chair.

The following communications were read:—

1. 'On the Morte Slates and Associated Beds in North Devon and West Somerset.—Part II.' By Henry Hicks, M.D., F.R.S. P.G.S. With Descriptions of the Fossils by the Rev. G. F. Whidborne, M.A., F.G.S.

In the first part of this paper, read by the author before the Society in February 1896, he described the Morte Slates as they occur in North Devon, and the fossils found in them. In this, the second part, he refers mainly to the rocks classified as Morte Slates in West Somerset. He shows that the latter differ in some important characters from those in North Devon, and have an entirely distinct fauna. The fossils obtained from North Devon show that there the beds must in the main be classed with the Silurian rocks; but in West Somerset, so far as discoveries have yet been made, the fossils indicate that they should be classed with Lower Devonian rocks. The author's contention that the Morte Slates which extend through the centre of North Devon and West Somerset from Morte Point to the north of Wiveliscombe, a length of about 40 miles, are the oldest rocks in the area and form an axis with newer rocks lying to the north and to the south, is therefore fully proved by stratigraphical and palæontological evidence. The fossils are carefully described by Mr. Whidborne, and he shows that there are numerous forms in common between them and those considered to be characteristic of the Lower Devonian rocks in the continent of Europe and in America.

2. 'The Glacio-Marine Drift of the Vale of Clwyd.' By T. Mellard Reade, Esq., C.E., F.G.S.

The local drift of the higher parts of the Vale of Clwyd is replaced by marine drift towards the mouth; and it is the object of this paper to give the results of a detailed examination of these marine drifts, rather than to explain the phenomena. The first part of the paper gives the results of an examination of the boulder-clay from Craig, west of Llandulas, to the Vale of Clwyd, south-east of Abergele.

Mechanical analyses of the clays are given; but the point of greatest interest is the occurrence of abundance of foraminifera, especially in the plastic brown and red Boulder Clays, which often contain intensely striated erratics. These foraminifera have been examined by Mr. Joseph Wright. Most of them occur in the boulder-clays of Crosby and Wirral; but some of them are very rare in British localities other than those now recorded, namely, *Rhabdogonium tricarinatum*, *Sphecoidina bulloides*, *Pullenia sphæroides*, and *Pulvinulina Menardii.*

The glacial sands and gravels of the east side of the Vale of Clwyd are also described, and especial attention is called to an esker-like mound of sand (gravel), occurring south-east of Diserth Castle, formed of bedded deposits, the bedding of which generally follows the outer form of the ridge. The ridge has Boulder Clay upon the flanks, and is described as rising through the Boulder Clay.

LXII. *Intelligence and Miscellaneous Articles.*

To the Editors of the Philosophical Magazine.

Gentlemen,

ALLOW me to call your attention to the fact that in January 1897 Messrs. Stroud and S. B. Henderson published in No. 260 of this Journal a paper entitled "A Satisfactory Method of Measuring Electrolytic Conductivity by means of Continuous Currents," in which several authors are quoted who have employed methods similar to that described in the paper above mentioned.

Messrs. Stroud and Henderson must have forgotten or overlooked my statement in Wiedemann's *Annalen*, vol. xxiii. pages 482–490, in which *not* a similar, but in fact *the very same* method is described by me.

The fig. 1 on page 21 of the paper in your journal is quite the same as the fig. on page 482 in my statement. I have employed for the equal resistance only 20 S. E., whereas Messrs. Stroud and Henderson have used 1000 ohms.

Certainly I was aware of the advantage of a higher degree of resistance, and the reason for the little power I used was, as I expressly mentioned on page 483, the fact that I had no greater resistance at my disposal.

I remain,
Yours faithfully,
F. Neesen.

THE HEATS OF VAPORIZATION OF LIQUIDS.

To the Editors of the Philosophical Magazine.

Gentlemen,

Mr. Griffiths has kindly drawn my attention to a slight error on page 298 of my paper in your April issue, viz.: that the measurements by himself and Miss Marshall of the latent heat of benzene extended up to 50° C., instead of 40° as I inadvertently stated.

I may also take this opportunity of noting that on page 290 of the paper on "The Variation of the Dissociation Coefficient" the last three equations are reduced to the ordinary form by expressing the gas-constant R in gram-calories, instead of in the work-units used previously. This explains the disappearance of J from the previous equation, and should have been stated at the time.

I am, Gentlemen,
Your obedient servant,
S. R. Milner.

University College, Bristol,
May 1, 1897.

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END OF THE FORTY-THIRD VOLUME.

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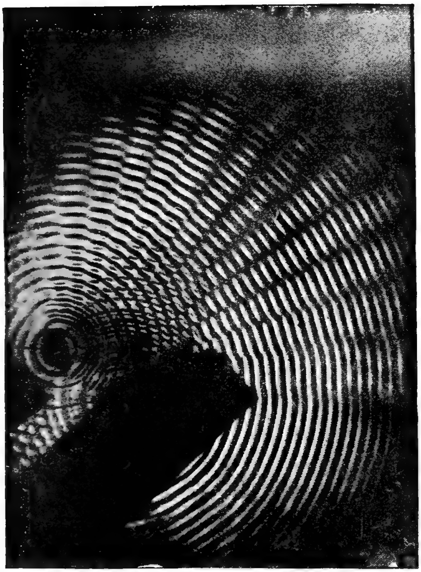

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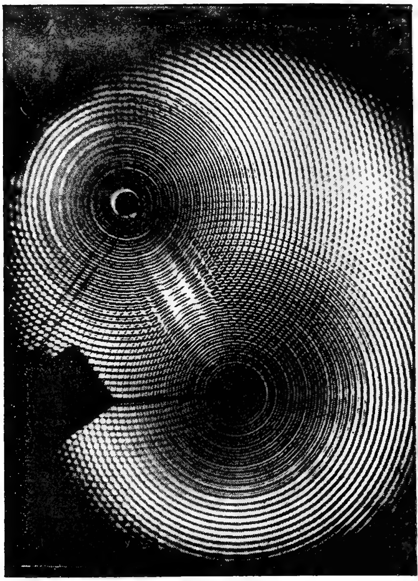

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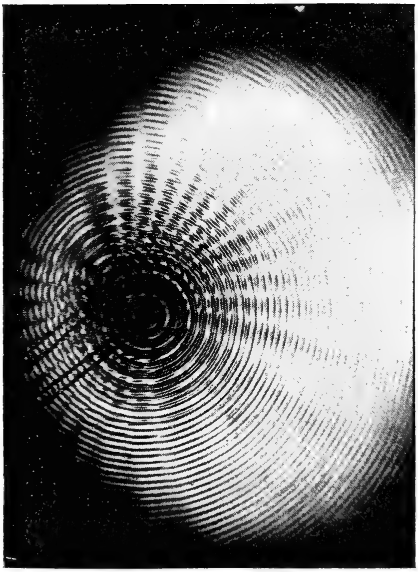

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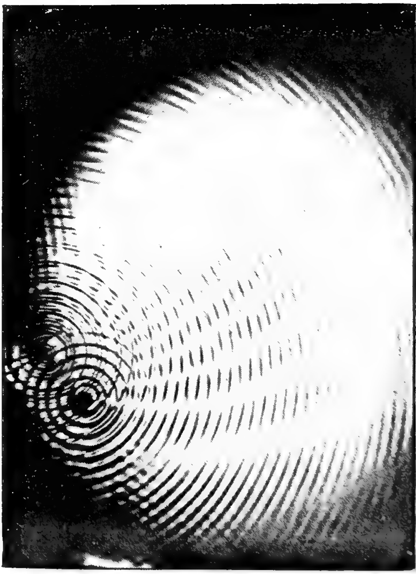

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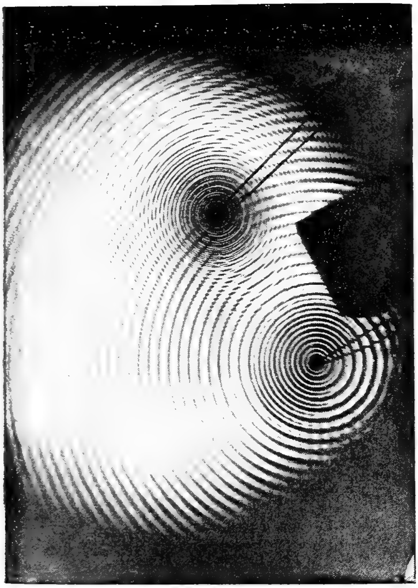

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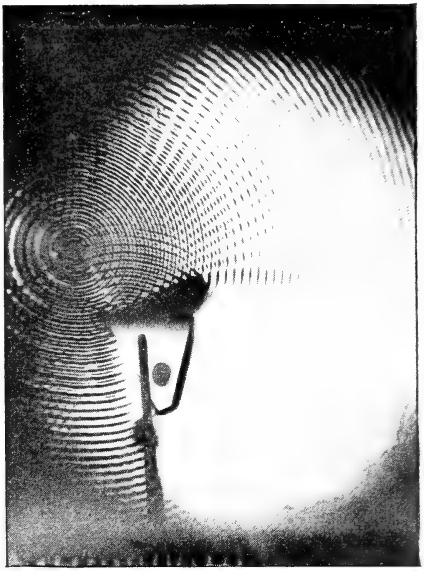

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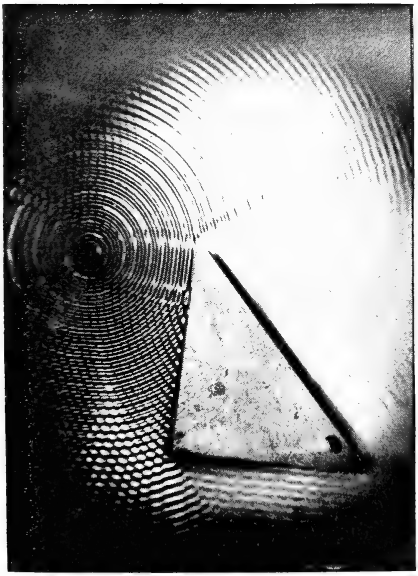

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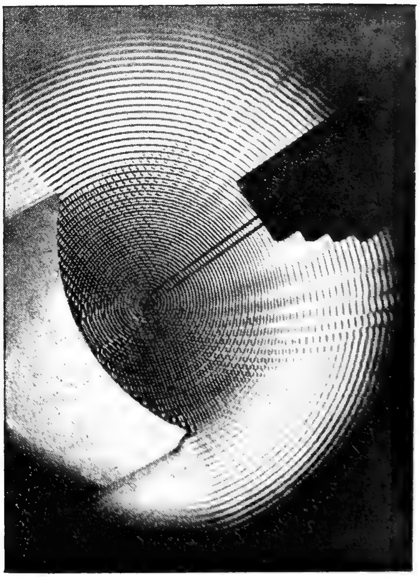

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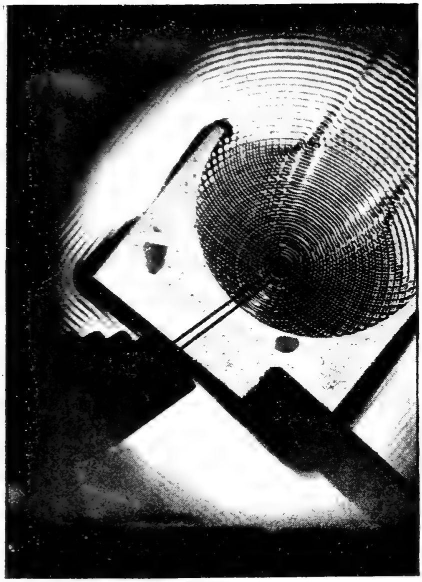

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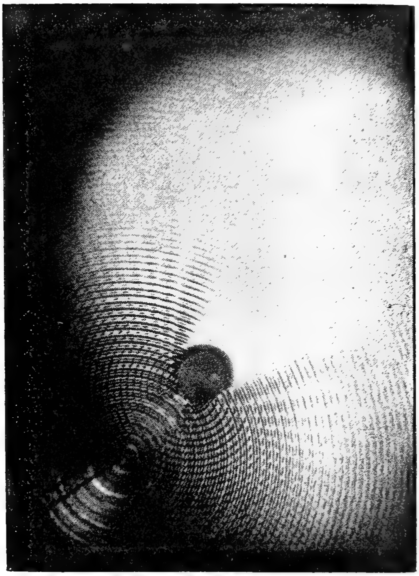

Fig. 10.

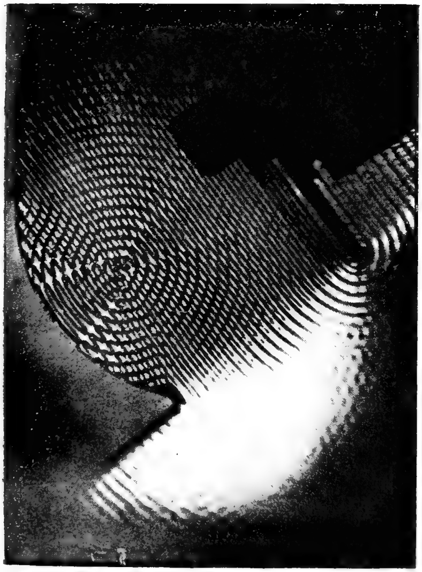

Fig. 11.

Fig. 12